Out of Step, Out of Office: Electoral Accountability and House Members' Voting

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Does a typical House member need to worry about the electoral ramifications of his roll-call decisions? We investigate the relationship between incumbents' electoral performance and roll-call support for their party—controlling for district ideology, challenger quality, and campaign spending, among other factors—through a series of tests of the 1956–1996 elections. The tests produce three key findings indicating that members are indeed accountable for their legislative voting. First, in each election, an incumbent receives a lower vote share the more he supports his party. Second, this effect is comparable in size to that of other widely recognized electoral determinants. Third, a member's probability of retaining office decreases as he offers increased support for his party, and this relationship holds for not only marginal, but also safe members.

> *You can only go so far in Texas...there's nothing more useless [to the Democrats] than a dead liberal.*
>
> *Spoken by Representative (and future President) Lyndon B. Johnson to Jim Rowe, a White House assistant under Franklin D. Roosevelt, when Rowe asked Johnson to vote for an administration proposal on civil rights (Dallek 1991, 168–9)*

A fundamental issue of American politics, as well as democratic theory, is whether the public holds elected officials accountable for their policy decisions. The founders of the United States clearly desired such accountability, and for no institution more so than the House of Representatives. As Madison ([1788] 1987, 323–4) argued in *Federalist 52*, "As it is essential to liberty that the government in general should have a common interest with the people, so it is particularly essential that the branch of it under consideration [the House of Representatives] should have an immediate dependence on, and intimate sympathy with, the people." He continued by surmising, "Frequent elections are unquestionably the only policy by which this dependence sympathy can be effectively secured."

Of course, frequent House elections achieve Madison's desired common interest only to the extent that electoral outcomes depend upon officials' policy decisions, and research in political science calls into question whether in fact voters hold members accountable for their policy choices. Perhaps most indicative of a lack of accountability is the plethora of studies that suggest that individual voters are fairly ignorant about members' policy actions (e.g., Campbell et al. 1960; Converse 1964; Delli Carpini and Keeter 1991; Miller and Stokes 1962; Smith 1989). Moreover, as Downs (1957) argued nearly half a century ago, such ignorance is rational for the typical citizen, who has little incentive to expend effort to gather political information given the extremely low probability that he is pivotal in an election. Thus the typical representative might be able to vote on legislative matters as she pleases without fearing that she could lose reelection.

In contrast to this body of work, research on congressional behavior suggests that House members believe that roll-call voting affects their probability of reelection, causing them to consider constituent ideology in deciding upon roll-call positions. This concern with the electoral effect of legislative voting is a standard component of theories of Congress[1] and a consistent finding of surveys of members.[2] Even research that emphasizes other influences on roll-call decisions, such as Matthews and Stimson (1975), still acknowledges members' attention to the potential electoral ramifications of the decisions. For example, Matthews and Stimson (1975, 30) highlight the following observation of a member:

> I've found out this much. When you are voting right, you build up points on a cumulative basis. You lose them on a geometric basis; you can lose all your points on one vote.

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[1] For example, see Arnold (1990), Fiorina (1974), and Mayhew (1974a).

[2] See Clausen (1973), Kingdon (1981), and Sullivan et al. (1993) among others. Collie (1985) provides an excellent review of this literature.

Studies that focus explicitly on electoral results do not resolve whether a typical member should be concerned with the potential electoral ramifications of legislative voting. Several studies find no evidence of an electoral impact (e.g., Gaines and Nokken 1999), while those uncovering one have generally focused on members' vote shares for specific elections (e.g., Jacobson 1996) or samples of legislators (e.g., Ansolabehere, Stewart and Snyder 2001). Notably, even the positive evidence does not establish that a member who has won with at least 60% of the two-party vote, a criterion applying to the majority of incumbents between 1956 and 1996, need fear that roll-call voting could affect his probability of defeat. Results on vote shares and seat loss are not in general equivalent, and moreover, the existing evidence suggests that a large shift in a member's voting, such as from a perfectly moderate record to one at his party extreme, decreases his two-party vote share by less than 10 percentage points.

In the following, we conduct three tests on the elections of 1956–1996 to analyze whether in fact House members should be concerned with the electoral impact of legislative voting. In each of the tests, we estimate the effect of an incumbent's increased support for the extreme of his party. The first test employs a standard model from previous work to assess whether such legislative voting had a significant effect on members' vote shares in each election. The second test then pools across the elections, estimating how the average impact of legislative voting compares to that of other factors, such as challenger quality and campaign spending. The third test proceeds to examine directly the relationship between members' voting and their probability of reelection. To incorporate that this probability may differ between "safe" members who have previously won with at least 60% of the vote and the remaining "marginal" members, the test distinguishes between them. In addition, it accounts for the fact that a member's safety may depend upon his prior voting record.

EXISTING EVIDENCE ON THE ELECTORAL EFFECT OF HOUSE MEMBERS' VOTING

Almost every existing study that finds an electoral effect of legislative voting focuses on a small set of elections. These include the studies by Schoenberger (1969) for 1964, Erikson (1971) for Republicans in the 1950s–1960s, Bernstein (1989) for 1978 and 1980, Johannes and McAdams (1981) for 1978, Erikson and Wright (1993) for 1990, and Brady et al. (1996) and Jacobson (1996) for 1994.[3] While details vary across the works, they generally establish that, holding district ideology constant, Republican incumbents' vote shares were lower the more conservative their roll-call voting and Democratic incumbents' vote shares lower the more liberal their roll-call voting.[4] In other words, members lost electoral support from increasing the extent to which they voted with the extreme of their party.

This research provides a critical basis for considering the importance of legislative voting in affecting electoral outcomes but, as a whole, does not establish that members' actual probability of seat loss is affected by their roll-call decisions, particularly for so-called safe members. In general, results on electoral margins are not equivalent to results on seat loss because, among other reasons, members behave differently if they are minimizing their probability of defeat rather than seeking the largest number of votes.[5] Even ignoring this nonequivalence, however, the existing results do not imply a conclusive impact. For example, Erikson and Wright (1993) find that a Democrat who shifted from perfectly moderate to perfectly liberal voting decreased his two-party vote share by only 5 percentage points in 1990, and Bernstein (1989) estimates that among Democratic incumbents who lost in 1980, only those receiving at least 46% of the vote could have conceivably altered the outcome by moderating their roll-call voting. Given such findings, one might conclude that a typical incumbent need not fear that roll-call voting could ever cost him reelection, and Bernstein explicitly makes this claim. After reviewing the existing evidence, he surmises that "members can generally afford to vote for what they think is right without expecting that their votes will cost them a seat in Congress..." (Bernstein 1989, 100). The other cited studies on electoral margins do not offer such a bold claim, but neither do they establish a systematic relationship between members' voting and their probability of retaining office.

