Persuasion and Ideological Voting in Legislatures

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Abstract

American legislators are generally thought to be ideologues who take consistently partisan positions, but recent research suggests they are also pliant targets of persuasion campaigns by special interests, lobbyists, fellow legislators, and even academics. This paper explores this seeming discrepancy. First, we revisit the credibility of findings of legislative persuasion to determine whether legislators' positions can be changed, and if so whether such changes are robust and long-lasting. Second, we examine whether persuasion works against, or alongside, ideology. Finally, we interview legislators to hear their experiences as targets of persuasion. We conclude that legislative persuasion can be long-lasting and found on bills other than those targeted by advocates. Persuasion can increase ideological position-taking and polarization by allowing legislators to better sort into positions consistent with their broader ideology. Finally, persuasion works through different mechanisms, including learning about policy expertise or electoral considerations, that vary in relevance across legislators. Our findings suggest legislative persuasion may play a key role in how parties, interest groups, and constituents drive polarization.

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> We have no good normative models of how persuasion ought to work in democratic legislatures, nor much empirical evidence on how it actually does work.
> — Jane Mansbridge (1994, p. 303)

> There may be changing minds, but they are not in Congress.
> — Keith Poole (2007, p. 435)

Legislators have generally not been considered to be persuadable. Whereas voters have long been feared susceptible to influence from friends, neighbors, political elites, the media, and government propaganda, legislators are thought to take consistent positions based on their ideological predispositions (Brimhall and Otis 1948; Bonica 2014; Shor and McCarty 2011; Fouirnaies and Hall 2021). Whether these ideologies are sincere personal policy goals, strategic presentations of self meant to win elections, or a mix of both, they predict an overwhelming share of legislators' votes (Poole and Rosenthal 1991; Poole 2005; Poole and Rosenthal 1997). While Converse (1964) may have taken aim at unconstrained policy thinking among the public, his position that political elites' policy preferences are constrained across issues, stable over time, and resistant to influence has become the conventional wisdom in legislative studies (Granberg and Holmberg 1996; Jennings 1992; Lee 2009; Lupton, Myers, and Thornton 2015; Baekgaard et al. 2017). Wilcox and Clausen (1991, p. 393) note that "Without much debate, researchers have accepted the view that a single liberal/conservative dimension accounts for much of congressional roll-call voting."

Over the past decade, studies have shown that lawmakers' positions on legislation can be influenced by lobbying, policy research, public opinion polls, and grassroots advocacy (Bergan 2009; Bergan and Cole 2015; Butler and Nickerson 2011; Hjort et al. 2021; Grose et al. 2022; Zelizer 2018, 2019, 2022). A five minute policy briefing or a handful of emails from constituents change 5 – 10% of legislators' support for anti-smoking bills or bills to provide benefits to veterans. Providing legislators with a public opinion poll may alter up to 30% of votes on budget bills. These are large effects on real policy proposals from relatively

circumscribed campaigns. How can we reconcile the conventional wisdom of ideologically constrained voting — where 90% of votes or more can be predicted by a single ideological dimension — with evidence that up to 30% of legislators' votes can be changed by a public opinion poll?

This paper sets out to answer three questions. The first is whether the literature presents credible evidence that interventions change legislators' positions, or whether findings are suggestive of publication bias or other questionable research practices. Our second question is how legislators can both be persuadable and ideologically constrained in their voting. Does persuasion work against, or facilitate, ideological voting? Third, what is the mechanism behind persuasion? What about legislators' decision calculus is affected by information given to them during these experiments?

To answer these questions, we analyze four legislative experiments fielded in different legislatures, with different treatments, on different types of bills. The diversity of settings, treatments, and bills abstracts away from the specifics of any one study to understand legislative decision-making writ large. It also gives us more confidence in the generalizability of these results than prior field experiments conducted in a single legislature. Our goal is to better understand the "normal decision-making" of legislators on the substantive, contested, low salience bills that constitute much of legislative agendas (Matthews and Stimson 1975).

We first examine the credibility of legislative persuasion by asking whether interventions crossed over to influence bills other than those analyzed in the original studies. In several studies, similar and even identical bills were filed contemporaneously or years later in the same legislatures where interventions occurred. If these interventions were truly persuasive, we would expect legislators' positions on these other bills to be affected.

We then examine how persuasion relates to ideological voting. We compare voting patterns in control and treatment groups to see whether persuasion increased or decreased ideological voting. Did legislators vote more or less in line with like-minded peers when assigned to treatments? We examine heterogeneity in treatment effects to see which kinds

of legislators were most susceptible to persuasion, and whether these patterns are consistent with a spatial model of ideological voting.

Finally, we interviewed legislators who participated in one of the decade-old studies to ask them broadly about advocacy and efforts to influence their positions, including with respect to the campaign at the center of the study. While we are interested in their perceptions about the frequency, scope conditions, and most effective methods of persuasion, we are most interested in hearing their views about the operative mechanisms behind persuasion. Their responses help guide our interpretation of the quantitative results and our thinking about how persuasion may look different among legislators than among the mass public.

We conclude that legislators are persuadable in the sense of learning policy-specific expertise or electoral considerations that changes their evaluation of specific bills. Downstream effects decay little from immediate effects. We estimate persuasion on similar, contemporary bills other than those targeted by advocates. Persuasion not only occurs alongside constrained ideological voting, but also contributes to it by means of helping legislators align their votes with their ideological predispositions. Moderates and electorally-vulnerable legislators appear most responsive to information. Persuasion campaigns by party elites, donors, lobbyists, or special interests may be a key factor in the increasing ideological constraint of legislative voting and the polarization between the parties by allowing legislators to more consistently sort into coalitions with like-minded peers.

Legislative Persuasion

> I've come to believe that at the heart of it all — indeed, at the heart of representative democracy itself — is persuasion.
>
> — Congressman Lee H. Hamilton (2019)

Persuasion is an understudied topic in legislative studies (Mansbridge 1994). Persuasion has long been central to the study of the Presidency and of public opinion, but legislators

themselves have less often been studied as targets of persuasion. One reason for this omission is the dominance of ideology in the study of legislative behavior (Lee 2009). If legislators' positions on a given bill are to be consistent with their broader ideological predispositions, there seemingly is little room for legislators' positions to be changed by advocates, constituents, or party leaders. They will ultimately take the position predicted by their ideology, so how can they be persuadable?

This strong, almost deterministic view of ideology and voting is at best incomplete. Even if legislators end up taking positions that are predicted by their ideology, persuasion may be the mechanism by which they learn to take the ideologically constrained position. Further, not all positions are correctly predicted by ideology, so persuasion may cause these occasional 'errors' in voting. Finally, persuasion may shift the position of many legislators — for example, by convincing all opponents to support a bill — such that legislators' votes can still be perfectly predicted by ideology simply by moving the cutting line between bill supporters and opponents.

To illustrate various mechanisms behind persuasion, consider the following stylized model of legislative decision-making. Each legislator i can be characterized by an ideal point y^i on the real line and a certain degree of electoral popularity M^i, the legislator's expected winning vote margin at the next election. Legislators of course do not know how many votes they will receive in the future, but they have expectations based on past elections and interactions with voters, interest groups, and other stakeholders that should lead them to have at least somewhat informed expectations for their next election.

Legislators must decide whether to vote for a policy at some ideological point $x > q$, the status quo. Voting for q provides the legislator with the following payoff:

$$-|y^i - q| + f(M^i) \tag{1}$$

The first term represents the ideological payoff and the second the electoral payoff of

voting for the status quo.[1] We can assume that $f(M^i)$ is strictly convex for $M^i \leq 0$ and strictly concave otherwise; the marginal value of increasing M^i is very small for legislators who expect to surely lose or win re-election and is large for those in competitive races.

Voting for the proposal x yields the following payoff:

$$-|y^i - x| + f(M^i + \kappa) + v \tag{2}$$

with v the valence, a separate, non-ideological dimension of policy over which legislators have common preferences (Hirsch and Shotts 2015), and κ the electoral benefit (or cost, if negative) of voting for the proposal. Legislators vote for the proposal if Equation (2) is greater than Equation (1). There is a threshold y^* below which legislators vote for the status quo and otherwise the proposal:[2]

$$y^*(x, v, M^i, \kappa) = \frac{x - q}{2} + f(M^i) - f(M^i + \kappa) - v \tag{3}$$

We can now distinguish several mechanisms of persuasion. First, imagine legislators have heterogeneous beliefs about the content of the proposal, such that $x^i = x + \epsilon^i$ with ϵ^i being random noise. In this case, the ideological threshold varies with legislators' beliefs x^i and thus need not neatly divide bill supporters and opponents by ideology. Figure 1 displays a stylized model of four legislators, with Panel (a) showing this case of apparently unconstrained voting.

Suppose a treatment clarifies that the proposal's ideology is x rather than x^i. Such information will have heterogeneous effects on bill support based on y^i and x^i. For legislators who thought $x^i > x$ (Legislator y^3 in the Figure), the threshold for bill support will decrease,

[1] This framework assumes ideological voting is expressive and separable from legislators' electoral considerations. This assumption will be true if the proposal in question does not neatly map to a primary underlying ideological dimension or is not salient to ideological interest groups. In practice, since we do not observe legislators' ideal points but only estimate them from revealed positions, estimated ideal points may not fully incorporate electoral considerations from new proposals.

[2] For simplicity, we assume that κ and v are small in this case relative to the ideological distance between q and x. Otherwise, there will still be a threshold for y that determines bill position, but it need not be located halfway between x and q.

potentially causing a legislator who would have supported the status quo in control to support the proposal in treatment. A similar, but opposite, effect may happen among legislators who thought $x^i < x$ (Legislator y^2). The result is that aligning beliefs about the bill may increase ideological sorting (Panel (b)); decrease support among legislators for whom $x^i < x$; and increase support for those with $x^i > x$. It is moderate legislators, those with ideal points near the threshold, who are most likely to change positions.[3] In the aggregate, treatment may increase or decrease support for x depending on the distribution of x^i and y^i.

Panel (c) shows a special case of this mechanism where $x^i > x$ for all legislators. In this case, learning that the bill is more moderate than thought increases bill support in the aggregate. Again, it is moderate legislators most likely to change positions.[4]

A second mechanism concerns valence v. Bill sponsors, advocates, or lobbyists may convince legislators that a bill is of high quality (Panel (c)). Increasing v will increase support for x in the aggregate; will not decrease support for x among any subset of legislators; and affects legislators near the threshold, i.e. moderates.

Changing beliefs about x and v have important empirical implications in common. It is legislators closest to the threshold y^* who are most likely to change their positions. In many cases, that will mean moderate legislators. Second, changing positions, even based on the perceived ideology of a proposal or legislators' ideal points — need not diminish the predictive power of ideology and, in some cases, might increase it.

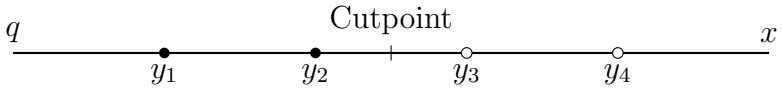
A final mechanism can generate distinct empirical predictions. Suppose legislators in treatment learn that the electoral benefit of supporting the bill is larger than thought by those in control, $\kappa^t > \kappa^c$. The added electoral benefit may again increase support among legislators near the ideological threshold. However, because of assumptions on $f(M)$, this

[3]We assume that we are considering contested bills that correlate with the ideological dimension. In this case, the threshold separating coalitions will fall among legislators with ideal points in the middle of the distribution.

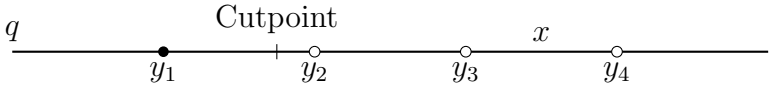
[4]An observationally equivalent mechanism to shifting the perceived location of the bill closer to all legislators' ideal points is shifting all legislators' ideal points toward the proposal. Ideological changes of heart due to personal experience, moving stories from others, or a general shift in social attitudes are relatively uncommon in politics but not unheard of.

(a) Unconstrained Voting

(b) Ideological Voting



(c) Ideological Voting with Updated Belief about Proposal



(d) Ideological Voting with Positive Update about Proposal Valence

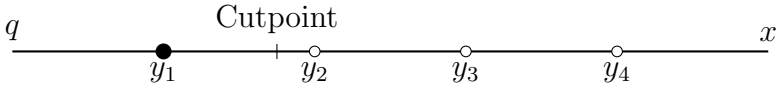


Figure 1: **Persuasion and Ideological Voting**. Solid circles indicate a vote for q; empty circles a vote for x.

added electoral benefit is largest for legislators from competitive districts. Thus while policy information should be influential only for legislators near the ideological threshold, political information should affect moderate legislators and those from competitive districts. Table 1 summarizes the four mechanisms of persuasion highlighted in this spatial model of voting and their empirical implications.

Table 1: **Mechanisms and Patterns of Legislative Persuasion**

Mechanism	Monotone Treatment Response	↑ Ideological Sorting	Openness to Persuasion
Aligning beliefs (ϵ_i)		✓	- Moderates
Bill location (x)	✓		- Moderates
Bill valence (v)	✓		- Moderates
Electoral benefits (κ)	✓		- Moderates - Competitive districts

This decision-making framework highlights how legislative persuasion differs from persuasion in the mass public. First, legislative policymaking requires expertise. Lawmakers' positions are not survey responses of broad issue attitudes or statements of support or opposition to a theoretical bill; their positions reflect both their policy goals and a particular means of achieving them. For legislators, details and expertise matter, and they cannot be expected to be experts on every policy they are asked to evaluate.

