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A God of Vengeance and of Reward? Voters and Accountability

Theories of democratic politics prize congruence between citizens' preferences and their elected representatives' actions in office. Elections are a critical means for achieving such policy congruence, providing voters the opportunity to chasten representatives who are out of step with constituent preferences and to reward the faithful. Do voters act this way? Recent studies based on observational data find they do, but these data are somewhat limited. We employ a survey experiment to estimate the extent to which information about policy congruence affects voters' evaluations of representatives. We informed some subjects how often their member of Congress's voting decisions match their own stated preferences on the same policies. We find that information about congruence enhances accountability by affecting constituent evaluations of representatives and may also affect citizens' propensity to participate in upcoming elections.

Responding to the "extraordinary" objection that the proposed constitution would not induce members of the House of Representatives to possess "sympathy for the mass of the people," James Madison in *Federalist 57* stressed that its members "will be compelled to anticipate the moment when their power is to cease, when their exercise of it is to be reviewed, and when they must descend to the level from which they were raised; there forever to remain unless a faithful discharge of their trust shall have established their title to a renewal of it."¹ Thus, the Constitution's design contemplates a key role for voters to ensure that elected officials will remain "sympathetic" to their needs and concerns. Voters need to reward the sympathetic and exact vengeance on the unsympathetic official come election time, else democracy may suffer.

Do voters act in this fashion? Recent studies largely find that they do, but these studies are based on observational data subject to complicated selection processes by both politicians and voters. We employ a randomized survey experiment to estimate the extent to which information about representational congruence affects citizens' evaluations of their representatives. Our treatment informs a randomly selected subset of our sample how often their MC actually voted in concert with their preference (i.e., their level of policy congruence) on five salient roll-call votes. We compare attitudes toward incumbents and intention to participate among this group to a control group that was not provided this information, accounting for each subject's underlying congruence.

We find that information about policy congruence enhances accountability. Overall, subjects presented with information about the extent of congruence between their stated preferences and their MCs' roll-call votes are much more likely than control-group subjects to reward or sanction their House member. This result holds across party groups. We also find that information about congruence may affect whether people intend to vote, though the evidence on this point is a bit less certain.

Background

To what extent does information enhance citizens' willingness to hold their representatives accountable for their actions in office? Given the fundamental importance of accountability in democratic politics, many scholars have examined dimensions of this question. Given the complex nature of the phenomenon, no research design can account for all facets of the question, and the conclusions many of these studies support are limited.

Scholars of various stripes contend that informed citizens are better for democratic politics (e.g., Dahl 1998; Delli Carpini and Keeter 1996; Junn 1991; Kuklinski et al. 2001; McCloskey and Brill 1983). Yet, most of the U.S. public has a very limited store of politically relevant information (e.g., Delli Carpini and Keeter 1996), including some of the basic information we might associate with accountability, like who their representative is (e.g., Jacobson and Carson 2016, 156) and how they voted on major legislation (e.g., Guisinger 2009). In addition, constituents may project their own views onto their representative's presumed behavior, assuming

their representative acted the way the constituent wanted him or her to (Wilson and Gronke 2000) or constituents may adopt their representatives' issue positions (Lenz 2012). It may be sufficient for a subset of citizens to be informed (Bullock 2011; Lazarsfeld, Berelson, and Gaudet 1944; Zaller 1990) or for citizens to employ various informational shortcuts to learn what candidates are likely to do once elected (Huckfeldt and Sprague 1995; Lau and Redlawsk 2006; Popkin 1991; Rahn 1993).

Whether or not we are convinced that citizens are sufficiently equipped to make reasoned choices, various studies find that incumbents who are ideologically "out of step" with their constituents tend to face punishment at the ballot box (e.g., Abramowitz 1988; Canes-Wrone, Brody, and Cogan 2002; Canes-Wrone, Minozzi, and Reveley 2011; Kassow and Finocchiaro 2011; Nyhan et al. 2012), although representatives appear to have more leeway in the present context than in the past (Bonica and Cox forthcoming). However, aggregate findings do not necessarily imply that individual voters are punishing out-of-step members because other mechanisms may lead to electoral defeat. Representatives perceived to be out of step may be more likely to attract qualified and well-funded challengers (Gordon, Huber, and Landa 2007), they may be less skilled politicians (Stone and Simas 2010; Zaller 1998), and/or they may be targeted for removal by interest groups (Kollman 1998), all of which might endanger an incumbent even when voters are themselves unaware of, or care little about, the legislator's level of policy congruence.

Recently, data and methodological advances have enabled individual-level analyses, many of which find that voters appear to punish and reward representatives based on ideological and/or policy considerations (e.g., Ansolabehere and Jones 2010; Hirano et al. 2015; Jessee 2009; Jones 2012; Sances 2017; Shor and Rogowski 2018; Simas 2013), although the effects of policy and ideological proximity to candidates can be swamped by partisanship (Highton 2018; Tausanovitch and Warshaw 2018). Individual-level studies of information and accountability face strong research design challenges. Levendusky argues that observational studies' estimates of information's impact may exaggerate the actual effect by two to four times due to omitted variables.² He concludes that for researchers, "to generate causal estimates [of information's effects], one would need some sort of randomized/natural experiment" (2011, 54).

Furthermore, the potential for endogeneity looms large in observational studies of accountability (Lenz 2012). Consider a voter who prefers a candidate for nonpolicy reasons (e.g., the candidate's charisma). When the voter learns that the candidate holds a particular policy position, she may adopt that position herself (Lenz 2012). Since the voter and candidate now have policy congruence on this issue, it may look like the new information about policy congruence caused the voter to be more favorable toward the candidate. However, in this case, policy congruence did not influence the voter's view of the candidate, but instead her non-policy-based affect toward the candidate influenced her policy position. Scholars have used instrumental variables (Ansolabehere and Jones 2010) and panel data (Hirano et al. 2015; Sances 2017) to deal with this challenge, concluding that voters really do appear to hold their elected representatives accountable. These studies tell us much but require assumptions that are unverifiable and may not hold.³ We consider the topic of information and accountability so fundamental to democratic politics that we should examine it via several different methods to ensure our understanding of how citizens reward and punish officeholders is robust.

