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Constituents' Responses to Congressional Roll-Call Voting

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Do citizens hold their representatives accountable for policy decisions, as commonly assumed in theories of legislative politics? Previous research has failed to yield clear evidence on this question for two reasons: measurement error arising from noncomparable indicators of legislators' and constituents' preferences and potential simultaneity between constituents' beliefs about and approval of their representatives. Two new national surveys address the measurement problem directly by asking respondents how they would vote and how they think their representatives voted on key roll-call votes. Using the actual votes, we can, in turn, construct instrumental variables that correct for simultaneity. We find that the American electorate responds strongly to substantive representation. (1) Nearly all respondents have preferences over important bills before Congress. (2) Most constituents hold beliefs about their legislators' roll-call votes that reflect both the legislators' actual behavior and the parties' policy reputations. (3) Constituents use those beliefs to hold their legislators accountable.

Modern democratic theory assumes a strong and direct relationship between representatives and the people they represent. Voters elect politicians to represent their interests and punish politicians who act against their wishes. This electoral connection provides the foundation for the study of congressional behavior and lawmaking (see Fenno 1978; Mayhew 1974; Schickler 2001) and for theories of representative democracy broadly (see, e.g., Przeworski, Stokes, and Manin 1999). Despite the centrality of this view to modern political science, there is surprisingly little direct evidence supporting its main assumptions—namely, that voters have preferences over the issues before the legislature, hold beliefs about their representatives' decisions on those questions, and use that information in deciding whether to support the legislator.

Aggregate election returns and long-term trends in public opinion in the United States are, for the most part, consistent with the notion that elections ensure policy representation. Legislators tend to vote with the general ideological leanings of their districts (Erikson and Wright 1989, 2000). Representatives whose voting records deviate from their district's general partisan orientation end up out of office (Ansolabehere, Snyder, and Stewart 2001; Canes-Wrone, Brady, and Cogan 2002). Jesse (2009), using state-level surveys, finds that Senators whose roll-call voting records were in agreement with a majority of constituents on those same issues were more likely to win. Over the broad sweep of time, the public policies that Congress enacts change in response to shifts in public opinion (Burstein 2002; Erikson, MacKuen, and Stimson 2002; Manza and Cook 2002; Page and Shapiro 1983).

Sixty years of survey research, however, raise serious questions about this view. Miller and Stokes's (1963) pioneering study found that many people could not identify the orientation of their representatives on questions of social welfare, racial integration, and foreign policy. Scholars have since concluded that the American public lacks the knowledge, interest, or even capacity to hold their representatives accountable (Campbell et al. 1960; Converse 1964; Delli-Carpini and Keeter 1996). Rather than choosing candidates on the basis of an informed view of the incumbents' voting records, voters, it is argued, rely primarily on the policy-free "symbols" of party identification (Stokes and Miller 1962). Politicians, it would seem, have little to fear from a public that knows little about what laws their representatives support or oppose in the legislature.

How can these two pictures of congressional representation be squared? One line of argument has suggested

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that the *threat* of discipline is enough to keep legislators in line (Arnold 1990) and that only a small percentage of highly attentive citizens is needed, as the rest will average out (Erikson, MacKuen, and Stimson 2002). The main line of critique, instead, identifies as likely explanations the difficulties of measurement and the potential simultaneity between approval and perceptions of legislators' positions in surveys. Achen (1978), Erikson (1978), Weissberg (1979), and Stone (1979) point to significant measurement errors as the explanation for Miller and Stokes's conclusions. The 1958 ANES asked legislators and constituents their attitudes on three broad issues but did not ask about specific roll-call voting behavior. It did not ask directly how people would have decided key questions before Congress; it did not ascertain how constituents thought their legislators had voted (Weissberg 1978). Recent research finds substantial biases in analyses of issue voting arising from measurement error (e.g., Ansolabehere, Rodden, and Snyder 2008; Goren 2004).

This article uses two national surveys conducted in 2005 and 2006 to test the assumptions behind the traditional theory of policy representation and to measure the effects of congressional roll-call votes on voters' behavior. We address both the measurement and simultaneity problems directly. First, the surveys take on the measurement problems squarely. The questionnaires asked about key roll-call votes that the legislators faced in the year leading up to the survey. They ascertained respondents' own preferences "if the decision were up to them" and their perceptions of how their House members voted on each specific key roll call. These surveys measure constituents' preferences, their beliefs about their legislators, and the legislators' actual decisions on the same scale.[1] Second, we can untangle the simultaneity problem. Because the questions concern specific key votes, we can match the perception to the reality and purge the perceptions of voters' projections and pure guessing. We use actual roll-call votes to construct instrumental variables for constituents' perceptions of their legislators' votes, and thus to measure the direct effects of perceptions of roll-call votes on approval and electoral votes. Although there is some evidence of simultaneity bias, the main problem appears to be measurement error.

We show that constituents are *capable* of and *do* hold their representatives accountable on salient roll-call votes. Survey respondents know how they themselves would vote on major bills if they were members of Congress. A large majority has beliefs about how their U.S. Representatives actually voted on these bills. Those beliefs, in turn, strongly and causally affect constituents' approval of their representatives and tendency to vote for their representatives. The effect is substantively large: all else being equal, the independent effect of policy representation on job approval has about as strong an overall effect as legislators' party on assessments of job approval. We do not, however, argue that citizens are highly attentive to their Representatives' legislative activities. Individuals' beliefs reflect a mix of hard facts learned from the media, campaigns, and other sources and inferences drawn from other facts, especially party labels (see, e.g., Aldrich 1995; Cox and McCubbins 1993). Importantly, though, constituents on average hold accurate beliefs about the roll-call voting of Representatives, which allows the public collectively to hold politicians accountable.

Survey Methodology

Our principal objective in this article is to measure the extent to which constituents' approval of their representatives depends on policy representation. We measure whether people have preferences over actual roll-call votes, whether they have beliefs about how their member of Congress voted on those bills, and whether their perceived policy agreement with their Representatives translates into higher levels of approval or electoral support for the incumbent.

We analyze two national surveys, one conducted in October–November 2005 and the second in October–November 2006. The 2005 survey has 1,200 respondents and was designed and distributed by the MIT Public Opinion Research Training Lab (PORTL). The 2006 survey has 1,000 respondents and is the MIT module of the 2006 Cooperative Congressional Election Survey (CCES). Both surveys consisted of nationally representative samples and were conducted by Polimetrix of Palo Alto, California. Polimetrix selected a matched-random sample designed to reflect the national population. The resulting samples mirrored the demographic and political characteristics of the U.S. adult population—gender, age, education, race, region, and income, as well as party identification and ideological orientation. The predicted division of the 2006 vote from the CCES sample forecast the election outcomes in the U.S. Senate and governor elections very well. See Table 7 at the end of the article for a summary of the survey samples' demographic characteristics. For a fuller discussion of sampling methodology and accuracy, please consult the guide to the study.[2]

[1]More recent ANES surveys ask about isolated votes, such as the confirmation of Clarence Thomas to the Supreme Court (Wolpert and Gimpel 1997).

