AJPS AMERICAN JOURNAL of POLITICAL SCIENCE

# Congressional Representation: Accountability from the Constituent's Perspective

**Stephen Ansolabehere and Shiro Kuriwaki** Harvard University

**Abstract:** *The premise that constituents hold representatives accountable for their legislative decisions undergirds political theories of democracy and legal theories of statutory interpretation. But studies of this at the individual level are rare, examine only a handful of issues, and arrive at mixed results. We provide an extensive assessment of issue accountability at the individual level. We trace the congressional roll-call votes on 44 bills across seven Congresses (2006–18), and link them to constituent's perceptions of their representative's votes and their evaluation of their representative. Correlational, instrumental variables, and experimental approaches all show that constituents hold representatives accountable. A one-standard deviation increase in a constituent's perceived issue agreement with their representative can improve net approval by 35 percentage points. Congressional districts, however, are heterogeneous. Consequently, the effect of issue agreement on vote is much smaller at the district level, resolving an apparent discrepancy between micro and macro studies.*

**Verification Materials:** The materials required to verify the computational reproducibility of the results, procedures and analyses in this article are available on the American Journal of Political Science Dataverse within the Harvard Dataverse Network, at: https://doi.org/10.7910/DVN/QOVWMM.

Representative democracy rests on a simple idea. Constituents elect politicians to be their agents in passing laws and setting public policy. If an individual constituent disagrees with the actions or decisions of a representative, the constituent may choose someone else at the next election. A majority of the electorate can elect another legislator or party to represent them. Modern political science has taken this notion of accountability as the cornerstone for theorizing about representation and for studying law making in representative democracies, especially within the American Congress (Mayhew 1974). Constitutional theory and even Supreme Court doctrine treat electoral accountability as the wellspring of legislative and executive authority in the United States (Eskridge 1987).

As important as the classical theory of accountability is, it is ultimately just a theory. The empirical foundations for this idea are, as Stephanopoulos (2018) recently surmised, underdeveloped. One significant line of research has established a connection at the district level between the legislator's congruence with their district and election results (Canes-Wrone, Brady, and Cogan 2002). But as Carson et al. (2010) suggest, without constituent-level data, these aggregate estimates might also reflect party loyalty, ideology, presidential approval, or other factors. A second line of research has explored the individual foundations for electoral accountability. In their path-setting article, Miller and Stokes (1963) concluded that most constituents lack the knowledge to hold their representatives accountable and, as a result, that there is low congruence between constituents and legislators.

The conclusion that Miller and Stokes reached has spawned a very different view of congressional politics in the United States than that embraced in the classical theory of representation. If voters cannot exert electoral accountability, representation breaks down. That gives elites, such as interest groups, policy ideologues, or wealthy donors, an opening to capture the political

Stephen Ansolabehere is Frank G. Thomson Professor, Department of Government at Harvard University, 1737 Cambridge St, Cambridge, MA 02138 (sda@gov.harvard.edu; https://scholar.harvard.edu/sansolabehere). Shiro Kuriwaki is PhD candidate, Department of Government at Harvard University, 1737 Cambridge St, Cambridge, MA 02138 (kuriwaki@g.harvard.edu; https://www.shirokuriwaki.com).

We thank Mathias Tromborg and participants at EPSA 2018, Chris Tausanovitch, and participants at SPSA 2019, as well as David Broockman, Christopher Cochran, Sebastian Thieme, Soichiro Yamauchi, and workshop participants at the University of California Santa Barbara, University of Texas Austin, Arizona State University, Princeton University, and Harvard University for their comments and suggestions. We acknowledge NSF's support for the Cooperative Congressional Election Study (CCES) over the years (Award Numbers SES-0924191 [2010], SES-1225750 [2012], SES-1430505 [2014], SES-1559125 [2016], SES-1756447 [2018], SES-1154420 [panel]), and the Harvard FAS Dean's Competitive Fund.

***American Journal of Political Science,*** Vol. 00, No. 0, XXXX 2021, Pp. 1–17

©2021, Midwest Political Science Association DOI: 10.1111/ajps.12607

process (Bartels 2008; Bawn et al. 2012). Bawn et al. (2012) challenge the notion that "to win elections politicians must do what voters want." They argue "voters do not pay so much attention to politics," and those "limitations of most voters to hold their legislators accountable" create the conditions for extreme partisanship in Congress (pp. 589–90). Has the constituents' side of the accountability mechanism actually broken down?

Scholarship in the past two decades has repeatedly debated the questions raised by Miller and Stokes. Clinton (2006) and Bafumi and Herron (2010) used key votes in Congress as better measures of ideological congruence, and Ansolabehere and Jones (2010) and Guisinger (2009) provided evidence that individual constituents reward representatives with whom they perceive to be in agreement on specific roll-call votes. Since those initial studies, the debate over whether partisan constituents can hold their representatives accountable on issues has intensified. Several studies have argued that partisanship distorts people's perceptions and swamps issue voting (Broockman and Butler 2017; Lenz 2012), but others reach opposite conclusions (Bullock 2011; Fowler 2020).

Here, we offer an extensive empirical assessment of congressional accountability, tracing representative's roll-call votes to constituent's perceptions about those specific votes, and finally from those pictures in people's heads to the electoral evaluations that they make of their representatives. We study the entire 12-year span of the Cooperative Congressional Election Study (CCES), which covers over 67 roll-call votes and asks constituent's perception on 44 of them. Dramatic swings in political control of the U.S. government from 2006 to 2018 mean that our study captures nearly every political constellation: unified Republican control, unified Democratic control, divided control with a Republican President and a Democratic Congress, and divided control with a Democratic President and a Republican Congress. The key votes we track reflect the wide-ranging policy agenda during this time, including war, health care, trade, banking, wages and labor discrimination, the budget and taxation, welfare programs, immigration, crime, guns, education, abortion, agriculture, and gay rights.

This study contributes to four foundational questions on electoral accountability. We significantly extend past findings on these questions with new data and designs, and we reconcile seemingly contradictory claims in the literature.

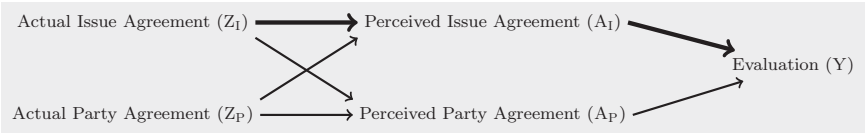
First, what is the relationship between legislators' congruence with their constituents and constituents' evaluations of their legislators? Consistent with past work, we find that an individual constituent's agreement between representatives leads to positive evaluations of that representative. In contrast to the work that focuses on ideological agreement, we also measure agreement in terms of the specific votes Members of Congress cast. We find that agreement on these key votes affects evaluations even after controlling for agreement in terms of party affiliation and latent ideology, suggesting that each key vote matters. We call this relationship the reduced form because it is simply the relationship between what legislators do and what voters do, and does not establish the mechanism operating in voters' minds.

Second, are constituent's perceptions of their representative's actions in Congress accurate? This is the critical first step in the accountability process. Do people have, to use Walter Lippmann's expression, a picture in their heads about how their representatives voted, and is that picture close to reality? The answer is largely yes. Most people have a belief about how their representatives voted, and among those who provided an answer, a majority have the correct belief about their representative's votes. To the extent that there is slippage, it takes two forms: uncertainty and misperception. About two in five people do not readily express a belief about how their representatives voted on the average bill. Also, copartisans tend to perceive more issue agreement than actual agreement, but these biases appear to be second-order effects compared with the main effect of correct issue perception.

Third, do constituents in fact support representatives *because* they think they agree on key legislation that Congress has voted on, independently from party? Or, is the correlation between issue agreement and evaluation actually partisan projection (Lenz 2012)? We start with difference-in-means estimates suggesting that perceived issue agreement has independent effects on approval and vote choice. We then replicate the instrumental variable (IV) identification strategy of Ansolabehere and Jones (2010), albeit over a much wider span of time and issues. Finally, we conduct two survey experiments and conduct sensitivity analyses to address the concerns that the IV conditions may not hold. These correlational, IV, and experimental approaches all show that perceived issue agreement on key legislation *does* translate into electoral support, and the effects are substantial and operate independent of party.

