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The Causal Effects of Elite Position-Taking on Voter Attitudes: Field Experiments with Elite Communication*

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Abstract

Influential theories depict politicians as, alternatively, strongly constrained by public opinion, able to shape public opinion with persuasive appeals, or relatively unconstrained by public opinion and able to shape it merely by announcing their positions. To test these theories, we conducted unique field experiments in cooperation with sitting politicians in which US state legislators sent constituents official communications with randomly assigned content. The legislators sometimes stated their issue positions in these letters, sometimes supported by extensive arguments but sometimes minimally justified; in many cases, these issue positions were at odds with voters'. An ostensibly unrelated survey found that voters often adopted the positions legislators took, even when legislators offered little justification. Moreover, voters did not evaluate their legislators more negatively when representatives took positions these voters had previously opposed, again regardless of whether legislators provided justifications. The findings are consistent with theories suggesting voters often defer to politicians' policy judgments.

Replication Materials: The data, code, and any additional materials required to replicate all analyses in this article are available on the American Journal of Political Science Dataverse within the Harvard Dataverse Network, at: <http://dx.doi.org/10.7910/DVN/YZQTAH>.

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Classical conceptions of representative democracy envision public opinion placing strong constraints on politicians' decisions: electoral accountability compels politicians to support policies that are broadly popular among citizens, lest they face electoral sanction (Downs 1957; Loewen and Rubenson 2010). Consistent with these conceptions, there is a strong relationship between public opinion and politicians' policy positions (e.g., Brody and Page 1972).

Research on opinion leadership has long offered an alternative explanation for the correspondence between public opinion and the politicians' policy positions: citizens may adopt politicians' positions as their own. Over the last several decades, evidence that elites do indeed influence public opinion has mounted (e.g., Abramowitz 1978; Gabel and Scheve 2007; Lenz 2009, 2012; Minozzi et al. 2014).

Despite the breadth of opinion leadership research, the nature of opinion leadership and its implications for democratic accountability are increasingly contested. On the one hand, classic theoretical accounts characterize opinion leadership as reflecting *elite persuasion*. These accounts contend that elites shape public opinion primarily by highlighting how their policy proposals are consistent with citizens' preexisting values or arguing they will accomplish shared goals (e.g., Chong and Druckman 2007a, 2007b; Grose et al. 2014; Kinder and Sanders 1990; Jacoby 2000; Tesler 2014; Zaller 1992). Such theories generally imply that politicians' accountability to public opinion remains relatively robust: if citizens reliably demand that politicians persuasively justify their positions, politicians will eschew taking positions they do not believe they can persuasively defend (e.g., Fenno 1978; Kingdon 1989).

However, a different theoretical perspective suggests that, at least some of the time, opinion leadership is better characterized as a process of *position adoption*. This *position adoption* perspective claims that citizens often defer to politicians' policy judgments and do not

require persuasive arguments to change their views on issues. Citizens thus are not expected to reliably evaluate their representatives more negatively when learning their representatives support policies they oppose, and may even adopt their representatives' positions as their own – even in the absence of persuasive justifications (e.g., Achen and Bartels 2006; Bartels 2005; Cohen 2003; Leeper 2013; Lenz 2009, 2012; Mackie and Cooper 1984). Such *position adoption*, to the extent it occurs, implies that citizens are inclined to defer to politicians' judgment without demanding justifications. Consequently, public opinion may not constrain politicians' decisions much at all.

The notion that such position adoption can describe public opinion even some of the time has been understandably controversial (for review, see Bullock 2011), but is consistent with a number of broader theories. On the one hand, democratic theory suggests citizens may believe elites have superior policy information, expertise, or judgment (Burke 1774; see also Bianco 1994; Fox and Shotts 2009). Likewise, psychological studies suggest citizens may tend to defer to authority figures on even significant personal matters (e.g., Milgram 1974, Epilogue). Nevertheless, this perspective sits at odds with most contemporary theories of democratic politics, which tend to envision democracy as the aggregation of citizens' preferences (Miller 1992). Moreover, as we review below, existing empirical evidence in favor of this view remains explicable with plausible alternatives.

In this paper we examine the empirical merit of this *position adoption* hypothesis in a series of unique tests. Our evidence comes from field experiments conducted in collaboration with sitting politicians in which they randomly assigned aspects of their position-taking to voters. In the experiments, US state legislators sent their constituents official communications with randomly assigned content. In letters to constituents in treatment groups, the legislators staked

out positions on salient and controversial issues such as decriminalizing marijuana, the minimum wage, and policies toward undocumented immigrants. Further, legislators sometimes justified their positions on these issues with extensive arguments, but sometimes only minimally justified them. Ostensibly unrelated telephone surveys then probed the effects of this position-taking on citizens' evaluations of legislators and views on the issues.

Constituents' reactions to these official legislative communications lend support to the position adoption perspective. When legislators sent their constituents a letter containing an issue position, these constituents were significantly more likely to adopt this position. Moreover, when the letters staked out positions the constituents had previously said they opposed, constituents learned legislators disagreed with them yet their evaluations of their legislators did not sour. (We knew voters' issue positions because we surveyed them prior to the letters being sent.) Finally, these patterns did not depend on whether legislators provided extensive justifications for their positions or largely omitted them: legislators appeared to move their constituents' opinions towards their own positions just as successfully by essentially announcing their positions; likewise, citizens did not judge their legislators negatively even when their legislators baldly asserted a position with which the constituent had previously disagreed.

