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Parties as Procedural Coalitions in Congress: An Examination of Differing Career Tracks

We examine the degree to which parties act as procedural coalitions in Congress by testing predictions from the party cartel theory (Cox and McCubbins 1993, 1994, 2002). We gain leverage on the question of party influence in Congress by focusing on three types of House members: reelection seekers, higher-office seekers, and retiring members. We argue that retiring House members are no longer susceptible to party pressure, making them the perfect means (when compared to higher-office seekers and reelection seekers) to determine the existence of party influence. Results from a pooled, cross-sectional analysis of the 94th through 105th Congresses (1975–98) suggest that party influence is indeed present in Congress, especially where the party cartel theory predicts: on procedural, rather than final-passage, votes. Moreover, we find that procedural party influence is almost exclusively the domain of the majority party. This latter finding is especially important because most prior studies have been limited to investigating interparty influence only.

Introduction

Research investigating party influence in Congress has exploded over the last decade. This trend has been due, in part, to concerns raised about what constitutes evidence of party influence. Traditionally, congressional scholars have viewed roll-call-based measures of partisanship, such as measures of party strength or party cohesion, as sufficient to make the case for strong party influence (or discipline) in Congress. Recently, however, Krehbiel (1993, 1999a, 2000) has taken these scholars to task. Having developed a preference-based model of congressional behavior, he contends that the typical measures of party

influence “increasingly . . . seem to be artifacts of preferences rather than evidence of party discipline, party cohesion, party strength, or party government” (Krehbiel 2000, 225).

While Krehbiel’s contentions have resonated with congressional scholars, they have not squelched efforts to uncover evidence of party influence in Congress. Moreover, the use of roll-call votes as the *means* of uncovering that evidence has not been viewed as problematic, as new and innovative roll-call-based approaches have been developed in an effort to separate the effects of preferences and partisanship: examples include a nonpartisan interest group score¹ (Binder, Lawrence, and Maltzman 1999), a party pressure measure² (Snyder and Groseclose 2000), partisan roll rates³ (Cox 2001; Cox and McCubbins 2002), partisan cut points⁴ (McCarty, Poole, and Rosenthal 2001), and a net Rice index of party difference (Cox and Poole 2002).

This article fits in the aforementioned tradition by searching for party influence in Congress using a different approach to the study of roll-call voting. Rather than developing and applying new methods to tease out party influence, we apply an existing research design in a different way to a new set of data.

Our main task will be to examine whether or not parties act as procedural coalitions, that is, as cartels that organize the institution, via rule making and committee assignments, for partisan benefit (Cox and McCubbins 1993, 1994, 1997, 2002). Specifically, we examine the varying degrees to which parties influence the behavior of their members on two substantively different vote types: final-passage and procedural votes. Party cartel theory suggests that parties should exhibit more influence (exert more pressure) on procedural votes, relative to final-passage votes, all else being equal. Moreover, the majority party, because of its control over the legislative organization and agenda, should exhibit disproportionate influence relative to the minority party.

To conduct our analysis, we examine House members’ vote choices after they have decided whether to run for reelection, retire, or pursue higher office. Our assumption is that retiring House members, relative to higher-office seekers (with reelection seekers serving as the baseline), no longer feel the “noose” of party discipline and that this freedom will be reflected in their voting behavior. Thus, by comparing the vote choices of these three member types across different subsets of votes, we have a unique opportunity to isolate and identify evidence of party influence in various legislative contexts.

The article proceeds as follows. In Section 1, we discuss parties as procedural coalitions, focusing specifically on the party cartel theory. Section 2 lays out our research design and explains how we gain leverage

on the question of party influence in Congress by examining the behavior of exiting House members. Section 3 provides predictions from the party cartel and pure-preference theories and tests them, both across and within parties, using final-passage and procedural-vote data from the 94th through 105th Congresses (1975–98). Section 4 concludes.

1. Procedural Cartel Theory

Cox and McCubbins (1993, 1994, 1997, 2002) characterize political parties as legislative cartels that usurp the procedural (rule-making and committee assignment) powers in the chamber to produce outcomes favorable to (majority) party members. In effect, majority party members delegate authority to central agents (chamber leaders), who structure the legislative agenda to foster the success of the party. This is done in two ways. First, policy logrolls are constructed, with individual party members extracting district-specific benefits while supporting the partisan agenda as a whole. Second, gatekeeping is employed at the committee level, so that policies opposed by a party majority are not referred to the floor. To prevent defection from the partisan agenda, majority party leaders wield various “carrots and sticks” that can affect members’ electoral fortunes. For example, prime committee assignments and privileged positions on the legislative calendar can be bestowed or taken away, depending upon the degree of partisan loyalty that members exhibit. At the extreme, members can be kicked out of the caucus.⁵

At the heart of this partisan cartel is the insistence upon procedural control. While majority party leaders stress the importance of generating policy outputs, they also understand the electoral realities that individual party members face. Often, district-specific politics will not allow certain members to support the party’s policy positions. On those occasions, when those members’ votes are not crucial to the outcome, leaders will allow them to bow to electoral pressure and defect. Procedural matters, however, are quite different. As Sinclair (2000, 134) states, “Defecting from your party on procedural issues is considered a greater offense than defecting on substantive issues.” This is because majority party leaders view the creation of new policies as *conditional*—contingent, for example, on the size and degree of preference homogeneity within the majority party—but they view blocking policies that would be detrimental to the party as *unconditional* (Cox and McCubbins 2002).⁶ That is, while majority party leaders realize that voters may monitor final passage votes on substantive matters fairly closely (Arnold 1990), leading to the need for occasional

defection by party members, the same relation does not hold for procedural matters. Rather, procedural issues are fairly obscure, and their connection to policy is beyond the purview of (most) voters. Thus, majority party leaders do not associate electoral costs with procedural votes, and, as a result, they require party members to toe the line.⁷

