

Can Learning Constituency Opinion Affect How Legislators Vote? Results from a Field Experiment*

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ABSTRACT

When legislators are uninformed about public opinion, does learning constituents' opinion affect how legislators vote? We conducted a fully randomized field experiment to answer this question. We surveyed 10,690 New Mexicans about the Governor's spending proposals for a special summer session held in the summer of 2008. District-specific survey results were then shared with a randomly selected half of the legislature. The legislators receiving their district-specific survey results were much more likely to vote in line with constituent opinion than those who did not. Our results suggest that legislators want to be more responsive to public opinion than they are in their natural state and can be if given solid information about constituent beliefs.

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When legislators are uninformed about public opinion, does learning constituents' opinion affect how legislators vote? For legislators acting strictly as trustees, the answer is no. Such legislators vote as they deem best, regardless of constituent opinion. Similarly, under the gyroscopic conception of representation (i.e., the idea that a representative looks within when making decisions) learning constituent opinion should have no effect on legislators' behavior (Mansbridge, 2003). In contrast, other conceptions of representation suggest that legislators should/do try to vote in line with constituent opinion and thus learning that opinion should impact their voting behavior (see e.g., Achen, 1978; Mansbridge, 2003). For example, if legislators act as delegates, they should be responsive to constituent opinion. Under these alternative perspectives on representation, much of the normative appeal of representative democracy hinges on the expectation that elected officials are responsive to their constituents' views.

Reflecting the importance of this question, the empirical study of the relationship between elected representatives and constituents is central to political science and has a long history in the discipline (e.g., Miller and Stokes, 1963; Kuklinski, 1978; Arnold, 1990; Bartels, 1991; Stimson *et al.*, 1995; Wlezien, 1995, 2004; Gilens, 2005; Clinton, 2006; Kousser *et al.*, 2007; Peress, 2008; Guisinger, 2009; Matsusaka, 2010). The majority of these studies conclude that legislators are responsive to public opinion because they find a positive correlation between measures of public opinion/preferences and how legislators vote.

While legislator responsiveness to public opinion is one data generation process that would lead to this observed relationship, many other causal mechanisms and/or routes to achieving representation could account for the observed correlation between public opinion and legislators' actions. For example, elections affect the ideological leanings of representatives by causing turnover in who holds office across districts and over time (e.g., Brady and Sinclair, 1984; Rhode, 1991; Poole and Rosenthal, 1997; Poole, 2007). If liberal districts elect liberal candidates, conservative districts conservative candidates, and out of step-incumbents are voted out of office, then a positive correlation would exist between district opinion and the behavior of the elected officials even if those officials were not at all responsive to their constituent's opinion. In other words, the observed correlation may simply be the result of the replacement rather than the conversion of current legislators.

Similarly, the observed correlation could be driven by how candidates self-select into running for office in the first place. Citizen-candidate models (Osborne and Slivinski, 1996; Besley and Coate, 1997; see also Luttbeg, 1968; Erikson and Tedin, 2007) suggest that individuals who are likely to vote in a liberal (conservative) fashion in office are more likely to run if they are from a liberal (conservative) district. Because people can only take an elected position if they run in the first place, such candidate self-selection would lead to a positive correlation between public opinion and legislators roll-call votes.

Alternatively, elites might influence public opinion (e.g., Jacobs *et al.*, 1998; Jacobs and Shapiro, 2000). Rather than elites taking their cues from the public, it might be that individuals in the public are changing their attitudes and opinions to match those of their elected officials. Again, this would cause a positive relationship between public opinion and the behavior of elected officials even if those elected officials were completely unresponsive to their constituents.

While some of these alternative mechanisms are straightforward and legitimate routes to representation, they potentially confound the empirical task of determining whether legislators are responsive to public opinion. Yet knowing whether legislators are responsive to public opinion provides a test for one of the most fundamental questions of representation: are legislators willing to act as delegates or only as trustees?

We use a randomized field experiment to isolate whether legislators are responsive to public opinion (and thus whether they are willing to act as delegates). Rather than attempt to completely model the dense web of complicated endogenous feedback mechanisms involved in representation, information about constituent opinion can be isolated and randomly assigned to be provided to legislators or not. The construction of a control group not provided the information on constituent beliefs allows a comparison of behavior between similarly situated legislators differing only in exposure to the poll results. While not offering a picture of the entire process of representation, the randomized field experiment allows an accurate measure of the extent to which information about constituent opinion affects legislators' vote.

The randomized field experiment was conducted during New Mexico's special legislative session in the summer of 2008. The experiment consisted of three parts. First, 10,690 New Mexicans were surveyed to measure opinions

on the issues discussed at the special session. Second, a letter containing the district-specific results of the survey was sent to a randomly selected half of the legislators. The random selection of legislators to receive the information ensured that the alternative mechanisms discussed above did not affect the estimates of responsiveness. Finally, the voting behavior of the legislators on the relevant bill was observed and compared across treatment conditions.

We intentionally chose an issue for the experiment where our letter with poll results would provide legislators new information. From a methodological standpoint, this strategy was important because there could be issues on which legislators were responsive to public opinion but the legislators were already informed about constituent opinion on the issue and our treatment could not change behavior. From a theoretical standpoint, the decision to select a topic with uncertainty influences how the results are interpreted. We find no clear relationship between district opinion and votes for legislators in the control group. In contrast legislators' voting behavior in the treatment group closely tracks opinion in the district. In their seminal study, Miller and Stokes (1963) found that legislators were responsive to what they thought public opinion was on various issues but their perceptions of public opinion were only weakly correlated with actual public opinion on those issues. Combined with our own results, this suggests that legislators want to act more like delegates but often fail because they are uninformed or misinformed about public opinion. In other words, legislators want to be more responsive to their constituents than they are in their natural setting.