Second, legislators' positions face high stakes. Voters are typically free to make decisions in private and without worry that their choice will be pivotal. Legislators' votes, in contrast, are carefully watched and sometimes consequential. A single vote can pass or defeat a policy, or cost a legislator her career. Voters rarely face such drastic consequences.

Third, it follows that legislators are more strategic than voters. For most studies of public opinion, it is plausible to assume that voters report sincere preferences. Politicians, on the other hand, face strong career incentives to be strategic. Their positions may not reflect their sincere preferences in any meaningful sense. More often than not, legislative persuasion will refer to changes in beliefs, attitudes, values and policy positions related to a

calculated, strategic decision-making process.

Fourth, legislators' susceptibility to influence will depend on the strategic environment. Legislators are more informed, sophisticated, and experienced in political matters than voters, so we would expect their personal political attitudes to be more resistant to influence (Hill and Huber 2019; Druckman and Leeper 2012; Baekgaard et al. 2017). But since legislators' position-taking is strategic, they may quickly abandon even deeply-held values for political expediency.

The conditions mediating persuasion fundamentally differ for legislators and voters. For voters, it is their own knowledge, political engagement, and information environment that predicts susceptibility to influence; for politicians, the preferences of other political actors and the costs and rewards for aligning or opposing them. Legislators may be more or less susceptible to persuasion than voters, which leads us to ask: just how persuadable are legislators?

Are Findings of Legislative Persuasion Credible?

Over the past decade, experimental studies have shown that legislators' choices on real policy proposals can be changed.[5] The most striking feature of these experiments is how mutable policy positions appear to be. Information about the content of proposals or the preferences of constituents is estimated to change up to 20 – 30% of legislators' votes. Furthermore, most studies estimate that legislators' positions can be changed; only one published paper presents a null result.

There are reasons not to accept findings from this literature uncritically. Policy experiments with legislators have small sample sizes, since they are typically limited to a single bill or issue at a time, in a single legislature, which may have only 100 members. Small sample sizes yield noisy estimates which, combined with p-screening or publication bias,

[5]Table F1 in the Appendix lists nine such studies, their settings, treatments, dependent variables, and results.

might magnify the apparent effects of interventions, creating what has been called Type-M bias (Gelman and Carlin 2014). We can look for Type-M bias by comparing the estimated magnitudes and p-values of treatment effects.[6] Three papers — Hjort et al. (2021); Grose et al. (2022); Zelizer (2018) — estimate intent-to-treat effects of 4, 12, and 17 percentage points, respectively, on policy adoption, public support, and roll call voting. Despite these large estimated effects, p-values range between 0.05 – 0.10. Four analyses with p-values between 0.05 – 0.01 estimate treatment effects of 5, 12, 20, and 29 percentage points. These are enormous estimated treatment effects. By way of comparison, one of the most influential treatments in the get-out-the-vote literature, informing voters that their turnout will be publicized to their neighbors, generated an estimated 8 percentage point increase in turnout (Gerber, Green, and Larimer 2008).

Only one paper, Zelizer (2018), estimates a p-value below 0.01, and the estimated treatment effect on cosponsorship is a relatively modest 5 percentage points. This analysis features the largest set of independently-randomized observations, with 1,216 positions across 76 legislators and 16 bills. Again, only one study, Camp, Schwam-Baird, and Zelizer (2023), reports null effects, and it estimates effects of lobbying of between -1 – 0 percentage points across four different experiments. The large proportion of substantively large, borderline statistically-significant findings in the literature is consistent with type-M bias: the magnitudes of published estimates may be exaggerated due to small sample sizes, low statistical power, and publication bias.

Re-analyses with Alternative Outcomes

We examine whether the advocacy campaigns in four studies influenced outcomes other than those examined in the original studies. We start by looking at downstream outcomes. Four studies — Bergan (2009), Bergan and Cole (2015), Zelizer (2019), and Butler and Nickerson (2011) — feature bills that were refiled in similar, if not identical, form in sessions after

[6]We do not analyze the distribution of p-values, i.e. via a p-curve, as there are only six analyses with p-values below 0.05 and eight below 0.10 (Simonsohn, Nelson, and Simmons 2014).

the original studies occurred. These are the only four legislative experiments for which we have individual-level treatment assignments and a sufficiently large number of observations to examine alternative outcomes.

Persistence speaks to the credibility of and mechanisms underlying persuasion. If the literature is marked by spurious findings, p-hacking, and publication bias, we would not expect to see evidence of influence on downstream outcomes that were not even included in the original studies. Further, political psychology and behavioral economics study the persistence of treatment effects to differentiate deliberative from behavioral channels of persuasion (Chaiken 1980; Petty and Cacioppo 1986; Fazio and Towles-Schwen 1999; Baden and Lecheler 2012; Chen and Chaiken 1999; Brandon et al. 2017; Crano and Prislin 2006)[7].

Zelizer (2018) estimates the effects of policy research reports provided by a legislative caucus. The policy research briefings described the problem the bill meant to solve and how it intended to do so. They were led by an intern for the Joint Legislative Veterans Caucus, a group of legislators who had served in the military and met regularly to advance veterans' interests. The research made legislators more supportive of the bills: cosponsorship increased by nearly 60% on average when legislators were provided policy research. Veterans affairs is a popular issue among elected officials, so the briefings likely informed legislators that the content of the bill was unobjectionable and the aims of the bill popular.

Seventy-six legislators were provided policy research on four of 16 bills. The four bills were chosen at random for each legislator, so each legislator was assigned to treatment and control conditions on the same proportion of bills. The unit of observation is the legislator-bill dyad.

Of the 16 bills in the Zelizer (2018) experiment, four were sponsored again in nearly identical form in the ensuing assembly (2017-2018), and three more in the assembly after that (2019-2020).[8] Some of the original bills had not passed, so legislators refiled them.

[7]Also called central vs. peripheral routes or System 1 vs. System 2 thinking, generally deliberative persuasion refers to the conscious consideration of information, while behavioral persuasion includes subconscious mechanisms.

[8]Bill descriptions are provided in Appendix Tables A1 and A2.

Others passed but only partially achieved their intended objectives. Sixty-six of the original 76 subjects remained in the legislature in 2017-2018 to take positions on the refiled bills, and 47 remained in 2019-2020.[9] There are 264 observations of legislators' policy positions on the refiled bills in 2017-2018 and 141 in 2019-2020.

Bergan (2009) and Bergan and Cole (2015) estimate the effects of grassroots advocacy on legislators' roll call voting behavior. In Bergan (2009), a coalition of interest groups supporting a smoke-free workplace bill randomized outreach to legislators in the New Hampshire General Court, the lower chamber of the state legislature. Some legislators received form emails from constituents who supported the bill, while other legislators did not. On average, treated legislators received three emails.

The study assigned 143 legislators to treatment or control conditions. 120 of the 143 cast votes on the final passage of the bill, HB 1177, when it came up for a vote in 2006. The bill passed the House (189-156), but failed in the Senate (12-11). Treated legislators were 20 percentage points more likely to support the legislation on votes necessary for passage.

The anti-smoking bill was refiled again the session after the experiment as Senate Bill 42. SB 42 passed the House (224-117) and Senate (17-7) and was signed by the governor. Due to the 2006 election, only 90 of the original 143 members remained in the legislature in 2007, and 80 cast votes on the final passage of SB 42.

In Bergan and Cole (2015), a group supporting an anti-bullying bill placed calls to voters who, if supportive of the bill, were directly patched through to legislators' offices. Legislators were randomly assigned to an uncontacted control condition or three treatment conditions of varying dosages of phone calls.[10] The study examined roll call voting by all 148 members of the Michigan legislature on House Bill 4163, known as the 'Matt Epling Safe School Law'. The bill required all school districts to adopt an anti-bullying policy.

The bill passed the House (88-18) and the Senate (35-2) and was enacted in December

[9]It is implausible that legislators' re-election depended on their treatment or positions for these bills, so we consider attrition as independent of potential outcomes.

[10]Treated legislators were assigned to 22, 33, or 65 calls, though the actual number of delivered calls varied slightly from assignment. No control legislators were called.

2011. Like Bergan (2009), the study estimated a large effect of advocacy on roll call voting; in this case, treated legislators were 12 percentage points more supportive of the bill on the final passage vote.

In 2013, Michigan legislators considered a similar bill, Senate Bill 74, that required schools' anti-bullying policies to include cyber-bullying as well. The bill passed the House (65-45) and Senate (30-7) and was enacted in December 2014. 116 of the original 148 legislators remained in the legislature for the refiled bill, and 115 cast votes on it.

For Bergan and Cole (2015), there is another relevant outcome outside the scope of the original study. In 2011, an identical bill to HB 4163 was sponsored by a senator, and this bill — SB 137 — had progressed far enough to receive a vote in the Senate before it was shelved in favor of the House version. 37 senators voted on SB 137 on November 2, 2011, before they were asked to concur on the house version on November 29. The grassroots outreach had occurred in September, so senators had been treated prior to both votes. Further, the treatment occurred closer to the vote on the senate bill than the house bill.

Butler and Nickerson (2011) differed in several ways from the prior three studies. It provided legislators with another type of information: the results of a survey of constituents' support for budget bills. New Mexico legislators had been called into a special session to consider a capital spending and tax cut bill to use projected windfall profits from natural resources. However, the fall in oil prices preceding the Great Recession eroded the projected surplus. Butler and Nickerson (2011) asked voters in each legislative district whether legislators should 1) fully fund the spending and tax bill; 2) partially fund the bill; or 3) not fund the bill at all.

Treatment consisted of a letter from the researchers to legislators containing constituents' overall support for each course of action. Letters were randomly-assigned across legislators, such that some legislators received letters and others did not. Since the letters were truthful, treated legislators received heterogeneous information reflecting their constituents' actual support for the legislation. Legislators who were informed their constituents opposed the bill

were substantially less likely to vote for it; the estimated treatment effect for this subgroup of legislators is about 30 percentage points.

New Mexico legislators faced a similar fiscal dilemma in 2009 due to the ongoing Great Recession. During the 2009 regular session, legislators passed a General Appropriations Act (House Bill 2) that cut appropriations by 9% and an omnibus capital investment bill (House Bill 154) that substantially cut investments in local projects (New Mexico Legislative Council Service 2009). During a special session called later in 2009 to further address the budget crisis, legislators considered bills to tap reserve funds, cut spending, or issue long-term bonds. These bills, along with those considered during the 2009 regular session, are described in Appendix Table A3. Because no single bill directly matches the experimental bill, we collapse the seven downstream bills into a single index as our main dependent variable for this study.

All four studies estimated substantial treatment effects from the advocacy or information. On the one hand, this is a benefit, if not a necessary precondition, for analyzing the persistence of persuasion. Revisiting interventions that did not influence their intended outcomes would not likely find unintended effects, either. The larger the influence on intended outcomes, the larger the changes in legislators' beliefs or attitudes, and presumably the larger the influence on unintended outcomes.

On the other hand, we might worry that selecting on contemporary effects could bias analysis of downstream outcomes. Any chance imbalance in treatment assignment for contemporary outcomes — if legislators who were more likely to support the bill happened to be assigned to treatment — would necessarily persist to downstream ones (Fowler and Montagnes 2023).[11] Conditioning downstream analyses on the results of contemporary ones will be particularly problematic if bill positions are highly correlated across periods. While we did not set out to sample studies based on a p-value or treatment effect threshold, relying on studies that estimated substantial contemporaneous treatment effects is in some ways

[11]Table A5 in Appendix A shows balance tests for key covariates and finds no significant imbalances.

unavoidable.

Appendix Table A9 shows how conditioning on the contemporary results biases downstream analyses. If researchers were maximally prone to publication bias — such that the original papers estimated effects that were just large enough to publish — downstream analyses would estimate p-values below 0.05 8.9% of the time under the sharp null hypothesis. If they would have published effects that were 50–75% as large as observed, 5.7–7.2% of estimated p-values would fall below 0.05. There is some risk of bias analyzing downstream effects, but it does not appear substantial on average. Estimated downstream effects are larger in magnitude than we would expect due to bias alone. Across studies, bias from conditioning on contemporary results decreases in the study's power and increases in the correlation between contemporary and downstream outcomes. Zelizer (2019), with the largest sample size and low correlation across outcomes, exhibits minimal bias.

Estimated Downstream Treatment Effects

We present estimated downstream treatment effects in Table 2. The table displays contemporaneous and downstream estimated treatment effects using the same specifications as in the original papers.[12] One-sided p-values are indicated as we are interested in whether downstream treatment effects match the direction of contemporaneous effects. The final column in the table indicates an estimate of decay: how much smaller the downstream estimate is than the original estimate. The final row indicates aggregate estimated treatment effects across the studies, weighted by the number of legislators in each study.[13]

The estimated weighted average contemporaneous effect across the original studies was 14.1 percentage points ($\hat{p} < 0.001$). The estimated weighted average downstream effect is

[12]Covariates are included for Bergan (2009) (legislator party and an index of voting on other tobacco bills) and Butler and Nickerson (2011) (legislator party and district Dem Presidential vote share). Zelizer (2018) and Bergan and Cole (2015) do not include covariates in the main analyses. We limit analysis to the final floor vote for Bergan (2009).

[13]The downstream analyses include 37 – 116 legislators. We weight each study by the number of legislators to give more importance to studies with more participants but avoid over-weighting the one study with multiple bills and thus by far the most observations, (Zelizer 2018).