These findings lend unique support to the position adoption perspective and have important implications for understanding the nature of politicians' accountability to public opinion. To be sure, this evidence does not provide a comprehensive account of democratic accountability: it does *not* suggest politicians can always convince their constituents of anything, nor that they can reliably escape electoral punishment for everything. We hope future research further explores the scope conditions on these findings, as they are sure to exist. However, our results do provide unique evidence underscoring a notion deeply engrained in political thought:

across a wide range of policy matters, citizens appear willing to follow their leaders.

How Voters React to Politicians' Policy Positions: Theoretical Perspectives And Existing Evidence

To appreciate the theoretical issues at stake in our studies, consider the answers three prevalent theoretical perspectives would offer to the following question: how do voters react when politicians support policies they oppose? Table 1 summarizes these predictions.

Table 1. How Do Voters React When Politicians Support Policies They Oppose? Empirical Predictions of Competing Perspectives on Opinion Leadership

Empirical Question	<i>Issue Voting</i> Predictions	<i>Elite Persuasion</i> Predictions	<i>Position Adoption</i> Predictions
Do leaders lose support from citizens?	Yes	Yes, unless persuasive arguments provided	No
Do leaders change citizens' issue opinions?	No	No, unless persuasive arguments provided	Yes

The traditional view of democracy conceives of citizens as *issue voters*. In its strictest form, it suggests that citizens hold politicians in less esteem when politicians support policies citizens oppose, and that politicians cannot meaningfully influence citizens' policy preferences. Indeed, in this view, just about the least politically advantageous thing a politician could do is announce to a constituent that they have a contrary position on an issue. The *elite persuasion* perspective would offer the same predictions if politicians did not offer persuasive arguments justifying their positions; but, if politicians can argue that their positions are consistent with citizens' values and predispositions or frame them as such, they might successfully shape public opinion and avoid paying electoral costs for taking positions citizens once opposed. Finally, the

position adoption perspective predicts that, as citizens often defer to legislators' policy judgments, citizens will not react negatively when political leaders take positions they oppose and may even adopt their positions, regardless of whether politicians justify them.

No one thinks any of these perspectives describes how all voters think about all issues in all circumstances. Nevertheless, the possibility that 'position adoption' can describe public opinion on salient issues is itself controversial (see Bullock 2011 for review). However, although a growing body of research is consistent with this notion, it contains potentially important ambiguities.¹

One common empirical approach examines how citizens respond to elite position-taking and rhetoric in the real political world, but does not alter aspects of the elite communication to which citizens are exposed (e.g., Abramowitz 1978; Lenz 2009, 2012; Zaller 1992).² While these studies find that citizens often adopt the views of their favored politicians, they leave the nature of this opinion change unclear because elite communication tends to contain a mix of source cues and persuasive messages. To appreciate this ambiguity, consider Abramowitz (1978)'s pathbreaking study of the Carter-Ford debates. Abramowitz (1978) found that voters who viewed the debates tended to adopt their favored candidates' position on unemployment insurance and did not alter their evaluation of the candidates based on their prior view on the issue. When Democrats adopted Carter's position on unemployment insurance after the debate, were they swayed by Carter's arguments, which presumably were justified on the basis of the liberal values Democrats tend to share? Or, did Democrats change their opinions simply because they learned

¹ We do not review the limitations of issue voting research at length due to space constraints. However, we have no doubt that citizens judge politicians based on their issue positions at least some of the time; our concern here, in part, is whether it is also common that they do not.

² See also Achen and Bartels (2006); Arceneaux (2006); Bartels (2006); Gabel and Scheve (2007); Jacoby (1988); Layman and Carsey (2002); Matsubayashi (2013); Mondak (1993); Minozzi et al. (2014).

of Carter's positions and deferred to his judgment?³ These processes imply quite different implications but are observationally equivalent in panel data often read as supporting the position adoption view (e.g., Lenz 2009, 2012). Because political discourse tends to contain a mix of elite source cues and persuasive messages, there is little direct evidence about how elites lead public opinion when they do.

Helping address this weakness, studies taking a second empirical approach have traded naturalism for greater control over the contents of elite communication. In particular, experiments in lab and survey settings have attempted to assess the processes that condition opinion leadership and electoral accountability to public opinion (e.g., Bullock 2011; Cohen 2003).⁴ However, while theoretically innovative, questions about external validity remain (e.g., Findley et al. 2013). For example, citizens aware they are being studied may feel compelled to misrepresent their true policy opinions and evaluations of politicians (Bullock et al. 2013). Alternatively, psychological theories of attitude change argue that "if people are not thinking very much about the persuasive message...their attitudes should be affected little, if any, by...argument quality," (Petty et al. 1995) and individuals may think about political messages more effortfully when they know they are being studied.

We contribute to this body of evidence with unique field experiments that combine the naturalism of field studies and the control over content previously only available in lab and survey settings. These experiments, described in the next section, were designed to test the predictions from Table 1: how citizens react when politicians stake out policy positions, including those these citizens oppose, and the extent to which extensive justifications condition

³ For similar reasons, that campaign advertisements which contain issue content can persuade (e.g., Rogers and Nickerson 2013) does not indicate whether the issue content they include is responsible for their effects (e.g., Martin 2014).

