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Estimating Party Influence in Congressional Roll-Call Voting

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This article develops and implements a simple procedure to estimate the extent to which party influences roll-call voting in the U.S. Congress. We find strong evidence of party influence in both the House and the Senate, in virtually all congresses over the period 1871–1998. We do not find any large, systematic differences in influence between the House and Senate. Over the post-war period, party influence in the House occurs especially often on key procedural votes—the rule on a bill, motions to cut off debate, and motions to recommit. In terms of substantive issues, party influence appears most frequently on budget resolutions, tax policy, social security, social welfare policy, and the national debt limit, while it is relatively rare on moral and religious issues and civil rights, and entirely absent on issues such as gun control. On some issues, such as agriculture, public works, and nuclear energy, party influence has varied dramatically over the period we study.

Virtually all studies of roll-call voting in the United States Congress find that political party affiliation is one of the best predictors of voting behavior. As several researchers have noted, however, one cannot interpret a high correlation between partisanship and voting as evidence of strong institutional party influences inside Congress. Rather, it may simply reflect a high correlation between party affiliation and representatives' personal or constituency preferences (e.g., Shannon 1968; Shade et al. 1973; Fiorina 1974). Krehbiel makes the point clearly:

> In casting apparently partisan votes, do individual legislators vote with fellow party members *in spite of their disagreement* about the policy in question, or do they vote with fellow party members *because of their agreement* about the policy question? In the former case, . . . partisan behavior may well result in a collective choice that differs from that which would occur in the absence of partisan behavior. In the latter case, however, . . . the apparent explanatory power of the variable, party, may be attributed solely to its being a good measure of preferences. (1993, 238; italics in original)

This article explores a simple procedure to estimate the extent to which partisan considerations affect roll-call voting, *independently* of legislators' preferences. The logic underlying the procedure is as follows. First, we assume that on roll calls that turn out to be quite lopsided, parties generally know in advance what the outcome will be (passage or failure). Second, we assume that on such roll calls parties do not try to influence their members' decisions, but allow them to vote as they wish. Given limited resources, this is a sensible strategy. Third, we apply a standard scaling technique to a set

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of sufficiently lopsided roll calls. If the first two assumptions are valid, then scaling these lopsided votes produces consistent estimates of each legislator's "true" preferences that are independent of party influences.[1] Finally, we run regressions on all close roll calls, using the estimated preference parameters plus a party dummy variable as regressors. If the coefficient on the party variable is large and statistically significant in a large fraction of these regressions, then one can conclude party influence is frequently present.

We performed the steps above on the 42nd–105th Congresses (1871–1998). The results indicate that in virtually all Congresses party had a large influence on representatives' voting decisions. On average, in the House of Representatives the coefficient on the party dummy variable was highly significant in nearly 54 percent of the close roll calls. This is much greater than one would expect simply by chance.

Hidden in the average is a considerable amount of variation, both over time and across different types of roll calls. The percentage of roll calls with a significant party effect falls sharply from over 70 percent in the 55th–65th Congresses to under 35 percent in the 91st–99th Congresses. This decline is consistent with well-known findings based on more traditional measures such as "party voting" and "party difference" indices (e.g., Sinclair 1977; Brady, Cooper, and Hurley 1979; Collie and Brady 1985; and Hurley and Wilson 1989). Our results suggest that the decline in party voting and party difference was not due entirely to changing preferences. Rather, part of the change was due to a genuine decline in the parties' abilities to enforce discipline, and this was due most likely to a decline in resources available to the party. As Collie and Brady (1985) and others have pointed out, the amount of party resources is influenced by factors external to the House, especially the electoral environment.

We also disaggregated the set of close roll calls by procedural type, legislative significance, and issue area. Party influence appears much more frequently on certain types of procedural votes—rules on bills, motions to end debate, and motions to recommit—than on amendments and final passage. This is consistent with previous analyses, which show using more traditional measures that partisanship tends to be higher on procedural roll calls than on "substantive" roll calls (e.g., Froman and Ripley 1965; Turner and Schneier 1970). Also, over the post-war period party influence has appeared most regularly on large, bread-and-butter issues that divide the parties—taxation, budget resolutions that set spending priorities, social security, and social welfare policy. Interestingly, we find almost no evidence of party influence on "moral" issues such as abortion, homosexuality, and school prayer. These are often described as issues where members are free to vote their conscience.[2]

Before proceeding, we should discuss in a bit more detail what we mean by "party influence." The clearest form of influence is direct pressure applied by the party leaders or caucus—rewards and punishments that are clearly tied to a member's votes. Rewards may include favorable committee assignments and leadership positions, campaign funds, district visits by party notables, federal projects targeted to a member's district, expedited treatment for a member's favorite bills, and invitations to serve as speaker pro tem. Punishments may include dismissal from key committees, roadblocks placed in front of a member's bills, and reallocation of federal funds away from the member's district.

Why might parties want to discipline their members in this way? One possibility is that party labels serve as "brand-name" labels that convey useful, low-cost information to voters (e.g., Cox and McCubbins 1993; Snyder 1994). For example, a party might have a reputation for producing pro-labor or pro-business policies, and party discipline might be required from time to time in order to remind voters what the party's position is and how strongly it holds that position. By voting against their district interests some of the time, a party's members can put forth a more coherent message to the electorate. Risk aversion might also be a factor. Risk-averse voters prefer candidates and parties with more definitive stances on issues, and this definitiveness can work as a valence advantage for the party at election time (e.g., Hinich and Munger 1989). Finally, rather than being associated with particular ideological or policy orientations, brand names might instead advertise parties' reputations for competence in the job of governing. Parties may be more or less capable at identifying good public policies to attack pressing problems, striking the necessary bargains among their members to pass these policies, avoiding wasteful, pork-barrel-laden, logrolled outcomes, and choosing honest, noncorruptible members. Establishing and maintaining a good reputation may require some party discipline.

Given our method, we cannot rule out the possibility that the influence we find is actually due to more subtle

[1]These "true" preferences may reflect a combination of factors, such as a legislator's personal preferences, the preferences of the legislator's constituents, interest group influences, and cue taking.

[2]Even in the U.K., where the parties are otherwise highly disciplined, votes on such matters as abortion, divorce law, homosexuality, and Sunday entertainments are frequently "free votes" on which no whips are issued (see Richards 1970).

mechanisms. First, the influence members feel may in fact be a sense of personal loyalty to the party or duty to vote the party line, a psychic cost. Second, party influence may take the form of *voluntary* provision of a public good for the party.[3] Finally, in some cases a party's leaders may need to *coordinate* its members' votes on issues that are a public good to the party (e.g., Calvert 1992; Calvert and Weingast 1993). For example, members may be willing to sacrifice their district's interests for the party's good, but only on limited occasions. To insure that their sacrifice is not wasted, a leader may need to specify the occasions on which members are expected to vote the party line. On such occasions, the leader does not trade favors or twist arms, but acts as a focal point for choosing an equilibrium.

Even these more subtle forms of party influence satisfy Krehbiel's criterion of "significant party behavior," however. In each case, the member votes the party line in spite of his or her preferences, not because of them. Furthermore, even if party influence manifests itself only in these less direct forms, the influence still has significant policy consequences. For instance, suppose that the majority party changes between elections, but because there are enough moderates in each party, the ideal point of the median legislator does not change. While policy would not change in a pure median-voter world, it would when party influence exists, even when it is one of the subtle forms previously mentioned.

Details of the Estimation Procedure

Recall that the estimation proceeds in two stages. In the first stage we estimate legislator preferences using one subset of roll calls (lopsided votes). In the second stage we estimate the importance of party influence, focusing on a different subset of roll calls (close votes) and using the preference estimates from the first stage.

To estimate legislators' preferences, we employ the linear factor model studied in Heckman and Snyder (1997). Let legislators be indexed by i and roll calls by j. Let $\mathcal{J}$ be a set of lopsided roll calls, on which there is by assumption no direct party influence. Each legislator is fully described by a preference parameter vector $\mathbf{z}_i = (z_{i1}, \ldots, z_{iK})$. Each roll call $j \in \mathcal{J}$ is described by a vector of characteristics $(a_j, \mathbf{b}_j) = (a_j, b_{j1}, \ldots, b_{jK})$. Letting p_{ij} denote the probability that legislator i votes yea on roll-call j, the linear factor model assumes

$$p_{ij} = a_j + \mathbf{b}_j'\mathbf{z}_i \quad (1)$$

That is, the choice probabilities are *linear* in the preference parameters $z_{i1}, \ldots, z_{iK}$. Heckman and Snyder (1997) show that Equation 1 can be justified in several different ways. One way is to assume that the preference parameter vectors represent legislator ideal points, that legislators' utility functions are all quadratic, and that the random "shocks" associated with each roll call all have uniform distributions. Another justification is that Equation 1 provides a good approximation to *any* binary stochastic choice model, if K is chosen large enough, via a Taylor-series approximation argument. Consistent estimates of these preference parameters can be obtained by ordinary factor analytic techniques applied to the covariance matrix of the roll-call data.[4]

Now consider roll calls on which party has a direct influence on legislators' choices. Let c_{ij} denote the "reward" legislator i receives from the party for voting yea on roll-call j (alternatively, c_{ij} could be the negative of the "punishment" i receives for voting nay). Then the probability i votes yea on roll-call j is

$$p_{ij} = a_j + \mathbf{b}_j'\mathbf{z}_i + c_{ij} \quad (2)$$

Although the c_{ij}'s are generally unobserved, we can investigate various hypotheses about them. For example, consider the hypothesis that party influence is felt uniformly by all members of a party (we call this the "uniform party influence" hypothesis). Let D denote the set of Democratic legislators, let R denote the set of Republicans, and ignore independents and members of third parties. Let c_{Dj} be the influence on Democratic members and c_{Rj} the influence on Republican members. Then party influence exists if (i) $c_{ij} = c_{Dj} \neq 0$ for all $i \in D$, or (ii) $c_{ij} = c_{Rj} \neq 0$ for all $i \in R$, or (iii) both. In any of these cases, Equation 2 can be written as

$$p_{ij} = a_j + \mathbf{b}_j'\mathbf{z}_i + c_j d_i \quad (3)$$

where d_i is an indicator variable for Democrats, so $d_i = 1$ for $i \in D$ and $d_i = 0$ for $i \in R$.