7.2 – 8.6 percentage points, a 40 – 50% decline in magnitude.[14] Even with up to 50% decay, the weighted average effect is unlikely to have arisen by chance, as estimated p-values range from 0.02 – 0.05. Taken together, this evidence shows substantial downstream influence on legislators' position-taking via both cosponsoring and roll call voting long after treatment.

We can also examine the downstream analyses separately by study. Four of the five estimated downstream effects point in the same direction as the contemporaneous estimates; the one exception is Butler and Nickerson (2011), the one study for which we do not have a clear, single downstream bill. The magnitudes of estimated downstream effects are large; they range from 4 – 16 percentage points. Nevertheless, several of the downstream estimates decline substantially in magnitude from the original studies: Bergan (2009) declines by 49% and Butler and Nickerson (2011) by 86 – 114%. Two studies, Zelizer (2018) and Bergan and Cole (2015) estimate downstream effects that decline by less than 10%. These two studies feature the largest sample sizes of both the contemporaneous and downstream analyses; are the two studies for which the simple difference-in-means treatment effect estimate achieves conventional levels of statistical significance; and are the two studies least prone to bias due to conditioning on the contemporary treatment effect estimates.[15]

The estimated effect on a contemporaneous bill excluded from Bergan and Cole (2015) is actually 37% larger than the estimate from the original paper, though attrition is acute due to its consideration in only one chamber of the Michigan legislature. None of the downstream estimates reach conventional levels of statistical significance, as the number of observations is 19 – 74% fewer than in the original studies.

We report several robustness checks in the Appendix, including limiting contemporary analyses to legislators who served in the downstream periods, estimating effects separately for each downstream New Mexico bill, and changing how we weight the studies. Inferences

[14] We report a range, rather than point estimate, to account for the unclear directionality of the Butler and Nickerson (2011) downstream effect.

[15] Bergan (2009) and Butler and Nickerson (2011) both report the simple difference-in-means treatment effect estimate, and in both cases it is slightly smaller, though with larger standard errors, than the covariate-adjusted estimate.

Table 2: **Estimated Downstream Effects (in pp)**

DV: Bill Support	Contemporaneous Bills	Downstream Bills	Decay
		Zelizer (2018)	
Treatment	5.4**	5.0	7.4%
(SE)	(1.9)	(5.8)	
N	1216	405	
		Bergan (2009)	
Treatment	13.8*	7.1	48.6%
(SE)	(7.7)	(8.3)	
N	122	80	
		Bergan and Cole (2015)	
Treatment	12.0*	11.4	5.0%
(SE)	(6.1)	(9.8)	
N	143	115	
		Bergan and Cole (2015): Contemporaneous bill SB 137	
Treatment	12.0*	16.4	-36.7%
(SE)	(6.1)	(16.7)	
N	143	37	
		Butler and Nickerson (2011)(1)	
Treatment	-29.3*	4.2	85.7 – 114.3%
(SE)	(16.6)	(6.2)	
N	67	58	
		Weighted Average	
Treatment	14.1	7.2 – 8.6	39.0 – 48.9%
(SE)	(4.2)	(4.3)	
p	< 0.001	0.023 – 0.047	

Significance indicated at $p < 0.05$ (*) and $p < 0.01$ (**) one-sided.

(1) For Butler and Nickerson (2011), the table displays estimated interaction effect of treatment and low constituent support. Standalone estimated treatment effects are near zero: -1.9 (11.4) percentage points in the original analysis and -0.4 (4.3) percentage points in the reanalysis. We also report bounds for the estimated downstream effects because the directionality of effects is unclear given a lack of single downstream bill for this study.
Coefficients for covariates are reported in Appendix Table A7 for Butler and Nickerson (2011) and Table A12 for Bergan (2009).

do not change.

We cannot say whether it is the same legislators who were convinced to change their positions on both the immediate and downstream bills. Appendix C considers several mechanisms and provides suggestive evidence that legislators who were persuaded downstream represent a subset of those who were persuaded contemporaneously.

In all, we observe meaningful persistence of treatment effects over long periods. There is variation across studies, with less decay for the larger, more powerful studies. We would expect to see lasting effects on position-taking if the interventions changed legislators' beliefs or attitudes about policies, and that is what we observe.

Estimated Crossover Treatment Effects

In addition to downstream effects, we can also examine alternative, contemporaneous outcomes that might have been influenced by these experiments. Were legislators able to use what they learned from advocates about one bill in their consideration of other bills?

One of the studies provides the most direct evidence on this question. Zelizer (2018) included sixteen bills in the study. All addressed the same general issue of veterans affairs, and several addressed closely related policy problems. Two bills relate to establishing in-state residence for children's services (HB 126 and HB 798); two to preferential hiring of veterans or tax credits for hiring veterans (HB 804 and HB 1082); two to permits for hunting or gun licenses (HB 476 and HB 492); and two to in-state tuition for veterans or their children (HB 715 and HB 126). One bill, HB 657, was not included in the experiment, but it is nearly identical in content to HB 1201, which was included.[16] While all bills intended to help the same constituency, veterans, these nine bills were substantially more similar, including in one case identical, to another included bill.

We use these similar bills to estimate crossover effects. If legislators are learning policy-relevant information, we would expect crossover effects to be limited to pairs of closely related

[16] Appendix Table D1 describes the sixteen bills in the experiment plus the similar non-experimental bill.

bills. Learning about the nuts-and-bolts of a veterans bill about educational credit for service would convey little about the technical aspects of a veterans bill on retirement benefits. On the other hand, if treatments changed legislators' deep-seated attitudes or electoral considerations from aiding veterans, we would expect to see more broad-based crossover effects. Alternatively, if legislators were persuaded to support bills because interventions raised their salience, or if the original results are spurious, we might not expect any crossover effects.

We test for pairwise crossover effects by estimating whether cosponsorship of bill b depends on treatment on bill b':

$$Y_{ib} = \beta_0 + \beta_1 d_{ib} + \beta_2 d_{ib'} + u_{ib} \quad \text{for } b \neq b' \tag{4}$$

where Y_{ib} represents cosponsorship by legislator i on bill b; d_{ib} direct treatment of legislator i on bill b; and $d_{ib'}$ treatment of legislator i on bill b'. This model estimates two treatment effects: β_1 is the bill-specific direct effect of treatment on bill b on cosponsorship of bill b; β_2 is the crossover effect of treatment on bill b' on cosponsorship of bill b.[17] With five pairs of closely-related bills, there are nine crossover effects of particular interest among the total 256 crossover effects.[18]

We begin our analysis with a focus on the five closely-related pairs of bills. The estimated average direct effect is large in magnitude (7.9 percentage points) and unlikely to have arisen due to chance ($p < 0.01$).[19] Evidence of crossover effects is more muted. The average estimated crossover effect is 3.2 percentage points, with an estimated standard error of 2.1 percentage points.

We would expect crossover effects to be smaller than direct effects, since the crossover

[17]Both effects are identified because legislators were assigned to treatment for multiple bills. This specification assumes additive separability.

[18]Since HB 657 was not included in the experiment, we can only estimate one crossover effect for this pair of bills.

[19]Interestingly, across all bills, crossover effects do not substantially bias estimation of direct treatment effects. Excluding crossover effects from Equation (1), the average of the 16 direct treatment effect estimates is 4.91 percentage points. Including crossover effects, the average of all 256 direct treatment effect estimates is 4.89 percentage points.

Table 3: **Estimated Crossover Effects from Zelizer (2018) (in pp)**

Bill b	Bill b'	Direct $\widehat{ITT}$ ($\widehat{SE}$)	Crossover $\widehat{ITT}$ ($\widehat{SE}$)	N
HB 126	HB 798	9.3 (8.4)	6.9 (7.1)	76
HB 798	HB 126	−1.2 (1.3)	5.7 (5.7)	76
HB 804	HB 1082	−2.0 (2.0)	−2.0 (2.1)	76
HB 1082	HB 804	10.3 (7.0)	−1.7 (1.5)	76
HB 476	HB 492	6.8 (7.6)	3.0 (7.7)	76
HB 492	HB 476	29.5* (11.7)	10.2 (9.3)	76
HB 715	HB 126	2.3 (6.5)	10.0 (8.2)	76
HB 126	HB 715	8.5 (8.2)	−6.9* (3.5)	76
HB 657	HB 1201	–(1)	3.5 (5.5)	76
Average:		7.9** (2.6)	3.2 (2.1)	608

(1) No direct treatments were administered for bill HB 657

Robust standard errors reported. Significance indicated at p < 0.05 (*) and p < 0.01 (**) two-sided. Dependent variable is bill cosponsorship.

effects are based on information that was provided in the context of another bill, and that is what we observe. On average, these nine crossover effects are smaller in magnitude than direct effects by approximately 60%. Nevertheless, the average estimated crossover effect is a reasonably-sized 3 percentage points, and two-thirds of individual estimates are positive.

We use a randomization test to determine whether the observed crossover is greater than we would expect under the sharp null hypothesis of no crossover effect for any bills. In each of 10,000 simulations, we re-randomized the crossover treatment variable and estimated crossover effects with the permuted treatment assignment vector.[20] Among the simulations, 7.1% yielded average crossover effects larger than 3.2 percentage points, and 9.1% larger in absolute value.

There is clearer evidence that treatments do not spread across bills more broadly. Looking across all 256 pairs of bills, instead of only those pairs that share similar content, the average crossover effect estimate is -0.01 percentage points. 41% of estimates are positive; 53%

[20]In each simulated analysis, we left the direct treatment variable unchanged but re-randomized the crossover treatment variable; since each legislator was assigned to treatment on 4 of 16 bills, we know the exact probability of treatment assignment to crossover conditioning on the observed direct treatment assignment. We then estimate Regression (2) with the simulated data. This procedure is performed separately for each combination of b and b'.

negative; and 6% are zero. For most bills, information about one bill has no impact on support for another.

We utilize another randomization-based test to determine whether the nine estimated crossover effects for substantively similar bills were larger than for all other bill pairs. We examine whether picking five pairs of bills at random, rather than the five pairs we identified due to their content, yields estimated average crossover effects as large as those we observe.[21] Of the simulations, only 4.9% produced average crossover effects as large or larger than 3.2 percentage points, and 10.1% as large or larger in absolute value. Again, it appears unlikely that we would observe the largest crossover effects among the pairs of closely-related bills due to sampling variability alone.

Taken together with evidence that the Bergan and Cole (2015) experiment also influenced a near identical contemporaneous bill, we find that two campaigns by advocacy and affinity groups crossed over to change legislators' positions on related legislation. Although we note this analysis is preliminary and exploratory, we find evidence of crossover effects only among bills with similar content, and not across all bills that help a specific interest group.

Observing persuasion on these alternative outcomes makes us more confident that findings of legislative persuasion are credible and not false positives. In some cases, a meaningful number of legislators are susceptible to persuasion.

This analysis also begins to distinguish mechanisms behind persuasion. Whatever legislators are learning, they are applying to other bills, in some cases a year or more after treatment. These results suggest persuasion is more consistent with deliberation than a more fleeting, subconscious mechanism. And while multiple mechanisms from the spatial model can explain observing persuasion on the same bill refiled downstream, that we observe crossover effects for similar bills — but not generally for bills affecting the same constituency — suggests persuasion is related to bill content. Legislators do not appear to be changing

[21] The procedure included randomly selecting nine bills — under the restriction that HB 657 could only be included as a bill influenced by other bills' treatments, since there was no treatment for the bill — and placing them into five pairs, as one bill, HB 126, appeared in two pairs.

their ideal points or electoral considerations on this issue, such that they then align with other legislators on unrelated bills. It is also less likely they are learning about bill valence, which should be less transferable across bills, than other bill information like the magnitude of policy problems, the efficacy of various solutions, or the ideological content of a proposal. These inferences are preliminary efforts to think about the different ways persuasion may manifest in behavior.

Persuasion and Ideological Voting

To examine how persuasion relates to constrained ideological voting, we plot legislators' votes by their Shor-McCarty ideology scores, separately for those assigned to control and treatment for each study. Figure 2 plots legislators' votes in the Michigan, New Hampshire, and New Mexico studies.[22] Each plot includes a cutting line that maximizes the number of correctly predicted votes along with predicted support from a logistic regression of actual support on legislators' ideology scores.[23]

Holding legislators' ideological ordering fixed, we see that persuasion not only is consistent with, but also can enhance ideological voting. In each state, voting is correlated with ideology in the control group. Liberals supported each bill at higher rates than conservatives. A substantial share of votes are correctly predicted by ideology: 85% in Michigan; 77% in New Hampshire; and 81% in New Mexico. Baseline voting is ideological.

Treatment increased the relationship between ideology and voting. In Michigan and New Mexico, treatment caused legislators to vote more in line with their ideology; the predicted vote curve steepens substantially in both states. The cut lines between support and opposition largely do not change, however. This pattern indicates legislators made fewer voting "errors" in treatment than control; ideological sorting improved, consistent with legislators aligning their beliefs about the content of the proposal. In Michigan, many

[22]These three studies analyze voting, so they are appropriate for comparison to Shor-McCarty scores estimated with data from and a model of roll call voting.

[23]Where multiple cutting lines achieve the same optimal prediction scores, a box connects those lines.