[2]Data and codebooks are available at http://web.mit.edu/polisci/portl/index.html. See also Douglas Rivers, "Representative Sample Matching from Internet Surveys," http://web.mit.edu/polisci/

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Both surveys were designed to measure public opinions on key roll-call votes, perceptions of the behavior of constituents' own members of Congress, attitudes about the members, and electoral voting behavior. The survey instrument contained standard questions about approval of the respondents' own member of Congress as well as standard questions about issue preferences, ideology, and party (see, e.g., Page and Shapiro 1992). Such questions are commonly used to construct measures of respondent ideology and issue preferences, but they do not allow us to assess the congruence between legislators' behavior and their constituencies' preferences since congressional behavior is expressed in terms of discrete, specific votes and not broad evaluative scales of attitudes (Stone 1979). Even if we were able to match individual responses with congressional behavior, surveys do not ask respondents where they believe their representative stands on the same issue. As a result, previous surveys rarely allow us to assess constituents' perceived agreement with their representatives' legislative votes.

The design innovation in this study is to ask respondents directly about salient roll-call votes. We selected several key roll-call votes as identified by *Congressional Quarterly* and the *Washington Post*. These votes captured a range of domestic and foreign policy questions, were highly salient, were unique (i.e., there weren't several different votes on the same question in a year), and did not divide the Congress perfectly along partisan lines. The surveys asked respondents' own preferences and their perceptions of the votes of their Representatives on each of the roll-call votes. The 2005 PORTL survey asked respondents about (1) reimportation of prescription drugs, (2) a ban on "partial-birth" abortion, and (3) a constitutional amendment to ban gay marriage. The 2006 CCES asked about an even wider range of roll-call votes—(1) a ban on partial-birth abortion, (2) federal funding for stem cell research, (3) extending capital gains tax cuts, (4) ratifying CAFTA, (5) immigration reform, (6) bankruptcy reform, (7) tax breaks for energy companies, and (8) reauthorizing the Patriot Act.

Each question was presented with the language used by members of Congress during the floor debate on the bill and in media interviews, as reported by *Congressional Quarterly Weekly Reports* and the *Washington Post*.[3] Each roll-call question described the bill in a preamble, without assigning partisan or ideological labels to each side, and then asked, "What about you? If you were faced with this decision would you vote for, against or not sure?" "What about your member in the U. S. House of Representatives?"[4]

In this way, the PORTL and CCES surveys address the measurement problem. They ascertain constituents' preferences (if any) on actual roll-call votes and constituents' beliefs about their Representative's votes on exactly the same roll calls. We use these questions to gauge the survey respondents' "perceived agreement" with the decisions of their members of Congress. A constituent perceives agreement on a particular bill if the constituent would vote the same way that he or she thinks that the Representative voted.

We measure the accuracy of constituents' beliefs by matching those beliefs to reality. Respondents' perceptions about their Representatives' votes are verified against the actual roll calls, and respondents' perceptions of Representatives' party affiliation are checked against actual party affiliation. We do not assume that people know the actual roll-call vote, but that they harbor a belief, and the effect of that belief is what we wish to measure. We are agnostic about where these beliefs come from, so long as they are not merely artifacts of the survey measurement. We will show that these beliefs are on average correct and correlate with the actual positions legislators take.

It is possible that respondents are guessing and perceptions merely reflect measurement error. We consider three models of guessing and find none of them can explain the results. First, people may simply guess at random. If there is no "signal" from the actual roll-call vote, such random guessing would produce zero difference in the dependent variable (electoral votes or approval) between those who state agreement and those who state disagreement with the Representative's policy votes. Second, people might just make a "partisan" guess, based on the partisan division of a given vote or the general ideological leanings of Democrats and Republicans. If this is true, then the effect of perceived policy agreement on approval or electoral votes would vanish once we control for party identification and agreement with the representatives' party affiliation and ideology when analyzing the effect of each roll-call vote. Third, respondents might project policy agreement on those whom they approve of—this is simultaneity between the dependent and independent variable.

The first and second issues are readily addressed through multiple regression analysis. The third requires an appropriate instrumental variable that explains

portl/cces/material/sample_matching.pdf. The surveys had too few very low-income minorities and nonvoters. Sample weights offer some correction for this. Regression analyses controlled for information and education to compensate for possible biases.

[3]Descriptions of votes used the *Washington Post*'s U.S. Congress Votes Project; http://projects.washingtonpost.com/congress/ (last accessed 11/21/2007).

[4]Complete questionnaire and exact question wordings are available at http://web.mit.edu/polisci/portl/index.html.

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perceptions of roll-call votes, but does not directly affect the vote. The actual roll-call votes provide such an instrument. They directly affect people's perceptions of roll-call votes, as they are the signal on which the belief is partly based. Controlling for party, the actual vote ought to be a valid instrument for perceptions of agreement versus disagreement on a given key roll-call vote. We use the actual vote to predict the perceived vote and construct an instrumental variable to untangle the potential simultaneity between perception and approval.

A possible confound arises with question order and priming. The study was designed to minimize possible order and priming effects. The roll-call vote questions were not placed near the questions on approval or vote choice, and each roll-call vote item appeared on a separate screen.[5] The item ascertaining job approval of a House member is asked at least five minutes away from roll-call vote questions, and the vote question is asked in a separate wave of the survey. If priming is a problem, it more likely affects the party questions than the issue questions, as the party identification question is asked shortly before the job approval question.

Constituents' Preferences and Perceptions

The classical formulation of representation assumes that constituents have preferences about the issues before the legislature and know about their representative's actions on those issues. Respondents in the 2006 CCES and 2005 PORTL surveys did articulate their own preferences on salient roll-call votes, even though most did not say they followed closely or cared about public affairs. Almost all of the CCES 2006 respondents were able to give substantive responses (i.e., something other than "don't know") to the roll-call questions: over 90% of the sample answered at least four of the questions. A plurality of respondents (44%) was able to offer their views on all eight.

A similar story emerges from the 2005 PORTL data: less than 1% of the sample was unable to state their own preferences regarding legislative roll-call votes taken on the gay marriage, partial-birth abortion, and prescription drug importation bills. We make no claims about how deeply people hold these opinions, but we are at least able to conclude that constituents can articulate what they would like Congress to do on salient issues.

The respondents' expressions of their own positions on each roll-call vote allow us to estimate the average number of bills on which they take a liberal or conservative position. We computed a Roll-Call Voting score for each respondent as the percent of times that he or she would have taken the conservative position among all the roll-call votes for which the respondent took a position. A score of 100 means always taking the conservative position and a score of zero would indicate the respondent took the liberal position every time. A score for each House member's voting record is constructed in exactly the same way.