We tackle these methodological challenges experimentally. We advance our understanding of voter behavior by examining the effect of information about the level of congruence between an individual's policy preferences and the roll-call votes of his or her member of Congress on voter behavior. Our treatment informed a randomly selected subset of our sample how often their MC voted in concert with their preference (i.e., their level of policy congruence) on a number of salient roll-call votes. We did not tell subjects which votes were congruent with their views. Because we tell subjects no new information about how their representative voted on specific votes, voters presumably cannot change their policy views to match their representative's views (except in the extreme cases where congruence is perfect or totally absent: 10% of our sample). Consequently, we can assess the impact of information about policy congruence largely absent the possibility that voters changed their issue position to match their preferred candidate's position.

Finally, at present, very little is known about when citizens' reactions to perceived (in)congruence with officials will motivate citizens to take political action (Arceneaux 2003; Flavin and Griffin 2009). Policy congruence can increase political

involvement, as constituents with particularly high or low levels of policy congruence with final policy outcomes are more likely to increase their political participation in later elections (Flavin and Griffin 2009). Unfortunately, these studies cannot determine the direction of causality between participation and representation because political participation has been shown to influence policy congruence (Griffin and Newman 2005; see also Bullock 1981; Campbell 2003; Hill and Leighley 1992; Keech 1968; but see Ellis, Ura, and Ashley-Robinson 2006).

In sum, we evaluate the process of accountability in an experimental setting. Doing so provides a research design free of endogeneity and omitted variable bias, allowing us to estimate the impact of policy congruence information on voters' willingness to hold their representatives accountable. In addition, we present evidence suggesting that congruence information affects intended turnout, an essential element in the accountability process often left unexamined.

Data

We employ a randomized survey experiment to evaluate the effect of information about policy congruence on citizens' willingness to hold their MC accountable. To measure the congruence between constituent opinion and legislator behavior, our survey asked respondents their opinions on actual bills voted on by the U.S. House of Representatives, an approach akin to that adopted in several studies (Ansolabehere and Jones 2010; Bafumi and Herron 2010; Highton 2018; Jessee 2009; Tausanovitch and Warshaw 2018; see also Broockman 2016). We identified five roll-call votes that took place in the 111th Congress by applying four criteria. First, the roll call needed to pertain to a politically salient issue so that respondents would have a meaningful preference and care about how their MC voted on the issue. A vote was deemed salient if the issue received extensive coverage from the media over a prolonged period (e.g., Afghanistan war) or dealt with an issue most respondents would have direct experience with (e.g., credit card disclosures). Second, the roll-call vote needed to divide at least one of the parties (at least 60 MCs breaking party ranks). This requirement assures that our treatment conveyed new information for respondents beyond the MC's party affiliation. Using party-line votes would likely produce an abundance of citizens who either agreed with their representative on all five

votes or disagreed with their representative on all five votes. In addition, Hill and Hurley (1999) have shown that legislators are not especially responsive to constituents on issues that are “party defining,” and constituents are likely to already know how MCs vote on these issues so the treatment provides no new information. The third requirement was that nonvoting by members of Congress be very limited (abstention < 20 per vote). Finally, we aimed to balance the bills so that each party was split by at least two bills. This requirement was aimed at producing roughly equivalent distributions of congruence among Democrats and Republicans. Ultimately, we used the roll calls enumerated in Table 1.

The survey was fielded just prior to the November 2, 2010 election (October 24–30), with a total of 2,324 respondents. The mean completion time was 21 minutes. Among those who began the survey, 92% completed it. Each page of the survey included a university logo to boost the survey’s credibility. Subjects were recruited in partnership with Qualtrics,⁴ which randomly drew respondents from existing online consumer survey panels.⁵ Our primary concern in this experiment was internal validity, but it is useful to know the population to which the findings generalize. Table A1 in the online supporting information compares the Qualtrics sample to the national population as reported in the 2010 U.S. Census. In general, the sample differs little in its demographic characteristics from the makeup of the U.S. population. Latinos and people with a high school diploma or less are somewhat underrepresented in the survey, which suggests that the results might not generalize to these populations as well as they do to other groups.⁶

We introduced the issue attitude items as follows: “We’d like to ask you about your positions on a number of current issues. Many people do not know all the details about these issues, so even if you are not sure about your opinion, please provide your best guess.” In doing so, we intentionally did not mention that the issues already had been voted on in Congress in order to limit the likelihood that constituents’ issue self-placements would be affected by their perceptions of either how their own MC voted or the final policy outcome.

TABLE 1
111th Congress Roll-Call Votes that Were Salient, Divide the Parties, and Have Limited Abstention

Roll Call # and Session	Bill #	Title	Vote Breakdown
98, 2nd Session	H.CON.RES.248	Directing the President, pursuant to section 5(C) of the War Powers Resolution, to remove the United States Armed Forces from Afghanistan	Dem. 60 Y, 189 N Rep. 5 Y, 167 N NV 9
143, 2nd Session	H R 1586	On Motion to Suspend the Rules and Pass to impose an additional tax on bonuses received from certain TARP recipients	Dem. 243 Y, 6 N Rep. 85 Y, 87 N NV 10
884, 1st Session	H R 3962	On Agreeing to the Amendment Stupak of Michigan Amendment	Dem. 64 Y, 194 N Rep. 176 Y, 0 N Present 1 NV 0
228, 1st Session	H.R.627	Passage of Credit Card Accountability Responsibility and Disclosure Act of 2009	Dem. 252 Y, 1 N Rep. 105 Y, 69 N NV 7
722, 1st Session	H R 3548	On Motion to Suspend the Rules and Pass, as Amended, the Unemployment Compensation Extension Act of 2009	Dem. 227 Y, 17 N Rep. 104 Y, 66 N NV 18

Note. NV denotes the number of members not participating in the roll-call vote.