Experiment During New Mexico's 2008 Special Legislative Session

Broadly, the experimental design was to survey registered voters on an issue that the legislature would subsequently be voting on and then randomly select half of the legislators to receive the survey results for their district.¹

In order to carry out this experiment, we needed to identify a bill that was very likely to come up for a vote. We also needed sufficient time to survey constituents and communicate the results of the poll to legislators. Finally,

¹ IRB approval was obtained before conducting the experiment.

we decided to look for a bill on a topic where legislators might be uncertain about constituent opinion because we wanted to test the effect of learning information when that information is new to the legislators.

After several months of following the news for what was occurring in the state legislatures throughout the U.S., these conditions were met during New Mexico's 2008 special legislative session. Governor Richardson called for the special session and announced its agenda on July 21, three weeks before the session was to be held. Three weeks allowed us sufficient time to survey the registered voters and give the results to the legislators in the treatment group before the session began. During the three weeks between when the session was called and when the session was held, the expected budget surplus from natural resource revenues declined precipitously making it unclear where voters and legislators stood on the short term spending proposed. In other words, the sudden change in the financial situation made it difficult for legislators to have their finger on the pulse of their constituents and polling data about the special session would constitute new information. Thus, we are able to test whether providing legislators information, when they are relatively uninformed, affects how they vote.

In particular, on July 9 New Mexico's governmental departments responsible for following the state's general revenue fund estimated that New Mexico would have a one-time budget surplus of nearly \$400 million for the Fiscal Year 2009 because of increased gas and oil prices (Miller and Homans, 2008a). Based on those estimates, and just 10 days after oil prices had reached their new record high, Governor Bill Richardson informed legislators on July 21 that he was calling them to the state capital for a special session that would start on August 15 and would determine how to spend the expected \$400 million one-time budget surplus. However by the time the session began, the expected size of the surplus had decreased because of dropping prices for oil and natural gas. Figure 1 shows that by the week of August 15, when the session began, the price for a barrel of oil had fallen from over \$140, where it had been a month earlier, to around \$115. Estimates during the week of the special session suggested that the expected budget surplus had fallen to \$225 million or even less (Nash, 2008; see also Miller and Homans, 2008b). Because the Governor's original spending proposals had been based on the expectation of a \$400 million dollar surplus, one of the issues going into the 2008 special session was whether there were still enough funds to pay for these proposals.

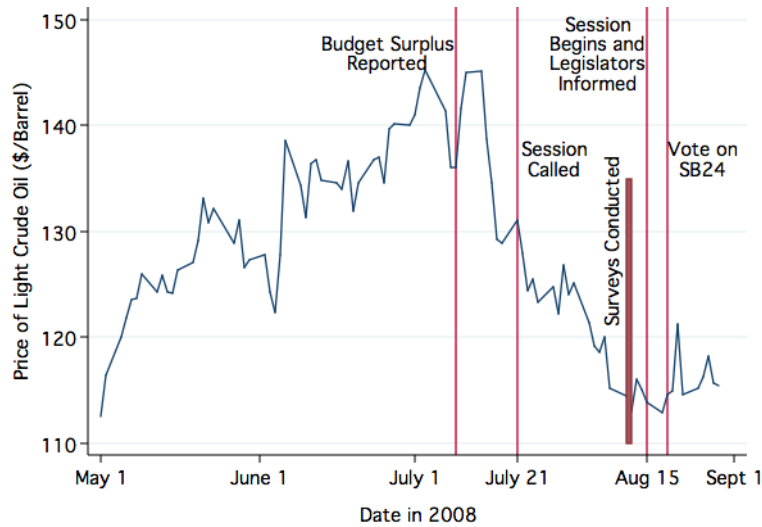


Figure 1. Oil prices in the summer of 2008 and the timing of New Mexico’s special legislative session.

Notes: The x -axis gives the dates in 2008 and the y -axis the price of light crude oil. The vertical lines indicate the timing of key components of the research design.

Measuring Constituency Opinion and the Outcome

For the first step of the experiment, Direct Leap Technologies, Inc. conducted a large-scale automated phone survey of a random sample of registered voters in New Mexico from Sunday, August 10 through Tuesday August 12 for a total of 10,690 valid responses statewide (all the districts had between 85 and 252 respondents with an average of 153 individuals per House district). We designed two survey questions with help from reporters at the Santa Fe New Mexican, the daily newspaper in New Mexico’s capital, to measure the level of support for the two proposals that Governor Richardson had announced when calling the special session: (1) covering more children with health insurance and (2) spending money on uncompleted road projects and a one-time tax rebate. The full text of both questions is provided in the template of the letter sent to the legislators who received the treatment (see Appendix A).

Our analysis focuses on the question regarding whether the legislature should pass the Governor’s spending proposals for road construction projects and tax rebates even though state revenue projections were lower than

originally expected (children's health insurance never came up in the session²). Individuals chose one of the following responses for what the legislature should do: (1) pass the original spending proposals, (2) fund these proposals but at lower levels, or (3) not spend any money on these proposals at this time.

We asked about the spending proposals together because that was how these issues were discussed in the time leading up to the session; would the state have the money to pay for the proposals? However, in the analysis we only look at the proposal for extending a one-time tax rebate. This outcome does not perfectly map onto the question we asked constituents. However, the responses to our question — especially the percentage of constituents supporting spending the full amount — were indicative of whether constituents thought that the tax rebate bill should pass because the question focused on whether the legislature should pass the spending proposals even if the expected budget surplus could not cover the costs. At the time of special session, the expected budget surplus was still around \$200 million. In the session, the legislators took up the question of the road construction bill first. Because the road construction projects would cost about \$200 million, the expected budget surplus was enough to cover those costs. The legislators thought they had enough funds for these road projects and had approved them in a prior session; it is not surprising that they unanimously approved the road construction projects contained in House Bill 10.