Initial reviews of party cartel theory were mixed. Evidence was either at odds with a story of party control or consistent with a story of majoritarian control (Schickler and Rich 1997a, 1997b). Moreover, the very prospect of finding definitive evidence to support party influence was challenged on methodological grounds. Krehbiel (1993, 2000) argued that measuring members' preferences net of party was problematic because traditional party-influence measures, such as party voting scores and party cohesion scores, could not distinguish between partisan- and preference-based sources. Krehbiel went on to suggest that partisanship could simply be an electoral label used to distinguish different ideological beliefs (that is, a sorting device) and thus institutionally could be nothing more than a good measure of preferences.

Partisan theorists rose to the challenge and began searching for measures of member preferences that were not tainted by party.⁸ Binder, Lawrence, and Maltzman (1999) identified an interest group index that was less correlated with party. Snyder and Groseclose (2000) parsed roll calls into "close" and "lopsided" categories and claimed that, when scaled, the former could be construed as party-pressured preferences and the latter as party-free preferences. McCarty, Poole, and Rosenthal (2001) developed a multiple-cut-point model to estimate a partisan dimension, separate from the primary preference-based dimension. Ansolabehere, Snyder, and Stewart (2001) incorporated a non-roll-call-based measure of preferences based on member responses to surveys administered by Project Vote Smart. Finally, Cox and Poole (2002) generated an expected Rice cohesion score to compare with the actual score.

Each of these studies uncovered evidence of party influence. And although none are methodologically impervious to criticism,⁹ they each contribute to a larger goal. No one study can be the silver bullet to put the parties-versus-preferences question to rest once and for all. Rather, a body of evidence is required, representing different methods, measures, time periods, and theoretical designs, to make a strong case.

This article is an attempt to add to that body of evidence by offering a different perspective on the search for party influence in Congress. Whereas other scholars have focused on developing better methods, measures, or both, we broaden the scope by incorporating a research design developed by Rothenberg and Sanders (2000) that allows us to compare the behavior of exiting House members—retiring members

and higher-office seekers—to that of reelection-seeking House members. In this way, we have a rare quasi-experiment with which we can examine behavioral differences across members given the existence or nonexistence of a party constraint. A further examination of this research design and its application to different subsets of votes is the subject of the next section.

2. Research Design

In their analysis of ideological shirking in the contemporary Congress, Rothenberg and Sanders (2000) employed an innovative research design that compares changes in House members' vote choices (using W-NOMINATE scores) in the last six months of consecutive Congresses. The logic is straightforward: in the final six months (or fourth quarter) of a given Congress, members will know with relative certainty whether or not they will seek reelection, and they will vote accordingly. The Rothenberg-Sanders approach thus provides an ideal means to investigate behavioral change. Examining change across longer periods of time, such as across sessions or entire Congresses, introduces possible measurement error since many House members will switch types (from running for reelection to retiring, for example) and perhaps their behavior as well. As Rothenberg and Sanders (2000, 318) explain, "[W]hen searching for evidence of moral hazard, it is important to identify a preshirking period when the pursuit of reelection is certain and a postshirking period when exit is definite."

Whereas Rothenberg and Sanders focused on the general question of ideological shirking—whether or not exiting members alter their voting behavior more than reelection-seeking members do—we believe that this research design, applied differently, offers a unique way to study party influence in Congress. That is, Rothenberg and Sanders (and most others working in the area) make the implicit assumption that shirking relates to movement away from constituent preferences: once members decide to exit the chamber, the electoral connection (and the accompanying representative-constituency link) is severed and members begin to vote not according to constituent preferences, but to personal preferences. Of course, what is overlooked by such reasoning is the party constraint. If parties exert pressure on members to comply with the party agenda—especially on the *procedural* party agenda, as the party cartel theory argues—then there is more to shirking than meets the eye. In effect, there is also a partisan connection (and an accompanying representative-party link) that must be taken into account.

Here, the two types of exiting House members—retiring members and higher-office seekers—provide a unique opportunity for sorting out potential party influence. We argue that the determinants of vote choice for the different member types are as follows:

- (1) Reelection seekers = $f(\text{personal preferences, party pressure, constituent preferences})$,
- (2) Higher-office seekers = $f(\text{personal preferences, party pressure})$, and
- (3) Retiring members = $f(\text{personal preferences})$.

Reelection seekers serve as the baseline. Since their immediate future is within the House, they face both an electoral connection and a partisan connection, and thus respond to their district constituencies and party leaders. Their personal preferences also influence their vote choices.