Even the contention that legislative voting systematically affects electoral margins is put into question by work that analyzes elections across several decades. For example, political economics research on members' ideological "shirking" from 1960 through 1990 has produced inconsistent findings. In studies that estimate shirking as the absolute difference between interest-group voting scores and the predicted values from these scores regressed on constituency characteristics, some evidence has suggested that shirking is correlated with electoral defeat (e.g., Wright 1993), while other evidence has suggested that the factors are uncorrelated (e.g., Goff and Grier 1993). One possible reason for the discrepancy is that these studies equate shirking with noise that could result from omitted variables; to the extent that the constituency characteristics included in a particular analysis do not fully capture district preferences, shirking is overestimated

[3] In related work, Wright (1978) and Erikson and Wright (1989, 1997) show that members' publicly stated campaign positions affected their electoral margins in 1966, 1982, and 1994, respectively.

[4] The two exceptions are Johannes and McAdams (1981) and Brady et al. (1996). In the former, the authors construct a measure of ideological discrepancy that allows any member, regardless of partisan affiliation, to be out of step with district ideology in either a conservative or a liberal direction. In the latter, the authors analyze the relationship between incumbents' presidential support and their probability of reelection.

[5] See Aranson, Hinich and Ordeshook (1974).

and the effects of it underestimated.[6] To avoid this problem, Lott and Bronars (1993) equate shirking with change in a member's voting pattern. They find some evidence of a positive correlation between such change and electoral defeat, but this result is not robust across the various econometric specifications employed.

Other research that examines elections over time, but that does not characterize itself as studying shirking, also produces inconclusive results as to whether legislative voting systematically affects elections. Gaines and Nokken (1999) analyze all midterm elections between 1958 and 1994 and find that in almost every year, an incumbent's roll-call support for the president did not significantly affect his probability of reelection. Brady, Canes-Wrone and Cogan (2000) find more evidence of an impact, showing that for the 1954–1994 elections, winning incumbents tended to have more moderate voting records than losing incumbents. This difference, however, is significant for only about one-third of the elections.

The most compelling evidence of an electoral effect over time comes from Ansolabehere, Snyder and Stewart (2001) and Erikson and Wright (2000). Ansolabehere, Snyder and Stewart pool the elections of 1952–1974 and 1976–1996 to compare candidates' voting records in races in which a sitting incumbent was defeated and find that a candidate's vote share was lower the more she supported the extreme of her party. The key limitation of the analysis is that it does not examine the roll-call voting of members who are never unseated, and therefore, as the authors acknowledge, it remains possible that most House races are not affected by legislative voting. Erikson and Wright do examine all incumbents for the elections of 1976–1996 and similarly find that voting with the ideological extreme of one's party decreases one's vote share. Their analysis does not, however, control for standard electoral determinants such as challenger quality and campaign spending, and thus it remains possible that the effect of legislative voting would disappear once such controls were added to the analysis.[7] In addition, as with other analyses of electoral margins, the results do not establish a significant probability of seat loss, particularly for safe members.

In sum, the literature is far from conclusive that legislative voting systematically affects House electoral outcomes. It offers some evidence that roll-call voting, particularly voting with the extreme of one's party, affects electoral margins, but this relationship has not been established across time and members. Moreover, even the existing support does not show that legislative voting affects the probability of reelection, particularly for safe members. What is needed, therefore, is a study that examines the relationship between vote shares and roll-call decisions across elections for all districts and that explicitly analyzes the effect of the decisions on the probability of winning for safe as well as marginal members.

LEGISLATIVE VOTING AND ELECTORAL MARGINS

Building on previous research, we estimate the electoral impact of legislative voting by analyzing the effect of voting with the extreme of one's party, a phenomenon we refer to as roll-call ideological extremity. We hypothesize that controlling for district ideology, Democrats lose electoral support by voting more liberally and Republicans by voting more conservatively. Thus for two Democratic (Republican) incumbents from districts with identical voter preferences, the Democrat (Republican) with the more liberal (conservative) voting record should have a lower electoral vote share, holding all else equal. We refer to this prediction as the *Roll-Call Ideological Extremity Hypothesis*.

While much of the previously discussed work on electoral margins details the theoretical framework underlying the Roll-Call Ideological Extremity Hypothesis (in particular, see Erikson 1971), we briefly review the logic here. Arguably the most critical assumption is that the Republican and Democratic candidates of each district diverge ideologically, with the Democrat being more liberal than the Republican. Evidence of such divergence is provided by Ansolabehere, Snyder and Stewart (2001), and other studies justify this phenomenon by describing the long-run career benefits a member may obtain from voting with his or her party (e.g., Aldrich 1995; Cox and McCubbins 1993; Rohde 1991; Snyder and Groseclose 2000). Thus even on roll-calls for which a member's electoral incentive is to vote against her party, she can have long-term career incentives to vote with it.

Applying Downsian (1957) logic to this stylized fact of ideological divergence between the Republican and the Democratic candidates in a district, ideological moderation should increase an incumbent's vote share. Given Downs' assumptions that voters can order the candidates on an ideological spectrum, that voters have positions on the spectrum, and that electoral choices depend upon candidates' positions, either candidate can increase her vote share by moderating towards the other. Thus if voters' assessment of the incumbent's ideology is not independent of her legislative voting, her electoral margin is higher the less she votes with the ideological extreme of her party.[8]

Notably, while this framework assumes that voters select candidates on the basis of ideology, the Roll-Call

[6] For other problems with the two-stage residual approach, see Bender and Lott (1996).

[7] Notably, most of the other cited analyses also do not control for these electoral determinants.

[8] The directional theory of issue voting (Rabinowitz and MacDonald 1989) suggests that a candidate may increase his vote share by moving toward his party extreme. While we base our theoretical prediction on Downsian logic, our empirical analysis allows for this alternative hypothesis (and in fact tests for it). Moreover, we are careful to use two-tailed significance tests in evaluating the Roll-Call Ideological Extremity Hypothesis given the alternative prediction of the directional theory.

