Supplementary Materials

Can Elites Escape Blame by Explaining Themselves?
Suspicion and the Limits of Elite Explanations

Contents

Online Appendix A: In-Text Models

Table OA1: Sample Characteristics

	Experiment 1 (MTurk)	**Experiment 2 (MTurk)**	**Experiment 3 (Lucid)**	**ANES 2016**	**ANES 2018 Pilot**
% Female	53.64	50.56	50.97	51.97	51.23
Age					
Mean	38.03 (12.29)	37.66 (12.45)	52.21	47.37 (SE: 0.36)	47.16 (SE:0.47)
18-24	8.98	10.48	11.24	11.98	10.62
25-34	39.70	40.26	13.42	16.53	19.43
35-44	25.06	24.07	14.26	14.52	16.62
45-54	12.67	12.10	9.65	17.27	16.99
55-64	9.31	8.81	9.31	19.48	17.23
65+	4.30	4.28	42.11	20.23	19.11
Education					
<HS	0.33	0.56	3.80	9.1	12.72
HS	9.88	10.07	28.02	28.86	27.5
Some College/Associates	37.53	37.44	35.12	30.15	31.0
Bachelor	37.69	37.31	20.58	18.76	18.2
Post-Bachelor	14.57	14.62	12.48	13.12	10.6
Race/Ethnicity					
White	77.35	73.15	72.55	69.71	63.96
African American	7.20	8.95	10.70	10.99	12.03
Hispanic	7.31	9.76	10.20	11.91	15.91
Asian	6.04	6.15	3.57	2.8	5.31
Other	2.10	1.99	2.99	4.59	2.78
Household Income (Median Category)	$50,000 to $59,999	$50,000 to $59,999	$40,000 to $49,999	$55,000-$59,999	$40,000 - $49,999
PID					
Mean	3.48 (2.00)	3.33 (2.06)	3.86 (2.27)	3.77 (SE:0.04)	3.74 (se: 0.06)
% Democrat	54.69	59.63	44.27	46.68	46.13
% Republican	30.54	30.19	38.75	39.49	35.53
% Pure Independent	14.77	10.19	16.98	13.82	18.33
Symbolic Ideology [a]					
Mean	3.55 (1.73)	3.47 (1.77)	3.97 (1.88)	4.15 (SE: 0.04)	3.93 (SE: 0.05)
% Liberal	48.34	53.72	35.29	32.1	35.63
% Moderate	22.59	16.98	28.58	26.87	30.6
% Conservative	29.08	29.3	36.12	41.03	33.78

Notes: ANES estimates are based on weighted analyses. [a] The MTurk and ANES TS ideology measures use the same fully labeled scales with "extremely" liberal/conservative as the ends and moderate as the middle. The Lucid item uses the same wording as the ANES 2018, which asks whether one is very liberal, somewhat liberal, closer to liberals, neither liberal nor conservative, closer to conservatives, somewhat conservative, or very conservative.

Table OA2: Experiment 1 Models

	(1) Figure 1	(2) Figure 2	(3) Figure 3
No Information Baseline	-0.12**	-0.12**	-0.05+
	(0.02)	(0.02)	(0.03)
No Justification	-0.16**	-0.16**	-0.16**
	(0.02)	(0.02)	(0.03)
Justification w/Counter	-0.19**		
	(0.02)		
Teacher Counter		-0.20**	-0.22**
		(0.02)	(0.03)
Non-Partisan Counter		-0.22**	-0.25**
		(0.02)	(0.03)
Partisan Counter		-0.16**	-0.13**
		(0.02)	(0.03)
Legislator Partisanship			
Democratic Legislator	0.11**	0.11**	
	(0.01)	(0.01)	
Respondent Partisanship			
Opposing Partisan			-0.20**
			(0.03)
Resp=Ind.			-0.06
			(0.04)
Interactions			
No Information Baseline # Opposing Partisan			-0.14**
			(0.04)
No Information Baseline # Resp=Ind.			-0.12*
			(0.06)
No Justification # Opposing Partisan			0.00
			(0.04)
No Justification # Resp=Ind.			-0.08
			(0.06)
Teacher Counter # Opposing Partisan			0.06
			(0.04)
Teacher Counter # Resp=Ind.			-0.06
			(0.06)
Non-Partisan Counter # Opposing Partisan			0.07+
			(0.04)
Non-Partisan Counter # Resp=Ind.			-0.03
			(0.06)

Partisan Counter # Opposing Partisan			-0.01 (0.04)
Partisan Counter # Resp=Ind.			-0.08 (0.05)
Constant	0.59** (0.01)	0.59** (0.01)	0.75** (0.02)
Observations	1813	1813	1813
Adjusted R^2	0.119	0.123	0.231

Standard errors in parentheses; (Baseline Categories: Justification Condition; Republican Legislator [Models 1 & 2] or Co-Partisan Legislator [Model 3])

$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

Table OA3: Experiment 2 Models

	(1) Fig 1 (In Text)	(2) Fig 1 (No Control)	(3) Fig 2 (In Text)	(4) Fig 2 (No Control)	(5) Fig 3 (In Text)	(6) Fig 3 (No Control)
No Justification	-0.01	-0.00	-0.01	-0.00	-0.01	-0.01
	(0.01)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)
Just. w/Counter	-0.04**	-0.04**				
	(0.01)	(0.01)				
HC Counter			-0.05**	-0.05**	-0.07**	-0.06**
			(0.01)	(0.02)	(0.02)	(0.02)
LC Counter			-0.02*	-0.03	-0.03*	-0.05*
			(0.01)	(0.02)	(0.02)	(0.02)
Republican Legislator	0.01	-0.03**	0.01	-0.03**		
	(0.01)	(0.01)	(0.01)	(0.01)		
Thermometer (Pre-Test)	0.73**		0.73**		0.73**	
	(0.02)		(0.02)		(0.02)	
Partisanship						
Opposing Partisan					-0.00	-0.16**
					(0.02)	(0.02)
Independent					-0.04	-0.16**
					(0.03)	(0.04)
Interactions						
No Justification # Opposing Partisan					-0.02	0.01
					(0.02)	(0.03)
No Justification # Independent					0.06	0.06
					(0.04)	(0.06)
HC Counter # Opposing Partisan					0.02	0.03
					(0.02)	(0.03)
HC Counter # Independent					0.06+	0.03
					(0.04)	(0.05)
LC Counter # Opposing Partisan					0.01	0.04
					(0.02)	(0.03)
LC Counter # Independent					0.05	0.08
					(0.04)	(0.06)
Constant	0.09**	0.49**	0.09**	0.49**	0.10**	0.56**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
Observations	1610	1610	1610	1610	1609	1609
Adjusted R^2	0.577	0.010	0.578	0.011	0.578	0.088

Standard errors in parentheses; Baseline Categories: Justification (all models); Democratic Legislator (model 1-4); Co-Partisan legislator (model 5-6); $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

Table OA4: Experiment 3 Models, Figure 1 Analyses

	(1) Gillibrand	(2) Gillibrand	(3) Corker	(4) Corker
No Justification	-0.06*	-0.07**	-0.01	-0.00
	(0.02)	(0.02)	(0.02)	(0.02)
Just. w/Counter	-0.04+	-0.04*	-0.04*	-0.05*
	(0.02)	(0.02)	(0.02)	(0.02)
Prior Politician:				
Unfavorable		-0.26**		-0.19**
		(0.02)		(0.02)
No Opinion		-0.13**		-0.11**
		(0.02)		(0.02)
Co-Partisanship:				
Opposing Partisan		-0.06**		-0.04+
		(0.02)		(0.02)
Independent		-0.09**		-0.06*
		(0.02)		(0.02)
Issue Proximity:				
Lose Proximity		-0.19**		-0.13**
		(0.02)		(0.02)
No Attitude		-0.10**		-0.07**
		(0.03)		(0.03)
Treatment Order:				
Gillibrand First		0.00		0.00
		(0.01)		(0.02)
Constant	0.50**	0.73**	0.46**	0.66**
	(0.02)	(0.02)	(0.02)	(0.02)
Observations	1211	1201	1212	1200
Adjusted R^2	0.003	0.286	0.003	0.160

Standard errors in parentheses

Baseline Categories: Justification (all models); Favorable Prior; Gained Proximity; Co-Partisan; Corker First

$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

Table OA5: Experiment 3 Models, Figure 2 Analyses

	(1) Gillibrand	(2) Gillibrand	(3) Corker	(4) Corker
No Justification	-0.06*	-0.07**	-0.01	-0.00
	(0.02)	(0.02)	(0.02)	(0.02)
LW Counter	-0.03	-0.04+	-0.02	-0.03
	(0.02)	(0.02)	(0.02)	(0.02)
RW Counter	-0.05*	-0.05*	-0.06**	-0.07**
	(0.02)	(0.02)	(0.02)	(0.02)
Prior Politician:				
Unfavorable		-0.26**		-0.19**
		(0.02)		(0.02)
No Opinion		-0.14**		-0.11**
		(0.02)		(0.02)
Co-Partisanship:				
Opposing Partisan		-0.06**		-0.03+
		(0.02)		(0.02)
Independent		-0.09**		-0.06*
		(0.02)		(0.02)
Issue Proximity:				
Lose Proximity		-0.19**		-0.13**
		(0.02)		(0.02)
No Attitude		-0.10**		-0.07**
		(0.03)		(0.03)
Treatment Order:				
Gillibrand First		0.00		0.00
		(0.01)		(0.02)
Constant	0.50**	0.73**	0.46**	0.66**
	(0.02)	(0.02)	(0.02)	(0.02)
Observations	1211	1201	1212	1200
Adjusted R^2	0.003	0.286	0.004	0.162

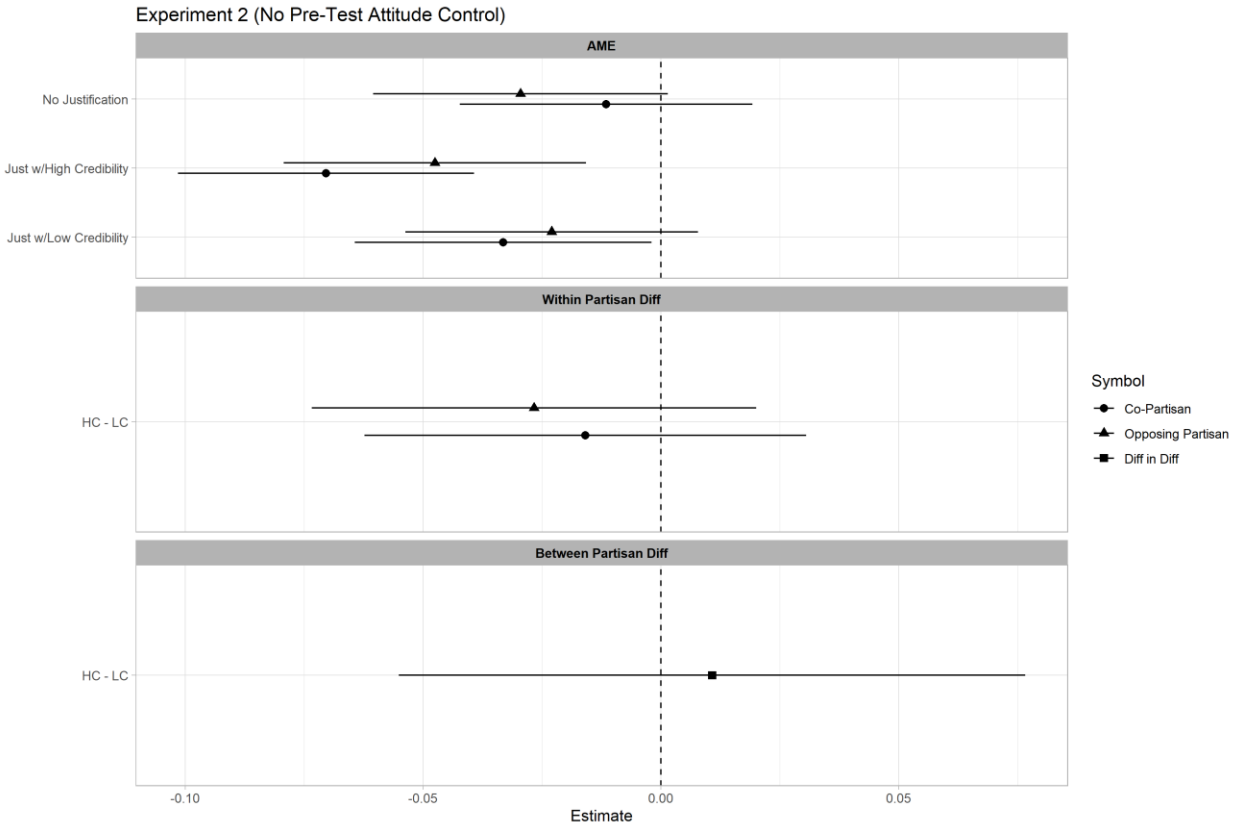
Standard errors in parentheses
Baseline Categories: Justification (all models); Favorable Prior; Gained Proximity; Co-Paritsan; Corker First
+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

Table OA6: Experiment 3 Models, Figure 4 Analyses

	(1) Gillibrand	(2) Gillibrand	(3) Corker	(4) Corker
No Justification	-0.10**	-0.08*	-0.05	-0.05
	(0.03)	(0.03)	(0.04)	(0.03)
LW Counter	-0.03	-0.03	-0.07*	-0.07*
	(0.04)	(0.03)	(0.03)	(0.03)
RW Counter	-0.06+	-0.06+	-0.08*	-0.08*
	(0.04)	(0.03)	(0.04)	(0.03)
Co-Partisanship				
Opposing Partisan	-0.23**	-0.07*	-0.21**	-0.09**
	(0.04)	(0.03)	(0.03)	(0.03)
Independent	-0.19**	-0.08+	-0.14**	-0.06
	(0.05)	(0.04)	(0.05)	(0.05)
Interaction Terms				
No Justification # Opposing Partisan	0.05	0.01	0.11*	0.11*
	(0.05)	(0.05)	(0.05)	(0.05)
No Justification # Independent	0.06	0.01	-0.01	0.02
	(0.07)	(0.07)	(0.06)	(0.06)
LW Counter # Opposing Partisan	0.02	0.01	0.10*	0.09+
	(0.05)	(0.05)	(0.05)	(0.05)
LW Counter # Independent	-0.02	-0.05	0.01	0.01
	(0.07)	(0.06)	(0.07)	(0.06)
RW Counter # Opposing Partisan	0.02	0.01	0.06	0.03
	(0.05)	(0.05)	(0.05)	(0.05)
RW Counter # Independent	0.02	-0.00	0.01	-0.00
	(0.07)	(0.06)	(0.06)	(0.06)
Politician Prior				
Unfavorable		-0.26**		-0.19**
		(0.02)		(0.02)
No Opinion		-0.14**		-0.11**
		(0.02)		(0.02)
Proximity Status				
Lose Proximity		-0.19**		-0.13**
		(0.02)		(0.02)
No Attitude		-0.10**		-0.07**
		(0.03)		(0.03)
Treatment Order				
Gillibrand First		0.01		0.00
		(0.01)		(0.02)
Constant	0.62**	0.73**	0.58**	0.69**
	(0.03)	(0.03)	(0.02)	(0.03)
Observations	1210	1201	1211	1200
Adjusted R^2	0.103	0.283	0.066	0.163

