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Reassessing the Relationship Between Roll Call Extremity and Reelection Safety in the U.S. House

Richard Born
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Abstract

We employ a fixed effects, instrumental variable approach to reexamine the question of whether U.S. House members with more extreme roll call records are punished at election time. This approach, we argue, is better suited to the problem than previous techniques that have been applied in that it combines the rigor of fixed effects regression with the ability to accommodate the putative endogeneity of roll call ideology. At the same time, it better approximates the dynamics faced by members who may be motivated to calibrate their voting record over time in order to achieve balance between adherence to ideological principle and electoral security. Extremity, in conformity with most previous research findings, indeed emerges in our study as a potent cause of vote losses. It is not the case, as Carson, Koger, Lebo, and Young have contended, that elevated party unity on roll calls is what actually impairs reelection margin; rather, both indices matter.

The congressional literature leaves little doubt that the degree of electoral apprehension felt by U.S. legislators affects their ideological positioning on roll call votes. House members and senators moderate their voting in response to the approach of the next general election and the magnitude of the electoral threat they face. Thus, Thomas (1985, 102-8) and Levitt (1996, 436) find movement toward the center by non-retiring senators entering the sixth year of their term. At the House level, whether electoral security is assayed in terms of the presidential vote in the member's district (Erikson and Wright 2000, 159-64; Erikson and Wright 2013, 104-5; Mann 2006, 275-77; Mayhew 2011, 9) or in terms of expected reelection margin as a function of short-term forces in a particular election year (Erikson and Wright 2000, 160-61), more vulnerable incumbents are more likely to moderate. Likewise, when the ideological position of the challenger is also taken into account, greater incumbent-challenger convergence results when the challenger has had elected office experience—presumably a sign of a stronger candidacy (Burden 2004, 221-22).[1]

But is such roll call moderation, in turn, actually rewarded at the polls? The weight of the evidence suggests it is. Ansolabehere, Snyder, and Stewart (2001, 151-52) in their examination of House elections from 1874-1996 find that incumbents and challengers with positions more moderate than that of their party as a whole were able to reap modest election gains from the mid-1960s onward, even though gains prior to that period were all but non-existent. Likewise, Erikson and Wright (2013, 105-8)—only singling out incumbent House candidates—discover that moderation paid off both for Democrats and, to a somewhat lesser extent, Republicans in 2010. Previous analyses by them of elections from 1976 to 2006 generally have uncovered electoral effects of comparable magnitude for both parties (Erikson and Wright 1989, 104-5; 1993, 103-8; 1997, 145-51; 2001, 82-86; 2005, 92-95; 2009, 85-88; 2000, 156-59).[2] Brady, Canes-Wrone, and Cogan (2000, 181-89), somewhat anomalously employing the extremity of House members' roll call votes as a left-hand rather than right-hand variable, find from 1954 to 1994 that losing Democratic incumbents are more likely to have more liberal roll call records, while losing Republican incumbents are more likely to have more conservative records. Finally, in what remains the most extensive investigation of the phenomenon, Canes-Wrone, Brady, and Cogan in a follow-up study (2002, 132-37) show that roll call extremity from 1956 to 1996 is as powerful a determinant of House members' reelection security as are such commonly utilized predictors as campaign spending or freshman incumbency status.

All these studies, however, have been based upon cross-sectional analysis, or, in the case of multi-election studies, pooled cross-sectional analysis. For a given member, the findings answer the question of how his or her ideological stance compared to that of other members influences reelection safety. But there is another relevant question this kind of analysis cannot answer, one that may well be more central to members' own reelection calculus. This involves the potential for manipulating their reelection fortunes by adjusting roll call positioning over time. All members face the prospect of a tradeoff between fidelity to the issue positions they and their strongest supporters embrace, and alienation of the median general election voter (Erikson and Wright 2013, 103). Thus, a member may engage in an ongoing process of adjustment over time, trying to find an acceptable mix of upholding ideological principle and maintaining sufficient electoral security. In this sense, the relevant comparisons to be drawn are *between the particular member at varying points in time*, rather than between the member and other members at the same time.

The proper procedure for carrying out such over-time analysis is fixed effects regression. This technique, by treating each member as his or her own control, therefore examines how changes in roll call records across Congresses affect reelection outcomes. Unlike the case with standard cross-sectional or pooled cross-sectional models, unmeasured variables with time-invariant impacts on members' ideology or safety can be controlled; i.e., such static variables wash out of the fixed effects equation (Allison 2009, 6-7). Thus, fixed effects regression provides an opportunity for more precise estimation of the independent variable parameters, at the same time that it better proxies the actual dynamics of the ideology-electoral safety interplay faced by members.

A number of roll call-based studies have productively incorporated the fixed effects approach into their analysis, although not with the goal that motivates our own analysis. Levitt (1996) and Wood and Andersson (1998) investigate the effects on senators' voting over the course of their careers of changes in such variables as their own personal ideology or the public's policy preferences. Stratmann's analogous longitudinal study of House members' voting (2000) focuses on the effects of redistricting and increasing seniority. Griffin, on the other hand, mainly relies upon pooled cross-sectional analysis in his study of how members' roll call responsiveness to constituency ideology varies according to district competitiveness. However, fixed effects regression is used as a more rigorous supplementary tool, and its corroboration of his earlier results reinforces confidence that competitiveness indeed leads to greater responsiveness (2006, 918-19).

Regardless of what fixed effects analysis may reveal about the impact of roll call extremity on reelection safety, any study like ours must also deal with a challenge posed by relatively recent work of Carson, Koger, Lebo, and Young (2010). Here, the authors find evidence that House members' loyalty to the roll call positions of their party majority, rather than ideological extremity, is what constituents really punish at the polls. Party unity and extremity obviously bear a strong relationship to one another (2010, 601).[3] Nonetheless, analyses based on both experimental and aggregate data suggest to them that constituents react more adversely against the former.

By way of explanation, two arguments—both rather problematic in our view—are offered by Carson at al. (2010, 603). First, they contend that partisanship is a less abstract concept to voters than is ideology, which in itself is hardly a controversial claim. Partisanship, however, is not the same thing as the member's degree of party unity. The latter is a less fundamental concept, defined as the proportion of the time that the party majority is supported on roll call votes where party majorities are in disagreement. More is demanded of voters than simply knowing the incumbent's party membership. Furthermore, many studies have found that citizens' reliance on ideology to structure their political evaluations has become increasingly common in the era of polarization (Jones and McDermott 2010, 69-77). Voters, especially the more politically engaged, have become more seriously divided on the same issues that divide political elites (Abramowitz 2013, 12). Greater numbers of Americans are currently able to place themselves on the ANES seven-point ideological scale, as well as to see the Democratic Party as more liberal than the Republican Party (Abramowitz 2010, 122-23). Accordingly, more votes for the House are being cast along ideologically coherent lines (Jacobson 2003, 10).

