Is Legislative Persuasion Real? Evidence from Four Re-analyses of Three Legislative Field Experiments

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Abstract

In the past decade, several field experiments have shown that information can change legislators' support for specific bills, often to a surprising degree. However, legislative experiments are limited by small sample sizes, a focus on noncontroversial legislation, and little external validity, such that it is unclear how strong the evidence for persuasion really is. I re-analyze data from three legislative experiments to determine the robustness and validity of findings of legislative persuasion. I operate under the assumption that, if the initial interventions genuinely changed legislators' support for experimental bills by means of their underlying beliefs or preferences, the interventions might also have changed legislators' support for related bills that were not examined in the original studies. Two tests find evidence of robust persuasion: influence persists on refiled versions of experimental bills for two to three years after treatment, and influence crosses over to contemporaneous bills with similar content to the targeted bills. Two analyses, on the downstream effects of political information and the issue-wide effects of policy research, find no evidence of persuasion. Persuasion appears more robust when legislators are provided hard information about the content of bills than political intelligence about the preferences of voters.

Word count: XXXX

> 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)

> 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 essential characteristic of legislatures. In an earlier era, legislators may have been persuaded to vote in a specific way by bribes or pressure from party bosses (Mill 1861; Ostrogorski 1902; Gosnell 1924; Luce 1924; Tanner 1888; Bryce 1937 [1888]; Reinsch 1907). More recently, party machines, smoke-filled back-rooms, and quid pro quo corruption have been replaced by the more public influence of pressure groups, party activists, and lobbyists (Zeller 1937; Key 1955; Dahl 1961; Wilson 1966; McCarthy 1912; Reinsch 1907). While there are many ways to influence legislative outcomes — from electing or subsidizing like-minded legislators to setting the policy agenda — once a bill has reached the floor, informational persuasion rules (Hall and Deardorff 2006; Crawford and Sobel 1982; Gilligan and Krehbiel 1987; Austen-Smith and Riker 1987; Curry 2015).

Over the past decade, a proliferation of field experiments have illustrated that lawmakers can be persuaded to change their positions on legislation via information (Bergan 2009; Bergan and Cole 2015; Butler and Nickerson 2011; Hjort et al. 2021; Grose et al. 2021; Zelizer 2018, 2019, 2021). Information about the content of policy proposals or the preferences of constituents has convinced policymakers to go public in their support for legislation via Twitter and cosponsorship; vote for bills that they otherwise would have opposed; and enact policies that they otherwise would have passed over. Some studies have found null effects of information on legislators' beliefs or positions, especially when information does not reach the legislator (Camp, Schwam-Baird, and Zelizer 2021; Kalla and Porter 2019). But the majority of published studies have concluded that information changes legislators' policy positions, and is thus persuasive.

That legislators are so persuadable is surprising. The magnitudes of estimated treatment

effects range from substantively meaningful to enormous. Bergan (2009) and Butler and Nickerson (2011) estimate that informing New Hampshire and New Mexico legislators of their constituents' positions on a bill changes the votes of 20 – 30% of legislators. Yet, Poole (2007, p. 436-437) captures a widespread belief that legislators are "true believers" who "do not compromise their principles" and vote in consistently predictable ways (Brimhall and Otis 1948; Lee 2009; Poole 2005; Bonica 2014; Fouirnaies and Hall 2021). It is hard to square the idea that many votes are predictable, based on legislators' fixed political ideologies, with the fact that phone calls from constituents can change the votes of 10% of legislators (Bergan and Cole 2015). These findings are all the more surprising given a meta-analysis of election experiments found voters' candidate choices are largely immune to pressure (Kalla and Broockman 2018). Taken together, these results turn the classic findings and argument of Converse (1964) upside down: Converse used the high correlations among legislators' votes to demonstrate how ideologically unconstrained voters were, but it appears legislators' votes are more susceptible to influence than voters'.

Legislators may appear so persuadable for several reasons. Taking results at face value, legislators' positions may simply be highly contingent. Information may be persuasive because it changes legislators' beliefs or preferences in ways that manifest in cosponsorship, voting, or policy adoption. Perhaps the time constraints and informational burdens placed on legislators, particularly those in relatively less professional settings considering relatively less controversial legislation, make them susceptible to influence. Another possibility is that these findings are artefactual. Perhaps legislators are subject to Hawthorne or demand effects such that they do indeed change their positions, but only in response to the unusual prospect of being studied by researchers. Even more troubling, the literature on legislative persuasion may be characterized by p-screening, p-hacking, or chance imbalances such that legislators are not actually influenced by treatments at all, but only appear to be.

This paper investigates the robustness and validity of findings of legislative persuasion. One way to conclusively demonstrate the persuadability of legislators would be to conduct

a large-scale intervention with overwhelming statistical power. That ideal approach is prevented by the same factors that constrained past legislative experiments. It is difficult and expensive to craft high-quality, relevant treatments about pending legislative proposals and to deliver them directly to legislators with confidence. Further, legislatures offer a fixed, and limited, number of subjects, and it is difficult to predict when or whether multiple legislatures will consider the same policy proposal at the same time. Thus while legislative audit studies can enroll all state legislators in the country, studies of legislative policymaking are nearly always limited to a single legislature at a time.