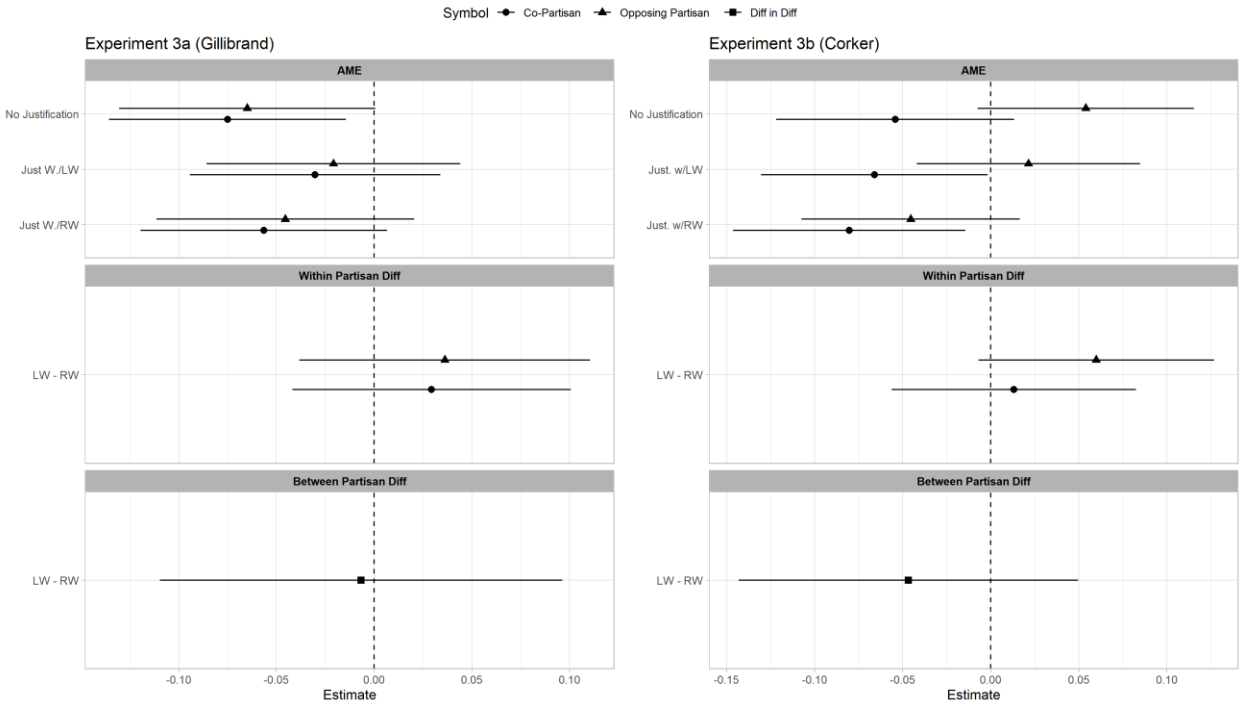
Standard errors in parentheses
Baseline Categories: Justification (all models); Favorable Prior; Gained Proximity; Co-Paritsan; Corker First
$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

Figure OA1: Figure 3 (Experiment 2) Analyses sans Controls



Notes: The top facet provides the average difference in evaluations based on treatment condition with separate markers for co-partisans (circle) and opposing partisans (triangles). The middle facet provides the difference *within* partisan group between the counter-explanation coefficients. The final facet provides a difference in difference.

Figure OA2: Figure 4 Analyses sans Controls



Notes: The top facet provides the average difference in evaluations based on treatment condition with separate markers for co-partisans (circle) and opposing partisans (triangles). The middle facet provides the difference *within* partisan group between the counter-explanation coefficients. The final facet provides a difference in difference (e.g., Co-Partisan[LW-RW]– Opposing Partisan[LW-RW]).

Justifications, Our Study and the Literature

One intriguing question raised by a reviewer concerns the comparability of the effects of the justifications in our study with those of the broader literature. Do we find significantly weaker effects of justifications than existing work?

We begin to address this question by comparing the effect sizes of receiving a justification (versus receiving none) from our four experiments with those found in experiments in Levendusky and Horowitz (2012), the two studies (involving four explanation treatments) reported in Robison (2017), and from Grose et al. (2015). We focus on these articles for pragmatic purposes. First, they each contain a no justification counter-factual – this is not true, for instance, of Butler and Broockman's (2017) study where individuals were not informed of the politician's disagreeable policy position sans justification. Second, these studies all make their underlying data publicly available which enabled us to calculate effect sizes. Perhaps the obvious omission here are the landmark studies by McGraw and her co-authors as these unfortunately neither make available the underlying data nor report the statistics needed to calculate effect sizes (e.g. mean evaluations and standard deviations by treatment condition) given their focus on the effects of account satisfaction (McGraw 1990, 1991; McGraw, Best, and Timpone 1995; McGraw, Timpone, and Bruck 1993).[1] Likewise, Peterson and Simonovits (2017) are missing from below for similar reasons. This is thus obviously not a systematic meta-analysis and should not be read as such, but rather as a first look at this literature.

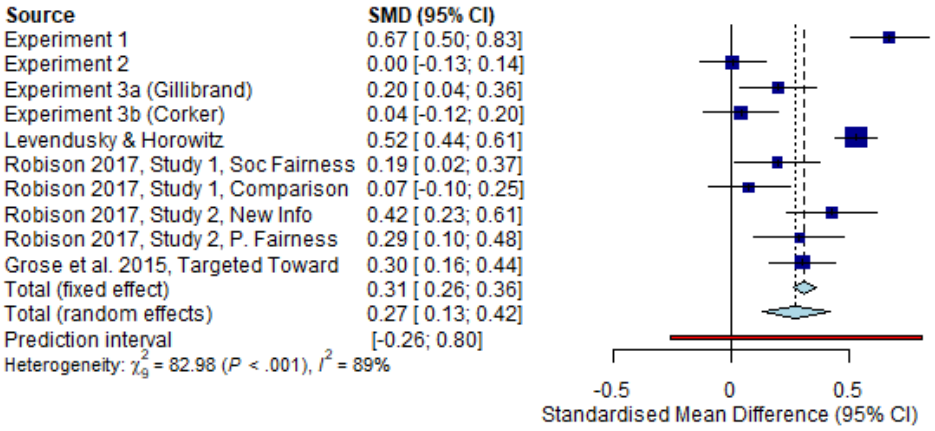
The average effect size for these other studies is 0.37 (fixed effects), whereas it is 0.2 for our four studies combined. Figure OA3 provides a forest plot of all the experiments; combing our experiments with these experiments yields an average effect size of 0.31 (fixed effects model). The estimates for the studies that find a significant effect of the explanation tend to be fall within a rather narrow range of effect sizes around this 0.3 mark with our Experiment 1 and Levendusky and Horowitz clearly to the right of these effects. Three of the effects are not statistically significant with two of these stemming from our current study (Experiment 2 and 3b).

Our studies thus do show a lower average effect size due to the null effects in Experiments 2 and 3b. This could imply that we have shown that the effects of justifications are smaller than one might presuppose based on existing work. However, we are cautious in making this claim. The effect size for the three studies we consider here is likely an over-estimate. Peterson and Simonovits, for instance, are missing from our analysis and they find null effects of explanation giving in their study. Meanwhile, it is not clear that any explanations had a significant effect in McGraw (1991) judging from Table 2 of that study. While it is clearly the case that evaluations are more positive in that study among individuals who are highly satisfied with the explanation than among those who are unsatisfied, these evaluations nevertheless remain about the same level, or lower, than those in the Control group. If we include these as 'null' results, then the estimated effect of justifications outside of our studies would naturally come down.

Ultimately, we believe that the available evidence shows that justifications *can* work, but that we should also expect some heterogeneity in this effect (sans counter-explanation). As McGraw argues, explanations are likely to work insofar as they are deemed 'satisfactory' (much as frames are likely to be effective if they are 'strong'). Not all explanations will work. Likewise, not all actions may be 'explainable' as we discuss in the conclusion of the manuscript.

[1] These studies generally focus on regressions of post-test evaluations on explanation condition and (post-test) account satisfaction without first reporting the overall mean for those in the various conditions. McGraw (1991) does report means, but not standard deviations, for her experimental conditions, which would be required to calculate Cohen's *d* statistics.

Figure OA3: Forest Plot for Explanation Giving



Power and Combined Analyses

Let us summarize the analyses we perform in text. We first examine the influence of justifications and counter-explanations, where we found that they led to significantly more positive evaluations in two of the four cases and that receiving a counter-explanation led to worse evaluations (relative to the justification condition) in all four experiments. We then consider the influence of credibility; here we find that counter-explanations were more damaging when from a high than low credible source in Experiment 1 and 2. We finally consider the role of partisanship, where we found at best inconsistent evidence that partisans paid greater attention to credibility.

One important question here concerns power: do we have sufficient power to detect significant differences?[2] This is an especially pertinent question regarding the analyses involving partisanship and counter-explanation credibility where we move from rather straightforward comparisons to ones involving not just one difference (i.e. are evaluations worse in the high credibility condition than in the justification condition) but two or more (i.e. is the difference in high credibility and justification conditions itself difference from the difference between low credibility and justification; does this difference in difference vary by respondent partisanship?). While the inclusion of pre-test covariates increases the power of our design, it may nevertheless be the case that we fail to find more consistent evidence for H4 because of an inability to reliably parse signal from noise

It is not immediately clear what type of power test to conduct for the complicated analyses of Hypothesis 4. As a beginning, then, we can consider power of our most straightforward analyses. In particular, we will consider what the minimum detectable effect is given our sample size, which is the smallest "true effect" obtainable for a given power and significance level (Bloom 1995). Table OA7 below provides the results of this exercise.[3] There, we show the sample size for each comparison relevant to the hypothesis, the observed Cohen's *d* statistics when comparing the two conditions r, and the minimum detectable effect given a power of 0.8 and an alpha of 0.05. Note that the observed *d* statistic is obtained from a t-test and thus does not include any covariates, which should increase the power of our analyses in Experiments 2 and 3a/3b. Broadly, we see that we have more than sufficient power in Experiment 1, and just about sufficient power in the remainder of the experiments to detect the effect we see in-text, although perhaps just barely in the latter cases (although in these latter cases the inclusion of covariates in our models should amplify power).

On the one hand, this is somewhat reassuring. On the other, however, the results for Experiments 2, 3a, and 3b, suggest that we may be running into issues of power for the more complex analyses implied by Hypothesis 3 and 4. If so, then what can be done? We obviously cannot go back in time to recruit more participants, but we can take one (non pre-registered) step available to us: combine the experiments into a single omnibus regression model. Doing so will naturally increase the number of observations for each test per cell and thus should improve the power of our analyses. However, this does have a drawback in that we cannot control for covariates in Experiment 1 and thus cannot do so in the combined analyses. Likewise, the credibility treatments in Experiments 3a/3b do not neatly map onto high and low credibility divisions which makes testing Hypotheses 3 and 4 more complicated.

Table OA8 provides analyses from three models that focus on Hypotheses 1-3. The first model regresses post-test evaluations on a three-category indicator for experimental treatment condition (Baseline: Justification; No Justification; Justification with any type of counter) and fixed effects for the experiment.[4] As in text we find a significant effect of the justification (as indicated by the *negative* coefficient for the No Justification condition) and likewise a significant negative effect of the counter-explanation. The difference between these two coefficients is also statistically significant (difference = -0.03 [-0.04, -0.01], $F = 25.67$, $p < 0.01$) indicating that evaluations were significantly worse when the counter-explanation was provided than when not.

The second two models focus on Hypothesis 3: high credibility counters are more influential than low credibility ones. Model 2 restricts the data to just Experiments 1 and 2 where we have a clear demarcation between high and

[2] We thank an anonymous Reviewer for calling attention to this question.
[3] Results were obtained using the "WebPower" package for R
[4] We also investigating fitting this model as a multilevel model with respondents nested within experiments and the effect of the treatment indicators allowed to vary across experiment. Doing so led to the same results.

low credibility sources. Consistent with Hypothesis 3 we find that the high credibility counter-explanation had a significantly stronger effect than the low credibility counter-explanation, although both undermined evaluations relative to the Justification condition. In Model 3 we add in data from Experiment 3. Here, we sorted respondents into high and low credibility based on the type of counter-explanation they received and their pre-test partisanship; thus, Democrats [Republicans] exposed to a left-wing [right-wing] source are coded as receiving a high credibility message while the inverse combinations are coded as low credibility messages. This transformation means that we lose Independents from Experiment 3 in these latter analyses. Adding Experiment 3 leads to smaller coefficients for the two counter-explanation conditions, but both remain negative and statistically significant and, as the bottom of Table OA8 shows, the difference between the two remains significant as well.

Tables OA9 and OA10 provide the results for tests of Hypothesis 4. Table OA9 regress evaluations on treatment condition, partisanship, and their interaction again with separate models for analyses conducted with just Experiments 1 and 2 versus all experiments. Table OA10, meanwhile, provides the marginal effect of assignment to the counter-explanation conditions for co- and opposing partisans; the difference in marginal effects within partisan grouping; and, finally, the between party differences. We see quite similar results to in-text when we just focus on Experiments 1 and 2; the high credibility counter-explanation effect had a significantly greater effect for co-partisans but a smaller and less precise one for opposing partisans, although the resulting difference in difference is not itself statistically significant. Adding Experiments 3a and 3b into the mix leads to estimates of the influence of the low credibility message that are greater in size (due to the effectiveness of the right-wing message among Democrats in Experiment 3a) and thus to a more precisely measured but now small difference in difference estimate. Combining the data sources thus does not provide additional evidence in favor of Hypothesis 4, suggesting that our failure to find more consistent evidence in support of this claim is not being driven solely by power considerations.