Issue attitude items were presented one at a time. The question wording for these items was derived from the bill summaries of the Congressional Research Service:⁷

- Do you think that the government should impose a 90% tax on bonuses received by employees of banks and financial institutions that received bailout money from the government last year?
- Do you think that the new health care bill should prohibit federal funds from being used to pay for any abortion except where the mother's life is in danger or when the pregnancy is the result of an act of rape or incest?
- Do you think that the government should impose restrictions on credit card companies so that they only offer cards to people who can afford them, they do not change a card's interest rate quickly and without notice, and they do not issue cards to people under the age of 21?
- Do you think that Congress should require the President to remove the U.S. Armed Forces from Afghanistan within 30 days?⁸
- Do you think that the federal government should have provided funds to extend unemployment benefits last year in states where the unemployment rate was over 8.5%?⁹

The response options for each of the five items were limited to "Yes" or "No."¹⁰ Notably, responses for four of the five issue items predict respondent ideological self-placement (seven-point scale) at $p < .001$, with the abortion position being the most highly correlated of the four. The one outlier is the credit card regulatory vote, which was not systematically related to ideological self-placement.

We mapped constituent issue attitudes to their actual MC's roll-call votes using respondents' reported ZIP codes and a mapping of ZIP codes to congressional districts for the 110th House made available by the U.S. Census.¹¹ ZIP codes that overlapped multiple congressional districts are assigned to the congressional district where the majority of the residents live.¹²

Using the matched respondent preference and his or her legislator's roll-call records, we generated a congruence score ranging from 0 to 1 for each subject. We calculated congruence as the number of votes on which the subject and her representative agreed, divided by the total number of votes for which the subject expressed a preference and her legislator cast a roll-call vote.¹³ Of the respondents whose attitudes were matched on all five roll calls, 9% agreed with their MC on all five votes, 27% agreed with

their MC on four of five votes, an additional 33% agreed with their MC on three of five votes, 21% agreed on two of five votes, 8% agreed on just one of five votes, and only 1% agreed on none of the five votes.

Our treatment was to inform a random half of the sample of the level congruence between their preferences and their MC's roll-call voting.¹⁴ Subjects were only told their congruence score (e.g., two of five votes), not which votes were congruent and which were incongruent. We took this approach because we wanted to ensure that subjects did not immediately adopt their MC's issue position (Lenz 2012). In addition, our approach provided homogeneity of the treatment within groups that shared the same underlying frequency of congruence. Reporting congruence on each bill separately would have three downsides. First, subjects would have the opportunity to adopt their MC's issue position on the issue (Lenz 2012). Second, it would increase the number of unique cells from six possible levels of congruence (i.e., 0, 1, 2, ..., 5) to 32, which would not allow for sufficient sample size within each cell for reliable estimation. Our approach provided homogeneity of the treatment within groups that shared the same underlying frequency of congruence. Third, there would be no way of knowing the particular piece of information that altered respondent behavior. Given the multidimensional nature of 32 treatment categories, it would be impossible to estimate a single sensible parameter. Reporting a single ratio cleanly captures our concept of interest (i.e., degree of congruence) and applies the same treatment to all subjects sharing a ratio.¹⁵ All respondents were informed of their MC's name and party affiliation.¹⁶ The treatment and control groups were balanced on all observable measures (see Table A2 in the online supporting information). Thus, the two samples differ only in that the treatment group was informed of the level of policy congruence with their member of Congress.

We note two potential design limitations that may actually lead us to underestimate the power of information about congruence. First, some subjects in our control group likely knew how their MCs voted on the bills we examined (Ansolabehere and Jones 2010). Therefore, some in our control group likely had a preconceived notion of their level of agreement, which could attenuate the effect of the treatment. Second, some in our treatment group may not have believed the information we provided, coming as it did from a third-party source rather than a trusted source (e.g., local news or a challenger's campaign). Both factors

should minimize the effect of the treatment. Under these conditions, finding a treatment effect would lend confidence that information really does enhance citizens' doling out of punishments and rewards.

Finally, we obtained three outcome measures: respondent evaluations of their incumbent's job performance, respondents' intended congressional vote (for/against incumbent where standing for re-election), and intent to participate in the 2010 election.¹⁷ Between the treatment and the outcome variables we placed a series of "distractor" questions that pertained to media consumption, attitudes about immigration and trade, and civic participation. The goal in doing so was to more closely approximate the experience of voters in the real world where negative or positive information about their incumbent's voting record might be obtained close to, but not immediately before, the act of voting.

Results

We estimated ordered probit models of incumbent approval, incumbent electoral support, and intended turnout as a function of congruence, whether the subject received the treatment, the interaction between the two, and theoretically justified covariates. The quantity of interest is the interaction term. A significant positive interaction indicates that treating subjects by providing information about the degree of congruence between them and their representative increases the impact of congruence, thereby enhancing accountability. Given party identification's overwhelming impact on candidate evaluation and vote choice (e.g., Bartels 2000; Jacobson and Carson 2016), we included copartisanship as a covariate for approval and electoral support (coded 1 for subjects whose party identification matched their representative's party, 0 for Independents, and -1 for out-partisans). Along the same lines, since education and income are two strong drivers of turnout (e.g., Leighley and Nagler 1992; Sheilds and Goidel 1997; Verba, Schlozman, and Brady 1995), we included them as covariates for intended turnout. We note, however, that models excluding covariates generate similar estimates of the treatment's effects. We also had to decide how to handle subjects who said they did not know how to answer our outcome items. Coding such subjects at the middle of the outcome scale (Table 2, columns 1, 2, 4, 5)

or excluding them (Table 2, columns 3 and 6) generated similar results. We omitted subjects whose incumbent representatives were not running for re-election in the electoral support model and subjects who reported already having voted in the intended turnout model.