The average survey respondent appears centrist, taking the conservative position on 49% of the roll-call votes. The average Democratic identifier took the conservative position on 30% of the questions he or she answered; the average Independent on 40%; and the average Republican on 75%. In contrast, the average Democratic House member took the conservative position on 21% of these bills on which he or she voted, while the average Republican Representative toed the conservative line on 92% of these bills.

Theories of representation assert both that people have preferences over issues and that they have beliefs or perceptions about how their legislators voted on those issues. What is the picture in voters' heads of their Representatives' decisions on salient policies? The surveys asked respondents to state how they think their Representative in the U.S. House voted on each of the specific roll-call votes. In every case, a sizable majority of respondents stated how they thought their member of Congress voted on each roll-call vote. In the 2006 survey, 68% believed they knew how their member voted on at least four of the roll calls, and 83% indicated that they thought they knew how their member voted on at least one roll-call vote. Twenty-two percent said they knew how their member of Congress (MC) voted on all eight questions, while 17% could not say how their Representative voted on any of the eight roll-call votes. A similar picture emerges from the 2005 survey: 22% did not attribute positions to their MC on any of the roll calls, but 42% were able to offer positions on each of the three roll-call votes. Below, we usually include the 17% of respondents who did not place their Representatives on any of the issues, but the substantive results change little when we exclude them.

These findings clearly indicate that there is at least the foundation for classical theories of representation to work. Almost all of the respondents in the surveys are able to offer their own opinions about the roll calls we

[5]Tourangeau, Couper, and Conrad (2004) find that order and context effects vanish in web surveys when questions are on different screens. Roll-call questions came after the approval questions. Any order effects are of the form of reverse causation modeled here with instrumental variables.

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asked about, and most attempt to place their member of Congress on the same issues.

Most theoretical accounts of policy-based representation hold a further and more substantive expectation about perceptions of roll-call votes—constituents, at least in the aggregate, should get it right. Survey researchers often test whether individuals know the facts about politics, such as the names of their Representatives. Highly informed individuals are certainly sufficient for policy representation, and theoretical analyses often assume that voters are completely informed. Very high levels of information at the individual level are not necessary for representation to work. One need only require that the average perceptions of constituencies square with the voting records of Representatives. The law of large numbers would make the electorate as a whole act as if individuals were highly informed (Erikson, MacKuen, and Stimson 2002).

Using the 2006 survey, we calculate the percent of Representative's roll-call votes that the respondents got right. For every respondent, we compared how he or she thought the Representative voted to how the Representative actually voted for each measure for which the respondent gave an answer. Table 1 excludes the 17% of the sample that answered none of the roll-call vote questions. Of those who did offer a judgment about how their members voted on some roll calls, the average percent right equals 72% and the median percent right is 75%. Over half of the sample offered an answer for six or more of their House members' roll-call votes. Among these 551 cases, the average percent correct was 75% and the median percent correct was 83%. That is, of those who held a belief about their Representative's votes, the typical respondent held the correct belief three out of four times. If the respondents were just guessing at random, then we would expect that they would get only a very small fraction correct. For example, if respondents were simply guessing, the probability of getting five or six right would be 10%, but in our sample 60% of those who offered answers for six Representatives' roll-call votes correctly identified at least five.

To the extent that constituents make errors in their perceptions, it is in perceiving their representatives to be too moderate. Compare the Perceived Roll-Call Voting score to the actual percentage of the time the Representative took the conservative position. Table 1 presents summary statistics for these measures. The average respondent saw his or her Representative as taking the conservative position on 58% of the roll calls, almost exactly the true value of 60%. Among those who had Democratic Representatives, members were seen as taking the conservative position 31% of the time compared with the Democratic Representatives' actual score of 21%. Among those who had Republican Representatives, members were seen as taking the conservative position 81% of the time, compared to the true 92%. Perceptions, in the aggregate, were within approximately 10 points of the Representatives' Actual Roll-Call Voting score. If anything, representatives are more polarized than their constituents thought they were.

TABLE 1 Comparing Perceptions of Representatives' Roll-Call Voting Records with Reality

	All CCES Respondents Mean (SD)	Party of Representative	
		Democrat Mean (SD)	Republican Mean (SD)
Aggregate level			
Perceived	58.26 (11.34)	30.61 (6.51)	81.44 (3.59)
Actual	59.84 (15.75)	21.35 (6.24)	91.67 (1.18)
Perceived – Actual	−1.39 (6.54)	9.47 (7.10)	−10.53 (4.27)
t-statistic	−1.03	4.58	−7.16
Individual level			
Perceived – Actual	−2.89 (10.21)	9.69 (11.07)	−12.63 (7.39)
t-statistic	−1.82	5.25	−8.98
N	881	390	491

Note: Scores were calculated by taking the percent of the roll calls on which respondents believed their representative took the conservative position. Aggregate-level analysis calculates the mean perceived and mean actual score for each member of Congress with a constituent in the sample. Individual-level analysis calculates the difference between perceptions and reality for each constituent. Data are from the CCES 2006 survey.

Table 1 also presents the consistency between Perceived and Actual Roll-Call votes at the individual level. For each individual roll-call vote, we calculated the difference between the respondent's belief about how the legislator voted and the actual vote for every vote on which the respondent held a belief, and, then, computed the average across votes. Individuals represented by Democrats saw their member taking the conservative position about 10 percentage points more often than the member actually did, and individuals represented by Republicans saw their members as taking the conservative position around 13 percentage points less frequently than they actually did. Although statistically significant, these perceptual biases are substantively small. Individuals still saw their members as in roughly the right location, if a bit too extreme. Table 1 reveals that on average, voters perceive their representatives' positions correctly.

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TABLE 2 Accuracy of Constituents' Beliefs and Party-Line Voting

	MC Voted with Party	MC Voted Against Party
Partial-birth abortion ban	78.1 ($N = 565$)	53.8 ($N = 80$)
Stem cell research	77.2 ($N = 637$)	46.3 ($N = 95$)
Capital gains tax	81.4 ($N = 675$)	66.6 ($N = 27$)
CAFTA	66.0 ($N = 512$)	44.3 ($N = 70$)
Immigration reform	83.4 ($N = 511$)	35.2 ($N = 71$)
Bankruptcy reform	90.8 ($N = 444$)	29.9 ($N = 77$)
Patriot Act	92.9 ($N = 571$)	30.8 ($N = 91$)
Energy tax breaks	83.9 ($N = 411$)	29.8 ($N = 114$)

Note: Percentage of respondents correctly identifying MC's vote on each roll call, by whether the MC's actual vote was with a majority of his or her party or not.

