Abstract

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 source (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 in that most prior studies have been limited solely to investigating interparty influence.
I. Introduction

Research investigating party influence in Congress has exploded over the last decade. This 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, like 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. Developing 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 “non-partisan” interest group score\(^1\) (Binder, Lawrence, and Maltzman 1999), a “party pressure” measure\(^2\) (Snyder and Groseclose 2000), partisan “roll rates”\(^3\) (Cox 2001; Cox and McCubbins 2002), partisan “cut points”\(^4\) (McCarty, Poole, and Rosenthal 2001), and a “net” Rice index of party difference (Cox and Poole 2002).

This paper 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, however, we apply an existing research design in a different way to a new set of data.
Our main focus will be to examine whether parties act as *procedural coalitions*, i.e., “cartels” that organize the institution, via rulemaking 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 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 or not 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 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 paper proceeds as follows. Section II discusses parties as procedural coalitions, focusing specifically on the party cartel theory. Section III 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 IV 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 V concludes.
II. Procedural Cartel Theory

Cox and McCubbins (1993, 1994, 1997, 2002) characterize political parties as legislative cartels that usurp the procedural (rulemaking 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, based 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. However, procedural matters 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 polices as conditional – based, for example, on the size and degree of preference homogeneity within the majority party – but view blocking policies that would be detrimental to the party as
unconditional (Cox and McCubbins 2002). 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 hold party members to toe the line.

Initial reviews of party cartel theory were mixed. Evidence was presented that 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, like “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 (i.e., 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. 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 “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 while none are methodologically impervious to criticism, they each contribute to a larger goal. That is, 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 paper 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 and/or measures, 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 reelection-seeking House members. In this way, we have a rare quasi-experiment with which we can examine behavioral differences across members based on 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 following section, to which we now turn.

III. Research Design

In their analysis of ideological shirking in the contemporary Congress, Rothenberg and Sanders (2000) employ 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 vote accordingly. This approach therefore provides an ideal forum to investigate behavioral change.
Examining change across longer periods of time, such as across sessions or entire Congresses, introduces possible measurement error, as many House members will switch “types” (from running for reelection to retiring, or from running for reelection to running for higher office, for example) and perhaps their behavior as well. As Rothenberg and Sanders (2000: 318) explain, “when 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 focus on the general question of ideological shirking – whether exiting members alter their voting behavior more than reelection-seeking members – we believe that their 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, i.e., once members decide to exit the chamber, the “electoral connection” (and the accompanying representative-constituency linkage) is severed, and they begin to vote based not on constituent preferences, but rather on their personal preferences. Of course, what is overlooked 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 linkage) 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(personal preferences, party pressure, constituent preferences)

(2) Higher-office seekers = f(personal preferences, party pressure)
(3) Retiring members = f(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 not the partisan connection. That is, higher-office seekers, while exiting the House, have another elective office in their sights, and 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. 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. We assume, then, that higher-office seekers’ vote choices are shaped by both party pressure and their personal preferences.
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 assume that their vote choices are shaped solely by their personal preferences.\textsuperscript{13}

Our argument, then, is that potential party influence can be ascertained 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 median. If these two conditions hold, we will interpret such results as evidence of party influence.

\textbf{IV. Model and Results}

To test the predictions of the party cartel theory, we apply 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.\textsuperscript{14} To generate our final-passage and procedural vote categories, we employ a dataset designed by David Rohde of Michigan State University that classifies all House roll-call votes since the 83rd Congress by vote type.\textsuperscript{15}

We begin 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 start by generating first-
dimension W-NOMINATE scores for House members, using only those votes in the last six months of each election year (i.e., fourth-quarter of a given Congress). We then compute the absolute difference of individual members’ W-NOMINATE scores between consecutive congresses. A larger absolute difference corresponds to a greater amount of ideological change.

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). We begin with the 94th Congress due to data constraints – prior to the 93rd 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. Nevertheless, the congresses in question are a logical set for analysis, as 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 utilize OLS (with Huber-White standard errors) for our analysis.

Our primary set of independent variables mirrors that of Rothenberg and Sanders (2000). We consider two ways of exiting the chamber: members may either retire or pursue higher (statewide) office. Retirement occurs when a member decides either not to seek reelection to another term (for whatever reason) or loses a primary (House or higher-office) election. Dummy variables are used as proxies for each method of exit. Members who are retiring are coded one, with all other cases equal to zero. Members who run in the general election for higher office are coded one, with all remaining cases equal to zero.