⁴ See also Arceneaux (2008); Cobb and Kuklinski (1997); Chong and Druckman (2007a, 2007b); Iyenger and Valentino (2000).

any impacts of such position-taking.

Political Context and Design Overview

We conducted our experiments in collaboration with eight Democratic state legislators from a Midwestern state.⁵ We conducted the first experiment in the summer of 2013 in collaboration with one legislator and the second experiment in the spring of 2014 with seven additional legislators.

Table 2. Descriptive Statistics on Legislators and their Districts

Legislator	Terms of Service	2012 Obama Vote Share	District Description
<u>Study 1</u>			
A	3	55%	Rural
<u>Study 2</u>			
B	1	90%	Urban
C	1	65%	Urban/Suburban
D	4	75%	Urban/Suburban
E	2	80%	College Town
F	1	50%	Rural
G	1	65%	Urban/Suburban
H	2	50%	Rural

The eight collaborating legislators all came from the same state, but represented diverse districts (see Table 2). Some of the legislators represented swing districts where Obama won the 2012 election by only a few percentage points, while others represented Democratic strongholds. The legislators were also split between urban, suburban, and rural areas.

Both experiments followed the same basic protocol, although there were important differences in the manipulations we describe later:

1. *Identification of Issues.* We worked with each legislator to identify 5-10 salient policy disputes in the legislature that year on which each legislator was actively supporting or opposing a side. We then conducted small statewide pilot surveys using Google

⁵ We received permission from the Human Subjects Committee at Yale to carry out these studies prior to carrying them out (IRB protocol #1303011700).

Consumer Surveys to identify which four of the legislator's positions were likely to have the *least* public support in their district.

2. *Baseline Surveys.* We conducted baseline surveys of registered voters in each legislator's district, querying their positions on their legislator's four issues and their approval of their legislator. TargetSmart Communications provided the voter lists and Winning Connections conducted the surveys.⁶ We started each survey with a question about their attitude towards President Obama. We started with this question to capture their attention for the survey. Supplementary Appendix D presents question wordings.
3. *Exclusions.* When constituents already agreed with a legislator on all four issues, we dropped them from the study. Less than five percent of the subjects fell in this category.
4. *Randomized Treatment Assignment.* The remaining >95% of survey takers were assigned to treatment conditions along two dimensions:
 - a. *Whether Legislators Took An Issue Position Or Not; And, In Study 2, How It Was Justified.* All subjects were first randomly assigned to whether they received a position-taking letter or not and, in the second study, in what manner this position was justified. These treatments differ by study and are described in greater detail later on. The key feature of both studies is that legislators sent some, randomly-chosen constituents letters that contained the legislators' issue positions while others did not receive letters with the legislator's issue positions.
 - b. *On Which Issue The Legislator Took A Position.* For all subjects who were randomly assigned to receive a letter with an issue position, we randomly assigned on which issue the legislator took a position. Citizens were only eligible

⁶ Winning Connections surveyed only one person per household. In households with multiple voters, we randomly chose one person who the interviewers asked for on their initial call. If the person was unavailable, we instructed interviewers to conduct the survey with another person on the voter list if they were available.

to be randomly assigned to receive issue positions with which they did not previously share the legislator’s position, either because they had no opinion or because they disagreed. (We expected ceiling effects for the issues on which citizens already shared the legislator’s position.)

5. *Treatment Delivery.* Legislators sent the assigned letters from their legislative offices, on their official letterhead, and in envelopes clearly marked as coming from the state capitol.
6. *Follow-up Surveys.* The week after the letters arrived, we conducted a follow-up survey to appraise the effect of the letters on constituents’ issue positions and evaluations of the legislator. In Study 1, among those who completed the first survey 33% completed the reinterview; for Study 2, 41% did. Tables A2 and A3 in Supplementary Materials show that the treatment groups are balanced on pre-treatment attitudes among survey takers, allaying concerns that differential attrition biases the estimates.⁷

This design presents several advantages. Because we randomly altered aspects of the communication voters received, we are able to test the theoretical predictions described in Table 1. However, our field experimental design allows us to examine how citizens react to elites’ actual position-taking in the real world and when they are not aware they are being studied (e.g., Findley et al. 2013; Loewen et al. 2010). Moreover, the treatments provided a strong and credible signal about legislators’ positions, as they came directly from citizens’ legislators.

Study 1 – One Legislator: Letter With Position versus No Letter Control

In this section we briefly discuss the first study we conducted, with Legislator A.

⁷ Supplementary analysis (see Table A4 in the Supplementary Materials) also shows that we were somewhat more likely to successfully re-interview older voters and, in Study 2, those who initially held opinions on more issues. In this sense, as in nearly all experiments, our results represent a local average treatment effect for a slightly unrepresentative population, in our case one that is older and more politically engaged.

Constituents in this study received either (1) a letter from Legislator A in which this legislator A took a position on an issue related to policies he was working on in the legislature and that he believed were important to his district, or (2) no letter at all (a control group).

We first surveyed 1,210 voters in Legislator A's district to determine who would be included in the experiment's sampling frame. Of these 1,210 voters, 64 (5%) already agreed with the legislator on all four issues and were removed from the sampling frame. The remaining 1,146 voters were randomly assigned with equal probability to either receive a letter or to not receive any letter.⁸ Legislator A then sent letters that contained his policy positions to the constituents in the treatment group (see Supplementary Appendix B). Finally, we successfully re-interviewed 395 voters a week after the letters were sent.