As a further test of the fact that actual roll-call votes, as well as party, structure preferences, we compare the beliefs of those whose legislators voted with the majority of the party and those whose legislators voted against the majority of the party. Table 2 presents the percent of respondents who correctly stated the roll-call vote of the representative on each roll-call vote across the two conditions. In those instances when representatives voted with their party on a given bill, the constituents stated the correct vote 82% of the time, on average. In those instances when representatives deviated from their party, constituents stated the correct vote 42% of the time, on average. The difference between these figures indicates that party is highly informative. The asymmetry between them reveals that the actual vote matters. If respondents simply guessed party, they would have been correct only 18% of the time (1 – .82) in those instances when the legislator voted against the party.

Table 3 presents further evidence that constituents' perceptions of their Representatives' roll-call positions stem from the *actual* roll-call votes their legislators cast. We fit an OLS model predicting the percentage of roll-call votes on which the Representative is believed to have taken a conservative position. As independent variables, we include the percentage on which the Representative *actually* took the conservative position, the percentage on which the respondent took the conservative position (to measure whether respondents are projecting from their own positions to those of their Representatives), and dummy variables measuring perceptions of the member's party affiliation (to measure the extent to which party labels structure constituents' perceptions). Column 1 corresponds to all respondents, and the three remaining columns repeat the analysis by perceived party of the Representative.

The regression results in Table 3 show that a Representative's actual roll-call votes strongly predict respondents' beliefs about the Representative's votes. The coefficients for both 2005 and 2006 are substantively large (approximately .3) and statistically significant. The coefficient is similar across different categories of the perceived party of the Representative. Beliefs about the member's party do help to structure respondents' beliefs about their positions. Holding constant actual votes, Democrats are believed to take fewer conservative positions, Republicans to take more. Importantly, though, the results in Table 3 reveal that constituents do not simply guess their legislator's positions based on party affiliations: controlling for perceptions of the member's party, legislators actual roll calls strongly affect the beliefs constituents hold about

TABLE 3 Predicting Constituents' Perceptions of Representatives' Roll-Call Votes

	All CCES Respondents	Perception of Representative's Party		
		Republican	Democrat	Don't Know
Intercept	0.32 (0.03)	0.50 (0.05)	0.12 (0.02)	0.35 (0.07)
MC's actual positions	0.35 (0.03)	0.34 (0.06)	0.36 (0.05)	0.29 (0.07)
Respondent's positions	0.10 (0.02)	0.07 (0.03)	0.13 (0.04)	0.12 (0.10)
MC Democrat	−0.18 (0.03)			
MC Republican	0.17 (0.03)			
Adj. R^2	0.59	0.08	0.15	0.13
N	1013	461	341	201

Note: OLS regressions in which the dependent variable is the percentage of roll-call votes on which the Representative is believed to have taken a conservative position. Independent variables are the percentage of votes on which the Representative actually took a conservative position; the percentage of votes on which the constituents themselves took a conservative position; and measures of the constituents' perceptions of the Representative's party affiliation.

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those positions. Table 3 provides little evidence of systematic perceptual biases, but shows strong evidence that perceptions have real content.

Voters, then, express their own positions on important policy questions. They hold definite beliefs about the policy decisions made by Representatives. Those beliefs reflect, at least partly, the Representative's actual decisions and are, on average, accurate.

Roll-Call Votes and Accountability

Do voters, in fact, reward politicians with whom they agree and punish those with whom they disagree? And, if so, how strong are the effects of perceived agreement on constituents' opinions about their representatives?

The traditional model of representation leads us to expect that voters will approve of the job and want to retain representatives with whom they perceive relatively high levels of policy agreement. These effects work through *perceived agreement*, the pictures in people's heads, rather than actual agreement, making it necessary to measure congruence at the individual level. We also wish to understand the relationship between actual roll-call votes and constituents' perceptions, but first we establish whether that connection is present in the voters' minds, not just in aggregate correlations.

We estimate a set of regressions predicting respondents' support for their Representatives as a function of perceived policy agreement, as well as perceived partisan agreement and ideological agreement. The independent variable of interest is **Perceived Agreement** across a set of roll-call votes. Perceived Agreement *on any given bill* is a trichotomy that equals +1 for agreement, −1 for disagreement, and 0 for neither. For each roll-call vote, we coded Perceived Agreement as 1 if the respondent favors the bill and *believes* the Representative voted for the bill or if the respondent opposes the bill and believes the Representative voted against the bill. Perceived (dis)agreement is coded as −1 if respondents believe their Representative voted contrary to how they would have. Perceived agreement on a given bill equals 0 if the respondent did not have a position on the bill or did not have a belief about the Representative's vote. In addition, we constructed the Average Perceived Policy Agreement, which equals the average of perceived agreement across all roll-call votes in each survey. This variable ranges continuously from −1 (if the respondents always perceived disagreement with their Representative) to +1 (if the respondents always perceived agreement with their Representative). Average Perceived Policy Agreement equals the percent of bills on which the respondent perceived agreement minus the percent of bills on which the respondent perceived disagreement.

The dependent variables are approval of the job the legislator is doing and propensity to vote for the legislator. **Job Approval** is measured by the standard question, "Do you approve or disapprove of the way [name] handles his/her job as a member of Congress?" The variable takes five values: +1 for "Strongly Approve," +0.5 for "Somewhat Approve," 0 for "Neither," −0.5 for "Somewhat Disapprove," and −1 for "Strongly Disapprove." Respondents who said "Not sure" were coded as 0. **Vote for Incumbent**, observed in the 2006 CCES postelection wave, equals 1 if the respondent reported voting for the incumbent House member and 0 if the respondent reported voting but not for the incumbent.

Other important considerations also affect approval and vote choice, and their omission from the regression might bias estimates of the effect of Perceived Agreement on attitudes toward representatives. For instance, voters might have a general sense of the ideological orientation of the legislator and like ideologically similar representatives. Ideology would correlate with actual policy choices made in the legislature, so not including ideology in the regression analysis would bias our estimates of the effects of Perceived Agreement on actual policy decisions made by the legislator. Similarly, party of the legislator and of the respondent surely affects approval ratings and propensity to vote for the legislator.

The regression analysis includes measures of party, ideology, and agreement or congruence on party or ideological label. We code Perceived **Party Agreement** as a 1 if the respondents believe their Representative to be of the same party as them, −1 if the respondents believe their Representative to be of a different party from them, and 0 for respondents who do not identify with a party or who do not know their Representative's party. We also include a measure of **Ideology Agreement**, although the measure is different in 2005 and 2006 due to differences in the survey items. For the models using the 2005 PORTL data, we match respondents' ideological identification on a 5-point scale to the perceived partisan affiliation of their Representative. Those who identified as liberals (conservatives) and identified their Representative as a Democrat (Republican) were coded as a 1. Liberals represented by Republicans and conservatives represented by Democrats were coded as a −1. Moderates and those who did not know the party of their Representative were coded as a 0. We include a separate indicator for those who correctly identified their member's party. The 2006 CCES respondents were asked to place both themselves and their

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Representatives on an ideological scale ranging from 0 to 100. We construct two measures of ideological agreement from responses to these items: **Ideological Difference** is the legislator's perceived ideology minus the respondent's ideology. **Ideological Distance** is the absolute value of this score.[6] We include both to capture potential directional voting (Rabinowitz and MacDonald 1989).