Republican moderates, predicted to vote for the bill by their ideology, opposed it in the control group, but not the treatment group. In New Mexico, conservative Republicans in control were actually more likely than moderate Republicans to side with Democrats; we see a more reasonable monotonic relationship between ideology and bill support in treatment. In both states, treatment substantially increased the percentage of correctly predicted votes, from 85% to 96% in Michigan ($p = 0.017$ via randomization inference) and 81% to 88% in New Mexico ($p = 0.155$).[24]

A different pattern emerges in New Hampshire. First, treatment shifted the cut line from left-of-center to right-of-center. Ideological moderates shifted from opposing the bill, on average, to supporting it. Second, the predicted vote curve slightly weakens, but largely remains the same. Left-wing legislators are predicted to support the bill at 90%+ rates; right-wing 10–20%.

Persuasion campaigns can facilitate ideologically constrained voting. The advocacy campaigns in these three studies, which occurred against a backdrop of pressure from other interest groups, constituents, and elected officials, increased the share of votes correctly predicted by ideology from 81% — 86% on average ($p = 0.081$). A meaningful share of ideological constraint may come down to persuasion. In two of the three studies, we observe information reducing the number of errors and increasing the predictive power of ideology. In the third, we see a significant shift in the cutpoint between support and opposition, suggesting persuasion was primarily influential for legislators in a certain ideological range. While advocacy campaigns could influence votes independently of legislators' ideology scores — a scrambling of coalitions or idiosyncratic switching across the ideological spectrum — that is not what we observe.

The results are consistent with different, and various, mechanisms of persuasion occurring in different interventions. The most intuitive results are perhaps in New Mexico, where the

[24]P-values are estimated by permuting the treatment assignment variable, calculating the percent of positions that can be correctly predicted by ideology in treatment and control groups in each permutation, taking the difference, and comparing the generated distribution to the observed difference in correctly classified votes between treatment and control. A related test is reported in Appendix H.

treatment conveyed electorally-relevant information that varied across legislators' districts. Right-wing legislators predicted to be equally split in their support of the bill in control become predictable opponents in treatment after being informed that their constituents opposed the bill.

The results in Michigan and New Hampshire are apparently more difficult to reconcile. In both cases, legislators were treated with appeals from voters who supported the bills. We might think the operative mechanism would be the same — that legislators learned there was a vocal, supportive constituency in their district for the bill — but patterns of support look different. Specifically, in Michigan the cutpoint between support and opposition does not move, but the predicted support curve steepens, while in New Hampshire the predicted vote curve flattens and the cutpoint shifts.

One solution to this discrepancy is that proposals' prior popularity differed, which created different ideological voting patterns. The cutpoint between coalitions fell among moderates in New Hampshire, but right-wing legislators in Michigan. The Michigan bill was a priori popular, with 82% support in control and an extreme cutpoint indicating that most legislators were predicted to support it. The New Hampshire bill was opposed by a slight majority of 58% of legislators in control, but with strong support from the most left-wing legislators. The spatial model of voting predicts that several mechanisms, including electoral considerations, are most influential for legislators near indifference, i.e. near the ideological cutpoint. In both Michigan and New Hampshire, we see legislators near the cutpoint convinced to support the bill. However, because the cutpoint is extreme in the control group in Michigan, there is only a mass of legislators to the left of the cutpoint who can be, and are, convinced to support the bill. In New Hampshire, legislators to the left of the cutpoint in control are nearly unanimous in their bill support, so it is only legislators to the right of the cutpoint who can be, and are, persuaded. Thus in both cases it is legislators near the cutpoint who are persuaded, but the asymmetry in density of legislators on each side of the cutpoint causes different changes in patterns of persuasion in the two studies.

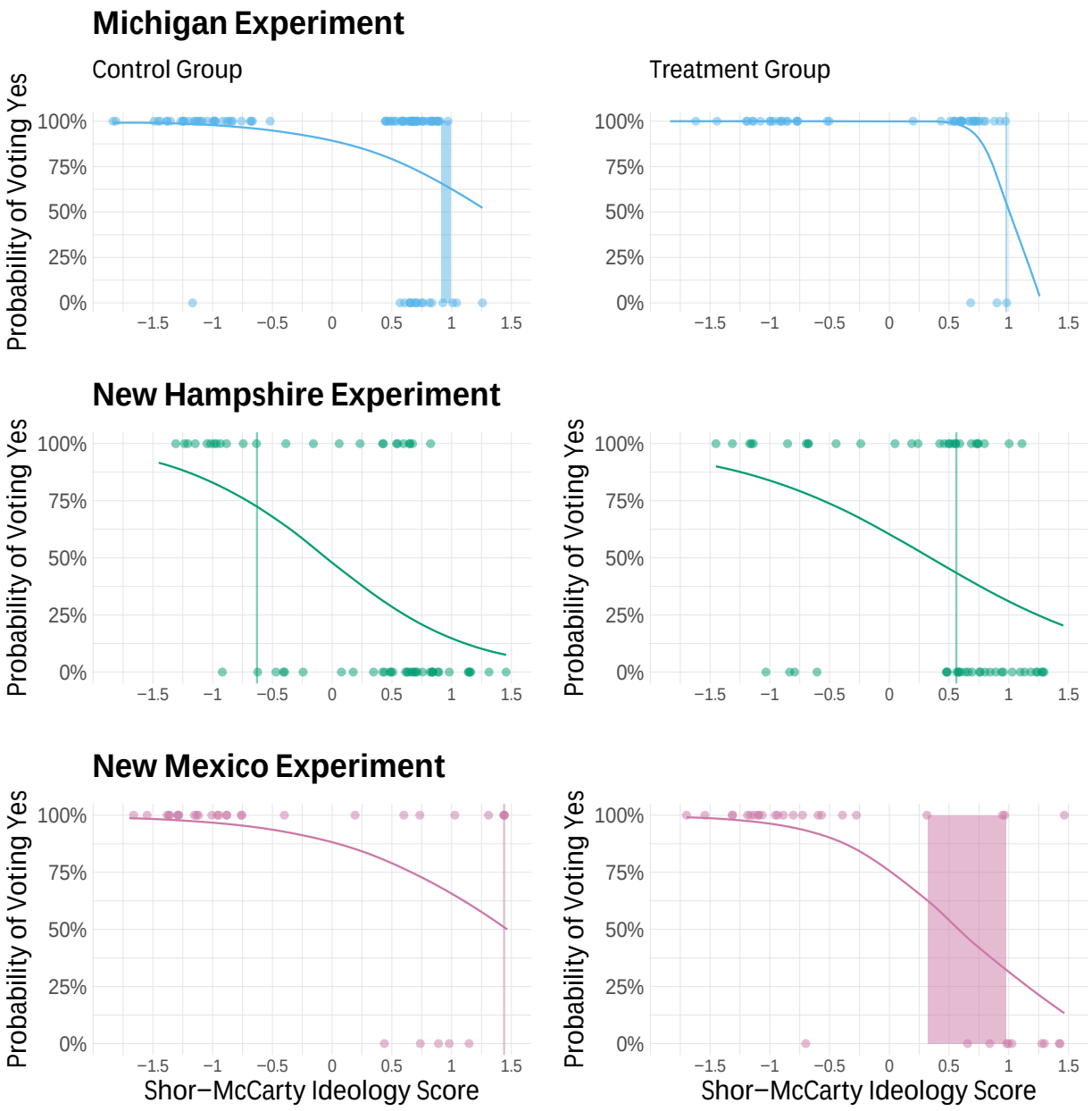


Figure 2: **Roll Call Voting by Legislator Ideology**. The points indicate votes of specific legislators; loess curves average support for the bill at each level of ideology score; and vertical lines (rectangles) the cutpoints that separate support and opposition.

Heterogeneous Persuasion Across Legislators

To systematically examine whether susceptibility to persuasion correlates with ideology, we estimate heterogeneous treatment effects across groups of legislators. Whereas each study is underpowered to estimate heterogeneity on its own, together the three experiments

have 367 legislators from three states.[25] We estimate whether advocacy had heterogeneous impacts across groups of legislators based on their partisanship, ideological extremity, district competitiveness,[26] and tenure in office.

Pooling data from several campaigns has both costs and benefits. The biggest drawback is that different interventions may influence different legislators, such that we are estimating a weighted average of individual effects that may wash out or coincide due to chance, study-specific factors. These specific campaigns may or may not be influential for the same reasons and thus to the same types of people. The risk is that we miss patterns by pooling dissimilar interventions.

At the same time, combining separate campaigns would give us more confidence that any consistent, robust patterns of influence we do observe across legislators are not due to the idiosyncrasies of a particular campaign, bill, or state legislature. The three studies focus on different substantive issue areas. Most importantly, our model of ideological voting suggests that moderates should consistently be most persuadable for contested bills through a variety of mechanisms. Even if one experiment conveyed valence and another changed perceptions of bill ideology, the treatments in both cases would influence ideological moderates the most.

We begin by visualizing heterogeneous persuasion by legislators' ideology. Figure 3 shows estimated effects, and uncertainty, of treatments by legislators' Shor-McCarty ideology scores (Shor 2020).[27] It appears that treatments influenced bill support primarily for ideological moderates. Estimated effects among ideologically-extreme Democrats and Republicans are close to zero, but are about 15 percentage points for moderates.

We also estimate heterogeneous treatment effects with the following weighted-least squares

[25]We exclude Butler and Nickerson (2011) from the main heterogeneity analysis, as legislators received different treatments as a function of their district partisanship; results including it are presented in Appendix C.

[26]To account for New Hampshire's multi-member districts, a comparable measure of district competitiveness was constructed by dividing candidates' votes by the sum of their votes and the votes of the highest placing candidate who did not win a seat in the district. New Hampshire had the most legislators in competitive races (67%) followed by Michigan (32%) and Tennessee (9%).

[27]Effects are estimated from a bayesian regression of bill support on five indicator variables for legislators' ideological quintile and bill by party fixed effects.

regression:

$$Y_{i,p,b,s} = \beta_0 + \beta_1 d_{i,p,b,s} + \beta_2 \text{Group}_{i,p,b,s} + \beta_3 d_{i,p,b,s} \text{Group}_{i,p,b,s} + \mu_{p,b,s} + u_{i,p,b,s} \tag{5}$$

where $Y_{i,p,b,s}$ indicates whether legislator i of party p supported bill b in state s; d indicates treatment; Group indicates that the legislator is a member of the group of interest for that analysis; and $\mu_{p,b,s}$ are party by bill by state fixed effects used to improve the precision of treatment effect estimates. We account for differential probabilities of treatment assignment and the differing number of observations per legislator using inverse probability weights.[28]

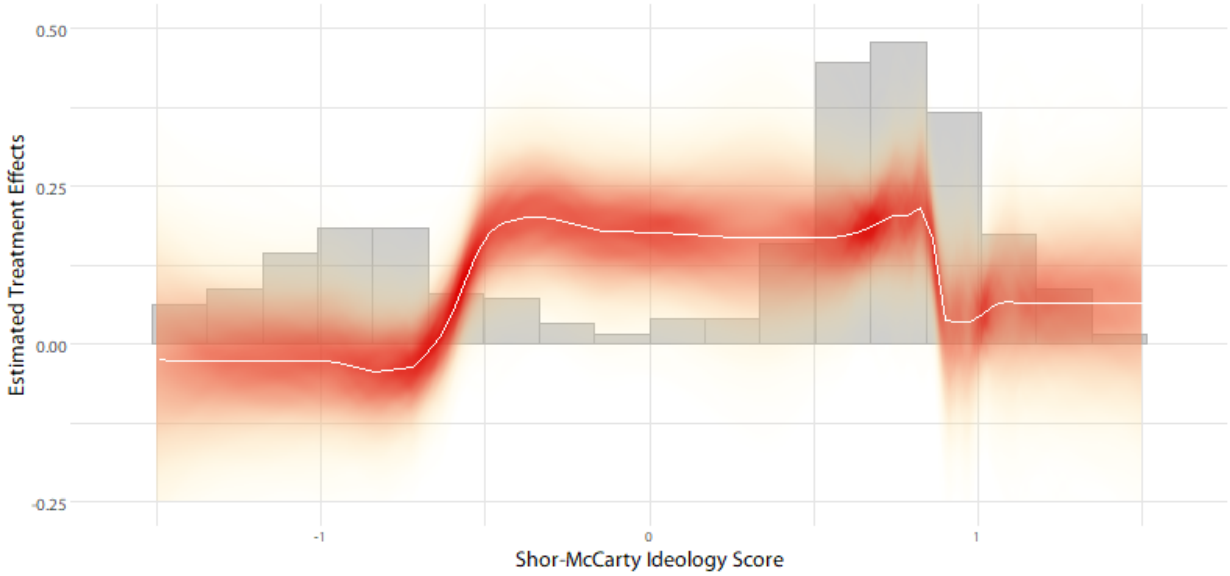


Figure 3: Heterogeneous Effects by Legislator Ideology. The histogram indicates the percentage of legislators in the three studies with an ideology in a given range.

Table 4 displays results. Moderates do appear more susceptible to influence than extremists. Among the 35% of the sample with Shor-McCarty ideology scores in the range $[-0.75, 0.75]$, treatments on average increased bill support by about 20 percentage points; among legislators outside of this range, estimated treatment effects are approximately zero.

[28] Weights are equal to the inverse of each observation's probability of assignment to its realized treatment condition. For the legislators in Zelizer (2018), we further divide each weight by 16 to equalize these legislators' overall weight with legislators from other states.

Treatments are likewise estimated to be larger for legislators facing competitive elections than for their peers from safer seats. Among legislators who received less than 60% of the top-two vote share in the prior election, treatments again increased bill support by about 20 percentage points. Estimated effects among legislators facing less competition were again approximately zero.