Ideological Extremity Hypothesis does *not* require voters to be fully informed about all details of an incumbent's legislative record. The literature identifies several mechanisms by which roll-call voting should affect constituents' assessments of an incumbent's ideology absent their complete knowledge of his or her record.[9] As Erikson (1971) discusses, one such mechanism is a diffusion model in which elites pay attention to the legislative voting of members, and voters take cues from elites that share their policy positions.[10] A second mechanism, suggested by the analysis of Bailey (2001), is that challengers are likely to inform constituents about the policy positions of a member when he votes out of step with district preferences. In fact, Miller and Stokes (1963), while finding that constituents are not fully informed about representatives' positions, actually provide some support for Bailey's analysis; specifically, Miller and Stokes show that when an incumbent had voted out of step with his district on the issue of race, all interviewed voters could identify his and his challenger's positions on the issue.[11] We do not take a stance on whether one of these informational mechanisms predominates but merely emphasize that any observed relationship between electoral outcomes and roll-call decisions does not depend upon the assumption that voters are fully informed about the details of representatives' voting records.[12]

Model, Data, and Measurement for Analysis of Electoral Margins

To test the Roll-Call Ideological Extremity Hypothesis, we employ an econometric model that is similar to those adopted in previous studies of the relationship between legislative voting and electoral margins. In particular, we regress each incumbent's vote share on a measure of roll-call ideological extremity, controlling for a range of factors.[13] Formally, we estimate the following model for each incumbent i who faces a major party challenger in election t:

$$\begin{aligned}
&\text{Incumbent's Vote Share}_{\text{it}}\\
&\quad= \beta_0 + \beta_1 \text{ Roll-Call Ideological Extremity}_{\text{it}}\\
&\quad+ \beta_2 \text{ Presidential Vote}_{\text{it}} + \beta_3 \text{ Challenger Quality}_{\text{it}}\\
&\quad+ \beta_4 \text{ (ln (Challenger Spending)}\\
&\quad- \text{ln (Incumbent Spending))}_{\text{it}}\\
&\quad+ \beta_5 \text{ Freshman}_{\text{it}} + \beta_6 \text{ In Party}_{\text{it}}\\
&\quad+ [\beta_7 \Delta\text{Personal Income}_{\text{it}} \text{ (coded by In Party)}\\
&\quad+ \beta_8 \text{ Presidential Popularity}_{\text{it}} \text{ (coded by In Party)}\\
&\quad+ \beta_9 \text{ Midterm Loss}_{\text{it}} \text{ (coded by In Party)}] + \varepsilon_{\text{it}} \qquad \textbf{(1)}
\end{aligned}$$

where the variables in brackets are naturally included only in tests that pool across elections. Because the Roll-Call Ideological Extremity Hypothesis does not imply asymmetric effects across parties, we analyze Democratic and Republican incumbents jointly, with the parties in a given year separated by the factor *In Party*. The substantive results still hold, however, if the effect of roll-call ideological extremity or the entire model is estimated separately for each party.[14] We analyze contested races only because otherwise, unnecessary error may be introduced by assigning an uncontested member a vote share of 100%. The substantive results hold, however, if such races are included.[15]

The measurement of each variable in the model is as follows.

Incumbent's Vote Share. As is common in studies of electoral margins (e.g., Jacobson 1996), the dependent variable equals the incumbent's percentage of the two-party vote.[16]

Roll-Call Ideological Extremity. We base our measure of legislative voting on Americans for Democratic Action (ADA) scores, which reflect the proportion of liberal positions taken by a member in a given year on votes selected by the Americans for Democratic Action. These votes generally encompass the key policy issues of a session. For example, the scores include the major tax and budget votes of the 1980s and 1990s,

[9] Even Downs (1957, 80) explicitly states that his framework assumes that voters "may be completely unaware of certain actions being carried out by the government, or of alternatives the government could have undertaken, or of both."

[10] Among others, see Kuklinski, Metlay and Kay (1982) and Lupia (1994) for evidence of citizen cue-taking from elites.

[11] Jacobson and Kernell (1983) identify a related mechanism, arguing that a high-quality challenger is more likely to enter the electoral race if an incumbent has voted out of step with district preferences. This mechanism still presumes that there exists some relationship between an incumbent's voting record and voters' assessment of the incumbent; otherwise, potential challengers' decisions on whether to enter the race would not be based on the incumbent's record.

[12] Complementing these mechanisms is the "running tally" or "online" cognitive model of voters' evaluations of candidates. In this cognitive model, voters update their judgment of a candidate with each new piece of information received and, when they produce an overall evaluation, retrieve the tally without reviewing the contents upon which it has been formed. See, for example, Lodge, McGraw and Stroh (1989).

[13] Snyder (1996b) formally derives the conditions under which a linear factor structure can be employed to analyze aggregated voting data, and Snyder (1996a) employs such a model.

[14] When we estimate the effect of roll-call ideological extremity separately by party, but the remainder of the model jointly, the null that the coefficients for the two parties are equivalent cannot be rejected. Specifically, for the time series as a whole, the coefficient on Roll-Call Ideological Extremity for Democrats is 0.067 (with a standard error of 0.006) and the analogous coefficient for Republicans is 0.055 (with a standard error of 0.008). The difference between these two coefficients, 0.012, is not significant at conventional levels ($F = 1.13$, $p > 0.2$). Estimating only the elections for which we have data on candidates' spending, we again cannot reject the null that the two coefficients are identical ($F = 2.41$, $p > 0.1$).

[15] The main difference when uncontested elections are included is that if separate coefficients are estimated for Democratic and Republican incumbents, the magnitude of the difference between Democrats and Republicans becomes relatively large, with roll-call ideological extremity having approximately three times the impact on the former than the latter. (Each coefficient remains significant, however.) Consistent with this result, Democrats held 82% of uncontested seats during the time period of our analysis.

[16] The independents Dale Alford (AR), John Moakley (MA), Thomas Foglietta (PA), and Bernie Sanders (VT) have been coded as Democrats since each caucused with this party.

the abortion and school prayer votes of the 1970s and 1980s, and the Civil Rights votes of the 1960s. The ADA ratings are widely known among members and often publicized in their districts. Therefore, to the extent that legislative voting affects members' electoral fate, the scores should have this effect. The specific measure that we adopt provides a consistent ranking of ideological extremity across the parties and is based on a member's ADA score in the year prior to the election if the member is Democratic and 100 minus this score if the member is Republican. Thus for a Democrat, a higher value indicates a more liberal member, while for a Republican, it reflects a more conservative one.[17] So that the factor is on the same scale as the dependent variable, we divide the raw score by 100 for purposes of presentation.

Although we focus on the ADA ratings, we have also analyzed the model with *Roll-Call Ideological Extremity* based on Keith Poole and Howard Rosenthal's DW-NOMINATE scores. These scores, which are a similar, more recent version of Poole and Rosenthal's (1991) D-NOMINATE scores, utilize a wider selection of members' votes than the interest group ratings.[18] The results, which are available upon request, are substantively similar to those presented.

Presidential Vote. Following Erikson and Wright (1989, 1993, 1997) and Brady et al. (1996), among others, we use the presidential vote to control for district ideology. Specifically, *Presidential Vote* equals the average proportion of the two-party vote received by the presidential candidate of the incumbent's party in the two most recent elections in his or her district, with each combination of presidential elections normalized around its mean. Thus for Democratic incumbents, the variable is based on the vote for the Democratic presidential candidate, and for Republican incumbents, on the vote for the Republican presidential candidate. We average across the two most recent elections and normalize each of these averages, to mitigate the impact of factors that may be specific to individual presidential races.