Table OA7: Minimum Detectable Effects (Hypotheses 1 & 2)

Experiment	Condition 1	Condition 2	Hypothesis	n	Observed d	MDE
Exp. 1	Justification	No Justification	H1	605	0.66	0.11
Exp. 1	Justification	Teachers	H2	599	0.84	0.11
Exp. 1	Justification	Non-Partisan	H2	601	0.92	0.11
Exp. 1	Justification	Partisan	H2	605	0.67	0.11
Exp. 1	Teachers	Partisan	H3	606	0.19	0.11
Exp. 1	Non-Partisan	Partisan	H3	608	0.25	0.11
Exp. 2	Justification	No Justification	H1	806	0.01	0.1
Exp. 2	Justification	High Credibility	H2	807	0.22	0.1
Exp. 2	Justification	Low Credibility	H2	810	0.11	0.1
Exp. 2	High Credibility	Low Credibility	H3	805	0.11	0.1
Exp 3a.	Justification	No Justification	H1	603	0.2	0.11
Exp 3a.	Justification	Left Wing	H2	604	0.1	0.11
Exp 3a.	Justification	Right Wing	H2	609	0.16	0.11
Exp 3a.	Left Wing	Right Wing	H3	608	0.07	0.11
Exp. 3b	Justification	No Justification	H1	611	0.04	0.11
Exp. 3b	Justification	Left Wing	H2	609	0.08	0.11
Exp. 3b	Justification	Right Wing	H2	614	0.21	0.11
Exp. 3b	Left Wing	Right Wing	H3	602	0.13	0.11

Table OA8: Combining the Experiments H1-H3

	(1) Hyp 1 & 2	(2) Hyp 3, Exp 1 & 2	(3) Hyp 3, All Exp
No Justification	-0.0522**	-0.0670**	-0.0522**
	(0.0103)	(0.0126)	(0.0103)
Justification w/Counter	-0.0778**		
	(0.00892)		
High Credibility		-0.126**	-0.0890**
		(0.0119)	(0.0102)
Low Credibility		-0.0821**	-0.0539**
		(0.0127)	(0.0107)
Experiment 2	-0.0497**	-0.0560**	-0.0525**
	(0.00856)	(0.00859)	(0.00859)
Experiment 3a	-0.0450**		-0.0420**
	(0.0108)		(0.0113)
Experiment 3b	-0.0667**		-0.0643**
	(0.0103)		(0.0107)
Constant	0.558**	0.581**	0.557**
	(0.00930)	(0.0104)	(0.00940)
Observations	5544	3121	5319
		Difference in Credibility Effects	
High Credibility – Low Credibility		-0.04 [-0.07, -0.02]	-0.04 [-0.06, -0.02]

Standard errors in parentheses
+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

Table OA9: Combining the Experiments: Hypothesis 4

	(1) Exp 1 & 2	(2) All Exp
No Justification	-0.0703**	-0.0747**
	(0.0167)	(0.0140)
High Credibility	-0.142**	-0.108**
	(0.0162)	(0.0138)
Low Credibility	-0.0823**	-0.0775**
	(0.0171)	(0.0147)
Opposing Partisan	-0.171**	-0.193**
	(0.0181)	(0.0147)
Independent	-0.0860**	-0.132**
	(0.0293)	(0.0239)
No Justification # Opposing Partisan	0.00477	0.0404+
	(0.0251)	(0.0212)
No Justification # Independent	-0.0415	-0.00273
	(0.0386)	(0.0323)
High Credibility # Opposing Partisan	0.0371	0.0330
	(0.0241)	(0.0203)
High Credibility # Independent	-0.0290	-0.0115
	(0.0361)	(0.0315)
Low Credibility # Opposing Partisan	0.0115	0.0429*
	(0.0254)	(0.0211)
Low Credibility # Independent	-0.0170	0.0297
	(0.0392)	(0.0350)
Experiment 2	-0.0606**	-0.0585**
	(0.00817)	(0.00818)
Experiment 3a		-0.0543**
		(0.0107)
Experiment 3b		-0.0661**
		(0.0103)
Constant	0.672**	0.666**
	(0.0130)	(0.0114)
Observations	3120	5316

Standard errors in parentheses
+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

Table OA10: Marginal Effects and Difference in Difference Tests

	(1) Just Exp 1 & Exp 2	(2) All Experiments
	High Credibility Marginal Effects	
Co-Partisan	-0.142**	-0.108**
	[-0.173, -0.110]	[-0.135, -0.0805]
Opposing Partisan	-0.104**	-0.0745**
	[-0.140, -0.0692]	[-0.104, -0.0449]
	Low Credibility Marginal Effects	
Co-Partisan	-0.0823**	-0.0775**
	[-0.116, -0.0488]	[-0.106, -0.0486]
Opposing Partisan	-0.0708**	-0.0346*
	[-0.108, -0.0339]	[-0.0647, -0.00438]
	Within Partisan Difference	
Co-Partisan	-0.06**	-0.03*
	[-0.09, -0.03]	[-0.06, -0.002]
Opposing Partisan	-0.03+	-0.04**
	[-0.07, 0.01]	[-0.07, -0.01]
	Between Partisan Difference	
Co-Partisan [HC – LC] – Opposing Partisan [HC – LC]	-0.03 [-0.07, 0.02]	0.01 [-0.03, 0.05]
N	3120	5316

95% confidence intervals in brackets

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Online Appendix OB: Experiment 1

Treatment Wordings

Table OB1: Treatment Wordings, Experiment 1

[Background Information; Received by All:
Representative A is a member of the [Democratic/Republican] Party and has been in the state legislature for twelve years. During this time, Representative A has served on the Appropriations Committee, which oversees budgeting. Representative A has also served as Chair of the Energy Committee. Each term a group called the Americans for Democratic Action provides a score concerning the voting record of legislators. The score ranges from 0-100 with higher scores indicating a more liberal voting record. In its most recent publication, Representative A received a score of [80/20] from the ADA.

[Policy Treatment:
During the previous session, the Appropriations Committee voted on an amendment to the annual budget plan. The amendment proposed a change to the criteria used to distribute education funds to local communities, resulting in cuts in funding for some communities, including Representative A's district, but increases for others. Representative A voted in favor of this amendment.

[Justification:
When asked about the vote, Representative A said: "I voted for the amendment because I believe that it will make the distribution of school funding across the state fairer for all citizens than under the present budget, with more money going to those who need the funding the most."

[Counter-Narrative:
However, [local teachers / non-partisan experts on education policy / [Republican/Democratic] legislators in the state house] quoted in media reports about the cuts say that campaign donors of Representative A benefit financially from the amendment and that this is really why the Representative voted for it.

Notes: The Democratic version always had an ideological score of 80, while the Republican always received a score of 20. The partisan counter-narrative source was paired with the partisanship of Representative A; when the latter was a Democrat, then the partisan treatment read "Republican legislators in the state house", etc.

Pre-Test: Anger and Cutting Economic Spending

One question may be whether these voting to cut education funding is a controversial action. We thus fielded a separate sample of 301 respondents on MTurk. Respondents to this survey were restricted from participating in the experiments. We asked respondents the following question:

> There has been a good deal of discussion recently about how local and state governments can best avoid budget problems. One potential way to avoid such issues is by cutting spending. Below is a list of areas where governments could cut spending. How angry would you be if your local government cut spending in these areas?

Respondents could then indicate their level of anger on a 1 (not angry at all) to five (very angry) fully labeled scale for the following areas of spending: animal control, community events, sanitation, the fire department, parks and recreation, the school department, the policy department, transportation, and libraries. The order of the items was randomly assigned per respondent. Table OB2 provides the mean levels of anger per program. Cuts to the education spending earns a great deal of ire including from all respondents and from both Democrats and Republicans, albeit more from the former the latter.

Table OB2: Mean Anger for Program Cuts

	Means	Democrats	Republicans	t-test
Animal Control	2.63 (1.12)	2.68 (1.08)	2.5 (1.33)	t=1.27; p = 0.20

Community Events	2.24 (1.11)	2.37 (1.14)	2.13 (1.01)	t=1.66; p = 0.10
Sanitation	3.47 (1.16)	3.55 (1.10)	3.28 (1.18)	t=1.85; p = 0.07
Fire Dept.	3.72 (1.19)	3.67 (1.18)	3.82 (1.20)	t=0.99; p = 0.32
Parks & Recreation	2.98 (1.19)	3.19 (1.15)	2.69 (1.17)	t = 3.28; p = 0.001
School Dept.	3.78 (1.28)	3.98 (1.15)	3.58 (1.37)	t = 2.53; p = 0.01
Police Dept.	3.41 (1.23)	3.21 (1.33)	3.87 (1.18)	t=3.95; p = 0.0001
Transportation	2.99 (1.23)	3.16 (1.23)	2.76 (1.18)	t=2.52; p = 0.01
Libraries	3.17 (1.23)	3.36 (1.20)	2.85 (1.19)	t=3.28; p = 0.001
N =	301	163-4	92	

Pre-Test: Explanation Strength and Source Credibility

Prior to Study 1 we completed pre-tests to identify a 'strong' justification as well as to identify credible and less credible counter-narrative providers.

Justification Strength Pre-Test Results

We first recruited 322 respondents from MTurk to assess justification strength. [As with all of our pre-tests, these respondents are restricted from participating in our experiments.] The survey begins with the following introduction:

> On the next few pages we will ask you to imagine a scenario where your state legislature has voted on an amendment to the annual budget plan. In addition, you will be provided with a prospective explanation that a legislator might offer for taking a particular position on this amendment. Please read this account as if it was provided by your own state representative in this situation

Respondents were then (randomly) provided with a vignette where the legislator either voted *for* cuts to spending for his/her district or voted *against* an amendment that would have led to increased spending for his/her district. The legislator could offer one of four randomly presented arguments: one based in fairness; a tailored explanation; one based on personal conscience; and one based either on costs (against spending) or benefits (for cuts). The wording for each justification is available on request and will be posted with the replication materials.

After receiving the vignette, respondents were asked how satisfied they were with the explanation (1 = extremely dissatisfied, 7 = extremely satisfied); how effective they found it (1 = definitely not effective, 7 = definitively effective); how angry they would be to their state legislator in this situation (1 = not at all, 5 = extremely); and how much credit or blame the legislator deserves for taking the position (1 = great deal of blame, 5 = great deal of credit). We provide summary statistics for all four measures as well as a summary measure of 'warmth' toward the vignette legislator formed from a factor analysis of the four items in Table OB3 below. Two things were apparent to us. First, the vignette legislator was liked less when cutting spending than when obtaining benefits; we thus went with this behavior in our experiment to provide a stronger test for the initial explanation effect hypothesis. Second, the fairness account appeared to be the most effective of the four, hence its use in our experiment.

Table OB3: Justification Strength Statistics

	Satisfaction	**Effectiveness**	**Anger (Higher = more)**	**Blame (Higher = Blame)**	**Factor: Warmth**
Scenario					
Against; Tailored	4.10 (1.43)	4.05 (1.43)	2.00 (0.95)	3.18 (0.97)	-0.19 (0.78)
Against; Conscience	4.31 (1.83)	4.23 (1.77)	1.81 (0.98)	2.88 (1.21)	0.02 (1.01)

Against; Fairness	5.47 (1.29)	5.24 (1.41)	1.53 (0.88)	2.22 (1.05)	0.67 (0.84)
Against; Costs	4.45 (1.44)	4.54 (1.43)	2.06 (1.06)	3.84 (1.0)	0.05 (0.83)
For; Conscience	4.11 (1.47)	3.71 (1.54)	2.06 (1.00)	3.2 (1.16)	-0.27 (0.93)
For; Fairness	4.76 (1.53)	4.51 (1.53)	1.93 (1.07)	2.8 (1.18)	0.15 (1.09)
For; Tailored	3.57 (1.50)	3.66 (1.56)	2.32 (1.12)	3.30 (0.97)	-0.50 (1.03)
For; Benefits	4.33 (1.69)	4.28 (1.60)	2.02 (1.05)	2.93 (1.16)	-0.04 (0.95)
Policy					
Against Spending Increase	4.68 (1.56)	4.60 (1.57)	1.82 (0.97)	2.72 (1.11)	0.20 (0.92)
For Spending Cut	4.16 (1.59)	4.03 (1.59)	2.10 (1.07)	3.06 (1.12)	-0.18 (1.03)
Explanation					
Tailored	3.79 (1.49)	3.83 (1.53)	2.18 (1.06)	3.25 (0.97)	-0.36 (0.94)
Conscience	4.2 (1.63)	3.93 (1.65)	1.95 (0.99)	3.07 (1.18)	-0.14 (0.97)
Fairness	5.14 (1.44)	4.90 (1.50)	1.72 (0.99)	2.49 (1.14)	0.44 (1.14)
Benefits/Costs	4.38 (1.57)	4.40 (1.53)	2.04 (1.05)	2.89 (1.10)	-0.001 (0.87)

Credibility Pre-Test Results

Later on in the same survey we asked respondents to answer questions to tap the credibility of various speakers.

(1) how much [local teachers; the National Education Association; the American Federation of Teachers; the Democratic Party; and the Republican Party] knows will happen if the level of education funding is changed (1-4 scale, from nothing to a lot);
(2) how favorable or unfavorable they feel toward each group (1-5, from very unfavorable to very favorable)
(3) how trustworthy the group, or elected officials from the two parties, are (1-5 from very untrustworthy to very trustworthy).

Table OB4 breaks down the describe statistics for all four variables both overall and by respondent partisanship. Local teachers were highly rated on all four measures, hence our choice of them as 'highly credible' sources. Meanwhile, the Democratic and Republican parties do worse overall than the other three groups.

Table OB4: Credibility

	Overall	Democrats	Republicans
Knowledge (Range: 1-4)			
Local Teachers	2.98 (0.90)	3.14 (0.90)	3.03 (0.85)
NEA	3.11 (0.81)	3.24 (0.80)	3.11 (0.79)
ATF	3.04 (0.79)	3.13 (0.81)	3.01 (0.79)
Dem Party	2.78 (0.87)	2.90 (0.79)	2.65 (0.95)
Rep Party	2.61 (0.92)	2.39 (0.96)	2.95 (0.80)
Favorability (Range: 1-5)			
Local Teachers	4.11 (0.90)	4.31 (0.78)	3.94 (0.90)
NEA	3.25 (0.93)	3.41 (0.91)	3.09 (0.98)
ATF	3.46 (0.90)	3.65 (0.89)	3.31 (1.01)
Dem Party	2.93 (1.18)	3.70 (0.87)	2.16 (1.04)

Rep Party	2.34 (1.20)	1.63 (0.79)	3.68 (0.82)
Trustworthiness			
Local Teachers	3.95 (0.93)	4.20 (0.80)	3.79 (0.99)
NEA	3.30 (0.92)	3.46 (0.92)	3.19 (0.98)
ATF	3.43 (0.94)	3.63 (0.95)	3.24 (0.97)
Dem Party	2.94 (1.14)	3.54 (0.85)	2.26 (1.11)
Rep Party	2.41 (1.22)	1.86 (1.05)	3.46 (0.91)

We were somewhat worried that local teachers might nevertheless be discounted given their potential material interest in the issue of education cuts. We thus performed a follow up pre-test with a different sample of 181 respondents from MTurk. In varying order, we provided the following question stem:

> "News stories concerning cuts in education funding often feature quotes from [local teachers; representatives of non-partisan research groups that specialize in education policy, such as the National Education Policy Center; politicians that support the proposed funding cuts; politics that oppose the proposed funding cuts]".

For each target we asked respondents to agree or disagree on a 1-7 scale to the following four statements:

1. they [the speaker] is well informed on the issue and understands the consequences of funding cuts;
2. they have a personal interest in whether funding cuts are passed;
3. they are trustworthy concerning the desirability of funding cuts;
4. they are a good source of information concerning why the cuts were passed or not passed.

Table OB5 below provides the summary statistics. Again, local teachers appear highly credible, being highly informed and trustworthy, although their personal interest in the matter may cut against this credibility somewhat. Meanwhile, the non-partisan source had a lower personal interest, while still being regarded as informative if not definitively trustworthy about the desirability of the cuts. More specifically political sources, meanwhile, were deemed less trustworthy and not particularly informed. We thus included the non-partisan source as a secondary highly credible source in the experiment.

