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On Measuring Partisanship in Roll Call Voting:

The U.S. House of Representatives, 1877-1999*

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Abstract:

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We propose a method of assessing party influence, based on a spatial model. Our method provides the first test of whether observed values of the widely-used Rice index of party dissimilarity are consistent with a "partyless" null model. It also avoids problems that beset previous estimators.

Substantively, we find evidence of party influence in all but one Congress since 1877. Moreover, our indicator of party pressure is systematically higher for the sorts of roll calls that party theorists believe are more pressured—procedural, organizational and label-defining votes. Our results refute the widespread notion that parties in the House have typically had negligible influence on roll call voting behavior. They also document important changes in party influence associated with the packing of the Rules Committee in 1961 and the procedural reforms of 1973.

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In this paper, we devise a general estimator of (variation in) party influence in roll call voting. Our approach entails comparing the actually observed Rice index for each roll call to the expected index under a null model in which party pressures are constant. Rejecting the null thus entails rejecting the hypothesis that party pressures are nil. We propose our method because we believe the path-breaking Snyder-Groseclose (2000) estimator is biased toward finding party effects, while the McCarty, Poole and Rosenthal (2001) paper misinterprets the statistical tests they provide.[1]

When applied, our method uncovers evidence of party influence in all but one U.S. House since 1877. Our results thus refute the notion, articulated by Mayhew (1974) among many others, that parties in the House have generally had negligible influence on roll call voting behavior. We also find important changes in party influence associated with the packing of the Rules Committee in 1961 and the subcommittee bill of rights in 1973.

The rest of the paper proceeds as follows. We first describe a standard unidimensional spatial model of legislative voting. Poole (2001) provides a method of estimating the parameters of our model—in particular, each member's ideal point and standard error and each roll call's cutpoint and gap parameters. Given these parameter estimates, we show how to derive a theoretical distribution for the Rice index of party difference, under the null hypothesis of constant party influence (which includes nil influence as a special case). We then contrast our method to previous techniques (Snyder and Groseclose 2000; McCarty, Poole and Rosenthal 2001). Empirically, we implement our tests for all roll calls (with cutpoints between the party medians) in all congresses from the

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[1]. These new approaches were motivated by the fact that traditional roll call-based measures of party voting suffer a significant and well-known problem: they increase in size not only when parties devote

45th (1877-79) to the 105th (1997-1999). We also present tests based on a two-dimensional model, to demonstrate that unidimensionality does not drive our results. As will be seen, evidence for party effects is clear and consistent, especially on the sorts of roll call that party theorists have argued should exhibit higher party pressures.

A spatial model of legislative voting

The model we use is similar to that of Ladha (1991). There are 435 legislators, n_D Democrats and n_R Republicans. Legislator i has an ideal point x_i on a single left-right dimension. We denote by $x = (x_1,\ldots,x_{435})$ the vector of all members' ideal points.

The various policy proposals from which legislators must choose are represented by points on the real line. The utility to legislator i of policy alternative a is $u_i(a;x_i) = -(a-x_i)^2 + e_{ai}$, where $e_{ai} \sim N(0,2\sqrt{2}s_i)$ for all a. The error e_{ai} is independent of e_{bi} for all alternatives $a \neq b$. We refer to the parameter s_i as member i's standard error. If $s_i = 0$, then member i votes perfectly in accord with his or her ideal point; otherwise, there is some residual error in predicting behavior. We denote by $s = (s_1,\ldots,s_{435})$ the vector of all members' standard errors.

An important assumption is that members' errors are statistically independent. Formally, if $i \neq k$, then e_{ia} and e_{ka} are independent for all a. Thus, knowing whether legislator i evaluated proposal a more favorably than would have been expected, based on the distance between the policy and his ideal point tells one nothing about whether legislator k will evaluate that proposal more or less favorably. This is a standard, if sometimes implicit, assumption of most of the scaling literature. It is violated, for example, when members of the same party have positively correlated errors or members of opposed parties have negatively correlated errors.

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more resources to influencing their members' votes but also when preferences within parties simply become more similar (see, e.g., Kingdon 1973; Cox 1987, p. 29; Krehbiel 2000).

There are n votes held. Vote j pits an alternative a_j against an alternative b_j. The vote is more conveniently characterized by a cutpoint $c_j = (a_j+b_j)/2$ and a distance (or gap) $d_j > 0$, where $a_j = c_j - d_j < b_j = c_j + d_j$.

Legislator i's voting behavior on roll call j can be characterized by his or her probability of voting for alternative a_j: $p_{ij} = \Pr[\text{i votes for } a_j]$. That is, p_{ij} is the probability of voting for the left-of-cutpoint alternative. As it turns out (see e.g. Londregan 1999), $p_{ij} = \Phi(d_j(c_j-x_i)/s_i)$, where Φ is the standard normal cumulative distribution function. We shall use the random variable $V_i(c_j,d_j)$ to denote member i's vote on roll call j (characterized by cutpoint c_j and gap d_j). Note that $V_i(c_j,d_j)$ is distributed as a binomial variate with probability p_{ij}.

How the parties vote

In order to examine how parties vote within the model presented above, begin by considering a single roll call vote, j. The proportion of Democrats voting left on this roll call can be written $D_L(c_j,d_j;s,x) = (1/n_D)\Sigma V_i(c_j,d_j)$. That is, $D_L(c_j,d_j;s,x)$ is a random variable equal to the sum of the n_D independent binomial variates representing the Democrats' votes on roll call j (divided by n_D). Similarly, $R_L(c_j,d_j;s,x) = (1/n_R)\Sigma V_i(c_j,d_j)$ is the sum of n_R independent binomials (divided by n_R).