The second argument made by Carson at al. for why ideology should matter less in elections than party unity is that members marked by ideological extremity may nonetheless be viewed as "principled," whereas members with high party unity may be viewed derisively as "hacks." But this speculation does not take notice of the growing distance between the ideological self-placement of rank and file partisans and their placement of opposite party House candidates. (Jacobson 2006, 87-88). Such ideologically estranged voters may not be in a forgiving mood on Election Day just because they acknowledge that an extremist position has been arrived at on the basis of sincere conviction.

These qualms about the authors' assertions notwithstanding, the analyses that follow in our paper always will be conducted using both types of roll call measures. We restrict ourselves to House members and focus on all elections from 1980 to 2008. The starting point is determined by the fact that, as Canes-Wrone, Brady, and Cogan (2002, 131) point out, 1980 is the first year in which the Federal Election Commission had the resources to edit spending data reported by candidates. These spending data will be employed as an important control in equations explaining election outcomes. In addition, since the longitudinal nature of fixed effects regression necessarily requires at least two observations on each case, (Allison 2009, 1), the analyses based on this technique will include only members running in two or more reelection campaigns.

Data and Methods

The dependent variable in our study is the incumbent's proportion of the two-party vote. Elections lacking a major party challenger are excluded.[4] Our principal independent variable, of course, is the ideological extremity of the member's roll call record. Here, we rely upon the adjusted ADA (Americans for Democratic Action) measure developed by Groseclose, Levitt, and Snyder, Jr. (1999).

Every year the ADA selects approximately 20 House roll calls it considers most pivotal to the liberal policy agenda. A member's score is the percentage of votes supporting the ADA position. Groseclose et al., however, point out that scores from year to year are not directly comparable, because of the "shifting" and "stretching" of the ADA scales. In the case of shifting, members may score higher or lower not because of change over time in their underlying ideological ideal points, but simply because the votes selected by the ADA may on average constitute more lenient or more strict tests of liberalism. In the case of stretching, members' scores can vary from year to year because the roll calls selected have more or less dispersion in their cutting points. For example, if all Democrats voted the liberal position on every roll call and all Republicans voted consistently conservative, each member would then be scored either zero or one hundred—meaning maximum dispersion in members' scores. The authors' adjustment for making the ADA scale compatible from year to year, thus permitting inter-temporal comparisons, involves subtracting a shift parameter from the original ADA score, and then dividing the resulting difference by a stretch parameter.

As an alternative, the first dimension of Poole and Rosenthal's DW-Nominate measure (2007, 28-30) might conceivably be used to tap roll call ideology.[5] Like adjusted ADA scores, DW-Nominate scores overcome the problem of scale inconsistency over time. However, when a member's ideology changes, the DW-Nominate procedure, unlike the adjusted ADA procedure, restricts these changes to be of equal, unidirectional magnitude across each time interval. In so doing, it may well exaggerate the stability of members' roll call ideology over the course of their careers (Ensley, Tobias, and de Marchi 2013, 26-27; Treier 2006, 14; Treier 2011, 815-16). This does not matter in studies like those mentioned above that have examined the roll call extremity-reelection safety question using cross-sectional or pooled cross-sectional analysis. But in situations where one is interested in over-time analysis and believes that members' roll call preferences may change non-linearly in response to influences such as assumption of a party or committee leadership position, DW-Nominate is inappropriate, and Groseclose et al. recommend application of their adjusted ADA score procedure (1999, 46-47). This situation certainly corresponds to our own fixed effects analysis, where we assume that members may be prompted to modify their roll call ideology from one Congress to the next in an attempt to affect reelection safety.

Adjusted ADA scores, to be sure, have the shortcoming of being unable to differentiate changes in individual members' voting behavior from general trends toward liberalism or conservatism, a criticism that applies to DW-Nominate scores as well (Groseclose et al. 1999, 47-48). Furthermore, the measure has been criticized for its "coarseness"; i.e., the smaller number of roll calls it uses in comparison to DW-Nominate's reliance upon all non-unanimous votes (Poole 2007, 448). The upside of this lower n, however, is that a much greater percentage of roll calls comprising the adjusted ADA measure will involve the high profile issues before Congress that are most likely to elicit constituent interest (Canes-Wrone et al. 2002, 130-31). In any event, since Carson et al. do employ first dimension DW-Nominate scores as their ideological measure when they contrast the effects of ideological extremity and party unity on reelection safety, we shall also utilize these scores whenever possible to supplement our primary reliance upon the adjusted ADA variable.

For Democratic incumbents in each Congress, our adjusted ADA measure simply is the mean of their two yearly scores, divided by 100. For Republicans, we subtract their mean adjusted ADA score from 100, and then divide this difference by 100. Therefore, as in the study by Canes-Wrone et al., higher scores regardless of party indicate greater roll call extremity; i.e., reelection margin is hypothesized to decline as Democrats become more liberal and Republicans become more conservative.[6]

DW-Nominate scores, unlike adjusted ADA scores, are available only on a Congress-by-Congress rather than yearly basis.[7] Across the entire period considered in our study, they range in value from -0.757 (most liberal) to 1.264 (most conservative). Arriving at a transformed scale that is the equivalent of our adjusted ADA scale demands, once again, that greater liberalism for Democrats and greater conservatism for Republicans always be regarded as greater extremism. A Democrat (or Republican) who is at a given distance below (or above) the zero point must be considered more extreme than a Democrat (or Republican) the same distance above (or below) the zero point. To achieve this end, we leave the original DW-Nominate scores intact for Republicans, and multiply them by -1 for Democrats. Carson et al. in their own study employ a different transformation—calculating the absolute values of all members' original DW-Nominate scores—with the same goal of realizing a scale where higher values represent greater extremity (2010, 606). But this transformation means that distance from the zero point is now what determines extremity, regardless of whether that distance is positive or negative. Members who are out of line with their party's prevailing ideology will therefore appear to be less moderate relative to their more orthodox party colleagues than they actually are. A Democrat initially at 0.2, for example, becomes indistinguishable from a less moderate Democrat initially at -0.2, while a Republican initially at -0.2 becomes indistinguishable from a less moderate Republican initially at 0.2. Our own measure, therefore, likely will yield a stronger relationship between ideology and reelection margin, even though the small number of members having a "wrongly signed" original DW-Nominate score (only 1.5 percent of cases in the forthcoming analyses using this variable) means that in practice, the difference will be minor.