Table OB5: Credibility

	Local Teachers	Non-Partisan	Supporters	Opposes
Informed	5.40 (1.53)	5.08 (1.38)	3.94 (1.76)	4.52 (1.66)
Personal Interest	5.99 (1.40)	4.32 (1.69)	5.14 (1.78)	5.04 (1.56)
Trustworthy	5.03 (1.66)	4.31 (1.61)	3.25 (1.65)	3.96 (1.62)
Process	4.72 (1.62)	4.36 (1.64)	3.88 (1.79)	4.34 (1.66)

Higher = agree with statement

Additional Measures

We asked respondents some further items about the legislator and their behavior related to our discussion of the psychology of blame. On the one hand, we asked respondents whether they agreed (+1), disagreed (-1) or neither (0) with the vote taken by the representative. On the other, we asked respondents to rate the importance of a variety of "possible reasons for Representative A's vote on the education-funding amendment to the budget plan". (Those in the No Policy Information condition were instead asked to rate the importance of the motives for explaining the representative's "voting behavior while in the state legislature.") We will examine two indices created via factor analysis of these items (M=0, SD=1). First, an index of 'good representative' motives formed from the items "desire to help constituents," "desire to help all state residents", and "desire to make good policy" ($\alpha = 0.84$). Second, we examine an index of negative 'political' motives formed from the items "pandering to voters," "influence of special interests", and "winning re-election" ($\alpha = 0.69$). It should be noted that the order in which respondents answered the policy approval and motives questions was randomly varied, i.e. some answered the policy approval item before the motives while others received the inverse order.

Tale OB6 provides the mean scores for these items by condition for all respondents and by respondent/legislator partisanship along with 95% confidence intervals. The patterns seen in Table OB6 closely resemble those in text. Those assigned to the Justification condition report greater agreement with the policy change and place more importance on "Good Representative" motives in explaining the legislators behavior than those in the No Policy Info or No Explanation counter-factuals. Ratings on these variables, however, are more in line with the two no explanation conditions when a counter-narrative is present. Again, credibility matters as the message from the Partisan Opponents is less effective at undermining evaluations than the counter-message from the two more credible speakers.

Table OB6: Policy Agreement and Motive Attributions

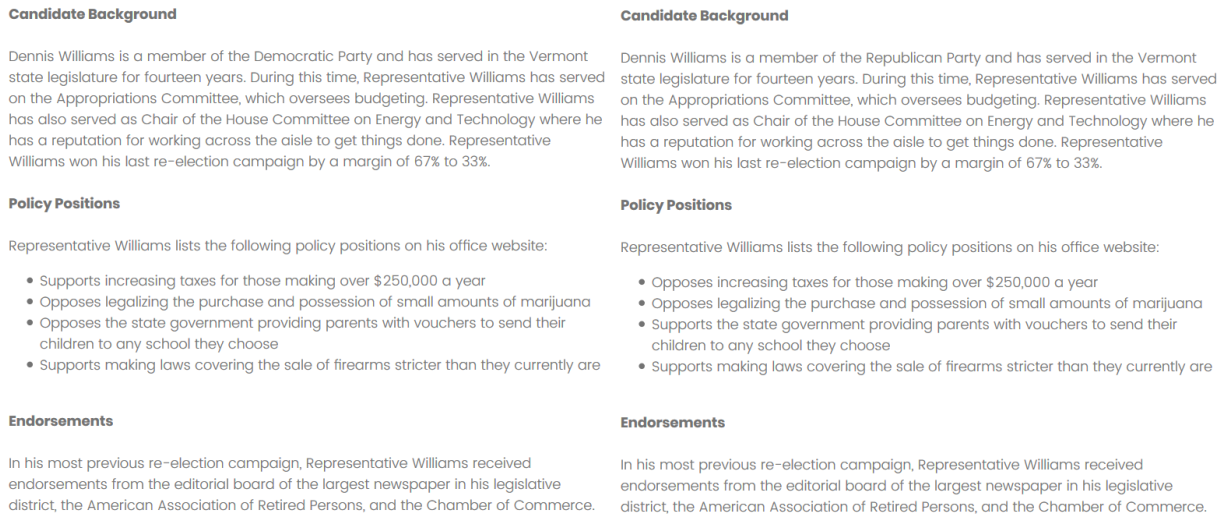
	All Respondents					
	No Policy Info	No Expl.	Expl.	Teachers	Experts	Partisan
Policy Agreement	N/A	-0.08 (-0.16, 0.01)	0.51 (0.43, 0.59)	-0.02 (-0.11, 0.08)	-0.06 (-0.16, 0.03)	0.16 (0.07, 0.25)
"Political" Motives	0.37 (0.26, 0.47)	-0.26 (-0.37, -0.14)	-0.31 (-0.44, -0.18)	0.03 (-0.07, 0.14)	0.06 (0.05, 0.17)	0.12 (0.01, 0.22)
"Good Representative" Motives	0.16 (0.05, 0.27)	-0.03 (-0.14, 0.09)	0.28 (0.18, 0.37)	-0.14 (-0.26, -0.02)	-0.25 (-0.38, -0.13)	-0.02 (-0.12, 0.09)
	Co-Partisans					
Policy Agreement	N/A	0.12 (-0.01, 0.25)	0.65 (0.54, 0.77)	0.20 (0.04, 0.35)	0.10 (-0.05, 0.24)	0.49 (0.36, 0.62)
"Political" Motives	0.28 (0.14, 0.43)	-0.34 (-0.51, -0.17)	-0.65 (-0.84, 0.45)	-0.10 (-0.26, 0.07)	0.04 (-0.13, 0.21)	-0.04 (-0.23, 0.14)
"Good Representative" Motives	0.49 (0.36, 0.62)	0.24 (0.11, 0.38)	0.51 (0.38, 0.64)	0.18 (0.02, 0.34)	-0.02 (-0.19, 0.15)	0.36 (0.21, 0.51)
	Opposing Partisans					
Policy Agreement	N/A	-0.30 (-0.44, -0.17)	0.38 (0.25, 0.51)	-0.14 (-0.29, 0.002)	-0.24 (-0.39, -0.09)	-0.08 (-0.22, 0.06)
"Political" Motives	0.47 (0.30, 0.63)	-0.27 (-0.45, -0.09)	-0.09 (-0.28, 0.09)	0.02 (-0.14, 0.19)	0.14 (-0.02, 0.30)	0.18 (0.03, 0.33)
"Good Representative" Motives	-0.13 (-0.32, 0.05)	-0.26 (-0.45, -0.07)	0.08 (-0.08, 0.25)	-0.31 (-0.51, -0.11)	-0.42 (-0.62, -0.22)	-0.24 (-0.41, -0.08)

Notes: Cells provide condition means alongside 95% confidence intervals.

Online Appendix OC: Experiment 2

Treatment Wordings

Figure OC1: Initial Vignette about Politician



Candidate Background

Dennis Williams is a member of the Democratic Party and has served in the Vermont state legislature for fourteen years. During this time, Representative Williams has served on the Appropriations Committee, which oversees budgeting. Representative Williams has also served as Chair of the House Committee on Energy and Technology where he has a reputation for working across the aisle to get things done. Representative Williams won his last re-election campaign by a margin of 67% to 33%.

Policy Positions

Representative Williams lists the following policy positions on his office website:

- Supports increasing taxes for those making over $250,000 a year
- Opposes legalizing the purchase and possession of small amounts of marijuana
- Opposes the state government providing parents with vouchers to send their children to any school they choose
- Supports making laws covering the sale of firearms stricter than they currently are

Endorsements

In his most previous re-election campaign, Representative Williams received endorsements from the editorial board of the largest newspaper in his legislative district, the American Association of Retired Persons, and the Chamber of Commerce.

Candidate Background

Dennis Williams is a member of the Republican Party and has served in the Vermont state legislature for fourteen years. During this time, Representative Williams has served on the Appropriations Committee, which oversees budgeting. Representative Williams has also served as Chair of the House Committee on Energy and Technology where he has a reputation for working across the aisle to get things done. Representative Williams won his last re-election campaign by a margin of 67% to 33%.

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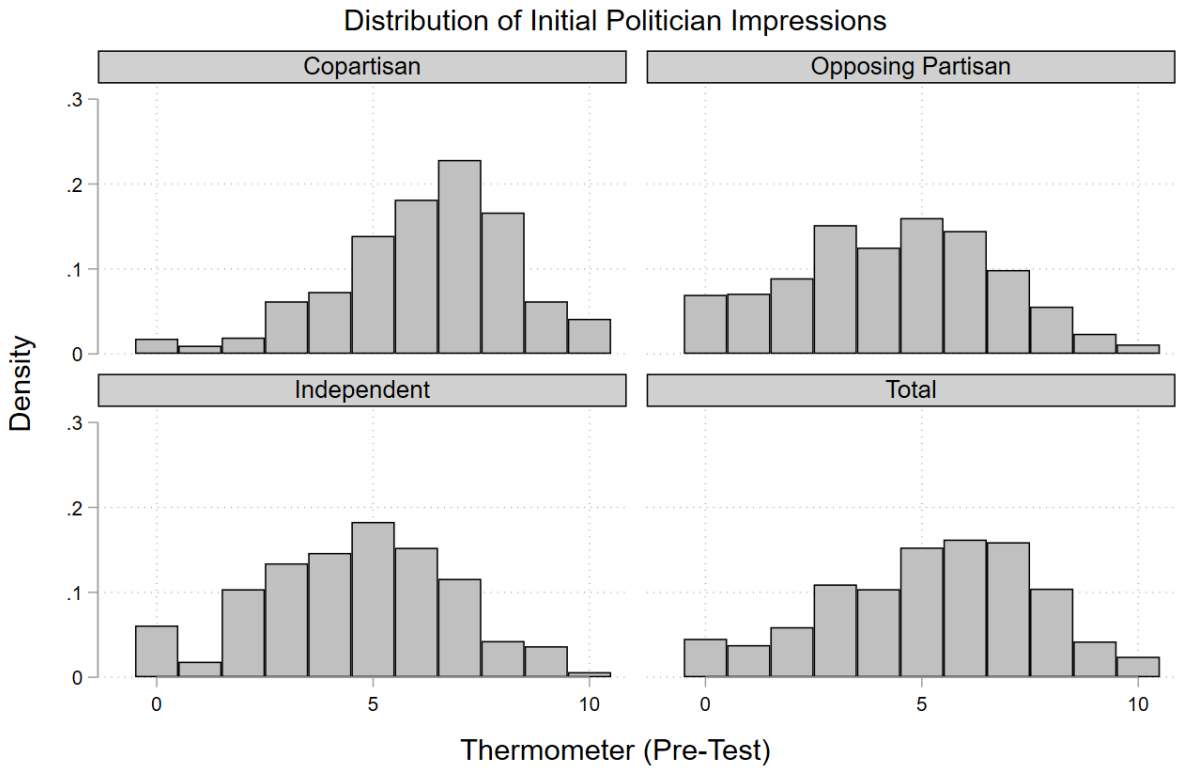
Endorsements

In his most previous re-election campaign, Representative Williams received endorsements from the editorial board of the largest newspaper in his legislative district, the American Association of Retired Persons, and the Chamber of Commerce.

Notes: This image provides the background information subjects received concerning the legislator as it was seen by respondents. The left-hand image shows the information received by those randomly assigned to read about a Democratic legislator, the right shows the Republican legislator.

Figure OC2: Initial Impressions of the Politician



Note: The overall mean is 5.20 [5.09, 5.32]. Not surprisingly, this mean is significantly larger among co-partisans (6.21 [6.06, 6.36]) than both opposing partisans (4.33 [4.16, 4.51]) and Independents (4.57 [4.24, 4.93]). ANOVA: $F = 135.34$, $p < 0.001$.

Article Wordings, Justification Portion of Experiment

Baseline (No Justification or Counter)

MONTPELIER – Higher electric rates for Burlington Electric Department customers starting September 1 are one step closer to being imposed.

Tuesday night, the House Committee on Energy and Technology voted 4-3 to narrowly approve an amended version of the budget that includes the rate increase among other provisions. The deciding vote was cast by the Chair of the Committee, Representative Dennis Williams.

Under the increase, customers of the publicly-regulated Burlington Electric that use 1,000 kilowatt hours of electricity a month – what officials say is the average home – would see their monthly bills go up by an average of $10.42 to $107.20. That is a 10.77 percent jump. However, some customers would see higher-than-average rate hikes while others lower-than-average increases depending on their rate classes and overall electricity use.

The budget process now moves on to the full House for a final vote next week before the State Senate takes up its own budget bill. The key debate in the Senate will concern revenue. One potential amendment likely to be discussed is an increase in the state's sales tax from 7% to 7.3%. However, it is unclear whether there will be enough support for the amendment and most experts expect the Senate to adopt the House's budget before sending it on to the Governor for final approval.

Below are the wordings of the justification and counter-narratives. These always came after the "under the increase" paragraph and before the "the budget process now moved" paragraph; the latter paragraph always ended the article.

Justification (4th paragraph in article, when it appears):

Williams defended his vote to a group of constituents after the vote by noting the need for additional spending to keep electricity costs down in the long run. "This was a difficult decision," Williams said, "but if we do not make an investment now then prices will increase even more dramatically over time. Sometimes you have to make tough choices that you believe are in the best long-term interests of the community."

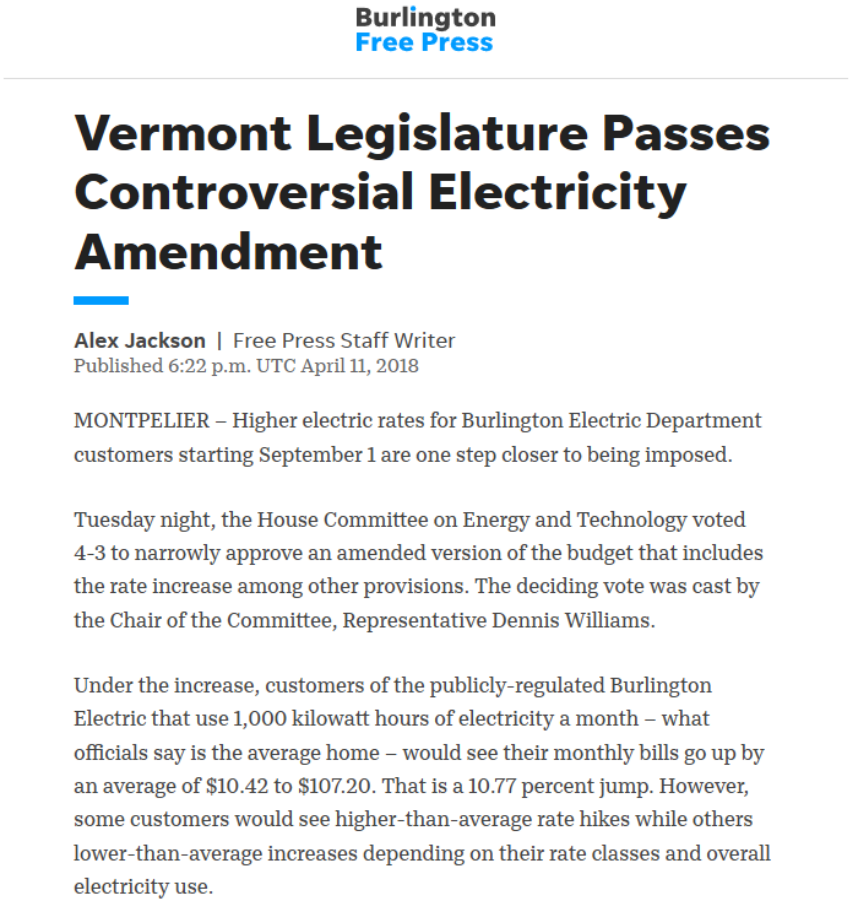
High Credibility Counter-Narrative (5th paragraph in article when it appears):

However, some were not convinced. Gary Allison, an economist at the non-partisan Center on Budget and Policy Priorities, noted that lobbyists for Burlington Electric Department had extensively lobbied committee members to vote for the rate increase. "The rate increase means a big cash inflow for Burlington Electric," Allison said, "and I'm sure they were holding out future campaign donations as a carrot to vote for the hike."