Because the road construction project passed before Senate Bill 24 (i.e., SB 24) — a one-time tax rebate to lower and middle income New Mexicans — was considered, the legislators already knew that the expected budget surplus, even if it came to fruition, was fully spent when they voted on the tax rebate. Thus, when they evaluated whether to pass the one-time tax rebate they were confronted with the issue posed in our question — should the legislature spend money on the proposals even if the expected budget surplus would not cover those costs? For this reason we use the percentage of respondents who supported spending the full amount of the

² At the beginning of the first day of the special legislative session a proclamation from Governor Richardson that delineated the purpose of the session was read before the legislature. In that letter, Governor Richardson outlined 13 issues that the legislature could consider (Article IV, Section 6 of the New Mexico constitution forbids the legislature from considering issues not given by the Governor in that proclamation). Because the issue of requiring parents to provide health insurance for children under 18 was not among the 13 issues in the Governor's proclamation, it did not come up during the special session.

original proposals as the primary measure of constituent opinion. These respondents were indicating their willingness to support these spending proposals even if the costs exceeded the expected budget surplus (in Online Appendix C we conduct robustness tests that allow for the possibility that legislators draw information from all three response categories — the substance of the results remains unchanged).

Figure 2 plots the *percentage of respondents favoring spending the full windfall* in the district against the *2004 Democratic, two-party, presidential vote share* in the district — a common measure for the general partisan/ideological orientation of the district (Levendusky *et al.*, 2008) and the *2006 Democratic two-party gubernatorial vote share*. Figure 2 highlights a

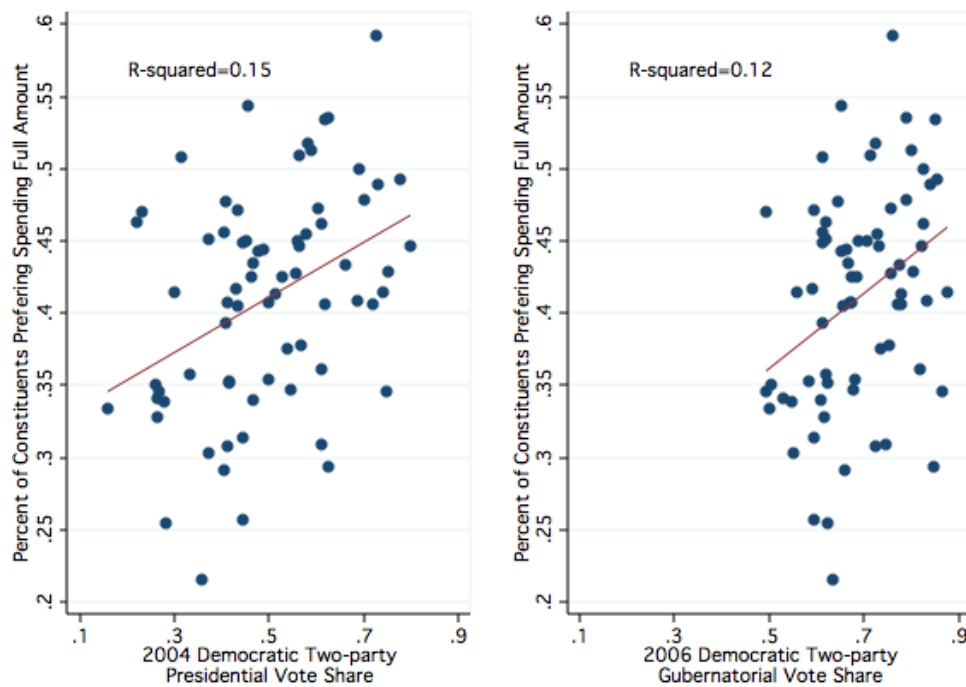


Figure 2. Percent of constituents preferring to spend the full amount vs. 2004 two-party presidential vote share and 2006 two-party gubernatorial vote share.

Notes: Each observation represents one of the New Mexico state House districts. The *y*-axis in both graphs gives the *percentage of respondents favoring spending the full windfall* in the district with the *x*-axis representing how the Democratic candidate did in the given election. The line represents the linear-best fit.

significant amount of variation across the districts in terms of their expressed level of support for these funding proposals. Figure 2 also shows that there is only a weak relationship between the spending preferences and the Presidential vote share in the district. The r -square for the binary relationship between these two variables is only 0.15. In other words, the general partisan/ideological orientation of the district, as measured by the presidential vote share, predicts only 15 percent of the observed variation in the support for the governor's proposal across districts. Thus, the treatment provides legislators with new information that could not be simply inferred from the general partisan/ideological leaning of their district.

Understanding the Treatment

The relatively small sample size of the New Mexico State House (there are only 70 seats) raises two concerns.³ First, obtaining sufficient statistical power to identify the treatment effect is difficult. Second, the district-specific support for the spending proposal varies across districts, so the information provided to legislators also differs. This means that imbalances can occur not only in observed and unobserved ancillary variables but also in the treatment itself. To ensure a balanced distribution of support for spending across districts, we first matched legislators into pairs with the individual who looked most similar to them along the following dimensions: party of the legislator, the results of our survey for their district, whether they were up for re-election, and the 2004 vote share for George Bush in the district. We then randomized within each of the 35 matched pairs to determine which of the two would receive the treatment and which would be part of the control group. Table 1 presents randomization checks and shows there is covariate balance across treatment conditions.

A letter was sent reporting the district-specific results to the legislators in the treatment group. The letters were delivered in the morning on the day the special session began, Friday, August 15. In addition to using delivery confirmation, we confirmed with New Mexico's Legislative Council Service that the letters had been delivered that morning.⁴ The full text of the template used to create the letters can be found in Appendix A. We used

³ The reason for not using both chambers is that any information given to someone in one chamber could potentially be used to inform the legislator in the other chamber who had an overlapping district. The House as opposed to the Senate was selected because the House is larger with 70 members as compared to only 42 in the state Senate.

Table 1. Randomization checks.