Fourth, why are the individual-level effects of issue accountability so much larger than the district-level effects, as noted by Tausanovitch and Warshaw (2018)? Studies using survey data at the individual level typically report a 10 to 20 percentage point effect of congruence on an individual's vote choice (Ansolabehere and Jones 2010; Jessee 2009; Shor and Rogowski 2018). Yet, Canes-Wrone, Brady, and Cogan (2002) find that House

**FIGURE 1 Accountability from the Constituent's Perspective**




*Note:* Arrows show possible causal effects, and bold arrows show issue accountability. This is a Directed Acyclic Graph (DAG) for our estimation strategy. The random variables we use to denote each concept are shown in parentheses. Control variables and unobserved confounders are not shown for clarity but are addressed in the main text.

members gain only 1 to 3 percentage points of their vote share from moderating toward the party's median rather than from voting at the extreme of their party (see also Bonica and Cox 2018; Erikson, Mackuen, and Stimson 2002; Fiorina 1974). We show that the tension results from the aggregation of individual voters to the district level. Congressional districts are sufficiently heterogeneous that a Member of Congress voting on the side of the majority of her constituents would still disappoint a sizable minority. As a result, even when many constituents care about and are knowledgeable about salient issues and when a representative votes with the majority in a district, the aggregate congruence can come out quite low. This does not mean that issues are unimportant, because voting against the district majority would be even more costly.

In what follows, we uncover a picture of the electorate that, although not hyperinformed and hyperrational, is one in which constituents are sufficiently attentive that the majority can and does hold their representatives accountable for the decisions that they make on important pieces of legislation.

## Models and Methods

We start by outlining our model of constituent accountability, setting up our identification strategy, and describing how we combine CCES data, experiments, and roll-call votes on the House floor to identify these mechanisms.

### A Model of Reality, Perception, and Evaluation

Figure 1 depicts the causal process of issue accountability. We trace the bolded arrows from left to right: In the initial stage, representatives belong to a party, they cast a vote on a bill, and constituent's have a preference for that bill as well. A representative and her constituent are in actual issue agreement when they have the same preferences.

The subtlety in testing theories of accountability is that constituents can only act on what they know (Gilens 2001). Therefore, we distinguish between two sorts of agreement in Figure 1: actual and perceived. For example, a pro–Affordable Care Act (ACA) constituent might believe that his representative also voted for the ACA (perceived issue agreement), either correctly (they are also in actual issue agreement) or incorrectly (the representative in fact had voted against the ACA). Unlike most studies of accountability, our study measures these perceptions directly.

The purpose of Figure 1 is to distill our operationalization of accountability and estimation strategy. It does not exhaustively display alternative causal pathways, including the possibility of projection in which voters perceive to be in agreement because they approve of the representative (Lenz 2012), or the possibility that constituents infer party loyalty (Carson et al. 2010) or ideological extremity (Nyhan et al. 2012) from roll-call votes. These mechanisms may be occurring simultaneously with issue accountability, perhaps with some voters but not others. Our contribution is to estimate one particular quantity—issue accountability as envisioned by the classical theory of representation—by controlling away such alternative explanations from our estimates. We focus on the dyadic relationship between a constituent and his representative and assume that, if a constituent can and does hold his representative accountable, he does so regardless of whether the legislator is pivotal.

This model of constituent perception is far from novel: It mirrors Figure 1 of "Constituency Influence in Congress" (Miller and Stokes 1963), which conceptualized how representatives made decisions based on their perceptions of their constituents' preferences. In fact, one goal of the present study is to provide a constituent's perspective of the sort of accountability studied in Miller and Stokes.

## Estimation Strategy

We focus on estimating three components of issue accountability depicted in Figure 1 from observed data and survey experiments. Most studies of accountability estimate the effect of actual agreement on evaluations. We estimate this quantity through the reduced form equation, indexing constituents by $i \in \{1, \ldots, n\}$:

$$\text{Reduced form: } \mathrm{Y}_i = \rho_0 + \rho_{\mathrm{I}} \mathrm{Z}_{\mathrm{I}i} + \rho_{\mathrm{P}} \mathrm{Z}_{\mathrm{P}i} + \mathbf{X}_i' \rho_{\mathrm{x}} + \varepsilon_{1i}, \quad (1)$$

where $\mathrm{Y}_i$, following Figure 1, is constituent $i$'s evaluation of his representative, $\mathrm{Z}_{\mathrm{I}i}$ is their actual agreement on issues, $\mathrm{Z}_{\mathrm{P}i}$ is their actual agreement on party affiliation, and $\mathbf{X}_i$ denotes a set of control variables we discuss later. A positive value of $\rho_{\mathrm{I}}$, interpreted causally, represents "Out of Step, Out of Office" (Canes-Wrone, Brady, and Cogan 2002): When a representative takes a vote that is not in agreement with her constituent, the constituent reacts by lowering their propensity to vote to reelect that incumbent. We refer to Equation (1) as the reduced form anticipating our instrumental variables estimation strategy.

Although important, the reduced form does not describe *how* the constituent came to that evaluation. The first stage therefore asks whether the actual agreement implied by legislators' roll-call votes shapes constituents' perceived agreement on those votes. Estimating this relationship from data corresponds to a linear regression:

$$\text{First stage: } \mathrm{A}_{\mathrm{I}i} = \alpha_0 + \alpha_{\mathrm{I}} \mathrm{Z}_{\mathrm{I}i} + \alpha_{\mathrm{P}} \mathrm{Z}_{\mathrm{P}i} + \mathbf{X}_i' \alpha_{\mathrm{x}} + \varepsilon_{2i}, \quad (2)$$

where $\mathrm{A}_{\mathrm{I}i}$ refers to $i$'s perceived agreement with their representative on the issues. A positive value of $\alpha_{\mathrm{I}}$, again interpreted causally, indicates that reality shapes perception: Controlling for actual party agreement and other possible confounders $[\mathrm{Z}_{\mathrm{P}i}, \mathbf{X}_i]$, constituents form on the whole correct perceptions about their representative's votes in Congress.

The first stage then leads to the central question of issue accountability: To what extent do constituents act upon those perceptions, as measured by their evaluations (e.g., job approval or their propensity to reelect her)?

$$\text{Second stage: } \mathrm{Y}_i = \beta_0 + \beta_{\mathrm{I}} \mathrm{A}_{\mathrm{I}i} + \beta_{\mathrm{P}} \mathrm{A}_{\mathrm{P}i} + \mathbf{X}_i' \beta_{\mathrm{x}} + \varepsilon_{3i}. \quad (3)$$

One threat to inference that is new in interpreting the $\beta$ coefficients causally is projection, which we can formalize as the endogeneity of perceived agreement. For example, a respondent might have underlying trust in the representative, which both leads to higher job approval and also leads him to the belief that the representative probably agrees with him on key issues too. To remove such potential confounding, we implement an instrumental variables strategy, instrumenting perception with actual agreement with Equation (2) as suggested by Figure 1. If our instrumental variables specification is appropriate, a two-stage least squares regression combining Equations (2) and (3) will estimate the magnitude of issue accountability unconfounded by projection.

For the instrumental variables approach to produce unbiased estimates, three conditions must be met. First, the first stage outlined in Equation (2) must be strong. We show in our results that this condition is easily met in our data. Second, the effect of the instrument on the outcome must flow exclusively through the variable being instrumented. Although this is an untestable assumption, we point out that it is a natural one in our setting given that constituents can only form evaluations based on what they perceive.

The third condition, exogeneity of the instrument conditional on controls, is the most difficult of the three to meet in our setting. The same sort of exogeneity condition is required for interpreting the reduced form and the first stage coefficients causally as well.

To achieve conditional exogeneity, we control for five types of well-known potential confounders in all our regressions. (i) Representative fixed effects account for any time-invariant and issue-invariant characteristics of each Member of Congress, such as party affiliation or personality that may induce spurious correlation across districts. (ii) Theories of partisan heuristics predict that many constituents infer positions only from associations with party labels, so we treat actual party agreement ($\mathrm{Z}_{\mathrm{P}}$) as a control. Next, a member's voting patterns are surely correlated with her latent ideology, so we control for (iii) ideological agreement with the incumbent to account for the representative's residual voting pattern after accounting for the key votes of interest. For similar reasons, we control for the (iv) ideological distance from the challenger in general election years, as proxied by constituents' placements of those candidates. Finally, we include (v) sociodemographic characteristics of the respondent that may shape evaluations and perceptions, including age, gender, race, education, and income.

In a "perfect" experiment, representative's actual roll-call votes would be randomized, exogeneity would be satisfied by design, and then analysts would only need to compare constituents' approval of a representative who voted yea with approval among otherwise similar constituents of a representative who voted nay. To approximate that impossible experiment and anticipating potential violations of conditional exogeneity, we conducted two survey experiments that randomize information about representative's votes. We hasten to note that any randomized control trial faces a limitation when testing theories of electoral accountability. Almost all field experiments assign constituents to hear about representative's real positions (e.g., Broockman and Butler 2017), and therefore induce variation in perceived agreement

($A_I$) but not in actual agreement ($Z_I$). The inferential strengths of observational and experimental approaches complement each other's weaknesses.