The other core independent variable that figures in our analysis is party unity. This, as stated above, simply is the proportion of the time incumbents support their party on votes that pit majorities of each party against one other. Like the DW-Nominate scores, on-line party unity data are available only on a Congress-by-Congress basis.[8]

We shall be estimating three different models in this study. After preliminary analysis using OLS in a pooled cross-sectional regression, we then focus on two fixed effects models, one with and one without an instrumental variable. All three models include the following control variables, which are likewise used by both Canes-Wrone et al. and by Carson et al.:

District partisan homogeneity For Democratic incumbents, homogeneity is the two-party vote proportion for the Democratic presidential candidate in their district minus the mean Democratic presidential vote proportion across all 435 districts; for Republican incumbents, homogeneity is the mean Democratic presidential vote proportion across all districts minus the Democratic proportion in their district. When midterm House elections are analyzed, the presidential returns come from the immediately preceding on-year election; when presidential year House elections are analyzed, the presidential returns come from that same year.[9]

Campaign spending disparity between challenger and incumbent *ln*(challenger's spending) – *ln*(incumbent's spending). All amounts are in 1980 constant dollars.[10]

In-party member during midterm 1 if member is from presidential party during midterm election, 0 if presidential year election, -1 if member is from non-presidential party during midterm election

Change in personal income For members of presidential party, change in U.S. real per capita income during year prior to election; for members of non-presidential party, change is multiplied by -1. All amounts are in 1980 constant dollars.

Presidential job approval For members of presidential party, proportion of national respondents approving of president's job performance in final Gallup Poll before election; for members of non-presidential party, proportion is multiplied by -1.

In-party member 1 if member is from presidential party, 0 if member is from non-presidential party.

Member's prior margin Member's proportion of two-party vote in immediately preceding election year.[11]

As is common practice in congressional election studies, the presidential vote in each House district, because of its strong relationship to the distribution of constituents' party identification, is used to proxy underlying district partisanship. For the campaign spending variable, the natural logs of challenger and incumbent spending allow for diminishing electoral rewards as expenditures climb. Members' values on the next four variables depend on whether they belong to the presidential party. The least auspicious congressional election environment for a party can be expected during a midterm when it controls the White House. Greater income growth and a more popular president should make for larger electoral gains when a party holds the presidency and for larger losses when it does not. In addition, the main effect itself of whether a member is from the presidential party is included, because of its role in the construction of the previous three variables (Canes-Wrone et al. 2002, 132). (As Canes-Wrone et al. point out, the three variables are not operationalized in terms of midterm election year, income change, and presidential popularity in their original form interacted with in-party status, because a high level of multicollinearity would result.) The last control variable is the incumbent's vote proportion in the immediately preceding election.

A final preliminary matter requiring discussion at this point concerns the status of roll call extremity and party unity as exogenous or endogenous variables. Both variables may well be determined by some of the same exogenous factors that determine the member's reelection margin itself. For example, presidential party members facing the prospect of vote losses because of an approaching midterm election, weak growth in personal income, or presidential unpopularity may moderate their roll call record and reduce their party loyalty as a way of appealing to more centrist voters. Ideology and party unity would then be correlated with the error term (ε) in the equation, and failure to take into account such endogeneity would make for inconsistency in estimates of their effects.

The standard remedy for such a problem, which will be implemented later in the last of our three models to be estimated, is to treat the independent variable thought to be acted upon by other right-hand variables as endogenous, replacing it with an instrumental variable that will be uncorrelated with ε. Carson et al. recognize the importance of this instrumental variable approach in their own analysis. We differ from them in terms of the specifics of instrumental variable formulation, but in both of our analyses at least one lagged roll call variable is part of the formulation. The details of our procedure will be explained at length in the next section. We note at this juncture, however, that those freshman members who are analyzed in the first two, non-instrumented models must of necessity be excluded from the third model, since they have no lagged roll call records from the preceding Congress with which to construct the instrumental variable. Correspondingly as well, a control variable differentiating members according to freshman-non-freshman status becomes irrelevant. Oddly, however, Carson et al. do include a freshman-non-freshman variable in their own instrumental variable study, but it is not at all clear what this variable could represent, since by definition freshmen there too would have had to be dropped (Carson et al. 2010, 607-9).

The Impact of Roll Call Extremity and Party Unity on Reelection Margins

Before getting into either form of fixed effects regression, we start with the most basic model of all: OLS pooled cross-sectional regression without any use of instrumental variables.

Here, unlike the situation with the fixed effects regressions, members can be included even if there is only a single observation. (The seniority variable used to differentiate freshman from non-freshman members codes the former as 1 and the latter as 0.)

The first three regressions that appear in Table 1 each feature a single roll call measure, while equations four and five combine party unity with adjusted ADA scores or DW-Nominate scores, respectively. All equations are based on the same set of members; i.e., those with non-missing values on all three roll call indices. Parameters of the 14 dummy variables needed to delineate the 15 different election years from 1980 to 2008,

TABLE 1
OLS POOLED CROSS-SECTIONAL REGRESSION OF HOUSE INCUMBENTS' REELECTION MARGIN ON MEASURES OF ROLL CALL EXTREMITY AND PARTY UNITY, 1980-2008

	Model with Adjusted ADA Scores	**Model with DW-Nominate Scores**	**Model with Party Unity Scores**	**Model with Adjusted ADA Scores and Party Unity Scores**	**Model with DW-Nominate Scores and Party Unity Scores**
Adjusted ADA score	-.058*** (.017)	- - -	- - -	-.038* (.022)	- - -
DW-Nominate score	- - -	-.053*** (.016)	- - -	- - -	-.020 (.019)
Party unity score	- - -	- - -	-.077*** (.018)	-.044* (.023)	-.060** (.020)
District partisan homogeneity	.302*** (.026)	.293*** (.031)	.291*** (.030)	.310*** (.029)	.300*** (.032)
Campaign spending disparity	-.019*** (.001)	-.019*** (.001)	-.019*** (.001)	-.019*** (.001)	-.019*** (.001)
In-party member during midterm	-.031*** (.006)	-.032*** (.006)	-.031*** (.006)	-.031*** (.006)	-.031*** (.006)
Change in personal income	.005 (.007)	.005 (.008)	.007 (.008)	.006 (.007)	.006 (.008)
Presidential job approval	.127*** (.033)	.120** (.038)	.126*** (.037)	.125*** (.034)	.123** (.039)
In-party member	-.114*** (.027)	-.109*** (.030)	-.122*** (.027)	-.116*** (.027)	-.117*** (.030)

Member's prior margin	.359*** (.016)	.378*** (.016)	.375*** (.018)	.359*** (.016)	.373*** (.017)
Freshman status	.017*** (.004)	.018*** (.004)	.018*** (.004)	.017*** (.004)	.018*** (.004)
Uncentered R^2	.714	.711	.713	.716	.714
Number of Observations	4030	4030	4030	4030	4030
Number of Clusters	15	15	15	15	15

Note: Entries in parentheses are cluster-robust standard errors. Analyses are based upon cases with non-missing values on all three roll call measures. Parameters of election year dummy variables are not shown.
***Significant at .001 level (one-tail t-test); **significant at .01 level (one-tail t-test); *significant at .05 level (one-tail t-test).

with 1980 serving as the reference category, are not shown. Cluster-robust standard errors have been calculated, with each cluster defined in terms of the cases making up a given election year.