The estimate of c_j measures the *difference* in influence between the two parties. It does not necessarily reflect the total amount of party influence or the amount of influence applied by any one party. For example, if $c_{Dj} = c_{Rj}$, then $c_j = 0$ even if c_{Dj} and c_{Rj} are not zero. That is, if

[3] We thank Jim Stimson for making this point.

[4] Strictly speaking, we estimate linear combinations of the preference parameters, since they are identified only up to a shift and rotation of the underlying preference scale. See Heckman and Snyder (1997) for details.

Democrats and Republicans both feel their parties are pressuring them to vote yea (and they feel this pressure to the same degree), then we will not be able to detect any party influence. Similarly, if Democratic leaders pressure both Democratic and Republican members, while Republican leaders exert no pressure, then we may not be able to detect this pressure. On the other hand, if the parties both pressure their members to vote in opposite directions, then c_j will measure the sum of the two parties' separate effects.[5]

In order to estimate the coefficients a_j, $\mathbf{b}_j$, and c_j for roll-call j, two further complications must be addressed. First, we do not observe voting probabilities, but only voting decisions. Second, legislators' preference parameters are estimated rather than known. Let

$$v_{ij} = \begin{cases} 1, & \text{if } i \text{ votes yea} \\ 0, & \text{if } i \text{ votes nay} \end{cases}$$

be legislator i's actual vote on roll-call j. Also, let $\tilde{\mathbf{z}}_i$ denote the estimated preference parameters for legislator i. Then the equation we can actually estimate in the second-stage regressions is

$$v_{ij} = a_j + \mathbf{b}_j'\tilde{\mathbf{z}}_i + c_j d_i + \varepsilon_{ij}. \tag{4}$$

Since v_{ij} is dichotomous, ε_{ij} is heteroskedastic. We therefore use the Huber-White technique for consistently estimating the covariance matrix of the estimated coefficients (White 1980).

The fact that the preference parameters are estimated rather than known is more of a problem, since it means some of the regressors in Equation 4 contain estimation error, $\mathbf{z}_i - \tilde{\mathbf{z}}_i$. Unless this error tends toward zero as the number of legislators increases, least-squares (LS) estimates using the estimated values as regressors will be inconsistent. The size of this error, however, depends primarily on the number of lopsided roll calls used in estimating the $\mathbf{z}_i$. For some congresses, the number of lopsided roll calls is large enough that the error is probably negligible, but for other congresses this may not be the case. One way to deal with this problem is simply to ignore it and proceed with least-squares estimation as if the preferences were measured without error. In fact, this is the typical approach taken in previous studies—dozens of analyses have used roll call-based scores from interest groups such as the Americans for Democratic Action (ADA), or the parameter estimates from a scaling procedure such as Nominate, and treated these as errorless measures of preferences or "ideology." An alternative is to use instrumental variables (IV). In the following analysis we present both LS and IV estimates, using the IV estimator proposed by Durbin. For this estimator, the rank-orders of the independent variables are used as instruments (see Johnston 1972, for details).

In the empirical results presented below we focus on the uniform party influence analysis and estimates of Equation 4. Other hypotheses about the c_{ij}'s are possible, however, and we studied two in some detail. The first is that party moderates are offered rewards more often than extremists. The second is that junior members are influenced more often than senior members.[6] Initially, we anticipated that these hypotheses might yield even more interesting results than the uniform party influence hypotheses, since they are based on more subtle party strategies. In practice, however, problems of multicollinearity make it difficult to distinguish among them.

Results of Monte Carlo Simulations

The procedure previously described is somewhat complicated, and the quality of the estimates depends on several key factors. First, we must be able to adequately recover legislators' preference parameters from the set of lopsided roll-call votes. We know we can do this with "enough" roll calls (see Heckman and Snyder 1997), but we do not know how many roll calls is "enough." Second, we assume that party influence is never present on lopsided roll calls. What if this assumption is violated? Intuitively, this should cause us to *understate* the true amount of party influence, even on close roll calls, since our estimates of the preference parameters will already incorporate some of the effect of party. Is this intuition correct? Third, too much collinearity between party membership and preferences, together with the limits imposed by chamber size (there are at most 435 representatives and 100 senators voting on each roll call), will make it impos-

[5]Another key assumption underlying our approach is that all of the "other" factors, z_i, that influence voting on close votes also influence voting on some lopsided votes, or that such factors are uncorrelated with party. One type of factor that may appear much more on close votes than lopsided votes is interest-group pressure. However, since there are many different groups with many different agendas, and since groups probably target moderate legislators when they attempt to influence roll-call voting (a prediction of virtually all models of interest group politics, either of the "vote-buying" or "informational" variety), group influence is likely to be uncorrelated with party.

[6]Testing these hypotheses require only minor modifications of Equation 4. For example, to test the first hypothesis we estimate $v_{ij} = a_j + \mathbf{b}_j'\tilde{\mathbf{z}}_i + c_{D0j}d_i + c_{Dj}\tilde{m}_i d_i + c_{Rj}\tilde{m}_i r_i + \eta_{ij}$, where $\tilde{m}_i$ is a measure of the degree to which member i is a moderate. We constructed this measure by projecting ideal points onto the line containing $\bar{\mathbf{z}}_D$ and $\bar{\mathbf{z}}_R$, the mean ideal points of the two parties.

sible to distinguish empirically between preferences and party influence. Again, how much multicollinearity is "too much?"[7]

We conducted an extensive set of Monte Carlo simulations to provide some insight into these matters. Details of the simulations are in Appendix A. The basic points are as follows.

First, we obtain relatively accurate estimates of preference parameters, even when we use only lopsided roll calls, as long as we have "enough" roll calls—500 or so is plenty, 250 is good, and even 100 appears adequate. In addition, our estimates of moderate legislators are just as accurate as our estimates of extremists.[8]

Second, if party influence is absent on all roll calls, then in the second-stage regressions the estimated coefficient on party is significant only a small fraction of the time (3–6 percent). That is, our method produces few type-II errors.

Third, if party influence is almost never present on lopsided roll calls, but is present on a large fraction of close roll calls, then we do fairly well at estimating the fraction of close roll calls on which party influence is present. If anything, we make more type-I errors than type-II errors and therefore tend to understate the incidence of party influence. Also, the estimates of party influence are much better when we use lopsided roll calls to estimate legislators preferences than when we use *all* roll calls. When we use all roll calls to estimate preferences, we severely understate the amount of party influence.

Fourth, when party influence is also present on a large fraction of the lopsided roll calls, our method tends to understate the incidence of party influence, even on close roll calls, and the extent of this underestimation may be large. We do even worse at estimating party influence, however, by using all roll-call votes to estimate preferences.

Fifth, when the correlation between party and preferences is very high—this happens, for example, when the gap between the parties is large—we tend to underestimate the incidence of party influence. For example, if the correlation coefficient between the party dummy and estimated legislator-preference parameters is .98, then we may fail to identify a party effect, even when one is present, more than 50 percent of the time. Collinearity does not increase the number of type-II errors, however, at least in the ranges we explored.

Results on the Uniform Party Influence Hypothesis

In this section we apply the method previously described to roll-call voting in the 42nd–105th Congresses. The data are all from standard sources. The roll-call and party-affiliation data are from the Inter-University Consortium for Political and Social Research (ICPSR), augmented with data sets generously provided by Charles Stewart.[9] We calculated seniority using data in McKibbin and the ICPSR (1993) and *The Almanac of American Politics*. We determined the set of party leaders from Nelson (1976), the *Congressional Quarterly Guide to Congress* (1991), and the *Congressional Quarterly Almanac.*

To estimate representatives' preference parameters, for most congresses we define "lopsided" roll calls as those where more than 65 percent or fewer than 35 percent of representatives voted yea. In most congresses such roll calls are probably lopsided enough to safely assume that party influences on voting were minimal. For congresses where one or the other party controlled more than 62 percent of the seats, however, we use 70 percent and 30 percent as the cutoffs. Party influence may sometimes be used to signal party solidarity on an issue or party support for the president, rather than simply to assure that a motion passes or is defeated. When one party controls nearly 65 percent of the seats, using a 65-percent cutoff means that nearly all party-line votes are defined as lopsided.[10]

[7]Another important question is whether the estimates are robust to missspecification of the functional form. We do not report on this issue here because of space limitations. We have investigated the problem, however, and find that when extra preference factors and/or polynomial terms involving the preference factors are included in the second-stage regressions to pick up nonlinearities, then the estimated incidence of party influence is relatively good.