To overcome these limits, I adopt another approach. I conduct four re-analyses of three legislative experiments that found evidence of persuasion. Each re-analysis examines whether the informational intervention influenced legislators' support for bills other than those originally examined in the study. The logic behind this approach is that if the original interventions genuinely persuaded legislators to change their beliefs or preferences over the outcome bill(s) examined, these changes might have spilled over to related legislation. Following Zelizer (2018), for example, several experimental bills were re-filed in nearly identical form in the two sessions after the experiment occurred; we might expect treatment to have influenced legislators' positions not only contemporaneously, but also downstream on these identical re-filed bills. In another case, Butler and Nickerson (2011) sent legislators survey results about their constituents' budgetary preferences during a 2008 special session called to spend a dwindling budget surplus. In 2009, legislators were again called into special session to consider spending cuts due to a further worsening of the budget. If the policy research or public opinion polls, respectively, were informative and persuasive for the original bills, they might plausibly have influenced legislators' support for the later bills, too.

Results indicate that persuasion can persist and even spread across policies, but within limits. First, persuasion is found to persist on refiled bills for up to two to three years, but to fade away by years four to five. Second, persuasion can cross over to other bills than the one targeted. For spillover to occur, however, bills must overlap substantially in content. Third,

policy expertise is more likely than political intelligence to lead to persistent or crossover persuasion.

Together, these results recommend a relatively sanguine perspective on legislative persuasion. Legislators can be convinced to take certain policy positions, and these changes can be long-lasting; legislative persuasion is real. Nevertheless, persuasion is limited to the bills targeted or to bills with similar content. Persuasion shows that legislators are still trying to make good public policy, but are constrained by the limited information, research, and support provided to them.

Evidence of Legislative Persuasion

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 (Neustadt 1991; Lippmann 1922; Klapper 1960; Hovland, Janis, and Kelley 1953; Popkin 1994), but legislators themselves have only recently been studied as agents or objects of persuasion. One obvious reason for this delay is the difficulty of demonstrating persuasion among elites using experimental methods. Though political psychologists could experiment with propaganda on army conscripts during WWII (Hovland, Lumsdaine, and Sheffield 1949), elites did not make themselves available for similar investigation.

The past decade has seen an explosion in experimentation with political elites, including legislators. Many of these experiments examine the electoral relationships between elected officials and the public, either how legislators shape constituents' preferences or how constituents demand services from or access to legislators (Butler et al. 2017; Butler and Broockman 2011; Anderson, Butler, and Harbridge-Yong 2020; Butler, Karpowitz, and Pope 2012; Grose, Malhotra, and Parks Van Houweling 2015; Broockman and Butler 2017; Chin, Bond, and Geva 2000; Brodbeck, Harrigan, and Smith 2013; Richardson and John 2012; Malesky, Schuler, and Tran 2012). Other studies focus on the processes inside the legislature by which legislators come to support or oppose legislation. Scholars have used surveys to measure leg-

islators' beliefs (and biases) about policy, how they learn from new information, and whether that information changes their policy positions (Lee 2021; Lee et al. 2021; Pereira 2021a,b; Kalla and Porter 2019; Demaj and Schedler 2014; Liaqat 2019). A further subset of those experimental studies examine persuasion on active policy proposals under consideration in the legislature, with legislators' revealed policy positions as the outcomes of interest. Table 1 lists nine of these papers that examine twelve measures of legislators' policy positions.

Table 1 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.; Hjort et al. (2021) examines persuasion among mayors in Brazil. 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.

The most striking feature of Table 1 is the magnitude of estimated treatment effects. Information about the content of proposals or the preferences of constituents is estimated to change legislators' votes by 17, 20, and even 29 percentage points. Furthermore, every study except one finds evidence of persuasion. What explains these findings of persistent, excessive persuasion?

One possible explanation for this problem is that the existing literature is flawed through selective reporting, p-hacking, or low power. Table 1 shows that many studies of legislative persuasion have small sample sizes. Treatments in legislative experiments — whether conducting a poll of constituents, activating the grassroots to contact legislators, or preparing policy research reports — are costly to develop and difficult to deliver directly to legislators. As a result, existing studies are often limited to a single bill or issue at a time, in a single legislature. Four of the studies have fewer than 200 cases with which to estimate persuasion.

We can examine the relationship between statistical power and limited sample sizes in

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Table 1: ***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 (2021)	Northeast US House/Senate Tennessee House	210 Legs 5 Bills 52 Legs 16 bills	Higher Ed Veterans	Lobbying	Public Support Cosponsorship	-1 -1
Hjort et al. (2021)	Brazilian Cities	913 Mayors 1 Policy	Taxes	Policy Briefing	Policy Adoption	4*
Grose et al. (2021)	Unstated	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 (2021)	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 $p < .01$ (***).

these studies by comparing the estimated magnitudes and p-values of treatment effects.[1] Three papers — Hjort et al. (2021); Grose et al. (2021); 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 estimated treatment effects represent enormous changes in legislators' policy positions. One of the most influential treatments in the get-out-the-vote literature is one that informs voters that whether they vote will be publicized for their neighbors to see. This intervention caused only an 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 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 (2021), reports null effects, and it estimates effects of lobbying of between $-1 - 0$ percentage point across four different experiments. The large proportion of substantively large, statistically-significant findings are consistent with a literature subject to type-M bias due to small sample sizes, low statistical power, and publication bias (Gelman and Carlin 2014).

