Supplementary Materials for "Is Position-Taking Contagious? Evidence of Cue-Taking from Two Field Experiments in a State Legislature"

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Contents

1. Appendix A: Robustness Checks

Table A1: **Estimated ITTs for Study 2 (in percentage points).**

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

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

Table A2: **Estimated ITTs Excluding Legislator Fixed Effects (in percentage points).**

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

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

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

Table A3: **Estimated ITTs for Study 1, Excluding Legislator Fixed Effects (in percentage points).**

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

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

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

Table A4: **Estimated ITTs in One-Person Offices (in percentage points).**

	Study 1	Study 2	Combined
Briefing $\widehat{\text{ITT}}$	−5.3	5.7	1.1
$(\widehat{\text{SE}})$	(5.3)	(6.5)	(4.7)
N	128	304	432

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

2. Appendix B: Construction of Alternative Spillover Models

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

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

DW-NOMINATE ideology scores were constructed based on legislators' roll call voting during the first session.[1] Legislators were paired based on their first and second dimension ideology scores, again through an algorithm that minimized the aggregate distance within pairs.[2]

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

Spillover Across Bills

Another form of treatment contagion relates not to spillover across legislators, but instead within legislator. Briefing treatments might diffuse across bills. Briefings on one bill may

[1] Using roll call voting from the session during which Study 1 was implemented maximizes the number of legislators with valid ideology scores. This covariate is post-treatment for Study 1 and pre-treatment for Study 2. There is little reason to think treatment on the limited number of bills in these studies, many of which did not receive a vote and nearly all that did having passed unanimously, influenced legislators' DW-NOMINATE scores.

[2] Bowers, Frederickson, and Panagopoulos (2013) and Coppock (2014) utilize more complex models of treatment spillover across ideology networks. This simple two-person model is used to maintain parallelism with the other models.

convey information that is relevant to other bills, or legislators may have a budget for cosponsorships such that supporting one bill affects the likelihood they support another. Interdependence of voting or cosponsorship decisions across bills raises many interesting questions. For the purposes of this paper, the most relevant question is whether across-bill interference undermines our primary conclusions about across-legislator interference.

Introducing a model of decision-making that allows spillover across legislators and bills is beyond the scope of this paper. As a result, we look for within-legislator spillover among a limited group of observations from the experiments: those that were not assigned to either briefing or cue-taking treatments. We ask whether there is systematic variation in cosponsorship among these pure control group observations that were not, in our primary treatment contagion model, exposed to treatment.

The first way we test for across-bill spillover is to look for bill-by-bill spillover effects. In the experiments, some legislators were not assigned to any treatment for Bill 1. Among this group, some *were* assigned to treatment for Bill 2 while others were not. One test of across-bill spillover is whether cosponsorship on Bill 1 depended on treatment assignment to Bill 2. Among control group observations, does assignment to treatment for *other bills* influence legislators' cosponsorship behavior?

With only two bills in the study, it would be relatively straightforward to estimate this across-bill treatment effect. With 32 bills across two time periods, it is more complicated. We believe the best approach is first to estimate the following equation to gauge whether treatment assignment for legislators in Study 1[3] to Bill 1 influenced their cosponsorship of all bills other than Bill 1 in that study:

$$Y_{ib} = \beta_0 + \beta_1 d_{i1}^{10} + u_{ib} \quad \forall\ b \in 2, 3, ..., 16 \tag{1}$$

[3]Including legislators in one-person suites. Since we are not interested in across-legislator spillover, one-person suites can be included in this analysis. Results are similar if one-person suites are excluded.

Then Equation 2 can be fit fifteen more times for the remaining bills in Study 1, before the whole exercise is repeated for Study 2.[4] Repeatedly fitting this model raises several questions. The results from the sixteen regressions for each study are not independent. They rely on partitions of the same sample. Further, the regression model allows contagion from one bill at a time, but there may be contagion from multiple bills at once. Although there are valid questions about how much we can learn about spillover across bills from this approach, we should at least look for any evidence that might suggest within-legislator contagion.

Figures B1 and B2 show the estimated effect of assigning the specified bill to treatment on cosponsorship rates of other bills. Of the thirty-two bill-specific estimates, only one falls far from the mean of the sampling distribution generated under the assumption of no across-bill spillover effects. Even this bill, Bill 8 in Study 1, falls short of the conventional 5% level of statistical significance.

Another way to estimate within-legislator spillover effects independent of across-legislator effects is to examine legislators in one-person offices. In Study 2, legislators in one-person suites were assigned to the caucus staffer, advocate, or no treatment condition (again let us set aside the advocate treatment condition). Then, legislators selected for the staffer treatment were assigned to be briefed on four of the sixteen possible bills. If there is no across-bill effect from the staffer briefing, cosponsorship rates should be the same, on average, for these twelve untreated bills as they are for legislators who are assigned to the full no treatment condition for all sixteen bills.