Variable	Treatment	Control	<i>p</i> -Value
Republican	40%	40%	1.00
Constituent support for spending	41%	41%	0.81
Constituent support for health care	54%	53%	0.35
Bush vote-share '04	51%	49%	0.59
Member vote-share '06	61%	61%	0.87
Running for re-election	91%	89%	0.70
Running unopposed	59%	58%	0.92
Supported prior health care bill	54%	51%	0.82

This table reports the results of balance tests for a number of pre-treatment covariates. The second and third columns report the percent of legislators in that treatment condition with the pre-treatment characteristic and the last column reports the *p*-value for the associated difference in means tests between legislators assigned to the treatment and control conditions.

letterhead and envelopes embossed with our University logo to increase the likelihood that the legislators read the letter. We also emailed all of the legislators in the treatment group, using one of the author's university account, in order to tell them that they would be receiving the letter.⁵ The emails also included the full text of the letter.

The treatment then consists of contacting the legislators with a letter containing information about their constituents' opinions. The letter also informed legislators that the newspaper had covered the statewide polling results. At least three potential mechanisms for how this treatment could influence legislators exist. First, legislators may have learned new information about their constituents' opinions. Second, legislators may have thought that we were monitoring them. Third, it may have been that the legislators

⁴ One concern might be that the letters arrived too late and that the legislators would not have had a chance to read the letters before having to vote on the bills. However, because the vote on the Governor's proposed tax rebates, which is what we are analyzing here, did not occur until the last day of the session, Tuesday, August 19, the treated legislators would have had ample opportunity to read their letter.

⁵ The legislators in the treatment group without a publicly available functioning email address were called.

learned that the newspaper covered the story and therefore they expected voters to monitor how they voted on this issue more than usual.

This third explanation is unlikely to drive our results. The newspaper article itself was publicly printed and so all legislators were most likely exposed to it. Response to this newspaper article would also be constant across the treatment group because the article simply reported the statewide results, and we provide below strong evidence of heterogeneity in response to the treatment. Furthermore, the Santa Fe New Mexican is the largest paper in New Mexico's capital, but it is not the largest paper in New Mexico.⁶ If learning about the newspaper article made legislators think that voters were more likely to monitor their behavior it should primarily have only affected those legislators who represent part of Santa Fe County where the newspaper is distributed. Removing the eight legislators who represent at least part of Santa Fe County does nothing to change our results.

Distinguishing between the other two mechanisms is more difficult. This difficulty arises, in part, because our treatment mimics reality. Interest groups and media outlets providing legislators with information about public opinion are also likely to monitor legislators' voting behavior. Thus, the practice of information provision is intertwined with monitoring and this dynamic may have shaped legislators' response to our treatment. Ultimately these two mechanisms can only be individually tested by future research that uses a larger sample than we had already available for this study. That said, even if the threat of monitoring is a cause of our observed treatment effects, informing legislators of their constituents' opinion is still a necessary condition for this mechanism to work (they need know what they are being held accountable to).

External Validity of Experiment

We intentionally conducted this experiment under conditions that were favorable to finding an effect. Our goal was to determine whether providing legislators *new* information about their constituents' opinion an issue affected how they voted. The vote examined only became contentious in the few days leading to the vote because of the state's changing financial situation. For topics with long-standing debates, legislators may have a better

⁶ That honor belongs to the Albuquerque Journal, which is by far the largest circulating paper statewide.

sense of constituent opinion and respond less to our treatment. Because New Mexico is among the least professionalized legislatures in the United States (Squire, 2007), we can be reasonably confident legislators are not receiving this information from other sources. In more professionalized legislatures, representatives may have access to polling data that effectively mimics our treatment and inoculate themselves against the treatment. Thus, any treatment effect detected in this experiment is likely to be larger than would be found for votes on long-standing issues in more professionalized legislatures.

We also surveyed constituents on one specific bill/issue: should the legislature spend more on a one-time tax rebate even if the expected surplus could not cover the cost? Not only was this bill well publicized, but it also dealt with a relatively specific and simple financial matter. It is not clear whether the results would apply to either less publicized matters (Arnold, 1990) or social issues. Even for economic issues, it may matter whether constituents are facing expected budget surpluses or deficits. Our results ultimately cannot answer whether legislators would be as responsive to constituent opinion on other issues. Further experimentation is necessary to see how and when information can influence legislators' voting behavior.

Expectations: The Heterogeneous Treatment Effect of Learning Public Opinion

If legislators want to vote constituents' preferences and the only obstacle to doing so is learning those preferences, a simple Bayesian updating model predicts that providing legislators public opinion data should have a heterogeneous treatment effect. When the public opinion results contradict the legislators' previously planned position on the vote, then learning that information should cause the legislators that are responsive to public opinion to vote differently. In contrast, when legislators learn that public opinion is in line with the position that they would otherwise take, learning public opinion should only reinforce their position and not lead to any observed difference in how they vote. Thus, only the legislators for whom the information from the public opinion poll contrasts with their previously planned position should be affected by learning the results of the poll.

As noted above, our specific experiment studies how legislators voted on SB 24, a bill to give low- and middle-income citizens a one-time tax rebate. Given that tax rebates are generally popular and that this vote was one

of the last votes taken two months before an election, it is not surprising that legislators' default position was to vote in favor of SB 24 (84 percent of the control group voted to pass the bill — see Appendix B). Thus, we expect that receiving the letter containing constituent opinion about the governor's spending proposal regarding road projects and these tax rebates should cause legislators in whose districts the levels of support for spending was low to vote against SB 24, but have little or no impact on legislators in whose districts levels of support for spending was high.