We do not observe meaningful heterogeneity by partisanship or tenure. Treatments appeared slightly less persuasive to Democrats than Republicans, though estimated differences across party are consistent with sampling variability. First-term legislators were not meaningfully more susceptible to influence than their longer-serving peers.

Since legislators from competitive districts might also be more ideologically moderate, we would like to distinguish which of these characteristics is associated with larger treatment effects. We estimate a triple-interaction by regressing bill support on treatment, election competitiveness, ideological moderation, and interactions between the three regressors. Results in Appendix Table A13 suggest that influence is larger in both competitive districts and among moderate legislators and does not suggest that it is one or the other of these conditions that observes larger effects.

What should we make of these results? None of the experiments set out to estimate heterogeneity by these dimensions, so this analysis is purely ex post. Although the larger sample sizes gained from aggregating experiments improve power, heterogeneous effects are estimated imprecisely. We should interpret these results with caution.

As predicted by a model of ideological voting, moderates are more open to changing their minds than extremists on these contested bills. Multiple mechanisms, such as legislators' learning policy ideology, valence, or electoral considerations, predict larger effects among moderates. Moderate persuasion may also be driven by non-ideological factors outside of our spatial model. Moderates may have a mix of liberal and conservative attitudes such that advocacy or research can more easily trigger a relevant decision-making consideration. Moderates may appear moderate simply because they are more willing than extremists to

Table 4: **Estimated Heterogeneous Treatment Effects Across Legislators (in pp)**

	Ideology[(a)]	
Treatment	-0.6	0.8
	(6.5)	(4.3)
Treatment*Moderate	21.3*	19.0*
	(9.4)	(7.7)
	Partisanship	
Treatment	11.6	12.8*
	(6.0)	(5.1)
Treatment*Democrat	-7.3	-9.6
	(8.6)	(7.1)
	District Competitiveness	
Treatment	1.7	0.5
	(5.8)	(4.2)
Treatment*Competitive	18.5	23.0**
	(9.9)	(8.1)
	Tenure	
Treatment	7.5	8.7
	(6.3)	(4.8)
Treatment*First-Term	4.2	2.2
	(9.3)	(7.7)
N	1,452	1,452
Fixed Effects?	No	Yes

Significance indicated at $p < 0.05$ (*) and $p < 0.01$ (**) two-sided. Robust standard-errors reported in parentheses.

(a) Appendix Table C1 tests the robustness of results to alternative specifications of ideological moderation. Two New Hampshire legislators without ideology scores are excluded from analysis.

listen to outsiders and change their positions. Or willingness to change one's mind might be an independent personal characteristic that happens to correlate with legislators' ideology.

Heterogeneous Persuasion Across Legislators and Treatments

The studies varied in whether they provided policy or political information. Zelizer (2018) randomized policy research, whereas Bergan (2009); Bergan and Cole (2015) assigned contacts from constituents. If this information enters legislators' decision calculus differently, either by changing perceptions of the bill or of the electoral consequences of supporting the legislation, we might see different patterns of heterogeneous effects. Specifically, policy information should be more influential for legislators near indifference on the bill's merits, which in many cases will be moderate legislators, whereas political information will also influence legislators in competitive districts regardless of their ideological orientation.

Table 5: **Estimated Heterogeneous Treatment Effects Across Legislators and Studies (in pp)**

Information Type:	Zelizer (2018) Policy	Bergan (2009) Political	Bergan and Cole (2015) Political
		Ideology	
Treatment	2.6	-5.8	4.8
	(2.0)	(10.4)	(6.9)
Treatment*Moderate	8.7	30.6	13.0
	(4.4)	(16.3)	(10.5)
N	1187	120	143
		District Competitiveness	
Treatment	5.7**	-21.6	5.5
	(1.9)	(14.1)	(6.4)
Treatment*Competitive	-5.7	46.5**	18.6
	(6.2)	(17.4)	(10.1)
N	1187	122	143

Significance indicated at $p < 0.05$ (*) and $p < 0.01$ (**) two-sided. Robust standard-errors reported in parentheses. Base terms for ideology and district competitiveness, along with bill by party fixed effects, excluded from display.

Table 5 displays heterogeneous treatment effects by ideological moderation and district

competitiveness. Although these analyses are less powerful than the aggregate analyses due to the decline in sample size, the patterns of estimated treatment effects are consistent with predictions from the spatial voting model. Policy information affects ideological moderates more than extremists, but has no estimated effect on legislators in competitive districts. Perhaps the only legislators with room to maneuver and respond to policy expertise are those in safe seats. Political information has substantially larger effects both on legislators with moderate ideologies and those from competitive districts. Although imprecisely estimated, effects are consistent with policy and political information entering legislators' decision-calculus differently and affecting different subsets of legislators.

These analyses are exploratory and suggestive. We do not make strong claims based on them due to power limitations and the ex post nature of the analyses. They do, however, suggest ways of differentiating the impact of various types of information by means of heterogeneous effects across legislators and treatments. Pressure groups may want to adopt a mix of persuasive strategies for different legislators.

Persuasion In Legislators' Own Words

To further examine mechanisms behind persuasion, we interviewed legislators who participated in one of the studies to distinguish which mechanisms legislators themselves find most plausible. We collected contact information for state legislators who were in office at the time of the antibullying vote discussed in Bergan and Cole (2015). Since Michigan has term limits, most of the 148 legislators in office in 2011 had left the legislature. Ten legislators had moved from the House to Senate; one was the sitting governor; one a member of Congress; and many had been elected to local offices. Four were found to be deceased.

We were able to locate email addresses for 45 of the remaining legislators. If an email address was not available, we attempted to contact the former legislator via social media. Among legislators with no email addresses available, we located social media accounts for another 58 legislators. We thus obtained contact information for 103 members, or 72 percent

of the surviving legislators.

Interviews were scheduled on Zoom and lasted 15 minutes. Legislators were first asked about the antibullying bill: they were reminded of its title and purpose, asked whether they support or oppose it, and then prompted to recall whether they received communications that changed their mind about the issue. The conversation then turned to more general instances of advocacy and issue change. The interview protocol and recruitment materials are available in Appendix E.

We successfully interviewed 12 legislators in the fall of 2022, or 8% of surviving legislators and 12% of legislators with contact information, a response rate higher than recent survey work with policymakers.[29] Key themes of the interviews are summarized below.

Attitudes about Anti-Bullying Legislation

The first set of questions concerned the antibullying bill. Interviews occurred over 10 years after legislators voted to pass the bill, and legislators' recollections of it were somewhat hazy.

All 12 of the legislators we spoke to now support the anti-bullying bill. Although all 12 support the bill today, two of them voted against it at the time, and five voted against the downstream cyberbullying bill considered in 2013. One explanation for their changing views is that their underlying policy attitudes shifted along with society's attitudes towards bullying. Another possibility is that legislators recounted their sincere policy opinion in the interview, but voted strategically when they were in office.

None of the legislators recalled the specific constituent outreach randomized by the researchers. Legislators did recall conversations with other elected officials, constituents, and interest groups about the issue. Legislators 4 and 5 recalled communications from then Governor Rick Snyder, who publicly supported the bill with his account of being bullied as a child. Others recalled speaking to colleagues and constituents who described the difficulty of monitoring their kids' online behaviors and the troubles their children were having with

[29]Similar efforts by Anderson, Butler, and Harbridge (2016) (5%), Anderson, DeLeo, and Taylor (2020) (3%), and Niederdeppe, Roh, and Dreisbach (2016) (7%) have attained slightly lower response rates.

bullying. Legislator 5 recalled communications from interest groups or community stakeholders: "I absolutely remember getting calls into my office. I remember [name removed], calling me... he had a Christian conservative organization that was very passionate about this issue." The same legislator remembered also hearing from "the lobbyist for the LGBT organization.... They were equally passionate about inclusion." Two legislators specifically mentioned they did not receive communications on the issue. While a majority of the legislators we spoke to recalled some conversation with stakeholders about the antibullying bill, none reported that those communications specifically changed their vote.

Thoughts on Legislative Persuasion

The interview then turned to the influence of constituent and interest group communications more broadly. They reported that communications did change legislators' votes; that personal appeals and stories were most convincing; and that persuasion was most likely on issues that did not map obviously to their ideology, of which there were many.

Several legislators stated unambiguously that they changed positions in response to feedback. Legislator 3 said "there was a bill that I was supporting, and I got feedback from a constituent that changed my mind. That did happen. I went the other way," while Legislator 5 remarked "I wouldn't say it was often that I was open to just being swayed. But there were certainly circumstances, and [I could be swayed] if it was done in a respectful manner and I could hear a real story." Legislator 12 stated "And so there have been times, yeah, where I've been swayed. I'd even pull off the policy that I was initially supporting because I got more information, and, you know, was moved in the other direction, or recognize at least, this policy needs more work before I get behind it and champion it."

Legislators reported that personal stories, from those experiencing hardship or with expertise in an issue, influenced them. Legislator 5 said "On that issue I remember changing my position multiple times before the floor vote... I think I ended up voting to save the Promise Scholarship and against my... caucus. And it was directly because of how many,

what I felt were genuine calls of families." Legislator 1 recalled, "there was a little kid... he couldn't have been more than seven years old. And [the bill] was about funding for type one diabetes... that interaction really gave me pause," and Legislator 4 said "the state senator from my district and me teamed up with the mother of the daughter, of the sixteen year old daughter that died, and got a law passed.... And so that really sticks out because we were with the parent group all the time lobbying it."

Others pointed to interest groups and professionals. Legislator 1 stated "when I was on [the] education [committee], a lot of what I did was just rely on the teachers and what they thought because they were the professionals" and "when it came to agriculture issues... I listened to what the farmers had to say." Legislator 5 said "we had an issue before us ... Would physical therapists be able to write maybe prescriptions. Or could you go directly to a physical therapist to get treatment on your back or do you have to get a referral from your doctor?... I had a physical therapist in my district come meet with me at Starbucks. And he made just a really compelling case why it shouldn't have to go through a doctor first. And then he also came to the committee hearing where we were taken aback. That absolutely had a major impact on me."

Other legislators suggested their positions were less likely to change by advocacy even if they learned something. Legislator 8 said "I feel like there were often instances where I was not necessarily swayed. But the more I learned, the more I kind of understood." Legislator 5 starkly explained "I certainly would agree it would inform my position, but as far as change my position..." Legislator 9 stated, "you get kind of hard-headed. And I'm no exception to that. I would say, [communications have] not necessarily changed a point of view, but sometimes you realize certain things are [important] to other people that you might not have thought was [important] yourself."

While legislators described their own experience as one of learning, they attributed to some of their peers less virtuous motives such as pandering or a lack of backbone. For example, Legislator 5 said, "I had some colleagues, oh, my gosh, any kind of pressure is

going to really throw them haywire. And then I had other colleagues that were, you know, rock solid ideological about everything." These second hand reports of others' experiences are less informed and may reflect our interviewees' personal opinions more than facts about their peers. However, legislators may also more accurately perceive the capriciousness of position-taking when on display by others rather than themselves.

On what types of issues were legislators persuaded? Many legislators said that their positions were influenced on obscure issues about which they knew little. They spoke about swing beds for acute care in nursing homes and veterans affairs (Legislator 10), and auto insurance and subprime payday loans (Legislator 12). Sometimes legislators were uninformed about issues not because they were arcane or low salience, but because they were complex. One such example was the Detroit school bankruptcy (Legislator 6).

Some legislators referred specifically to constraints on persuasion. Legislator 10 remarked that ideological convictions would limit the influence of communication: "there's some issues that are, you know, hard and fast in my way of thinking that... you don't change your mind on those things, regardless of... the number of constituent contacts you get on that issue." Legislator 5 explained that constituents would only be able to change their mind on "issues where I didn't have a predisposed philosophical position, right? Like I was a conservative, I'm just against tax increases, right? If someone from my local school district is going to come to Lansing, and tell me, we need to raise taxes, that's going to be a tall order for them. But on these issues, where I didn't have a predisposed, ideological, or just personal position on, which was a majority of the things we voted on [I could be swayed].... an issue like the physical therapy or on the bullying, yeah, [communications] could certainly make a difference."

Although we did not ask about the influence of parties in the interview, some legislators mentioned that the party could influence voting and limit the impact of other communications. Legislator 9 said "there were times when I voted for things... because I was kind of a good caucus member. I didn't necessarily agree." Legislator 5 explained that "what I think

people from the outside have to realize is, I was, and all of my caucus mates, you're under tremendous pressure from the Speaker of the House. You can lose committee assignments, you might not get your bills through committee if you cross them. So you're weighing that against, you know, one constituent in your district."

Finally, we asked legislators about recommendations for groups to effectively communicate with their legislator, and their agreement on guidance was striking. Many recommendations could be characterized as helping legislators learn about issues. They emphasized respectful, personal, factual communication that includes information about how an issue affects the advocate over emotional appeals and form letters.

The interviews provide qualitative evidence for many mechanisms behind persuasion. Legislators describe stories of children with diabetes, families investing for college, and of teenagers dying in road accidents not as invoking ideological or electoral considerations, but a separate, valence aspect of legislation. Legislators implied some of their peers were highly susceptible to pressure due to electoral considerations. They did not explicitly mention learning the ideological component of bills, but they did note that persuasion was more likely for low salience or complex legislation, which may well relate to difficulty connecting the content of bills to their underlying ideology. Legislators clearly mentioned that ideological predispositions posed a constraint to persuasion.