Regardless of which model is estimated, informing voters of their congruence level had a systematic impact on incumbent favorability and on incumbent electoral support, as seen in the positive and statistically significant interaction terms in Table 2, models 1 through 6. In general, respondents who enjoy a higher level of congruence feel more favorable toward and are more likely to vote for their incumbent representative. Directly sharing actual levels of congruence with subjects amplifies this relationship. The effect of the information persists even when accounting for subjects' copartisan status and regardless of whether subjects unable to respond to the incumbent evaluation questions are coded as holding a middling evaluation or omitted from the analysis.

Figures 1 and 2 illustrate the treatment's impact on incumbent favorability and support compared to the control group. For ease of interpretation, the figures are based on regression versions of Table 2, models 1 and 4, reporting the predicted level of support within groups that shared the same level of actual congruence. The regression and ordered probit models are quite similar (see the online supporting information for regression results). Figure 1 reveals three important patterns. First, consistent with observational evidence of accountability (e.g., Ansolabehere and Jones 2010; Simas 2013), among the control group, evaluations of incumbents improve as congruence increases (see the statistically significant estimate for Congruence). However, it is important to note that the upward slope for the control group may be the result of both increased congruence and copartisanship since copartisans tend to have greater congruence.

Second, the treatment enhanced accountability. The correlation between incumbent approval and congruence is higher for the treatment group than the control group. The difference between control subjects with low (20%) and high (80%) actual congruence was 0.37 points on a scale of -2 to $+2$. The difference is more than double that (0.97) for the treatment group. Subjects in the treatment group (punished) rewarded incumbents for their

TABLE 2
Models of Incumbent Favorability, Incumbent Support, and Congruence

	Incumbent Favorability			Electoral Support		
	(1)	(2)	(3)	(4)	(5)	(6)
Congruence × Treatment	1.01** (0.20)	0.99** (0.20)	1.04** (0.23)	0.67** (0.23)	0.68** (0.22)	0.83* (0.34)
Congruence	0.31* (0.14)	0.54** (0.14)	0.65** (0.17)	0.57** (0.16)	0.83** (0.16)	1.25** (0.24)
Treatment	-0.37** (0.13)	-0.37** (0.13)	-0.37** (0.16)	-0.25 (0.15)	-0.27 (0.15)	-0.31 (0.22)
Copartisan Status	.46** (0.03)			0.59** (0.03)		
<i>N</i>	2,129	2,129	1,532	1,910	1,910	1,134
DK Omitted			x			x

p* < 0.05; *p* < 0.01.

FIGURE 1
Effect of Congruence Information on Incumbent Approval

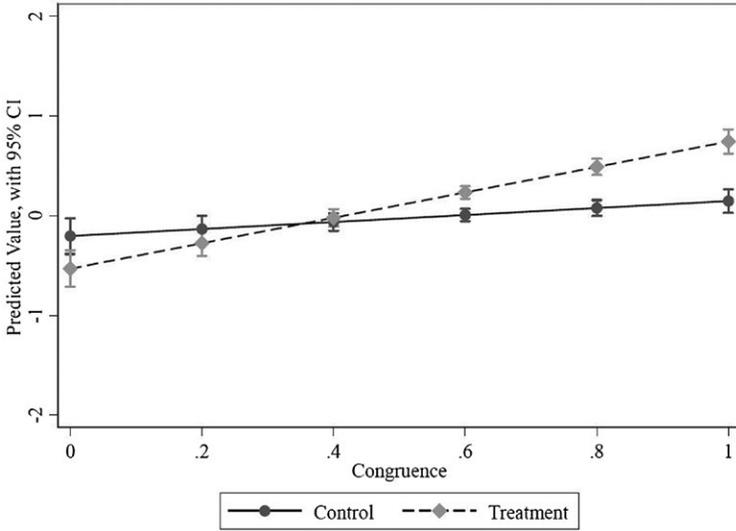
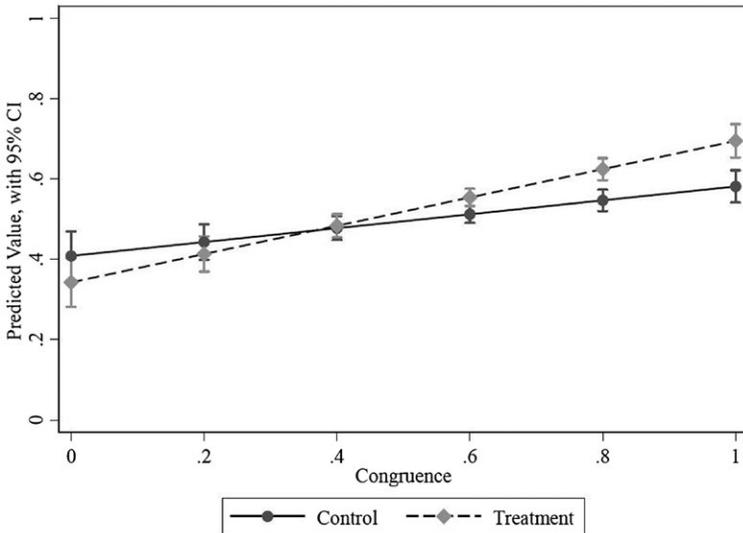


FIGURE 2
Effect of Congruence Information on Incumbent Support



(lack of) policy congruence more than those in the control group. Congruence information enhanced accountability.

Third, the threshold at which citizens will deem their MCs sufficiently congruent to merit their approval appears to be in the neighborhood of 50%. We infer this from the approximate level of congruence at which the trend line for the treatment group crosses zero, meaning that average evaluations go from being unfavorable to favorable. Citizens appear not to expect anywhere near pure congruence in exchange for their approval; congruence a majority of the time suffices. This novel and important finding builds on previous work stressing that studies of democratic representation and accountability should test for and not assume the level of agreement that citizens will expect of elected officials to earn their support (Griffin and Flavin 2011; Harden 2013). We will return to this final point in our concluding section.