The final way we address potential violations to the exogeneity assumption is by applying sensitivity analyses. Recent statistical developments in this area provide a benchmark of how large the unobserved confounding must be for our main conclusions to reduce to null (Cinelli and Hazlett 2020). Put together, our experiments and sensitivity analyses indicate that constituents *do* react to information consistent with classical theories of accountability, and that any unobserved confounding in our observational analyses would have to be larger than the effect of copartisanship on the same outcome to flip our main findings.

## Data and Measurement

To operationalize our measures, we rely on the CCES from 2006 to 2018, covering the 109th Congress under the presidency of George W. Bush through the 115th Congress under the first 2 years of President Trump. A measure of constituent opinion on key votes is available for all CCES respondents, but measures of constituent's perceptions of those votes is only included in one or two team modules a year (Supporting Information, p. 1).[1] We therefore primarily use these respondents in the team module, and append information from the common content.

Each year CCES polls important issues from Congress' agenda, identified by the *Congressional Quarterly* or the *Washington Post Key Vote*. The Supporting Information (p. 2) lists all the issues on the CCES on which there were corresponding bills in Congress, and how the House and Senate dealt with that bill. In particular, we analyze 44 floor votes in the U.S. House of Representatives[2] for which perception questions were asked. In the first three Congresses (2006–10), both the House and the Senate usually took up key legislation. But during 2013 and 2015, Republican congressional leaders put few substantive policies for a vote on the House floor, fighting the President to a stalemate over the budget resulting in a government shutdown, which clogged the legislative agenda. Once the Republican party gained unified control in 2017, they passed more significant bills.

Representative behavior and constituent opinion are difficult to compare on the same scale. The CCES addresses this measurement challenge by presenting issues as a key vote that Congress is considering or is anticipated to consider,[3] and describing the issue in concrete terms. These questions have been widely used in other work on representation (Ahler and Broockman 2018; Bafumi and Herron 2010; Tausanovitch and Warshaw 2018).[4]

After each congressional term, we find the roll-call vote corresponding to each of the questions, if a floor vote was held, and link the respondent's U.S. House representative's vote to that response for our measure of actual agreement. Our measure of perceived agreement is built from the interaction between a constituent's perception of their representative and their own preferences. An example of a short[5] perception question comes from 2017:

> This year Congress considered several bills to repeal or change the Affordable Care Act. For each of the following bills we would like to know how you think your member of Congress voted and whether you support or oppose the bill.
>
> *A Bill to repeal the Affordable Care Act, known as Obamacare.*
> Do you support or oppose this bill?
>
> ☐ Support
> ☐ Oppose
>
> Do you think [Representative][6] voted for or against a bill to Repeal Obamacare?[7]
>
> ☐ For
> ☐ Against
> ☐ Not Sure

[1]Each team module and the common content are separately sampled and weighted to be representative of the national adult population.

[2]Throughout this article we focus on representation in the U.S. House. The U.S. Senate requires an even more complex analysis, owing to the multiple representatives per district and the possible effects of state size on representation. We leave that for future work.

[3]The CCES is fielded in the fall of each year. The legislative calendar is such that for almost all questions, the roll-call vote has already occurred before respondents answer the CCES.

[4]The benefits of this issue-by-issue approach are summarized well in Lax, Phillips, and Zelizer (2019). Hill and Huber (2019) show that providing contextual information such as the party leader's positions moves respondent's reported preference toward those positions. We use the responses to our questions as a measure of preferences before such cues are made explicit.

[5]Other bills were described more concretely, for example the Dodd-Frank Act was described as "Protects consumers against abusive lending, regulates high risk investments known as derivatives, and allow government to shut down failing financial institutions." All question wordings are available on the CCES Dataverse.

[6]The name of the representative is filled in with the respondent's U.S. House incumbent representative, without displaying their party affiliation.

[7]Several dozen unrelated questions are typically placed in between the first question (asked in the common content) and the second question (asked in the team modules) to minimize demand effects.

We then represent respondent $i$'s *Perceived Issue Agreement* on issue $j \in \{1, \ldots, m\}$ as $A_{Iij}$ and assign it a value of 1 if respondent $i$'s preference agrees with his perception of his representative's roll-call vote on issue $j$ (i.e., for–for or against–against). If the respondent's preference is in disagreement with his belief (i.e., for–against or against–for), then $A_{Iij} = -1$. In addition, $A_{Iij} = 0$ if the respondent either does not have a belief or does not express a preference on the issue. We then compute respondent $i$'s *Perceived Issue Agreement* with his representative as the average across issues, that is, $A_{Ii} = \frac{1}{m}\sum_{j=1}^{m} A_{Iij}$.

We code the rest of the variables similarly, from $-1$ to 1 to facilitate comparison. *Perceived Party Agreement* ($A_{Pi}$) is the party equivalent of *Perceived Issue Agreement*: It is 1 if respondent $i$ identifies himself as the same party as he perceives the representative to be (i.e., Republican–Republican or Democrat–Democrat). It is $-1$ if he thinks the representative is of the opposite party, and 0 if either the respondent is not sure of the party of his representative or he identifies as an Independent.

*Actual Issue Agreement* ($Z_{Ii}$) is the counterpart to perceived issue agreement, with the respondent's belief about the vote replaced by the representative's actual roll-call vote. In other words, $Z_{Iij} \neq A_{Iij}$ indicates respondent $i$'s perception of his representative's vote on issue $j$ is incorrect. *Actual Party Agreement* ($Z_{Pi}$) is the counterpart to perceived party agreement, with the respondents' perceptions of their representative's party replaced with their representative's actual party affiliation.

We measure evaluations of the representative $Y_i$ by approval and vote. *Approval* of the current representative ranges from "Strongly Disapprove" to "Strongly Approve," rescaled from $-1$ to $+1$ with equal intervals. The CCES measures *Vote Choice* in general election years by asking who the respondents intend to vote for, presenting candidates' name and party. The variable is 1 if the respondent intends to vote for the incumbent, $-1$ if voting for the challenger, and 0 if he does not plan to vote or is unsure. If an incumbent is not running for reelection, the observation is dropped from this analysis.

The control variable *Actual Ideological Agreement* is measured as the proximity between the representative's DW-NOMINATE score and the voter's ideological self-placement. This is an admittedly coarse measure because we do not jointly scale constituents and candidates. But Broockman (2016) highlights the challenges of scaling public opinion. And more importantly, our main task is to test models of accountability, in which voters evaluate incumbent legislators *ex post* (Fearon 1999)—not to explore whether candidate choice is a function of spatial distance. Further details on operationalization are left to the Supporting Information (p. 1).

**TABLE 1 Descriptive Statistics**

**(a) Predictor variables and their instruments**

| | Mean | Standard deviation | Observations |
|---|---|---|---|
| Perceived Issue Agreement | 0.09 | 0.59 | 51,115 |
| Perceived Party Agreement | 0.11 | 0.71 | 49,195 |
| Actual Issue Agreement | 0.10 | 0.66 | 51,172 |
| Actual Party Agreement | 0.12 | 0.79 | 47,664 |

**(b) Outcome variables**

| | Mean | Standard deviation | Observations |
|---|---|---|---|
| Job Approval | 0.07 | 0.71 | 45,600 |
| Vote for Incumbent | 0.23 | 0.84 | 25,984 |

*Note*: All variables range from $-1$ to 1.

Table 1 presents summary statistics for each of the variables. To substantively interpret these values, note that the mean of a variable that is coded $\{1, 0, -1\}$ is the difference between the percent of the sample coded 1 and the percent coded $-1$. For example, the mean value of *Vote Choice* is the percent of the sample who would vote for the representative minus the percent who would vote for the challenger. In other words, on average, incumbents enjoy a 23-point vote margin from their constituents. Panel (a) shows that representatives have a 10-point net agreement on all four measures. In other words, House representatives are 10 percentage points more likely to vote on the same side of their constituent, in terms of roll-call vote and party, in perception as well as in reality.