To account for other factors that may influence member behavior, we include 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. Some studies assume that members’ positions become more entrenched as they become more established in the chamber, while 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 her voting behavior in order to represent her new constituency. To control for district political change, we calculate 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. 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. Thus, in non-redistricting years or for districts that have not been redrawn, district political change is simply equal to zero.

We also include a party switcher variable, to control 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, as their change in parties also 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.

A possibility exists that higher-office seekers, while severing ties with their House constituency, may begin to vote in accordance with their prospective state-wide constituency.
While evidence is mixed in the literature, we nevertheless attempt to control for this possibility by including a variable, *state heterogeneity*, which is equal to the total population in each state (measured in millions) based on the most recent census. Following Lee and Oppenheimer (1999), we assume that more populous states tend to have more heterogeneous constituencies; thus, higher-office seekers from more populous states will be more in need of altering 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 include dummy variables to control for the twelve different congress-pairs in our dataset. The inclusion of congress-specific fixed effects is necessary because the W-NOMINATE procedure estimates individual Congresses *separately*. This is a potential problem, as the congressional environment is *not* static; among other things, the electoral context, the membership distribution, and more importantly, the issue agenda (a key element for measuring member ideology) vary from congress to congress. 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; 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 the retiring variable must be *positive*, *significant*, and *significantly greater* than the coefficient on the higher office variable. Again, we assume retiring members
are no longer representatively accountable to either constituency or party (the electoral and partisan connections have been severed), while higher-office seekers are no longer accountable to constituency but still accountable to party (only the electoral connection has been severed). Thus, party influence is the “net” influence – the degree of ideological change above and beyond that attributable to constituency influence (i.e., simply severing the electoral connection).

Regarding member behavior on final-passage votes, party cartel theory is more neutral. As Stewart (2001: 262) suggests, “party leaders 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, demanding loyalty on final-passage votes generally is viewed as running counter to overall party goals. Thus, rather than take hard stands on policy issues and force reluctant members to vote accordingly, party 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). As a result, we do not expect to observe evidence of party influence in the final-passage vote regression, so the coefficient on the retiring variable should not be significantly greater than the coefficient on the higher office variable.

What about a pure-preference theory? Krehbiel (1999a: 58 fn 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 from constituency preferences. However, 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. First, we find no evidence of party influence on final-passage votes, consistent with the predictions of both theories. However, we do uncover evidence of party influence in the procedural-vote regression: the coefficient on the retiring variable is positive, significant (p < .01), and significantly greater than the coefficient on the higher office variable (t = 2.66, p < .004, one-tailed test). 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.

[Table 1 about here]

Intraparty Results

While the prior results are encouraging for the party cartel theory, we want to dig deeper. Specifically, we want 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 Rules Committee, which biases outcomes toward the interests of its members. After settling on a policy agenda, the majority-party leadership seeks to bring it to fruition by demanding strict loyalty on procedural matters. Time is a scarce resource, and behavior that will delay or obstruct the agenda will not be tolerated. Thus, for the party cartel theory to truly be valid, (a) party influence must be observed within the majority party and (b) this majority-party influence should be disproportionately large relative to minority-party influence.
Often, however, methods and measures developed to uncover party influence, like the “party pressure” method developed by Snyder and Groseclose (2000), are unable to determine the source of the party influence. This 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-party 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 in nature – 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 will allow 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 is unclear about what to expect within the minority party. While 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 whether 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. First, we uncover no evidence of majority-party influence in the final-passage regression, consistent with the predictions of both theories. However, we do find evidence of majority-party influence in the procedural-vote regression – the coefficient on the \textit{retiring} variable is positive, significant ($p < .037$), and significantly greater than the coefficient on the \textit{higher office} variable ($t = 1.88$, $p < .03$, one-tailed test).

[Table 2 about here]

Regarding the minority party, we uncover no evidence of party influence in the procedural-vote regression. This suggests that the minority party does not operate as a procedural cartel, and the evidence of party influence uncovered in the full model is due solely (or nearly so) to majority-party discipline. Finally, we observe what appears to be evidence of minority-party influence in the final-passage vote regression. However, upon closer inspection, a party influence story is ruled out: while the coefficient on the \textit{retiring} variable is positive and significant ($p < .01$), it is not significantly greater than the coefficient on the \textit{higher office} variable ($t = 0.86$, $p < .195$, one-tailed test). This suggests, rather, that the explanatory power from the \textit{retiring} variable is being driven by deviations from constituency preferences.