[8]The following provides some intuition for why this is true. Imagine legislators arranged ideologically from left to right. In our first stage we mainly exclude all roll calls for which the cut point falls between the 35th and 65th percentile members (since we only include roll calls for which the winning side was 65 percent or greater). Thus, if there were no "shocks" in voting, the moderates would generally vote as a block, and our method would not estimate their preferences well. With shocks, however, they do not vote as a block. Rather, some moderates vote on the left-wing side of roll calls with left-wing cut points, and on the right-wing side of roll calls with right-wing cut points. This is rarely true for extremists, however. For instance, unless the voting shock is huge a right-wing extremist will never vote on the left-wing side of a left-wing cut point. Consequently, two sets of cut points—extreme right ones and extreme left ones—help to identify moderates, while only one set helps to identify extremists.

[9]We code "paired for" as a yea vote and "paired against" as a nay vote.

[10]For votes that require a supermajority—e.g., treaties and cloture votes in the Senate and votes to suspend the rules in the House—we define a roll call as lopsided if it received less than 51.7 percent support or more than 81.7 percent support (the cutoffs are defined

In the second-step regressions we must include *all* of the parameters (factors) required to describe representatives' preferences. Omitting any preference parameters that are correlated with party affiliation will tend to bias the results towards finding significant party effects. Of course, including too many factors biases the results in the opposite direction, since it reduces the efficiency of the estimates. This is probably a less serious problem, however, and since our challenge is to isolate the effects of party from preferences, it is better to err on the side of not finding a party effect. We therefore use a conservative criterion for retaining factors, which leads us to retain between three and fourteen factors depending on the Congress studied (the average is seven factors). This criterion is described in Appendix B.[11,12]

We were forced to drop nineteen out of sixty-four Houses and seventeen out of sixty-four Senates from the analysis due to data problems. First, we dropped all cases in which there are fewer than fifty lopsided roll calls, since we have little confidence in the preference parameter estimates based on so few roll calls.[13] Second, in a number of congresses multicollinearity between party affiliation and the estimated preference parameters is so severe that distinguishing between party effects and preferences in the second-step regressions is effectively impossible. This typically occurs when the minority party is very small and homogeneous.[14] The lack of variation in the preferences of the minority party's membership makes the label "Democrat" or "Republican" virtually synonymous with a particular point in the preference parameter space. We dropped a congress if the multiple correlation between the Democratic dummy variable and the preference parameter variables is greater than .95.[15] Finally, we dropped congresses where one party controlled more than 68 percent of the seats. The Houses dropped are the 43rd, 44th, 51st–54th, 56th–59th, 61st, 63rd, 67th, and 70th–75th, and the Senates dropped are the 42nd–44th, 46th, 51st, 53rd, 56th–60th, 63rd, 64th, and 74th–76th.

Figure 1 presents a summary of the House results based on least-squares estimates of Equation 4. The top two plots in the figure give the fraction of "close" (65 percent majority or less) and "very close" (60 percent majority or less) roll calls for which the coefficient on the party-dummy variable is estimated to be statistically significant at the 1 percent level. The lower plot gives the fraction of estimated significant coefficients on lopsided roll calls.

As noted above, we also estimated Equation 4 using instrumental variables. In most congresses, the least-squares estimates produce a slightly larger number of statistically significant party coefficients than the instrumental variables estimates. However, the differences between the two are small—on average, the IV estimates of the party effects are statistically significant on 2.7 percent fewer roll calls than for the LS estimates. In what follows we focus on the least-squares estimates, since they are probably more familiar to most readers.

Several features of Figure 1 are noteworthy. First, party influence is quite high by our measure. Except in the 42nd and 93rd Houses, the percentage of close roll calls with a statistically significant party coefficient is always greater than 30 percent, and the average for the entire period is 54 percent. Since the significance level used

by 66.7 ± 15 percent). Also, we drop roll calls on which more than 99 percent voted with the majority. As Poole and Rosenthal (1997) note, such near-unanimous roll calls contain little information about relative preferences.

[11]After a point, the exact number of factors retained is not critical. That is, after choosing "enough" factors, the estimated c_j's do not change significantly as more factors are included as regressors. Details on this point are available from the authors on request.

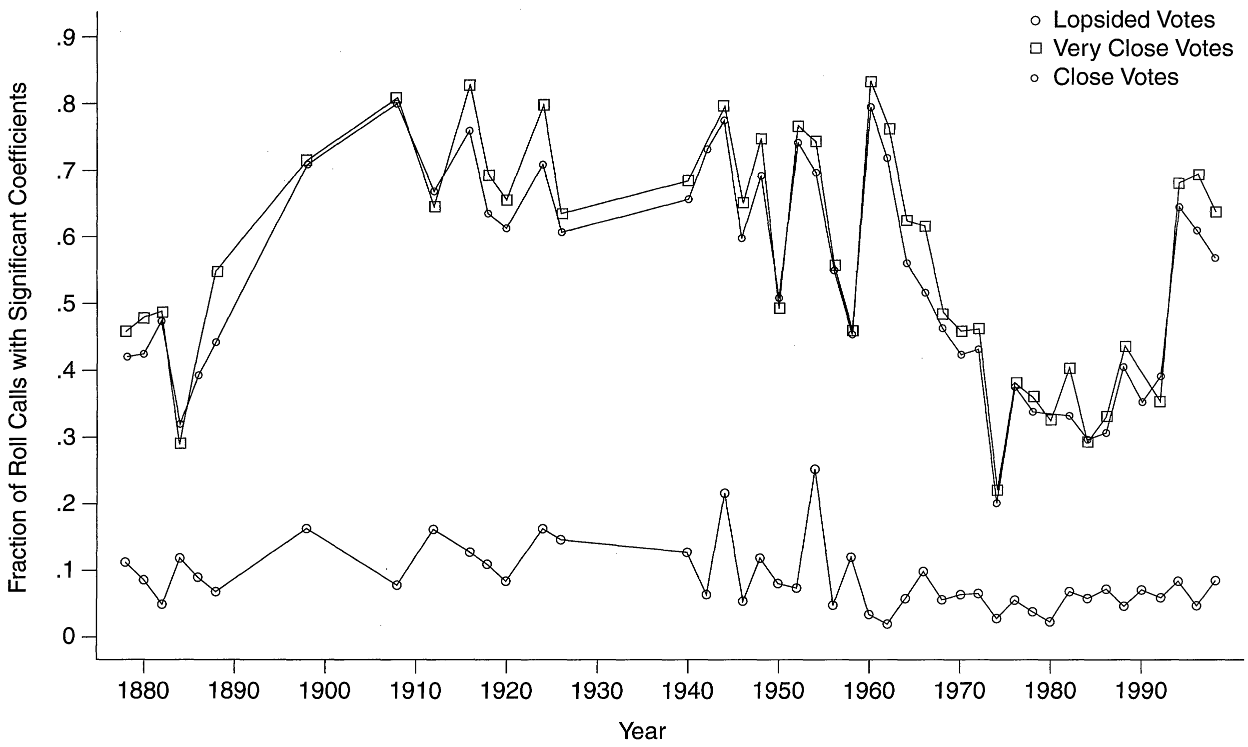
[12]Using goodness-of-fit measures such as classification success rate and geometric mean probability, Poole and Rosenthal (1997) argue that only one or perhaps two spatial dimensions are necessary to characterize the broad pattern of congressional voting for almost all of U.S. history. On the other hand, Heckman and Snyder (1997) employ a more rigorous statistical-testing procedure, which gives a lower bound on the number of factors, and find that there are at least five or six significant factors in most of the postwar congresses. This is also consistent with the work of Clausen and his colleagues, who argue that at least five "policy dimensions" are necessary to capture the structure of congressional voting (see Wilcox and Clausen 1991). Such a large number of factors might or might not be necessary to adequately describe the "broad" patterns of congressional voting. More than two factors are necessary, however, to adequately capture all of the detailed structure of the representatives' preferences that is correlated with party affiliation. In every congress in our study there is at least one factor in addition to the first two factors that is significantly correlated with party, and in most congresses there are several such factors.

[13]This may be even be too generous, given the results of our Monte Carlo simulations.

[14]In a few congresses, such as the 51st, 57th, and 60th, unusually strong party discipline may have produced the multicollinearity. In the 51st and 57th Houses, for example, nearly half of the roll calls pitted 90 percent or more of the Republicans against 90 percent or more of the Democrats. The leadership in these congresses might have commanded so many resources that they could afford to spend them even on lopsided roll calls (see, e.g., Brady and Althoff 1974).

[15]The 46th, 49th, 53rd, 98th, and 103rd Houses, and the 92nd and 97th Senates, exhibited a very high level of multicollinearity, but fell below the .95 threshold and are therefore included in the analysis. We also included the 102nd–104th Houses and the 103rd–104th Senates, even though the multicollinearity of these congresses was greater than the .95 threshold, because of the natural interest many scholars have in the apparent resurgence of party in recent congress.

FIGURE 1 Party Pressure in House of Representatives



is 1 percent, it is extremely unlikely that the party influences we detect are due to chance alone. The percentage of very close roll calls with a significant party effect is typically slightly higher than the percentage for close roll calls, and the average for the entire period is 57 percent.