For higher-office seekers, the electoral connection is severed, but the partisan connection is not. That is, higher-office seekers, while exiting the House, have another elective office in their sights, and they campaign for that office under their traditional party banner. As a result, they endeavor to maintain good relations with the national party hierarchy for a variety of campaign-related reasons and strive to send signals that they are loyal party members.¹⁰ One powerful signal, we contend, is to toe the line on votes important to party leaders in the House, even as they are exiting the chamber. As Jacobson (2004, 232) states,

Members . . . have . . . found it more expedient to be loyal to their parties in recent Congresses because of the expanded role of national party committees, leadership PACs . . . and other allied PACs in recruiting, training, and financing congressional candidates. Members elected as part of a team, using common campaign themes and issues, with considerable help from party committees, should be more disposed to cooperate on legislative matters. Members hoping for generous party assistance in future campaigns should be more susceptible to persuasion by leaders who influence the distribution of the party's funds.¹¹

We assume, then, that higher-office seekers' vote choices are shaped by both party pressure and their personal preferences.¹²

Finally, for retiring members, both the electoral connection and partisan connection are severed. As a result, we view them as free agents, no longer constrained by constituents or party, and we assume that their vote choices are shaped solely by their personal preferences.¹³

Our argument, then, is that potential party influence can be detected by comparing the relative behaviors of retiring members and higher-office seekers. First, retiring members must exhibit significant behavioral change that is also above and beyond that exhibited by higher-office

seekers. Second, the direction of retiring members' behavioral change must be significantly away from their respective party medians. If these two conditions hold, then we will interpret such results as evidence of party influence.

3. Model and Results

To test the predictions of the party cartel theory, we applied the Rothenberg-Sanders research design to two categories of votes in a pooled, cross-sectional analysis. The first category incorporates only final-passage votes, which include all (final) actions taken on bills, conference reports, and joint resolutions, as well as those that occur under suspension of the rules. The second category incorporates all procedural votes, which include, among other things, motions to end debate, rise from the Committee of the Whole, recede and concur, disagree, order the previous question, recommit, and instruct conferees.¹⁴ To generate our final-passage and procedural-vote categories, we employed a dataset designed by David Rohde of Michigan State University that classifies all House roll-call votes since the 83d Congress by vote type.¹⁵

We began by examining basic ideological change. (The direction of said change will be examined later in this section.) Our dependent variable is similar to that of Rothenberg and Sanders (2000), except that we measure ideological change within each vote-based category, not over all votes. To calculate the ideological-change variable, we generated first-dimension W-NOMINATE scores for House members, using only those votes in the last six months of each election year (that is, the fourth quarter of a given Congress). We then computed the absolute difference of individual members' W-NOMINATE scores between consecutive Congresses.¹⁶ A larger absolute difference corresponds to a greater amount of ideological change.¹⁷

Our dataset consists of 3,844 observations representing all House members who served in the last six months of consecutive electoral cycles from the 94th through 105th Congresses (1975–98).¹⁸ We began with the 94th Congress because of data constraints; prior to the 93d Congress, we were not able to obtain a sufficient number of roll calls in each vote-based category to generate reliable fourth-quarter W-NOMINATE estimates.¹⁹ Nevertheless, the Congresses in question are a logical set for analysis because they cover the entire postreform period in the House, when procedural reforms were enacted to strengthen majority party control (see Rohde 1991). Since our dependent variable is theoretically continuous, ranging from 0 to 2, we utilized ordinary least squares (with Huber-White standard errors) for our analysis.

Our primary set of independent variables mirrors that of Rothenberg and Sanders (2000). We considered two ways of exiting the chamber: members may either retire or pursue higher (statewide) office. Retirement occurs when a member either decides not to seek reelection to another term (for whatever reason) or loses a primary (House or higher-office) election. We used dummy variables as proxies for each method of exit. Retiring members were coded as 1, and all other cases were coded as 0. Members running in the general election for higher office were coded as 1, and all remaining cases as 0.²⁰

To account for other factors that might influence member behavior, we included a number of covariates. *Electoral Security* represents the percentage of the two-party vote that the member received in the previous election. This variable, which ranges from 0.5 to 1, allows us to tap short-term political forces that may affect the degree of “safeness” for each incumbent. *Seniority* measures a member’s prior service (in years) at the beginning of each Congress.²¹ Some studies assume that members’ positions become more entrenched as they become more established in the chamber; others suggest that with more seniority comes greater discretion.

In addition, a member who represents a district that has been redrawn may be more likely to change voting behavior in order to represent his or her new constituency. To control for *District Political Change*, we calculated the absolute difference in the Democratic presidential candidate’s vote share in the old and new district for congressional elections immediately following a redistricting cycle.²² As noted by Jacobson (2000), among others, district-level presidential vote share can serve as a proxy for constituent preferences, and changes from one election to the next (as a result of changes in district composition) can therefore reflect shifts in these underlying preferences. In non-redistricting years or for districts that have not been redrawn, District Political Change is simply coded as 0.²³

We also included *Party Switcher*, a variable controlling for those members who switched parties between consecutive Congresses. As Nokken (2000), McCarty, Poole, and Rosenthal (2001), and Nokken and Poole (2004) illustrate, party switchers tend to alter their behavior significantly because their change in parties comes with a change in the underlying constituency being represented. Without a sufficient control variable included in the model, the outlier nature of these party-switcher cases would lead to findings of greater ideological change than would in fact be the case.²⁴

A possibility exists that higher-office seekers, while severing ties with their House constituency, may begin to vote in accordance with

their prospective statewide constituency. Although evidence is mixed in the literature,²⁵ we nevertheless attempted to control for this possibility by including a variable, *State Heterogeneity*, which is equal to the total population in each state (measured in millions) according to the most recent census. Following Lee and Oppenheimer (1999), we assumed that more-populous states tend to have more-heterogeneous constituencies; thus, higher-office seekers from more-populous states will be more likely to alter their vote choices in order to represent a more diverse set of citizen preferences. To isolate these possible population effects, *State Heterogeneity* takes on a non-zero value only for higher-office seekers.