Low Credibility Counter-Narrative (5th paragraph in article when it appears):

However, some were not convinced. Gary Allison, a [Democratic/Republican] city council member trailing Williams in the polls in their upcoming primary election, noted that lobbyists for Burlington Electric Department had extensively lobbied committee members to vote for the rate increase. "The rate increase means a big cash inflow for Burlington Electric," Allison said, "and I'm sure they were holding out future campaign donations as a carrot to vote for the hike."

Figure OC3: Example of Policy Vote Treatment



Burlington
Free Press

Vermont Legislature Passes Controversial Electricity Amendment

Alex Jackson | Free Press Staff Writer
Published 6:22 p.m. UTC April 11, 2018

MONTPELIER – Higher electric rates for Burlington Electric Department customers starting September 1 are one step closer to being imposed.

Tuesday night, the House Committee on Energy and Technology voted 4-3 to narrowly approve an amended version of the budget that includes the rate increase among other provisions. The deciding vote was cast by the Chair of the Committee, Representative Dennis Williams.

Under the increase, customers of the publicly-regulated Burlington Electric that use 1,000 kilowatt hours of electricity a month – what officials say is the average home – would see their monthly bills go up by an average of $10.42 to $107.20. That is a 10.77 percent jump. However, some customers would see higher-than-average rate hikes while others lower-than-average increases depending on their rate classes and overall electricity use.

Notes: A partial screengrab of what the newspaper article looked like to respondents. This specific example comes from the No Justification condition.

Alternative Specifications

In text we regressed respondents' post-test evaluations on treatment conditions and their pre-test evaluation. In Table OC1 we provide an alternative way of using this pre-test information: a difference score (post-test – pre-test; rescaled to range from 0-1). We see the same results as in-text: providing a justification (versus not) did not lead to better evaluations, while the counter-explanations undermined these evaluations. The high credibility account was again more effective (difference = -0.02 [-0.03, -0.01], $F = 8.21$, $p < 0.01$). And, as in text, we find that a more reliable difference in effectiveness based on credibility for co-partisans (-0.02 [-0.04, -0.004], $F = 5.36$, $p < 0.05$) than for opposing partisans (-0.02 [-0.04, 0.005], $F = 2.3$, $p = 0.13$), but again with an insignificant difference in difference (-0.01 [-0.04, 0.02]).

We also report below the results of a final specification: one where we simplify the difference score to three categories: a negative change in evaluations (post-test < pre-test), no change in evaluations (post-test = pre-test), and a positive change in evaluations (post-test > pre-test). Table OC2 focuses on an analogue to our Figure 2 analyses where we do not combine the two credibility conditions. Respondents assigned to the justification condition were more likely to report a positive change in evaluations than where those assigned to the no explanation condition as indicated by the significant negative coefficient for No Justification in Table OC2. Meanwhile, those assigned to the High Credibility counter were the most likely to report a negative change and were significantly more likely to do so than those in the Justification condition.

Table OC2 provides the interaction between treatment assignment and co-partisanship while Figure OC4 plots the results of the interaction models. The markers provide the difference in probability of giving each response by treatment assignment (y-axis) and partisanship (circles = co-partisan, triangles = opposing partisans). Co-partisans were more likely than those in the justification condition to report 'worse' evaluations both when a low and high credibility source offered the counter-explanations, whereas opposing partisans only did so when the high credibility source was involved. This cuts against our prior results using the full scale items. The multinomial models thus provide a bit more evidence in favor of H1, and less for H4, than do the in-text models.

Table OC1: Difference Score

	(1) Figure 1	(2) Figure 2	(3) Figure 3
No Justification	-0.01	-0.01	-0.01
	(0.01)	(0.01)	(0.01)
Just. w/Counter	-0.02**		
	(0.01)		
Republican Legislator	0.01	0.01	
	(0.00)	(0.00)	
Thermometer (Pre-Test)	-0.17**	-0.17**	-0.17**
	(0.01)	(0.01)	(0.01)
HC Counter		-0.03**	-0.04**
		(0.01)	(0.01)
LC Counter		-0.02*	-0.02*
		(0.01)	(0.01)
Opposing Partisan			-0.00
			(0.01)
Independent			-0.03
			(0.02)
No Justification # Opposing Partisan			-0.01
			(0.01)
No Justification # Independent			0.04
			(0.03)
HC Counter # Opposing Partisan			0.01
			(0.01)
HC Counter # Independent			0.04+
			(0.02)
LC Counter # Opposing Partisan			0.01
			(0.01)
LC Counter # Independent			0.03
			(0.02)
Constant	0.68**	0.68**	0.69**
	(0.01)	(0.01)	(0.01)
Observations	1610	1610	1609
Adjusted R^2	0.160	0.164	0.163

Standard errors in parentheses

Baseline Categories: Justification (all models); Democratic Legislator (model 2); Co-Partisan legislator (model 3)

$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

Table OC2: Multinomial Logit Models, Experiment 2

	Negative Change	No Change [Baseline Category]	Positive Change
No Justification	-0.02	0.00	-0.49*
	(0.15)	(.)	(0.21)
HC Counter	0.57**	0.00	-0.30
	(0.16)	(.)	(0.22)
LC Counter	0.25	0.00	-0.17
	(0.16)	(.)	(0.21)
Opposing Partisan	-0.37**	0.00	0.33+
	(0.11)	(.)	(0.17)
Independent	-0.55**	0.00	0.34
	(0.19)	(.)	(0.26)
Constant	0.11	0.00	-1.04**
	(0.12)	(.)	(0.17)
Observations		1609	
Pseudo R^2		0.017	
		Predicted Probabilities	
No Justification	0.41 [0.36, 0.46]	0.46 [0.42, 0.51]	0.12 [0.09, 0.16]
Justification	0.39 [0.34, 0.43]	0.43 [0.38, 0.47]	0.19 [0.15, 0.22]
HC Counter	0.55 [0.50, 0.60]	0.34 [0.30, 0.39]	0.11 [0.08, 0.14]
LC Counter	0.46 [41, 0.51]	0.39 [0.35, 0.44]	0.15 [0.11, 0.18]

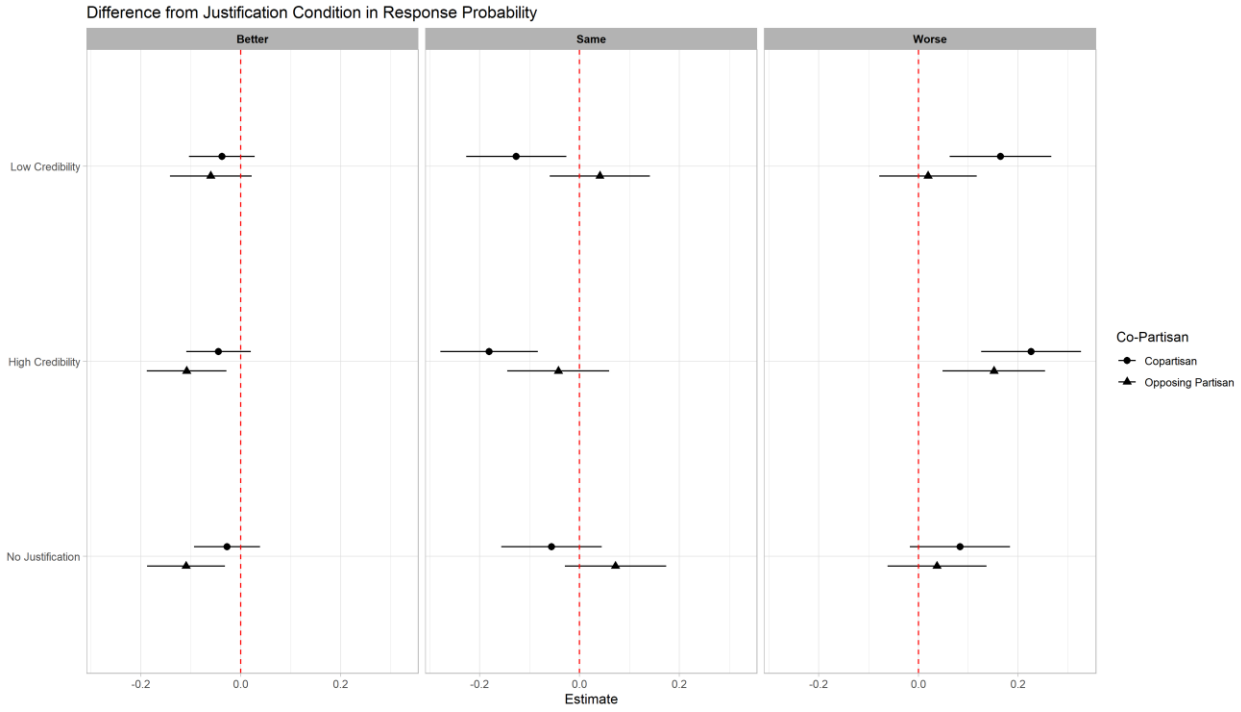
Standard errors in parentheses
Reference category is 'No Change in Thermometer'
$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

Table OC3: Multinomial Logit Models: Interaction, Experiment 2

	Baseline Category: No Change in Evaluation	
	Negative Change	Positive Change
No Justification	0.32	-0.10
	(0.22)	(0.34)
HC Counter	0.94**	0.09
	(0.23)	(0.37)
LC Counter	0.66**	-0.01
	(0.23)	(0.36)
Opposing Partisan	0.02	0.70*
	(0.23)	(0.30)
Independent	0.75+	0.74
	(0.42)	(0.56)
No Justification # Opposing Partisan	-0.38	-0.68
	(0.32)	(0.45)
No Justification # Independent	-2.34**	-0.64
	(0.67)	(0.77)
HC Counter # Opposing Partisan	-0.47	-0.59
	(0.33)	(0.48)
HC Counter # Independent	-1.65**	-0.79
	(0.54)	(0.75)
LC Counter # Opposing Partisan	-0.70*	-0.37
	(0.33)	(0.45)
LC Counter # Independent	-1.25*	0.01
	(0.58)	(0.76)
Constant	-0.16	-1.25**
	(0.16)	(0.23)
Observations	1609	
Pseudo R^2	0.024	

Standard errors in parentheses
Reference category is 'No Change in Thermometer'
+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

Figure OC4:


Difference from Justification Condition in Response Probability
Better
Same
Worse
Low Credibility
High Credibility
No Justification
-0.2
0.0
0.2
Estimate
Co-Partisan
Copartisan
Opposing Partisan

Pre-Test Analyses

Prior to conducting Experiment 2 we conducted pre-tests to examine the credibility of speakers and potential justifications. Our goal was to identify a 'consensual' low credibility source in the former case and a satisfactory justification in the latter.

Credibility

We recruited 302 subjects from MTurk. We provided these respondents with the Justification version of the legislator vignette from Study 1. We then provided them with one of the four following question stems (randomly either before or after evaluating the legislator in the vignette);

- A Democratic candidate running against Representative A in an upcoming primary was quoted in media reports saying that campaign donors of Representative A benefit financially from the amendment and that this is why the Representative voted for it.
- A Republican candidate running against Representative A in an upcoming primary was quoted in media reports saying that campaign donors of Representative A benefit financially from the amendment and that this is why the Representative voted for it.
- School administrators whose jobs are threatened by the cuts were quoted in media reports saying that campaign donors of Representative A benefit financially from the amendment and that this is why the Representative voted for it.

We then asked them to agree/disagree with whether the speaker would be well informed, have a personal interest in the cuts being passed, trustworthiness, and a personal interest in how Representative A is seen by voters. Table OC4 provides the descriptive statistics from this exercise separately for the Republican or Democratic version of Representative A and combined across legislator partisanship. The rival partisan tended to be seen as a bit less trustworthy, as having a stronger personal interest in what voters think, a greater personal interest in the cuts, and is slightly less informed. We thus chose this source as our low credibility source in Study 2.

Table OC4: Credibility Pre-Test Results

	Dem:Rival	Rep:Rival	Rival	Dem:Admin	Rep:Admin	Admin
Informed	4.67 (1.30)	4.15 (1.29)	4.41 (1.42)	4.45 (1.49)	4.94 (1.39)	4.70 (1.46)
Interest in Cuts	3.27 (1.30)	3.51 (1.33)	3.39 (1.31)	2.38 (1.55)	2.21 (1.25)	2.29 (1.40)
Trustworthy	3.89 (1.24)	3.57 (1.42)	3.73 (1.34)	3.91 (1.50)	4.17 (1.33)	4.04 (1.42)
Interest in Voters	2.45 (1.54)	2.61 (1.55)	2.53 (1.54)	2.97 (1.58)	2.57 (1.32)	2.77 (1.47)

Note: Higher = *more* credible (i.e. more informed, more trustworthy, *less* of a personal interest). For the interest measures, lower scores = more agreeing with the items; hence, we should see lower scores on the interest in cuts for the administrators than the rivals, but lower scores on the voters in the reverse direction.

Justification

We fielded two pilot tests of Study 2 to identify a 'strong' justification (n=292 and 257 respectively). The pilot tests varied in two important respects. First, the policy positions of the legislator varied across the two versions. In the first pilot test the Democratic (Republican) legislator supported (opposed) raising taxes on those making over $250,000 a year; supported (opposed) instituting a carbon tax to regulate greenhouse gases; opposed (supported) raising the state minimum age to $15 an hour; and supported (opposed) decriminalizing possess of marijuana. The legislator in the second pre-test had the same positions as we used in the final study. Second, the 'long-term benefits' justification, which we would ultimately use in our study, was slightly modified between the pilots. In the first test, the legislator says: "This was a difficult decision, but prices will keep on rising without additional funding to improve the efficiency of the grid." We ultimately decided that discussions of grid efficiency might come off as an 'environmentalist' message and thus turn off Republicans. We thus kept the spirit of the justification the same

(i.e. planning for the long haul) but described the plan as an 'investment'. A third difference as well is that we also tested a 'contemporary' benefits styled justification: "This was a difficult decision, but the budget also increases spending for our school system and includes supplemental transportation funding." Figures OC5-OC6 below plot the mean post-test thermometer for the legislator (left panel) and its difference from the pre-test (right panel). In both pre-tests the long-term benefits treatment led to a significantly reduced decline in evaluations compared to the No Justification baseline. This, of course, deviates from what we observed in text where we see a weaker effect of the justification.