The regression model to test this form of within-legislator spillover is the following:

[4]This assumes no within-legislator spillover across the studies, which occured approximately one year apart. Bill-specific fixed effects and indicator variables indicating whether legislators inhabited two- or three-person suites are also included in the regression, but not displayed. Conditional on office size, treatment assignment probabilities are equal across units. This approach gives similar results to weighted least squares regression with inverse probability weights. We are interested in within-legislator spillover of the briefing treatment, but Equation 2 must also account for assignment to cue-taking or advocate treatments, although these treatments are not displayed.

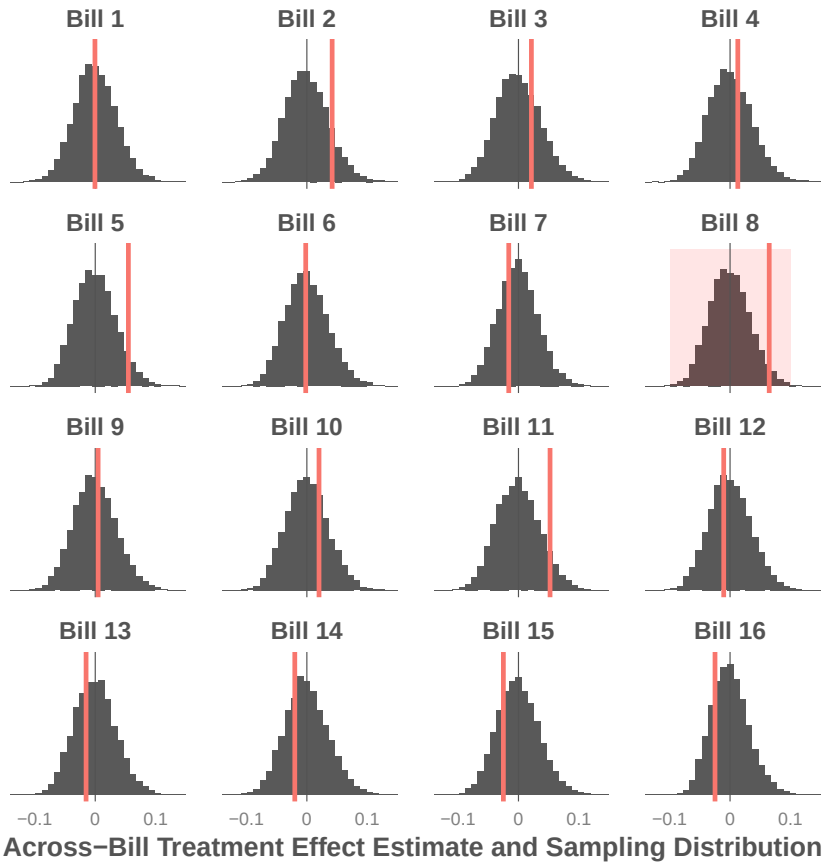

Figure B1: **Bill Spillover in Study 1. Thick, red lines indicate bill-specific treatment effect estimates. Histograms reflect the sampling distribution from 10,000 simulated random assignments. Treatment effect estimates that are significant at $p < 0.1$ two-tailed are indicated by the shaded background.**

$$Y_{ib} = \beta_0 + \beta_1 \, d_i^{10} + u_{ib} \tag{2}$$

The weaknesses of this model are that the treatment indicator is clustered at the legislator level and that there are only 19 legislators in one-person suites in Study 2. Nevertheless, there are 248 observations in the 19 clusters, allowing us to estimate effects with at least a minimal degree of power.

Among legislators assigned to the no treatment condition, average cosponsorship rates