Finally, we included dummy variables to control for the 12 different Congress-pairs in our dataset.²⁶ The inclusion of Congress-specific fixed effects is necessary because the W-NOMINATE procedure estimates individual Congresses separately. This is a potential problem because the congressional environment is *not* static; among other things, the electoral context, the membership distribution, and more important, the issue agenda (a key element for measuring member ideology) vary from Congress to Congress.²⁷ These substantive across-Congress differences combined with the W-NOMINATE estimation procedure almost certainly produce different Congress-by-Congress choice spaces. The inclusion of Congress-specific fixed effects controls for these shifts in the mean ideological change from one pair of Congresses to the next (see Poole and Romer 1993 and Poole and Rosenthal 1997).

Initial Results

To reiterate, party cartel theory posits that party members are first and foremost held to support the party on procedural votes. Thus, if the theory is valid, evidence of party influence should be uncovered in the procedural-vote regression. What will constitute evidence of party influence? The coefficient on *Retiring* must be *positive, significant, and significantly greater* than the coefficient on the *Higher Office* variable.²⁸ Retiring members should no longer be representatively accountable to either constituency or party (both the electoral and partisan connections having been severed), and higher-office seekers should no longer be accountable to constituency but still accountable to party (only the electoral connection having been severed). Thus, party influence is the net influence—the degree of ideological change above and beyond that attributable to constituency influence (that is, to severing simply the electoral connection).

Regarding member behavior on final-passage votes, party cartel theory is more neutral. As Stewart (2001, 262) suggests, “[P]arty leaders

TABLE 1
Ideological Change by Vote Type
(94th–105th Congresses)

Variable	Final-Passage Votes	Procedural Votes
Retiring	0.006 (0.022)	0.063 ^{††} (0.026)
Higher Office	−0.015 (0.036)	−0.023 (0.035)
District Political Change	0.147 (0.272)	0.449* (0.211)
Electoral Security	0.003 (0.025)	0.037 (0.031)
Seniority	0.0005 (0.0006)	−0.0003 (0.0007)
Party Switcher	0.078 (0.065)	0.267* (0.132)
State Heterogeneity	0.001 (0.007)	0.001 (0.003)
Number of Cases	3,844	2,931
R ²	0.422	0.466

Note: Congress-specific dummy variables not reported. Huber-White standard errors appear in parentheses. One-tailed tests are used for the Retiring and Higher Office variables because we have directional hypotheses for each. Two-tailed tests are used for all other variables.

* $p < .05$; ** $p < .01$; *** $p < .001$ (two-tailed tests).

[†] $p < .05$; ^{††} $p < .01$; ^{†††} $p < .001$ (one-tailed tests).

may excuse some disloyalty on substantive votes, particularly for electoral reasons. . . .” Again, party leaders’ primary concern is maintaining (or winning) control of the chamber, so they are cognizant of members’ need to represent constituent preferences, especially on votes that are likely to be monitored. While some anecdotal evidence exists to suggest that party leaders will, on occasion, pressure members to toe the party line,²⁹ party leaders generally seem to accept that demanding loyalty on final-passage votes runs counter to overall party goals. Thus, rather than take hard stands on policy issues and force reluctant members to vote accordingly, leaders carefully consider the

strategic environment and often select only those policies that comport with the preferences of most party members (Cox and McCubbins 1993, 155–57). Hence, there should not be observable evidence of party influence in the final-passage-vote regression, that is, the coefficient on Retiring should *not* be significantly greater than the coefficient on Higher Office.

What about a pure-preference theory? Krehbiel (1999a, 58, n. 5) states, “I interpret ideal points as electorally *induced* preferences independent of intra-legislative partisan forces. This does not preclude . . . personal views from being components in preferences, in addition to the more salient constituency basis of representation.” Thus, both the Retiring and Higher Office variables *could* be positive and significant in *either* regression, if constituency preferences make up a sizeable portion of reelection-seeking members’ ideal points and exiting members shirk constituency preferences. But the pure-preference theory makes no allowance for party influence, yielding a prediction that the coefficient on the Retiring variable will *not* be significantly greater than the coefficient on the Higher Office variable in *either* model.

Results of the two regressions appear in Table 1. We found no evidence of party influence on final-passage votes, a result consistent with the predictions of both theories. However, we did uncover evidence of party influence in the procedural-vote regression: the coefficient on Retiring is positive, significant ($p < .01$), and significantly greater than the coefficient on Higher Office ($t = 2.66, p < .004$, one-tailed test).³⁰ This result supports the party cartel prediction and suggests that the pure-preference prediction can be rejected, as the observed ideological change was due to more than simply severing the electoral connection.

Intraparty Results

The initial results were encouraging for the party cartel theory, but we wanted to dig deeper. Specifically, we wanted to investigate the “engine” of party influence in Congress: the majority party. As Cox and McCubbins (1993, 1994, 2002) argue, the majority party sets the legislative agenda in the House (via its control over the speakership, committee chairmanships, and the Rules Committee), which biases outcomes toward the interests of its members. After settling on a policy agenda, the majority party leadership seeks to bring that agenda to fruition by demanding strict loyalty on procedural matters. Time is a scarce resource, and behavior that delays or obstructs the agenda is not to be tolerated.³¹ Thus, for the party cartel theory to be valid, party

influence must be observed *within* the majority party and this majority party influence should be disproportionately large relative to minority party influence.