Results

We test our prediction by graphing the relationship between the likelihood of voting in favor of SB 24 and the percent favoring full spending on the Governor's proposals in the district separately for both the treatment and control group. We used locally weighted regression (lowess) to estimate the predicted probabilities and present them in Figure 3, with the dashed (solid)

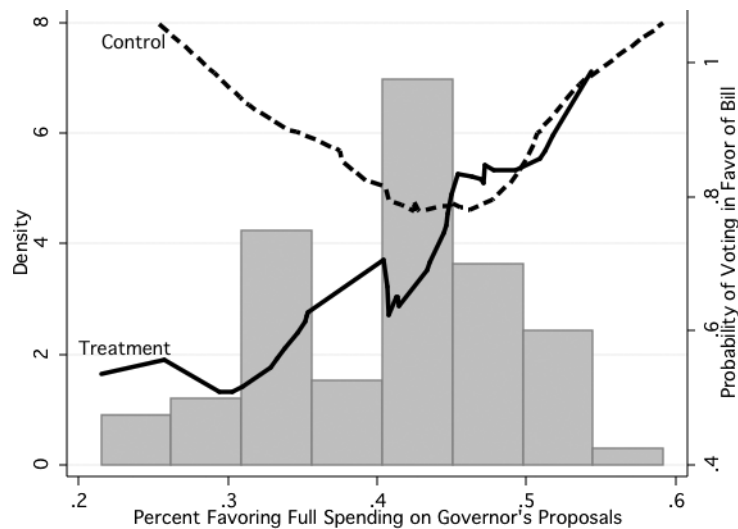


Figure 3. Relationship vote on SB 24 and district opinion by treatment groups.

Notes: We used locally weighted regression (lowess) to estimate the predicted probabilities for voting in favor of SB 24, with the dashed (solid) line representing the lowess line for the control (treatment) group. The histogram shows the distribution of constituent opinion across the New Mexico state House districts in the sample.

line representing the lowess line for the control (treatment) group. Figure 3 also displays a histogram that shows the distribution of constituent opinion across the legislative districts. We use lowess smoothing because it does not constrain the data to a linear relationship and illustrates the contrast between the treatment and control groups across all levels of constituent opinion rather than at the means.

Among the legislators assigned to the treatment group, there is a strong positive relationship between constituents' support for full spending on the Governor's proposals and their likelihood of voting yes. This relationship is nearly linear. While only about 50 percent of those in districts where support for the spending was lowest voted yes on SB24, all of those in districts where support for spending was highest voted in favor of the motion.

This pattern stands in sharp contrast to legislators assigned to the control group. Legislators in the control group did not receive information about their constituents' opinion on this issue and it shows. The correlation between constituents support for the spending and the legislators vote on SB 24 is nearly 0 ($\rho = -0.07$) for the control group. Although it looks like there might be a non-linear relationship, there is no statistical evidence for this. The joint F-test for a simple quadratic model for the control group that regressed voting in favor of SB 24 on the percent of constituents favoring full spending and the square of the percent favoring full spending had a p -value of 0.46. In contrast a simple linear model for the treatment group that regressed voting in favor of SB on the percent of constituents favoring full spending had a statistically significant positive coefficient (one-sided p -value = 0.045). The lack of a relationship among legislators in the control group suggests that we provided legislators with new information that they acted on. Without this information, legislators in districts with low levels of support for the spending were even less likely vote in line with their constituents' preferences.

The treatment effect can be found by taking the difference between the estimated probabilities of voting yes for the treatment and control groups (i.e., the difference between the two lowess lines in Figure 3). As predicted, in districts with high levels of support for the governor's spending proposals, the treatment and control groups vote in favor of SB 24 at approximately equal rates. The default position for most of these legislators was to vote in favor of the tax-rebate and those who learned that their district supported the spending proposals at high levels had no reason to deviate from that

position. In contrast, in districts with low levels of support for the spending proposals the legislators assigned to the treatment group were much less likely to vote for the tax rebate than their counterparts in the control group. Again, we can measure the treatment effect by taking the difference between the two lowest regression lines. At some points this difference gets as large as 50 percentage points. However, because the behavior of the control group appears to be somewhat haphazard, at least in regard to constituents' preferences, it may be better to compare the difference between the lowest line for the treatment group and the low point for the control group (0.8). By this standard, the treatment effect for legislators in districts where support for spending was low ranges between 10 and 30 percentage points. Even if the treatment effect is only 10 percentage points, informing legislators can have a substantively large effect and determine the passage of many bills.

We now turn to test whether the substantively significant treatment effects we observe among those legislators from districts where support for the governor's spending proposals was low are also statistically significant.⁷ For these tests we use a probit regression model, where the dependent variable takes a value of 1 if the legislator voted for the bill and 0 if he/she did not. The key independent variable is a dichotomous indicator of whether the district had *low levels of support for spending* (which takes a value of 1 for observations below the median of the percentage of respondents who favored full spending, and 0 otherwise). This coding models a decision making process where legislators do not focus on the precise level of support but instead make a determination of whether support is high or low in their district and then vote accordingly. We feel this level of granularity captures the actual consumption of the information by legislators, but as a robustness check, we provide the results of using the continuous level of constituent support in Online Appendix C, Table C.4. The regression also includes a dummy variable for whether the legislator received the treatment, and an interaction between *low levels of support for spending* and the *treatment*. The regression results are presented first without any control variables and then including controls for whether the legislator was a Republican and the district's 2004

⁷ The raw difference in votes against SB 24 between the treatment and control group are statistically significant (see Online Appendix B). These results do not take into account the heterogeneity in the treatment provided or the response to it and simply confirm that the treatment had an effect.

Table 2. Effect of treatment letter on voting in favor of tax rebates.

Independent variable	Dependent variable: vote yes on SB 24	
Treatment: received letter	0.16	-0.07
Std. error	(0.39)	(0.52)
Low support for spending	0.70	1.50
Std. error	(0.64)	(0.80)
Low support for spending*treatment	-1.49	-1.86
Std. error	(0.63)	(0.90)
Republican legislator		-1.61
Std. error		(0.59)
2004 Democratic two-party presidential vote share		4.06
Std. error		(1.88)
Constant	0.76	-0.16
Std. error	(0.33)	(1.17)
<i>N</i>	67	67
Clusters	35	35
Log-pseudolikelihood	-33.4	-18.7
Pseudo R ²	0.09	0.49

The data comes from the 2008 New Mexico special legislative session that was held from August 15–19. The dependent variable is a dummy variable for whether or not the House member voted yes on Senate Bill 24. The model estimates a probit regression. Standard errors are calculated clustering on the 35 matched pairs and are given in parentheses.

presidential election results. Standard errors are clustered on the 35 matched pairs on which the randomization was based.