When each of the three roll call measures appears by itself in a regression, larger values, as hypothesized, always make for significant reduction in reelection safety (columns 1-3). The other independent variables likewise operate as anticipated, except that personal income change never achieves significance. Note that while freshmen on average receive lower reelection margins than do non-freshmen, the freshman variable is significantly positive. This is expected because the lagged election margin regressor appearing in the equation makes the dependent variable equivalent to inter-election change in margins, which is more positive for freshmen owing to their acquisition of incumbency status between elections.

Entering party unity simultaneously with the adjusted ADA measure, and then simultaneously with the DW-Nominate measure, tells a somewhat different story, however. Columns 4 and 5 reveal that while party unity remains significant in both equations, the only significant ideological variable is adjusted ADA scores. The magnitude of the DW-Nominate effect is reduced by well over 50 percent from column 2 to column 5. In their own analysis, Carson et al. likewise find that when unity and DW-Nominate are jointly entered in their original, non-instrumented form, only unity is significant. This is cited as evidence buttressing their contention that loyalty to one's party rather than extremity is what actually alienates voters (2010, 610). But as we have just seen, the evidence in fact is an artifact of the choice they have made to use DW-Nominate scores rather than adjusted ADA scores to tap ideology.

In the second model, the OLS estimation with the same right-hand variables is replicated, except this time fixed effects regression without an instrumental variable is employed.[12] There are slightly fewer cases than before, because, as stated above, fixed effects analysis requires eliminating any member with only a single usable observation (e.g., a member who is first reelected against major party opposition in 2000, wins uncontested reelection in 2002, and then runs for the Senate in 2004). A total of 847 members enter the analysis, with the mean number of elections analyzed per member equal to 4.7. The average absolute value of inter-Congress change in the three roll call variables ranges from a fairly considerable 0.055 in the case of adjusted ADA scores, to just 0.012 for DW-Nominate scores (not surprising in light of the frequency with which the DW-Nominate procedure leads to members retaining identical values over the course of their congressional career). Party unity change averages 0.034.

Table 2 reports the results for the second model.[13] Cluster-robust standard errors once again appear, now, however, with each cluster defined in terms of the observations corresponding to a given member.[14] This means that the fixed effects standard errors will be robust not only to arbitrary heterogeneity, but to within-cluster autocorrelation as well. What stands out here is that none of the three roll call measures being focused upon significantly affects reelection margin, regardless of whether a single variable enters an equation or whether party unity is paired with either ideology measure. The parameters of adjusted ADA scores and party unity—always significant in Table 1—maintain their hypothesized negative signs, but with much diminished magnitudes. Inflation in the standard errors, which is possible in fixed effects regression because only within-member variation and not between-member variation is used (Allison 2009, 3), clearly is not a

TABLE 2

FIXED EFFECTS REGRESSION OF HOUSE INCUMBENTS' REELECTION MARGIN ON MEASURES OF ROLL CALL EXTREMITY AND PARTY UNITY, 1980-2008 (WITHOUT INSTRUMENTAL VARIABLES)

	Model with Adjusted ADA Scores	Model with DW-Nominate Scores	Model with Party Unity Scores	Model with Adjusted ADA Scores and Party Unity Scores	Model with DW-Nominate Scores and Party Unity Scores
Adjusted ADA score	-.012 (.014)	- - -	- - -	-.008 (.015)	- - -
DW-Nominate score	- - -	.023 (.030)	- - -	- - -	.035 (.030)
Party unity score	- - -	- - -	-.016 (.020)	-.011 (.021)	-.024 (.020)
District partisan homogeneity	.426*** (.028)	.427*** (.029)	.427*** (.029)	.427*** (.028)	.426*** (.029)
Campaign spending disparity	-.019*** (.001)	-.019*** (.001)	-.019*** (.001)	-.019*** (.001)	-.019*** (.001)
In-party member during midterm	-.025*** (.001)	-.025*** (.001)	-.025*** (.001)	-.025*** (.001)	-.025*** (.001)
Change in personal income	.006** (.002)	.006** (.002)	.007** (.002)	.006** (.002)	.007*** (.002)
Presidential job approval	.140*** (.009)	.141*** (.009)	.138*** (.009)	.139*** (.009)	.140*** (.009)
In-party member	-.137*** (.008)	-.138*** (.008)	-.137*** (.008)	-.137*** (.008)	-.139*** (.008)
Member's prior margin	.044** (.016)	.043** (.017)	.045** (.017)	.045** (.017)	.044** (.017)
Freshman status	-.011*** (.003)	-.011*** (.003)	-.011*** (.003)	-.011*** (.003)	-.011*** (.003)
Uncentered R^2	.504	.504	.504	.505	.505
Number of Observations	3971	3971	3971	3971	3971
Number of Clusters	847	847	847	847	847

Note: Entries in parentheses are cluster-robust standard errors. Analyses are based upon cases with non-missing values on all three roll call measures. Parameters of election year dummy variables are not shown.

***Significant at .001 level (one-tail t-test); **significant at .01 level (one-tail t-test); *significant at .05 level (one-tail t-test).

problem and hence shoulders no responsibility for the loss of significance. For the DW-Nominate variable, the parameters in both equations actually have a non-significant positive sign, whereas the parameter was significantly negative in Table 1 when the variable appeared by itself.[15]

As stated above, however, a more precise test of how members' roll call records influence their reelection fortunes requires the formulation of instrumental variables substituting for party unity and ideology in order to address the endogeneity issue. We thus turn to this task at some length. Creation of each instrumental variable involves generating the predicted values that result from regression of the original variable on the variables constituting its reduced form (i.e., first stage) equation. These reduced form variables are the predetermined (i.e., exogenous and lagged endogenous) factors that are direct causes either of reelection margin or of roll call voting. The instrumental variable, like the predetermined factors themselves, will then be uncorrelated with the error term when it is substituted in the second stage equation explaining reelection margin, hence making for consistent estimation.