Challenger Quality. Research suggests that an incumbent's vote share is lower when she faces a challenger who has previously won an elected position (e.g. Jacobson and Kernell 1983). We therefore control for challenger quality with a variable that equals 1 if the challenger has held elective office and 0 otherwise.

ln(Challenger Spending) − ln(Incumbent Spending). A great deal of work has examined the electoral effects of candidates' spending, and while the findings vary, the literature as a whole suggests that a candidate's spending increases his vote share. As in some previous research (e.g., Erikson and Wright 1993), we control for this effect with a variable reflecting the difference between challenger and incumbent spending, taking the natural log of each. Our data are from Federal Elections Commission (FEC) Reports. Because these reports were not edited by the FEC until 1980, we include the control for spending only in the 1980 through 1996 elections.[19] To account for the fact that candidates are not required to report expenditures below $5000, we adopt Jacobson's (1990) practice of assuming that each spent at least this amount.

Freshman. This dummy variable equals 1 if the House term was the incumbent's first and 0 otherwise. Research suggests that there exists an incumbency advantage due to, among other factors, members' capacity to bring home pork and perform constituency service (Mayhew 1974a). Because freshmen generally have less political clout than more senior members, for example, less influential committee assignments (Munger 1988), freshmen should have a lower incumbency advantage and thus a lower average vote share.

*ΔPersonal Income (*coded by *In Party), Presidential Popularity (*coded by *In Party), Midterm Loss (*coded by *In Party),* and *In Party.* We include several factors that vary over time[20] and that research suggests have a differential effect according to whether a member is in the president's party. The first factor is based on Tufte's (1975) standard control for the economy and reflects his argument that a good economy will help the in party at the expense of the out party. Specifically, the variable equals the change in real income per capita in the year prior to the election for members of the president's party and −1 times this value for other members. The second factor accounts for his contention that a president's public approval should be positively correlated with his fellow partisans' electoral fortunes. Like Tufte, we measure presidential popularity with the percentage of positive responses to the long-standing Gallup Poll asking "Do you approve or disapprove of the way [the current president] is handling his job as president?" using the poll taken most recently prior to the election. As with income, this value is multiplied by −1 for members of the out party. The third factor, which we call midterm loss, controls for the so-called midterm loss tendency of the president's party to lose seats in off-year elections. Previous work provides a variety of explanations for the

[17] We are aware of the limitations of pooling interest group scores over time (Groseclose, Levitt and Snyder 1999), but the available corrections constrain the way in which members' scores can change over time, and we do not want to eliminate the possibility that a member could vote moderately for a number of sessions and then more extremely in a particular session. Still, we have analyzed our model with the Groseclose, Levitt and Snyder adjusted scores, and our substantive results hold.

[18] In conducting this analysis, we used the average of each member's first- and second-dimension DW-NOMINATE scores.

[19] As Souraf (1992, 248, note 10) writes, "Since the Federal Election Commission did not begin to operate at full power until the 1978 elections, complete data on congressional campaign finance are not available before that election. Even then, the FEC never had the resources to edit and reconcile the data for the receipts and spending of congressional candidates in 1978; one must use a final preliminary report and its data. Worse than that, FEC data for 1976 are sketchy and for 1974 one has to rely on the much smaller volume of data that Common Cause assembled."

[20] We have also conducted the analysis with a set of election indicators. However, because the controls for presidential popularity, midterm loss, and the macroeconomy already account for a good deal of inter temporal variation, multicollinearity prevents including a full set of election effects. For example, in the 1980–1996 test, only five election effects can be included without creating perfect multicollinearity among some of the controls. The results are substantively similar with the effects.

TABLE 1. House Incumbents' Legislative Voting and Electoral Vote Share: By Election, 1956–1996

	Roll-Call Ideological Extremity with Controls of Presidential Vote, In Party, Challenger Quality, Candidates' Spending, Freshman		Roll-Call Ideological Extremity with Controls of Presidential Vote, In Party, Challenger Quality, Freshman
1996 ($n = 362$)	−0.082*** (0.023)	1978 ($n = 311$)	−0.114*** (0.022)
1994 ($n = 330$)	−0.074*** (0.019)	1976 ($n = 329$)	−0.109*** (0.021)
1992 ($n = 311$)	−0.107*** (0.021)	1974 ($n = 311$)	−0.087*** (0.024)
1990 ($n = 315$)	−0.113*** (0.021)	1972 ($n = 313$)	−0.110*** (0.018)
1988 ($n = 327$)	−0.101*** (0.022)	1970 ($n = 325$)	−0.090*** (0.019)
1986 ($n = 319$)	−0.095*** (0.020)	1968 ($n = 347$)	−0.130*** (0.024)
1984 ($n = 340$)	−0.067** (0.021)	1966 ($n = 342$)	−0.046* (0.020)
1982 ($n = 309$)	−0.032* (0.016)	1964 ($n = 346$)	−0.119*** (0.017)
1980 ($n = 337$)	−0.033* (0.016)	1962	—
		1960 ($n = 321$)	−0.108*** (0.019)
		1958 ($n = 292$)	−0.058* (0.016)
		1956 ($n = 336$)	−0.092*** (0.021)

Note: Robust standard errors given in parentheses below OLS coefficients. * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$. All significance levels in the tables are two-tailed.

trend, ranging from voters' dissatisfaction with presidential performance (Kramer 1971) to voters' desire for a greater balance of power between the executive and legislative branches (Erikson 1988).[21] As with the previous two factors, we code midterm loss on the basis of whether a member is in the president's party.

We include each of these factors as a single term that is coded by affiliation with the in party rather than without this coding but with interaction terms according to affiliation with the in party because the latter approach would create a high degree of multicollinearity.[22] For example, the correlation is 0.93 between a main effect for whether a member is in the president's party and an interaction of this term with the president's popularity entered without differentiating across members. In our analysis, we still include the main effect *In Party*, which equals 1 if a member is in the president's party and 0 otherwise, given that our coding is based on this differentiation.[23]

Results on Electoral Margins

We first analyze Eq. (1) for each election of 1956 through 1996.[24] Given that previous work has focused on more limited subsets of elections and/or members, the individual-election analysis allows us to discern whether legislative voting affects electoral margins only in certain years or more consistently across time. The only year that we do not analyze is 1962, which is excluded because the extensive reapportionment following the Supreme Court decision in *Baker v. Carr* hinders mapping the 1960 presidential vote onto the 1962 districts.[25] Also because of this reapportionment, for the 1964 and 1966 elections, *Presidential Vote* is based only on the most recent presidential election.

Table 1 presents the central results. In particular, the table describes the parameter estimates on

[21] Other rationales include the macroeconomy and presidential popularity, which are controlled for directly.

[22] Our approach is akin to that of Jacobson (2000).

[23] An additional control variable with which we have conducted the analysis is the incumbent's prior vote, and the results from this analysis are substantively similar to those presented. Disagreement exists in the literature over whether to include lagged vote share as an explanatory variable of current vote share. For example, in the previous studies of roll-call voting cited, some do not control for lagged vote (e.g., Ansolabehere, Snyder and Stewart 2001; Eriskon and Wright 2000) while others do (e.g., Jacobson 1996). Our main rationale for not including the variable derives from Ansolabehere, Snyder and Stewart's (2000), which shows that it does a poor job of measuring the normal vote, which is the traditional reason for including lagged vote. We therefore do not include the variable in our analysis.