Second, although our measure of party influence is somewhat related to traditional measures of party unity, the relation is far from perfect. For instance, Rohde (1991, 46) notes the steady and steep increase in party-unity voting among the Democrats during the 92nd–100th Congresses (1971–1988). However, we find party influence to be low and mostly flat during the period. This suggests that things like greater homogeneity of interests in the party and an agenda that produced fewer conflicts among party members (i.e., fewer roll calls with cut points in the middle of the distribution of the party's ideal points) were primarily responsible for the increase in unity, not genuine party influence.

Third, party influence is only loosely related to party polarization. For instance, during the 56th–87th Congresses (1899–1962) the level of party influence is relatively high and fairly steady. On the other hand, the degree of party polarization exhibits some large swings during that same period. Using Poole and Rosenthal's (1997) measure party polarization begins at a high level in 1900, falls drastically between 1900 and 1910, then remains steady at a relatively low level throughout the remainder of the period (1910–1962).[16] Poole and Rosenthal also find that party polarization reaches a low point around 1979 (the first session of the 96th Congress), then begins a steady climb, reaching noteworthy high levels in the 1990s.[17] However, we find nothing remarkable about 1979 in terms of party influence. For example, while the level of party influence in the 96th Congress was low, the level is approximately as low as the level in the three preceding and three subsequent congresses.

As one check on our method, we measure the percentage of *lopsided* roll calls with a significant party coefficient. On average this is just 8.8 percent (see the lower plot in Figure 1). Although this is larger than 1 percent, implying that the assumption of no party effects on lopsided roll calls is false, it is much lower than

[16]Poole and Rosenthal (1997) measure polarization as the difference in mean Nominate scores of the parties (see p. 83 for these measures).

[17]See King (1997) for a discussion of the recent high levels of party polarization in Congress. These levels, however, while high, are fairly low in comparison with the 1900–1910 period.

the 54-percent figure for close roll calls. Moreover, if we consider only those congresses with more than 200 lopsided roll calls—cases where we have relatively precise estimates of representatives' preferences—then the average percentage of lopsided roll calls with a significant party coefficient drops to 5.8 percent. The corresponding percentage for close roll calls in these congresses is 41 percent (44 percent for very close roll calls).

Part of the difference between close and lopsided roll calls may be a statistical artifact. There is much less variation in the dependent variable on lopsided roll calls than on close roll calls, and consequently less room for a dummy variable such as party to have much explanatory power. Interestingly, however, controlling for preferences has an even *greater* effect on lopsided votes than on close votes. If we do not control for preferences in the regressions, the party dummy is significant on nearly all close roll calls—about 93 percent, on average. However, the party dummy is also significant on almost 73 percent of all lopsided roll calls. As noted above, when we control for preferences the party dummy is significant 54 percent of the time on close roll calls, but only about 9 percent of the time on lopsided roll calls. The difference in these percentages—93 – 54 = 39 percent for close roll calls and 73 – 9 = 64 percent for lopsided roll calls—yield crude estimates of the percentage of roll calls where party differences are due entirely to preferences. The fact that this is much higher for lopsided roll calls suggests that the large differences between close and lopsided roll calls shown in Figure 1 is *not* due simply to differences in the variability of the underlying dependent variable.

In any case, since the results indicate that the estimated preference parameters based on lopsided votes are not completely "purged" of party, it is likely that the party coefficients in the second-stage regressions are biased toward zero (recall the Monte Carlo results). With cleaner preference measures we might find significant party effects even more often.

Importantly, the results above are robust to different functional forms for specifying preferences. For example, we estimated a variant of Equation 4 that includes the squared values of the z_{ik} terms and another variant where we included polynomials of the dominant first factor up to the 5th degree. These specifications do not significantly change the conclusions. For instance, in the specification where we include 2nd-order to 5th-order terms, the average percentage of close roll calls with a significant c_j is 48 percent. This is only slightly below the 54-percent figure previously reported. Moreover, part of the difference may be due to statistical inefficiencies introduced by the existence of a large number of superfluous regressors in the model that contains the higher-order terms. We also estimated a variant that includes a dummy variable indicating whether the member was one of the 50 percent most "liberal" members in the chamber.[18] Again, the results do not change significantly (e.g., the party dummy is significant on 51 percent of close roll calls, on average, and on fewer than 8 percent of lopsided roll calls).

The Revealed Rationality of Party Influence

Some basic assumptions about rational strategies by party leaders provide predictions about the sign of c_j. These serve as further checks that we are measuring genuine party influence and not omitted factors for which party is a proxy.

For each roll call, c_j measures the party influence felt by the members of one party *relative* to that felt inside the other party. It does not necessarily indicate the absolute amount of party influence, nor the amount of influence inside any one party. This lack of determinism, however, does not mean that there are no clear predictions about the signs of the c_j's, at least in the case of uniform party influence. If party leaders or caucuses allocate their resources rationally, then the c_j's must be consistent with party leaders or caucuses trying to influence members of their party to vote *with* them rather than against them. For example, if Democratic leaders favor a yea vote and Republicans favor a nay vote, then c_j should be positive. Alternatively, if Democrats favor a nay vote and Republicans a yea vote, then c_j should be negative. Similar predictions hold if one party's leaders favor a yea or nay vote and the other party's leaders are divided. On the other hand, if both groups of leaders want a yea vote or both groups want a nay vote, then there is no clear prediction for c_j. Finally, if both groups of party leaders are divided then c_j should be close to zero.

The sign of c_j is almost always consistent with these predictions. We coded the leadership of a party as "favoring a yea (nay)" if at least three-fourths of the party leaders voted yea (nay). We then examined the estimated sign of c_j relative to its predicted sign for all close roll calls where c_j is statistically significant at the 1-percent level. For most congresses c_j is consistent with rational use of party influence nearly 100 percent of the time—in the

[18] To construct this variable we use the means of each party, $\bar{z}_D$ and $\bar{z}_R$. We then project each member's ideal point z_i to the line containing $\bar{z}_D$ and $\bar{z}_R$. The half on the $\bar{z}_D$ side are coded as 1, the others as 0.

House, the average for the entire period is over 96 percent. If we consider all close roll calls (including those for which c_j is statistically insignificant), then on average c_j is consistent with rational party influence 99 percent of the time. The figures are similar for the Senate.[19]

Since it is rare for leaders of both parties to be divided, it is also rare for rational party influence to predict a zero coefficient for c_j. In the average congress this occurs on just 4.5 percent of the close roll calls. Even here, however, the rational party influence hypothesis generally makes the correct prediction. On close roll calls for which the predicted value of c_j is zero, the estimated c_j is significantly different from zero only 14 percent of the time. By contrast, on average c_j is significant in 56 percent of the close roll calls on which at least one party's leaders are united.

Party Influence on "Party Priority" Legislation

In a few cases, congressional scholars have identified more direct measures of leadership influence—or at least leadership interest—on bills. If our procedure is accurate, then we should find a higher incidence of party influence on these roll calls than on other roll calls.

We check three lists of roll calls: (1) the roll calls taken during 95th and 96th Congresses on which the House Democratic leadership assigned a "Speaker's Task Force" (Sinclair 1983, 139); (2) the House leadership's "top priority legislation" for the 95th and 96th Congresses (Sinclair 1983, 245); and (3) Speaker Jim Wright's "legislative priorities" for 1987 (Rohde 1991, 110).

The results are shown in Table 1. We compare the close roll calls on the relevant list with all close roll calls on regular legislation (bills, joint resolutions, and concurrent resolutions) taken during the same Congress. Also, we break the roll calls into three types: (1) votes on rules, motions to end debate, and motions to recommit; (2) votes on final passage and conference reports; and (3) votes on amendments.

The results in Table 1 strongly support our expectations. For each list and all types of votes, party influence appears more frequently for the subset of roll calls *on* the list than for the subset of roll calls *off* the list. Also, except in three cases these differences are statistically significant at the .01 level (based on t-tests). In addition, on the Leadership Task Force votes we also know for certain whether the leadership wanted a "yea" or "nay" outcome; on *all* these roll calls where the party dummy is statistically significant its sign is consistent with the leadership's preferences.

More Disaggregated Results

In this section, we disaggregate the set of bills along three different dimensions that have been explored previously in the literature. The purpose is partly to show the extent to which the results of our procedure comport with intuition and conventional wisdom and partly to begin using the procedure to identify new patterns in the data.

Using short descriptions of each roll call given in the ICPSR roll-call data set codebooks and *Congressional Quarterly Almanacs*, sometimes augmented by additional information in the *Congressional Quarterly Almanacs* and the Appendix to the U.S. Government Budget, we coded virtually all roll calls taken during the 80th–101st Congresses in terms of their issue content and procedural type (rule, amendment, passage, etc.). We also use data on the relative "legislative significance" of bills, generously supplied by Charles Cameron, for all bills that ultimately became law (see Cameron et al. 1996, for details).

Numerous observers of congressional policy making have remarked on the importance of rules and procedural votes in determining the fate of legislation. For example, Oleszek (1996) tracks the sharp increase in the Democratic leadership's use of restrictive rules, from 15 percent in the 95th Congress to 70 percent in the 103rd. These rules often limit the amendments that can be offered on a bill, helping bill managers control the proceedings. Oleszek also notes that the House rarely rejects a rule proposed by the Rules committee, and that "it is an expectation within the majority party that support for rules is a given and that deviations from this behavioral norm could be held against a lawmaker when, for example, plum committee assignments are handed out" (1996, 151). As another example, Froman (1967) notes that voting on the motion to recommit a bill is sometimes considered more important than the vote on final passage.