A manipulation check shows that constituents noticed the letters. We asked voters on the post survey, "*Do you happen to recall if you've received anything in the mail from Representative [Legislator A] this year?*" Over 50 percent of the voters assigned to the letter condition answered in the affirmative versus only 20 percent in the no letter control ($p < 0.001$).⁹

We next estimate the effect of the letter on voters' issue positions using OLS. For the dependent variable in column 1, we recoded the voters' responses to the issue position questions on the follow up survey as a 0-1 scale taking the following values: 1 = the constituents' position is the same as the legislator's, 0.5 = the constituent is undecided; 0 = the constituents' position is opposite the legislator's. Table 3 shows that Legislator A's letters moved his constituents' policy opinions to be more line with his positions. Voters who were assigned to receive a letter were

⁸ If constituents did not agree on more than two issues, we also randomized which two of the issues the letters discussed. This necessitates two steps to assure unbiased inference (Gerber and Green 2012). First, because voters were eligible for different numbers of issue positions on the basis of how many issues on which they did not agree with the legislator, we condition our analyses on strata indicators for the number of issues on which each voter did not agree. Second, when we analyze data at the issue-respondent level to estimate the effect of including an issue in a letter on opinion, we cluster standard errors at the respondent level.

⁹ Table A1 in Supplementary Appendix A shows these results.

moved 0.063 scale points towards agreeing with the legislator on the issue their letter contained.

The next two columns present OLS results where we recode the dependent variable into two dummy variables for whether the voter either agreed or disagreed with the legislator. The issue letters caused voters to be about 6.5 percentage points more likely to agree with the legislator (and 6 percentage points less likely to disagree with him) relative to those who did not receive the letter.

Table 3. Study 1 - Effect of Letter On Issue Opinion, OLS

Independent Variables	(1) Agree Scale (0-1)	(2) Binary: Agree vs. No Opinion or Disagree	(3) Binary: Disagree vs. No Opinion or Agree
Sent Policy Letter on this Issue	0.063* (0.025)	0.065* (0.030)	-0.060 (0.031)
Lagged Opinion: Disagreed with Legislator (vs. No Opinion)	-0.359* (0.130)	-0.325* (0.142)	0.393* (0.181)
Dummy Variables for Strata?	Yes	Yes	Yes
Observations (Issue-Respondents)	865	865	865
Clusters (Respondents)	386	386	386
R-squared	0.167	0.074	0.220

Note: Models estimated using linear regression models. Cluster standard errors in parentheses * p<0.05, two-tailed.

To analyze the effect of the letters on citizens' attitudes towards the legislator, we restrict the sample to those who *disagreed* with the legislator's positions on the issues in the letters they received and compare them to people who could have received such letters but received no letter at all. This means we exclude constituents who received letters with positions they merely had no position on and constituents in the control group who only could have received such letters. We restrict the sample in this way because the relevant theoretical predictions relate to opinions held by voters who at baseline had disagreed with the issue position the legislator took (or, in the control group, would have taken) in a letter.

In Column 1 of Table 4 we use OLS to estimate the impact of the policy letter on voter's opinion of the legislator as measured on a 5-point scale (that we rescale to 0-1) where increasing values indicate more positive evaluations. The results suggest that Legislator A did not face significant backlash for sending letters to his constituents taking positions they opposed – indeed, although not significant, the voters in the treatment group had, on average, a more *positive* evaluation of Legislator A. The remaining columns estimate the effect on binary indicators for whether voters viewed the legislator either more positively or negatively. These regressions indicate constituents were significantly more likely to approve of the legislator after getting a letter containing a position they disagreed with, and offer only weak evidence that some constituents reacted negatively.

Table 4. Study 1 - Effect of Letter on Approval of Legislator, Constituents Who Disagreed On Issues Only

Independent Variables	(1) Approval Scale (0-1)	(2) Binary Approve	(3) Binary Disapprove
Sent Policy Letter	0.037 (0.032)	0.135* (0.058)	0.070 (0.044)
Prior Approval of Legislator	0.160* (0.014)	0.220* (0.025)	-0.139* (0.019)
Constant	0.549* (0.023)	0.251* (0.041)	0.133* (0.031)
Observations	193	193	193
R-squared	0.421	0.311	0.224

Note: Models estimated using linear regression models. Standard errors in parentheses. * p<0.05. Sample only includes those *who opposed the legislator's position* on both issues in letter.

Study 1 demonstrates that legislators' communications can affect constituents' opinion on issues and hints at minimal downsides of staking out policy positions constituents claim to oppose. Study 2 builds on the evidence of Study 1 to answer two important questions.

First, why did Legislator A's communications cause constituents to adopt his positions?

Legislator A's letter offered (short) arguments for his positions that could have persuaded his constituents that his position was consistent with their predispositions and values (consistent with *elite persuasion*). Alternatively, voters may simply be adopting his stances (consistent with *position adoption*). Study 2 therefore manipulated the extent to which legislators attempted to persuade so as to better distinguish between these explanations of opinion leadership.

Second, did Legislator A really not lose support with constituents for taking a position they disagreed with? Receiving a letter could have a positive effect on the legislator's favorability, which could be offset by a theoretically important negative effect of the positions it contained. Study 2 therefore included a control letter without any positions in order to hold constant the presence of a letter and vary only the presence of counter-attitudinal issue positions.