In creating our party unity and adjusted ADA instrumental variables, it is obligatory that all control variables previously employed to explain reelection margin be included in the reduced form equation. Also as a standard part of the procedure, we enter into the reduced form equations two "excluded" variables, which are direct causes of roll call voting but not of reelection margin: lagged party unity and lagged adjusted ADA scores from the previous Congress. Alternatively, Carson et al. in manufacturing their own party unity instrumental variable utilize as excluded variables lagged unity scores, but contemporaneous DW-Nominate scores. As we shall demonstrate later, however, current adjusted ADA scores are in fact an endogenous cause of reelection margin, and it would thus be erroneous to treat them, as opposed to lagged scores, as a predetermined variable in the reduced form equation predicting unity.

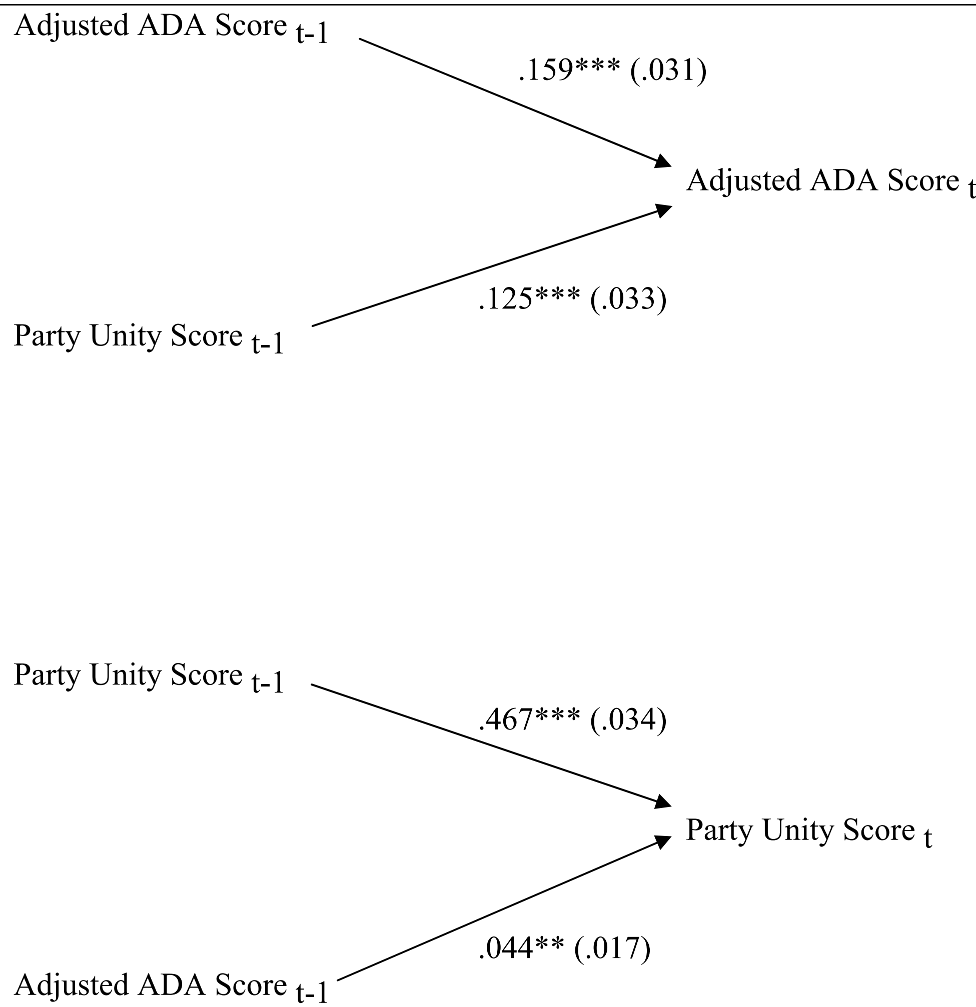
In Figure 1, we perform cross-lagged analysis to establish how lagged ideology and lagged party unity affect contemporaneous unity values. Fixed effects regression is used, again with each member's observations making up a cluster. Likewise shown are the results of cross-lagged analysis involving the influence of the same two lagged variables on contemporaneous adjusted ADA scores.[16] Comparable analysis of contemporaneous DW-Nominate scores cannot be performed, however, because of this measure's aforementioned constraint on change over time; i.e., with members forced either to retain identical scores or to change by a uniform amount each Congress, the regression of contemporaneous values on lagged DW-Nominate values would be unity. For the same reason, a reduced form equation regressing DW-Nominate scores on the excluded variable comprised of its lagged values cannot be estimated, making creation of an instrumental variable infeasible.[17]

The key finding in Figure 1 is that both cross-lagged parameters are significant at least at the $p<.01$ level. A member's adjusted ADA score at t-1 indeed affects subsequent party unity, and unity at t-1 affects the subsequent adjusted ADA score. Causality between the two roll call measures hence runs in both directions. Accordingly, we proceed to construct instrumental variables for ideology and party unity that, in each case, use lagged values of these two variables as the excluded variables.

Second stage, fixed effects equations incorporating the instrumental variables appear in Table 3. Estimation is carried out with the xtivreg2 command that is included as

FIGURE 1
CROSS-LAGGED ANALYSIS OF THE RELATIONSHIP BETWEEN CONTEMPORANEOUS VALUES OF ROLL CALL MEASURES AND THE LAGGED VALUES OF THESE

MEASURES, 1980-2008



Note: Fixed effects regression parameters are shown. Entries in parentheses are cluster-robust standard errors. Analyses are based upon cases with non-missing data (3213) on both Adjusted ADA Scores and Party Unity Scores (the number of clusters is 717). Lagged values of both roll call variables are from the preceding Congress. Parameters of election year dummy variables are not shown.

***Significant at .001 level (one-tail t-test); **significant at .01 level (one-tail t-test); *significant at .05 level (one-tail t-test).

part of Baum, Schaeffer, and Stillman's user-written ivreg2 module for STATA (2002). This module generates a number of auxiliary statistics for assessing the adequacy of instrumental variable estimation, which will prove useful for our purposes.[18] As mentioned above, because no lagged roll call measures exist for freshman members, they have been dropped from the analysis along with the corresponding freshman status variable. Also, we do not include both roll call instruments in the same equation because of the identification problem that would result. With two right-hand variables considered as endogenous (k) and two excluded variables (l), exact identification would exist. Only when there is overidentification (i.e., $l > k$) can it be determined whether the excluded variables, as hypothesized, are in fact uncorrelated with the error term (Baum 2011, 33). Furthermore, given that both instrumental variables have been based upon reduced form equations incorporating the same right-hand variables, it is not surprising that the collinearity between them is very high (i.e., $r = 0.851$).