## Effects of Actual Roll-Call Votes on Evaluations

Our model of accountability (Figure 1) is a three-component process, tracing the votes representatives make to constituent's perceptions and evaluations. We examine the reduced-form effect of actual agreement on downstream evaluations first, because the finding is a

**TABLE 2 Actual Agreement and Evaluations (Reduced Form)**

| | Outcome: Approval | | | | |
|---|---|---|---|---|---|
| | All years | Bush 2nd (2006–2008) | Obama 1st (2009–2012) | Obama 2nd (2013–2016) | Trump (2017–2018) |
| Actual Issue Agreement | 0.23 | 0.33 | 0.29 | 0.06 | 0.23 |
| | (0.01) | (0.01) | (0.01) | (0.01) | (0.03) |
| Actual Party Agreement | 0.22 | 0.11 | 0.23 | 0.23 | 0.22 |
| | (0.01) | (0.01) | (0.01) | (0.01) | (0.02) |
| Average of outcome | 0.07 | 0.04 | 0.08 | 0.05 | 0.06 |
| *R*-squared | 0.39 | 0.36 | 0.47 | 0.32 | 0.46 |
| Clusters | 847 | 482 | 529 | 498 | 434 |
| Observations | 42,559 | 10,010 | 23,675 | 6,286 | 2,588 |
| | **Outcome: Vote choice** | | | | |
| | All even years | Bush 2nd (2006, 2008) | Obama 1st (2010, 2012) | Obama 2nd (2014, 2016) | Trump (2018) |
| Actual Issue Agreement | 0.20 | 0.28 | 0.24 | 0.08 | 0.30 |
| | (0.01) | (0.04) | (0.01) | (0.02) | (0.04) |
| Actual Party Agreement | 0.35 | 0.35 | 0.30 | 0.49 | 0.45 |
| | (0.01) | (0.03) | (0.01) | (0.02) | (0.04) |
| Average of outcome | 0.23 | 0.24 | 0.24 | 0.21 | 0.19 |
| *R*-squared | 0.48 | 0.63 | 0.48 | 0.53 | 0.67 |
| Clusters | 786 | 411 | 484 | 445 | 368 |
| Observations | 24,051 | 1,801 | 16,946 | 3,749 | 1,555 |

*Note*: Estimate of the reduced form models in Equation (1). Standard errors clustered by representative in parenthesis, and control variables not shown.

familiar one to the literature and it sets the stage for the two remaining psychological mechanisms.

*Actual Issue Agreement* appears to have a strong, consistent effect on people's evaluations of their representatives. Table 2 presents estimates from the regression in Equation (1). These regressions include the controls previously discussed—representative fixed effects, ideological agreement, ideological distance from the challenger, and demographic variables. Standard errors are clustered at the representative level to account for correlated errors. The coefficient on *Actual Issue Agreement* in predicting approval is 0.23. That means a person whose own preferences on issues are in complete agreement with their representatives' roll-call votes on those issues is 11 percentage points more likely to approve of the representative than another constituent of the same representative, of the same party, and of the same ideology, but agrees with only half of the issues. The coefficient on *Actual Issue Agreement* in predicting vote choice is 0.20. That means the incumbent's share of the votes is 10 points higher among constituents who are in complete agreement on issues than, again, another constituent with the same observable characteristics but is in agreement with only half of the issues. These are substantively large effect sizes, given the strict match on covariates we are enforcing with the controls in the regression.

*Actual Party Agreement* is also, unsurprisingly, associated with strong evaluations. The effect of *Actual Party Agreement* is 0.22 on approval and 0.35 on vote choice. Party and issues have comparable effects on approval, but party agreement controlling for issue agreement has a larger effect on vote choice.

The threat to inference remaining after Table 2 is the lingering suspicion of omitted variable bias. We take the sensitivity analysis approach by Cinelli and Hazlett (2020) to address this risk, asking how strong an unobservable omitted variable would have to be to render the coefficient on *Actual Issue Agreement* null. In the Supporting Information (pp. 19–20), we find that an unobserved confounder would have to be more than twice as strong as copartisanship to explain away our results in the approval regression, and about 1.5 times as strong in

the vote choice regression. Given the predominance of partisanship in vote choice, it is hard to imagine such a variable that is not already in our list of controls.

The conclusions we can draw from these findings are similar to aggregate studies, which measure agreement in terms of roll-call scores and presidential vote, party loyalty, and individual-level studies that measure ideological distance. Where our results part from these studies is that we suggest that specific votes on key issues may move evaluations, even holding party or ideological congruence constant. But this reduced-form effect is incomplete. It is unclear from this quantity alone how or if constituents perceive issue agreement and whether they act on it, which we turn to next.

## Reality and Perception

The first requirement for accountability is accurate perception of actual agreement. Issue voters who are nevertheless misinformed about how their representative stands on those issues lead to what Bartels (2008) called "unenlightened self-interest" (p. 150). Survey researchers have long documented that citizens appear to have thin factual knowledge about Congress (Delli Carpini and Keeter 1997; Fowler and Margolis 2014). Other scholars argue that the electorate reasons, even with incomplete or partial information, to draw fairly accurate inferences about politics, such as the positions their representatives take (Lupia and McCubbins 1998). Our measures of perceived and actual agreement are well-suited to adjudicate these claims.

### Correct Perceptions

Twice as many respondents in the CCES data hold correct beliefs about how their representatives voted as those who hold incorrect beliefs. For the average issue, 43% of voters perceive correctly, 42% are not sure, and 19% have an incorrect perception. Seventy-three percent can name the correct party affiliation, 21% are not sure, and 6% are incorrect. The Supporting Information (p. 5) presents these numbers for each issue and for party.

Further, constituents who are more educated, express higher interest in the news, and are from higher income are significantly more likely to have correct perceptions. In addition, constituents of extremist representatives are also more likely to have correct perceptions of how their member voted compared to constituents with similar individual demographics but in a district represented by a more moderate representative (also see Dancey and Sheagley 2016). These patterns are borne out by a Heckman selection model, which estimates first the likelihood of a constituent to make a guess, and second the likelihood that the guess is correct conditional on making a response (Supporting Information, pp. 10–11). All together, the factors that shape correct perceptions for issues fall squarely into theories of communication that find the receptivity of the respondent (in this case, the constituent) and the strength of the signaler (in this case, the representative) to be important determinants of perceptions.

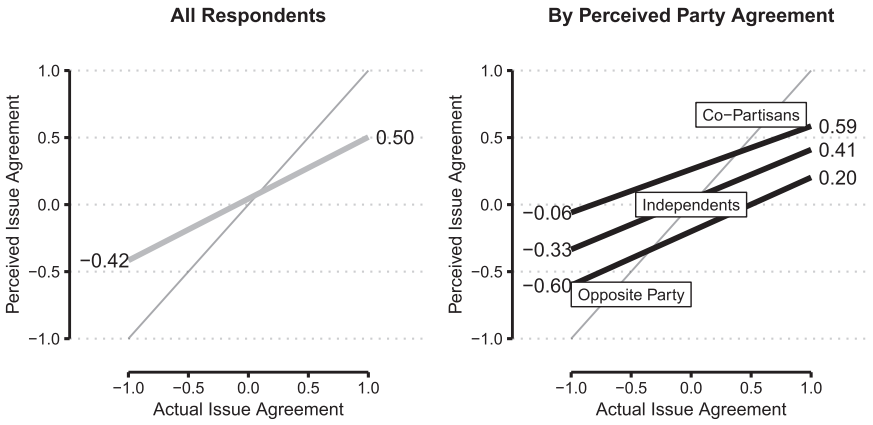
One possible concern with our measurement of perception is that respondents might have looked up the answers while taking the online survey. In the Supporting Information (pp. 11–13), we provide evidence showing this is unlikely. The CCES tracks how many seconds each respondent spent on each page. Respondents take about as long answering the perception question as they do answering other questions of the same length and format, and respondents who take longer to answer are actually less likely to provide correct answers.

### Actual Agreement and Perceived Agreement

Modeling the relationship between perceived and actual issue agreement illustrates the coexistence of partisan bias and accurate learning more clearly. The left panel of Figure 2 shows that constituents who in fact disagree with their representative on all the issues asked in the CCES also perceive to be in disagreement: an average of $-0.42$ on a $-1$ to 1 scale. Those who agree with their member on all bills perceive an agreement of 0.50. The resulting slope of 0.46 reflects how perception does track reality on average, but not perfectly (which would result in a slope of 1). The attenuation is explained by both incorrect perceptions and "not sure" responses.

This relationship between reality and perception might be spurious, however, driven by the composition of partisan loyalists who are oblivious to actual issues. The right panel controls for the perceived party agreement and lends support to both stories: Perception is both biased in terms of party, but it is also responsive to actual agreement. Among perceived copartisans, those who are in-truth in complete disagreement ($-1$) on the issues perceive an agreement of $-0.06$ on average. If they were perfect perceivers, the score would be at $-1$. Similarly, perceived opposite partisans who are in-truth in complete agreement with the member on the specific issues have a net perceived agreement of only 0.20. The slopes of all three groups are attenuated toward zero but

**FIGURE 2 Does Perceived Agreement Reflect Actual Agreement?**




*Note:* Lines show OLS best fit lines and numbers show predicted value at endpoints. The 45 degree line indicates perfect correspondence.

are still positive and significantly distinguishable from a flat line. It does not appear that the correlation is completely driven by partisan biases.