To evaluate the robustness and validity of findings of legislative persuasion, I re-analyze data from three experiments included in Table 1: Zelizer (2018, 2019) and Butler and Nickerson (2011). I chose these papers based on two criteria: 1) legislator-specific treatment assignments are available and 2) the interventions happened long enough ago that examining follow-up outcomes is feasible. The logic of the re-analysis is that if the interventions genuinely persuaded legislators to change their positions due to modifications of their beliefs or preferences, those updated beliefs or preferences might also affect legislators' positions on bills outside the original scope of study.

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[1]I 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 three experiments differ in important, but useful, ways. Zelizer (2018, 2019) reports two experiments that provided legislators with research reports prepared by a joint legislative veterans caucus. In Zelizer (2018), seventy-six legislators were provided policy research on four of 16 bills related to veterans affairs, with the four bills chosen at random for each legislator. Each legislator was thus part of both treatment and control conditions. The policy research described the problem the bill meant to solve and how it intended to do so. The research made legislators more supportive of the bills: cosponsorship increased by nearly 60% on average when legislators were provided policy research, likely due to the fact that veterans affairs is a popular issue among elected officials, so the briefings informed legislators that the content of the bill was unobjectionable and the aims of the bill popular. Zelizer (2019) was fielded one year later, in 2016, in the same state legislature, and similarly provided caucus research reports on veterans bills directly to legislators. It also estimated large effects of the research. This study featured one important modification: a pure control group of legislators who were not briefed on any bills.

These two experiments allow me to estimate the persistence and transferability of persuasion due to their timing and inclusion of multiple bills. Both studies were fielded during the 2015-2016 assembly, so there have been two full legislative assemblies since (2017-2018 and 2019-2020). During these two additional assemblies, seven bills were refiled with nearly identical content to experimental bills from 2015. Further, of the 16 bills in the 2015 study, many featured similar content, such that a treatment targeting one bill might have plausibly influenced legislators' support for another, related bill. Thus I estimate whether persuasion persisted to influence re-filed bills in the two assemblies after the experiment or crossed over to influence bills other than the one intended in the original study.

The pure control group in Zelizer (2019) facilitates an even cleaner test of crossover. This test compares bill support between legislators who were treated *at all* and those who were never treated. I estimate this form of crossover effect both contemporaneously, during the original assembly, and in the two following the 2016 intervention. Because of the pure control

group and a second treatment arm, there are substantially fewer treated legislators in this study than in Zelizer (2018). This fact, combined with the across-legislator assignment to pure control, diminishes statistical power compared to the re-analyses of Zelizer (2018).[2]

Butler and Nickerson (2011) provided legislators with a different type of information treatment: the results of a survey of constituents on a bill being considered during the 2008 New Mexico special session. Due to a fall in oil prices that eroded the state's projected budget surplus, the legislature considered how to proceed with a spending and tax cut proposal that had anticipated the full budget surplus. Butler and Nickerson (2011) asked voters across the state whether they thought the legislature should 1) fund the full proposal, despite not having the money (and presumably spending rainy day funds, cutting funds elsewhere, or relying on creative accounting to balance the budget); 2) fund the proposal, but at lower levels; or 3) not fund the proposal at all. Treatment was a letter to legislators stating the percentage of constituents in their district who supported spending the full amount. Treatment was randomly-assigned across legislators, such that some legislators received the results of the poll and others did not. The content of the letters was truthful, so legislators who received letters were informed of support for the bill in their specific district. The only outcome analyzed was legislators' votes on the bill, SB 24.

The Great Recession continued to impact New Mexico's budget after the 2008 special session, such that legislators had to cut the budget during regular and special sessions held in 2009, with the special session called specifically to tackle the budget problems. The legislature considered a range of proposals to void or cut appropriations; reallocate funds from reserve to general accounts; or use long-term bonds to fund operating expenses. Knowing whether constituents preferred the tight or loose fiscal policy from the previous year might have helped legislators decide whether to cut spending or rely on stopgap funding measures during the 2009 sessions. I estimate whether the public opinion poll designed around SB 24

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[2]The limited power, along with the fact only one bill from 2016 was filed in nearly identical form and another in similar form, explains why I do not repeat the analyses of the 2015 experiment with the 2016 data.

in 2008 might have influenced legislators' votes on spending and funding proposals during the 2009 sessions.

All three experiments, like most of the others in Table 1, estimated substantial treatment effects in the original study. On the one hand, this is a benefit, if not a necessary precondition, for analyzing the robustness of persuasion. Re-analyzing 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 preferences, and presumably the larger the influence on unintended outcomes.

On the other hand, it is unclear whether selecting only studies where persuasion occurred might bias any re-analysis. If large treatment effects were estimated due to chance imbalance in treatment assignment in the original study, this imbalance would persist in the re-analysis. This concern may be more severe for Butler and Nickerson (2011) and Zelizer (2019), where the unit of assignment is the legislator, than Zelizer (2018), where treatment was block assigned within, and thus balanced across, legislators. It is easier to think of imbalance in terms of whether pro-veteran legislators were disproportionately assigned to treatment or control than whether legislators who were more supportive of a specific handful of veterans bills were imbalanced across conditions. Further, imbalance is also less likely given the substantially larger sample size and smaller p-value in Zelizer (2018).