To correct for the baseline values and to capture asymmetric effects of Party Agreement and Ideological Distance, we also include simple measures of ideology and party. **Ideology** measures self-identified positions, either on the 5-point scale for 2005 PORTL respondents or on the 100-point scale for 2006 CCES respondents. We include this variable to saturate the model with interactions and asymmetric effects, since the ideological difference and distance terms can be thought of as interaction effects with the Representatives' ideology. **Moderate** and **Independent** are dummy variables that capture respondents who identified themselves in this way when asked about their ideology (response options were "very liberal," "liberal," "moderate," "conservative," and "very conservative") and partisan affiliation (using the standard party ID question). Finally, we code **Party Correct** as a dummy variable which measures whether the respondents correctly identified the party affiliation of their Representative, to tap their general level of knowledge about and attention to politics. Other factors, such as gender, income, and education, showed no substantial effects and are not included in the analyses here; their inclusion does not alter the estimates. Descriptive statistics for all of these variables can be found in Table 6 at the end of the article.

Table 4 presents the results from six regressions that test for evidence of accountability. The analyses examine three dependent variables—Member's Job Approval in the 2005 PORTL survey, Member's Job Approval in the 2006 CCES, and House Vote in the 2006 CCES. For each dependent variable we report two specifications. One estimates the effect the Average Perceived Agreement on approval or vote; the other specification estimates the effects of Perceived Agreement on each of the roll-call votes. All specifications are OLS estimates with robust standard errors; the substantive results are identical with ordered probits (for approval) and logits (for vote). Across all the models, respondents' perceived agreement with the legislators' roll-call voting records strongly predicts the respondents' level of approval of the MC's performance in office and vote for their Representative during the election.

Consider, first, the relation between perceived agreement on policy and approval of the job the legislator is doing. The relevant estimates are displayed in the first four columns of the table. For the 2005 PORTL respondents, the coefficient on Average Perceived Agreement is 0.38, with a standard error of .03. This implies that a difference of one standard deviation on the Average Perceived Agreement score (SD = .58) corresponds to a .22 difference in approval ratings of the legislator. For the 2006 CCES, the coefficient on Average Perceived Agreement is .53, with a standard error of .04. A one standard deviation difference in Perceived Agreement (SD = .48), then, translates into a .25 difference in job approval, which is nearly the same effect as implied in the 2005 study. A one standard deviation difference in Average Perceived Agreement translates into about a quarter of the total range of this variable, and the corresponding change in approval corresponds to one-half of the standard deviation in the dependent variable and 10% of the total range of approval.

The second specification in each panel presents the estimated effects of each individual roll-call vote separately. In the 2005 study, all three roll-call votes have approximately the same coefficient, .12 to .13. In the 2006 study, the roll-call vote agreement had heterogeneous effects on approval. The coefficients range from a low of .04 for stem cell research to a high of .17 for the Patriot Act, and average .08. Combining the individual roll calls into a single Average measure loses very little fit in 2006 and none in 2005, which leads us to prefer the much simpler model using the Average Perceived Agreement.[7]

The differences between the estimated effects of roll-call positions in 2005 and 2006 invite speculation. The coefficient on Average Perceived Agreement is noticeably larger in 2006 than in 2005. The difference may reflect a substantial change, with respondents becoming more issue oriented during the election year. It may also be a consequence of measurement, as there are more items in the 2006 study and averaging more items will improve the quality of the measure.[8] It does not appear to result from

[6] Approximately 20% of the respondents did not offer an ideological placement for the incumbent. We imputed missing values using the multiple imputation routine in R and using demographic information observed for the entire sample—age, income, education, race, gender, political interest, and party identification. We estimated the models without imputed values and observed very similar coefficients to the model with imputations, but much smaller numbers of observations.

[7] In 2006, the R-square declines by 1%, and the F-statistic for the difference between the first and second specifications in the 2006 study is 2.8, which is significant at the .05 level but not the .01 level.

[8] See Peffley and Hurwitz (1985), Goren (2004), and Ansolabehere, Rodden, and Snyder (2008) for evidence that averaging many specific issues produces superior measures of policy voting than either individual issue questions or general ideology questions.

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TABLE 4 The Effect of Party, Ideology, and Roll-Call Votes on Constituents' Evaluations of Representatives and Vote Choice

	Job Approval 2005 (−1 to +1)		Job Approval 2006 (−1 to +1)		Vote for Incumbent 2006 (0, 1)	
Intercept	−.01 (.03)	−.01 (.03)	.34 (.09)	.35 (.09)	.70 (.07)	.69 (.07)
Roll-call agreement						
Average	.38 (.03)		.53 (.04)		.29 (.03)	
Gay marriage amendment		.12 (.03)				
Prescription drug imports		.12 (.02)				
Partial-birth abortion ban		.13 (.02)		.04 (.02)		−.003 (.02)
Stem cell research				−.01 (.02)		.02 (.02)
Capital gains tax				.05 (.03)		.07 (.02)
CAFTA				.08 (.02)		−.02 (.02)
Immigration reform				.06 (.03)		.03 (.02)
Bankruptcy reform				.08 (.03)		.04 (.02)
Patriot Act				.13 (.03)		.09 (.02)
Energy tax breaks				.11 (.03)		.05 (.02)
Party agreement	.29 (.03)	.29 (.03)	.17 (.03)	.16 (.03)	.11 (.02)	.10 (.02)
Ideological agreement	.05 (.02)	.05 (.02)				
Ideological difference			−.13 (.07)	−.12 (.07)	−.01 (.05)	−.02 (.05)
Ideological distance			−1.08 (.09)	−1.04 (.09)	−.74 (.07)	−.69 (.07)
Ideology	.007 (.02)	.01 (.01)	.02 (.02)	.01 (.02)	.01 (.02)	.01 (.02)
Moderate	.03 (.03)	.03 (.03)	−.15 (.04)	−.14 (.04)	−.08 (.03)	−.06 (.03)
Independent	−.08 (.04)	−.08 (.05)	.05 (.04)	.05 (.04)	.03 (.03)	.03 (.03)
Party correct	.05 (.04)	−.05 (.04)	.07 (.05)	.07 (.05)	.06 (.04)	.05 (.04)
Republican MC	−.01 (.04)	−.01 (.04)	−.14 (.04)	−.13 (.05)	−.07 (.03)	−.06 (.03)
N	1115	1115	842	842	747	747
R^2	.42	.42	.56	.57	.49	.50

Note: Estimates of the effects of perceived party, ideology, and roll-call votes on respondents' approval of their Representative (columns 1 through 4) and vote for or against the incumbent Representative (columns 5 and 6). OLS estimates with robust standard errors in parentheses. Vote choice models estimated only for respondents who voted in the 2006 elections and whose Representative ran for reelection with opposition. Individual roll-call agreement measured −1, 0, and +1. 2006 estimates combined from five multiply imputed data sets.

nonresponse, as the percent of respondents able to offer ideas about how their MC voted was strikingly similar in 2005 and 2006.