Often, however, methods and measures developed to uncover party influence, such as the party pressure method developed by Snyder and Groseclose (2000), are unable to determine the *source* of the party influence.³² That is because the measures themselves are inherently partisan in nature—in the Snyder-Groseclose case, the measure is a party dummy variable—which provides a way to investigate party influence *across* parties, but not *within* parties. In response to these methodological limitations, Groseclose and Snyder (2003, 107) state, “We are confident that scholars with more creativity will think of other ideas and assumptions that will form the basis for additional tests of [minority-versus-majority party influence].”

While we do not claim to be more creative than Groseclose and Snyder, we believe our approach can distinguish between majority and minority party influence, as our method for identifying party influence is inherently nonpartisan, comparing the relative behavior of retiring members and higher-office seekers or, more specifically, the coefficients on the Retiring and Higher Office variables. Thus, we can break our full dataset into majority and minority party components and run our basic econometric model on each. This allows us to identify majority and minority party influence on both final-passage and procedural votes.

In terms of predictions, the party cartel and pure-preference theories correspond on final-passage votes: party influence should not be observed within either the majority or minority party. With regard to procedural votes, the party cartel theory predicts that party influence should be observed within the majority party, but it is unclear about what to expect within the minority party. Although Cox and McCubbins (1993, 262–69) provide some evidence to suggest that majority party influence should be greater than minority party influence, it is not clear if minority party influence should be *significant*. Finally, the pure-preference theory predicts no procedural party influence within either the majority or minority parties.

Results of the final-passage and procedural-vote regressions by majority and minority party status appear in Table 2. We uncovered no evidence of majority party influence in the final-passage regression, a result consistent with the predictions of both theories. However, we did find evidence of majority party influence in the procedural-vote regression: the coefficient on Retiring is positive, significant ($p < .037$), and significantly greater than the coefficient on Higher Office ($t = 1.88$, $p < .03$, one-tailed test).

TABLE 2
Ideological Change by Vote Type and Party Status
(94th–105th Congresses)

Variable	Majority Party Model		Minority Party Model	
	Final-Passage Votes	Procedural Votes	Final-Passage Votes	Procedural Votes
Retiring	−0.028 (0.028)	0.056 [†] (0.031)	0.088 ^{††} (0.036)	0.037 (0.038)
Higher Office	−0.00003 (0.041)	−0.026 (0.046)	0.038 (0.062)	0.008 (0.039)
District Political Change	0.047 (0.328)	0.506 (0.287)	0.128 (0.436)	0.115 (0.319)
Electoral Security	−0.015 (0.031)	0.033 (0.038)	0.081 (0.043)	0.020 (0.045)
Seniority	0.0008 (0.0007)	0.0001 (0.0009)	−0.0007 (0.0007)	−0.0019 (0.0012)
Party Switcher	0.071 (0.051)	−0.027 (0.029)	0.042 (0.066)	0.353*** (0.107)
State Heterogeneity	−0.002 (0.005)	0.004 (0.006)	0.005 (0.009)	0.0006 (0.0021)
Number of Cases	2,242	1,707	1,602	1,224
R ²	0.517	0.344	0.320	0.740

Note: Congress-specific dummy variables not reported. Huber-White standard errors appear in parentheses. One-tailed tests are used for the Retiring and Higher Office variables because we have directional hypotheses for each. Two-tailed tests are used for all other variables.

* $p < .05$; ** $p < .01$; *** $p < .001$ (two-tailed tests).

[†] $p < .05$; ^{††} $p < .01$; ^{†††} $p < .001$ (one-tailed tests).

Regarding the minority party, we uncovered no evidence of party influence in the procedural-vote regression. This (non)finding suggests that the minority party does *not* operate as a procedural cartel and that the evidence of party influence uncovered in the full model is due solely (or nearly so) to majority party discipline. Finally, we observed what appeared to be evidence of minority party influence in the final-passage-vote regression. Upon closer inspection, however, we ruled out a party influence story: although the coefficient on Retiring is positive and significant ($p < .01$), it is *not* significantly greater than the coefficient on Higher Office ($t = 0.86, p < .195$, one-tailed test). This result suggests, rather, that the explanatory power from the Retiring variable is being driven by deviations from constituency preferences.

Validity Check: Directional Results

While the preceding results support the notion of majority party influence on procedural votes, a further check is necessary for validation. Although our findings of significant ideological change are consistent with majority party influence, they could in theory support the *opposite* story. That is, retiring members may in fact be moving more than other members, but their movement may not be away from the majority party position. Rather, they could be moving *toward* the majority party position. If so, then the significant ideological change that we uncovered is *not* evidence of party acting as a constraint on member behavior.

As a result, simply examining the magnitude of the ideological change is not sufficient; we must also examine the direction of the ideological change. We did so by constructing a new dependent variable that taps changes in the distance of members' fourth-quarter W-NOMINATE scores from their respective party medians. In effect, for each member in each Congress, we first calculated the distance from his or her ideal point to the party median. We then created a change-in-ideological-distance measure for each member in each set of consecutive Congresses, by subtracting the absolute value of the member's ideological distance in Congress $t-1$ from the absolute value of the member's ideological distance in Congress t .³³ Positive values for this measure indicate greater distance from the party median in Congress t ; negative values indicate greater distance from the party median in Congress $t-1$.