If treatment effects occurred purely due to p-hacking or Hawthorne effects, in contrast, there would be less reason to worry. Large, statistically-significant treatment effect estimates on intended outcomes would convey little information about unintended outcomes if the academics conducting the original study had no intention of examining these outcomes, or subjects had no idea such outcomes would be monitored.

If legislators were really persuaded to change their policy positions due to the policy expertise or political intelligence interventions, I would expect to see some pattern of influence on these other, related bills. The exact pattern of influence, though, is unclear. Either expertise or intelligence may be more persistent or robust. The three experiments were conducted

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in two different states, so the political context and the composition of the legislatures differed. The original studies varied in terms of statistical power, and so too do the re-analyses. Thus the main question for analysis is whether there is any evidence of persuasion on these unintended outcomes, and any questions about the relative influence of different types of information, or of different types of outcomes, are secondary and exploratory in nature.

Persistence of Policy Expertise from Zelizer (2018)

The first research design estimates the downstream effects of policy research from Zelizer (2018). Of the 16 bills in the original experiment, four were sponsored again in nearly identical form in the ensuing assembly, and three more in the assembly after that.

Table 2 and Table 3 display text from the legislative website's bill summaries for the original experimental bills and the subsequent bills re-filed in the 2017-2018 and 2019-2020 assemblies, respectively. Two of the four 2017-2018 bills had not passed the previous session, so legislators refiled them. The two other bills had passed but only partially achieved their objectives: House Bill 828 reduced burial fees for veterans' spouses by less than the sponsor had wanted, so she refiled it in the 2017-2018 session to further lower fees. House Bill 715 was passed in 2015 to require the public university system to develop a plan for granting veterans academic credit for their military experience. In the 2017-2018 session, House Bill 433 required the system to "readily provide" the credit. By the 2019-2020 session, the original goal of House Bill 828 still had still not been achieved, so it was refiled, as were two bills that had failed during the original experiment, House Bills 1201 and 183.

The reintroduction of bills gives us an opportunity to examine the downstream effects of information. Because information on the original bills was randomized across legislators, so too was information on the refiled bills. 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.[3] Thus there are 264 observations of legislators' policy positions on the refiled bills in

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[3]It is highly implausible that legislators' re-election depended on their treatment or positions for these bills, so attrition is ignored.

Table 2: **Reintroduced Bills 2017-2018**

Original Bill	New Bill	Same Sponsor?
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	No
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	Yes
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	Yes
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	No

Table 3: ***Reintroduced Bills 2019-2020***

Original Bill	New Bill	Same Sponsor?
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	No
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.	No
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	No

2017-2018 and 141 in 2019-2020. What were the effects of the policy research on positions for the refiled bills?

Table 4 displays legislators' support for legislation by treatment assignment and assembly. It includes cosponsorship and roll call voting. For the original bills, cosponsorship in the treatment group was higher than the control group by a modest amount: 3.9% in control and 7.4% in treatment. Support for the original bills was relatively low in both conditions, dragged down by low support for the four bills that failed to make it out of committee.

Table 4: **Support for Four Bills, by Treatment Assignment and Assembly**

| | 2015–2016 | 2017–2018 | | 2019-2020 | |
	Cosponsorship	Cosp.	Vote	Cosp.	Vote
Control	3.9%	46.8%	92.1%	15.6%	100.0%
Treatment	7.4	55.7	98.4	12.5	100.0
Nbills	6	4	4	3	1
N	339	264	264	141	47

Roll call voting is not presented for 2015–2016 because only two of the six bills reached a vote, and they passed without opposition.

For the refiled bills, cosponsorship support is substantially higher in the assembly following the experiment at 47% in control and 56% in treatment. Support is also higher for roll call voting: 92% in control and 98% in treatment. Legislators' generally higher support for these bills may reflect legislators' increased familiarity with them from the prior session or the coalition-building efforts of bill sponsors. Support remains higher than baseline levels in the 2019-2020 session, at 16% cosponsorship in control and 13% in treatment. All legislators voted for the one bill that reached a vote in 2019-2020. The simple difference-in-means treatment effect estimates are positive and substantial in magnitude in 2017-2018 (6–9 percentage points), but zero or negative in 2019-2020.

I present estimated intent-to-treat effects of the policy research on downstream support in Table 5. Results pool across the two ensuing assemblies. Estimates are obtained by

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regressing outcomes on a dichotomous treatment indicator.[4] Treatment is estimated to have increased cosponsorship of the refiled bills by 5.0 percentage points ($\widehat{SE} = 5.6$) and roll call support by 5.2 percentage points (3.0). With 405 observations in each analysis, only the voting estimates attain conventional levels of statistical significance. The 5 percentage point estimated downstream effects are large, but comparable in magnitude to estimated immediate information effects.

Table 5: **Estimated Downstream Effects from Zelizer (2018) (in pp)**

	Cosponsorship	Roll Call Vote
$\widehat{ITT}$	5.0	5.2*
$(\widehat{SE})$	(5.8)	(2.2)
N	405	405

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

This is a difficult test of persuasion. Rather than testing whether information changes legislators' responses to a survey fielded shortly after the treatment, this analysis examines behavioral outcomes two to three years after information was provided. Despite the difficulty of the test, treatment effects appear to persist for a meaningful length of time. Persuasion has disappeared four to five years after treatment, however, which could indicate that persuasion fades over time. This finding in particular must be interpreted carefully, however, as the number of observations declined due to legislators leaving office and fewer bills being re-filed, and the COVID-19 pandemic influenced the 2020 session of the assembly in myriad ways.