The first stage of Equation (2) in Table 3 further confirms that it is the facts of the roll-call vote, rather than party heuristics, that predominantly shape the perception of votes. The coefficient on *Actual Issue Agreement* in predicting *Perceived Issue Agreement* is 0.34, meaning that if a constituent supports a bill and is represented by a member who voted for that bill, he is 34 percentage points more likely to believe they are in agreement with the legislator on that bill—compared to if the representative had voted against the bill. Sensitivity analyses in the Supporting Information (pp. 19–20) show how unlikely it is that this relationship is confounded by unobserved variables. Even if there was an omitted confounder that is as strong as *Actual Party Agreement*, the coefficient estimate would only drop to 0.30.

**TABLE 3 Actual Agreement and Perceived Agreement (First Stage)**

| | Outcome: Perceived Issue Agreement | Outcome: Perceived Party Agreement |
|---|---|---|
| Actual Issue Agreement | 0.34 | 0.05 |
| | (0.01) | (0.01) |
| Actual Party Agreement | 0.10 | 0.63 |
| | (0.01) | (0.01) |
| Actual Ideological Agreement | 0.30 | 0.14 |
| | (0.01) | (0.01) |
| Average of outcome | 0.09 | 0.12 |
| Std. dev. of outcome | 0.59 | 0.71 |
| *R*-squared | 0.36 | 0.60 |
| *F*-statistic for weak instruments | 2,116 | 6,389 |
| Clusters | 848 | 848 |
| Observations | 46,574 | 46,585 |

*Note*: Each column is an OLS regression. Controls, representative fixed effects, and year fixed effects not shown. Clustered standard errors by representative.

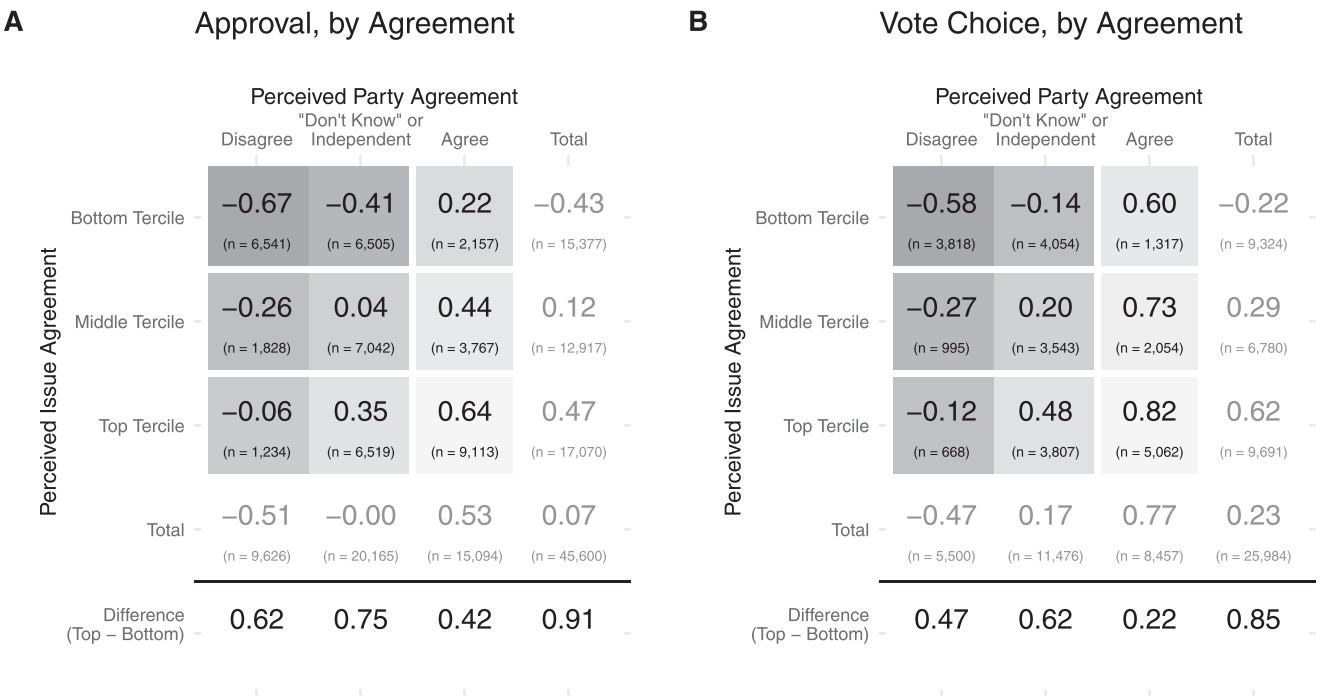
Party does appear to serve as a heuristic in shaping beliefs about representative's legislative decisions. However, the coefficient sizes suggest that it is of secondary importance: The effect of *Actual Issue Agreement* is three times larger than the effect of *Actual Party Agreement* on how voters perceive issue agreement. In addition, a parallel pattern emerges with constituent's perceptions of party agreement. In the second column of Table 3, the coefficient on *Actual Party Agreement* in predicting *Perceived Party Agreement* is 0.63.

There is a symmetry, then, between the two regressions in Table 3: Constituents learn about issues more from issues than from party, and learn about party more from party than from issues. We provide estimates by issue and find some variation over time, which we in part attribute to the Congressional agenda (Supporting Information, pp. 9, 15–16).

On the whole, the public's perceptions are rooted in the reality of the decisions representatives make.[8] There is evidence of uncertainty and copartisan misperception, but Table 3 shows these to be of second order. The typical person's understanding of how their representative voted

[8]In the context of our instrumental variables strategy, this means that our instruments are strong. The *F*-statistic is over 2,000 instrumenting for perceived issue agreement.

**FIGURE 3 Differences in Evaluation by Perceived Agreement**




*Note:* Cells show averages of approval values $\{-1, -0.5, 0, 0.5, 1\}$ for panel A and averages of incumbent vote values $\{-1, 0, 1\}$ for panel B. The middle column includes respondents who (i) were unsure of their representative's party as well as (ii) those who self identified as an Independent and therefore neither disagree nor agree with the representative's party affiliation in our coding.

on key legislation is, on balance, a fairly accurate reflection of their legislator's actual behavior. The question we turn to next is how much constituents use that information to hold legislators accountable.

## Perception and Evaluation

The reduced form indicates that there are downstream consequences to a representative's votes, and the first stage indicates that constituent's perceptions about those positions are noisy and biased, but on average track actual positions. This sets the stage for the third and final component of electoral accountability in Figure 1: How constituents translate perceived agreement to the evaluations of their representative. We take three approaches to isolate this causal quantity.

### Difference in Conditional Means

We first sketch out the relationship with conditional difference in means. Figure 3 displays weighted averages of approval and vote choice by subsets of perceived agreement. Recall that our outcome variables range from $-1$ to 1, so the average of the vote choice variable is equivalent to the electoral margin of the incumbent. The average of approval is similarly interpreted as the net approval, percent approval minus percent disapproval. Consider first the differences in row averages in panel (A). Constituents who perceive low levels of issue agreement with their representatives, displayed in the top row, express a net approval rating of $-0.43$, but those who see themselves in agreement with their representatives' roll-call votes have a net approval of 0.47.

These differences due to issue agreement are not explained away by partisan agreement. The row below the solid line of Figure 3 shows the difference in outcomes between the top and bottom terciles of issue agreement, within each level of party agreement. Take incumbent vote choice in panel (B). Among people who believe they are the *same* party as the incumbent (the third column), the difference between high and low issue agreement is 22 points in vote margin. Among people who believe they are the *opposite* party as their representatives, the same difference is 47 points in vote margin. In addition, among *independents* and those who did not know the party of their representatives, the improvement is 62 points.