\textit{Validity Check: Directional Results}

While the above results support the notion of majority-party influence on procedural votes, a further check is necessary for validation. In a sense, while our findings of significant ideological change are consistent with majority-party influence, they could in theory support the opposite story. That is, while retiring members may in fact be moving more than other members, their movement may not be \textit{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 uncover 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. This can be done in a relatively straightforward manner, 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 calculate the distance from her ideal point to her party median. We then create a change-in-ideological-distance measure for each member in each set of consecutive Congresses, by subtracting the absolute value of her ideological distance in Congress $t-1$ from the absolute value of her ideological distance in Congress $t$. Positive values for this measure indicate greater distance from the party median in Congress $t$, while negative values indicate greater distance from the party median in Congress $t-1$.

If party truly acts as a constraint on member behavior, the coefficient on the retiring variable should be positive and significant. A positive coefficient indicates that retiring members move further away from the party median than reelection seekers. Substantively, this 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, we do not expect the coefficient on the higher office variable to 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 reelection seekers.

Because we compare two sets of discrete distances for each member, we employ 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 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 deals 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 the retiring variable is positive and significant ($p < .022$) in the majority-party model. Moreover, the higher office variable is not significant. Finally, as in the previous set of results, there is no evidence of minority-party influence, as the retiring variable in the minority-party model is not significant.

[Table 3 about here]

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.”

V. Conclusion

In this paper, we examine the extent to which parties in Congress operate as procedural coalitions. In particular, we test 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.

To this point, determining whether party influence exists in Congress has been hampered by the problem of finding good measures to differentiate between partisan and preference-based influences. While innovative strides have been made in recent years to obtain better preference-based measures, they can only help us resolve part of the story. That is, such measures can help us determine whether party influence generally is present, but not the distribution of that party influence, i.e., whether the influence is due more to the majority party or the minority party.

Our approach allows for intraparty analysis, thanks to the nonpartisan nature of our key variables. We focus on exiting House members and compare 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 – allows us to isolate potential party influence. That is, we argue that retiring members’ vote choices are solely a function of their personal preferences, while 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 that is above and beyond that exhibited by higher-office seekers, and this change represents movement away from their party median, we take that as evidence of the constraining influence of party.
Our initial findings suggest that the general prediction of the party cartel theory is substantiated, as 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 Snyder and Groseclose 2000; Ansolabehere, Snyder, and Stewart 2001; Cox and Poole 2002). In addition, we find that this procedural party influence is driven almost exclusively by the majority party. In fact, we find no evidence of procedural influence in the minority party analysis. These results also support the party cartel theory. Moreover, they are especially noteworthy in that few studies are 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; whereas, members who face a party constraint move on average .031 toward the party median. 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 (Rohde 1991; Aldrich 1995; Poole and Rosenthal 1997; McCarty, Poole, and Rosenthal 1997, 2003; Poole 2003). 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 hyperbole) that we have laid an additional brick in the wall of scientific inquiry. That is, our conception of social science is 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. Quoting Fiorina (1995: 311) once again: “It’s all part of the conversation, and collectively we are the better for it.”
References


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

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<tr>
<td></td>
<td>(0.022)</td>
<td>(0.026)</td>
</tr>
<tr>
<td>Higher Office</td>
<td>-0.015</td>
<td>-0.023</td>
</tr>
<tr>
<td></td>
<td>(0.036)</td>
<td>(0.035)</td>
</tr>
<tr>
<td>District Political Change</td>
<td>0.147</td>
<td>0.449*</td>
</tr>
<tr>
<td></td>
<td>(0.272)</td>
<td>(0.211)</td>
</tr>
<tr>
<td>Electoral Security</td>
<td>0.003</td>
<td>0.037</td>
</tr>
<tr>
<td></td>
<td>(0.025)</td>
<td>(0.031)</td>
</tr>
<tr>
<td>Seniority</td>
<td>0.0005</td>
<td>-0.0003</td>
</tr>
<tr>
<td></td>
<td>(0.0006)</td>
<td>(0.0007)</td>
</tr>
<tr>
<td>Party Switcher</td>
<td>0.078</td>
<td>0.267*</td>
</tr>
<tr>
<td></td>
<td>(0.065)</td>
<td>(0.132)</td>
</tr>
<tr>
<td>State Heterogeneity</td>
<td>0.001</td>
<td>0.001</td>
</tr>
<tr>
<td></td>
<td>(0.007)</td>
<td>(0.003)</td>
</tr>
<tr>
<td>Number of Cases</td>
<td>3844</td>
<td>2931</td>
</tr>
<tr>
<td>$R^2$</td>
<td>0.422</td>
<td>0.466</td>
</tr>
</tbody>
</table>