Why might persuasion be long-lasting? It is unlikely that the policy research intervention changed behavior two to three years later simply by increasing the salience of the bills or alerting legislators to their positions being monitored. The original bills had already been resolved, either passed or killed, and the briefings gave no indication whether bills would be refiled or the issue revived in future sessions. The monitoring story is also hard to

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[4]Estimates from regressions with bill fixed effects are included in the Appendix.

believe, as the individual who administered the treatments was long gone by the time bills were reconsidered. The most plausible explanation is that legislators learned about the content of the proposals. Legislators were modestly more supportive of the bills initially, and when the bills were refiled with years of extra work done on their behalf, legislators were more supportive if they had studied their impacts previously. It appears that policy-specific expertise persists.

Crossover Effects of Policy Expertise from Zelizer (2018)

This section makes use of the 16 bills in Zelizer (2018) to estimate crossover effects: the extent to which a policy research report on one bill influenced legislators' support of another bill. All of the bills addressed the same general issue of veterans affairs, and several addressed even more related policy problems within the field. Having bills with related content allows us to examine whether policy information meant for one bill 'spills over' and influences support for other bills.

Table 6 displays descriptions of the sixteen bills included in the experiment. 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. While all bills addressed veterans affairs and may share details that allow information to be transferred from one to another, these five pairs of bills are those where information would appear most transferable.

I test for crossover effects between these pairs of bills by examining whether cosponsorship of bill b depends on treatment on bill b':

Table 6: *Description of Veterans Bills*

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..

$$Y_{ib} = \beta_0 + \beta_1 d_{ib} + \beta_2 d_{ib'} + u_{ib} \quad \text{for } b \neq b' \tag{1}$$

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. With five pairs of bills, there are a total of nine possible crossover effects of interest.[5]

The direction of crossover effects is unclear ex ante. Since direct effect estimates were positive, we might infer that information conveyed the popularity of veterans legislation. In this case, crossover effects may well be positive too. However, crossover effects might be negative. Legislators may have a budget for cosponsorship such that supporting one bill accomplishes their goal, perhaps of showing themselves as pro-veteran, and makes them less likely to publicly support other veterans bills. The direction of crossover effects thus will depend on the mechanism underlying persuasion.

The average crossover effects for these five pairs of bills is 3.2 percentage points. The average crossover effect estimate is substantially smaller than the average direct effect estimate of 7.9 percentage points. Six of the nine crossover effect estimates are positive; three negative.

Table 7: **Estimated Crossover Effects from Zelizer (2018) (in pp)**

	Mean Effect
Crossover $\widehat{\text{ITT}}$	3.2
Direct $\widehat{\text{ITT}}$	7.9
N	608

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

I conduct two randomization-based tests to determine whether these estimates reveal real crossover of persuasion. First, I examine whether selecting five pairs of bills at random from the sixteen in the study could yield estimated crossover effects as large as those observed.[6] This test assumes that the five pairs of bills whose content appears most similar are uniquely likely to exhibit crossover effects; if estimated crossover effects are as large between bills without such similar content, that would indicate either that estimated effects are consistent with sampling variability or crossover effects are similar in magnitude across all sixteen bills in the study. Of the simulations, only 4.9% produced average crossover effects as large or larger than those observed of 3.2 percentage points.

The second randomization test examines whether the crossover effects among the five sets of bills are greater than we would expect under the sharp null hypothesis of no crossover effect for any bills. In each of 1,000 simulations, the crossover treatment variable is re-randomized, and crossover effects are re-estimated with this permuted treatment assignment for the five sets of related bills[7] 9.7% of simulations yielded an average crossover effect as large as the one observed.

Taken together, this evidence is suggestive of crossover effects between pairs of substantively similar bills. We would expect crossover effects to be smaller than direct effects, since the crossover effects are based on information that was provided in the context of another bill, and that is what we observe. On average, the crossover effects that are most likely to occur given similarity in bill content are smaller in magnitude, by approximately 60%, than direct effects for those same bills. Nevertheless, average estimated effects are a reasonably-sized 3 percentage points, and two-thirds of estimates are positive. Average crossover effect estimates straddle conventional levels of statistical significance, with p-values ranging from .05 to .10.

While these tests for the most likely form of crossover effects retain some ambiguity,

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[6]Specifically, the five pairs include nine bills, and HB 657 can only be included as an outcome, not a treatment, bill.

[7]The direct treatment variable is unchanged from the observed value.

there is much clearer evidence about crossover effects more generally. Among all possible pairs of crossover effects, the average crossover effect estimate is -0.01 percentage points. 41% of estimates are positive; 53% negative; and 6% are zero. For most combinations of bills, it seems that crossover of persuasion is unlikely. Thus while there may be crossover influence between the most similar bills, there are likely no effects among most bill pairs in the experiment.

One remaining question about crossover effects is whether they cause bias for the estimation of direct treatment effects. Excluding crossover effects from Equation (1), the average of the 16 direct treatment effects for the 16 bills in the study is estimated to be 4.91 percentage points. Including crossover effects, the average direct treatment effects across all 256 pairs of bills is estimated to be 4.89 percentage points. Accounting for crossover effects in this way leads to de minimis changes to estimated direct treatment effects. This result is good news for legislative experiments. Even if some crossover effects occur in studies with related bills, there appears to be minimal impact on estimates of direct treatment effects. Legislative experiments can include multiple bills to improve power without causing bias due to across-bill spillover.