Study 2 – Varying the Presence of Positions and Extensive Arguments With Seven Legislators

Our second study followed the same basic protocol as Study 1 but introduced a few important design changes. First, we recruited seven additional legislators to participate (see Table 1 for information about these legislators and their districts). With the larger number of legislators, we were able to recruit a larger sample of constituents and increase the precision of the estimates. Assessing the average effect of seven legislators and seventeen policy issues also grants the experiment a stronger claim to generalizability.

More importantly, we altered both the control and treatment conditions. As before, legislators in the second experiment sent letters to all their constituents who did not already agree with their position on at least one issue (excluding the small number of voters who agreed with them on all issues already). However, constituents were randomly assigned to receive one of

three letters:¹⁰

- (i) a “control” letter where the legislator introduced themselves, described the services their office could conduct for constituents, and described a few locally oriented achievements (e.g., designating a building downtown a historic place), serving as a baseline,
- (ii) a “basic justification” policy letter that added language in which the legislators took a position the recipient had not agreed with previously and made only the briefest justification for their position, and
- (iii) an “extensive justification” policy letter that added more extensive arguments for this position designed to appeal to citizens’ values.

Comparing the “basic justification” and “extensive justification” treatments allows us to test how providing extensive arguments affect a legislator’s ability to persuade constituents. In both cases, the legislator stated their position on the issue with one line, always the same. In the “basic justification” policy letter, they then provided only restatements of their positions and brief, general assertions that their position was desirable such as “This would have a positive impact on the lives of many [STATE] residents and their families.” For example, one of the legislators wrote the following about a proposed state pension plan in the “basic justification” condition:

I am supporting the creation of a state-administered retirement plan open to all [STATE] workers. I believe that [STATE] workers should have access to a system that supports them during retirement. This new system would have a positive impact on the lives of many [STATE] residents and their families.

By contrast, legislators provided more detail about the reasons behind their positions in the “extensive justification” condition, attempting to appeal to citizens’ values and

¹⁰ A pure control group would have been desirable, however the finite size of the state legislative districts we studied limited our sample size.

predispositions. For example, the same state legislator instead included the following paragraphs in the “extensive justification” condition for the pension plan:

I am supporting the creation of a state-administered retirement plan open to all [STATE] workers.

[STATE] boasts one of the best pension systems in the nation for government workers. For decades, [STATE] state employees have relied on the [STATE RETIREMENT SYSTEM] pension for their livelihood after retiring. The fund is solvent and often modeled by other states with failing systems. Because this fund for public workers has been so successful, I support the creation of a similar state-administered retirement plan for private workers.

I also support the creation of a [STATE] system because everyone who works hard should be able to look forward to a stable retirement, regardless of if they are a public or private worker. Right now, less than one-half of Americans have any retirement assets at all, and financial insecurity in retirement is all too common for our seniors. Establishing a [STATE SYSTEM] for private sector workers would make sure that everyone who works hard can look forward to a stable retirement.

To the extent elite opinion leadership is generated as elites justify their positions persuasively and highlight how they are consistent with citizens’ values, we should expect the “extensive” condition to tend to produce stronger issue opinion change, and, potentially, for the “basic justification” condition to generate backlash. However, to the extent citizens are willing to adopt positions merely because an elite has taken it, we should expect the content of the “basic justification” condition to have a similar effect on citizens’ opinions as the “argument” condition.¹¹

As with the first experiment, the letters all shared the same basic structure: a paragraph of introductory text providing biographical information about the legislator, issue positions in the “basic justification” and “extensive justification” conditions, and a closing paragraph.

¹¹ An alternative possibility is that citizens will not care to read the arguments although would be more strongly influenced if they did. We see this possibility as largely consistent with the position adoption view, insofar as it envisions citizens not caring to consider legislators’ arguments before changing their view. As the experiment takes place in a field setting, this would be consistent with the notion that such justifications are not *necessary* for issue opinion change in the real world (even if they might have effects if they were encountered) even if they can enhance it in survey or lab settings.

Table 5 lists the issues used in the letters and the percent of issue-respondent observations in our analysis for each. With seven different legislators choosing four different issues, the policy letters covered a broad range of topics. These issues included raising the minimum wage, allowing undocumented immigrants to obtain drivers' licenses, putting pregnant women suspected of using drugs in police custody, school vouchers, taxes, redistricting commissions, economic development in their region, and much more. In some cases, more than one legislator wrote about the same issue. In all such cases, the legislators all took the same position on the issue, though they sometimes used different arguments for that position. Anonymized versions of these letters are provided in Supplementary Appendix C.

Table 5. Issues from Study 2

Issue	Percent of sample
Budget deficit	5.0%
Pregnant women suspected of drug use can be put in jail	2.0%
Criminals have records expunged from minor crimes	3.5%
Gas tax increase	3.5%
Driver's licenses for undocumented immigrants	6.0%
Marijuana decriminalization	3.5%
Medical marijuana legalization	6.6%
Minimum wage increase to \$10.10	19.5%
State-sponsored pension plan	9.1%
Workplace protections for pregnant women	3.9%
Local property tax increase	2.0%
New state commuter rail system	4.0%
Non-partisan redistricting	8.3%
Trying seventeen-year-olds as juveniles, not adults	3.4%
Undocumented immigrants eligible for in-state tuition at state colleges and universities	3.5%

Voter identification requirements	4.0%
Private school vouchers	12.2%

In Study 2, some individuals in the treatment group were eligible to receive the legislators' positions on multiple issues but the letter contained the legislator's position on only one randomly determined issue. As in Study 1, we use the issue-respondent as the unit of observation when estimating the effects of the letters on issue opinion and base our analysis on a sample of 2,528 voter-issue observations from 1,047 individual voters. For the analysis, we cluster our standard errors at the respondent level and include dummies for the number of issues the respondent was eligible for receiving a position on to account for the fact that the probability of receiving each issue treatment depended on the number of issues where the voter was eligible for treatment.