Legislators' emphasis on imperfect information and learning suggests how persuasion drives ideological sorting. For some bills, legislators had not considered them through their standard ideological or partisan frames, or had not considered them much at all. Contact from interest groups, lobbyists, and constituents draws legislators' attention, clarifies the content and impacts of legislation, and expands the scope of ideological and partisan thinking to these bills.

We note the limitations of relying on legislators' self-reported beliefs about persuasion. While self-reported measures of political activity are often biased, self-reported measures of the efficacy of a persuasive message are even less reliable (Vavreck 2007). Politicians may

pander or exhibit social desirability bias by overstating their receptiveness to constituents or interest groups; that most of our subjects had left the legislature diminishes this concern. And the small sample size and low response rate may raise questions about the generalizability of the results.

Conclusion

We re-visit four studies to explore the credibility, scope, and importance of efforts to change legislators' votes. We find consistent patterns of influence across legislators; on downstream bills; and on related, contemporaneous legislation. Persuasion is consistent with a process of learning about policy details or electoral considerations rather than deep-seated opinion change, and is consistent with, and even a cause of, increasingly ideologically-constrained voting.

Taken together with findings that voters' choices are minimally influenced by political campaigns (Kalla and Broockman 2018; Coppock, Hill, and Vavreck 2020), these legislative experiments turn the argument of Converse (1964, p. 6, 45) upside down. Converse used the high correlations among legislators' votes to demonstrate a lack of ideological constraint among voters, but it appears legislators' votes are more mutable than voters', and persuasion is one mechanism that allows legislators to take constrained, ideologically consistent positions (see also Glazer and Grofman 1989).

After a decade of legislative experiments, we can now speak with some generality about their contribution to the study of legislative decision-making. Legislators are receptive to appeals to the point of changing their cosponsorship or voting decisions. Experimental results complement qualitative studies that show a meaningful share of legislators' decisions are influenced by the legislative process (Kingdon 1989; Matthews and Stimson 1975; Evans 2018). Normal decision-making is characterized by persuasion.

This study takes seriously the challenges of experiments with elites. Small sample sizes limit statistical power such that findings from legislative experiments may not be as robust

or conclusive as those from other fields. In an ideal world, we would illustrate persuasion by conducting a new experiment in several legislatures, with large samples, multiple treatments, and varied outcomes. Since that approach is not practicable, we adopt the next best strategy: approximating that experiment by aggregating four previously fielded interventions. We do not claim that the evidence says each experiment robustly changed positions by means of influencing legislators' mental states; but, on average across them, we see evidence consistent with these outcomes.

Our findings may be limited in important ways. Most significantly, we analyze experiments in state legislatures on relatively low-salience, less-partisan policy issues. On the highest-profile bills, legislators might have more considered positions that are immune to pressure. Members of Congress may be shielded from public pressure and advocacy by staff or incumbency and constrained by partisanship.

Nevertheless, there are reasons to expect meaningful persuasion even in a polarized, professional legislature. While members of Congress may be tougher to persuade, they likely experience more organized and sustained pressure than the state legislators in our experiments. While many issues are mired in partisan gridlock, Congress has long passed substantial legislation with bipartisan majorities, and it continues to do so (Erikson, MacKuen, and Stimson 2002; Curry and Lee 2020). Legislating in the so-called "secret Congress" may depend just as much on persuasion as we observe in our studies. Ultimately, differences in persuasion across legislatures or over time are interesting questions to be addressed with additional research.

Understanding how legislators are persuaded to take specific policy positions is central to many normative concerns about contemporary democracy, like increasing polarization. Are legislators convinced by lobbyists to adopt the positions of moneyed interests? By their elite social circles to oppose fiscal policy that would reduce economic inequality? By party leaders to take consistently partisan positions? All of these questions relate to persuasion.

This paper proposes an apparent paradox — recent field experimental work demonstrates

legislative persuasion that seemingly conflicts with abundant evidence of policymakers' ideological voting. Our reanalysis of four recent works bolsters recent findings, confirming that persuasive effects are credible in the aggregate. We have provided one explanation why the paradox is only apparent — persuasion can in fact facilitate ideological constraint in legislatures. We hope that future work will further explore the relationship between persuasive communication and policymaker ideology.

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Online Appendix for "Persuasion and Ideological Voting in Legislatures"

Supplemental Information is intended for online publication only

Contents

Appendix A. Supplementary Results from Persistence Analysis

Table A1: ***Description of refiled bills from Zelizer (2018): 2017-2018***

Original Bill	**New Bill**
HB715: develop uniform methods to assess and maximize academic credit awarded by public institutions of higher education to veterans and military service members for military experience, education, and training obtained during military service	HB433: develop and implement uniform procedures for awarding academic credit applicable toward a degree or credential for military education, training, experience, and occupational specialties in the form of course credit equivalencies, and that these institutions readily provide these course equivalencies to veterans and service members as they transition from military service to higher education
HB828: specifies that no fee may be charged for the interment of an eligible veteran in a state veterans' cemetery and limits the fee to $610 for the interment of an eligible veteran's spouse	HB1308: lowers from $610 to $300 the maximum fee for the interment of an eligible veteran's spouse
HB804: authorizes private employers to establish a preference in employment policies for hiring certain veterans, spouses of veterans, widows of veterans, and widowers of veterans	HB165: authorizes private employers to give hiring preference to honorably discharged veterans, spouses of veterans with service-connected disabilities, unremarried widows or widowers of veterans who died of service-connected disabilities, and unremarried widows or widowers of members of the military who died in the line of duty
HB53: exempts from sales tax, registration fee, and motor vehicle privilege tax, any motor vehicle sold to a veteran or service member who has a service-connected disability and who is eligible for a United States department of veterans affairs automobile grant under the Disabled Veterans' and Servicemen's Automobile Assistance Act of 1970	HB15: creates exemptions from sales tax, registration fee, and motor vehicle privilege tax, for any motor vehicle sold to a veteran or service member who has a service-connected disability and who is eligible for a United States department of veterans affairs automobile grant under the Disabled Veterans' and Servicemen's Automobile Assistance Act of 1970

Table A2: ***Description of refiled bills from Zelizer (2018): 2019-2020***

Original Bill	**New Bill**
HB828: specifies that no fee may be charged for the interment of an eligible veteran in a state veterans' cemetery and limits the fee to $610 for the interment of an eligible veteran's spouse	HB1187: eliminates the fee for interment of an eligible veteran's most recent spouse in a state veterans' cemetery
HB1201: corrects a reference to the federal Uniformed Services Employment and Reemployment Rights Act in the statute governing retirement credit for military service	HB1979: allows members who served in the armed forces during certain periods of armed conflict to establish retirement credit for the military service under certain conditions.
HB183: creates the "veterans traumatic brain injury treatment and recovery fund" in order to provide hyperbaric oxygen treatment (HBOT) to veterans who suffer traumatic brain injury (TBI)	HB2405: authorizes certain medical professionals to prescribe hyperbaric oxygen therapy treatment for veterans with traumatic brain injury or post-traumatic stress disorder

Table A3: ***Description of refiled bills from Butler and Nickerson (2011)***

Bill	**Description**	**Outcome**
HB 2	Appropriations cuts, transfers to general fund.	Enacted
HB 154	Capital investment cuts.	Enacted
HB 3 (S)	Transfers balances from various state funds to general fund.	
HB 6 (S)	Transfers balances in operating reserve and tax stabilization reserve to general fund.	Enacted
HB 17 (S)	Reduce approriations.	Enacted
HB 27 (S)	Revert appropriations and capital investments.	Passed House, not Senate.
HB 28 (S)	Issue bonds, transfer proceeds to general fund.	Passed House, not Senate.

(S) indicates special session bill.

Unlike the other three experiments, there is no single downstream bill for Butler and Nickerson (2011) that neatly matched the experimental bill. As a result, we might not expect the treatment to have influenced votes other than the one targeted. Nevertheless, there are reasons why the treatment may have persisted and influenced legislators' subsequent voting behavior. Legislators appeared to learn about the fiscal preferences of their constituents. Some learned so much that nearly one in three legislators changed their votes. Such knowledge might plausibly be transferable to other budget bills.

A second reason to think the treatment would last is that its effects were compatible with and potentially reinforced by enduring partisan attachments. Table A4 reproduces the main analysis from Butler and Nickerson (2011) in Column (1). It shows that treatment was on its own not very influential, decreasing support for SB 24 by 2 percentage points. However, the treatment was highly influential for legislators whose constituents least supported the bill; those legislators whose districts fell below the median ("Low Spending") and were treated were 30 percentage points less likely to vote for the bill.

Column (2) in Table A4 examines results in a slightly different way (see Butler and

Dynes (2016) for a similar analysis). SB 24 was a bill proposed by the state's Democratic Governor and supported mostly by Democrats in the public and in the legislature (Butler and Nickerson 2011, p. 62). Democratic legislators supported the bill nearly unanimously, while Republicans were evenly split. Looking at treatment effects by the party of the legislator, rather than the district support for the bill, shows that treatments were primarily effective at turning Republicans against the bill. Putting Columns (1) and (2) together, it was Republican districts where support for the Governor's bill was low, so it was Republicans who were convinced not to vote for a tax bill that they otherwise were inclined to support. It may have been that legislators learned not about constituents' preferences over fiscal responsibility, but instead their supporters' interest in supporting policies proposed by the opposition. All of the bills under consideration in 2009 were proposed by (and passed with the votes of) New Mexico Democrats.[30]

Table A4: **Estimated Contemporary Effects from Butler and Nickerson (2011) (in pp)**

	(1)	(2)
Treatment	-1.9	-4.0
($\widehat{\text{SE}}$)	(11.4)	(10.5)
Low Spending	20.5*	7.4
	12.2	(8.7)
Treatment x Low Spending	-29.3*	-
	(16.6)	-
Republican	-42.0**	-30.3**
	(12.2)	(14.8)
Dem Vote %	-45.1	-38.6
	(40.4)	(40.2)
Treatment x Republican	-	-30.3*
	-	(16.8)
N	67	67

Significance indicated at $p < 0.1$ (*) and $p < 0.05$ (**) two-sided.

Thus the reanalysis of Butler and Nickerson (2011) differs from the prior three in that

[30]See Appendix Table A7.

we are ex ante unsure about the direction of downstream treatment effects. It is not clear whether opponents of the 2009 bills thought they were too fiscally conservative or not conservative enough. From a partisan perspective, we would expect treatment to have universally lowered support, since all of the 2009 bills were Democrat-supported. If the 2008 treatment convinced Republicans not to support Democratic bills, they should also not support the 2009 bills. Since there is not one refiled bill that exactly matches the 2008 bill, but instead several budget-balancing bills in 2009, we average seven downstream bills into a single index as our main dependent variable for the Butler and Nickerson re-analysis.

Table A5: **Balance Tests for Downstream Analyses**

Covariate	(1) Control Mean	(2) Treatment Mean	Difference (SE)
		Bergan (2009)	
% Democrat	32.4	27.9	-4.5 (10.4)
# Votes for 3 Tobacco Bills	2.0	1.7	-0.3 (0.3)
		Bergan and Cole (2015)	
% Democrat	38.8	31.4	-7.3 (9.8)
Chamber (% in House)	67.5	71.4	3.9 (9.5)
		Butler and Nickerson (2011)	
% Democrat	68.0	61.3	-6.7 (13.1)
% Two party Repub. Pres. Vote Share	48.2	51.3	3.1 (4.2)
% Low support districts	44.0	51.6	7.6 (13.6)

Significance indicated at $p < 0.05$ (*) and $p < 0.01$ (**) one-sided.

Zelizer (2018) is omitted due to block random assignment within legislator, which ensures balance across legislators.

Table A6: **Estimated Downstream Effects from Zelizer (2018) with Bill FEs (in pp)**

$\widehat{\text{ITT}}$	4.0
$(\widehat{\text{SE}})$	(5.2)
N	405

Significance indicated at $p < 0.05$ (*) and $p < 0.01$ (**) one-sided.

Table A7: **Estimated Bill-Specific Downstream Effects from Butler and Nickerson (2011) (in pp)**

	HB 2	HB 154	HB 3	HB 6	HB 17	HB 27	HB 28	Index
Treatment	0.7	3.4	1.6	0.5	3.5	1.2	-11.8	-0.4
$(\widehat{\text{SE}})$	(4.9)	(8.2)	(6.8)	(5.1)	(9.9)	(7.3)	(9.8)	(4.3)
Low Spending	0.3	-5.7	0.4	0.0	8.8	-8.2	-15.2	-3.0
	(5.2)	(8.4)	(7.3)	(5.3)	(10.3)	(7.5)	(10.2)	(4.5)
Treatment x Low Spending	5.0	-14.4	11.3	5.1	-3.7	12.4	11.0	4.2
	(7.1)	(11.7)	(9.7)	(7.2)	(14.2)	(10.5)	(14.1)	(6.2)
Republican	-93.5**	-69.5**	-82.1**	-93.3**	-72.9**	-91.5**	-79.3**	-83.6**
	(5.4)	(9.0)	(7.5)	(5.5)	(11.2)	(7.9)	(11.1)	(4.7)
Dem Vote %	-13.0	-66.8**	-41.9*	-12.9	-61.4*	-5.4	-23.3	-31.0**
	(16.9)	(27.4)	(22.9)	(17.1)	(34.6)	(25.0)	(34.4)	(14.8)

Significance indicated at $p < 0.1$ (*) and $p < 0.05$ (**) two-sided.