The analyses also show substantial effects of Party and Ideology on approval. Consistent with other research, party has an independent effect, apart from policy and ideology (e.g., Rahn 1993). The coefficient on Perceived Party Agreement is between .2 and .3 in the specifications predicting Job Approval and highly significant. The standard deviation on this variable is .86 in 2005 and .65 in 2006, which implies that the marginal effect of Perceived *Party* Agreement is in the neighborhood of .2 to .25 and comparable to the effect of a one standard deviation difference in perceived agreement on roll-call votes. Ideological distance from the Representative also affects approval ratings strongly. Consistent with spatial theories of approval and voting, politicians seen to be ideologically more distant from a given voter receive less support from that constituent. Ideological distance has a coefficient of .7 (SE = .07) in the 2006 study; a one standard deviation difference in ideological distance corresponds to a .18 difference in approval. Ideological agreement has a small coefficient in 2005 and is marginally significant.

Voting provides the real test of policy accountability. The 2006 CCES data allow us to measure the effects of Perceived Policy Agreement on the vote. We limit the analysis to those respondents who voted for the House in 2006 and were located in a district with an incumbent Representative running for reelection with opposition. The fifth and sixth columns in Table 4 present linear probability model (OLS) estimates predicting whether the respondent voted for the incumbent or for someone

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TABLE 5 Instrumental Variables Analysis of the Effects of Party, Ideology, and Roll-Call Votes on Approval of, and Vote for, Incumbent MC

	PORTL 2005		CCES 2006		
	Perceived Agreement (First Stage)	Job Approval (Second Stage)	Perceived Agreement (First Stage)	Job Approval (Second Stage)	Vote for Incumbent (Second Stage)
Intercept	−.01 (.03)	−.01 (.03)	.24 (.06)	.23 (.09)	.54 (.08)
Roll-call agreement					
Actual agreement	.21 (.03)		.48 (.03)		
Instrument		.64 (.13)		.77 (.09)	.64 (.07)
Party agreement	.18 (.03)	.24 (.04)	.11 (.02)	.13 (.03)	.07 (.03)
Ideological agreement	.13 (.02)	.01 (.03)			
Ideological difference			.04 (.05)	−.14 (.07)	−.03 (.06)
Ideological distance			−.58 (.06)	−.81 (.13)	−.35 (.10)
Ideology	.07 (.01)	−.01 (.02)	.002 (.02)	.02 (.02)	.01 (.02)
Moderate	.02 (.03)	.02 (.03)	−.03 (.02)	−.12 (.04)	−.03 (.03)
Independent	−.01 (.04)	−.08 (.05)	−.02 (.02)	.05 (.04)	.04 (.03)
Party correct	.12 (.03)	.01 (.04)	.04 (.03)	.05 (.05)	.03 (.04)
Republican MC	−.15 (.03)	.04 (.05)	−.07 (.03)	−.11 (.05)	−.02 (.04)
Hausman test statistic		4.38		9.50	33.09
		p = .82		p = .39	p = .0001
N	1120	1115	844	842	747
R^2	.33	.39	.58	.50	.42

Note: First column in each year fits perceptions of roll-call agreement as a function of the actual roll-call votes the Representative cast. The second column in each year presents the Two-Stage Least Squares (2SLS) analysis of job approval ratings using the roll-call instrument. For the vote choice model in 2006, second-stage coefficients are taken from a linear probability model with White's heteroskedasticity-consistent standard errors in parentheses.

else. Logits yield identical substantive conclusions. We present the OLS here for brevity, ease of interpretation, and for comparison with 2SLS estimates in Table 5.

The analyses show strong evidence that constituents choose Representatives with whom they agree on public policies and vote against those with whom they disagree. The coefficient on Average Perceived Agreement equals .29, which implies that a one standard deviation (.48) difference in Perceived Agreement translates into a .15 difference in percent voting for the incumbent. Party Agreement and Ideology matter as well. Ideological Distance has a coefficient of −.7, which also implies that a one-standard deviation difference has a .15 effect on vote support. Perceived Party Agreement has a coefficient of .11, which implies that the difference in the proportion of Democrats who vote for Democrats and Republicans who vote for Democrats is .22, holding policy and ideology constant.

The size of the coefficient on Party Agreement is somewhat surprising, as party typically swamps all else in individual-level models of voting behavior. Of course, other analyses of congressional voting do not control for Roll-Call Vote Agreement. If we omit Ideology and Perceived Policy Agreement, the coefficient on Perceived Party Agreement rises to .34, which implies a 70-point different between Democrats and Republicans in voting for a given legislator. That difference is consistent with traditional analyses of party on congressional voting. Inclusion of Perceived Policy Agreement in models of Approval and Vote Choice, then, reduces the coefficient on Party, suggesting that more than half of the effect of party in congressional vote choice stems from policy agreement.

The substantive results from these models remain the same across specifications. We varied treatment of respondents who said "don't know" to the roll-call questions. We tried three different approaches: exclude them from the analysis, use dummy variables to indicate which questions they registered no opinion on, and impute missing values. The coefficient on Average Perceived Agreement remains approximately the same. We also

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TABLE 6 Descriptive Statistics for Variables Used in Regression Analyses

	PORTL 2005		CCES 2006		Range	
	Mean	SD	Mean	SD	Min.	Max.
Job approval	0.11	0.66	0.10	0.71	−1.00	1.00
Roll-call agreement						
Overall percent	0.08	0.57	0.09	0.48	−1.00	1.00
Gay marriage amendment	0.13	0.77			−1.00	1.00
Prescription drug imports	−0.07	0.75			−1.00	1.00
Partial-birth abortion ban	0.19	0.78	0.14	0.80	−1.00	1.00
Stem cell research			0.14	0.81	−1.00	1.00
Capital gains tax			0.11	0.81	−1.00	1.00
CAFTA			0.02	0.72	−1.00	1.00
Immigration reform			0.10	0.74	−1.00	1.00
Bankruptcy reform			0.04	0.71	−1.00	1.00
Patriot Act			0.08	0.80	−1.00	1.00
Energy tax breaks			0.08	0.70	−1.00	1.00
Percentage conservative positions						
Perceived MC			0.59	0.38	0.00	1.00
Actual MC			0.63	0.40	0.00	1.00
Constituent			0.49	0.33	0.00	1.00
Party agreement	0.14	0.82	0.09	0.63	−1.00	1.00
Ideological agreement	0.14	0.68			−1.00	1.00
Ideological distance			0.18	0.24	0.00	1.00
Ideological difference			0.05	0.42	−1.00	1.00
Ideology (5-point)	−0.12	1.01			−2.00	2.00
Ideology (percentage)			0.60	0.28	0.00	1.00
Moderate	0.39	0.49	0.35	0.48	0.00	1.00
Independent	0.16	0.36	0.39	0.49	0.00	1.00
Party correct	0.67	0.46	0.70	0.43	0.00	1.00
Republican MC	0.59	0.49	0.58	0.50	0.00	1.00