That policy expertise spills over between bills chosen ex ante for their substantive similarity provides additional evidence that persuasion is a real, information-driven phenomenon. Persuasion does not spill over between bills, even in the same issue area, that address different policy problems. Only when the content of the message is relevant to other bills is there spillover. This finding suggests that persuasion is about information, rather than alternative mechanisms like pressure from an interest group. The next section further examines whether receiving a policy briefing from an interest group changes a legislators' behavior broadly on bills relevant to that interest.

Crossover Effects of Policy Expertise from Zelizer (2019)

This section provides more tests of persuasion spillover across bills in the same substantive issue area. The prior section examined crossover between pairs of bills, but there are other ways that persuasion may spread. Perhaps simply meeting with the staffer for the legislative veterans caucus made legislators more likely to support all veterans bills. If that were the case, the prior section would not be able to recover this type of persuasion, because all subjects met with the staffer.

This section uses data from Zelizer (2019) to examine broad persuasion spillover. Unlike Zelizer (2018), Zelizer (2019) includes a pure control group of legislators who were not treated on any bills; this pure control condition was assigned at random across legislators. This control group allows us to estimate persuasion spillover contemporaneously — by asking whether meeting with the staffer increased (or decreased) support for bills for which legislators were not actually briefed — and downstream — since treatment was randomly assigned across legislators, we can simply compare support for veterans bills between legislators who met with the staffer and those who did not.

Table 8 shows the estimated contemporaneous direct and crossover effects of policy research in the 2016 experiment on bill cosponsorship based on the following equation:

$$Y_{ib} = \beta_0 + \beta_1 d_{ib} + \beta_2 max(d_i) + u_{ib} \tag{2}$$

It includes two estimands: β_1 is the conventionally-estimated effect of a bill briefing on a legislator's support for that specific bill; β_2 is the effect of briefing the legislator *at all* on their average support for all the bills. One can interpret β_2 as the relative difference in legislators' support for untreated bills between legislators who were, and were not, treated on any bills in the study.

The first column displays the estimated effect of assigning a bill to treatment (β_1) among legislators who are ever treated; the estimated effect of treating a legislator on a specific

bill are 12.1 percentage points ($\widehat{SE} = 4.5$). Treatment increased cosponsorship rates by approximately 70% over baseline cosponsorship rates of 17%.

The second column displays the estimated effect of assigning a legislator to treatment compared to assigning that legislator to pure control (β_2). Assigning a legislator to treatment appears to have exerted little impact beyond the bill treatment: the estimated effect is -2.3 percentage points (2.6). This estimate is smaller in magnitude than the direct effect of treatment, opposite signed, and short of conventional levels of statistical significance. It does not appear that simply meeting with a legislator causes them to broadly change their support of the issue in question.[8]

Table 8: **Estimated Crossover Effects from Zelizer (2019) (in pp)**

	β_1 (Bill Treated)	β_2 (Legislator Treated)
$\widehat{\text{ITT}}$	12.1**	-2.3
($\widehat{\text{SE}}$)	(4.5)	(2.6)
N	912	
$\overline{Y}_{\text{control}}$	17.0	

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

With the legislator-level randomization, we can estimate whether meeting with the staffer influenced legislators' support of veterans' bills in future sessions as well. Forty-eight of the 57 legislators in the 2016 study returned in the 2017-2018 session; 36 returned in the 2019-2020 session. Were legislators who were briefed in 2016 significantly more (or less) likely to support veterans legislation in ensuing sessions?

Table 9 shows the effects of legislators being briefed at all on their support for all veterans bills in the two assemblies following the 2016 experiment. The dependent variable is each legislator's average cosponsorship across all veterans bills filed during that timeframe. The first column displays results for the 41 veterans bills filed in the 2017-2018 session; the second

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[8]Appendix A shows that the estimate of β_1 is sensitive to the inclusion of bill fixed effects. The estimate of β_2 is consistently close to zero and short of statistical significance.

Table 9: **Estimated Downstream Crossover Effects from Zelizer (2019) (in pp)**

	2017-2018	2019-2020	2017-2020
$\widehat{\text{ITT}}$	0.5	-1.7	-0.6
$(\widehat{\text{SE}})$	(2.5)	(1.3)	(1.8)
N Legislators	48	36	48
$\overline{Y}_{\text{control}}$	16.1	4.8	12.4

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

column the 31 bills filed in the 2019-2020 session; and the third column all 72 bills filed from 2017-2020.[9]

The estimated effects of briefing a legislator in 2016 on their support of veterans-related bills in the future ranges from -1.7 — 0.5 percentage points; the average estimated effect for the entire period is -0.6 percentage points ($\widehat{SE} = 1.8$). These estimates are small both in absolute terms and relative to sampling variability. The analysis cannot rule out modest but meaningful changes in legislators' downstream support for veterans' issues, but it finds little support for policy expertise generally influencing legislators' policy positions downstream of treatment.