Next, we illustrate the effect of the treatment on subjects' vote intentions (based on Table 2, model 4) and again find that the treatment enhanced the impact of congruence (see Figure 2). Intent to vote for the incumbent was coded 1, to vote for the challenger was coded 0, and uncertain voters were coded 0.5. Subjects in both the control and treatment groups planned to hold their MCs accountable at the ballot box, as reflected in the upward-sloping trend lines for both groups (again, the upward slope for the control group could reflect the impact of both congruence and copartisanship). The treatment had no clear impact among those with low underlying levels of congruence. All respondents with limited congruence were less likely to say they would vote for the incumbent than those with high levels of congruence. However, the treatment did affect the extent to which citizens planned to reward MCs with high congruence. Those informed of high (0.8) levels of congruence were more likely to vote for the incumbent compared to the control group (average responses of 0.60 versus 0.51). Again, the treatment enhanced accountability, especially at high levels of policy congruence.

We also examined the effect of congruence information on citizens' intent to vote (omitting individuals who indicated that they had voted prior to taking the survey or said they did not know if they were likely or unlikely to vote). Table 3 suggests that conveying information about congruence to constituents may help to motivate people who agree with the politician to turn out.

TABLE 3
Models of Intended Turnout and Congruence

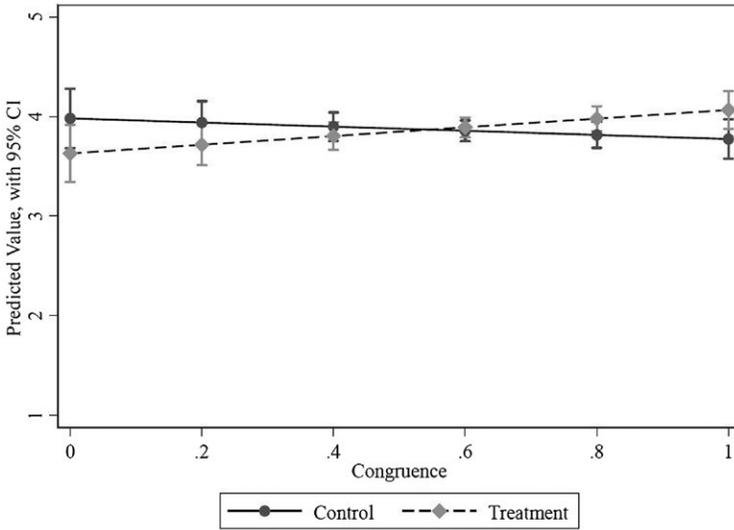
	(1)	(2)
Congruence × Treatment	0.51* (0.25)	0.51* (0.25)
Congruence	-0.11 (0.18)	-0.14 (0.18)
Treatment	-0.26 (0.17)	-0.26 (0.17)
Income	0.05** (0.01)	
Bachelor's Degree	0.30** (0.07)	
Advanced Degree	0.36** (0.11)	
<i>N</i>	1,497	1,497

* $p < 0.05$; ** $p < 0.01$.

The dependent variable is a five-point scale ranging from 1 to 5 reflecting whether respondents “will not vote” to whether they “definitely will vote.” Treating the subjects appeared to magnify the relationship between congruence and intended turnout, as evidenced by the positive and statistically significant interaction effects in models including (column 1) and excluding (column 2) covariates.

Figure 3 illustrates the treatment's substantive impact. There are several things to note. First, for the control group, congruence was negatively related to turnout, but the relationship is not statistically significant (see the insignificant congruence estimate).¹⁸ Second, for the treatment group, there is a positive relationship between congruence and turnout. This estimate suggests that citizens are stimulated to participate in elections when they learn that their representative is voting in such a way as to advance their needs and concerns. From the perspective of accountability, we would also like to observe that citizens who learn of their low congruence are also stimulated to participate, which does not appear to be the case. Whether this null finding is due to demoralization from learning of “poor” representation or a savvy assessment of the likelihood of the incumbent leaving office, we cannot say. A relatively small proportion of our subjects experienced a low level of congruence, so we cannot interrogate this finding further. We hasten to add that these findings should be received with some caution, as they are somewhat less certain than those reported above (note overlapping confidence intervals) and less robust (see the online supporting information for details).

FIGURE 3
Effect of Congruence Information on Intended Turnout



Heterogeneous Treatment Effects

We examined whether the treatment effects of congruence information were concentrated only among some citizens. Some studies have found that policy or ideological congruence is less strongly related to candidate evaluations for partisans (Shor and Rogowski 2018; Simas 2013) while others (Adams et al. 2017) find moderates focus less than nonmoderates on congruence. In addition, out-partisans may be susceptible to motivated reasoning, “arguing against” positive information about their representative (e.g., Taber & Lodge, 2000). In contrast, copartisans who are provided with discouraging congruence information may discount it, trusting their representative (e.g., Bianco 1994).

To examine whether copartisan status moderates the effect of the treatment, we modeled our three outcome variables with a three-way interaction of Congruence, Treatment, and Copartisan Status (three category). The full specification of these models is reported in Table 4. These estimations identified a statistically significant three-way interaction for the incumbent approval model but insignificant results for the other

TABLE 4
Effect of Congruence on Outcome Variables, by Treatment and Copartisan Status