We took all close roll calls on bills and joint resolutions and divided the set of roll calls into procedural

[19] Using overall party caucus preferences to predict the sign of c_j, rather than just party leaders, produces similar results. Suppose we predict c_j to be positive (negative) when the average Democrat favors a yea vote more (less) than the average Republican—i.e., the predicted sign of c_j is positive (negative) when $\hat{\mathbf{b}}_j\bar{\mathbf{z}}_D > (<) \hat{\mathbf{b}}_j\bar{\mathbf{z}}_R$. Focusing on roll calls where there was a nontrivial difference between the parties' estimated preferences ($|\hat{\mathbf{b}}_j\bar{\mathbf{z}}_D - \hat{\mathbf{b}}_j\bar{\mathbf{z}}_R| > .05$) we find, again, that the estimated sign of c_j is almost always consistent with the predictions—95 percent of the time, on average, on close roll calls with a significant party coefficient.

TABLE 1 **Party Influence on Selected Subsets of Roll Calls**

	All Votes		Rule/Motion		Passage		Amendment	
Speaker Wright's Priority Legislation, 1987								
On Priority List	.66	(29)	.89	(9)	.50	(4)	.56	(16)
Not on List	.35	(258)	.67	(52)	.31	(36)	.26	(170)
Votes with a Speaker's Task Force, 95th–96th Congress								
Had Task Force	.72	(25)	1.00	(4)	.83	(12)	.44	(9)
No Task Force	.33	(919)	.55	(96)	.20	(124)	.31	(623)
Leadership's Top-Priority Legislation, 95th–96th Congress								
On Priority List	.59	(133)	.78	(27)	.29	(17)	.59	(85)
Not on List	.29	(811)	.49	(73)	.25	(119)	.27	(547)

Each cell lists the fraction of close roll calls of the given type for which there is a significant party effect. The total number of close roll calls for the cell is listed in parentheses.

votes—rules, motions to end debate, and motions to recommit bills—amendments, and votes on final passage. The coefficient on the party variable is statistically significant on 60 percent of the rules on bills (n = 266), 68 percent of the motions to end or limit debate (n = 193), and 61 percent of the motions to recommit, (n = 516), but only 31 percent of the votes on final passage (n = 836) and 32 percent of the roll calls on amendments (n = 2517). Thus, the party variable is significant about twice as often on key procedural votes. These results are broadly consistent with theoretical results on vote-buying (e.g., Snyder 1991; Groseclose and Snyder, 1996; Groseclose, 1996). Buying votes on roll calls is generally expensive, and it may be cheaper for bill managers to avoid fighting off a large set of amendments by buying votes just once, on a restrictive rule or a motion to move the previous question. On the other hand, the motion to recommit a bill to committee (with or without instructions) is highly privileged in the House and can rarely be avoided—discipline among a bill's supporters will be required to defeat the recommittal motion.

A "purer" sample consists of bills for which there were roll calls both on a motion to recommit and on final passage, and both roll calls were close. We found 172 such cases. Interestingly, for 26 percent (45 out of 172) of these bills, party influence appears to be present on the motion to recommit but not on the vote on final passage. There are almost no cases where the reverse is true (only 2 out of 172). Altogether, the effect of party is significant on 64 percent of the recommittal motions on these bills, but only on 39 percent of the final passage votes.

We also find that estimated party influence is somewhat higher on "landmark" legislation than on "ordinary" bills. On one hand, this is to be expected because party leaders should have an especially strong desire to win votes on landmark bills. On the other hand, rank-and-file legislators may have an especially strong desire to vote their own preferences, or their constituents' preferences, on these votes. It may therefore be especially difficult for party leaders to influence them. We use Cameron et al. (1996) to define landmark and ordinary bills and analyze all close roll calls on these bills.[20] On procedural votes there is no noticeable difference between landmark and ordinary bills—the party coefficient is statistically significant 61 percent of the time on landmark bills (n = 95) and 64 percent of the time on ordinary bills (n = 289), and the small difference of 3 percent shown in the table is not statistically significant. On final passage and amendments, however, the frequency of party influence is noticeably higher on landmark bills than on ordinary bills. For final passage votes, the party coefficient is significant 36 percent of the time on landmark bills (n = 53), but only 23 percent of the time on ordinary bills (n = 248). For amendments, the percentages are 45 percent for landmark bills (n = 120) and 29 percent for ordinary bills (n = 730). Both of these differences are statistically significant at the .05 level using t-tests. This suggests that party leaders can achieve some degree of discipline even on highly salient issues.

Finally, Table 2 shows how estimated party influence varies across issue areas.[21] Evidence of influence appears most frequently and steadily on large issues that have

[20] These also correspond closely to Mayhew's (1991) "Sweep One" bills. We found little difference in party influence across the other categories defined by Cameron et al. and therefore lump all these bills together in the "ordinary" category.

[21] We coded all roll calls using the descriptions in ICPSR study number 0004 and various *Congressional Quarterly Almanacs*.

TABLE 2 **Party Influence by Issue Area, 1947–1990**

	Congresses							
Policy Area	**80–83**	**84–86**	**87–89**	**90–92**	**93–95**	**96–98**	**99–101**	**overall**
Budget Resolutions	—	—	—	—	.67	.63	.78	.68
					(51)	(90)	(45)	(186)
Tax Policy	.88	.67	.64	.63	.54	.67	.31	.60
	(16)	(6)	(14)	(8)	(65)	(24)	(13)	(146)
Debt Limit	.50	.00	.87	.56	.50	.75	.30	.58
	(2)	(2)	(15)	(16)	(20)	(20)	(17)	(92)
Continuing Appropriations	.93	1.00	.67	.90	.40	.49	.47	.57
	(15)	(2)	(3)	(10)	(10)	(37)	(49)	(126)
Govt. Org./Ethics/Elections	.77	1.00	.70	.60	.53	.44	.59	.56
	(13)	(8)	(23)	(53)	(128)	(88)	(69)	(382)
Social Security	.50	—	1.00	.33	.69	.29	.50	.54
	(4)		(2)	(3)	(13)	(7)	(6)	(35)
Energy Policy	.80	.80	.75	.60	.38	.51	.00	.45
	(10)	(10)	(8)	(5)	(130)	(57)	(7)	(233)
Social Welfare	.50	.82	.70	.57	.33	.32	.32	.43
	(46)	(17)	(40)	(67)	(93)	(98)	(93)	(454)
Agriculture/Rural Development	.67	.81	.59	.53	.22	.19	.23	.42
	(36)	(32)	(44)	(49)	(89)	(43)	(43)	(336)
Labor Policy	.78	.56	.51	.54	.35	.28	.26	.40
	(23)	(30)	(43)	(83)	(168)	(82)	(86)	(515)
Business Regul. & Assist.	.42	.57	.77	.52	.31	.27	.36	.38
	(12)	(7)	(13)	(23)	(80)	(45)	(33)	(213)
Public Works/Transportation	.59	.80	.48	.28	.12	.22	.26	.35
	(29)	(44)	(31)	(32)	(67)	(60)	(57)	(320)
Education	.38	.36	.64	.35	.15	.18	.31	.28
	(8)	(11)	(14)	(34)	(41)	(38)	(16)	(162)
Environment	—	.67	.60	.10	.29	.18	.19	.26
		(6)	(10)	(21)	(109)	(40)	(43)	(229)
Foreign Policy/Foreign Aid	.52	.11	.43	.18	.14	.31	.33	.26
	(23)	(19)	(53)	(55)	(162)	(108)	(83)	(503)
Defense	.86	.82	.44	.25	.14	.16	.27	.24
	(7)	(11)	(9)	(24)	(84)	(112)	(168)	(415)
Civil Rights	.44	.50	.39	.15	.05	.07	.19	.19
	(9)	(4)	(28)	(26)	(37)	(28)	(21)	(153)
Moral/Religious Issues	—	—	—	.00	.11	.05	.04	.06
				(3)	(26)	(22)	(26)	(77)

Each cell gives the fraction of close roll calls of the given type for which there is a significant party effect. The total number of close roll calls for the cell is listed in parentheses.

clearly distinguished the parties for much of the post-war period, such as budget resolutions that set overall spending priorities, tax policy, social security, social welfare policy, and the national debt limit. There is less evidence of party influence on defense, foreign-aid votes, and civil rights votes. Influence on labor votes and business regulation fall at an in-between level. Finally, party influence appears to be very rare on moral and religious issues (e.g., abortion and school prayer) and *never* occurs on gun control.[22] This last finding offers some strong support for our method, analogous to Sherlock Holmes' "dog that didn't bark." If, as a critic might claim, our party variable only acts as proxy for unseen preferences, then why doesn't this variable bark on moral issues and gun control?

The importance of party influence has changed dramatically over time on some issues, such as agricultural policy, public works and transportation spending, and nuclear energy policy. For example, the party dummy was significant on 63 percent of the roll calls on agricultural bills taken during the 80th–92nd Congresses, but this fell to 21 percent during the 93rd–101st Congresses. Similarly party influence on public works bills fell from

[22]Gun control is not listed separately in Table 2. There were nine roll calls on gun control over the period, and the party dummy was statistically insignificant for all of them.