Why might this experiment not recover evidence of downstream persuasion due to policy briefings? One reason is the experimental design. This analysis relied on across-legislator variation. Due to the limited, and relatively small, number of members of this legislature who were enrolled in the experiment, such approaches are less powerful than bill-by-bill tests of information spillover, like those in the prior two sections. Another explanation is the nature of the treatment. Policy expertise is a relatively bill-specific treatment. If the intervention had been a general request to be more supportive of veterans bills — perhaps the kind of demand that a veterans group would make — we might be more likely to see downstream

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[9]Since some legislators did not serve two additional terms after the experiment, the analysis for the "2017-2020" column includes varying numbers of veterans bills in the denominator for each legislator. It can be interpreted as the cumulative effect of information on legislators over their ensuing tenure in office. All analyses assume that legislators' tenure is unrelated to their treatment status in the experiment.

influence. But with this treatment and research design, there is little evidence that policy expertise spills over to broadly influence legislators' positions in a given issue area.

Crossover Effects of Constituency Opinion from Butler and Nickerson (2011)

Since policy expertise appears to influence only bills that are strongly related in content — up to and including those that are identical — this section examines an intervention that provides a more general persuasive treatment: information about constituents' policy preferences.

Butler and Nickerson (2011) (hereafter BN) provide legislators with results from a public opinion poll that asked constituents how the state should respond to the unexpected tightening of the budget. The poll asked specifically about one spending bill under consideration in 2008, SB 24, with the following language:

> During the session proposals for spending on road construction projects and tax rebate checks will be considered. The funding for these proposals was expected to come from windfall revenues on state oil and gas. However, recent drops in the price of oil and gas means the windfall may not be as large as expected. Our last question is how you would like the legislature to proceed on these proposals? (Butler and Nickerson 2011, p. 77)

The two funding items included in the poll are road construction and tax rebates. The poll's response options included to pass the original funding plan, authorize a revised, lower level of funding, or not fund the proposals at all.

Legislators faced a similar dilemma in 2009 due to the Great Recession. During the 2009 regular session, legislators passed a General Appropriations Act (referred to as House Bill 2) that cut appropriations by 9% from forecasted levels (New Mexico Legislative Council Service 2009). Legislators also passed an omnibus capital investment bill (House Bill 154) that substantially cut investments in local projects to match the state's lower bonding capacity.

Despite these cuts, legislators were called into special session in October 2009 to further address budget shortfalls. Legislators were given the option by the Governor's proclamation[10] to fix the gap by either transferring money from reserves to the general fund, cutting spending, or issuing long-term bonds to replenish the general fund. These bills, along with those considered during the 2009 regular session, are described in Table 10.

Table 10: **Description of New Mexico's 2009 Budget Bills**

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 to appropriations and capital investments**.	Passed House, not Senate.
HB 28 (S)	Issue bonds, transfer proceeds to general fund.	Passed House, not Senate.

I examine whether learning public opinion about spending policy has persistent effects on legislators' votes. One could argue that we should not expect BN's treatment to have influenced votes other than the one targeted. The chief argument against persistent persuasion is that BN's treatment targeted a specific bill, SB 24, so it did not communicate meaningful information on other bills. The exact content of SB 24, a mix of tax cuts and capital outlays, was not duplicated in bills introduced in 2009. SB 24 was a highly salient bill considered shortly before a general election; by 2009 legislators had faced the electorate and would not do so again for another year or more. Further, BN's treatment mentioned that they were working with a local newspaper, the *Sante Fe New Mexican*, which published

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[10]The Governor of New Mexico, as in many other states, has substantial authority to set the agenda for special sessions, and Governor Bill Richardson refused to consider tax raises to address the budget shortfall.

the statewide results of the survey. It's possible that legislators thought that they were being monitored by the academics, the newspaper, or both. This fear of monitoring likely would have diminished by the 2009 sessions.

Nevertheless, there are reasons why BN's treatment may have persisted and influenced legislators' subsequent voting behavior. The main argument for persistence would be that legislators learned something about the fiscal preferences of their constituents — indeed learned so much that some legislators became dramatically less likely to support what they thought was a popular bill — and that knowledge is transferable. Legislators may have thought that voters always want tax cuts and spending, but BN's survey informed some legislators (those whose districts least supported the proposal) that voters actually preferred to be fiscally responsible. That knowledge would be directly applicable to the suite of bills that intended to address the 2009 budget gap.

A second reason to think the treatment would last is that it was a partisan one. Table 11 reproduces the analysis from BN 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 nearly 30 percentage points less likely to vote for the bill.

Column (2) in Table 11 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 support among both the public and legislators was partisan (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. And all of the bills under consideration in 2009 were proposed by (and passed with the votes of) New Mexico Democrats.

Table 11: **Estimated Contemporary Effects from Butler and Nickerson (2011) (in pp)**

	(1)	(2)
Treatment	-1.9	-4.4
$\widehat{SE}$	(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)
Republican x Low Spending	-	-30.3*
	-	(16.8)

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

Table 12 estimates the effects of BN's intervention on legislators' voting on 2009 budget bills. The first seven columns show estimated effects for each of the seven bills in Table 10. The last column aggregates all seven bills into an index that ranges from 0-1 and indicates the percentage of the bills supported by each legislator.[11] Each bill was highly partisan; Republicans were 70 – 94 percentage points less likely to support bills than Democrats in the legislature.