Figure OC5: Pre-Test 1

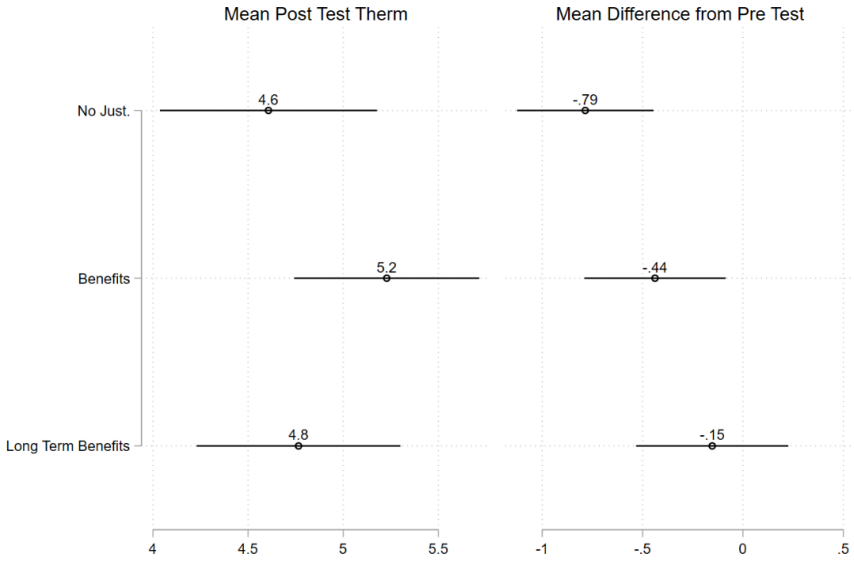
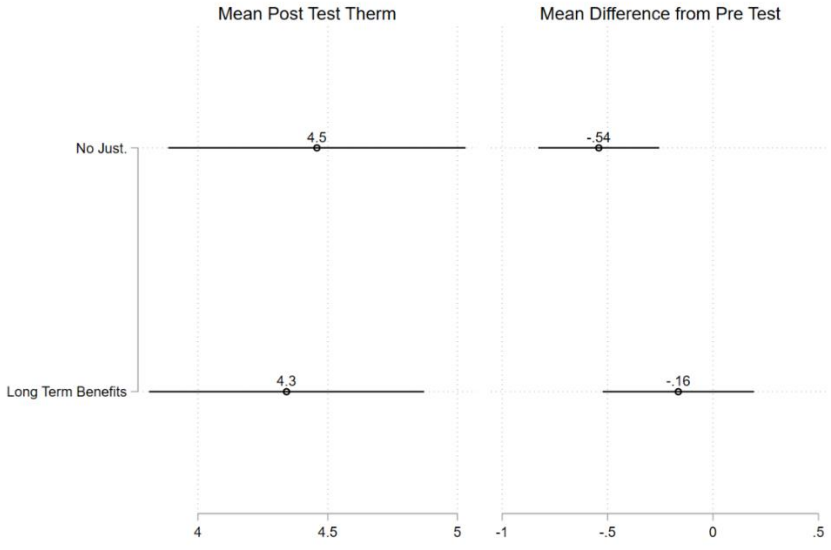


Figure OC6: Pre-Test 2



Additional Measures

Much as in Experiment 2 respondents also answered questions concerning policy approval and motive attributions. In the former case respondents were asked how strongly they approve or disapprove "of the decision to raise electricity rates that was described in the article" on a 1-7 scale (higher = approve). In the latter case, we again asked respondents to rate the importance of a "list of possible reasons" for the Representative's "vote on the electricity amendment." Much as in Experiment 1 we focus on two indices (M=0, SD=1): Good Representative Motives ($\alpha = 0.81$) and Political Motives ($\alpha=.79$).[5]

To begin with, we see that the Justification had a positive influence on policy approval and motive attributions, albeit more precisely on the former rather than the latter. The difference between the Justification and No Justification conditions are as follows: Policy = 0.37 [0.14, 0.61]; Good Representative Motives: 0.13 [-0.003, 0.27]; Political Motives: -0.10 [-0.25, 0.04]. Cohen's *d* for the three comparisons is 0.22, 0.14, and 0.10. This lack of a greater or more certain influence on motive attributions may be one reason why we did not see a stronger palliative effect of the justification in Study 2. On the other hand, the counter-narrative undermined evaluations on these items relative to the Justification Condition and particularly so when it came from a more credible source. The differences between the two credibility conditions and the Justification (No Counter) condition for the three items are: Policy: HC = -0.46 [-0.68, -0.23], LC = -0.37 [-0.59, -0.14]; Good Representative Motives: HC = -0.23 [-0.37, -0.09], LC = -0.18 [-0.32, -0.04]; Political Motives: HC = 0.16 [0.02, 0.30], LC = 0.19 [0.05, 0.33]. Thus, in all cases we see significantly worse impressions when the counter-narrative is present than when respondents just receive the justification.

Table OC5: Motives and Policy Agreement

	All Respondents			
	No Explanation	Explanation	Counter (HC)	Counter (LC)
Policy Agreement	2.94 (2.78, 3.09)	3.31 (3.14, 3.48)	2.85 (2.70, 3.01)	2.94 (2.79, 3.10)
"Political" Motives	-0.01 (-0.11, 0.09)	-0.11 (-0.22, -0.01)	0.05 (-0.05, 0.14)	0.08 (-0.02, 0.18)
"Good Representative" Motives	0.0004 (-0.10, 0.10)	0.05 (0.04, 0.23)	-0.09 (-0.19, 0.01)	-0.05 (-0.14, 0.05)
	Co-Partisans			
Policy Agreement	3.09 (2.87, 3.31)	3.58 (3.23, 3.85)	3.00 (2.76, 3.24)	2.99 (2.75, 3.23)
"Political" Motives	-0.15 (-0.30, -0.001)	-0.20 (-0.35, -0.05)	-0.01 (-0.15, 0.13)	0.001 (-0.15, 0.16)
"Good Representative" Motives	0.12 (-0.03, 0.27)	0.28 (0.15, 0.42)	-0.06 (-0.22, 0.09)	0.05 (-0.10, 0.19)
	Opposing Partisans			
Policy Agreement	2.77 (2.52, 3.01)	3.16 (2.91, 3.41)	2.77 (2.53, 3.00)	2,77 (2.66, 3.11)
"Political" Motives	0.15 (0.01, 0.29)	-0.01 (-0.16, 0.15)	0.10 (-0.04, 0.25)	0.18 (0.04, 0.32)
"Good Representative" Motives	-0.12 (-0.28, 0.03)	0.05 (-0.09, 0.18)	-0.12 (-0.27, 0.04)	-0.13 (-0.28, 0.02)

Notes: Cells provide condition means alongside 95% confidence intervals.

[5] The former index is based on the reasons: "desire to make good public policy", "a desire to help his constituents", "a desire to help all state residents", and "personal values". The latter index draws on the reasons: "the influence of special interests", "political ambition", "loyalty to higher-ups in his political party", and "wining re-election".

Online Appendix D: Experiment 3

Treatment Wordings

Gillibrand Treatment[6]

[No Justification, Baseline:

Kirsten Gillibrand used to have a moderate position on guns. Voters want to know why she's changed.

During a Tuesday night CNN town hall, Sen. Kirsten Gillibrand (D-NY) tried to tackle her biggest campaign problem: name recognition.

Gillibrand answered questions on her positions on issues such as climate change and health care during the town hall. She also reiterated her support for victims of sexual misconduct and pledged to continue to fight for legislation to combat assault and harassment.

One issue raised during the town hall that is likely to follow Gillibrand on the campaign trail is her changed stance on gun control. During her tenure in the US House of Representatives from 2007-2009, then-Rep. Gillibrand fought vigorously in defense of gun rights. During her 2008 bid for re-election to the House, Gillibrand campaigned on her pro-gun rights record, touting her "A" rating from the National Rifle Association. However, after she was appointed to the US Senate in January 2009, Gillibrand shifted from her conservative record on gun rights and her NRA grade changed from an "A" to an "F" by September 2010.

One broader challenge for Gillibrand will be cutting through the noise of a crowded field of candidates in the 2020 Democratic primary.

[Justification (Appears before "one broader challenge" ending):

Gillibrand told the audience that her position changed when she started meeting with the families of gun violence victims. "When I was a member of Congress from upstate New York, I was really focused on the priorities of my district. However, what I recognized pretty quickly when I became a senator was that I didn't spend enough time thinking about other people around the state and other families who were really suffering," she said. "When you talk to a mom and a dad who lost their teenage daughter because she was at a party with friend and a stray bullet hit her and killed her, and you meet her whole class, not only do you immediately know that you were wrong, but you know you have to do something about it."

[Counter-Explanation (appeared *before* justification when it was present):

The timing of the shift has opened her up to criticism that she flip-flopped for political reasons. "If you looked up 'political opportunism in the dictionary, Kirsten Gillibrand's photo would be next to it" said [conservative/liberal] columnist Frank Rich in an op-ed last week. "Gillibrand always goes where the political wind blows," he continued.

Corker Treatments[7]

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[6] The following news articles were consulted/used in the construction of this treatment:

- https://www.vox.com/2019/4/9/18303526/kirsten-gillibrand-cnn-town-hall-immigration-guns
- https://www.washingtonpost.com/politics/i-will-stand-up-for-what-i-believe-in-sen-kirsten-gillibrand-says--but-what-she-believes-quickly-changed-as-she-moved-from-house-to-senate/2019/01/19/1534b4ce-1b55-11e9-9ebf-c5fed1b7a081_story.html?noredirect=on&utm_term=.24ea1d6bfa7c
- http://www.msnbc.com/the-cycle/the-flip-flopping-nature-kirsten-gillibran
- https://www.washingtonexaminer.com/opinion/kirsten-gillibrand-shrugs-a-whole-career-of-flip-flopping

[7] The following news articles were used in constructing the treatment:

- https://newrepublic.com/minutes/146338/bob-corkers-flip-flop-tax-reform-sure-seems-fishy
- https://www.politico.com/story/2017/12/18/bob-corker-tax-bill-kickback-republicans-respond-302482

[No Justification Baseline:

Sen. Bob Corker reverses course, will vote for Republican tax bill

Sen. Bob Corker announced Friday that he will support the GOP tax bill, a reversal for the lone Republican to vote against the plan in the U.S. Senate.

Corker had long been critical of the proposal for not doing enough to address the national deficit. Corker was the only Republican to vote against the plan in the Senate after GOP leaders failed to satisfy his demands that the package must not increase the federal deficit.

The House and Senate each passed separate versions of the tax bill, and on Friday, GOP negotiators in the two chambers signed off on a final agreement.

A final vote is expected next week and Corker's announcement provides key support for its passage in the Senate.

[Justification (Appears before "the House and Senate each passed…"):

"After many conversations over the past several days with individuals from both sides of the aisle—including business owners, farmers, chambers of commerce and economic development leaders—I have decided to support the tax reform package. I believe this bill accompanied with the significant regulatory changes that are underway could have a significant positive impact on the well-being of Americans and help drive additional foreign direct investment in Tennessee" Corker said in a statement.

[Counter-Explanation (appears after the justification, when present):

[Conservative/Liberal] critics of Corker suggested a change to the bill may have affected his decision. Corker switched his vote after a provision was added that reduces taxes on real estate LLCs—and Corker, a real estate mogul, made $7 million in income from real estate LLCs last year. "Looked at in the best possible light," Corker's change "illustrated the unseemly haste with which this tax bill was written" said Brett Stevens, a Senior Fellow at the [conservative/liberal] think tank The Tax Policy Center. "At worst, it looks like Corker got bought off."

- http://nymag.com/intelligencer/2017/12/the-gop-tax-bill-was-corrupt-before-the-corker-kickback.html
- https://indyweek.com/news/archives/corker-kickback-one-senator-s-going-make-mint-gop-s-tax-reform-bill/

Table OD1: Summary Statistics, Covariates

	Support Stronger Restrictions	Oppose Stronger Restrictions	No Attitude
Gun Control Attitude	67.93	21.85	10.22
	Supports	Opposes	No Attitude
Tax Cut Attitude	41.68	45.72	12.60
	Co-Partisan	Opposing Partisan	Independent
Corker	38.75	44.27	16.98
Gillibrand	44.27	38.75	16.98
	Favorable	Unfavorable	No Opinion
Corker	23.03	28.60	48.38
Gillibrand	25.95	32.75	41.29

Notes: For the Corker experiment, gain proximity = those that support the tax cuts, lose proximity = those that oppose. For the Gillibrand experiment, gain proximity = support stronger restrictions, lose proximity = oppose stronger restrictions.

Additional Measures

As with Experiments 1 and 2, we asked additional measures on the post-test. In particular, we asked the following item: "The following are a list of possible reasons for why [Senator Gillibrand changed her stance on gun control; Senator Corker voted for the tax reform bill.]. Please rate how important you think each is in explaining the Senator's behavior. The following motives were asked about: winning re-election, political ambition, the influence of special interests, personal gain, a desire to help constituents, and a desire to make good public policy. Factor analyses support a two factor solution in both cases relating to negative motives (the first four) and positive ones (the final two).[8] Table OD2 provides the mean scores for these two dimensions (factor variable, m = 0, sd = 1) for Corker while Table OD3 provides regression results that include the covariates used in text. Tables OD4 & 5 do the same for Gillibrand.

Corker's justification did not significantly impact people's motive attributions relative to the No Justification counterfactual. However, the counter-explanation did influence these perceptions. In particular, the LW counter-explanation led to an increased attribution of negative motives (albeit not significant at $p < 0.05$) and a reduced attribution of positive motives to Corker in the aggregate. The same pattern occurs with the right wing source but with more precisely measured effects on the negative motives index. Co-partisans and opposing partisans look to have taken different lessons from the counter-explanation to some extent. Co-partisan attributions of negative motives are significantly higher when the counter-explanation is present than when it is absent, but no such effect emerges for opposing partisans. On the other hand, while co-partisans attribute less positive intentions to Corker, these effects are noisy, whereas they are a bit more sharply estimated for opposing partisans at least in the case of the right-wing source.

[8] Corker: Two factors had eigen values over 1 (F1: 2.64, prop explained = 0.44, F2: 1.57, proportion explained: 0.26). The scale for the four negative motives has an alpha reliability score of 0.81 while the two negative items are correlated at 0.61 (alpha = 0.76). Gillibrand: F1 (EV = 2.76, proportion explained = 0.46); F2 (EV = 1.56, proportion explained = 0.26). The negative items have an alpha of 0.84, the positive one of 0.75 (correlation = 0.61) as well.

Gillibrand's justification, on the other hand, led to a reduced attribution of negative motives (albeit not significantly so) and a significant growth in the attribution of positive motives relative to the no justification counter-factual. Both of these effects are driven by co-partisans. This influence is smaller and noisier when the counter-explanation. These results again speak to the role that justifications play in shaping motive attributions and the role that counter-explanations play in undermining this persuasive impact.