[24] We conducted all analyses regarding electoral margins with STATA version 6.0.

[25] We have also analyzed an alternative specification that excludes any observation for which redistricting occurred in the previous 2 years or since the most recent presidential election and that bases *Presidential Vote* only on this most recent presidential election. The results are substantively similar to those presented.

the key variable *Roll-Call Ideological Extremity*, reporting White's robust standard errors because the Cook and Weisberg (1983) test rejects the null of homoskedasticity in almost every election. We have chosen to make the parameter estimates on the control variables and the regression diagnostics available upon request to conserve space, particularly because these findings largely replicate those presented subsequently (and more succinctly) for the pooled-election analyses.

As Table 1 shows, the results of the individual-election analysis strongly support the Roll-Call Ideological Extremity Hypothesis. In *all* of the elections, an incumbent receives a significantly lower electoral margin the more he votes with the extreme of his party holding constant the range of control variables listed in the table. Thus comparing two Republican members who have identical district ideologies, who are running against the same quality of challenger, who are not freshmen, and who face the same distribution of spending between themselves and their challengers, the member with the more conservative voting record obtains a lower vote share. Similarly, the results suggest that between two otherwise equivalent Democratic incumbents, the one with the more liberal voting record will receive the lower vote share. In each year, this effect is statistically significant at the conventional level of $p = 0.05$ (two-tailed).[26]

To interpret the size of this effect, we consider a 25-point shift in a member's ADA score toward the extreme of his party, a shift that approximates one standard deviation of the data, or 3 to 4 high-profile votes.[27] The coefficients indicate that such a shift decreases a member's vote share by 1 to 3 percentage points. Thus, in every election between 1956 and 1996, the typical variation that occurs in roll-call behavior had a noticeable effect on incumbents' vote shares. Moreover, these estimates are arguably conservative since we have controlled for challenger quality and campaign spending; to the extent that roll-call voting works through these factors, controlling for them as we do will provide a conservative estimate of the impact of roll-call ideological extremity.[28]

Given the consistent significance of the relationship between roll-call decisions and vote margins, we proceed to estimate the average impact of this relationship. Pooling the data, we control not only for factors that vary only within an election but also ones that vary across time, as specified in Eq. (1). Because the control for candidate spending exists only for 1980–1996, we conduct two tests. The first examines these later years with the control, and the second all years without it.

TABLE 2. House Incumbents' Legislative Voting and Electoral Vote Share: Pooled Analysis, 1956–1996

	1980–1996	1956–1996
Roll-Call Ideological Extremity	−0.085*** (0.007)	−0.070*** (0.005)
Presidential Vote	0.466*** (0.015)	0.454*** (0.011)
Challenger Quality	−0.025*** (0.003)	−0.053*** (0.002)
ln (Challenger Spending) − ln(Incumbent Spending)	−0.030*** (0.001)	—
Freshman	−0.008** (0.003)	−0.020*** (0.003)
ΔPersonal Income (coded by In Party)	0.013** (0.004)	0.042*** (0.003)
Presidential Popularity (coded by In Party)	−0.089*** (0.019)	−0.044*** (0.010)
Midterm Loss (coded by In Party)	−0.033*** (0.004)	−0.015*** (0.003)
In Party	0.045** (0.017)	−0.040*** (0.011)
Constant	0.656*** (0.010)	0.739*** (0.007)
Number of observations	2950	6521
R^2	0.594	0.365
Root MSE	0.063	0.079

Note: Robust standard errors given in parentheses below OLS coefficients. ** $p < 0.01$; *** $p < 0.001$.

Table 2 presents the results. As with the individual-election analysis, the Cook and Weisberg (1983) test suggests evidence of heteroskedasticity, and White's robust standard errors are therefore reported.[29] Table 2 provides further support for the Roll-Call Ideological Extremity Hypothesis. Holding constant a variety of factors that are commonly presumed to determine elections, the effect of roll-call ideology is positive and statistically significant for each sample of the data.

Moreover, this effect has a magnitude comparable to that of other commonly recognized electoral determinants. To interpret the average impact of ideologically extreme voting, we again focus on a 25-point shift in a member's ADA score toward the extreme of his party, which, as mentioned previously, approximates a standard deviation. The coefficients for each

[26] These results, in supporting the Roll-Call Ideological Extremity Hypothesis, also support the Downsian logic of elections over the directional theory of issue voting, which allows that candidates may increase their electoral margins by voting more extremely.

[27] The standard deviation in members' ADA scores equals 24.43 points.

[28] Analyzing our model without controlling for challenger quality or campaign spending, a 25-point shift in roll-call extremity is estimated to decrease a member's vote share by 1.5 to 4 percentage points. Another reason that our estimate is arguably conservative is that we have excluded uncontested elections, which many previous studies include (e.g., Brady, Canes-Wrone and Cogan 2000; Erikson and Wright 2000). Analyzing our model with contested elections, a standard deviation increase in roll-call extremity is estimated to decrease a member's vote share by 2 to 7 percentage points.

[29] Specifically, $\chi^2(9) = 36.77$ ($p < 0.05$) for the 1980–1996 sample and $\chi^2(8) = 168.03$ ($p < 0.05$) for the 1956–1996 sample. We have also tested for autocorrelation and found that it is not significant. The lack of significant autocorrelation is not terribly surprising since open seats are excluded; in fact, less than 10% of the observations are in a district that has an uninterrupted time series. The DW statistic (calculated as if each observation may be correlated with the most recent previous election in which an incumbent ran in that district) equals 1.89 for the 1980–1996 sample and 1.90 for the 1956–1996 sample.

sample indicate that such a shift would decrease a member's vote share by approximately 2 percentage points. In comparison, the 1980–1996 results suggest that an incumbent's vote share is lower by 2.5 percentage points when she faces a high-quality challenger, by 1 percentage point if the ratio between the challenger's and the incumbent's spending increases by 25 percentage points, and by 1 percentage point if she is a freshman. Even the coefficient on the midterm loss phenomenon has a comparable impact; off-year elections are estimated to lower the vote share of members of the president's party by 3 percentage points. The 1956–1996 coefficients are similar, with a high-quality challenger lowering a member's vote share by approximately 5 percentage points, a freshman receiving a lower vote share of 2 percentage points, and off-year elections lowering the vote shares of members of the president's party by 2 percentage points.