Manipulation Checks: Did constituents remember the letters and learn legislators' positions?

First, we checked that respondents received and read the letters. At the end of the follow up survey, we asked a random subset of voters (each voter had a one-fourth probability of being asked) whether they remembered receiving a letter from the legislator in the past year. Over 60 percent of subjects responded in the affirmative. Although we have no pure control group to measure what share of voters would have falsely remembered receiving a letter from the legislator recently, we are encouraged that we observed a similar share of voters recalling receiving a letter in Study 2 as in the treatment group for Study 1.¹² Table A1 gives the breakdown by treatment group and shows that there were no significant differences across the treatments.

¹² We were also concerned that at least one of the legislative offices might have had difficulty getting their letters mailed and delivered before surveying began, but the share of constituents who recalled receiving a letter did not meaningfully differ across the districts.

Further, the voters who were sent a letter with a position were also far more likely to correctly identify their legislators' position on the issue the letter contained than voters sent the control letters or a letter on a different issue. In the post-survey we asked a random subset of voters (each voter had a three-fourths probability of being asked) to indicate their legislators' position on one of the issues they were eligible for treatment.¹³ Table 6 shows the results from a linear regression that tests whether respondents were more likely to correctly identify their legislator's position on a given issue if they were sent a policy letter on that issue. The outcome was coded as 1 if the recipient correctly identified the legislators' position and 0 if the recipient did not correctly identify the legislators' position. Subjects were significantly more likely to correctly recall their legislators' position on the issue their letter was randomly assigned to contain.

Table 6. Study 2 Manipulation Check – Did Constituents Learn Legislators' Positions?

DV=Correctly Identify Legislator's Position	(1)
Basic Justification	0.108* (0.035)
Extensive Justification	0.137* (0.038)
Dummy Variables for Strata?	Yes
Observations (Respondents)	1,077
R-squared	0.062

Note: Dependent variable is correct knowledge of legislators' positions. Each individual asked about only one of their legislator's issue positions. Model is estimated via an OLS regression model. Standard errors in parentheses. * $p < 0.05$, two-tailed.

Results: Opinion Leadership

We first examine whether the positions legislators took in the letters influenced voters' issue opinions. In the post-survey we asked constituents where they stood on the issues where they did not already agree with the legislator previously. As in Study 1, we recoded voters'

¹³ We only asked about one issue for each voter because our survey budget was constrained.

responses to these questions to create an ordinal variable on a three-point scale: the voter disagrees with the legislator's position, the voter is unsure on the issue, or the voter agrees with the legislator's positions, coded on a 0-1 scale as 0, 0.5, and 1 respectively.

Table 7 presents an OLS regression estimating the effect on respondent's issue opinions of the two different types of issue letters, relative to the baseline of receiving the control letter (i.e., the letter without any issue content). If successfully appealing to citizens' values is necessary for legislators to affect public opinion, we should see that the *extensive justifications* treatment has a larger effect on voters' opinions. However, if citizens were reacting to the mere fact that their legislator had taken a position, the *basic justification* treatment should affect opinion just as much as the *extensive justifications* letter.

We find little evidence that legislators' arguments were responsible for their persuasive impact. The results in Table 7 show that the *basic justification treatment* moved individuals about 0.04 scale points towards agreement and away from disagreement, similar to the same effect in Study 1. The coefficient on *extensive justification* treatment variable is also positive, but it is *smaller* in magnitude and not statistically significant; this suggests that including attempts at persuasive argument and appeals to citizens' values is unlikely to increase the effect of the letters.¹⁴ Legislators were able to move constituents' opinions by stating their own positions with minimal justification; but, there is no evidence that extensive justifications made these positions more persuasive. These results are most consistent with the *position adoption* view.¹⁵

¹⁴ Even as the coefficient for the extensive justification letters is smaller, we would not make too much of this difference. The difference between the two coefficients is not significant, as the *position adoption* theory would predict. Under *elite persuasion* theory, we would expect this coefficient to be larger than the coefficient on the basic justification letter, and for it to be significantly so.

¹⁵ We also tested for whether the letters that provided arguments polarized respondents. Perhaps some voters were pushed towards the legislator, while others were pushed away from the legislator. If that were true, then we might see that the variance was greater when the voters received one of the argument letters than when they did not. However, the variance of the outcome was very similar for the three groups:

Std. Dev. (No Arguments) = 0.81

Table 7. Study 2 - Effect of Letter On Issue Opinion

Independent Variables	(1) Agree Scale (0-1)	(2) Agree Binary	(3) Disagree Binary
Basic Justification	0.039* (0.015)	0.045 (0.026)	-0.071* (0.027)
Extensive Justification	0.022 (0.016)	0.032 (0.026)	-0.035 (0.028)
Lagged Opinion: Disagreed with Legislator (vs. No Opinion)	-0.141* (0.016)	-0.087* (0.026)	0.336* (0.030)
Dummy Variables for Strata?	Yes	Yes	Yes
Observations	2,528	2,528	2,528
Clusters (Respondents)	1,140	1,140	1,140
R-squared	0.101	0.029	0.158

Note: Models estimated using linear regression models. Cluster standard errors in parentheses * p<0.05, two-tailed.