If party truly acts as a constraint on member behavior, then the coefficient on Retiring should be positive and significant. A positive coefficient indicates that retiring members move further away from the party median than do reelection seekers. Substantively, this result would mean that the retirement decision frees a member from party influence, which triggers a shift *away* from the party position and toward a member's own ideological predilections. In addition, the coefficient on Higher Office should not be significant. Because higher-office seekers are still beholden to their party, they will *not* shift their behavior away from the party position after deciding to seek higher office. As a result, they will *not* move further away from the party median than will reelection seekers.

Because we compared two sets of discrete distances for each member, we employed heteroskedastic regression, a maximum-likelihood adaptation of the normal regression model that allows for variance differences in the substantive nature of the dependent variable (see Harvey 1976). Accounting for variance differences is important in our

TABLE 3
Change in Ideological Distance from Party Median
(94th–105th Congresses)

Variable	Procedural Votes	
	Majority Party	Minority Party
Retiring	0.055 [†] (0.027)	0.023 (0.023)
Higher Office	−0.001 (0.0425)	−0.001 (0.039)
District Political Change	0.143 (0.401)	−0.082 (0.213)
Electoral Security	0.077** (0.030)	0.017 (0.026)
Seniority	−0.001 (0.0007)	0.0004 (0.0006)
Party Switcher	0.154 (0.204)	0.213* (0.100)
State Heterogeneity	0.002 (0.002)	0.001 (0.003)
Number of Cases	1,707	1,224
LR ^{÷2}	444.18***	447.38***

Note: Heteroskedastic regression estimates with standard errors in parentheses. Congress-specific dummy variables and Congress-specific variance parameters not reported. One-tailed tests are used for the Retiring and Higher Office variables because we have directional hypotheses for each. Two-tailed tests are used for all other variables.

* $p < .05$; ** $p < .01$; *** $p < .001$ (two-tailed tests).

[†] $p < .05$; ^{††} $p < .01$; ^{†††} $p < .001$ (one-tailed tests).

analysis because each ideological distance calculation is, in part, a function of the distribution of ideal points in a given Congress. For a variety of reasons, some Congresses may have greater spreads than other Congresses. Failing to control for spread differences across consecutive Congresses could lead to findings of ideological shifts that are in fact spurious. The heteroskedastic regression procedure allows us to deal with this potential problem by estimating an additional variance parameter for each Congress-pair to control for any differences in spreads.³⁴

Regression results for the majority and minority party on procedural votes appear in Table 3. The evidence confirms our prior findings of majority party influence, as Retiring is positive and significant ($p < .022$) in the majority party model. Moreover, Higher Office is *not* significant. Finally, as in the previous set of results, there is no evidence of minority party influence, as Retiring is not significant in the minority party model.

These findings of disproportionate majority party influence (along with those in the previous subsection) support the view that Congress is structured to favor the majority party. As Aldrich and Rohde (2000) argue, minority party leaders may try to pressure members, but they simply do not possess the procedural and organizational advantages that majority party leaders enjoy. For example, the power loci in the House, like the Speaker and Rules Committee, that go hand-in-hand with majority status offer an assortment of “chits” that can be used to influence members—bonus committee seats, committee chairmanships, privileged positions on the legislative agenda, parliamentary insulation (via the granting of special rules), and pork-based side payments, just to name a few. There are no analogous power loci for minority party leaders, producing far fewer (and less enticing) chits for them to dispense in the party influence game.

4. Conclusion

In this article, we examined the extent to which parties in Congress operate as procedural coalitions. In particular, we tested predictions from the party cartel theory, which suggests that party leaders (especially majority party leaders) pressure party members to toe the line on votes that affect the legislative agenda in the House.

Up until now, determining if party influence exists in Congress has been hampered by the problem of finding good measures to differentiate between partisan- and preference-based influences. Innovative strides have been made in recent years to obtain better preference-based measures, but they can only help us resolve part of the problem. Such measures can help us determine whether or not party influence generally is present, but not the *distribution* of that party influence, that is, whether the influence is due more to the majority party or the minority party.

Our approach allows for intraparty analysis, thanks to the non-partisan nature of our key variables. We focused on exiting House members and compared their vote choices to those of members seeking reelection in the fourth quarter of consecutive Congresses. Parsing the

exiting members into two categories—retiring members and higher-office seekers—allowed us to isolate potential party influence. We argued that retiring members' vote choices are solely a function of their personal preferences, whereas higher-office seekers' vote choices are a function of their personal preferences as well as party pressure. Thus, if retiring members exhibit significant behavioral change above and beyond that exhibited by higher-office seekers, and this change represents movement away from the party median, then we take that change as evidence of the constraining influence of party.

Our initial findings suggest that the general prediction of the party cartel theory is substantiated; evidence of party influence is uncovered on procedural votes but not final-passage votes. These results are consistent with other recent studies that report greater party discipline on procedural votes (see Ansolabehere, Snyder, and Stewart 2001, Cox and Poole 2002, and Snyder and Groseclose 2000). In addition, we found that this procedural party influence is driven almost exclusively by the *majority party*. In fact, we found no evidence of procedural influence in the minority party analysis. These results also support the party cartel theory. Moreover, they are especially noteworthy because few studies have been able to offer empirical evidence at the intraparty level.