As these results suggest, the effects of the control variables are generally consistent with our expectations. The coefficients on challenger quality, presidential vote, candidates' spending, freshmen, personal income, and the midterm loss phenomenon all have the predicted signs with statistical significance ($p < 0.05$, two-tailed). The only control variable that has an unexpected effect is presidential popularity, and even though the factor was included merely to prevent overestimating any impact of legislative voting, we still investigated why the variable did not have the same effect as in the study by Tufte (1975). Employing Tufte's specification, which regresses seat change across the parties (rather than individual members' vote shares) on presidential popularity and personal income, we found presidential popularity to have the predicted impact, suggesting our finding derives from differences across the specifications. Specifically, the Tufte effect does not hold once the control for spending is added or if the dependent variable represents vote share rather than seat loss; our subsequent analysis on the probability of reelection demonstrates this variation in the impact.

Before moving on to this analysis, however, we first summarize the key findings in Tables 1 and 2. The results are important because previous work had left open the possibility that legislative voting affects members' electoral margins only for particular elections. By establishing a consistent effect in every year, we have foreclosed this possibility. Moreover, we have shown that the magnitude of the effect is not insubstantial but rather comparable to that of other widely recognized electoral determinants such as campaign spending, challenger quality, and the midterm loss phenomenon.

LEGISLATIVE VOTING AND THE PROBABILITY OF REELECTION BY DISTRICT SAFETY

The previous section still leaves open whether most members should be concerned that legislative voting might affect their probability of defeat. On the one hand, a majority of incumbents win by the so-called safe margin of at least 60% of the two-party vote, and the findings indicate that a shift from perfectly moderate to perfectly extreme voting alters a member's vote share by only 4 percentage points. On the other hand, the findings indicate that safety itself is likely to be dependent upon members' prior legislative records, and this relationship would suggest that even safe members might need to fear the electoral ramifications of legislative voting. For example, the results on electoral margins would lead us to expect that between two otherwise similar incumbents, the one with the more moderate record should be likelier to win by a safe margin, yet if that member were subsequently to shift toward her party's extreme, then her probability of winning reelection could significantly decrease. In fact, to the extent that a member is safe due to the moderation of her prior voting, a change toward more ideologically extreme voting could have as large of an effect as for a marginal member, whose lack of safety may derive from having voted ideologically extremely.

To assess whether members should indeed fear that roll-call voting might affect their prospects for reelection, we conduct a final test that estimates the relationship between legislative voting and the probability of reelection directly. In the test, we account for the possibility that safe members may not need to be concerned with the electoral ramifications of legislative voting even if marginal members do. Moreover, we incorporate that safety itself may depend upon members' prior voting records.

Model and Data for Analysis of the Probability of Reelection

The model that we employ is an endogenous switching regime regression, which has been utilized previously in political science (e.g., Kiewiet and McCubbins 1988; McCarty and Poole 1995). The model has similarities to the instrumental variables approach that has long been used to analyze congressional elections (e.g., Ferejohn and Calvert 1984) and consists of two equations. In the first-stage equation the binary indicator *Safety*, which equals 1 if the incumbent won at least 60% of the two-party vote in the previous election and equals 0 otherwise, is regressed via probit analysis on determinants of the previous election.[30] The second-stage equation, regresses *Incumbent Won*, which equals 1 if the incumbent won reelection as 0 otherwise, on determinants of the current election. In this equation, the independent variables are separated into regimes based on the probability the member is safe as estimated by the first-stage equation.

The regime-switching specification of the second-stage equation is important because if we estimated a single coefficient for roll-call voting, the effect could

[30] We use the cutpoint of 60% rather than the other, often employed cutpoint of 55% (e.g., Mayhew 1974b) to bias *against* a finding that safe members face a higher probability of electoral defeat from ideologically extreme voting. We have conducted the analysis with this alternative cutpoint and found substantively similar results.

be significant (in magnitude and standard error) even if the impact for safe members were negligible. Our specification allows that the effect may be negligible for these members. The first-stage equation is also important; it is necessary from an econometric standpoint because if the factors influencing safety are correlated with those determining the probability of reelection, then modeling safety as exogenous could bias the results in favor of finding a significant effect of roll-call voting (see Maddala 1983, 283–4). By modeling safety as endogenous, we avoid this problem.[31]

Equations (2) and (3) formally state the model.

$$\begin{aligned}\Pr[\text{Incumbent Won}_{\text{it}} = 1] &= \Phi(\Pr[\text{Safety}_{\text{it}} = 1]^{**} \\ &\times (\kappa_0 + \kappa_1 \text{ Roll-Call Ideological Extremity}_{\text{it}} \\ &+ \kappa_{2\text{–}8} \text{ Control Variables}_{\text{it}}) \\ &+ (1 - \Pr[\text{Safety}_{\text{it}} = 1]^{**}) \\ &\times (\delta_0 + \delta_1 \text{ Roll-Call Ideological Extremity}_{\text{it}} \\ &+ \delta_{2\text{–}8} \text{ Control Variables}_{\text{it}}) \end{aligned} \qquad \textbf{(2)}$$

where $\Pr[\text{Safety}_{\text{it}} = 1]^{**}$ equals the predicted values from

$$\begin{aligned}&\Pr[\text{Safety}_{\text{it}} = 1] \\ &= \Phi(\gamma_0 + \gamma_1 \text{ Roll-Call Ideological Extremity}_{\text{it}-1} \\ &+ \gamma_{2\text{–}8} \text{ Control Variables}_{\text{it}-1} + \gamma_9 \text{ Freshman}_{\text{it}-1})\end{aligned} \qquad \textbf{(3)}$$

and where the Control Variables include *Presidential Vote*, *Challenger Quality*, *ln(Challenger Spending)-ln(Incumbent Spending)*, *ΔPersonal Income* (coded by *In Party*), *Presidential Popularity* (coded by *In Party*), *Midterm Loss* (coded by *In Party*), and *In Party*.

Because we are interested in capturing the degree to which prior legislative voting affects safety, we include only members who in election t have a voting record in the Congress preceding election $t-1$, a sample that includes only nonfreshmen. Notably, this exclusion biases *against* finding that legislative voting significantly affects the probability of reelection; because freshmen tend to have a lower ability to bring home pork, perform constituency service, and otherwise serve their district in nonvoting capacities (Munger 1988), any effect of roll-call voting should be greater for these members. The control variable *Freshman* is thus not included in Eq. (2), but the lag is still included in Eq. (3) because sophomore members running in election t ran as freshmen in election $t-1$.

The model is identified by this necessary exclusion of *Freshman* from Eq. (2) and, more generally, the fact that all other variables in Eq. (3) are lags of the regressors in Eq. (2). Thus for a given observation, *Incumbent Won* is predicted by right-hand-side components distinct from those predicting *Safety*. For example, if an incumbent faces a high-quality challenger in election t but did not face one in election $t-1$, the value of *Challenger Quality*$_t$ equals 1, while *Challenger Quality*$_{t-1}$ equals 0.[32]

Our predictions for Eqs. (2) and (3) are similar to those we made regarding electoral margins. In terms of the incumbent's estimated safety, we predict that, holding district ideology constant, a member's likelihood of holding a safe seat will decrease as she shifts her voting toward the extreme of her party. In terms of the probability of reelection, we predict a negative effect of *Roll-Call Ideological Extremity*, expecting this effect to be significant for not only marginal but also safe members. We do not predict whether the impact should be greater for safe or marginal members because, as explained above, a significant relationship between safety and roll call voting would indicate that the impact could be similar for each type of member.