64 percent in the 80th–89nd Congresses to 21 percent in the 90th–101st Congresses. Party influence on nuclear energy votes fell from 85 percent in the 80th–92nd Congresses to just 9 percent in the 93rd–101st Congresses.

Assessing the Importance of Party Influence

Counting the number of roll calls with statistically significant c_j's is proper for establishing the existence of party influence. However, to measure the overall importance of this influence it is necessary to examine the c_j's themselves. This is especially true for comparing the House and Senate, since the chambers differ substantially in size (which causes the estimated standard errors to be larger in the Senate).

Figure 2 shows the average absolute value of the c_j's on close roll calls for both the House and Senate. Interestingly, party influence does not appear to be consistently higher in either chamber. The average values of $|c_j|$ are .37 in the House and .35 in the Senate. It is also interesting that the pattern of party influence over time is somewhat similar in the two chambers—roughly, up, down, then up again. The correlation between the average absolute c_j's in the House and Senate is .63. This suggests either that forces external to Congress have a strong influence on the overall level of party influence, or, perhaps, that each chamber reacts to what is happening in the other chamber.

More interestingly, we can use our estimates to divide more traditional party difference measures into the part that is due to preference similarity and the part that is due to influence. One common measure is the "party difference" index. The party difference on a roll call is defined as the absolute value of the difference between the fraction of Democrats who voted yea and the fraction of Republicans who voted yea, and the party difference index for a given congress is simply the average party difference across all roll calls taken in the congress:[23]

$$\delta = \left| \frac{1}{\# D} \sum_{i \in D} v_{ij} - \frac{1}{\# R} \sum_{i \in R} v_{ij} \right|$$

Since c_j represents the difference in party influence, the quantity

$$\tilde{\delta} = \left| \frac{1}{\# D} \sum_{i \in D} v_{ij} - \frac{1}{\# R} \sum_{i \in R} v_{ij} - \hat{c}_j \right|$$

provides an estimate of what the party-difference index would have been had no independent party influence been present, that is, what the party-difference index would have looked like based on preference similarity alone.[24]

Table 3 shows δ and $\tilde{\delta}$ for the House, calculated using all roll calls.[25] The table shows clearly that a large fraction of party votes would still have been party votes even in the absence of party influence. This illustrates Krehbiel's (1993) point that party can appear to have a causal effect simply because it is a good proxy for preferences.

The difference between δ and $\tilde{\delta}$ provides a measure of the amount of party voting that is due to direct party influence. This difference is shown in columns 4 and 8 of Table 3. The difference is correlated with the overall level of party difference, but not perfectly (the correlation is .78). The relative importance of preferences and party influence has changed over time. For example, while the high levels of overall party difference in the 42nd–76th Congresses are driven substantially by party influence, the similarly high levels of party voting in the 98th-102nd Congresses are due mainly to preference homogeneity among the members of each party.[26] The sharp decline in the share of the party difference that is attributable to party influence occurred during the period 1965–1970. Perhaps it is not coincidental that this coincides with the passage of major civil rights legislation, the escalation of the Vietnam war, and intense conflict inside the Democratic coalition.

The overall picture presented in Table 3 does not change if we restrict attention to close roll calls. The figures in the d column shift up noticeably, while those in the $\tilde{\delta}$ hardly move, implying a greater role for party influence relative to preferences on close roll calls. The intertemporal changes in the relative importance of direct influence and preferences in explaining party voting are

[23]Party difference has been used in numerous studies, including Turner and Schneier (1970), Shade et al. (1973), Clubb and Traugott (1977), Clubb, Flanigan, and Zingale (1980), and Rohde (1991). The party-difference index is highly correlated with the 50-50 party-vote index (defined as the percentage of roll on which at least 50 percent of Democrats voted against at least 50 percent of all Republicans). The correlation between the two measures is over .95 in both the House and the Senate.

[24]A bit of algebra shows that $\tilde{\delta}$ represents the absolute value of the expected fraction of Democratic yea votes minus the expected fraction of Republican yea votes, when the legislators vote only according to their personal preferences (z_i's)and random shocks.

[25]The overall values and patterns are similar for the Senate.

[26]It appears from Table 3 that high levels of party influence may have returned in the 103rd–105th Congresses. We hesitate to make much of this, however, since the correlation between members' preferences and the party dummy exhibited very high levels of multicollinearity in these Congresses (see note 14.)

FIGURE 2 Comparing Party Pressure in the House and Senate

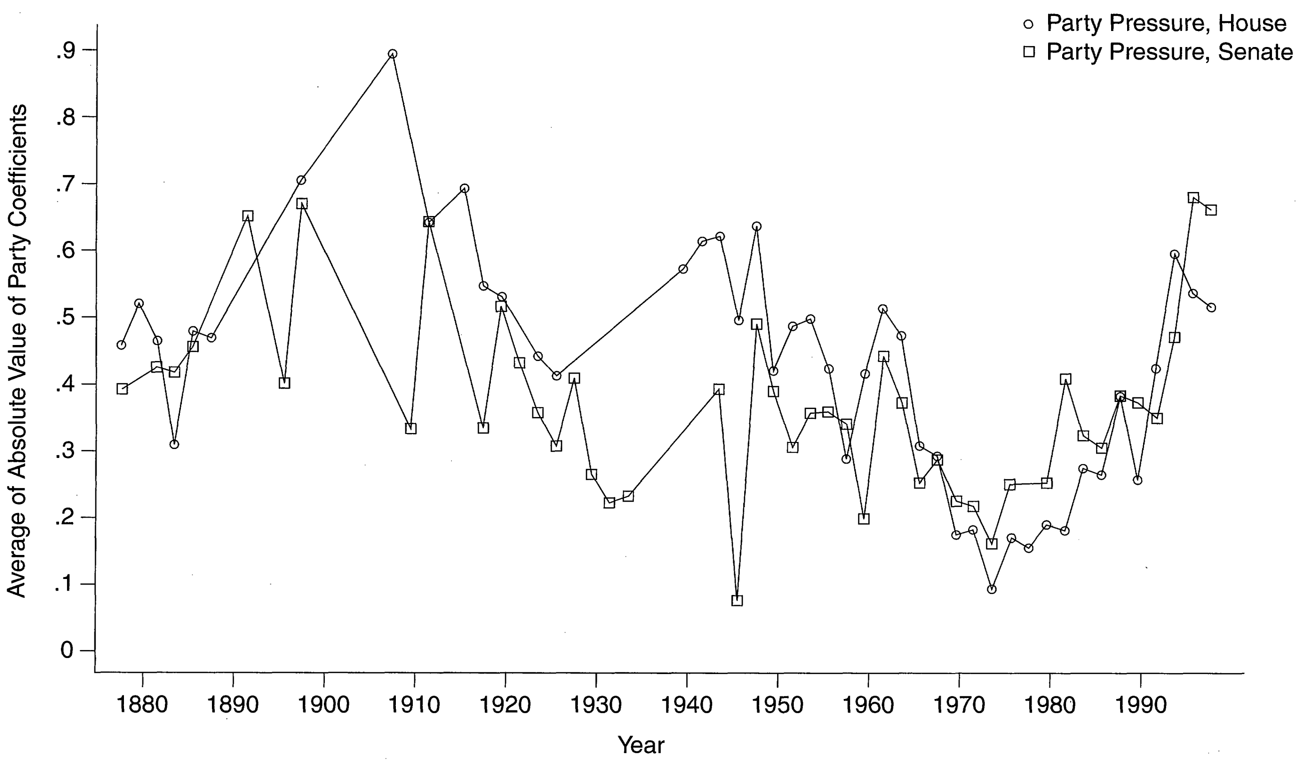


TABLE 3 Decomposition of Party Difference

Congress	Overall Due to Diff.	Due to Prefs.	Due to Party	Congress	Overall Diff.	Due to Prefs.	Due to Party
45 (1877–78)	0.59	0.32	0.27	84 (1955–56)	0.37	0.17	0.19
46 (1879–80)	0.62	0.36	0.26	85 (1957–58)	0.36	0.21	0.15
47 (1881–82)	0.59	0.35	0.24	86 (1959–60)	0.42	0.20	0.22
48 (1883–84)	0.49	0.35	0.13	87 (1961–62)	0.41	0.20	0.21
49 (1885–86)	0.51	0.31	0.21	88 (1963–64)	0.43	0.24	0.19
50 (1887–88)	0.45	0.29	0.16	89 (1965–66)	0.40	0.26	0.15
55 (1897–98)	0.72	0.32	0.40	90 (1967–68)	0.30	0.20	0.10
60 (1907–08)	0.62	0.20	0.42	91 (1969–70)	0.23	0.18	0.05
62 (1911–12)	0.54	0.25	0.30	92 (1971–72)	0.26	0.18	0.08
64 (1915–16)	0.51	0.18	0.33	93 (1973–74)	0.26	0.23	0.04
65 (1917–18)	0.41	0.22	0.19	94 (1975–76)	0.32	0.25	0.07
66 (1919–20)	0.41	0.19	0.22	95 (1977–78)	0.28	0.22	0.06
68 (1923–24)	0.49	0.25	0.24	96 (1979–80)	0.34	0.26	0.08
69 (1925–26)	0.38	0.28	0.10	97 (1981–82)	0.31	0.24	0.07
76 (1939–40)	0.57	0.29	0.29	98 (1983–84)	0.41	0.32	0.09
77 (1941–42)	0.44	0.16	0.28	99 (1985–86)	0.49	0.40	0.09
78 (1943–44)	0.45	0.19	0.26	100 (1987–88)	0.51	0.38	0.13
79 (1945–46)	0.42	0.23	0.19	101 (1989–90)	0.45	0.40	0.06
80 (1947–48)	0.46	0.23	0.22	102 (1991–92)	0.52	0.39	0.13
81 (1949–50)	0.42	0.26	0.16	103 (1993–94)	0.60	0.31	0.29
82 (1951–52)	0.46	0.19	0.27	104 (1995–96)	0.61	0.36	0.25
83 (1953–54)	0.41	0.22	0.19	105 (1997–98)	0.53	0.32	0.21

unchanged, however. We use all roll calls in constructing Table 3, rather than the subset of close roll calls, to make the table more comparable with those of previous studies that use party difference or party vote measures.