In terms of impact, when the party constraint is eliminated, members move, on average, .024 *away* from the party median; members who face a party constraint move, on average, .031 *toward* the party median.³⁵ Rather than suggest that the degree to which parties are able to pressure members is minimal, these results more likely indicate that parties do not often *have* to pressure members. That is, in the last several decades, preference homogeneity within congressional parties has increased substantially (Aldrich 1995; McCarty, Poole, and Rosenthal 1997, 2003; Poole 2003; Poole and Rosenthal 1997; Rohde 1991). As a result, cases in which members would prefer to defect from the procedural party agenda—which would then necessitate pressure from party leaders—are fairly rare. Thus, we should not expect the magnitude of party influence to be large.

Finally, we do not view our analysis as the last word in the debate regarding party influence in Congress. Rather, we believe (perhaps stretching the bounds of metaphor) that we have laid an additional brick in the wall of scientific inquiry. That is, our conception of social science is akin to one of *normal science*, where modest strides are made toward the construction of a collective body of evidence. The last decade has produced a great deal of new knowledge regarding the institutional organization of Congress, and tough questions (posed by Krehbiel, among others) have forced scholars to seek better theories

and more-definitive evidence. As Fiorina (1995, 311) argues, “The legislative subfield illustrates the progress made by a genuine research *community*.” In this paper, we have added to the existing empirical literature on party influence in Congress by showing in a new way that parties act as procedural cartels. In addition, we have taken the next step by showing that procedural party influence is almost exclusively the domain of the majority party. We hope that our latter finding will spark additional debate within the community of congressional scholars. To quote Fiorina (1995, 311) once again, “It’s all part of the conversation, and collectively we are the better for it.”

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NOTES

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1. For a critique, see Krehbiel 1999b.
2. For critiques, see Cox and Poole 2002; Krehbiel 2003a, 2003b; and McCarty, Poole, and Rosenthal 2001. For responses to the last two critiques, see Groseclose and Snyder 2003 and Snyder and Groseclose 2001.
3. For critiques, see Krehbiel 1999a.
4. For a critique, see Cox and Poole 2002.
5. A recent example of such expulsion is James A. Traficant (D-OH), who, after voting for Dennis Hastert (R-IL) for Speaker in the 107th Congress, was expelled from the Democratic caucus and had his committee assignments stripped (Cohn 2001).
6. The conditional nature of party government forms the basis of the conditional party government (CPG) theory, developed by Rohde (1991). For a fuller discussion of the similarities and differences between party cartel theory and CPG theory, see Finocchiaro and Rohde 2002.
7. The notion of partisan procedural control did not originate with cartel theory. Rather, it was a common feature in the traditional congressional literature (see, e.g., Froman and Ripley 1965 and Jones 1964). Moreover, the notion of parties as procedural

coalitions is a feature of other contemporary congressional theories, which are different from, but quite compatible with, cartel theory (see, e.g., Binder 1997 and Dion 1997).

8. Scholars also pursued other avenues to uncover evidence of party influence. Cox and Magar (1999), for example, used political action committee (PAC) contributions to assess the value of majority status in Congress, and Cox (2001) and Cox and McCubbins (2002) examined roll rates, the number of times the majority party opposes passage of a bill and loses.

9. See notes 1–4 for examples.

10. Hershey and Beck (2003) identified a number of campaign-related services and resources that national party committees provide to congressional candidates, including training, fund-raising, research, field work, media production, contributor lists, and, perhaps most important, hard and soft money contributions. These services and resources are especially important to higher-office seekers, who typically must reach a broader electoral audience.

11. The literature connecting national party resources and contributions with party unity in Congress is quite limited. Up to this point, scholars have focused strictly on the impact of monetary contributions made by national party committees to House members. For example, Leyden and Borrelli (1990, 1994) found that party money flowed disproportionately to loyal members (i.e., as a reward) and, consequently, served to motivate greater party unity in the future. More recently, Ansolabehere and Snyder (2000) found the effects of national party money on party unity to be considerably smaller. The impact of the plethora of other resources and services controlled by national party committees (see note 10) is woefully underexamined and is thus an area ripe for research.

12. In addition, we investigated whether or not *another* electoral connection could be present for higher-office seekers. Specifically, we examined whether or not these members, in their remaining days in the House, tried to be responsive to their prospective *state-level* constituency. We attempted to control for this possibility separately in our subsequent regression model. See Section 3.

13. Could retiring members' vote choices also be influenced by other factors, such as the preferences of (potential) future employers? Surprisingly little research has investigated members' postcongressional careers, but the existing work has uncovered no evidence to suggest such an influence. Herrick and Nixon (1996), for example, found that a majority of retiring House members between 1971 and 1992 left politics for good and only approximately 17% sought a career in pressure politics. Diermeier, Keane, and Merlo (2004) reported similar results for retiring members (aggregated across both chambers) over a longer time period (1947–93). Additionally, Palmer and Vogel (1995, n. 11) uncovered little evidence of a vibrant "political appointment market" for retiring House members. Specifically, they found that retiring House members between 1961 and 1992 were significantly *less* likely to receive a federal appointment.

14. A third category consists of amendment votes, which we do not consider in this analysis because we lack clear predictions. Party cartel theory does not deal specifically with amendment votes, most likely because amendment votes are fairly heterogeneous—some (such as those on gun control legislation) will be closely monitored by citizens and thus will be immune to party pressure, whereas others (such as those on less salient legislation) will not be closely monitored and thus will be ripe for party pressure. In future research, we will explore if pressure differentials in fact exist across various types of amendment votes.