Substitution of the instrumental variables substantially improves how both roll call indices perform compared to their performance in the Table 2 fixed effects regressions. The parameters for ideology and party unity grow in magnitude from -0.012 to -0.184, and from -0.016 to -0.136, respectively. Even with the enlargement in standard errors produced by replacing the original variables with their instruments, higher values of ideological extremity and unity now both significantly depress reelection margins.[19]

Four auxiliary statistics generated by the routine are displayed as well in Table 3. The Kleibergen-Paap rk LM and Kleibergen-Paap rk Wald F statistics test how strongly the contemporaneous roll call measures are correlated with the excluded variables of lagged adjusted ADA and lagged party unity scores. Rejection of the null hypothesis in

TABLE 3
FIXED EFFECTS REGRESSION OF HOUSE INCUMBENTS' REELECTION MARGIN ON MEASURES OF ROLL CALL EXTREMITY AND PARTY UNITY, 1980-2008 (WITH INSTRUMENTAL VARIABLES)

	Model with Adjusted ADA Scores	**Model with Party Unity Scores**
Adjusted ADA score	-.184*	- - -
	(.088)	
Party unity score	- - -	-.136**
		(.058)
District partisan homogeneity	.386***	.409***
	(.035)	(.033)
Campaign spending disparity	-.018***	-.019***
	(.001)	(.001)
In-party member during midterm	-.024***	-.020***
	(.002)	(.002)
Change in personal income	.006**	.010**
	(.002)	(.003)
Presidential job approval	.136***	.123***
	(.011)	(.011)
In-party member	-.133***	-.132***
	(.009)	(.009)
Member's prior margin	.060**	.058**
	(.021)	(.020)
Underidentification test	27.366	38.438
(Kleibergen-Paap rk LM statistic)	(p=.000)	(p=.000)
Weak identification test	28.530	91.150
(Kleibergen-Paap rk Wald F statistic)	(p<.10)	(p<.10)
Overidentification test	2.509	.089
(Hansen J statistic)	(p=.113)	(p=.765)
Endogeneity test	4.310	10.518
	(p=.038)	(p=.001)
Uncentered R^2	.467	.491
Number of Observations	3213	3213
Number of Clusters	717	717

Note: Entries in parentheses are cluster-robust standard errors. Analyses are based upon cases with non-missing values on both roll call measures. Parameters of election year dummy variables are not shown. Instrumental variables for Adjusted ADA scores and Party unity scores have been employed.

***Significant at .001 level (one-tail t-test); **significant at .01 level (one-tail t-test); *significant at .05 level (one-tail t-test).

the case of the first statistic rules out underidentification, while rejection in the case of the second statistic rules out weak identification. The consistently significant p values in Table 3 confirm that both excluded variables perform well in predicting roll call scores, thus reinforcing the results of the earlier cross-lagged analysis in Figure 1.[20] The third identification test, based on the Hansen J statistic, tests the overidentification restrictions. Failure to reject the null hypothesis would mean that an instrumental variable is independent of the error term and that the excluded variables are properly omitted from the second stage equation. Here, for the adjusted ADA as well as unity scores, the null hypothesis indeed cannot be rejected, yet another sign that both instrumental variables are valid. The last auxiliary statistic tests the null hypothesis that the variables we instrumented should more appropriately be handled as exogenous rather than endogenous. Both test statistics indicate rejection of the null hypothesis, therefore supporting the decision to treat the adjusted ADA as well as unity scores as endogenous.

Finally, we conduct two additional analyses to identify the conditions under which members' roll call voting has more or less impact on reelection margin. First, it seems likely that the impact would be magnified in more competitive districts, where there is greater probability of a viable, well-financed challenger who can drive home the message to the sizable number of opposition party identifiers that the incumbent is not voting their way. The Table 3 analysis is therefore repeated, this time separately within each of two groups of members defined according to the mean level of district partisan homogeneity obtaining across their reelection history. The partisan homogeneity dividing point here is 0.0642; i.e., for the average member, the mean cross-election showing in his or her district of the party's presidential nominee was 6.42 percent above the mean presidential vote calculated for all House districts. Second, roll call voting should matter more in the latter part of the 1980-2008 period under study, given the previously mentioned increasing estrangement between party identifiers and the issue positions taken by Congress members of the opposition party. Consequently, we do separate analysis of elections within the 1980-94 and 1996-2008 time spans. The 1994 midterm, aside from yielding periods of approximately equal length when used as the cutting point, also was an important milestone in the upsurge of polarization by ushering in conservative GOP majorities to confront a Democratic president.

To conserve space, Table 4 displays only the parameters and standard errors of the adjusted ADA and party unity variables themselves. (Complete results are available by request.) When members are divided according to the competitiveness of their districts, both variables perform in similar ways. Relative to their effects across all districts and election years in Table 3 (-0.184 for adjusted ADA scores and -0.136 for party unity), the effects are intensified in more competitive districts and diminished in safer districts. In safer districts, in fact, neither more extreme ideology nor greater unity significantly hurts the member's reelection safety. Expectations are likewise met when separate analysis is done within each of the two time periods. Stronger negative impacts for both variables are manifested during the more recent seven elections than across the entire 1980-2008 period, while the negative impacts do not reach the level of significance during the earlier eight elections.[21] Thus, the risk of vote erosion caused by roll call records out of line with district sentiment is most severe for members in more recent elections who represent more competitive districts, and for these members, of course, even losing a small percentage of the vote poses the danger of outright election defeat.

TABLE 4

FIXED EFFECTS REGRESSION OF HOUSE INCUMBENTS' REELECTION MARGIN ON MEASURES OF ROLL CALL EXTREMITY AND PARTY UNITY (WITH (INSTRUMENTAL VARIABLES)—SEPARATE ANALYSES PERFORMED FOR DISTRICTS OF DIFFERENT COMPETITIVENESS AND FOR DIFFERENT TIME PERIODS

	Adjusted ADA Scores	Party Unity Scores
Parameter for All Districts from 1980-2008 (repeated from Table 3)	-.184* (.088)	-.136** (.058)
Number of Observations	3213	3213
Number of Clusters	717	717
Parameter for Districts with Mean Partisan Homogeneity ≥ .0642	-.090 (.112)	-.041 (.080)
Number of Observations	1606	1606
Number of Clusters	347	347
Parameter for Districts with Mean Partisan Homogeneity < .0642	-.301** (.124)	-.186** (.073)
Number of Observations	1607	1607
Number of Clusters	370	370
Parameter for 1980-1994 Period	-.111 (.123)	-.093 (.096)
Number of Observations	1663	1663
Number of Clusters	418	418
Parameter for 1996-2008 Period	-.334* (.189)	-.227* (.138)
Number of Observations	1461	1461
Number of Clusters	379	379

Note: Entries in parentheses are cluster-robust standard errors. Analyses are based upon cases with non-missing values on both roll call measures. Instrumental variables for Adjusted ADA scores and Party unity scores have been employed. Only parameters and standard errors of these instrumental variables are displayed.