Table OD2: Motive Attributions, Bob Corker

	No Just	**Just**	**LW**	**RW**	**ANOVA**
	All Respondents				
Positive	0.10 [-0.01, 0.21]	0.13 [0.02, 0.24]	-0.14 [-0.25, -0.02]	-0.10 [-0.21, 0.02]	F = 5.64, p < 0.001
Negative	-0.10 [-0.22, 0.01]	-0.05 [-0.16, 0.06]	0.06 [-0.05, 0.17]	0.10 [-0.02, 0.21]	F = 2.60, p < 0.10
	Co-Partisans				
Positive	0.29 [0.12, 0.46]	0.45 [0.12, 0.46]	0.06 [-0.11, 0.24]	0.18 [-0.004, 0.36]	F = 3.71, p < 0.05
Negative	-0.19 [-0.40, 0.02]	-0.17 [-0.34, -0.01]	0.09 [-0.07, 0.25]	0.14 [-0.03, 0.32]	F = 3.64, p < 0.05
	Opposing Partisans				
Positive	0.02 [-0.15, 0.19]	-0.13 [-0.30, 0.04]	-0.21 [-0.40 [-0.03]	-0.24 [-0.42, -0.07]	F = 1.78, p = 0.15
Negative	0.08 [-0.07, 0.23]	0.17 [0.0001, 0.34]	0.18 [0.03, 0.33]	0.18 [0.002, 0.35]	F = 0.33, p = 0.80

Notes: "Co-P" = co-partisan, "OpP" = Opposing Partisan" Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table OD3: Experiment 3 Results, Corker Motive Attributions

	(1) Neg	(2) Pos	(3) Neg (Co-P)	(4) Pos (Co-P)	(5) Neg (OpP)	(6) Pos (OpP)
Justification	0.0458	0.0187	0.0122	0.168	0.0350	-0.120
	(0.0794)	(0.0778)	(0.130)	(0.125)	(0.112)	(0.120)
LW	0.152	-0.265***	0.279*	-0.215	0.0689	-0.242
	(0.0803)	(0.0786)	(0.129)	(0.124)	(0.115)	(0.123)
RW	0.222**	-0.230**	0.350**	-0.108	0.0716	-0.291*
	(0.0798)	(0.0782)	(0.132)	(0.127)	(0.113)	(0.121)
Unfavorable	0.155	-0.527***	0.0537	-0.491***	0.241	-0.499***
	(0.0833)	(0.0816)	(0.126)	(0.121)	(0.126)	(0.134)
No Opinion	-0.0552	-0.226**	0.0239	-0.269**	-0.0919	-0.127
	(0.0761)	(0.0745)	(0.103)	(0.0988)	(0.123)	(0.131)
Lose Proximity	0.155*	-0.318***	0.0715	-0.0694	0.276**	-0.508***
	(0.0728)	(0.0713)	(0.123)	(0.119)	(0.106)	(0.113)
No Attitude	-0.365***	-0.155	-0.367*	-0.118	-0.427*	-0.364
	(0.0991)	(0.0970)	(0.165)	(0.159)	(0.176)	(0.188)
Opposing Partisan	0.0577	-0.106				
	(0.0747)	(0.0731)				
Independent	-0.243**	-0.249**				
	(0.0902)	(0.0883)				
Gillibrand First	0.190***	0.0494	0.162	-0.0634	0.135	0.0886
	(0.0564)	(0.0552)	(0.0905)	(0.0871)	(0.0811)	(0.0868)
Constant	-0.229**	0.606***	-0.270*	0.546***	-0.184	0.624***
	(0.0857)	(0.0839)	(0.120)	(0.115)	(0.144)	(0.154)
Observations	1181	1181	456	456	521	521
Adjusted R^2	0.070	0.104	0.026	0.049	0.070	0.090

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table OD4: Motive Attributions, Kirsten Gillibrand

	No Just	**Just**	**LW**	**RW**	**ANOVA**
	All Respondents				
Positive	-0.11 [-0.22, 0.01]	0.08 [-0.04, 0.21]	-0.01 [-0.12, 0.10]	0.04 [-0.08, 0.14]	F = 1.96, p = 0.12
Negative	0.06 [-0.05, 0.17]	-0.08 [-0.20, 0.04]	-0.05 [-0.16, 0.07]	0.07 [-0.04, 0.18]	F = 1.70, p = 0.17
	Co-Partisans				
Positive	0.11 [-0.03, 0.25]	0.41 [0.25, 0.57]	0.39 [0.24, 0.54]	0.30 [0.17, 0.44]	F = 3.50, p < 0.05
Negative	0.08 [-0.05, 0.20]	-0.25 [-0.41, -0.08]	-0.12 [-0.28, 0.05]	-0.06 [-0.21, 0.08]	F = 3.25, p < 0.05
	Opposing Partisans				
Positive	-0.36 [-0.56, -0.16]	-0.17 [-0.37, 0.02]	-0.21 [-0.38, -0.04]	-0.25 [-0.46, -0.03]	F = 0.65, p = 0.58
Negative	0.07 [-0.15, 0.28]	0.23 [0.05, 0.42]	0.14 [-0.03, 0.32]	0.31 [0.11, 0.52]	F = 1.17, p = 0.32

Table OD5: Experiment 3 Results, Gillibrand Motive Attributions

	(1) Neg	(2) Pos	(3) Neg (Co-P)	(4) Pos (Co-P)	(5) Neg (OpP)	(6) Pos (OpP)
Explanation	-0.146	0.219**	-0.343**	0.252*	0.156	0.189
	(0.0809)	(0.0752)	(0.106)	(0.102)	(0.137)	(0.128)
LW Counter	-0.0852	0.124	-0.204	0.228*	0.106	0.141
	(0.0812)	(0.0754)	(0.108)	(0.104)	(0.138)	(0.129)
RW Counter	0.00768	0.175*	-0.148	0.149	0.206	0.121
	(0.0809)	(0.0752)	(0.106)	(0.102)	(0.141)	(0.132)
Unfavorable	0.0182	-0.531***	-0.0180	-0.455***	-0.191	-0.625***
	(0.0840)	(0.0781)	(0.113)	(0.109)	(0.155)	(0.145)
No Opinion	-0.143	-0.260***	-0.113	-0.295***	-0.396*	-0.390*
	(0.0739)	(0.0687)	(0.0848)	(0.0813)	(0.163)	(0.153)
Lose Proximity	0.241**	-0.583***	0.0139	-0.449***	0.286**	-0.698***
	(0.0756)	(0.0702)	(0.129)	(0.124)	(0.110)	(0.102)
No Attitude	-0.186	-0.231*	-0.0148	0.0260	-0.173	-0.318*
	(0.100)	(0.0932)	(0.177)	(0.170)	(0.168)	(0.157)
Opposing Partisan	0.203**	-0.204**				
	(0.0705)	(0.0655)				
Independent	-0.104	-0.332***				
	(0.0862)	(0.0801)				
Gillibrand First	-0.00184	0.0839	-0.0172	0.0845	-0.144	0.0750
	(0.0571)	(0.0530)	(0.0772)	(0.0741)	(0.0978)	(0.0914)
Constant	0.0116	0.393***	0.140	0.343***	0.291	0.353*
	(0.0810)	(0.0752)	(0.0947)	(0.0909)	(0.173)	(0.161)
Observations	1182	1182	520	520	462	462
Adjusted R^2	0.044	0.176	0.008	0.083	0.033	0.153

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Online Appendix E

In text we focus on experiments where participants are randomly assigned to one of the following three basic counterfactuals: a politician does something without explanation; they provide an explanation for their behavior; they provide this explanation while another actor provides a rival account to make sense of the politician's behavior. As we discuss in the Conclusion of the manuscript, an alternative counterfactual of interest is also present: what happens when the counter-explanation is actually the *only* account that people receive? Perhaps it is the case that politicians are not better off when a counter-explanation is present than when it is absent, but they are better off when they provide an explanation in a competitive environment compared to one where they do not explain themselves while some other actors does.

We fielded a small experiment that sheds some light on these considerations. Specifically, we recruited 319 participants from Mechanical Turk and asked them to consider a situation wherein their state representative voted in favor of a recently passed budget amendment that would "would change the criteria used to distribute funds to fire departments in local communities in the state," such that there would be "cuts in funding for some communities, including your own, but increases for others." [See below for full treatment wordings.] One important deviation from Experiment 1 is present: we did not provide information concerning the partisanship of the politician.

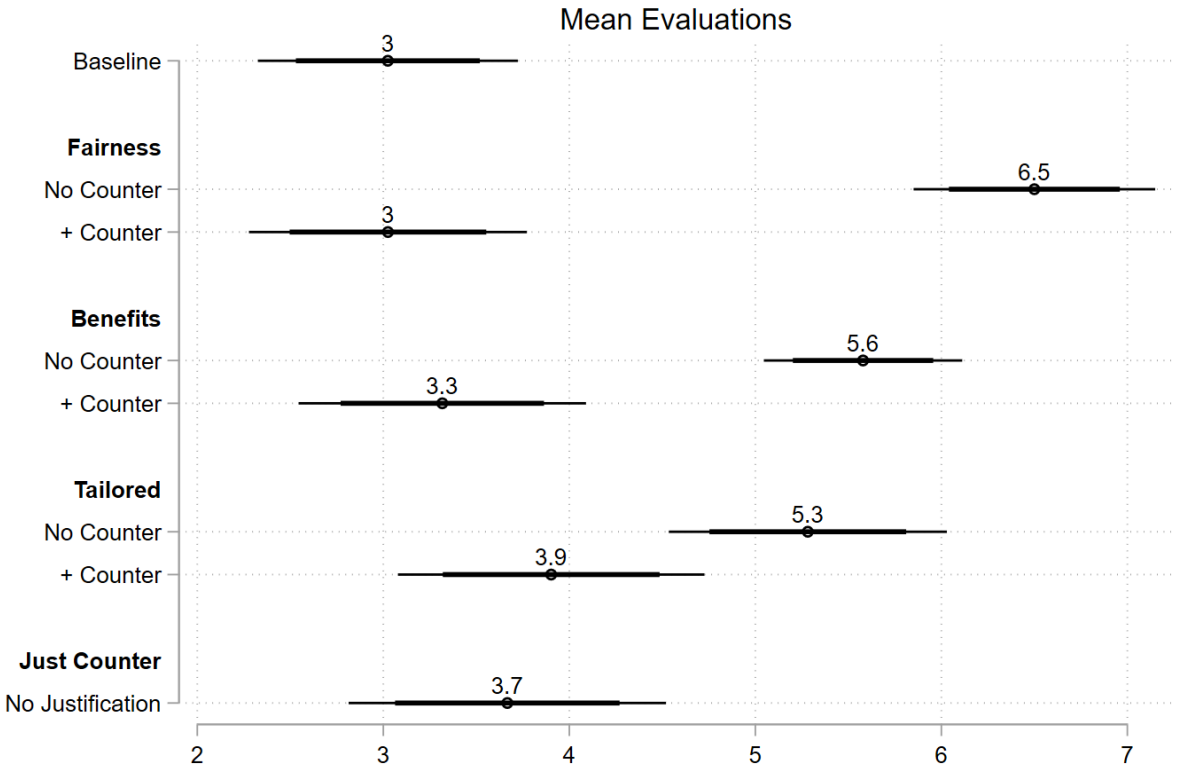
We randomly assigned participants to read one of eight such vignettes after which they evaluated the legislator on a 0-10 feeling thermometer measure. Respondents assigned to the baseline condition received information only concerning the legislator's vote for the amendment. Respondents in three of the remaining conditions also received a justification from the legislator for their vote. One focused on fairness motives, i.e. to "make the distribution of funds across the state fairer" (similar to the one used in Experiment 1). In the second justification, the legislator argues that his/her constituents will ultimately benefit because the budget "decreases property taxes and increases spending for the school system in my district". The third type of justification was a 'tailored' account along the lines used by Grose et al. (2015). Here, the legislator highlighted past positive actions on this issue ("last term I sponsored bills to increase funding to improve fire safety and to combat arson") and grounds their vote in the need for a legislative compromise to pass the budget. These three conditions replicate prior work and we expect them elicit to more positive evaluations relative to the baseline condition.

The final four conditions include a counter-narrative for the elite's behavior. Here, respondents read that "non-partisan budget experts quoted in media reports about the cut say that campaign donors" of the representative "benefit financially from the amendment and that this is why the Representative voted for it" (again, similar to Experiment 1). In one of these conditions the counter-explanation was provided by itself, i.e. absent one of the justifications. In the remaining conditions, meanwhile, we paired the counter-narrative with one of the above justifications.

Figure OE1 provides the mean ratings of the representative per condition alongside 95% and 83.5% confidence intervals; the latter are more appropriate for visually approximating whether the difference between two condition means is statistically significant (Bolsen and Thornton 2014; Goldstein and Healy 1995). Four results stand out in Figure 1. First, evaluations of the legislator in the no justification baseline condition are very low. A legislator taking the actions described in the vignette is likely to elicit quite negative reactions from constituents sans explanation. Second, evaluations are substantially more positive in all three conditions where the legislator justifies their actions. This is consistent with Hypothesis 1. Third the counter-explanation undermined the effectiveness of these otherwise effective justifications in all cases as we argued would occur in Hypothesis 2.[9] Finally, the counter-explanation, when presented by itself, did not undermine evaluations relative to the Baseline condition. If anything, then, the providing the justification did not lead the politician to *recover* from a negative situation that would otherwise occur. We hesitate to place too much weight on these analyses, however, given the small cell sizes in each condition (e.g., ~40 per condition). In addition, evaluations of the politician were already quite low in the Baseline which may constrain the ability of the counter-explanation to lead to *even worse* evaluations.

[9] The difference between the Explanation (Counter) and Explanation (No Counter) conditions is statistically significant in all three cases: Fairness (difference = -3.48 [-4.52, -2.43]); Benefits (difference = -2.26 [-3.31, -1.21]); Tailored (difference = -1.38 [-2.42, -0.34]).

Figure OE1: Evaluations of Representative in Study 1



Notes: Markers provide the mean thermometer rating for representative by treatment condition along with 95% and 83.5% confidence intervals.

Wordings of Treatments and Outcome Variable

Introduction to Vignette (common to all):

On the next few pages we will ask you to imagine a scenario where your state legislature has voted on an amendment to the annual budget plan. Please read this account as if it was provided by your own state representative in this situation.

No Explanation Vignette:

Imagine that your state's legislature recently passed an amendment to the annual budget plan which would change the criteria used to distribute funds to fire departments in local communities in the state. This amendment would result in cuts in funding for some communities, including your own, but increases for others. Your state representative voted in favor of this amendment.

Fairness Vignette:

Imagine that your state's legislature recently passed an amendment to the annual budget plan which would change the criteria used to distribute funds to fire departments in local communities in the state. This amendment would result in cuts in funding for some communities, including your own, but increases for others. Your state representative voted in favor of this amendment.

When asked about the vote, your representative said: "I voted for the amendment because my community generates enough funding for our local firefighters already and this amendment will make the distribution of funds across the state fairer for all communities with more money going to those who need the funding the most."

Fairness + Counter Vignette:

Imagine that your state's legislature recently passed an amendment to the annual budget plan which would change the criteria used to distribute funds to fire departments in local communities in the state. This amendment would result in cuts in funding for some communities, including your own, but increases for others. Your state representative voted in favor of this amendment.

When asked about the vote, your representative said: "I voted for the amendment because my community generates enough funding for our local firefighters already and this amendment will make the distribution of funds across the state fairer for all communities with more money going to those who need the funding the most." However, non-partisan budget experts quoted in media reports about the cuts say that campaign donors of Representative A benefit financially from the amendment and that this is really why the Representative voted for it.

Benefits Vignette:

Imagine that your state's legislature recently passed an amendment to the annual budget plan which would change the criteria used to distribute funds to fire departments in local communities in the state. This amendment would result in cuts in funding for some communities, including your own, but increases for others. Your state representative voted in favor of this amendment.

When asked about the vote, your representative said: "A compromise was required to pass this budget. In the end I believe my constituents will benefit from this budget because, while it unfortunately cuts funding to the fire department, it also decreases property taxes and increases spending for the school system in my district."

Benefits + Counter Vignette:

Imagine that your state's legislature recently passed an amendment to the annual budget plan which would change the criteria used to distribute funds to fire departments in local communities in the state. This amendment would result in cuts in funding for some communities, including your own, but increases for others. Your state representative voted in favor of this amendment.

When asked about the vote, your representative said: "A compromise was required to pass this budget. In the end I believe my constituents will benefit from this budget because, while it unfortunately cuts funding to the fire department, it also decreases property taxes and increases spending for the school system in my district." However, non-partisan budget experts quoted in media reports about the cuts say that campaign donors of Representative A benefit financially from the amendment and that this is really why the Representative voted for it.