Note: In both years, the minimum and maximum values of each variable are the same.

examined the possible asymmetric effects of Perceived Agreement. One hypothesis holds that voters weigh disagreement with a representative against him or her more heavily than they would weigh agreement in his or her favor. We find little evidence of such asymmetrical effects: in each case, perceived disagreement with the Representative hurts a legislator's approval ratings by roughly the same degree as perceived agreement helps.

Causality

One substantial reservation with the results concerns potential simultaneity between approval of the legislator and perceived policy agreement. Respondents who like their Representatives might infer that the Representatives must have voted the "right" way on issues. Controlling for party corrects this possibility to some extent, because party is one of the main predictors of behavior and belief, but such controls do not resolve the doubts. Random measurement error, as discussed, compounds the possible bias in OLS, and the direction of the biases arising from simultaneity and measurement error is ambiguous.[9]

[9]We wish to measure the effect of roll-call vote agreement on approval of the representative: $Y = \beta_0 + \beta_1 X^* + \varepsilon$. Survey response depends on the true agreement between the respondent's choice and the representative's roll call, X^*, projection of Y, and measurement error, u: $X = \alpha_0 + \alpha_1 Y + \alpha_2 X^* + u$. Let b_1 be the OLS estimator of the effect of X on Y, that is, the estimate of β_1 using X instead of X^*. Then,

$$p \lim b_1 = \frac{\beta_1(\alpha_1\beta_1 + \alpha_2)V(X^*) + \alpha_1 V(\varepsilon)}{(\alpha_1\beta_1 + \alpha_2)^2 V(X^*) + \alpha_1^2 V(\varepsilon) + V(u)}.$$

In general, the OLS estimate may deflate or exaggerate the magnitude of β_1.

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TABLE 7 Summary of Samples' Demographic Characteristics

	PORTL 2005	CCES 2006
Age		
Mean	47	47
Median	46	48
SD	16.1	14.4
Race		
White	72.1	74.3
Black	11.2	8.5
Hispanic	10.7	10.0
Asian	0.9	0.7
Native American	0.6	0.9
Mixed	2.3	2.1
Other	2.3	3.4
Gender		
Male	49.1	49.9
Female	50.9	50.1
Education		
No high school	10.9	3.7
High school graduate	34.7	37.4
Some college	24.4	23.0
2-year degree	4.5	9.3
4-year degree	15.6	16.0
Postgraduate	9.9	10.8
Income		
Mean	8.2	8.7
Median	8	9
SD	3.9	3.9
Region		
Northeast	22	17.9
Midwest	24	19.1
South	33	37.3
West	21	25.6

Note: Cells indicate percentage of respondents in each sample.

The reigning model of how voters think about policy representation, originally presented by Miller and Stokes (1963), points to a solution. In words, the model goes as follows. Actual roll-call votes and party identifications affect constituents' perceptions of legislative behavior on public policy. Those perceptions as well as party in turn affect assessments of the legislator's performance, affect toward the legislator, and vote choice. Simultaneity presents the further possibility that assessments of the legislator and affect toward the legislator shape constituents' perceptions of legislative behavior. This model clarifies the problem and the solution: Actual Behavior can be used to untangle the simultaneity between Perceived Behavior and Vote Choice or Job Approval because Actual Legislative Behavior affects Vote Choice or Approval only through Perceptions. Two-Stage Least Squares estimates that use Actual Roll-Call Votes to predict Perceived Roll-Call Votes ought to crack the simultaneity.

The difficulty implementing this solution has been that surveys rarely ask about roll-call votes explicitly, and when they do the votes are very unusual, such as Supreme Court nominations. The PORTL and CCES surveys were designed to solve this problem by asking about specific, salient roll-call votes across a range of issues. The actual vote of the Representative can be linked to the respondents' beliefs about Representatives' votes. Actual roll-call votes may, in turn, be used to predict perceived roll-call votes, and the predicted values from that first step provide an instrumental variable for estimating the effect of perceived roll-call vote agreement on Approval and Vote Choice.

Our two-stage analysis uses the same specification for the second stage as the OLS analyses in Table 4. The first-stage regression predicting Average Perceived Policy Agreement contains all of the other independent variables from the OLS plus a measure of Average Actual Policy Agreement. Average Actual Policy Agreement is constructed exactly the same way as Average Perceived Policy Agreement except that the Representative's actual roll-call votes on a given issue are used in lieu of the constituent's perception of the Representative's roll-call vote. We use linear 2SLS throughout. Linear 2SLS is appropriate for the model of Approval because Approval and Average Perceived Policy Agreement may be treated as continuous. To parallel the analysis of Approval, we assume a linear probability model for the vote equation, and estimate the effect of Average Perceived Policy Agreement on the Vote using linear 2SLS. Linearity of the probability model is reasonable because the potentially endogenous variable is continuous (Perceived Policy Agreement) and because of the similarity between the results using a logit or probit or OLS for the single-equation model.[10] We also estimated a nonlinear version of the vote model using Instrumental Variables Probit and a dichotomized version of Perceived Agreement using Newey's two-step IV Probit. The results are very similar results to the linear model. For brevity and ease of interpretation, we present the linear specification here.

Actual roll-call votes satisfy two key conditions for a valid instrument. First, the actual roll-call vote must not

[10] Angrist, Imbens, and Rubin (1996) offer sufficient conditions for linear 2SLS in causal models.

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directly affect the respondent's approval of the legislator. This exclusion condition stems from the model proposed originally by Miller and Stokes (1963), and it holds in an obvious sense. Constituents can only act on their personal beliefs; the actual roll-call vote can have no direct effect except through constituents' beliefs about how legislators voted. Also, representatives do not respond to particular constituents in their voting (but to all constituents), so the actual roll-call vote cast by a legislator is uncorrelated with the error in any individual's vote choice or approval function.