Table A8: **Estimated Contemporary and Downstream Effects with Comparable Samples (in pp)**

	(1) Contemporary Analysis	(2) Contemporary Analysis with Restricted Sample	(3) Downstream Analysis
		Zelizer (2018)	
$\widehat{\text{ITT}}$	5.4**	3.5	5.0
$(\widehat{\text{SE}})$	(1.9)	(2.7)	(5.8)
N	1216	339	405
		Bergan (2009)	
$\widehat{\text{ITT}}$	13.8*	3.9	7.1
	(7.7)	(9.0)	(8.3)
N	122	76	80
		Bergan and Cole (2015)	
$\widehat{\text{ITT}}$	12.0	9.2	11.4
	(6.1)	(7.2)	(9.8)
N	143	114	115
		Bergan and Cole (2015): Contemporary bill SB137	
$\widehat{\text{ITT}}$	12.0	8.0	16.4
	(6.1)	(8.4)	(16.7)
N	143	36	37
		Butler and Nickerson (2011)	
$\widehat{\text{ITT}}^{(a)}$	-29.3	-19.6	4.2
	(16.6)	(17.5)	(6.2)
N	67	56	58

Significance indicated at $p < 0.05$ (*) and $p < 0.01$ (**) one-sided.

(a) Table displays estimated interaction effect of treatment and low constituent support. Standalone estimated treatment effects are near zero: -1.9 (11.4) percentage points in the original analysis and -0.4 (4.3) percentage points in the reanalysis.

Table A9: **Downstream Effects Conditioning on Contemporary Effects**

	Zelizer (2018)	Bergan (2009)	Bergan and Cole (2015)	Bergan and Cole (2015) SB 137	Average
Contemporary $\widehat{\text{ITT}}$	5.4	13.8	12.0	–	10.4
Downstream $\widehat{\text{ITT}}$	5.0	7.1	11.4	16.4	10.0
Publication Bias at 100% of Observed Contemporary Estimate					
Mean Sim $\widehat{\text{ITT}}$	0.5	6.3	9.7	3.2	4.9
% Sim $\widehat{\text{ITT}} \geq$ observed	23.1	46.2	39.9	17.8	15.0
% $\widehat{\text{p}} \leq 0.05$	5.1	9.1	16.7	4.7	8.9
Publication Bias at 75% of Observed Contemporary Estimate					
Mean Sim $\widehat{\text{ITT}}$	0.5	4.7	8.4	2.0	3.9
% Sim $\widehat{\text{ITT}} \geq$ observed	21.5	38.5	34.6	17.3	11.1
% $\widehat{\text{p}} \leq 0.05$	4.9	6.5	13.0	4.2	7.2
Publication Bias at 50% of Observed Contemporary Estimate					
Mean Sim $\widehat{\text{ITT}}$	0.2	3.7	6.6	0.7	2.8
% Sim $\widehat{\text{ITT}} \geq$ observed	20.4	33.1	27.5	14.2	7.7
% $\widehat{\text{p}} \leq 0.05$	4.3	4.9	9.4	4.3	5.7

Results from 10,000 simulations.

Table A10: **Average Estimated Contemporary and Downstream Effects (in pp)**

	Contemporary Effects	Downstream Effects	Decay
	Precision Weighted Average		
$\widehat{\text{ITT}}$	6.7	3.8-6.4	3.9 – 42.9%
$(\widehat{\text{SE}})$	(1.8)	(3.4)	
$\widehat{\text{p}}$	< 0.001	0.032 – 0.135	
	Equally-Weighted Average		
$\widehat{\text{ITT}}$	15.1	7.1-8.8	41.7 – 52.8%
$(\widehat{\text{SE}})$	(4.8)	(4.5)	
$\widehat{\text{p}}$	< 0.001	0.026 – 0.058	

Table A11: **Patterns of Estimated Treatment Effect Over Time, Alternative Weightings**

	Support Initial / Oppose Refiled	Oppose Initial / Support Refiled	Support Initial / Support Refiled
	Precision Weighted Average		
$\widehat{\text{ITT}}$	1.7	1.4	4.5
$(\widehat{\text{SE}})$	(3.0)	(4.4)	(3.0)
$\widehat{\text{p}}$	0.29	0.38	0.07
	Equally-Weighted Average		
$\widehat{\text{ITT}}$	6.0	-1.3	5.9
$(\widehat{\text{SE}})$	(7.0)	(6.2)	(4.2)
$\widehat{\text{p}}$	0.20	0.59	0.08

Table A12: **Estimated Covariate Coefficients from Bergan (2009) (in pp)**

	(1) Contemporaneous Bill	(2) Downstream Bill
Democrat	9.2	53.5
	(10.1)	(10.8)
Tobacco Votes	21.7	12.6
	(3.9)	(4.3)

Table A13: **Estimated Heterogeneous Treatment among Moderates and in Competitive Districts (in pp)**

Treatment	-3.1	-2.8
	(7.4)	(4.4)
Treatment*Moderate	13.6	9.2
	(11.8)	(9.1)
Treatment*Competitive	7.5	9.9
	(14.7)	(10.0)
Treatment*Moderate*Competitive	14.2	18.3
	(20.1)	(16.0)
N	1,450	1,450
Fixed Effects?	No	Yes

Significance indicated at $p < 0.05$ (*) and $p < 0.01$ (**) two-sided.
Robust standard-errors reported.

Appendix B. Patterns of Estimated Treatment Effect Over Time: Further Discussion

The main persistence analysis shows that behavioral persuasion persists in the aggregate. That could occur for two reasons. First, treated legislators who initially changed their positions may have stuck to their newly-adopted positions when asked to reconsider the refiled bills. Second, different legislators may have been influenced both contemporaneously and downstream. A version of the sleeper effect (Kumkale and Albarracín 2004), whereby a message becomes more convincing over time, might have led some legislators who originally opposed bills to support them later. Or downstream results could simply arise due to sampling variability.

We examine whether the same legislators appear to be influenced contemporaneously and downstream. The treatments could cause legislators to support only the initial bill, only the downstream bill, or both. We combine the two votes, contemporaneous and downstream, to yield a new categorical dependent variable that indicates which combination of the two bills legislators supported. Did they support the bill both times? Oppose it both times? Support it initially but not downstream? Or downstream but not initially? And did treatment make some of these patterns of support more, or less, likely? We conduct this exploratory analysis for the three experiments where we find evidence of downstream persuasion in the prior section: Bergan (2009); Bergan and Cole (2015); Zelizer (2018).[31]

Let Y_i^t be support by legislator i for a bill considered at time t and d_i a legislator's treatment assignment. The original papers estimate the difference-in-means of Y_i^1 across treatment and control: $E[Y_i^1|d_i = 1] - E[Y_i^1|d_i = 0]$. The downstream analyses in this paper estimate the difference-in-means of Y_i^2 in the same manner. In an ideal world, we would estimate the downstream effects among subsets of the population that did or did not support the original bills. However, subsetting by Y_i^1 would incur post-treatment bias.

Instead, we define a new dependent variable as the permutation of possible positions

[31] we consider these analyses exploratory. Our decision to conduct these tests was explicitly conditional on the main results, so these results should be interpreted with this conditionality in mind.

on bills considered in both periods: $Y_i^{'} = (Y_i^1, Y_i^2)$. $Y_i^{'}$ can take on one of four values: $(0,0), (0,1), (1,0), (1,1)$. We now have a categorical dependent variable and a binary treatment variable; we could display the results in a standard 4 x 2 contingency table. This is a statistical classification problem in which an unordered dependent variable is predicted by an independent variable, which in our case is treatment assignment. If we wanted to show that there is any effect of treatment on the distribution of choices, we could estimate a chi-squared statistic or perform Fisher's exact test.

We are more interested in comparing specific patterns of support than in demonstrating any treatment effect on the distribution of choices. One could perform a multinomial or nested logit regression that considers all four choices simultaneously by comparing three of them to a baseline choice. We instead analyze the choices pairwise. For each of the "supportive choices" — either $(0,1), (1,0), (1,1)$ — we in turn estimate the difference, across control and treatment conditions, in the percentage of legislators who choose that option relative to the baseline condition of $(0,0)$. We perform this procedure for each of the "supportive choices." It is the same as creating 2 x 2 subtables from the larger contingency table and analyzing the share of legislators in each cell. We do not perform a multiple testing correction as we analyze each of the subtables.

Table B1 displays differences in the share of legislators who supported the bills at least once — who took the positions support-oppose, oppose-support, or support-support — compared to those who opposed them both times, in treatment versus control. The estimates illustrate whether more, or fewer, legislators took a given supportive pair of positions in the treatment than control condition. The table also reports the weighted averages of the three studies.[32]

Legislators were more likely to support the original but not the refiled bills by 6.9 percentage points ($\widehat{\text{SE}} = 8.6$) in the treatment condition. Similarly, legislators were 6.4 pp (4.8) more likely to support both bills than oppose both in treatment as opposed to control. Taken

[32]Precision-weighted and equally-weighted estimates are reported in Appendix Table A11.

together, legislators were just about equally more likely to support just the initial bill as to support both bills due to treatment.

In contrast, we find no evidence that legislators were more likely to support only the downstream bill. There is actually a slight decrease of 2.4 percentage points in the share of legislators who voted oppose-support than oppose-oppose, though this estimate is also subject to substantial uncertainty ($\widehat{\text{SE}} = 7.5$).

These estimates suggest that downstream treatment effect estimates occur among legislators who were also convinced to support the contemporaneous bills. We find no evidence consistent with a sleeper or delayed treatment effect, on average. Our interpretation is that treatment caused legislators to support the initial bills at higher rates, and that about half of legislators originally convinced to support the bills maintained their new position downstream.

Our interpretation suggests that legislators are robustly persuadable. They consider information provided to them and change their beliefs or attitudes in long-lasting ways, such that half of them convinced by interventions remain convinced years later. Such long-lasting individual effects are difficult to square with mechanisms such as Hawthorne or demand effects. Information treatments were relevant to legislators' decisions whether to support both the initial and the refiled bills because they changed legislators' beliefs or attitudes about the bills.

A second interpretation of these results is that legislative position-taking is characterized by habit (Gerber, Green, and Shachar 2003; Dillard, Hunter, and Burgoon 1984). Habit-based behavior might not rely on legislators' beliefs or attitudes about the bills at all. Perhaps other actors in the legislative process treated legislators who supported the initial bills differently when the bills were refiled; lobbyists, bill sponsors, or constituents may have exerted unequal pressure based on legislators' initial positions. Habit may also be rooted in legislators' psychological responses to supporting the initial bills. Perhaps they internalized the feeling of resisting partisan pressure or of voting for the public good instead of special

Table B1: **Patterns of Estimated Treatment Effect Over Time**

	Support Initial / Oppose Refiled	Oppose Initial / Support Refiled	Support Initial / Support Refiled
		Zelizer (2018)	
$\widehat{\text{ITT}}$	1.0	4.0	3.8
$(\widehat{\text{SE}})$	(3.2)	(5.9)	(3.5)
		Bergan (2009)	
$\widehat{\text{ITT}}$	6.4	-0.8	2.6
	(10.2)	(7.4)	(8.1)
		Bergan and Cole (2015)	
$\widehat{\text{ITT}}$	10.6	-7.1	11.3
	(17.9)	(15.8)	(8.9)
		Weighted Average[(2)]	
$\widehat{\text{ITT}}$	6.9	-2.4	6.8
$(\widehat{\text{SE}})$	(8.6)	(7.5)	(4.7)
$\widehat{\text{p}}$	0.21	0.63	0.08

Significance indicated at $p < 0.05$ (*) and $p < 0.01$ (**) one-sided.

(2) The weighted average is based on each study's number of unique legislators.

interests, and this broader reorientation lasted for the downstream bills. Perhaps they simply wished to remain consistent.

A determined skeptic might point out yet another explanation: that of imbalance across treatment and control groups. Because treatment assignment is the same downstream as contemporaneously, any imbalance in the initial studies would persist.

The designs of the experiments offer some evidence against this interpretation. Each study assigned units to treatment via block random assignment such that similar units were assigned to different conditions. In one study, Zelizer (2018), treatment was block assigned within legislator, such that each legislator was equally represented in treatment and control. Nevertheless, imbalance in potential outcomes may occur even with balance across individuals, so we consider how else to address this concern.

For results to be attributable to persistent imbalance, legislators' policy positions must be highly correlated across time. Is that the case? Among untreated observations, correlations in roll call voting over time are substantial in the two Bergan roll-call voting studies: 0.7 in the New Hampshire study and 0.5 in the Michigan study. However, correlations in bill cosponsorship across time are much more muted: in the Zelizer Tennessee study, the correlation in cosponsorship over time is only 0.09. If chance imbalance explains results, we should see the most persistent influence for the study with the most correlated outcomes over time — the New Hampshire study — and least persistent for the Tennessee study.

Estimated effects for individual studies are imprecise, but, if anything, exhibit the reverse pattern: influence is most persistent for the Tennessee study and least for the New Hampshire study. In the Tennessee study, treatment increased support for both contemporaneous and downstream bills by 3.8 percentage points, but only by 1.0 percentage points for the contemporaneous, but not downstream, bill. In the New Hampshire study, treatment shifted legislators from never supporting the bills to supporting only the first by 6.4 percentage points, but to supporting both bills by only 2.6 percentage points. Thus persistence is higher for the Tennessee study than the New Hampshire study. We interpret these results

with caution due to substantial sampling variability, but we do not see any evidence of more persistent treatment effect estimates where we would expect them if results are due to chance imbalance and correlated position-taking across time.