We hold constant more characteristics of the representative, the constituent, the issue at stake, and the congressional district by estimating Equation (3). These

**TABLE 4 Perceived Agreement and Evaluations (Instrumental Variables)**

| | Outcome: Approval | | | | |
|---|---|---|---|---|---|
| | All years | Bush 2nd (2006–2008) | Obama 1st (2009–2012) | Obama 2nd (2013–2016) | Trump (2017–2018) |
| Perceived Issue Agreement | 0.64 | 0.70 | 0.73 | 0.36 | 0.59 |
| | (0.02) | (0.03) | (0.02) | (0.08) | (0.06) |
| Perceived Party Agreement | 0.22 | 0.08 | 0.23 | 0.34 | 0.28 |
| | (0.01) | (0.02) | (0.01) | (0.02) | (0.03) |
| Average of outcome | 0.07 | 0.04 | 0.08 | 0.05 | 0.06 |
| Clusters | 847 | 482 | 529 | 498 | 434 |
| Observations | 42,417 | 9,999 | 23,625 | 6,205 | 2,588 |
| | **Outcome: Vote choice** | | | | |
| | All Even Years | Bush 2nd (2006, 2008) | Obama 1st (2010, 2012) | Obama 2nd (2014, 2016) | Trump (2018) |
| Perceived Issue Agreement | 0.58 | 0.96 | 0.60 | 0.48 | 0.78 |
| | (0.03) | (0.10) | (0.04) | (0.15) | (0.13) |
| Perceived Party Agreement | 0.43 | 0.43 | 0.33 | 0.87 | 0.69 |
| | (0.02) | (0.05) | (0.02) | (0.06) | (0.07) |
| Average of outcome | 0.23 | 0.24 | 0.24 | 0.22 | 0.19 |
| Clusters | 786 | 411 | 484 | 445 | 368 |
| Observations | 23,949 | 1,799 | 16,915 | 3,680 | 1,555 |

*Note*: Each column is an instrumental variables regression. Controls, representative fixed effects, and year fixed effects not shown. Clustered standard errors by representative.

estimates show that a one-unit increase in perceived issue agreement on a scale of $-1$ to $+1$ is associated with an increase in the respondent's net job approval of 19 percentage points, holding constant other correlates of issue agreement such as perceived party agreement and perceived ideological agreement (Supporting Information, p. 7). For vote choice, an improvement in perceived issue agreement from the middle of the scale to complete issue agreement is associated with an increase in the respondent's likelihood of voting for that incumbent by about 11 percentage points. These indicate that accountability does exist, smaller than the simple difference-in-means suggested by Figure 3 but on the same order of magnitude.

## Instrumental Variables Estimates

The difference-in-means approach may overstate the causal effect of perceived issue and party agreement on evaluations if there are unobserved confounders that are correlated with perceived agreement and correlated with evaluations. To correct for these statistical biases, we implemented our instrumental variables (IV) estimator shown in Figure 1 and Equations (2) and (3).

We see substantively large direct effects of issues on vote choice in every Congress studied, with some variation over time. Table 4 summarizes our key results (see also Supporting Information, p. 9, for issue-specific estimates). We start with the first column that uses the data from all years. The IV coefficient on *Perceived Issue Agreement* predicting approval is 0.64 and the coefficient on predicting vote choice is 0.58. Because the standard deviation of *Perceived Issue Agreement* is about 0.60 (Table 1), this indicates that a one standard deviation improvement in a constituent's *Perceived Issue Agreement* improves net approval or the vote margin of the incumbent by about 35 percentage points.

We further explore the possibility that the degree of issue voting varies across types of people, types of issues, and the context of specific roll-call votes. Details of those analyses are in the Supporting Information, pp. 14–18. First, the estimates may vary with the salience of the issue to the public. All of these issues were salient in the sense of being key votes in Congress that made it to a floor vote. Nonetheless, some of the issues, especially health

care, were routinely at the top of the legislative agenda for both parties. We divided the issues as highly salient and less salient, and found that the estimates were quite similar.

Second, we examined the heterogeneity of effects due to strategic roll-call voting. It may be the case that voters reward and punish legislators more sharply when the legislators' votes are pivotal to the passage of legislation (Snyder and Groseclose 2000). We found no evidence that voters responded more to their legislator being pivotal on close votes than to other contexts. Nor did we find evidence that abstention insulates legislators by creating ambiguity (Arnold 1990).

Finally, we examined whether the effect of issues only exists among high-interest voters. We divided the sample by level of political interest (following Bartels 1996) and found no consistent differences in issue voting: The coefficients on perceived agreement on issues were similar for high, medium, and low levels of interest. Higher interest voters were, however, more ideological and less partisan than low-interest voters. This pattern suggests an important way in which issue voting is distinct from ideological and partisan voting.

The interpretation of the IV estimate also deserves more nuance. There are two ways to interpret the IV coefficient—one from an omitted variable perspective and the other as a local average treatment effect. In the former, the IV coefficient represents the average effect of the treatment variable (in this case, perceived issue agreement) after controlling away attenuation biases due to measurement error and unobserved confounding. In other words, the IV estimate is an improved version of the ordinary least squares (OLS) estimate.

If the effect among respondents who change their perceived agreement in response to actual agreement is different than the effect among other groups, however, the IV coefficient identifies the average treatment effect among the former group, also known as compliers.[9] On the one hand, this means that the IV estimates are less generalizable to an average effect. Fortunately, the nature of our instrument means that the compliers are a theoretically important group in their own right. These are constituents who, by definition, respond to changes in reality. Our large IV estimates interpreted as a local average treatment effect therefore suggest that this perceptive subset of the electorate enforces a strong degree of accountability.

[9]These do not include constituents who, upon an increase in actual agreement *decrease* their perceived agreement, or vice versa. Such individuals would be defiers in the IV context and must be assumed away.

We do not deny the existence of projection. For example, one could imagine that prior approval (independent of actual issue agreement) affects perceived agreement—the reverse of our causal claim. If this is correlated with current approval, it would induce a correlation between perceived agreement and current approval. Still, what our IV results show is that another causal pathway, perhaps together with some partisan projection, exists: one in which actual agreement flows through perceived agreement such that constituents hold their representatives accountable.

## Experimental Evidence of the Causal Connection

Our third approach to measuring the effect of perceived issue agreement on evaluation addresses the concern that the instrumental variables estimates may suffer from a violation in the exogeneity condition. We conducted two randomized experiments, one during the Democratic Congress under President Obama and another during the Republican Congress under President Trump. Two of our CCES modules contained experiments that selectively provided respondents with information about their representatives.

The 2009 study ($n = 5{,}700$) provided *correct* information to randomly chosen subsets of respondents and no information to others. One type of information regarded roll-call votes. Respondents were told how their House representatives actually voted on two randomly chosen votes. The possible votes were the State Children's Health Insurance Program, the American Reinvestment and Recovery Act, the American Clean Energy and Security Act, and the Patient Protection and Affordable Care Act. An additional type of information was party: Half of the sample was randomly chosen to be told the correct party affiliation of their representative and half were told no party information.

The 2018 study ($n = 2{,}000$) provided respondents with *randomly* determined information about four roll-call votes. The study randomly assigned a Yes or No vote to the representative on four votes separately, regardless of whether that information was correct or incorrect. The bills were the 2018 Bipartisan Budget Act, the Tax Cuts and Jobs Act of 2017, the Mobilizing Against Sanctuary Cities Act, and the American Health Care Act, which partially repealed the ACA. There was no party treatment in this study. To limit the risks associated with deception, all participants were debriefed shortly after and informed that the information they were provided was randomly

**TABLE 5 Experimental Effects of Issue Agreement**

| | 2009 Study | | | |
|---|---|---|---|---|
| | | Subsets by Knowledge of Roll-call | | |
| | All | No prior | Some wrong | All correct |
| Vote information treatment (in agreement) | 0.10 | 0.08 | 0.11 | 0.01 |
| | (0.01) | (0.02) | (0.02) | (0.03) |
| Party information treatment (in agreement) | 0.07 | 0.13 | 0.06 | 0.03 |
| | (0.01) | (0.02) | (0.02) | (0.02) |
| Average outcome in control | 0.04 | 0.00 | 0.08 | 0.02 |
| Proportion high news interest | 0.57 | 0.27 | 0.59 | 0.79 |
| *R*-squared | 0.56 | 0.25 | 0.48 | 0.76 |
| Observations | 4,863 | 1,409 | 1,626 | 1,828 |
| | **2018 Study** | | | |
| | | Subsets by Knowledge of Roll-call | | |
| | All | No prior | Some wrong | All correct |
| Vote information treatment (in agreement) | 0.09 | 0.08 | 0.08 | 0.12 |
| | (0.02) | (0.06) | (0.03) | (0.07) |
| Average outcome in control | 0.03 | 0.01 | 0.10 | −0.23 |
| Proportion high news interest | 0.51 | 0.18 | 0.52 | 0.73 |
| *R*-squared | 0.62 | 0.36 | 0.59 | 0.78 |
| Observations | 1,947 | 284 | 1,348 | 315 |

*Note*: Each column is an OLS regression where the outcome is approval. Pretreatment controls not shown. Robust standard errors in parentheses.

chosen and was not a reflection of how their member of Congress actually voted on those issues.

To make estimates comparable with our observational analyses, we coded the issue treatment variables as 1 if the respondent had a preference that in fact agreed with the experimentally provided information about the representative's roll-call vote, −1 if the respondent in fact disagreed with the provided information, and 0 if no information on that issue was provided. We coded the party treatment similarly, with 1 indicating treatment providing copartisan information and −1 indicating treatment providing out-partisan information. We then computed the sum of the agreement measures for the different roll-call votes divided by the number of treated roll-call votes. Because our treatment variable includes nonrandomized preferences, we control for prereatment *Perceived Issue Agreement*, pretreatment Perceived Party Agreement, the baseline measure of approval, and demographic variables when estimating treatment effects.