Results: Approval of Legislator (Constituents Who Disagree on Issues Only)

Arguably the most controversial claim of the *position adoption* perspective is that citizens sometimes do not hold their representatives in less esteem when they learn that their representatives are pursuing a policy they oppose. While Study 1 presented evidence consistent with the view that citizens were actually indifferent to this information, Study 1 left open the possibility that a negative effect of this information was “cancelled out” by the positive effect of receiving a letter at all. Therefore, in Study 2, we compare a control group of citizens who disagreed with their legislator on an issue but received a “control letter” with no issue content to the treatment group citizens who disagreed with their legislator on an issue and received a letter in which the legislator stated a position with which the voter disagreed.

We find no evidence that legislators suffered electoral costs by taking positions

Std. Dev. (Basic Arguments) = 0.84

Std. Dev. (Extensive Arguments) = 0.84

The variance is slightly higher for the arguments treatment, but it is very far from being statistically significant. The p-value of the variance ratio test of whether the variance for the *no arguments* group is the same as the relative to the voters in the *arguments treatments* is 0.24. We acknowledge an anonymous reviewer for suggesting this test.

constituents disagreed with; citizens who received letters from their legislators taking positions they had disagreed with previously evaluated their legislators no less favorably than those who received a “control letter” with no position (but disagreed with their legislator on at least one issue, so could have received such a letter). Column 1 of Table 8 presents the results of the policy letters on voters’ approval of their legislators, coded on a 7-point scale we have rescaled to 0-1.¹⁶ There is little evidence that constituents reacted negatively; indeed, the point estimates are positive. Given the 95% confidence interval, we can rule out negative effects of sending letters with contrary positions larger than around 0.03 scale points.

Table 8. Study 2 - Effect of Letter on Approval of Legislator, Constituents Who Disagreed on Issues Only

Independent Variables	(1) Approval Scale (0-1)	(2) Binary Approve	(3) Binary Disapprove
Basic Justification	0.014 (0.023)	0.011 (0.038)	-0.029 (0.034)
Extensive Justification	0.016 (0.024)	0.027 (0.040)	-0.005 (0.035)
Prior Approval of Legislator	0.239* (0.016)	0.308* (0.026)	-0.218* (0.022)
Dummy Variables for Strata?	Yes	Yes	Yes
Observations	743	743	743
R-squared	0.338	0.231	0.199

Note: Models estimated using linear regression models. Standard errors in parentheses. * p<0.05. Sample only includes those who opposed the legislator’s position.

In contrast to the clear impact of these letters on constituents’ issue opinions, there appear to be no effects on their attitudes towards legislators. However, as with all null effect estimates,

¹⁶ We used a scale instead of a binary variable in order to increase statistical power; this is especially important given that the results suggest a null effect. The legislator favorability question was branching, similar to the standard party identification question on the ANES. Subjects coded at 0 reported an unfavorable impression of the legislator, then reported it was strong; subjects at 0.166 recorded an unfavorable impression of the legislator, then reported it was weak; subjects at 0.333 recorded being indifferent, then reported leaning towards an unfavorable impression; subjects at 0.5 reported being unfamiliar with the legislator, or, reported being indifferent, and that they did not lean either way; subjects at 0.666 reported being indifferent toward the legislator, then reported leaning towards a favorable impression; subjects at 0.833 recorded a favorable impression of the legislator, then reported it was weak; subjects at 1 recorded a favorable impression of the legislator, then reported it was strong.

it remains possible that there was some negative effect of these letters too small to detect, especially because not every constituent read the letter (see Table 6). Regardless of the mechanism for this finding, the results are most in line with the *position adoption* perspective.

Are some issue different? We have no doubt the dynamics of accountability operate differently for different issues, but we see little evidence of heterogeneity at the issue level in this data. Figures A1 and A2 in the Supplementary Appendix A examine this possibility by depicting the effects separately for each issue. Most issues have few observations associated with them so the effects are imprecisely estimated. Significantly, the results show that the pattern is not driven by any one issue. Although there is variation in the point estimates across issues, a Q-test could not reject that this variation is due to sampling variability.

Future work should also consider whether the treatment effects are moderated by voter attributes, such as party identification or strength of opinion. We consider the role of party briefly here. The best design for answering this question would experimentally manipulate the presence of party cues, a technique common in survey experiments. The legislators we cooperated with were ultimately responsible for the letter's content here, however, and these legislators preferred not to send a message that contained a party cue. As a result, we did not ask voters about their party identification. However, we did ask all respondents about their attitude towards Obama in the pre-survey. Using Obama approval as a proxy for party, we consider how Obama-approvers and Obama-disapprovers responded to the treatments in an exploratory analysis in Tables A9-A12. In brief, no clear pattern of results emerges from these analyses. Given the limitations of not having better measures of party identification, these results should be viewed as very preliminary. Future studies should clearly continue exploring the role of party identification, ideally beginning at the design stage.