Tailored Vignette:

Imagine that your state's legislature recently passed an amendment to the annual budget plan which would change the criteria used to distribute funds to fire departments in local communities in the state. This amendment would result in cuts in funding for some communities, including your own, but increases for others. Your state representative voted in favor of this amendment.

When asked about the vote, your representative said: "I have been a strong supporter of firefighters throughout my

career. Last term I sponsored bills to increase funding to improve fire safety and to combat arson. However, while this amendment has its flaws, we had to pass a budget and a compromise was required."

Tailored + Counter Vignette:

Imagine that your state's legislature recently passed an amendment to the annual budget plan which would change the criteria used to distribute funds to fire departments in local communities in the state. This amendment would result in cuts in funding for some communities, including your own, but increases for others. Your state representative voted in favor of this amendment.

When asked about the vote, your representative said: "I have been a strong supporter of firefighters throughout my career. Last term I sponsored bills to increase funding to improve fire safety and to combat arson. However, while this amendment has its flaws, we had to pass a budget and a compromise was required." However, non-partisan budget experts quoted in media reports about the cuts say that campaign donors of Representative A benefit financially from the amendment and that this is really why the Representative voted for it.

Just Counter Vignette:

Imagine that your state's legislature recently passed an amendment to the annual budget plan which would change the criteria used to distribute funds to fire departments in local communities in the state. This amendment would result in cuts in funding for some communities, including your own, but increases for others. Your state representative voted in favor of this amendment. Non-partisan budget experts quoted in media reports about the cuts say that campaign donors of Representative A benefit financially from the amendment and that this is why the Representative voted for it.

Table OE1. Sample Characteristics – Demographic Variables

	Sample	**ACS**	**ANES 2016 (Weighted)**	**ANES 2016 (Unweighted)**
% Female	39.81	51.3	51.96	52.90
Age				
Average	36.18		47.31	49.58
% 18-29	31.35	21.5	15.71	15.71
% 30-44	51.10	25.0	25.11	25.11
% 45-64	15.05	33.8	36.27	36.27
% 65+	2.51	19.7	22.92	22.92
Education				
% < HS	0.63	12.6	9.14	6.67
% HS	10.66	27.7	28.88	19.16
% Some College	31.66	31.0	30.97	35.49
% Bachelor Degree	43.26	18.3	18.33	22.59
% Post-Bach. Degree	13.79	10.5	12.69	16.09
Household Income				
Median	$50,000 to $59,999	57,617		$55,000-$59,999
<10,000	3.45	6.7	9.45	9.14
10,000-39,999	29.78		26.69	27.16
40,000-69,999	31.66		22.11	22.93
70,000-99,999	19.44		16.41	16.49
100,000-149,999	10.66	14.0	13.49	13.25
150,000+	5.02	12.1	11.85	11.03
Race		[see note]		
White	79.31	72.6		
African American	6.27	12.7		
Asian	12.23	5.4		
Other	2.19	9.3		
Latino/Hispanic (% Yes)	8.15	17.8 [see note]		
White (~Hispanic)	73.35		69.17	71.68
Black (~Hispanic)	4.70		10.92	9.39
Asian	12.23		3.12	3.49
Hispanic	8.15		11.89	10.62
Other	1.57		4.9	4.81
Party Identification				
Mean PID	3.50 (2.13)	N/A	3.82 (SE: 0.04)	3.86 (2.15)
% Dem	56.78	N/A	45.98	45.67
% Ind.	34.07	N/A	14.67	13.63
% Rep.	9.15	N/A	39.35	40.70

Notes: ACS Estimates stem from the 2016 ACS 1-year estimate files. Gender and Age statistics were constructed from Table B01001. Education is constructed from Table S1501. Income is constructed from Table S1901; the

income categories for the ACS do not neatly overlap as they begin from 10-14,999 and then move in ten thousand increments from there while our data begins at 10,000-19,999 and then proceeds via ten thousand increments until 100,000. The ACS estimates that do appear represent those categories that overlap with our own. Race & Latino/Hispanic estimates are from Tables B02001 and B03003. Note however that these tables focus on the *total* population including those < 18 years old. As younger Americans are more diverse than older, this yields estimates of higher diversity and particular for Latinos (see comparison between the ACS and ANES).

The Effectiveness of Elite Explanations in a Competitive Environment (#6530)

Created: 11/02/2017 08:33 AM (PT)
Shared: 01/07/2019 12:47 AM (PT)

1) Have any data been collected for this study already?
No, no data have been collected for this study yet.

2) What's the main question being asked or hypothesis being tested in this study?
Previous work suggests that political elites that take unpopular actions (i.e. voting for unpopular bills) can evade punishment for their actions by offering an explanation for why they acted the way they did. However, this work has not explored contexts wherein other actors interject to argue that the elite's actions were actually motivated by negatively valenced motives. We seek to understand whether these strategic counter-explanations undermine the effectiveness of the original explanation.

To answer this question, we will field an experiment with a 6 (Baseline Information [B], Vote for Cuts [V], Vote+Explanation [E], Vote+Explanation+Counter-Explanation1 [CH1], + Counter2 [CH2], + Counter3 [CL1]) x 2 (legislator partisanship: [Democrat, Republican]) design. The three counters have the same content but vary in speaker credibility (two high, one low [e.g. a partisan source]).

We have the following expectations regarding legislator evaluations: (1) B > V (the vote costs the legislator support); (2) E > V [the explanation mitigates evaluative costs]); (3) E > CL1 > (CH1 ≈ CH2) [the counter-explanation undermines the explanation's effects, particularly when given by a highly credible source]. We do not express a strong prior regarding whether the explanation will fully mitigate costs (thus, we expect: B ≥ E > V) or whether the counter will fully undermine the effects of the explanation (thus: [CH1 ≈ CH2] ≥ V).

We also postulate that respondent partisanship will matter in three ways, focusing on co-partisans to the legislator (CP) and opposing partisans (OP): (1) The effect of the elite's explanation for their own behavior will have a more positive effect among CP than OP; (2) the effect of the credible counter-explanation will not differ significantly across partisanship alignment (i.e. effect(CP) ~ effect(OP); (3) the low credibility counter-explanation will undermine evaluations (relative to the explanation only condition) most strongly among those that share a partisan allegiance with the counter-explanation giver as individuals that share a similar partisan identity as the legislator will discount this strategic charge.

3) Describe the key dependent variable(s) specifying how they will be measured.
Our core dependent variable is a general evaluation of the legislator scored on a 0-10 scale where higher = more positive affect.

We will also investigate the following variables. Our theory holds that the elite explanation mitigates the evaluative costs of taking an unpopular position via two potential routes: (1) by persuading some people that the act was the right one and thereby leading to less blame & more credit given for the action; and (2) by leading individuals to believe that the elite was positively (rather than negatively) motivated and thereby undermine blame attributions (albeit without the offering of credit for the behavior). In turn, the counter-explanations should upset these pathways. We will thus measure whether the respondent agrees or disagrees with the elite's action, the level of credit/blame accorded the elite for the action, and perceptions of the elite's motives for taking the action.

4) How many and which conditions will participants be assigned to?
12 conditions as noted above.

5) Specify exactly which analyses you will conduct to examine the main question/hypothesis.
Our key test for the first set of predictions above is a consideration of difference in condition means across the six treatment conditions; here we will use ANOVA/t-tests and OLS regressions. The second set of expectations are conditioned on respondent partisanship; we will thus focus on OLS regressions where co-partisanship is included as a moderator variable and compare the effect of the relevant treatment both within and across partisan co-identification.

6) Describe exactly how outliers will be defined and handled, and your precise rule(s) for excluding observations.
None currently planned

7) How many observations will be collected or what will determine sample size? No need to justify decision, but be precise about exactly how the number will be determined.
1800 respondents.

8) Anything else you would like to pre-register? (e.g., secondary analyses, variables collected for exploratory purposes, unusual analyses

planned?)

We expect to run three secondary analyses. First, prior work shows that account satisfaction matters; individuals that receive an explanation but find it unsatisfactory, i.e. reject it, should not better evaluate the elite (e.g. McGraw 1990, 1991). We will thus investigate whether subjective satisfaction moderates the reception of the accounts by including this factor as a moderating variable in the analyses described above.

Second, we will ask two manipulation checks wherein the respondents are asked to indicate the party of the legislator and how they voted regarding the amendment. We will generate a robustness check wherein we focus on cases wherein the respondents are correct on these items.

Finally, we will consider whether the elite's explanation is more effective when offered by a Democrat than a Republican. Voting for a cut to education funding is a potentially more surprising or counter-stereotypical action for a Democratic than Republican legislator; prior work suggests that actions such as these may grant the Democratic legislator enhanced credibility as their actions may be inferred as conflicting with their general interests (Alt, Lassen, and Marshall 2015; Berinsky 2017).

Explanations, Counter-Narratives, and Blame Management (#11852)

Created: 06/14/2018 02:37 AM (PT)
Shared: 01/07/2019 12:46 AM (PT)

1) Have any data been collected for this study already?
No, no data have been collected for this study yet.

2) What's the main question being asked or hypothesis being tested in this study?
Our study concerns when elected officials can escape blame for taking controversial actions by justifying the action and whether counter-narratives provided by alternative actors focusing on the ulterior motives of the elite can attenuate this explanation effect.

Our first hypothesis (H1) is that they can, i.e. that evaluations of the elite, the action, and their motives, will be more positive when a justification is offered than when it it absent. Our second hypothesis (H2) is that this positive effect will be attenuated when a counter-narrative suggesting ulterior motives were in play is also present. Our third hypothesis (H3) is that this attenuation will be greater when the counter-narrative is presented by a highly credible source. Our final hypothesis (H4) is that credibility will matter more for co-partisans or those with positive prior attitudes toward the legislator than opposing partisans who will be likely to take negative information about the actor from high and low credible sources.

3) Describe the key dependent variable(s) specifying how they will be measured.
We will examine the following dependent variables: (1) the change in evaluations of the legislator on the post-test from a pre-test measure; we will examine the 0-10 scaled post-test measure controlling for the 0-10 scaled pre-test measure, a difference score (i.e. post-pre), and a simplified version of the difference measured (-1 = negative change, 0 = same, +1 = positive change); (2) approval of the policy action on a 1-7 scale from strongly disapprove to strongly approve; (3) positive and negative motive attributions and the difference between them (i.e. positive motives - negative; both measures are formed from a battery of items and reduced via factor analysis); and (4) how much credit or blame the actor receives for taking the action in question.

4) How many and which conditions will participants be assigned to?
8 in total, from a 2 x 4 factorial.

Respondents will first take part in an impression formation task where they will be given information about a state legislator. The legislator will be randomly assigned to be either a Democrat or Republican. Later, after a series of buffer items, respondents will be assigned to one of four conditions: (1) No Explanation (legislator does not offer an explanation for their actions); (2) Explanation; (3) Explanation + Counter-Narrative (High Credibility Source); or (4) Explanation + Counter-Narrative (Low Credibility Source).

5) Specify exactly which analyses you will conduct to examine the main question/hypothesis.
We will regress our dependent variables on the four treatment condition assignment (base = Explanation Provided). We will also interact this indicator with a measure of co-partisanship to the legislator and with pre-test evaluations to examine moderation by prior evaluations and partisan allegiance.

We will also use t-test and Wald tests to compare the difference in evaluations between High and Low Credibility (both directly against each other and their difference from the Explanation condition, e.g. [(Explanation - High Credibility) - (Explanation - Low Credibility)].Likewise, we will investigate the difference between our outcome variables in the Counter-Narrative conditions to the No Explanation condition using t-tests.

Finally, we will use multinomial logit analyses to estimate the simplified change indicator (i.e. negative, no, and positive change) described above.

6) Describe exactly how outliers will be defined and handled, and your precise rule(s) for excluding observations.
The elected official in the experiment is described as a state legislator from Vermont. We will thus replicate our models without any Vermont residents to see if they are driving any results.

7) How many observations will be collected or what will determine sample size? No need to justify decision, but be precise about exactly how the number will be determined.
1600 respondents, yielding approximately 400 per our four main conditions or 200 per the full 2 x 4 factorial.

8) Anything else you would like to pre-register? (e.g., secondary analyses, variables collected for exploratory purposes, unusual analyses planned?)

Explanations and Counter-Explanations with Real Politicians (#22342)

Created: 04/18/2019 12:39 AM (PT)
Shared: 06/17/2019 11:58 PM (PT)

1) Have any data been collected for this study already?
No, no data have been collected for this study yet.

2) What's the main question being asked or hypothesis being tested in this study?
Do explanations offered by politicians mitigate blame for controversial actions when competing voices insinuate that the politician has ulterior motives for the action? We expect that explanations are effective in a non-competitive environment but that their influence will be mitigated in this type of competitive environment. Moreover, we expect that this mitigation will be stronger when a more credible source attacks the focal politician and that credibility will matter more for co-partisans to the focal politician.

3) Describe the key dependent variable(s) specifying how they will be measured.
Our core DV is a general evaluation of the legislator on a 0-10 scale where higher = more positive. We also assess the perceived motives of the legislator for the actions described in the experimental vignette on a 1-5 scale (higher = more important). We will analyze these both separately and as two separate sub-scales (dependent on scale reliability).

4) How many and which conditions will participants be assigned to?
Respondents will read about two politicians (Kirstin Gillibrand and Bob Corker) with the order randomly varied. Within each candidate experiment, they will be assigned to one of four conditions: (1) No Explanation (the legislator changes policy positions without explaining why); (2) Explanation (they explain why); (3) Explanation + Left-Wing Counter (they explain why and a left-wing source questions the explanation); (4) Explanation + Right Wing Counter (the counter information is from a right-wing source). We expect that a LW source should be deemed more credible by Democrats and a RW source as more credible among Republicans, all else equal.

5) Specify exactly which analyses you will conduct to examine the main question/hypothesis.
We will compare respondents across experimental treatments using ANOVA/t-tests and OLS regressions. We will first do so with a truncated version of treatment assignment (No Explanation vs Explanation vs Explanation w/Counter) before disaggregating the two counter-explanation conditions. We will perform two sets of regressions: one without covariates and one with (covariates will include: pre-test attitude toward the candidate; whether the respondent has gained/lost issue proximity from the politician's behavior; partisanship/co-partisanship; and the order of the politician experiment). To test whether credibility matters we will use Wald tests comparing the coefficients for the two counter-information treatments and also t-tests. To examine heterogenous treatment effects we will interact treatment assignment with respondent partisanship. For analyses of motives we will pay particular attention to two: personal ambition and personal gain. The counter-explanation in the Gillibrand experiment calls attention to ambition while the one in the Corker treatment focuses on personal gain.

6) Describe exactly how outliers will be defined and handled, and your precise rule(s) for excluding observations.
We do not expect any.

7) How many observations will be collected or what will determine sample size? No need to justify decision, but be precise about exactly how the number will be determined.
We have contracted for 1200 respondents (~400 per treatment condition).

8) Anything else you would like to pre-register? (e.g., secondary analyses, variables collected for exploratory purposes, unusual analyses planned?)
We will also analyze interaction models wherein experimental treatment is interacted with the respondent's pre-test attitude to the politician and whether they gained/lost proximity due to the politician's behavior. Generally, we would expect similar patterns as with co-partisanship, e.g. those with favorable pre-test attitudes/gain proximity should be more wiling to accept the explanation and more willing to counter-argue contrary information with the result being that credibility should matter more for these respondents as well.