Concluding Remarks

Numerous scholars have commented on the difficulty in isolating the effects of party and preferences in roll-call voting. We have proposed one possible solution, which is based on a few behavioral assumptions that seem reasonable. Our findings indicate that party clearly matters in congressional voting, even after controlling for preferences.

We conclude with suggestions for future research. First, our analysis rests in part on some strong functional form assumptions. Of course, strong functional form assumptions are always necessary to scale roll-call data. Experimenting with different functional forms is a straightforward (though tedious) matter, however, and one that deserves some attention.

Second, we were unable to analyze nearly a dozen congresses in which there are an insufficient number of lopsided roll calls. Also, the ideal point estimates in some of the included congresses are based on an uncomfortably small number of lopsided roll calls. The bulk of these congresses fall in two interesting time periods, the Reed-Canon era at the turn of the century and the New Deal era. Assuming that representatives' and senators' preferences are stable over time, one can increase the number of roll calls used in estimating preference parameters by combining adjacent congresses.

Third, one can use the procedure described here to estimate the extent to which differences in voting patterns between senators from the same state but different parties can be explained by party influence. Some preliminary estimates for the 90th–94th Congresses imply that only about 25 percent of the difference in voting patterns of same-state, different-party senators can be explained by such influence.

Fourth, one can apply the procedure to study the political influence of interest groups as well as political parties, by including measures of an interest group's influence across constituencies in the second-step regressions. The distribution of a group's membership, or socioeconomic proxies that capture that distribution, are obvious candidates.

Finally, we will of course want to study explanations of variations in party influence. A number of previous analyses have studied the determinants of party voting, party difference, and party cohesion over time (e.g., Clubb and Traugott 1977; Sinclair 1977; Brady, Cooper, and Hurley 1979; Patterson and Caldeira 1988; Hurley and Wilson 1989). It is natural to extend this work using our new measures of party influence, rather than the traditional measures, as the dependent variables. For example, Rohde (1991) argues that if a party has relatively homogeneous preferences, then its members will cede a large amount of power to their leaders. Is there a positive correlation between homogeneity of preferences within parties and the importance of party influence? The fact that the estimated effects of party influence in the House and Senate exhibit somewhat similar patterns over time suggests an important role for forces external to congress. Obvious possibilities include electoral forces, the role of the president—for example, the breadth of the president's policy agenda and the extent of his "mandate"—and the influence of public opinion on the congressional agenda.

Appendix A

There are four types of parameters we must set in order to simulate roll-call data: legislator ideal points, roll-call parameters, party-influence parameters, and random shocks.

In all of the simulations reported here we fix the number of legislators at 435, with 235 in the majority party and 200 in the minority party. We vary the number of roll calls, from 200 to 1000. We run simulations with one-dimensional and two-dimensional spaces. Since the results were similar, we only report the results for one-dimensional simulations.

Legislators' ideal points within each party are drawn either from uniform or normal distributions. In the uniform case, the ideal points of "left" party are drawn from a $U\left[\frac{-2-g}{2},\frac{-g}{2}\right]$ distribution, and those of the "right" party are drawn from a $U\left[\frac{g}{2},\frac{2+g}{2}\right]$ distribution. Thus, g parameterizes the gap or overlap between the parties—$g = 0$ means the gap is zero, $g = 1$ means the gap between the parties is equal to the within-party range, and $g = -1$ means the two parties' members are drawn from the same distribution.[27]

For each roll call we must choose two roll-call alternatives, x_1 and x_2. We let the median voter's ideal point be x_1, and we draw x_2 randomly from a $U[-q, q]$ distribution. We

[27]In the normal case the distributions generating the members' ideal points are $N\left(-\frac{1+g}{2},1\right)$ and $N\left(\frac{1+g}{2},1\right)$, respectively. We only report results for the uniform case, since the results for the normal case are similar.

choose q so that approximately half of the simulated roll calls are lopsided and half are close, which is the case for actual roll-call data in the U.S. House.[28]

This process is consistent with at least two interpretations of legislative behavior. The first is where the status quo is x_2, and, due to various shocks, varies randomly about the median legislator's ideal point. When x_2 gets too far out of line, the median proposes a bill, x_1, to bring policy back to his ideal point. The second interpretation is consistent with legislators acting as position takers (Mayhew 1974) or playing blame-game politics (Groseclose 1996; Groseclose and McCarty 1999). The status quo is x_1, the median's ideal point, and a legislator is drawn at random to propose a bill, x_2. Although x_2 is expected to lose against x_1, a legislator may propose it in order to demonstrate his or her preferences to constituents, or trap rivals into voting for an alternative that voters oppose.

For empirical verisimilitude, we disallow alternatives that are too close to each other (e.g., roll calls where a legislator proposes to increase the minimum wage by one penny). To operationalize this, we select x_2 from the set $[-q,-1/3] \cup [1/5, q]$. Several values give the same qualitative results as $-1/3$ and $1/5$ (including setting each to zero). We chose these values because they are consistent with Krehbiel's (1998) model of "pivotal politics."[29]

For each roll call the random shocks are drawn from a $U[-m_j, m_j]$ distribution. The parameter m_j is chosen so that the maximum probability of voting for one or the other alternative is equal to 1, guaranteeing that all vote probabilities lie between 0 and 1.

Finally, we model party influence as follows. If the alternatives on a roll call are such that the vote in the absence of any party influence is expected to be close, then party influence occurs with probability = .5. On roll calls where party influence is present, the average degree of influence, $\bar{c}$, is set at approximately .20 (the exact value of c_j on each roll call depends on m_j). This value implies that, when party influence is present, party and preferences have approximately equal average effects on legislators' vote probabilities.

Tables A.1 and A.2 present summary statistics from the simulations. Table A.1 presents information about the estimated ideal points from the first-stage factor analysis, and Table A.2 presents information about the estimated party coefficients from the second-stage regressions. In all cases we define lopsided roll calls as those where the percent voting on the winning side is greater than 65 percent. The results can be summarized as follows.

Rows 1–3 show what happens when there is no party influence on any roll calls:

(1) The correlation between the true and estimated ideal points is quite high, even if only lopsided roll calls are used, as long as the number of lopsided roll calls is large enough—250 roll calls produces excellent estimates, and 100 lopsided roll calls may even be adequate.

(2) Type-II errors are rare in the second-stage regressions, whether ideal points are estimated using all roll calls or only lopsided roll calls. That is, we rarely estimate that there is party influence even when there is not. When ideal points are estimated using approximately 100 lopsided roll calls, this happens only 6 percent of the time, and if we have 250 or 500 lopsided roll calls then it only happens 3–4 percent of the time.

Rows 4–6 show what happens when there is party influence on some close votes (approximately 50 percent), but not on lopsided votes:

(1) The correlation between the true ideal points and the estimated ideal points based on lopsided roll calls is about the same as when there is no party influence. The correlation between the true ideal points and the estimated ideal points based on all lopsided roll calls is only slightly lower than the case when there is no influence. What the correlations do not reveal, however, is that the ideal points estimated using all roll calls exhibit a noticeable "S"-shape relative to the true ideal points—that is, the estimated ideal points of moderate legislators are more extreme than their true values.

(2) When all roll calls are used to estimate ideal points, the incidence of party influence is greatly understated. Type-I errors occur 74–92 percent of the time.

(3) When only lopsided roll calls are used to estimate ideal points, the estimated incidence of party influence is much closer to the true level, but we still err on the side of understating the incidence of party influence. Type-I errors occur about 20–36 percent of the time. Having more lopsided roll calls does not always result in fewer type-I errors.

(4) Type-II errors are again rare, and using lopsided roll calls to estimate ideal points produces fewer type-II errors than using all roll calls. Using lopsided roll calls, type-II errors occur 3–5 percent of the time.

(5) When ideal points are estimated using lopsided roll calls, if the estimated party coefficient is significant it almost never has the wrong sign. When ideal points are estimated using all roll calls, "wrong signs" occur more often.

[28]The two alternatives define the parameters a_j and $\mathbf{b}_j$ found in Equations 1–4 in the text. In the one-dimensional model, if we denote the alternatives x_{j1} and x_{j2}, then $a_j = x_{j1}^2 - x_{j2}^2$ and $b_j = 2x_{j1} - 2x_{j2}$.

[29]Given a president to the left of the median legislator, the point $-1/3$ corresponds to the "veto pivot" and $1/5$ corresponds to the "filibuster pivot." In the pivotal politics model no roll calls occur when the status quo is between these two points. We also ran simulations where, as in Krehbiel's model, the median voter proposes bills strategically with the $-1/3$ and $1/5$ pivot players in mind rather than always proposing $x_1 = 0$. The results are virtually identical to those reported here.