These experiments are meant to confirm the causal inferences from the main IV estimation strategy. Experiments have the advantage that, by design, whether the respondent received the information in the treatments is independent of any other factor. Experiments, of course, have their limitations. For example, we do not change how legislators actually voted, but only offer information to respondents. These are messages that respondents may accept or reject. The 2018 experiment may also be limited in external validity because it presents off-equilibrium signals, counterfactual votes that representatives chose not to cast. Such counterfactuals strength inferences about causality, but may weaken effects.

Both the issue and party treatments moved approval by 7 to 10 percentage points. The first column in each panel of Table 5 presents the treatment effects for all respondents. The coefficients on the issue treatments are 0.10 in 2009 and 0.09 in 2018, and both are statistically distinguishable from 0 ($p < 0.01$). The coefficient on the party information treatment is of similar magnitude.

The effect of additional information should depend on voter's prior beliefs. We therefore divided the sample into subgroups of prior levels of completeness and correctness of beliefs. One subset did not have any belief about how their representatives voted on any votes; the second had incomplete and incorrect prior beliefs on some votes; and a third had correct prior beliefs on all votes. In the 2009 experiment, the information provided is correct so only the last group would not have received new information. In the 2018 experiment, the information treatment is orthogonal to prior beliefs so all groups are equally treated.

Our results are consistent with Bayesian updating. Respondents who had correct prior beliefs exhibited no statistically significant increase in approval in response to confirmatory information in the 2009 experiment. We also took the subset of 2018 respondents who had correct prior beliefs and estimated separate effects among those assigned correct information and those assigned incorrect information. The effect was concentrated among the latter (Supporting Information, p. 8).

Three implications of these experiments deserve emphasis. First, the experiments reaffirm the findings of issue accountability from the observational and instrumental variables regression. As Bullock (2011) found with a similar design to our 2009 experiment, people use information about roll-call votes, when it is available, to evaluate their legislators. Second, people value the roll-call vote information and party labels about equally in updating their evaluations of their representatives. Third, the subgroup comparisons confirm that our findings reflect real beliefs instead of random guessing.

## Reconciling Individual versus Aggregate Effects

Our estimates indicate that the effect of a one standard deviation increase in perceived issue agreement is approximately 35 percentage points on an individual's likelihood of voting to reelect the incumbent. That is in line with existing estimates from surveys, but much larger than estimates using aggregate election data. Canes-Wrone, Brady, and Cogan (2002) estimate that a one standard deviation change in the roll-call score of the legislator to change their vote share by 1 to 3 percentage points or less (see also Bonica and Cox 2018). Tausanovitch and Warshaw (2018) reasonably ask why the individual and aggregate estimates in this literature differ.

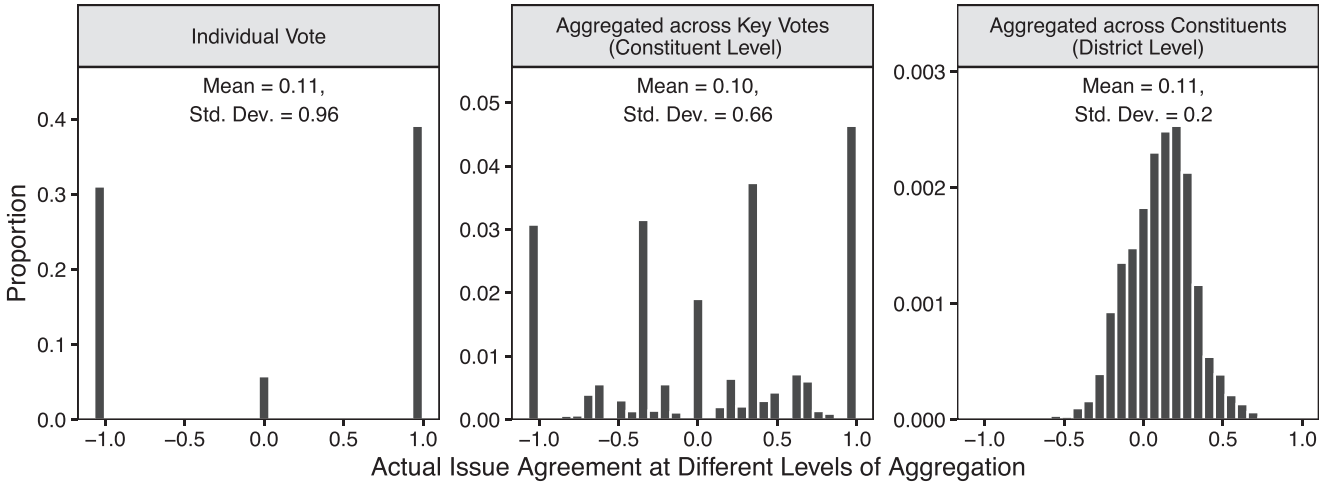
The answer lies in aggregation. It is well known that analyses of correlations among aggregates suffer from the ecological fallacy, and use of proxy variables, such as use of presidential vote to measure constituents' preferences, introduces measurement error. Even setting aside these measurement problems, there are two first-order consequences of aggregation.

First, aggregation cancels out individual-level effects of opposing signs. If 100% of constituents in a district support a bill, then, our estimates (Table 2) would suggest that a legislator can expect to see her vote share increase by 20 percentage points if she votes for the bill instead of voting against it. But constituencies are never completely for or against a bill. On the typical CCES issue, a congressional district's constituents are split 60–40. In that case, the representative will increase her standing among the 60% of people who support the bill by 20 percentage points, but will simultaneously lose 40% of her constituents by the same magnitude. The net gain is only 4 percentage points in vote margin. The average actual issue agreement in our data at the individual level is 0.10 (Table 1), which translates into 55% in agreement and 45% in disagreement. Therefore, even assuming that the effect of a roll-call vote on an individual constituent's vote choice is 20 points, its contribution to vote share is only 2 points.

The scale of comparison is also smaller at the aggregate level. Typically, studies report the effect of a one standard deviation unit change in agreement on vote or approval. But the variation in the mean of a variable is necessarily smaller than the variation in the variable itself. Figure 4 illustrates this using the CCES, showing the distributions of *Actual Issue Agreement* measured at three levels. The standard deviation of *Actual Issue Agreement* at the individual level is 0.66, whereas the standard deviation of its district-level counterpart is only 0.21. The two distributions have the same mean but that similarity masks stark differences in scale. Hence, the effect of a one standard deviation change in issue agreement at the individual level is 13 percentage points (i.e., $0.66 \times 0.20$), but the effect of a one-standard deviation change in issue agreement at the district level is only 4 percentage points in vote margin (i.e., $0.21 \times 0.20$), or a 2 percentage point change in vote share.

The aggregate effects of issue congruence implied by the individual level estimates are on the same order as those estimated by researchers using aggregate data, even setting aside potential aggregation and measurement biases with those analyses. Put another way, small aggregate differences can still reflect strong issue voting at the individual level.

**FIGURE 4 Consequences of Aggregation for Representation**




*Note:* Panels show the distribution of Actual Issue Agreement at the individual level (left), the constituent level (center), and the district level (right).

## Conclusion

This study has sought to advance the longstanding debate on electoral accountability by bringing extensive data on constituent knowledge and issue voting, combining multiple estimation strategies, and providing explanations to reconcile seemingly inconsistent findings. The classical theory of representation posits that constituents pay attention to and care about the policy decisions their representatives make. Key (1966), examining party switching between 1936 and 1960, argued that voters are "moved by concern about central and relevant questions of public policy" (p. 8). Many others have openly challenged the tenets of the classical theory. Miller and Stokes (1963), examining the 1958 National Election Study, reached the conclusion that "given the limited information the average voter carries to the polls, the public might be thought incompetent to perform any task of appraisal" (p. 53).

Twelve years of data across various political contexts demonstrate that the American electorate approximates the classical ideal in two essential respects. First, although the public is somewhat biased toward copartisan representatives, on the whole it sees Congress correctly. Second, constituents hold their representatives accountable for their votes on key legislative decisions. The typical constituent expresses considerably higher support for their congressional representatives when she or he sees that the representative has voted the way the constituent would have. Over 20 years ago Lupia and McCubbins (1998) asked whether "citizens can know what they need to know." Our findings on electoral accountability for key legislative decisions answer that question in the affirmative.