There is little evidence that the 2008 treatment from BN influenced legislators in 2009, six to twelve months after it was provided. Estimated average treatment effects are close to zero; on average across the seven bills, treatment decreased roll call support by 0.4 percentage

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[11]Legislators who were absent or abstained from voting are omitted from the index and from each of the seven bill-specific analyses.

points ($\widehat{SE} = 4.3$). There is also little evidence of heterogeneous treatment effects by voters' support of the 2008 spending levels; the estimated interaction effect between treatment and low support for spending in 2008 made legislators 4.2 percentage points more likely ($\widehat{SE} = 4.3$) to support the budget bills on average. The one bill with the largest estimated heterogeneous treatment effects is HB 154, which cut capital spending. Although this bill is relatively close in content to the original 2008 bill (which included spending on road construction), the direction of the estimated effect is opposite the one from 2008. Treatment made legislators less supportive of cutting spending in 2009, while they were less supportive of adding spending in 2008.

Table 12: **Estimated 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
($\widehat{SE}$)	(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.

The most likely explanation for the lack of influence is that the treatment was not relevant to these bills. Even that is informative, however, about the nature of persuasion. Butler and Nickerson provided a novel piece of information about constituents' preferences over budget policy that dramatically changed legislators' voting behavior. However, that information was not relevant or influential less than a year later when they considered legislation that was as similar as any legislators had considered since the experiment. Although there are many differences between this study and Zelizer (2018) beyond the nature of the intervention, the

fact that policy expertise persisted to influence behavior but political intelligence did not suggests legislators are more likely to recall, and be influenced by, technical expertise than political pressure.

Discussion

These four analyses looked for evidence of persuasion where we might expect to see it if the original interventions genuinely changed legislators' beliefs or preferences over policies. In the two places where we would arguably be most likely to see persuasion — on refiled versions of experimental bills and on contemporaneous legislation with related content to treated bills — we see evidence of persuasion. For the two more difficult tests — on contemporaneous and downstream bills in the same issue area, but less related to the original legislation — there is little evidence of persuasion. These latter tests are also less powerful, since they randomize treatment across legislators. Taken together, it is striking that we see influence of information even on bills other than those directly examined by the original studies.

These results begin to fill out our understanding of the mechanisms behind legislative persuasion. Persuasion was robust when legislators were provided technical expertise about bills; it lasted for years and crossed over to bills with related content. Legislators did not respond to a meeting with a special interest by simply supporting all of their bills, nor to a poll of constituents by persistently changing their voting behavior. This suggests that legislators are trying to learn about the content of proposals, recall what they are given, and rely on this information when deciding what position to take. It is a relatively sanguine view of legislative persuasion.

These findings also relate to theories of persuasion from other fields. Political psychology and behavioral theories of persuasion focus on the persistence of treatment effects to differentiate mechanisms (Chaiken 1980; Petty and Cacioppo 1986; Fazio and Towles-Schwen 1999). While persuasion may occur through informational channels, i.e. when an individual consciously considers and deliberates on a message, or through more behavioral channels, i.e.

nudges, such that the individual devotes little thought to the matter, informational persuasion is thought to be more robust and persistent (Coppock 2016; Baden and Lecheler 2012; Chen and Chaiken 1999; Brandon et al. 2017; Crano and Prislin 2006). Thus this paper is more consistent with legislative persuasion happening more through conscious deliberation than behavioral nudging.

At the same time (but in a quite different literature), formal models of legislatures have examined the transferability of policy information (Austen-Smith and Riker 1987; Gilligan and Krehbiel 1987; Callander 2008; Hirsch and Shotts 2012). Whether information is transferable or policy-specific plays an important role in whether specialization and delegation improve, or actually diminish, the informational efficiency of legislating. I find that information is largely confined in influence either to the bill targeted, or bills with very similar content, up to and including refiled versions of the exact same bill.

This study takes seriously the limits to legislative experiments. Small sample sizes limit statistical power, and including multiple bills adds observations but risks introducing across-bill interference. As a result, findings from legislative experiments may not be as robust or conclusive as those from other fields. I find these worries to be somewhat overblown. Extending the original research designs to analyze related outcomes adds further support for the persuasiveness of the original interventions, within limits. One of these analyses looks for, and finds, across bill interference, but even detectable levels of spillover do not substantially bias treatment effect estimates from the original study.

Legislative scholars have been slow to incorporate some theories of persuasion into the analysis of legislative behavior, but I hope this paper will encourage efforts to study position-taking in this way. Legislators, just like voters in the electorate or bureaucrats or other political actors, can be persuaded by information. Knowing how, why, or via what messages they are persuaded to adopt specific policy positions is central to many normative concerns about the performance of contemporary legislatures. 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, and there is much to be learned from a research program on legislative persuasion.

Appendix A: Robustness Checks

Table A1: **Estimated Downstream Effects from Zelizer (2018) with Bill FEs (in pp)**

	Cosponsorship	Roll Call Vote
$\widehat{\text{ITT}}$	4.0	4.5*
$(\widehat{\text{SE}})$	(5.1)	(2.2)
N	405	405

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

Table A2: **Estimated Crossover Effects from Zelizer (2019) with Bill FEs (in pp)**

	β_1 (Bill Treated)	β_2 (Legislator Treated)
$\widehat{\text{ITT}}$	4.0	-0.3
$(\widehat{\text{SE}})$	(3.2)	(1.9)
N		912
$\overline{\text{Y}}_{\text{control}}$		17.0

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

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