TABLE A.1 **Results About Estimated Preference Parameters**

Monte Carlo Simulations 500 Trials for Each Setting of Parameters					
		Preferences Estimated Using All Roll Calls		Preferences Estimated Using Lopsided Roll Calls	
Parameter Values	(#Lop)	Prediction	Collinearity	Prediction	Collinearity
I = O, G = 0.0, N = 1000	(496)	.99	.94	.99	.94
I = O, G = 0.0, N = 500	(247)	.99	.94	.98	.94
I = O, G = 0.0, N = 200	(98)	.98	.94	.94	.93
I = 1, G = 0.0, N = 1000	(508)	.99	.98	.99	.94
I = 1, G = 0.0, N = 500	(254)	.98	.97	.98	.94
I = 1, G = 0.0, N = 200	(102)	.97	.96	.94	.93
I = 2, G = 0.0, N = 1000	(487)	.98	.98	.99	.95
I = 2, G = 0.0, N = 500	(243)	.98	.97	.98	.95
I = 2, G = 0.0, N = 200	(97)	.97	.96	.94	.94
I = 1, G = –0.4, N = 1000	(502)	.98	.98	.98	.88
I = 1, G = –0.4, N = 500	(250)	.97	.96	.97	.88
I = 1, G = –0.4, N = 200	(100)	.96	.93	.93	.88
I = 1, G = 1.0, N = 1000	(497)	.99	.99	.99	.98
I = 1, G = 1.0, N = 500	(250)	.99	.99	.98	.98
I = 1, G = 1.0, N = 200	(100)	.99	.98	.96	.97

Parameters:
I = 0 if no party influence on any roll calls.
I = 1 if party influence exists on approximately 50 percent of the ex-ante close roll calls and 0 percent of the ex-ante lopsided roll calls.
I = 2 if party influence exists on approximately 65 percent of the ex-ante close roll calls and approximately 35 percent of the ex-ante lopsided roll calls.
G = gap between parties.
N = total number of roll calls.
Prediction = R^2 of regression predicting true preference parameters using estimated preferences parameters.
Collinearity = correlation coefficient between party and the estimated preference parameters.

Rows 7–9 show what happens when there is party influence on both close votes and lopsided votes (approximately 65 percent of the close roll calls and 35 percent of the lopsided votes):

(1) The correlation between the true and estimated ideal points is not much different than in the case with no party influence, whether we use all roll calls on lopsided roll calls to estimate ideal points. In both cases, however, there is a noticeable "S"-shape relative to the true ideal points—the estimated ideal points of moderate legislators are too extreme.

(2) When all roll calls are used to estimate ideal points, the incidence of party influence is greatly understated. Type-I errors occur 86–94 percent of the time.

(3) Even when only lopsided roll calls are used to estimate ideal points, the incidence of party influence is greatly understated. Type-I errors occur about 49–64 percent of the time. This means that if party influence is exerted frequently on lopsided votes, then our empirical estimates will tend to understate the amount of party influence. Again, having more lopsided roll calls does not always result in fewer type-I errors.

(4) Type-II errors are again rare, at least when ideal points are estimated using lopsided roll calls. Using lopsided roll calls, type-II errors occur 4–5 percent of the time.

(5) When ideal points are estimated using lopsided roll calls, we almost never estimate a significant effect of party in which the coefficient has the wrong sign. However, this happens relatively frequently when ideal points are estimated using all roll calls.

Rows 10–12 show what happens when there is a significant overlap between the parties' ideal points. We only present results for the case where party influence exists on close roll calls but not on lopsided roll calls. The results are similar in spirit to those in rows 4–6. The main difference is that collinearity between the party dummy and legislator ideal points is less of a problem because of the overlap, so the party influence coefficients are more accurately estimated when lopsided roll calls are used to estimate ideal points.

Rows 13–15 show what happens when there is a large gap between the parties and therefore substantial collinearity between the party dummy and ideal points. (We drop cases such as these in our empirical analysis.) Again, we only

TABLE A.2 Results About Estimates of Party Influence

Monte Carlo Simulations 500 Trials for Each Setting of Parameters								
	Preferences Estimated Using All Roll Calls				Preferences Estimated Using Lopsided Roll Calls			
Parameters	Corr. w/True	YY	NN	Wrong Sign	Corr. w/True	YY	NN	Wrong Sign
I = 0, G = 0.0, N = 1000	—	—	.97	—	—	—	.97	—
I = 0, G = 0.0, N = 500	—	—	.97	—	—	—	.96	—
I = 0, G = 0.0, N = 200	—	—	.97	—	—	—	.94	—
I = 1, G = 0.0, N = 1000	.29	.08	.96	.07	.81	.64	.97	.00
I = 1, G = 0.0, N = 500	.41	.16	.94	.03	.83	.68	.97	.00
1 = 1, G = 0.0, N = 200	.52	.26	.93	.02	.85	.80	.95	.00
I = 2, G = 0.0, N = 1000	.25	.06	.95	.20	.79	.36	.96	.00
I = 2, G = 0.0, N = 500	.36	.10	.92	.13	.81	.40	.96	.00
I = 2, G = 0.0, N = 200	.45	.14	.90	.06	.85	.51	.95	.00
1 = 1, G = –0.4, N = 1000	.20	.06	.96	.16	.87	.74	.97	.00
I = 1, G = –0.4, N = 500	.28	.09	.95	.10	.87	.76	.97	.00
I = 1, G = –0.4, N = 200	.45	.22	.92	.03	.88	.82	.96	.00
I = 1, G = 1.0, N = 1000	.41	.13	.96	.02	.66	.37	.97	.00
I = 1, G = 1.0, N = 500	.41	.13	.96	.02	.70	.48	.96	.00
I = 1, G = 1.0, N = 200	.41	.13	.96	.02	.74	.72	.90	.00

For definition of parameter configurations, see note to Table A.1.
Corr. w/True = correlation between c_j and $\hat{c}_j$.
YY = number of roll calls where cj is significantly different from zero and $c_j \neq 0$, divided by the number of roll calls where $c_j \neq 0$. So, 1–YY = fraction of roll calls with a "Type-I error."
NN = number of roll calls where $\hat{c}_j$ is not significantly different from zero and $c_j = 0$, divided by the number of rollcalls where $c_j = 0$. So, 1–NN = fraction of roll calls with a "Type-II error."
Wrong Sign = fraction of roll calls where $\hat{c}_j$ is significantly different from zero and $c_j \neq 0$, but $\hat{c}_j$ and c_j have the opposite sign.

present results where party influence exists on close roll calls but not on lopsided roll calls:

(1) The correlation between true and the estimated ideal points is even higher than in the cases, since the ideal points are more spread out. The degree of collinearity is also quite high, however, ranging between .97 and .99.

(2) When all roll calls are used to estimate ideal points, the incidence of party influence is greatly understated. Type-I errors occur 87 percent of the time.

(3) Even when only lopsided roll calls are used to estimate ideal points, the incidence of party influence is sharply understated. Type-I errors occur about 28–63 percent of the time. Again, having more lopsided roll calls does not always result in fewer type-I errors.

(4) In most cases, type-II errors are again rare. When lopsided roll calls are used to estimate ideal points, however, and the number of lopsided roll calls is small, then type-II errors occur about 10 percent of the time.

(5) We almost never estimate a significant effect of party in which the coefficient has the wrong sign, whether we use lopsided roll calls or all roll calls to estimate ideal points.

Appendix B

The following is an outline of the procedure for estimating the number of "significant" preference parameters. Intuitively, a factor is "significant" if it predicts voting across disjoint subsets of roll calls. Let $\mathbf{J} = \{1, \ldots n\}$ be the set of roll calls. If n is even, let $m = n/2$; otherwise, let $m = (n-1)/2$. Then execute the following steps:

1. Let $\mathbf{J}_t$ be a random subset of size m from $\mathbf{J}$, and let $\mathbf{J}_t^C$ be the complement of $\mathbf{J}_t$. Then $\mathbf{J}_t$ and $\mathbf{J}_t^C$ are disjoint, and the union of $\mathbf{J}_t$ and $\mathbf{J}_t^C$ is $\mathbf{J}$.
2. Let $\mathbf{z}_{t1}, \ldots, \mathbf{z}_{tJ}$ be the principal components derived from the covariance matrix of the vote data on the roll calls in $\mathbf{J}_t$, and let $\mathbf{z}_{t1}^C, \ldots, \mathbf{z}_{tJ}^C$ be the corresponding principle components based on the roll calls in $\mathbf{J}_t^C$.
3. Let ρ_{tj} be the absolute value of the correlation between $\mathbf{z}_{tj}$ and $\mathbf{z}_{tj}^C$, $j = 1, \ldots, J$.
4. Repeat 1–3 for $t = 1, \ldots, T$, where T is large, and let $\overline{\rho}_j = (1/T)\sum_{t=1}^{T}\rho_{tj}$, $j = 1, \ldots, J$. Retain factor j if $\overline{\rho}_j$ is above some threshold, ρ_0.

If the number of roll calls is odd, simply delete one roll call at random before drawing the subsets J_t and J_t^C, then proceed with the steps above.

In applying the procedure, we used $T = 100$ and $\rho_0 = .20$. In congresses with relatively few lopsided roll calls we retained one or two extra factors as an added precaution.

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