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, as 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)
Table 2: Ideological Change by Vote Type and Party Status (94th – 105th Congresses)

<table>
<thead>
<tr>
<th>Variable</th>
<th>Majority Party Model</th>
<th>Minority Party Model</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Final Passage Votes</td>
<td>Procedural Votes</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Retiring</td>
<td>-0.028</td>
<td>0.056†</td>
</tr>
<tr>
<td></td>
<td>(0.028)</td>
<td>(0.031)</td>
</tr>
<tr>
<td>Higher Office</td>
<td>-0.00003</td>
<td>-0.026</td>
</tr>
<tr>
<td></td>
<td>(0.041)</td>
<td>(0.046)</td>
</tr>
<tr>
<td>District Political Change</td>
<td>0.047</td>
<td>0.506</td>
</tr>
<tr>
<td></td>
<td>(0.328)</td>
<td>(0.287)</td>
</tr>
<tr>
<td>Electoral Security</td>
<td>-0.015</td>
<td>0.033</td>
</tr>
<tr>
<td></td>
<td>(0.031)</td>
<td>(0.038)</td>
</tr>
<tr>
<td>Seniority</td>
<td>0.0008</td>
<td>0.0001</td>
</tr>
<tr>
<td></td>
<td>(0.0007)</td>
<td>(0.0009)</td>
</tr>
<tr>
<td>Party Switcher</td>
<td>0.071</td>
<td>-0.027</td>
</tr>
<tr>
<td></td>
<td>(0.051)</td>
<td>(0.029)</td>
</tr>
<tr>
<td>State Heterogeneity</td>
<td>-0.002</td>
<td>0.004</td>
</tr>
<tr>
<td></td>
<td>(0.005)</td>
<td>(0.006)</td>
</tr>
<tr>
<td>Number of Cases</td>
<td>2242</td>
<td>1707</td>
</tr>
<tr>
<td>(R^2)</td>
<td>0.517</td>
<td>0.344</td>
</tr>
</tbody>
</table>

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, as 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)
Table 3: Change in Ideological Distance from Party Median (94th – 105th Congresses)

<table>
<thead>
<tr>
<th>Variable</th>
<th>Procedural Votes</th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Majority Party</td>
<td>Minority Party</td>
<td></td>
</tr>
<tr>
<td><strong>Retiring</strong></td>
<td>0.055†</td>
<td>0.023</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.027)</td>
<td>(0.023)</td>
<td></td>
</tr>
<tr>
<td><strong>Higher Office</strong></td>
<td>-0.001</td>
<td>-0.001</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.0425)</td>
<td>(0.039)</td>
<td></td>
</tr>
<tr>
<td><strong>District Political Change</strong></td>
<td>0.143</td>
<td>-0.082</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.401)</td>
<td>(0.213)</td>
<td></td>
</tr>
<tr>
<td><strong>Electoral Security</strong></td>
<td>0.077**</td>
<td>0.017</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.030)</td>
<td>(0.026)</td>
<td></td>
</tr>
<tr>
<td><strong>Seniority</strong></td>
<td>-0.001</td>
<td>0.0004</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.0007)</td>
<td>(0.0006)</td>
<td></td>
</tr>
<tr>
<td><strong>Party Switcher</strong></td>
<td>0.154</td>
<td>0.213*</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.204)</td>
<td>(0.100)</td>
<td></td>
</tr>
<tr>
<td><strong>State Heterogeneity</strong></td>
<td>0.002</td>
<td>0.001</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.002)</td>
<td>(0.003)</td>
<td></td>
</tr>
<tr>
<td><strong>Number of Cases</strong></td>
<td>1707</td>
<td>1224</td>
<td></td>
</tr>
<tr>
<td><strong>LR χ²</strong></td>
<td>444.18***</td>
<td>447.38***</td>
<td></td>
</tr>
</tbody>
</table>

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, as 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)
Endnotes

We thank Scott Adler, Gary Cox, Erik Engstrom, Larry Evans, Charles Finocchiaro, Jay Goodliffe, Jeff Grynaviski, Michael Herron, Greg Huber, Dan Lipinski, Mathew McCubbins, Lisa Milligan, Nathan Monroe, Jason Roberts, David Rohde, and three anonymous reviewers for their helpful comments. Special thanks to Larry Evans for his help and consideration throughout the review process.