Finally, results may result from interference, that legislators observed their peers' contemporaneous votes and changed their downstream votes. Interference is generally a problem, but it is not clear why it would be more likely on downstream outcomes — which occurred a year or more after treatment — than on contemporaneous ones.

Appendix C. Supplementary Results from Heterogeneity Analysis

Table C1: **Estimated Heterogeneous Treatment Effects Across Legislators, Alternative Ideological Moderates Specifications (in pp)**

	(1)	(2)
Moderates: $(-1, 1)$ (72.1% of sample)		
Treatment	-4.9	4.0
	(9.8)	(5.8)
Treatment $*$ Moderate	18.4	7.1
	(11.1)	(7.5)
Moderates: $(-0.875, 0.875)$ (52.6% of sample)		
Treatment	-3.6	-0.1
	(7.7)	(4.9)
Treatment $*$ Moderate	20.7*	15.4*
	(9.7)	(7.2)
Moderates: $(-0.625, 0.625)$ (17.0% of sample)		
Treatment	2.7	5.0
	(5.6)	(4.1)
Treatment $*$ Moderate	22.2*	14.4
	(10.7)	(9.7)
Moderates: $(-0.5, 0.5)$ (11.0% of sample)		
Treatment	6.6	7.7*
	(5.1)	(3.7)
Treatment $*$ Moderate	18.8	22.7
	(14.1)	(12.4)
Covariates?	No	Yes

Significance indicated at $p < 0.05$ (*) and $p < 0.01$ (**) two-sided.

Table C2: **Estimated Heterogeneous Treatment Effects Across Legislators, with Butler and Nickerson (2011) (in pp)**

	(1)	(2)
	Ideology	
Treatment	-5.5	-2.7
	(5.7)	(3.9)
Treatment*Moderate	25.4**	18.7*
	(8.7)	(7.4)
	Partisanship	
Treatment	6.8	7.9
	(5.8)	(5.0)
Treatment*Democrat	-4.3	-6.8
	(7.4)	(6.3)
	District Competitiveness	
Treatment	-1.5	-2.8
	(5.1)	(3.8)
Treatment*Competitive	18.2*	21.6**
	(9.2)	(7.6)
	Tenure	
Treatment	2.9	3.2*
	(5.6)	(4.3)
Treatment*First-Term	6.9	5.5
	(8.7)	(7.2)
Covariates?	No	Yes

Significance indicated at $p < 0.05$ (*) and $p < 0.01$ (**) two-sided.
Robust standard-errors reported.

Appendix D. Bill Information for Crossover Analysis

Table D1: ***Description of Veterans Bills in Zelizer (2018)***

Bill	Title	Briefing Information
HB715	Improving Veterans' Access to Higher Education	Bill extends eligibility for in-state tuition and establishes criteria for awarding academic credit for military service.
HB828	Discounted Burial Fees	Lowers veterans' burial fee from $700 to $300.
HB1201	Retirement Credit for Veterans in State Service	Equalize retirements benefits for veterans across conflicts.
HB854	Veterans Treatment Courts	Funds specialized courts or dockets that offer an alternative to incarceration for veterans arrested for drug offenses.
HB804	Preferential Hiring for Qualifying Veterans	Currently illegal for private companies to prefer a group of citizens in hiring.
HB1082	Veteran Employment Tax Credit	Provides tax credit for businesses that hire a qualifying veteran.
HB1202	Removing Limits on ROTC Courses for Scholarships	Bill excludes ROTC courses from relevant course cap for scholarship purposes.
HB800	Removing Limits on Military Service for Scholarships	Exempt students with demonstrable military obligations from the immediate enrollment requirement.
HB803	Drivers License Requirements	Waive commercial driver skills test.
HB53	Tax Exemption for Automobile Grants	Exempt federal program that pays for disabled veterans' to purchase cars adapted to work with their disability.
HB183	Traumatic Brain Injury Treatment and Recovery Act	Establish trial program for Hyperbaric Oxygen Treatment (HBOT).
HB126	Scholarships for Children of Military Personnel	Simplifies rules for legal residence and home of residence for scholarship eligibility.
HB476	Waiving Permit Fees on Gun Licenses	Waives fees on permit renewals; veterans already exempt from initial permit filing fees.
HB492	Exempting Disabled Veterans from License Fees	Extend hunting and fishing benefits currently available only for veterans with 30%+ disability.
HB798	Services for Children with Intellectual Disabilities	Exempt military families from the residency requirements to enter waiting list for state services.
HB1024	Medal of Honor School Program	Urge the state Board of Education to adopt the Medal of Honor curriculum.
HB657*	Relative to Military Service Credit	Adds [recent conflicts] to the definition of "period of armed conflict" for purposes of determining military service credit in the consolidated retirement system..

Appendix E. Interview Script

Thank you for agreeing to participate. Today, we have 3-4 questions for you. We know that you are busy and will do our best to keep the survey under 15 minutes. Your responses will be kept anonymous. Short answers will be fine for most questions, but you can feel free to elaborate if any answer needs more detail.

Before we start would you mind if I record this Zoom interview?

1a. First, I would like to ask about your overall support for antibullying legislation.

Specifically, the Michigan state legislature in 2011 considered and adopted the "Matt Epling Safe School Law." The bill required school districts to adopt anti-bullying policies specifying procedures for reporting incidents, notifying parents of the children involved, and investigating those incidents.

Would you say that you support or oppose the "Matt Epling Safe School Law?"

If "support" to 1a: 1b: would you say you strongly support the law or not so strongly?

If "oppose" to 1a: 1c: would you say you strongly oppose the law or not so strongly?

2a. Do you recall receiving any communications – such as email, phone calls, letters, in-person communications - from your constituents about the "Matt Epling Safe School Law"?

If "no" to 2a: skip to 3a.

If "yes" to 2a: 2b: What communications did you receive?

If "yes" to 2a: 2c: Do you recall changing your support for the "Matt Epling Safe School Law" in response to receiving those communications?

If "no" to 2c: skip to 3a.

If "yes" to 2c: 2d: How did the communications influence your views?

3a. More generally now, we are interested if there is any occasion while you were serving as a state legislator when you received a communication from a constituent or an interest group representative that changed your mind about something.

If "no" to 3a: Skip to 4.

If "yes" to 3a: 3b: Could you tell us more about that occasion? Why do you think it was effective in changing your mind?

4a. Do you recall ever changing your attitudes about an issue in general - as opposed to changing your positions on a specific piece of legislation - in response to changing events, community outreach, lobbying, or anything else?

If "no" to 4a: Skip to conclusion.

If "yes" to 4a: 4b: Could you tell us more about that occasion? Why do you think it was effective in changing your mind?

5. What recommendations would you make to groups or constituents attempting to persuade their representative?

Conclusion: Thank you very much for your participation! Is there anything else you would like to add?

Appendix F. Legislative Field Experiments

Table F1 lists nine papers that examine twelve behavioral measures of legislators' policy positions. It lists the setting, subjects, number of observations, issue area, treatment type, outcome, and estimated treatment effect for each study. Eight of the papers examine state legislators in the U.S.; the ninth examines mayors in Brazil.[33] Five of the studies examine a single bill, while four include multiple bills. The issues addressed tend toward less partisan topics like tobacco regulation, bullying prevention, and veterans services. Outcomes include relatively less formal position-taking, like social media posting or cosponsorship, and formal roll call voting and policy adoption.

[33] An outlier in terms of setting and subjects, we include this paper because the policy expertise treatment is similar to other papers included, it is the one paper that examines policy adoption as an outcome.

Table F1: ***Field Experiments on Legislative Persuasion***

Paper	**Setting**	**N**	**Issue**	**Treatment**	**Outcome**	**Est. ITT**[(1)]
Bergan (2009)	New Hampshire House	143 Legs 1 Bill	Tobacco	Grassroots Lobbying	Voting	20** perc. points
Bergan and Cole (2015)	Michigan House / Senate	148 Legs 1 Bill	Bullying	Grassroots Lobbying	Voting	12**
Butler and Nickerson (2011)	New Mexico House	70 Legs 1 Bill	Spending	Public Opinion Poll	Voting	29**
Camp, Schwam-Baird, and Zelizer (2023)	Northeast US House/Senate	210 Legs 5 Bills	Higher Ed	Lobbying	Tweeting Cosponsorship	-1
	Tennessee House	52 Legs 16 bills	Veterans			-1
Hjort et al. (2021)	Brazilian Cities	913 Mayors 1 Policy	Taxes	Policy Briefing	Policy Adoption	4*
Grose et al. (2022)	California Assembly / Senate	119 Legs 1 Bill	Higher Ed	Social Lobbying	Public Support	12*
Zelizer (2018)	Tennessee House	76 Legs 16 Bills	Veterans	Policy Briefing	Cosponsorship Voting	5*** 17*
Zelizer (2019)	Tennessee House	57 Legs 16 Bills	Veterans	Policy Briefing	Cosponsorship	4
Zelizer (2022)	Tennessee House	26 Legs 67 Bills	Varied	Group Deliberation	Cosponsorship Voting	5** 4

(1) Source for estimated effects: Bergan (2009): p. 342; p-value estimated via 95% interval. Bergan and Cole (2015): Table 5. Butler and Nickerson (2011): Table C.3. Camp et al (2021): Table 3. Hjort et al (2021): Table 7 for ToT, 37% attendance estimate from p. 1473. Grose et al (2021): Table 1. Zelizer (2018): Tables 3, 4. Zelizer (2019): Table A1. Zelizer (2021): Tables 4, 6.

P-values indicated at $p < .1$ (*), $p < .05$ (**), and $p < .01$ (***).

Appendix G. Ethical Considerations

No new experiments were conducted for this paper. The experiments that it reanalyzes were all conducted under the Common Rule, prior to the adoption of the Final Common Rule in 2017, and prior to APSA's "Principles and Guidance for Human Subjects Research." It is unclear whether and how we should consider the ethics of previously-fielded interventions, but we believe the exercise is useful and informative, so we include our thoughts below.

We believe that the interventions described are consistent with APSA guidance about interventions with elected officials. In our view, the advocacy campaigns posed minimal risk of harm to legislators. Receiving emails, phone calls, policy briefings, or public opinion polls is an everyday occurrence in legislatures. It is expected they will interact with the public, with staffers, or with academics. Analyzing old experiments, conducted on relatively low salience bills, reduces the prospects for reputational harm for legislators today, as many subjects no longer work in politics, and the issues we discuss have faded from public attention. Confidentiality, when employed, further shields them from reputational harm.

Consent was not obtained as part of these field experiments, as it was not required under the Common Rule and would have informed subjects that they were being studied. Interventions were not deceptive. The polls and briefings took pains and incurred substantial costs to provide factually accurate information. The advocacy campaigns were implemented by third party groups as part of their normal operations. Confidentially was not maintained by one of the studies, which publicly posted the names and treatment statuses of subjects. The other three studies maintain confidentiality.

Each intervention required researchers to consider the impact of their studies. In two cases, the interventions were conducted by third parties, which the APSA guidelines state "do not usually invoke this principle on impact." Nevertheless, each intervention facilitated public engagement with legislators. In the other two cases, legislators were provided either with factual briefings about proposals or the results of public opinion polls. In all four experiments, it seems like the most plausible impacts of these interventions would be to help

the public be better represented by legislators, or to help legislators make more informed decisions about the bills under consideration. To the extent the interventions affected bill passage, policy impacts could have been heterogeneous. Evaluating the ethical implications of heterogeneous policy impacts is difficult and likely to be contentious. Our view is that the aims of several of the campaigns — to pass anti-smoking, anti-bullying, or pro-veterans legislation — are relatively less contested and more broadly supported than if other types of bills had been selected for study.

Appendix H. Additional Test of Ideological Sorting

The test of ideological sorting we report in the paper reports the change in the percent of correctly predicted votes in control and treatment. This approach finds the point on the ideological spectrum that maximizes the correct prediction of votes separately in each condition and compares the rates of correct predictions.

We also explore a related test of ideological sorting. Recall that each vote is characterized by a rightward status quo and leftward policy proposal, as determined by the voting patterns of Democrats supporting the bills at higher rates than Republicans in each state. We identify the most right-leaning legislator (in each treatment condition) who voted in favor of the proposal and calculate the share of all legislators (in that condition) to the left who also voted for the proposal. We also identify the most left-leaning legislator who voted against the proposal and calculate the share to the right who also voted against it. The intuition is that clarifying the ideological content of the bill should help legislators sort into their 'correct' positions, which means both that the most conservative legislator who supports the bill may well shift to the right, and that all legislators to the left of this legislator should be more likely to also support the bill. The same should happen with the most liberal opponent and all legislators to her right.

This test also reveals increases in ideological sorting in two of the three states. In Michigan, treatment increased the share of left-wing legislators who voted for the proposal from 84% to 96% and right-wing legislators who voted against from 80% to 85%. In New Hampshire, left-wing legislators increase support from 50% to 55%; right-wing opposition from 31% to 45%.

New Mexico sees a decline in ideological sorting, from 84% to 69% among left-wing legislators and 64% to 44$ among right-wing. One explanation for this pattern, and potential drawback to the test, is its susceptibility to outliers and reliance on the most extreme, unlikely supporter or opponent of the bill. Note from Figure 2 that one Democrat voted against the proposal - that legislator was assigned to treatment - and a handful of the most right-wing

Republicans voted for the proposal, several of whom were assigned to control. As a result, using a cut point based on a single legislator may be more prone to sampling variability than the approach in our paper, which allows a single legislator to cast an unpredicted vote without then imposing the requirement that all legislators to one side vote the same way.