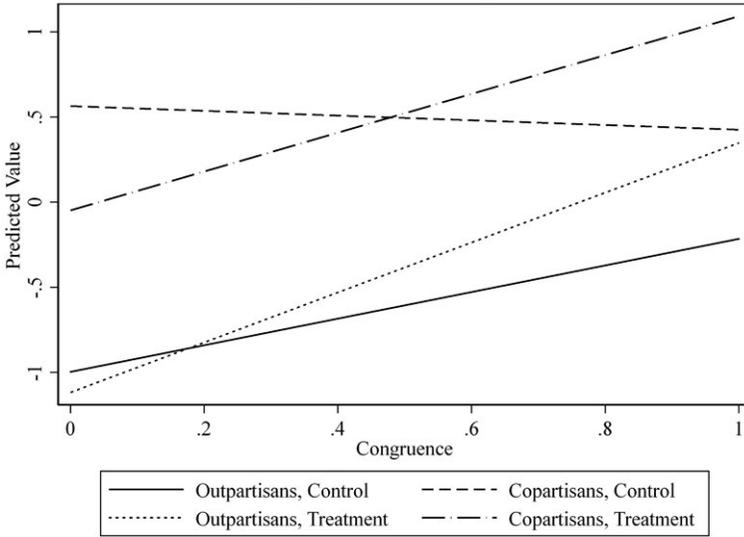
	Incumbent Approval	Electoral Support	Intended Turnout
Congruence × Treatment × Copartisan Status	0.43* (0.22)	0.17 (0.25)	0.19 (0.28)
Congruence × Treatment	1.04** (0.20)	0.69** (0.23)	0.45 (0.26)
Copartisan Status × Treatment	-0.31* (0.14)	-0.15 (0.17)	-0.03 (0.18)
Congruence × Copartisan Status	-0.48** (0.15)	-0.15 (0.18)	0.32 (0.20)
Congruence	0.29* (0.14)	0.56** (0.16)	-0.16 (0.18)
Treatment	-0.40** (0.13)	-0.26 (0.15)	-0.24 (0.17)
Copartisan Status	0.78** (0.10)	0.71** (0.12)	-0.12 (0.13)
cut1	-0.98** (0.10)	-0.28** (0.11)	-1.28** (0.13)
cut2	-0.31** (0.10)	1.00** (0.11)	-0.91** (0.12)
cut3	0.52** (0.10)		-0.55** (0.12)
cut4	1.77** (0.10)		-0.08 (0.12)
<i>N</i>	2,129	1,910	1,497

* $p < 0.05$; ** $p < 0.01$. Cell entries represent ordered probit coefficients.

two outcome variables.¹⁹ As a general matter, and as we elaborate below, we conclude from these results that the treatment's effects are widespread, affecting copartisans and others fairly similarly.

To probe the substantive meaning of the significant three-way interaction in the incumbent approval model (Table 4, model 1), we generated a figure reflecting predicted approval by level of Congruence, contingent on copartisan status (copartisan or out-partisan) and the treatment (Figure 4).²⁰ To simplify the presentation of the results, we pool the relatively small number of Independents with out-partisans for the purpose of these comparisons. The figure reveals that among the treated, the correlation between congruence and approval was similar for copartisans and out-partisans (the two lines for treatment groups are basically parallel). The figure also shows that the treatment induced greater accountability for copartisans—indeed, there was no correlation at all between congruence and approval among copartisans not receiving the information.²¹ Without the treatment information, copartisans did not appear to punish or reward their representatives for policy (in)congruence.

FIGURE 4
Effect of Congruence on Incumbent Approval, by Copartisan Status and Treatment



Among out-partisans, the treatment drastically sharpened the relationship between congruence and approval such that those with the highest levels of congruence favored the incumbent on average.²² In the end, the slope of the relationship between Congruence and Approval for out-partisans and copartisans is similar among those receiving the treatment, and any difference in the impact of the treatment is owing to a difference in the relationship between underlying congruence and approval among the untreated.

We also examined whether treatment effects varied by subjects' education and information levels. We found no statistically significant differences in the treatment effect across groups (see the online supporting information). In short, congruence information appears to have a broad impact on voters.

A final type of treatment heterogeneity, namely, that subjects' expectations of congruence varied even when their actual congruence was the same, is worthy of note (see the online supporting information for a full discussion). In general, we find that exceeding a subject's expectations for policy congruence improves favorability for the incumbent. Among those whose pessimistic

expectations were contradicted by the treatment, the treatment-group mean favorability was 0.28 points higher than the control-group mean for those informed of their middling congruence ($p < 0.05$, $N = 366$) and 0.68 points higher for those informed of their high congruence ($p < 0.01$, $N = 309$). Among those who expected modest congruence, learning that their positions lined up well increased favorability in the treatment group by 0.33 ($p < 0.01$, $N = 590$). These changes in favorability translated into electoral support. For respondents who expected they would be incongruent but were actually highly congruent, treatment subjects were more likely to say they would vote for the incumbent (+0.28 on a -1 to $+1$ scale, $p < 0.01$). Among subjects who thought they enjoyed moderate levels of MC congruence, likelihood of supporting the incumbent was higher among subjects who were informed of high congruence (0.14, $p < 0.01$) and lower among subjects who were informed of low congruence (-0.18 , $p = 0.06$). Providing surprising information to subjects had little systematic impact on turnout. Thus, it appears that citizen priors about policy congruence with incumbents are an important moderator to informational treatments and future research should explore them more completely.

Conclusion

Constituents' voting behavior can act as a mechanism to encourage policy congruence if constituents punish and reward their representatives for the extent of congruence those representatives provide. We found that subjects updated their views of the incumbent and their vote intentions to line up with the information provided in the treatment. The impact of congruence information is widespread, relatively consistently affecting copartisans, Independents, and out-partisans. In addition, congruence information appears to have motivated subjects with relatively high levels of congruence to want to vote. In all, congruence information appears to enhance accountability.

Our study makes several contributions. First, we add experimental evidence to the individual-level observational evidence showing that constituents whose MCs are out of step with the constituents' preferences will punish the MCs (e.g., Ansolabehere and Jones 2010; Nyhan et al. 2012; Shor and Rogowski 2018; Simas 2013). That is, we are able to isolate the impact of congruence information on incumbent evaluations from all the factors

correlated with congruence. Since citizens holding their elected officials accountable for their actions is at the heart of democratic theory, it is hard to overstate the significance of this finding. Second, observational studies struggle to isolate the directionality of congruence and evaluations (see Hirano et al. 2015). Unlike extant evidence that policy congruence shapes voting behavior, our experimental evidence can point to the causal direction of the link between congruence information and candidate evaluations. Isolating the causal impact of congruence information has a powerful political and normative implication: Since congruence information increases the reward for congruence, making this kind of information easily available to voters may encourage re-election-minded representatives to provide greater policy congruence.