***Significant at .001 level (one-tail t-test); **significant at .01 level (one-tail t-test); *significant at .05 level (one-tail t-test).

Partly mitigating this danger, on the other hand, is the fact that over time, more districts have gravitated away from having relatively even splits between the numbers of Democratic and Republican identifiers (Oppenheimer 2005, 141-45).

Overall, where are we left with regard to the question of the relative abilities of the two roll call measures to influence reelection margin? From one standpoint, ideological extremity is the superior measure. Members becoming one standard deviation more extreme on this variable in the full analysis presented in Table 3 could expect to lose as a consequence 3.29 percentage points of the vote (i.e., the standard deviation of 0.179 units for adjusted ADA scores times the -0.184 parameter listed in Table 3). In contrast, a one standard deviation gain in party unity (0.132 units) multiplied by the 0.136 parameter for this variable means an expected vote loss of only 1.80 percentage points. On the other hand, the standard error of the adjusted ADA parameter (0.088) is greater than that for unity (0.058), making for more imprecision in fixing the true adjusted ADA estimate. (The same pattern of larger parameters and larger standard errors for the adjusted ADA variable, of course, was replicated in Table 4 when members were separated according to district competitiveness and time period.) Likewise, the auxiliary test statistics that accompanied the Table 3 analysis show that the identification and endogeneity assumptions underlying the instrumental variable procedure are upheld at higher levels of statistical confidence when unity rather than ideology is employed.

Somewhat of a tradeoff exists, therefore, with regard to the question of which is the better roll call measure. One thing, however, seems clear. Discounting the direct effect of ideological extremity as a cause of vote loss at election time is unwarranted. The role of ideology—at least ideology in the form of adjusted ADA scores—is not that of an exogenous cause of party unity, as maintained by Carson et al. The case for the endogeneity of ideology may not be so strong as that for unity, but ideology still passes the endogeneity test at the conventional level of statistical significance.

Summary and Conclusions

The most elementary analysis performed in this study, OLS pooled cross-sectional regression, finds that greater roll call extremity and greater party unity both have negative electoral consequences for members on Election Day. When the more exacting estimation of fixed effects regression is substituted, however, neither variable proves to be significant. But in the model we have argued best captures the underlying dynamics between roll call votes and reelection safety—fixed effects regression with instrumental variables—both measures once again emerge as significant. Thus, employing members as their own control and accounting for the endogeneity of roll call voting makes it clear that members' adjustments to their voting records from Congress to Congress will produce corresponding shifts in their reelection safety. Members indeed have the opportunity to recalibrate their roll call record over time so as to achieve an acceptable balance between fidelity to ideological principle and an adequate margin of electoral safety.

Thus, supplementing the instrumental variable analysis of Carson et al. with the rigor of fixed effects regression fortifies these researchers' conclusions that levels of party unity matter. But at the same time, our results challenge their devaluation of ideology. This makes sense in light of the point raised above on pages 4-5 that the authors overplay the difficulty that voters have thinking in ideological terms, and that ideological distance between partisans and opposite party incumbents has grown over time.

Equally important, we think, is the question of voters' accessibility to information concerning their members' record on each of the two roll call voting measures. As a supplementary part of their study, Carson et al. report on experimental work they did in which test subjects explicitly provided with various depictions of a hypothetical member's ideology and party unity were less prone to say they would support a party loyalist than an extremist.[22] But in the real world, how likely are citizens to come across such information on their own?

Hutchings in studying newspaper reporting of selected roll call votes from the late 1980s to mid-1990s discovers that senators who dissent from their party's majority position receive more prominent coverage than do senators toeing the party line. No party-loyalty-related difference in coverage is unearthed for House members, however, thus handicapping the ability of the dissenters to favorably distinguish themselves in the eyes of constituents (2003, 27-30). Furthermore, work done by Arnold on House incumbents running for reelection in 1994 finds that only three percent of campaign-related articles in local newspapers mentioned members' stances on major issues of the 103rd Congress in the context of whether they supported or opposed their party's position. Ideological references to members, in contrast, were more than twice as numerous (seven percent) (2004, 176).[23]

Granted there are other means besides newspapers for disseminating policy information about members, such as television, the internet, campaign communications, etc. Nonetheless, if something like the ratio of ideological to party loyalty references discovered by Arnold in newspaper coverage were to be repeated in these alternative outlets, it should be easier for voters to acquire knowledge of the ideological kind. Thus, while perceptions of party loyalty compared to perceptions of ideological extremity may have, at least in the experiment of Carson et al., more detrimental effects on support for the incumbent, this effect could be counterbalanced by the lower prevalence of party loyalty perceptions in the first place. If so, it would make sense that in our own findings, the impact of party unity on the electorate's voting decisions did not swamp that of ideology.

Endnotes

[1] However, looked at from an alternative standpoint—how the partisan competitiveness of the constituency influences where members position themselves ideologically relative to the median voter they represent, rather than relative to other members—greater competitiveness has been found by Gulati (2004, 506-10) to push senators away from the median voter and toward fellow partisans. His finding replicates what other scholars, most notably Fiorina (1974, 100-8), have discovered. Gulati's own analysis, however, offers little support for Fiorina's interpretation of the phenomenon: that marginal incumbents, because of the heterogeneity of their constituencies, forego efforts to please the broad spectrum of voters in favor of siding with those belonging to their own party. Instead, Gulati suggests that marginal senators simply react to their trying electoral circumstances by seeking sustenance from their most intimate—and thus ideologically extreme—supporters (2004, 512).

[2] The 1989, 1997, 2001, and 2005 editions of Erikson and Wright's article also include estimates of the electoral effects of challenger ideology in 1982, 1994, 1998, and 2002, respectively, which are generally insignificant.

[3] Indeed, we find in our own study covering the period from 1980-2008 that the correlation between party unity and Carson et al.'s indicator of extremity (first dimensional DW-Nominate scores transformed so that higher values for both Republicans and Democrats denote greater extremity) is 0.773.

[4] Furthermore, the data set does not include elections in which two incumbents face off as a result of redistricting.

[5] This first dimension mainly represents inter-party conflict over economic policy and accounts for the majority of variance in roll call voting.

[6] The correlation between these transformed adjusted ADA scores and party unity across the 1980-2008 period is 0.684, smaller than the 0.773 r between party unity and transformed DW-Nominate scores reported above in footnote 3. Thus, disentangling the independent effects of extremity from party unity should for this reason alone prove more tractable in the case of the adjusted ADA measure.