In the present political context, these findings are particularly striking. Against a background of party polarization in Congress, one might expect that the electorate has abandoned their own preferences on issues and, instead, blindly taken sides with one party. The evidence mustered here shows that voters can punish representatives with whom they disagree on legislative decisions, even if the representative is a copartisan. The effects of issues are approximately as large as the effects of party on constituents' evaluations. This contrasts starkly with theories that begin with the claim that most voters are largely ignorant about legislative decisions and thus conclude that constituents must rely on elites and party labels for representation (Bawn et al. 2012). We are not arguing that elite capture does not occur. Rather, we suggest that theories of representational failure cannot rely on the premise that individual voters are unable to hold legislators accountable on issues.

Our findings also help reconcile two observations. On the one hand, individual constituents respond strongly to their legislators' roll-call votes. But on the other hand, aggregate vote shares are only modestly correlated with legislators' roll-call voting records. This is a result of aggregation. Many legislative districts are fairly evenly split on key legislation. A legislator may vote with the majority of her district and get the support of 55% of her constituents, but lose the support of the remaining 45%. Those with whom the legislator sides care deeply about the issue, as do those opposed to the legislator's vote. But, in the aggregate, the net effect is modest

because much of the support and opposition for the bill cancel out. Aggregate correlations should not be taken as measures of the true degree to which individuals care about or vote on the issues. By the same token, in extremely competitive districts, representatives have a difficult time satisfying the majority of the voters back home.

In the end, were Miller and Stokes wrong? No, they simply did not have a powerful enough microscope. Advances in survey methodology and technology have made it possible to measure with greater accuracy and statistical power how individual voters see and respond to their representatives' policy decisions. The portrait that emerges is not an inattentive and uncaring electorate; nor is it a hyperattentive, hyperrational electorate. Rather, the electorate on the whole is sufficiently attentive and sufficiently motivated by public policy to exert electoral control, albeit imperfectly, as envisioned by the classical theory of representation.

## References


Ahler, Douglas, and David E. Broockman. 2018. "The Delegate Paradox: Why Polarized Politicians Can Represent Citizens Best." *Journal of Politics* 80(4): 1117–33.

Ansolabehere, Stephen, and Philip Edward Jones. 2010. "Constituents' Responses to Congressional Roll-Call Voting." *American Journal of Political Science* 54(3): 583–97.

Arnold, R. Douglas. 1990. *The Logic of Congressional Action*. New Haven, CT: Yale University Press.

Bafumi, Joseph, and Michael C. Herron. 2010. "Leapfrog Representation and Extremism: A Study of American Voters and Their Members in Congress." *American Political Science Review* 104(3): 519–42.

Bartels, Larry M. 1996. "Uninformed Votes: Information Effects in Presidential Elections." *American Journal of Political Science* 37(2): 1051–68.

Bartels, Larry M. 2008. *Unequal Democracy: The Political Economy of the New Gilded Age*. Princeton, NJ: Princeton University Press.

Bawn, Kathleen, Martin Cohen, David Karol, Seth Masket, Hans Noel, and John Zaller. 2012. "A Theory of Political Parties: Groups, Policy Demands and Nominations in American Politics." *Perspectives on Politics* 10(3): 571–97.

Bonica, Adam, and Gary W. Cox. 2018. "Ideological Extremists in the U.S. Congress: Out of Step but Still in Office." *Quarterly Journal of Political Science* 13: 207–36.

Broockman, David E. 2016. "Approaches to Studying Policy Representation." *Legislative Studies Quarterly* 41(1): 181–215.

Broockman, David E., and Daniel M. Butler. 2017. "The Causal Effects of Elite Position-Taking on Voter Attitudes: Field Experiments with Elite Communication." *American Journal of Political Science* 61(1): 208–21.

Bullock, John G. 2011. "Elite Influence on Public Opinion in an Informed Electorate." *American Political Science Review* 105(3): 496–515.

Canes-Wrone, Brandice, David W. Brady, and John F. Cogan. 2002. "Out of Step, Out of Office: Electoral Accountability and House Members' Voting." *American Political Science Review* 96(1): 127–40.

Carson, Jamie L., Gregory Koger, Matthew J. Lebo, and Everett Young. 2010. "The Electoral Costs of Party Loyalty in Congress." *American Journal of Political Science* 54(3): 598–616.

Cinelli, Carlos, and Chad Hazlett. 2020. "Making Sense of Sensitivity: Extending Omitted Variable Bias." *Journal of the Royal Statistical Society. Series B* 82(1): 39–67.

Clinton, Joshua D. 2006. "Representation in Congress: Constituents and Roll Calls in the 106th House." *Journal of Politics* 68(2): 397–409.

Dancey, Logan, and Geoffrey Sheagley. 2016. "Inferences Made Easy: Partisan Voting in Congress, Voter Awareness, and Senator Approval." *American Politics Research* 44(5): 844–74.

Delli Carpini, Michael X., and Scott Keeter. 1997. *What Americans Know about Politics and Why It Matters*. New Haven, CT: Yale University Press.

Erikson, Robert S., Michael B. Mackuen, and James A. Stimson. 2002. *The Macro Polity*. Cambridge: Cambridge University Press.

Eskridge, William N. 1987. "Dynamic Statutory Interpretation." *University of Pennsylvania Law Review* 135: 1479–555.

Fearon, James D. 1999. "Electoral Accountability and the Control of Politicians: Selecting Good Types versus Sanctioning Poor Performance." In *Democracy, Accountability, and Representation*, eds. Adam Przeworski, Susan Stokes, and Bernard Manin. Cambridge: Cambridge University Press, 55–97.

Fiorina, Morris P. 1974. *Representatives, Roll Calls, and Constituencies*. Lexington, MA: Lexington Books.

Fowler, Anthony. 2020. "Partisan Intoxication or Policy Voting?." *Quarterly Journal of Political Science* 15: 141–79.

Fowler, Anthony, and Michele Margolis. 2014. "The Political Consequences of Uninformed Voters." *Electoral Studies* 34: 100–10.

Gilens, Martin. 2001. "Political Ignorance and Collective Policy Preferences." *American Political Science Review* 95(2): 379–96.

Guisinger, Alexandra. 2009. "Determining Trade Policy: Do Voters Hold Politicians Accountable?." *International Organization* 63(3): 533–57.

Hill, Seth J., and Gregory A. Huber. 2019. "On The Meaning of Survey Reports of Roll Call Votes Not Cast in a Legislature." *American Journal of Political Science* 63(3): 611–25.

Jessee, Stephen A. 2009. "Spatial Voting in the 2004 Presidential Election." *American Political Science Review* 103(1): 59–81.

Key, Vladimer O. 1966. *The Responsible Electorate: Rationality in Presidential Voting: 1936-1960*. Cambridge, MA: Harvard University Press.

Lax, Jeffrey R., Justin H. Phillips, and Adam Zelizer. 2019. "The Party or the Purse: Unequal Representation in the US Senate." *American Political Science Review* 113(4): 917–40.

Lenz, Gabriel S. 2012. *Follow the Leader? How Voters Respond to Politicians' Policies and Performance*. Princeton, NJ: Princeton University Press.

Lupia, Arthur, and Matthew D. McCubbins. 1998. *The Democratic Dilemma: Can Citizens Learn What They Need to Know?* Cambridge: Cambridge University Press.

Mayhew, David. 1974. *The Electoral Connection.* New Haven, CT: Yale University Press.

Miller, Warren E., and Donald E. Stokes. 1963. "Constituency Influence in Congress." *American Political Science Review* 57(1): 45–56.

Nyhan, Brendan, Eric McGhee, John Sides, Seth Masket, and Steven Greene. 2012. "One Vote Out of Step? The Effects of Salient Roll Call Votes in the 2010 Election." *American Politics Research* 40(5): 844–79.

Shor, Boris, and Jon C. Rogowski. 2018. "Ideology and the US Congressional Vote." *Political Science Research and Methods* 6(2): 323–41.

Snyder, James M., and Tim Groseclose. 2000. "Estimating Party Influence in Congressional Roll-Call Voting." *American Journal of Political Science* 44(2): 193.

Stephanopoulos, Nicholas O. 2018. "Accountability Claims in Constitutional Law." *Northwestern University Law Review* 112(5): 989–1068.

Tausanovitch, Chris, and Christopher Warshaw. 2018. "Does the Spatial Proximity Between Legislators and Voters Affect Voting Decisions in U. S. House Elections?." *Political Behavior* 40(1): 223–45.

## Supporting Information

Additional supporting information may be found online in the Supporting Information section at the end of the article.

**Appendix A:** Data Sources and Variable Construction
**Appendix B:** Additional Analyses
**Appendix C:** Predictors of Correct Perceptions
**Appendix D:** Heterogeneity Analyses
**Appendix E:** Sensitivity Analysis