Third, we show that information about congruence can mobilize. Subjects learning they had high levels of congruence were more likely to turn out to vote to support their incumbent, though these results are a bit less robust. This is consistent with studies showing that external efficacy, which is akin to perceived congruence, is associated with electoral participation (Abramson and Aldrich 1982). Fourth, our study illuminates a point not often, if ever, made clear in studies of accountability. How much congruence do constituents expect? Will voters punish incumbents for a single “wayward” vote? Our results suggest they may not, at least not on typical high-profile roll-call votes. As others (e.g., Highton 2018; Tausanovitch and Warshaw 2018) have found, we found that MCs often have some leeway. Constituents appear to forgive their MCs a few incongruous votes; they seem to trust their representatives, up to a point (Bianco 1994). In general, we find that constituents tend to favor incumbents who vote as they would like more than half the time. Of course, a particular bill may come along every now and then that is so important to voters that a “wrong” vote will immediately alienate constituents (e.g., Miller and Stokes 1963, 55; Nyhan et al. 2012), but, in general, constituents seem to forgive a few unpopular votes.

Fifth, our results have implications for campaign strategists. Our finding that constituents are less likely to vote for incongruent incumbents supports the common practice among challengers (together with the news media) of informing constituents of wayward votes (Arnold 1990, 2004 ; Sulkin 2005). In short, such a strategy appears likely to work. Along the same lines, our results lend support to the common practice among interest

organizations of generating and disseminating legislator “scorecards.” If informed about multiple votes that run counter to a constituent’s preferences, constituents appear ready to punish the incumbent.

Future research can refine and expand our understanding of information’s role in accountability. Since experimental studies of accountability are rare or nonexistent, we focused on developing an experiment with a high degree of internal validity. We provided subjects a single piece of information about a small set of roll-call votes from a relatively credible source and measured its impact after a few distractor items. Future studies could expand the external validity of similar experiments by varying each of these dimensions. Such studies could determine, for example, whether information about policy congruence beyond a few roll-call votes provided from campaigns or SuperPACs has a lasting impact in a campaign environment. In the real world, voters often encounter congruence data in the information-rich environment of a campaign. Future research should examine the effects of congruence information when such information is one of several pieces of information available. For example, incumbents may be able to explain incongruous votes in ways that constituents will tolerate (Fenno 1978) or to emphasize positive, nonideological information (e.g., the candidate’s charisma or experience, district services, or clout on a committee of interest to the district) enough that voters weigh the positive information more than the negative congruence information when casting ballots (e.g., Druckman, Jacobs, and Ostermeier 2004). Third, future studies should continue to examine turnout in the context of accountability. Field experiments may be especially equipped to identify the precise impact of congruence information on turnout. Finally, our study provides true information. In an age of “fake news” and “alternative facts,” researchers should examine the impact of false information on accountability, especially since false information appears to have long-term impacts on voters (e.g., Nyhan and Reifler 2010).

Our study demonstrates the need for continued study of the impact of information about congruence because such information affects the degree to which constituents hold their representatives accountable. Such information is a staple of congressional campaigns, be it in campaign advertisements or interest group scorecards. The availability of this information about incumbent congruence likely encourages greater levels of policy congruence

from representatives concerned about the possibility of voter backlash. This suggests that efforts to publicize information about incumbent congruence are an important component of democratic politics because they may promote greater accountability.

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NOTES

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1. "The Federalist Papers: No. 57," The Avalon Project at the Yale Law School, 26 January 2002, <https://avalon.law.yale.edu/18th_century/fed57.asp> (25 September 2018).

2. Levendusky argues that highly informed individuals probably differ in many ways from less informed individuals, some of which may be correlated with the dependent variables in question. Controlling for all of these factors "is a Sisyphean-task" (1963, 45; see also Althaus 2003; Barabas and Jerit 2009; Bartels 1996).

3. For example, Ansolabehere and Jones (2010) account for endogeneity statistically by using actual agreement between a voter and his or her MC to instrument perceived agreement in models of incumbent approval and electoral support. However, some MCs represent districts in which a majority of voters supported the opposing party's candidate in the most recent presidential election. These districts are likely to be targeted by the opposing party as likely "pick-ups," with substantial resources directed toward discrediting the incumbent. In this way, party strategies connect actual congruence to voters' affinity toward the incumbent and, crucially, this occurs even if voters are unaware of their actual congruence with their representatives. Thus, actual vote congruence violates the exclusion restriction and fails to provide an unbiased instrument for perceived congruence.

4. Respondents were recruited using pop-up windows on Qualtrics partner websites. For instance, respondents were rewarded with frequent flyer miles for completing the survey on an airline website.

5. Online panels have been found to be at least as reliable as telephone-based surveys (e.g., Chang and Krosnick 2009; Hays, Liu, and Kapteyn 2015; Yeager et al. 2011), but samples need to be drawn probabilistically. While many companies that supply survey samples online recruit their participants randomly, we have no way of knowing which vendors Qualtrics used and cannot speak to the sample recruitment process. Our sample clearly contains people who like to take online surveys. However, since the pop-up ad for our survey said nothing about politics, our sample is not biased towards political interest and is likely to be representative of online consumers willing to answer surveys.

6. Because individuals with higher levels of educational attainment are more prone to motivated reasoning, we may observe somewhat more of this behavior in our data than exists in the general population.

7. See <https://openncrs.com/>.

8. We note that the relevant roll-call vote was held on March 10, 2010, seven months before the survey. It is possible that respondents could have changed their views on this particular question during this period. Although there was some change, at least in the aggregate attitudes on troop levels in Afghanistan did not change dramatically over this period. For example, the Quinnipiac Poll asked a question closely related to the roll-call vote: “Do you think the U.S. is doing the right thing by fighting the war in Afghanistan now, or should the U.S. not be involved in Afghanistan now?” In March 2010, 37% said the United States should not be involved right now. In September 2010, the last time the item was asked before the election, 41% said the United States should not be involved right now.