Discussion

Students of democratic politics have long debated the role mass opinion plays in constraining elected officials' policy decisions. The consistent pattern that citizens tend to support the positions their elected officials take has inspired three distinct theoretical perspectives: an *issue voting* perspective that conceptualizes citizens' policy preferences as firm commitments that chiefly determine how they evaluate politicians and envisions legislators strongly responding to these preferences; an *elite persuasion* perspective that agrees issue opinions strongly determine voters' choices but that elites are capable of persuading citizens to change their issue opinions; and, a *position adoption* perspective that citizens defer to elite's policy judgments rather than judging them on the basis of their issue positions.

No one believes any of these perspectives explains the politics of all issues, under all circumstances, and for all voters. But the idea that *position adoption* describes the politics of salient issues even some of the time has been especially controversial. When political elites simply announce their positions without making persuasive appeals to citizens' values, can they shape public opinion and avoid electoral costs? In this paper we probed these questions with a series of unique field experiments conducted in cooperation with politicians themselves. In our studies, political elites randomly assigned aspects of their communications to voters about their policy positions, among the first such studies we are aware (however, see Wantchekon 2003).¹⁷ The policies in question were neither insignificant nor uncontroversial, and included decriminalizing marijuana, raising the minimum wage, and policies toward undocumented immigrants.

¹⁷ See Cover and Brumberg (1982), Loewen and Rubenson (2010), and Minozzi et al. (2014) for studies that do not manipulate the content of elite messages.

Our investigation first uncovered strong evidence that legislators can shape constituents' views on issues by merely staking out their positions. The constituents who received letters containing legislators' positions were significantly more likely to subsequently share their legislators' view. In our second study, we also found that constituents who received lengthy arguments from legislators justifying their positions were no more likely to change their opinions as constituents to whom legislators provided little justification.

We next examined whether citizens evaluated their legislators more negatively when their legislators directly stated a position citizens had opposed, examining a core mechanism central to theories of democratic accountability. Many traditional perspectives on democratic politics would suggest that just about the least politically advantageous thing a politician could do is inform their constituents they disagree with them on an issue – or, if they must do so, they are typically expected to have to provide persuasive reasons justifying their positions to avoid constituents' ire. Our experiment put these notions to a stark test, as the politicians we cooperated with randomly varied whether they did just this. Significantly, we found no evidence that constituents held their legislators in less esteem when legislators announced support for policies they had previously opposed – regardless of the extent to which legislators provided justifications for their positions. There was little evidence that these patterns meaningfully differed across issues.

We did not expect the context that produced these findings – a single letter from relatively unknown state legislators – to be especially friendly to the position adoption hypothesis. Most constituents likely knew little about their state legislator before they received the letter (indeed only 20 percent of the control group in Study 2 had heard of their legislator), making it surprising that information that their legislator disagreed with them had no detectable

impact on their evaluations of their legislator.

There is still a great deal our evidence does not tell us. We know little about the issue-, elite- or citizen-level factors that might condition these effects. Future studies should also examine which constituents are most likely to be affected. We also expect that these patterns may certainly differ across issues, such as moral issues that clearly implicate citizens' core values (e.g., Carmines and Stimson 1980; Ryan 2014; Tesler 2014) or that citizens find particularly personally important (e.g., Krosnick 1990). Likewise, competing communication may well change these effects (Chong and Druckman 2007b). Future research should explore these questions.

Our evidence also says little about *why* citizens adopt politicians' positions. Dating back at least to Burke (1774), democratic theorists have stressed that citizens may choose to delegate to elected officials because they recognize legislators may possess superior information, expertise, and judgment (see also Bianco 1994). Therefore, one conjecture is that voters defer to politicians because they trust elected officials or authority figures more generally. However, such opinion leadership is not necessarily "blind" (e.g., Lenz 2012): citizens may have simply inferred that their elected officials are more familiar with the details of the issues at stake and trusted their judgment. On the other hand, these elites may have earned this trust through prior activity or demonstrated similarity (Fenno 1978; Bianco 1994). For example, in our letters, all of the legislators included a biographical paragraph about their connection to the community (see Appendix B for the text of the letters). We look forward to seeing future studies build on the experimental approaches we have described to better understand the mechanisms at play.

With these important caveats aside, our results do underscore the continuing relevance of a controversial notion deeply engrained in political thought: under many circumstances,

democratic citizens follow their leaders. This does *not* imply citizens fail to hold their elected officials accountable altogether – for example, the citizens who behaved consistently with *position adoption* in our study may be judging these politicians based on their *results*, rather than their policy methods (Fiorina 1981). Our results also leave open the possibility that politicians think the constraints public opinion places on them are stronger than they are (e.g., Berinsky and Lenz 2014). Indeed, the legislators expressed surprise at their success in affecting constituent opinion; as the citizens who choose to interact with legislators tend to be more politically engaged, legislators may form inaccurate understandings of the degree to which the average constituent has firm positions on issues (Miller and Stokes 1963). Such questions will also be fruitful avenues for further research.

Reviewing a popular theoretical conception, Miller (1992) defines democracy as “the aggregation of [citizens’] independently formed preferences” (p. 55) into issue positions taken by politicians. Democracy may well function in this manner sometimes. However, the evidence presented here provides a rare window into how democracy can also serve precisely the opposite function: distributing politicians’ policy judgments to citizens.

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