A second requirement is that the actual roll-call votes must have a strong direct effect on perceived roll-call votes. Table 5 presents the first-stage regressions in columns 1, 3, and 5. In both the 2005 and 2006 studies, the first-stage regressions predicting perceived roll-call votes have healthy R-square. The foot of each column presents F-tests of the joint significance of the variables, and in all three cases the instrument appears quite strong. Importantly, the excluded exogenous variable, Average Actual Agreement, has a substantively large effect on Perceived Actual Agreement and large t-statistics.

Two important substantive conclusions about the nature and origins of constituents' perceptions emerge from the first stage. First, reality matters. The actual roll-call votes cast by members of Congress had a strong effect on constituents' perceptions. Actual policy decisions serve as strong signals that, through news reports, advertisements, word of mouth, and other means, eventually filter through to the public. Second, party structures perceptions as well. Several variables capture party—including Party Agreement, Independents, Party Correct, and Republican MC. All matter to some degree, but by far the most important is Party Agreement. If a respondent is a Democrat and the member is a Democrat or if a respondent is a Republican and the member is a Republican, the respondent is much more likely to believe that he or she agrees with the Representative on roll-call votes and much more likely both to approve of and vote for the legislator.

Actual roll-call votes provide enough leverage over perceptions to allow us to address the long-standing questions about causality in this research area. Table 5 presents the Two-Stage Least Squares estimates of the causal effects of perceived policy agreement on job approval and electoral support. The second-stage estimates are shown in columns 2, 4, and 6.

The estimates in Table 5 reinforce the results in Table 4. For all three dependent measures, the magnitude of the coefficient on Perceived Agreement is larger in the 2SLS than in the OLS. For the 2005 PORTL data, the coefficient on Average Perceived Policy Agreement on Approval is .64 (SE = .14) in the two-stage model, compared with .38 in the OLS model. For the 2006 CCES data, the coefficient on Policy Agreement on Approval is .78 (SE = .09) in the two-stage model, compared with .53 in the OLS model, and the coefficient on Policy Agreement on Vote Choice is .64 (SE = .07), compared with .29 in the OLS model. The effects of Ideological Distance and Party Agreement are similar to the OLS models in Table 4.

To test whether the differences between the OLS and 2SLS estimates provide evidence of biases, we implemented Hausman's test. This test measures whether there are statistically significant differences between the consistent but less efficient 2SLS and the efficient but possibly inconsistent OLS. For both the 2005 and 2006 models of Approval, the Hausman-tests indicate that OLS is preferred. The statistic was 4.38 with a p-value of .82 in 2005 and 9.9 with a p-value of .39 in 2006. In other words, the loss of efficiency of 2SLS was too great to justify any improvement over potential biases in OLS. For the 2006 vote equation, the Hausman-test indicates that 2SLS is preferred. The test statistic is 33.1 with a p-value less than .001. Hence, there is some evidence of simultaneity bias in the analysis of vote choice, but none in the analyses of approval.

We examined the robustness of these estimates by splitting the sample and altering the set of controls. One concern is that ideology and perceived party might be endogenous. We instrument for Perceived Party using actual party. Doing so does not diminish the estimate of the effects of policy congruence on approval and vote choice. We lack an instrument for ideological distance. Under the identification conditions it may be legitimate to exclude ideology variables from the 2SLS estimation, as their exclusion will likely only cost precision. When we exclude these variables, the patterns of estimated coefficients on Perceived Roll-Call Vote Agreement in the second, fourth, and sixth columns of Table 4 are affected little. The results might also mask substantial heterogeneity across parties, leading the estimates to exaggerate the coefficient for policy congruence. We split the sample according to levels of Perceived Party Agreement; the magnitude of the effect of policy congruence is similar across these groups.

The estimates in Table 5 offer the first test and measures of potential simultaneity between approval and perceptions. Simultaneity has been widely conjectured, but the prior surveys have lacked appropriate designs to crack the problem, and the survey designs of the PORTL and CCES provide a solution. Comparisons of the OLS and 2SLS estimates reveal little evidence of simultaneity between perceived policy and approval, and some with electoral support. Biases, to the extent that they exist, understate the effects of policy congruence.

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Conclusions

The central conclusion of our analysis is that constituents have the capacity to and do in fact hold their members of Congress accountable for roll-call votes. When people are asked about specific roll-call votes, they express definite preferences on the matters at hand; further, most harbor beliefs about how their member of Congress voted. In addition, the extent to which a constituent agrees with the policy positions of the member of Congress strongly affects the constituent's approval rating of the member and likelihood of voting for the member. That conclusion is strengthened in instrumental variables estimates that correct for possible measurement error and simultaneity biases. Citizens do not, of course, pay attention to every roll-call vote, and not every citizen is attentive, but the instrumental variables estimates reveal that actual roll-call votes directly affect constituents' beliefs, and those roll-call votes, in turn, have substantial effects on approval ratings and electoral behavior. We are agnostic about how people learn about the voting behavior of their members of Congress. We suspect it is based partly on facts learned from the media and campaigns and partly on inferences, but it is more than just guessing or partisan projections. The two stages of our analysis reveal that constituents respond directly to their Representatives' roll-call voting behavior.

This conclusion is the starting point for most contemporary theorizing about Congress, especially spatial theories of politics. But, this has been a surprisingly elusive conjecture to establish empirically. At least since Miller and Stokes's (1963) seminal work on this topic, the dominant view has held that constituents don't have preferences on the matters considered by Congress, they don't have clear opinions about how their legislators act on such questions, and they don't hold their members of Congress accountable on important subjects. This study reveals that the link does exist; it is quite strong; and it creates the conditions for electoral accountability.

Why does our account differ from prior survey research, especially Miller and Stokes's classic study? One possibility is the nature of the times. Miller and Stokes asked about racial and foreign policies during the 1950s, a decade in which the politics of race and of foreign trade became complicated and volatile. Voters may have been confused about where their Representatives stood on these questions. We think the real answer lies with measurement. With the exception of isolated cases, surveys have not asked directly about the roll-call votes of Representatives, even though that is the focus of much of the congressional research. Direct measure of constituents' preferences on salient roll calls and perceptions of their legislators' behavior reveals that voters indeed harbor beliefs about their legislators' policy choices and hold the representatives' accountable. Improved measurement, of which the methods here are just one development, promises to clarify further the nature of substantive policy representation.

Finally, there is the question of aggregate accountability and congruence. Our results yield a picture strongly consistent in the aggregate with the reigning model of representation. Constituents have preferences about the important matters of the day; they have beliefs, formed through whatever means, about their representatives' policy decisions. In the aggregate, constituents' beliefs are approximately right. That is, on average, voters see their politicians as taking approximately the general overall position across a variety of roll-call votes as the Representatives in fact did. And, as the regression analyses show, constituents rely on perceived policy agreement to hold legislators accountable. The electorate rewards those seen to be in agreement with their views, and they punish those seen to be out of step.

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