9. We included the phrase “last year” because by the time the survey was in the field, the national unemployment rate had climbed to 9.7%.

10. We considered but rejected offering a “Don’t know” response option. Omitting the “Don’t know” response maximizes the number of respondents we are able to match to their MC. Presumably, the disadvantage is increasing the number of “nonattitudes” offered, but this is less of a concern since our treatment reports back percent agreement with the MC.

11. See https://www.census.gov/geo/www/cd110th/natl_code/zcta_cd110_natl.txt.

12. Admittedly, this process would for a small fraction incorrectly match respondents and their representatives’ roll-call records. This does not concern us greatly, for two reasons. First, according to our data vendor, roughly 3,000 of the 42,000 zip codes (7%) lie in multiple congressional districts. At best, only 0.1% of the populace lives in the “wrong district”; at worst it would be 50%. For the moment, assume the real value is 25%. In that case, we would estimate that roughly 2% of the sample might be mistaken. No subjects reported being assigned to the wrong district. Second, we are interested in how respondents react to the information they are provided, regardless of its veracity.

13. A handful of respondents did not provide answers on all five issues. Similarly, the representative did not vote on all five issues for another handful of respondents. For the purposes of most of the analysis, these people were grouped with the respondents with the closest issue congruence (i.e., those with 25% congruence (one of four) were grouped with the one in five group, and those with 75% congruence were grouped with those with 80% congruence).

14. The treatment's language is as follows: "In case you were wondering, your Member of the U.S. House of Representatives, [MC Name], is a [Party Name]. All of the issues you just took a position on were voted on in Congress over the last two years. [Name], agreed with you on X of the Y issues on which you expressed an opinion and [MC Name] cast a vote."

15. We measured how long each subject viewed the screen with the treatment to assure ourselves that it was being read. The median subject viewed the treatment screen for 16 seconds. Limiting the analysis to only those members of the treatment and control group who looked at the "treatment" page for at least 3 seconds slightly strengthens the results reported below.

16. So, control condition subjects were informed: "In case you were wondering, your member of the U.S. House of Representatives, [MC Name], is a [Party Name]."

17. Question wordings: "Thinking about your Representative [Name], which of the following comes closest to your view? [Approve strongly of job he/she is doing (2); Approve somewhat of job he/she is doing (1); Disapprove somewhat of job he/she is doing (-1); Disapprove strongly of job he/she is doing (-2); No opinion (0)]."

18. "In this election, do you support the Incumbent, Representative [Name], or the challenger? [Incumbent (1); Challenger (-1); Don't know (0)]."

19. "In the upcoming Congressional election, how likely are you to vote? [Already voted; Definitely will vote; Very likely will vote; Somewhat likely will vote; Not very likely will vote; Will not vote; Don't know]." Respondents who indicated they had already voted were excluded from the turnout analyses.

20. Note that the vote-choice question only includes the incumbent's name. Although this fact may bias responses, there is no reason to expect that it would do so differently across treatment and control conditions. Since we are interested in measuring the effect of the treatment rather than the overall propensity to vote for the incumbent, this wording does not affect our analysis. This wording also has the virtue of keeping the questions comparable across subjects (i.e., the challenger name would introduce added heterogeneity).

21. Relatedly, Flavin and Griffin (2007) find that those with the highest level of disagreement with policy participate in politics more than those in somewhat more agreement do.

22. Note that the congruence by treatment interaction (the treatment effect for non-copartisans) in the intended turnout model falls short of statistical significance ($p = 0.08$).

23. A parallel figure for the electoral support model is included in the online supporting information.

24. This is a rather surprising finding, and one that has somewhat more sobering implications for the robustness of accountability in our political system. Copartisans who enjoy high levels of congruence are not more favorable toward their incumbent compared to copartisans who experience less congruence. It may be that relatively few copartisans are incongruent, and those that are have an affinity for their representative that is not issue based or is based on the party-defining issues that our experiment avoided.

25. Notably, and somewhat disconcertingly from the perspective of accountability, the treatment did not convert highly congruent out-partisans (few as they may be) into electoral supporters of the incumbent (see Figure A2 in the online supporting information). This could be due to the fact that our experiment avoided party defining issues such as taxes and abortion.

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Supporting Information

Additional supporting information may be found in the online version of this article at the publisher’s website:

Table A1: Comparison of Sample with Census Data

Table A2: Balance Across Treatment Conditions

Supplemental Models

Table A3: Effect of Congruence Information on Incumbent Approval and Electoral Support, Regression Models

Table A4: Effect of Congruence Information on Intended Turnout, Regression Models, Multinomial Logit Model of Combined Approval and Turnout

Table A5: Effect of Congruence Information, by Expected Congruence

Table A6: Effect of Congruence Information on Incumbent Approval, Incumbent Electoral Support, and Turnout, by Expected Congruence

Table A7: Effect of Congruence Information, by Party Identification

Table A8: Effect of Congruence Information, by Informedness

Table A9: Effect of Congruence Information, by Educational Attainment, Probit Models of Tables 2 and 3 from Main Text

Table A10: Probit Models of Incumbent Approval and Electoral Support

Table A11: Probit Models of Intended Turnout

Figure A1: Multinomial Logit Models, of Approval and Turnout Intention Supplemental Analysis of Potentially Heterogeneous Treatment Effects

Figure A2: Effect of Congruence on Incumbent Electoral Support, by Copartisan Status and Treatment

Figure A3: Effect of Congruence on Incumbent Approval by Condition, Based on Probit Model

Figure A4: Effect of Congruence on Incumbent Vote Support by Condition, Based on Probit Model

Figure A5: Effect of Congruence on Turnout, Based on Probit Model