The results of this regression, presented in Table 2, confirm the statistical significance of the heterogeneous relationship depicted in Figure 3. The coefficient on the treatment variable, which is the estimated treatment effect for legislators from districts that had high levels of support for the spending proposals, is close to 0, confirming that the treatment had no effect on how these legislators voted. In contrast, for legislators from districts with low levels of support for the governor’s spending proposals, receiving the letter with their constituents’ opinions on the issue strongly decreased the likelihood that they would vote in favor of the tax rebates and these differences

are statistically significant.⁸ These results confirm our prediction that the letter would have the largest effect on legislators from districts with low levels of support for the spending proposals.

For an experiment with 67 cases, these results are remarkably robust. In Online Appendix C we present a number of additional tests that we performed in order to see whether the treatment would continue to have a statistically significant effect. Among other things those results show that replacing the percent of constituents favoring spending the full amount with measures summarizing the full survey in accordance with the mean voter theorem (Caplin and Nalebuff, 1991) changes the location of the point estimate, but neither the substantive nor the statistical significance of the results (see Table C.1). Also, the specific value of the cut-off used to divide state house districts into high and low spending constituencies is not important below the middle of the distribution (Table C.2 presents the results when using a cut-off that is one standard deviation below the median for the relevant measure of constituent opinion). If the cut-off is placed much higher than the median, then cells begin to be empty and the probit model does not converge. That said, the limited dependent variable is not driving the findings, and the results are equally strong when a linear probability model is used for the analysis (see Tables C.2 and C.3). Finally, the results also hold when using the continuous measure for the percent of respondents who supported spending the full amount instead of a dummy variable for whether the district had *low levels of support for spending*. The interaction between the continuous variable and treatment is consistently substantively and statistically significant showing that the treatment group has a steeper positive slope than those in the control group. Not only do these alternative tests provide statistically significant results, but the substantive results are within the range of 10–30 percentage points (and sometimes larger). Thus, it appears that learning constituency opinion can have a substantial effect on a legislator’s vote, which is strong evidence on behalf of the delegate model of representation.

Discussion

The responsiveness of elected officials to constituency opinion is one of the great appeals of representative democracy and a fundamental difference

⁸ The statistical significance tests evaluated the following null hypothesis, $H_0: \beta_{\text{Treatment}} + \beta_{\text{Low Support for Spending} * \text{Treatment}} = 0$, and were significant at the 0.01 level.

between officials who act as delegates and trustees. Yet being responsive to voters requires knowing where they stand. A congressman interviewed in the 1960s reported, “That’s the big problem. You’re here to represent your people but you don’t know what they want. The only way to really know is to take a referendum” (quoted in Kingdon, 1973, p. 32). The situation has not necessarily changed over the past 40 years. We find that the votes of legislators in the treatment group, who are exposed to their constituents’ opinion, tracks the support among constituents, whereas there is no relationship between roll call votes and constituent opinion among legislators assigned to the control group. This suggests that legislators will act more like delegates when they have access to information about constituent beliefs. Thus, our experiment provides one explanation for why representation can be weak in some cases: legislators may not know their constituents’ opinions.

It is possible that this state of ignorance applies only to recently arising issues, but there is reason to believe that it also applies to more established issues. Miller and Stokes (1963) found that legislators were responsive to what they *thought* public opinion was on the issues, but often did not know their constituents’ preferences across a broad range of issues. Our findings provide further evidence that informing legislators makes them better delegates of their constituents’ preferences.

This finding has several implications for understanding, and possibly improving, how legislators represent their constituents’ preferences. First, our results speak to the debate over polling and the quality of representation (Geer, 1996; Geer and Goorha, 2003; Karol, 2007). Although we do not answer the question of whether the advent of polling increased the quality of representation, our results show that polling, at least under some circumstances, can be used to increase the level of congruence between legislators’ votes and their constituents’ opinions. Our own expectation is that this will depend on the credibility of the source of the polling. On one end of the credibility spectrum, interest groups and others who are attempting to pass specific legislation will be less credible unless the polling results disagree with their position. On the other end of the spectrum, we might expect non-partisan media outlets and academic polls to have greater credibility and thus a greater potential to influence legislators.

Because local media are the actors that commission opinion polls on issues before the state legislature (are the ones that monitor how legislators than

vote on the issue) our results suggest that the decline of the local media industry is likely to degrade the quality of representation at the state level. We expect the decline in the local media to have a particularly large effect on the quality of representation in less professionalized state legislatures where the legislators have fewer resources of their own to use in informing themselves about the public's opinion.

More generally, we expect that professionalized state legislators, because they have more resources to inform themselves about constituency opinion, might be better positioned to be responsive to their constituents than non-professional state legislators. They have more time and financial resources to invest in learning what their constituents want. This does not necessarily mean they are better representatives overall (such a conclusion would require an analysis of all of the different ways in which legislators might represent constituents), but we do think that they should be positioned to be more responsive to constituents' opinions because they are more likely to learn those opinions in the first place.

Although our experiment shows that informing legislators of public opinion can lead to a better match between how constituents' opinions and how legislators vote, our results also suggest that informing legislators of public opinion alone may not be enough to achieve perfect representation. Only 41 percent of constituents supported financing the spending proposals at the full level, yet the tax rebate passed by a large margin (and this would have been true if all legislators had been informed). It is possible that this discrepancy is simply a methodological artifact of the slight mismatch between our survey question we asked constituents and the bill that legislators voted on (or how we interpret the meaning of this response). A more likely possibility is that other considerations also influence how legislators vote. In our experiment we only manipulated whether legislators received information about their constituents' opinions. The behavior of legislators was possibly influenced by interest groups, party leaders, and their own preferences. In the regression analysis, the legislator's partisanship was one of the strongest predictors. While we demonstrate that legislators are responsive to public opinion when they learn new information, this does not mean that public opinion trumps all other factors when deciding votes on legislation.