Results on the Probability of Reelection

We estimate the two-equation system by maximum likelihood and, following our previous tests, employ robust standard errors.[33] Although the equations are estimated jointly, we describe the results separately for presentation purposes, beginning with the first-stage equation that estimates the safety of the incumbent. Table 3 states these results. It is immediately apparent from the table that safety is a function of the factors we previously found to determine incremental changes in vote shares. In other words, when a member is described as safe, this descriptor refers not to an exogenously imposed invulnerability but, instead, to an outcome based on political conditions, some of which are in the member's control.

Most importantly for our purposes, the results indicate that a member is significantly more likely to be designated safe the more moderate is his legislative voting holding other factors equal. Interpreting the probit coefficients at the means of the independent variables, we focus as before on a 25-point shift in an incumbent's ADA score toward the extreme of his party, a shift that approximates a standard deviation.[34] Specifically, such a change decreases a member's probability of holding a safe seat by 6% for each sample of the data. So-called safety is therefore not independent of legislative voting, suggesting that safe members may well need to fear the electoral ramifications of roll-call decisions.

[31] We have, however, estimated a one-equation probit model of the probability of winning regressed on roll-call ideological extremity, the control variables, and a control for the incumbent's vote share last year and found a significant effect of roll-call ideological extremity. These results are presented in Table A1 (Appendix).

[32] We do not include in Eq. (3) the determinants of election t because of the intertemporal inconsistency that would result from doing so.

[33] We used the package TSP 4.5 to maximize the likelihood function, which Kimhi (1999) defines.

[34] The standard deviation in members' ADA scores is 24.96 points. This standard deviation is close but not identical to that of the data in Table 2 since the current analysis excludes freshmen running in election t.

TABLE 3. The Effect of Legislative Voting on House Seat Safety

	1980–1996	1956–1996
Roll-Call Ideological Extremity *in* $t-1$	−1.320***	−0.806***
	(0.181)	(0.090)
Presidential Vote *in* $t-1$	5.799***	4.668***
	(0.488)	(0.242)
Challenger Quality *in* $t-1$	−0.446***	−0.729***
	(0.082)	(0.046)
ln(Challenger Spending) − ln(Incumbent Spending) *in* $t-1$	0.556***	—-
	(0.039)	
Freshman *in* $t-1$	−0.131	−0.253***
	(0.083)	(0.051)
ΔPersonal Income (coded by In Party) *in* $t-1$	0.635***	0.505***
	(0.133)	(0.064)
Presidential Popularity (coded by In Party) *in* $t-1$	−2.797***	−0.701***
	(0.622)	(0.103)
Midterm Loss (coded by In Party) *in* $t-1$	−0.038	−0.165**
	(0.110)	(0.057)
In Party *in* $t-1$	1.595**	−0.311**
	(0.518)	(0.102)
Constant	0.373	1.697***
	(0.310)	(0.090)
Number of observations	2446	5460
χ^2 test of joint significance of coefficients	669.865***	1727.650***

Note: Robust standard errors given in parentheses below probit coefficients. ** $p < 0.01$; *** $p < 0.001$.

The control variables generally have the same effect on safety as they did on marginal changes in vote share. The most notable difference is in the 1980–1996 sample: the coefficients on *Midterm Loss* and *Freshman* are no longer significant at $p = 0.05$ (two-tailed) in this sample. These effects are still in the expected direction, however, and that on freshman remains marginally significant ($p = 0.06$, one-tailed).

Turning to the main results, those on the probability of reelection, Table 4 presents them by the estimated regime of seat safety. In particular, the table describes the parameter estimates of each electoral determinant for a safe member versus a marginal one. The findings show that even safe members face a significantly lower probability of reelection when they increase the extent to which they vote with the extreme of their party. For both safe and marginal members and in each sample of the data, an incumbent's likelihood of retaining her seat decreases the more ideologically extreme is her voting.

These results support the long-standing findings that members view legislative voting as an important component of the electoral connection and consider constituency cues in roll-call decisions. Perhaps most notably, the results show why such legislative behavior is consistent with the fact that most incumbents win reelection by safe margins. Even safe legislators' roll-call voting affects their risk of electoral defeat. Thus members are correct in assuming that legislative votes have an impact on the probability of reelection.

Comparing the coefficients on *Roll-Call Ideological Extremity* across regimes, the one for marginal members is higher than that for safe members in the test with the control for campaign spending, while the reverse occurs in the other test. Neither difference, however, is statistically significant ($p > 0.4$ in each case). This result is consistent with the first-stage findings that safety is a function of members' prior voting records. As we argued at the outset, to the extent that safety depends upon roll-call decisions, they may have a similar impact on the probability of winning for safe and marginal members.

In terms of the magnitude of the coefficients, interpretation is less straightforward than for even a typical probit analysis because the means of the variables differ across the regimes of safety. To describe the magnitudes similarly by regime, we interpret the probit coefficients at specified parameter values, beginning with the following ones, which were chosen for their moderate values: 0.75 for roll-call ideological extremity (an ADA of 75 for a Democrat, 25 for a Republican); a difference of 0 between the district vote for the presidential candidate of the incumbent's party and the average vote for that candidate across the districts; 1 for challenger quality, midterm loss, and in party; 50% for presidential popularity; and 0 for the other variables. At these values, a member who shifts his ADA score 25 points toward the extreme of his party decreases his probability of reelection by 11% in the marginal regime of the 1980–1996 test, 9% in the safe regime of that test, and 4% in each regime of the 1956–1996 test. Altering the values such that the incumbent does not face a high-quality challenger, the magnitudes stay relatively constant, with such a shift in an incumbent's roll-call voting decreasing his probability of reelection by 9% in the marginal regime of the 1980–1996 test,

TABLE 4. The Effect of Legislative Voting on the Probability of Reelection

	1980–1996		1956–1996	
	Marginal	Safe	Marginal	Safe
Roll-Call Ideological Extremity	−1.306**	−0.868*	−0.422*	−0.602**
	(0.461)	(0.379)	(0.212)	(0.215)
Presidential Vote	4.940***	5.237***	1.295	4.266***
	(1.302)	(1.035)	(0.666)	(0.605)
Challenger Quality	−0.241	−0.417**	−0.466***	−0.556***
	(0.164)	(0.157)	(0.094)	(0.102)
ln(Challenger Spending) – ln(Incumbent Spending)	−0.696***	−0.794***	—	—
	(0.127)	(0.089)		
ΔPersonal Income (coded by In Party)	−0.042	0.563	0.690***	0.872***
	(0.340)	(0.307)	(0.186)	(0.178)
Presidential Popularity (coded by In Party)	0.451	−2.326	1.919***	0.686
	(1.330)	(1.508)	(0.530)	(0.620)
Midterm Loss (coded by In Party)	−0.584*	0.043	−0.729***	−0.103
	(0.227)	(0.221)	(0.125)	(0.131)
In Party	−0.910	1.042	−2.501***	−0.515
	(1.157)	(1.252)	(0.548)	(0.625)
Constant	3.097***	1.192	3.276***	2.951***
	(0.663)	(0.672)	(0.367)	(0.368)
Number of observations	2446		5460	
χ^2 test of joint significance of coefficients	483.867***		766.939***	
log likelihood of system of equations	−1091.703		−3498.372	

Note: Robust standard errors given in parentheses below probit coefficients. * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$. The regime of whether a member is classified as safe or marginal is derived endogenously from Eq. (3) in the text, the results of which are given in Table 3.