1 For a critique, see Krehbiel (1999b).

2 For critiques, see McCarty, Poole, and Rosenthal (2001), Krehbiel (2003a, 2003b), and Cox and Poole (2002). For responses to the first two critiques, see Snyder and Groseclose (2001) and Groseclose and Snyder (2003).

3 For critiques, see Krehbiel (1999a).

4 For a critique, see Cox and Poole (2002).

5 A recent example 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., Jones 1964; Froman and Ripley 1965). 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; Dion 1997).

8 Scholars have also pursued other avenues to uncover evidence of party influence. Cox and Magar (1999), for example, use PAC contributions to assess the value of majority status in Congress, while Cox (2001) and Cox and McCubbins (2002) examine “roll rates,” the number of times the majority party opposes passage of a bill and loses.

9 See footnotes 1-4 for examples.

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

11 The literature connecting national party resources/contributions and party unity in Congress is quite limited. 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 litany of other resources/services controlled by national party committees (see footnote 10) is woefully underexamined, and is thus an area ripe for research.

12 In addition, we investigate whether another electoral connection could be present for higher-office seekers. Specifically, we examine whether they, in their remaining days in the House,
seek to be responsive to their prospective state-level constituency. We attempt to control separately for this possibility in our subsequent regression model.

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’ post-Congressional careers. However, the existing work in the area uncovers no evidence to suggest that this is the case. Herrick and Nixon (1996), for example, find that a majority of retiring House members between 1971 and 1992 left politics for good, and only about 17 percent sought a career in pressure politics. Diermeier, Keane, and Merlo (2004) report similar results for retiring members (aggregated across both chambers) over a longer time period (1947-1993). Additionally, Palmer and Vogel (1995 fn 11) uncover little evidence of a vibrant “political appointment market” for retiring House members. Specifically, they find that retiring House members between 1961 and 1992 were significantly less likely to receive a Federal appointment.

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

See Rohde (2003). For a similar application of this dataset, see Cox and Poole (2002).
We focus on absolute difference because a given member could be compelled to converge to her party’s median from either the left or the right. Stated another way, a member’s personal preferences could be more extreme than her 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 her personal ideological inclinations more fully.

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 93rd and 94th Congresses. Those members who did not serve in both congresses are dropped from the analysis.

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

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.

Examples of higher-office positions would include Senate seats, Governorships, and Lieutenant Governorships.

We utilized ICPSR study 7803 and the Biographical Directory of the U.S. Congress, 1774 to Present (http://bioguide.congress.gov/biosearch/biosearch.asp) for seniority and retirement/higher-office data.

District-level presidential vote share data were taken from various issues of The Almanac of American Politics.
Since constituency-level factors are no longer relevant for those members exiting the chamber, we follow Rothenberg and Sanders (2000) by coding both the electoral security and the district political change variables as zero for those members who are either seeking higher office or retiring.

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.

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

Like Poole and Romer (1993), we run the model with a full set of dummies included, and thus without a constant. While the final-passage vote regression includes twelve dummies, the procedural vote regression only includes ten dummies, since the 96th Congress is dropped (see footnote 19).


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 our dependent variable (ideological change) is measured in absolute-value terms, a positive coefficient signifies greater change than reelection-seeking members (who are
constrained by party throughout). Likewise, if constituency is also assumed to be a constraint, then the coefficient on the higher office variable should be positive as well.

29 According to Smith, Roberts, and Vander Wielen (2003, Ch. 6: 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.”

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.

30 For example, Smith, Roberts, and Vander Wielen (2003, Ch. 6: 17) note: “[In late 2002, Speaker Dennis Hastert] endorsed Majority Leader Tom Delay’s (R-Texas) proclamation that a Republican member of the party’s organization who voted against the party on any procedural matter would be excused from service.” While this is almost certainly an exaggeration, it indicates the priority that party leaders place on procedural party loyalty.

31 In a recent exchange with Krehbiel (2003a, 2003b), Groseclose and Snyder (2003: 104) acknowledge that their “statistical method cannot discriminate between majority-party influence and minority-party influence.” Krehbiel (2003a: 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.”

32 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 her party’s median from either the left or the right. As a result, we could observe either a leftward or
rightward shift away from the party median when a member decides to retire and indulge her personal ideological inclinations more fully.

Different opinions exist regarding the correct specification of cross-scale models (see, e.g., Carson et al 2004; Rothenberg and Sanders 2004; Crespin, Carson, Jenkins 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; Poole and Rosenthal 1997, p. 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.

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