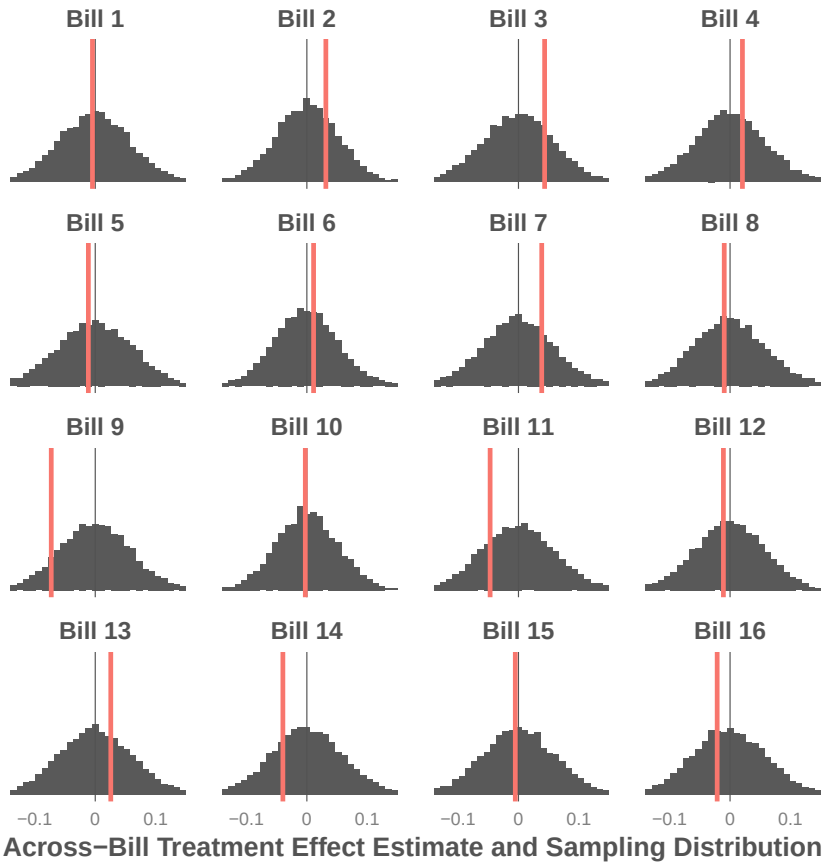

Figure B2: **Bill Spillover in Study 2. Thick, red lines indicate bill-specific treatment effect estimates. Histograms reflect the sampling distribution from 10,000 simulated random assignments. Treatment effect estimates that are significant at $p < 0.1$ two-tailed are indicated by the shaded background.**

of untreated bills was 15%. Among those in the staffer treatment condition, untreated bills were cosponsored at an 11% rate. The difference-in-means treatment effect estimate is 4.2 percentage points. From 10,000 simulated random assignments, the estimated standard error of this treatment effect estimate is 5.8 percentage points and estimated two-tailed p-value 0.46.

3. Appendix C: Why Information Influences Position-taking

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

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

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

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

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

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

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

Suppose legislators support a bill if their utility exceeds a critical threshold, u^* (Peress 2013).[5] Support could mean voting for the bill or choosing to cosponsor it. The legislator's probability of supporting the bill can be given by a random utility choice model that allows bill support to be increasing in utility with a particularly large increase when utility approaches the threshold:

[5]This threshold could also be the utility from a status quo policy.

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

In this framework, information can influence support via utility in two ways. It can reduce uncertainty ($Var(x_p)$) or correct a prior expectation ($\bar{x}_p$). Reducing uncertainty can only increase utility, so we expect informational briefings to increase average cosponsorship.

4. Appendix D: Timing of Combined Treatment Effects

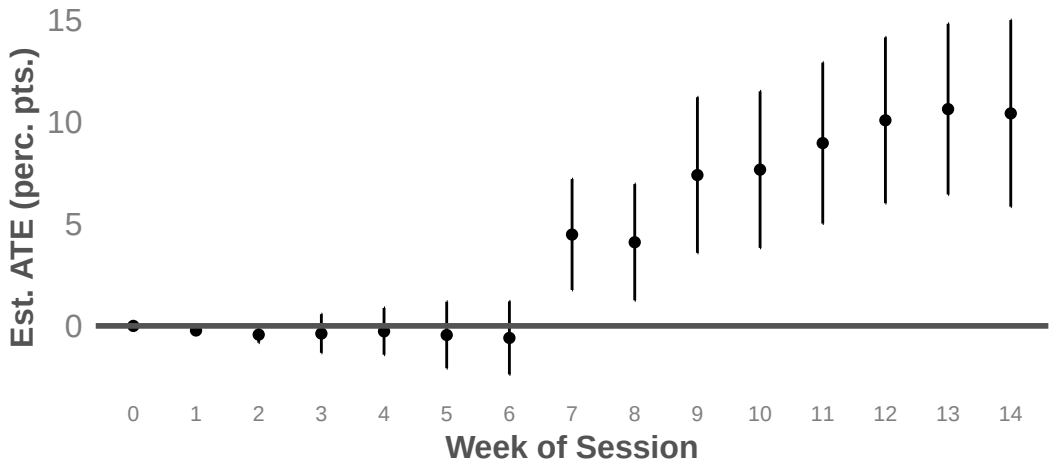

Figure D1: **Combined treatment effects by date of cosponsorship.**