[7] Yearly adjusted ADA scores were downloaded from Tim Groseclose's website: http://www.sscnet.ucla.edu/polisci/faculty/groseclose/ Adj.Int.Group.Scores/. Congress-by-Congress DW-Nominate scores were downloaded from the website of Royce Carroll, Jeff Lewis, James Lo, Nolan McCarty, Keith Poole, and Howard Rosenthal: http://www.voteview. com/dwnominate.asp.

[8] Congress-by-Congress party unity scores were downloaded from Carroll et al.'s website listed above in footnote 7.

[9] The presidential returns used to determine district partisan homogeneity in 1982 and 2002 are the results from the 1980 and 2000 elections, respectively, recomputed within the new district lines produced by decennial redistricting. Similarly, recomputed presidential returns are used in the case of states undergoing significant mid-decade redistricting; e.g., 2004 returns have been recomputed within Georgia's new 2006 district boundaries.

[10] By law, congressional candidates do not have to disclose to the FEC campaign spending less than $5000. Consequently, we follow the procedure of Canes-Wrone et al.

(2002, 131) and Jacobson (1990, 338) in assigning $5000 in current spending to such candidates, which is then converted into 1980 constant dollars.

[11] We did not have access to Gary Jacobson's data on whether the challenger had held elected office, which were utilized by Canes-Wrone et al. and by Carson et al. Jacobson, however, has pointed out very recently that the independent effect of this variable on the incumbent's margin has diminished over time, becoming negligible during the last two decades (2013, 138).

[12] In their tables employing incumbent reelection margin as the dependent variable, Carson et al. refer to the models they estimate as "Fixed effects panel-data models with instrumental variables and 2-SLS." Fixed effects in that context should not be confused with our own employment of the term, which seems more typical in the literature. Clusters (or "groups") in Carson et al. are composed of observations on all members in a single election year, meaning that they are performing pooled cross-sectional analysis. Conversely, in our own analysis clusters consist of all observations on a single member across time. (We also control for election year effects, however, by means of t-1 dummy variables.) The question of how changes in individual members' roll call voting affect changes in their reelection margin, which of course is the major concern of our study, cannot be answered by the former kind of analysis.

[13] In the handful of cases where a member switched parties, two different fixed effects were calculated; i.e., the first culminates with the final election in which the member ran under the original party banner, and the second begins with the immediately succeeding election.

[14] Fixed effects models in general are more appropriate than random effects models because of their superiority in controlling for the effects of unobserved, time-invariant variables. They make no assumptions about correlations between unobserved and observed variables, whereas random effects models assume no correlation (Allison 2009, 2-3; Kennedy 2001, 227). Thus, parameters generated by fixed effects regression are less susceptible to bias, even though they may have larger standard errors. Empirically, tests of fixed effects versus random effects, relying upon Mark Schaffer and Steven Stillman's user-written xtoverid module for STATA, were performed on all Table 2 models by means of the artificial regression procedure (Arellano 1993; Wooldridge 2002, 290-91). (The conventional Hausman test used for such comparisons is invalid here because of the clustered error structure.) In each case, the null hypothesis favoring the random effects model was rejected at $p<.001$.

[15] Table 6 in Carson et al. (2010, 614), unlike their earlier tables employing incumbent reelection margin as the dependent variable, does incorporate a dynamic element in that the dependent and independent variables now are in the form of election-to-election changes in the values of the original variables. This technique, which is labeled the "first differences" approach by Finkel (2008, 480), yields consistent parameter estimates, as does fixed effects. Finkel faults the first differences approach, however, for being inefficient; i.e., fixed effects utilizes all longitudinal variation by expressing variable values in terms of deviations from their overall mean, whereas first differences expresses variable values in terms only of deviations from the immediately preceding lag. He also points out that fixed effects regression has more frequently been applied to multi-wave panel data, such as those employed by Carson et al. and ourselves.

[16] Finkel (2008, 490) points out that while cross-lagged models do not permit analyzing contemporaneous causal linkages between variables, "in the absence of strong theory the cross-lagged model is usually a satisfactory initial model." Later, when we perform estimation employing instrumental variables, it will be possible to further test whether lagged unity scores and lagged adjusted ADA scores jointly determine contemporaneous values of each variable.

[17] For a similar application of cross-lagged regression using fixed effects, in which the authors attempt to sort out the causal relationship between the make-up of corporate boards and the corporations' financial success, see Davidson III and Rowe (2004).

[18] Equations generated by xtivreg2 do not include a constant. For the sake of consistency, therefore, the other equations appearing in this paper have been generated without a constant as well.

[19] In these fixed effects regressions with instrumental variables, as well as in the previous fixed effects regressions without instrumental variables, we experimented with setting a larger minimum number of observations in order for a member to be analyzed than the value of two actually employed. Changing this minimum value to three, four, five, or six, respectively, did little to change the parameters of the extremity and unity variables that were generated. In all cases, significant parameters remained significant and insignificant parameters remained insignificant.

[20] Since critical values for the Kleibergen-Paap rk Wald F statistic do not exist, Stock-Yogo critical values for the Cragg-Donald F statistic are used to provide guidance in interpreting the significance of the former statistic. The Wald F values of 28.53 and

91.15 for adjusted ADA and unity, respectively, allow rejection of both null hypotheses of weak identification at the highest level that has been tabulated; i.e., for 10 percent maximal instrumental variable size (Stock and Yogo 2005, 101). (Baum, Schaffer, and Stillman, however, recommend that the Stock-Yogo critical values be applied with "caution" when clustered standard errors are computed, as we have done here (2007, 24). They suggest alternatively that F should have a value of 10 or greater to reject weak identification, which of course also holds true for both of our own F statistics.)

[21] For the within-period analyses, the combined number of observations (3124) is lower than the number in Table 3 (3213), because some members in a time period were no longer left with the minimum two observations necessary to meet the criterion for inclusion. On the other hand, the combined number of clusters (797) is greater than the number in Table 3 (717), because for some members, there were enough observations in each time period to form separate clusters (in the full 1980-2008 analysis, of course, all observations for a member formed a single cluster).

[22] In contrast, however, note that alternative experimental work done by Huckfeldt, Mondak, Craw, and Morehouse shows that when test subjects are presented with a discrepant combination of party and ideology purportedly describing a candidate (i.e., "liberal Republican" or "conservative Democrat"), evaluations of the candidate are dominated by ideology (2002, 75-76).

[23] Recent work by Bishin (2009, 40-53) suggests that "activated identity" groups on issues particularly relevant to their identity (e.g., African Americans with regard to Clarence Thomas's Supreme Court nomination in 1991) may be more knowledgeable than even those in the broader population with normally higher levels of media exposure.

Whether a House member's degree of party unity on roll call votes is a sufficiently strong motivating force to spur some comparable segment of the electorate to seek out such information in the low coverage environment sketched by Arnold, however, seems problematical at best.

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