15. See Rohde 2003. For a similar application of this dataset, see Cox and Poole 2002.

16. We focus on absolute difference because a given member could be compelled to converge to his or her party's median from either the left or the right. Stated another way, a member's personal preferences could be more extreme than the party's median in either a liberal or conservative direction. Hence, we could observe either a leftward or rightward adjustment in voting behavior when a member decides to retire and indulge personal ideological inclinations more fully.

17. To generate an ideological change score in the 94th Congress, for instance, we calculated the absolute difference between members' W-NOMINATE scores in the last six months of the 93d and 94th Congresses. Those members who did not serve in both Congresses were dropped from the analysis.

18. Following the lead of Rothenberg and Sanders (and others), we excluded members from Louisiana from our analysis because of the unique system of House elections in that state.

19. In addition, we were not able to obtain a sufficient number of roll-call votes in the procedural-vote category in the 96th Congress to generate reliable fourth-quarter W-NOMINATE estimates. This explains the difference in sample sizes between the final-passage vote and procedural-vote categories.

20. Examples of higher-office positions would include Senate seats, governorships, and lieutenant governorships.

21. For seniority data and retirement and higher-office data, see *Biographical Directory* (1998–) and ICPSR and McKibbin (1997).

22. District-level presidential vote share data were taken from various issues of *The Almanac of American Politics*.

23. Since constituency-level factors are no longer relevant for members exiting the chamber, we followed Rothenberg and Sanders (2000) by coding both Electoral Security and District Political Change as 0 for members either seeking higher office or retiring.

24. As McCarty, Poole, and Rosenthal (2001, 686) argue, significant changes in party switchers' behavior are consistent with a party effect, but the "source may not be internal to the legislature." Again, per Krehbiel's argument, it may be that party switchers are simply responding to new constituencies, in which case party may have an influence at the electoral level, but not at the institutional level. Since we are only concerned with the latter, we do not attempt to obtain leverage on the question of party influence via the party-switcher variable.

25. Hibbing (1986) finds some support for a shift in voting behavior, but Grofman, Griffin, and Berry (1995) and Poole and Rosenthal (1997) find no support.

26. Like Poole and Romer (1993), we ran the model with a full set of dummies included, and thus without a constant. The final-passage-vote regression includes 12 dummies, but the procedural-vote regression only includes 10 dummies, since we dropped the 96th Congress (see note 19).

27. See Crespín, Rohde, and Vander Wielen 2002, Roberts and Smith 2003, and Rohde 1991, 1992.

28. The coefficient must be positive because we assume that parties constrain behavior; when that constraint is removed, retiring members should shift their behavior toward their personal preferences. Because we measure our dependent variable (ideological change) in absolute-value terms, a positive coefficient signifies greater

change than reelection-seeking members, who are constrained by party throughout. Likewise, if we assume constituency to be a constraint, then the coefficient on Higher Office should be positive as well.

29. According to Smith, Roberts, and Vander Wielen (2003, chap. 6, p. 7), “Such situations usually involve legislation that is a high priority of a president of the same party, whose success or failure will reflect on the party, and for which there are not enough supportive members of the opposition party to muster a majority.”

30. Here, $t = \frac{\hat{\beta}_1 - \hat{\beta}_2}{\text{se}(\hat{\beta}_1 - \hat{\beta}_2)}$ where $\hat{\beta}_1$ is the coefficient on Retiring and $\hat{\beta}_2$ is the coefficient on Higher Office.

31. For example, Smith, Roberts, and Vander Wielen (2003, chap. 6, p. 17) note, “[In late 2002, Speaker Dennis Hastert] endorsed Majority Leader Tom Delay’s (R-TX) proclamation that a Republican member of the party’s organization who voted against the party on any procedural matter would be excused from service.” The binding nature of Delay’s proclamation is almost certainly an exaggeration, but the threat itself indicates the priority that party leaders place on procedural party loyalty.

32. In a recent exchange with Krehbiel (2003a, 2003b), Groseclose and Snyder (2003, 104) acknowledged that their “statistical method cannot discriminate between majority-party influence and minority-party influence.” Krehbiel (2003b, 95) contends that this is problematic: “Because the literature on parties in Congress emphasizes majority-party strength, the inability of the coefficient to isolate party-specific effects is a serious drawback in the ongoing hunt for genuine party discipline.”

33. Again, we focus on absolute difference because member ideal points are distributed on both sides of the party median. Hence, a given member could be compelled to converge to the party’s median from either the left or the right. As a result, we could observe either a leftward or rightward shift from the party median when a member decides to retire and indulge personal ideological inclinations more fully.

34. Different opinions exist regarding the correct specification of cross-scale models (see, e.g., Carson et al. 2004, Crespin, Carson, and Jenkins 2004, and Rothenberg and Sanders 2004). Our contention is that point-estimate comparisons, along the lines of those in Tables 1 and 2, necessitate a Congress-specific fixed-effects model with heteroskedastic standard errors to control for potential mean shifts across the scales (see Poole and Romer 1993 and Poole and Rosenthal 1997, 75). Distance comparisons require more; specifically, potential mean *and* variance changes across the scales must be controlled. Hence, a broader technique, like heteroskedastic regression, that allows for both mean and variance differences is needed.

35. These results are based on a weighted average of the 10 Congress-specific variables in our model, measuring continuous variables at their means and dichotomous variables at their modes.

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