More research is needed to better understand the mechanism and the external validity of these results. In terms of the mechanism, we designed the

letter to isolate the effect of informing legislators of constituency opinion. However, legislators may have thought that they were being monitored and would be held accountable by publishing the survey results for their district. Arnold (1990) argues that politicians worry not only about citizens' preferences but also the likelihood that citizens are likely to hold them accountable for their vote on that issue. Given a larger legislature on which to experiment, treatments could be designed to isolate the effect of monitoring.

It is also important to understand the conditions for which these results hold. Our experiment provides a measure for a bill in a legislature at a specific point in time. Testing a range of bills with variance in the information legislators have about constituent preferences (i.e., does the treatment provide new information), issue salience (i.e., how likely is the legislator to be held accountable), and content of the bills (e.g., economic vs. social, status quo vs. change) would allow researchers to understand when legislators act as delegates and when they act as trustees.

If future researchers use this study as a model we suggest the following steps to help ensure that their study is ethically carried out. First, as is the case with other public opinion polls that academics conduct (e.g., Quinnipiac University polls), no deception regarding the poll results should be used and pertinent information should be reported (e.g., sample size, exact question wording, etc.). Second, we suggest blocking the randomization by party to ensure that the treatment is not correlated with partisanship and that any benefit legislators obtain from receiving the poll results will not be biased in favor of either party. Third, if possible, researchers should keep the identities of the legislators involved in the experiment anonymous. We believe that following these steps will go a long way towards helping researchers carry out such research in an ethical manner.

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Appendix A. Template Used for Letters Sent to Legislators in the Treatment Group

August 14, 2008

Dear Representative < Last Name > ,

My name is Dan Butler and I am a political science professor at Yale University. This past week my colleagues and I surveyed a random sample of registered voters in New Mexico to find out their opinions about some of the issues that will be dealt with in this week's special session. The results of our survey were reported in the Santa Fe New Mexican on Thursday (see story "Poll: Majority favor health care for kids").

I am writing because we thought you might be interested in the survey results for constituents from your district. Below are the responses from the <Number> respondents from your district who answered our two-question survey. There is a standard error of X.X% for the results.

First Question:

What is your opinion about the proposal to require parents to provide health care coverage for their children who are 18 years old and younger?

XX% were in favor and the rest against.

Second Question:

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

XX% thought the legislature should still pass the original spending proposals.

XX% thought the legislature should provide funding for these proposals but at lower levels.

XX% thought the legislature should not spend any money on these proposals at this time.

If you have any questions about the survey or its methodology, please feel free to contact me at the phone number or email address listed below.

Best Regards,

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Supplemental Online Materials for “Can Learning Constituency Opinion Affect how Legislators Vote? Results from a Field Experiment”

Online Appendix B

Table B.1. Percent of treatment and control legislators voting yes on SB 24.

	No	Yes
Control	15.6% [5]	84.4% [27]
Treatment	31.4% [11]	68.6% [24]
Difference		−15.8% (7.4)

Numbers in brackets report the number of subjects in the cell. Number in parentheses reports the standard error of the difference between treatment and control groups.

Online Appendix C. Robustness Checks of Empirical Results

As a first robustness check we tested the effect of using other reasonable measures of constituents’ opinion on the spending issue. This check is important because the letters sent to legislators (see Appendix A) contained information on all three response categories. While the percentage of constituents who supported spending the full amount was likely the most informative component, legislators could draw information from all three responses. The Mean Voter Theorem suggests that legislators should be responsive to the average of their constituents’ preferences (e.g., Caplin and Nalebuff, 1991), so a *constituency spending preferences* variable was created as a summary measure for responses to this question where *constituency spending preferences* = $1 \times (\% \text{ in favor of full spending}) + \gamma \times (\% \text{ in favor of partial spending}) + 0 \times (\% \text{ in favor of no spending})$. When constructing *constituency spending preferences*, how to weight respondents saying spend the full amount (1) and

Table C.1. Using *constituency spending preference* to measure public opinion, probit regressions.

The value of γ in <i>constituency spending preference</i> [= 1*(% for full spending) + γ *(% for partial spending) + 0*(% for no spending)]							
Variable	0	0.1	0.2	0.3	0.4	0.5	0.6
Treatment	-0.07 [0.52]	-0.27 [0.53]	-0.12 [0.53]	0.08 [0.53]	0.03 [0.53]	-0.18 [0.53]	-0.18 [0.53]
Low spending	1.50 [0.80]	0.71 [0.75]	0.96 [0.74]	1.24 [0.71]	1.22 [0.70]	0.70 [0.71]	0.70 [0.71]
Interaction	-1.86 [0.90]	-1.10 [0.81]	-1.33 [0.81]	-1.61 [0.71]	-1.55 [0.71]	-1.05 [0.79]	-1.05 [0.79]
Republican	-1.61 [0.59]	-1.77 [0.61]	-1.81 [0.61]	-1.91 [0.61]	-1.95 [0.60]	-1.95 [0.62]	-1.95 [0.62]
2 Party vote share	4.06 [1.88]	2.80 [2.00]	2.79 [1.88]	2.58 [1.83]	2.56 [1.84]	2.15 [1.57]	2.15 [1.57]
Constant	-0.16 [1.17]	0.69 [1.30]	0.57 [1.26]	0.52 [1.21]	0.58 [1.20]	0.98 [0.95]	0.98 [0.95]
Observations	67	67	67	67	67	67	67

Robust standard errors in brackets. Low spending is a dummy variable for being below the median value for *constituency spending preference*.

nothing (0) is uncontroversial. The value to place on those favoring partial spending, γ , is less obvious. Table C.1 presents the results for a number of different values of γ . While this alternative measure changes the size of the point estimate, the results remain statistically and substantively significant.