8% in the safe regime of this test, 3% in the marginal regime of the 1956–1996 test, and 2% in the remaining regime. Likewise, reducing the initial value of roll-call ideological extremity does not substantially alter the results. For example, at 0.50 (an ADA of 50 for each member) a 25-point shift in a member's ADA toward his party's extreme decreases his likelihood of reelection by 8% in each regime of the 1980–1996 test and by 3 to 4% in each regime of the 1956–1996 test.

As with the results on vote shares, the impact of roll-call voting is comparable to that for other electoral determinants. For example, at the initial parameter values of the previous paragraph, the 1980–1996 test suggests that an incumbent's probability of winning would decline by 5 to 7% if the ratio of challenger to incumbent spending increased by 25%. Similarly, each test indicates that a high-quality challenger decreases an incumbent's probability of reelection by 6 to 15%. The impact of legislative voting on the probability of reelection is thus not only statistically significant for safe as well as marginal members, but also similar to the effect of factors that are commonly recognized to be important determinants of electoral outcomes.

In general, the results on the control variables comport with our expectations. The effects of campaign spending and district ideology are consistently in the expected direction and statistically significant. Those on challenger quality also have the correct sign in each regime and sample and are significant with the exception of the marginal regime of the 1980–1996 test. In addition, the coefficients for the remaining variables that are not included as a main effect typically have the predicted sign, and they are significant only with the expected sign. Specifically, the effect of the economy is significant in each regime of the 1956–1996 test, the effect of the midterm loss is significant in the marginal regime of each test, and the effect of presidential popularity is significant in the marginal regime of the 1956–1996 test.

Overall, the results in Tables 3 and 4 are important because they show that a typical member should rationally be concerned with the electoral impact of legislative voting. Controlling for a wide range of other electoral determinants, roll-call voting has a significant effect on the probability of reelection. Notably, this effect holds for not only marginal but also safe members. These results are consistent with our finding that safety itself is a function of members' voting. That is, safe members are "safe" partially as a consequence of their roll-call decisions. Consequently, when such members change their voting pattern toward their party's extreme, they face a significantly higher probability of defeat, just as other members do.

CONCLUSION

While surveys of members have consistently found that they are concerned with the potential electoral ramifications of legislative voting, previous work had not established that this concern is necessarily rational. Surveys of the mass public have suggested that the typical voter is fairly ignorant of her representative's policy decisions, and studies of elections themselves have provided mixed results as to whether legislative voting affects electoral margins. Moreover, even the positive evidence has left open the possibility that roll-call decisions may not affect the likelihood of defeat for the majority of incumbents. The literature has thus failed to provide evidence that a typical member should believe that he is accountable to voters with regard to his roll-call decisions.

Our study presents three types of evidence to this end. First, we show that, holding district ideology constant, in every election between 1956 and 1996 an incumbent's vote share decreased the more he voted with the extreme of his party. Second, pooling across the years, we establish that the average impact of this effect is comparable to that of commonly recognized electoral determinants such as challenger quality. Third, by directly examining the probability of reelection, we demonstrate that the probability decreases significantly as an incumbent's voting support for his party increases, with this effect holding not only for marginal but also safe incumbents. The vulnerability of the safe members occurs in part because moderate voting increases the probability of holding a safe seat or, in other words, because safety itself derives from a member's roll-call positions.

Notably, while we find evidence of House members' electoral accountability for their policy actions, we certainly do not conclude that voters are highly knowledgeable about all such actions. Earlier we noted several rationales for the seemingly distinct states of the world. For example, "uninformed" voters may take cues from informed elites or, alternatively, may become informed by challengers when an incumbent votes out of step with her district. Accordingly, future work might examine how elites' and challengers' behaviors influence the degree to which legislative voting affects electoral outcomes.

Our analysis also provokes a number of other issues for future research. One question is the extent to which the effect of legislative voting varies according to the salience of the policy issue. Research on legislative behavior finds that members are more likely to enact legislation reflecting public opinion when the policy issue is salient (Canes-Wrone 2001; Hutchings 1998; Kollman 1998). In combination with our findings, this research suggests that members' legislative votes regarding relatively salient issues may have larger electoral effects than votes on less salient matters.

A second extension would be to examine whether changes in the number of marginal seats derive in part from variation in party discipline. Standard explanations for the "vanishing marginals" of the 1960s–1980s emphasize member activities other than legislative voting, such as constituency service (Fiorina 1989). Meanwhile, research has found substantial variation in the influence of parties over members' voting throughout these decades (e.g., Snyder and Groseclose 2000). The evidence presented in this paper suggests that these phenomena of party discipline and the vanishing marginals could be related.

APPENDIX

TABLE A1. Alternative Specification of Analysis of Probability of Reelection

	1980–1996	1956–1996
Roll-Call Ideological Extremity	−0.678*	−0.363**
	(0.263)	(0.126)
Incumbent's Previous Vote Share	1.105	3.671***
	(0.707)	(0.622)
Presidential Vote	4.354***	3.299***
	(0.760)	(0.439)
Challenger Quality	−0.367***	−0.491***
	(0.099)	(0.059)
ln(ChallengerSpending) − ln(Incumbent Spending)	−0.769***	—
	(0.078)	
Freshman	0.058	−0.080
	(0.118)	(0.072)
ΔPersonal Income (coded by In Party)	0.300	0.521***
	(0.197)	(0.098)
Presidential Popularity (coded by In Party)	−1.020	1.181**
	(0.955)	(0.366)
Midterm Loss (coded by In Party)	−0.565***	−0.610***
	(0.141)	(0.085)
In Party	0.325	−1.851***
	(0.831)	(0.376)
Constant	1.043	1.016*
	(0.651)	(0.420)
Number of observations	2912	6462
χ^2 test of joint significance	205.830***	421.352***
log likelihood	−1191.507	−406.619

Note: Robust standard errors given in parentheses below probit coefficients. * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$.

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