We also tested how sensitive the results were to our decision of how to divide the state house districts into high and low spending constituencies. The specific value of the cut-off used is not important below the middle of the distribution. Table C.2 presents the results when using a cut-off that is one standard deviation below the median for the relevant measure of constituent opinion. If the cut-off is placed much higher than the median, then cells begin to be empty and the probit model does not converge.

Further, the limited dependent variable (and the associated decision to use a probit regression model for the analysis) is not driving the findings.

Table C.2. Results using a lower cut-off point & OLS.

	<i>% Full spending</i>	<i>Constituency spending preference</i>
Treatment	-0.11 [0.07]	-0.07 [0.05]
Low spending	0.28 [0.15]	0.42 [0.14]
Interaction	-0.34 [0.20]	-0.42 [0.19]
Republican	-0.44 [0.13]	-0.45 [0.13]
2 Party vote share	0.43 [0.28]	0.54 [0.24]
Constant	0.77 [0.17]	0.68 [0.15]
Observations	67	67

Robust standard errors in brackets. Low spending is a dummy variable for being below *more than* one standard deviation below the median value for the given measure of constituents support for the spending proposal.

Tables C.2 and C.3 present the results from linear probability models that were estimated using OLS regression and show that the results generally hold.

Finally, the results also hold when using the continuous measure for the percent of respondents who supported spending the full amount instead of a dummy variable for whether the district had *low levels of support for spending*. The interaction between the continuous variable and treatment is consistently significant and nearly 5 probits larger than the non-interacted treatment (see Tables C.4 and C.5).

Table C.3. Testing different values of gamma on *constituency spending preference* using OLS.

Variable	The value of γ in <i>constituency spending preference</i> [= 1*(% for full spending) + γ *(% for partial spending) + 0*(% for no spending)]						
	0	0.1	0.2	0.3	0.4	0.5	0.6
Treatment	-0.02 [0.08]	-0.05 [0.08]	-0.02 [0.08]	0.01 [0.07]	0.00 [0.07]	-0.02 [0.07]	-0.02 [0.07]
Constituent spending preference	0.21 [0.12]	0.13 [0.13]	0.20 [0.13]	0.26 [0.12]	0.29 [0.13]	0.21 [0.13]	0.21 [0.13]
Interaction	-0.29 [0.15]	-0.22 [0.14]	-0.29 [0.15]	-0.34 [0.13]	-0.34 [0.13]	-0.28 [0.15]	-0.28 [0.15]
Republican	-0.42 [0.14]	-0.44 [0.14]	-0.45 [0.14]	-0.46 [0.13]	-0.49 [0.13]	-0.49 [0.13]	-0.49 [0.13]
2 Party vote share	0.45 [0.32]	0.35 [0.34]	0.41 [0.35]	0.42 [0.34]	0.41 [0.34]	0.35 [0.30]	0.36 [0.30]
Constant	0.70 [0.21]	0.79 [0.23]	0.73 [0.24]	0.69 [0.23]	0.71 [0.22]	0.77 [0.19]	0.77 [0.19]
Observations	67	67	67	67	67	67	67
R-squared	0.45	0.43	0.45	0.46	0.47	0.44	0.44

Robust standard errors in brackets. Low spending is a dummy variable for being below the median value for *constituency spending preference*.

Table C.4. Using linear measure of constituents' preferences.

	<i>% Full spending</i>		<i>Constituency spending preference</i>	
	Probit	OLS	Probit	OLS
Treatment	-5.15	-1.02	-8.08	-1.65
	[1.77]	[0.37]	[2.86]	[0.63]
Spending preference	-8.14	-1.56	-11.33	-2.38
	[4.55]	[0.79]	[5.33]	[0.98]
Treatment* spending preference	10.57	2.08	13.22	2.70
	[4.43]	[0.83]	[5.22]	[1.07]
Republican	-1.78	-0.44	-1.85	-0.45
	[0.60]	[0.14]	[0.59]	[0.13]
2 Party vote share	3.25	0.50	3.14	0.54
	[1.82]	[0.32]	[1.70]	[0.31]
Constant	4.19	1.42	7.16	2.08
	[1.70]	[0.28]	[2.77]	[0.48]
Observations	67	67	67	67

Robust standard errors in brackets.

Table C.5. Testing different values of gamma when using a linear measure of constituents' preferences, probit regressions.

Variable	The value of γ in <i>constituency spending preference</i> [= 1*(% for full spending) + γ *(% for partial spending) + 0*(% for no spending)]						
	0	0.1	0.2	0.3	0.4	0.5	0.6
Treatment	-5.15 [1.77]	-6.06 [2.10]	-7.05 [2.47]	-8.08 [2.86]	-9.08 [3.27]	-9.93 [3.70]	-10.43 [4.12]
Constituent spending preference	-8.14 [4.55]	-9.16 [4.86]	-10.24 [5.14]	-11.33 [5.33]	-12.33 [5.43]	-13.09 [5.41]	-13.39 [5.25]
Treatment* spending preference	10.57 [4.43]	11.48 [4.69]	12.39 [4.96]	13.22 [5.22]	13.87 [5.45]	14.18 [5.65]	13.94 [5.81]
Republican	-1.78 [0.60]	-1.80 [0.59]	-1.82 [0.59]	-1.85 [0.59]	-1.89 [0.60]	-1.94 [0.60]	-1.99 [0.62]
2 Party vote share	3.25 [1.82]	3.25 [1.80]	3.22 [1.76]	3.14 [1.70]	3.02 [1.64]	2.83 [1.57]	2.60 [1.51]
Constant	4.19 [1.70]	5.04 [2.02]	6.04 [2.39]	7.16 [2.77]	8.35 [3.14]	9.51 [3.48]	10.45 [3.75]
Observations	67	67	67	67	67	67	67

Robust standard errors in brackets.