Given that n_D and n_R, the number of Democrats and Republicans respectively, are both over 175 in typical congresses, $D_L(c_j,d_j;s,x)$ and $R_L(c_j,d_j;s,x)$ are both approximately normally distributed. Given the independence

assumption, we can write their means as follows:

$$E[D_L(c_j,d_j;s,x)] = (1/n_D)\Sigma\Phi(d_j(c_j - x_i)/s_i) \qquad (1a)$$

$$E[R_L(c_j,d_j;s,x)] = (1/n_R)\Sigma\Phi(d_j(c_j - x_i)/s_i) \qquad (1b)$$

where the sum in (1a) is over all i such that i is a Democrat and the sum in (1b) is over all i such that i is a Republican. The variances can be written (using standard formulas) as follows:

$$\mathrm{Var}[D_L(c_j,d_j;s,x)] = (1/n_D^2)\sum\Phi(d_j(c_j - x_i)/s_i)(1-\Phi(d_j(c_j - x_i)/s_i)) \quad (2a)$$

$$\mathrm{Var}[R_L(c_j,d_j;s,x)] = (1/n_R^2)\sum\Phi(d_j(c_j - x_i)/s_i)(1-\Phi(d_j(c_j - x_i)/s_i)) \quad (2b)$$

where again the sums are over all Democrats and all Republicans, respectively.

Now consider the joint distribution of $D_L(c_j,d_j;s,x)$ and $R_L(c_j,d_j;s,x)$ for the jth roll call—call it G_j.[2] Given the independence assumption, G_j is bivariate normal with zero covariance, variances as given in equations (2) and means as given in equations (1).

The Rice index of party dissimilarity

In this section, we consider a widely-used statistic with a long pedigree—Rice's index of party dissimilarity (Rice 1928). For a given roll call, Rice's index is the absolute difference between the proportion of Democrats voting yes and the proportion of Republicans voting yes. In terms of the model above, Rice's index is just $\mathrm{Rice}(c_j,d_j;s,x) = |D_L(c_j,d_j;s,x) - R_L(c_j,d_j;s,x)|$. Ignoring the probability that more Republicans vote "left" than Democrats, the absolute value signs can be dropped. Thus, the Rice index for the jth roll call is normally distributed, as it is a linear combination of normal variates. Its mean and variance are as follows:

$$\mu_j = E[\mathrm{Rice}(c_j,d_j;s,x)] = E[D_L(c_j,d_j;s,x)] - E[R_L(c_j,d_j;s,x)] \text{ and}$$
$$\sigma_j^2 = \mathrm{Var}[\mathrm{Rice}(c_j,d_j;s,x)] = \mathrm{Var}[D_L(c_j,d_j;s,x)] + \mathrm{Var}[R_L(c_j,d_j;s,x)].$$

A statistical test for party-separating forces

Suppose that we have estimated the parameters of the model—s, x, $d=(d_1,\ldots,d_n)$, and $c=(c_1,\ldots,c_n)$—using roll calls from a given Congress. To begin with, ignore the error in these estimates—assume they are all the true parameters. In this case, for each roll call j, we can look at the empirically observed value of the Rice index and test the null hypothesis that it could have been generated by the model (with the stipulated parameters)—that is, by a normal distribution with mean μ_j and standard deviation σ_j. We reject the null hypothesis for any roll call on which the empirically computed Rice index is surprisingly large, exceeding $\mu_j + 1.96\sigma_j$, or surprisingly small, falling short of $\mu_j - 1.96\sigma_j$.

One interpretation of our test is in terms of party-separating pressures. Let π_{DDj} denote the total amount of pressure applied by Democrats on Democrats to vote left; π_{DRj} denote the total amount of pressure applied by Democrats on Republicans to vote left; with π_{RRj} and π_{RDj} denoting Republican pressures to vote right. Focusing on roll calls with cutpoints between the party medians, as we do in our empirical analysis below, the pressures π_{DDj} and π_{RRj} act to *separate* the parties: the larger are such pressures, the more differently the two parties will vote. In contrast, the pressures π_{DRj} and π_{RDj} *unite* the parties: the larger are such pressures, the more similarly the two parties will vote. Thus, $Q_j = (\pi_{DDj} + \pi_{RRj}) - (\pi_{DRj} + \pi_{RDj})$ is the net party-separating force exerted by the parties on roll call j. Positive values of Q_j indicate a preponderance of party-separating forces, while negative values indicate a preponderance of party-uniting forces.

To explain the relationship between our test and party-separating pressure, denote the average pressure across all roll calls by Q and suppose that $Q_j = Q$ for all j: the parties exert a constant pressure on all roll calls. Without loss of generality, consider the case in which Q > 0. In this case, Democrats' *estimated* ideal points will be farther left than their partyless ideal points would be, while Republicans' estimated ideal points will be farther right. The location of legislators' ideal points, in other words, will already reflect their parties' "average" pressures.[3]

If one seeks to detect party pressure while controlling for the estimated ideal points, one will only detect variation (from roll call to roll call) around the mean level of party pressure. When party pressure is well above-average, the

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[2]. G_j depends on the model parameters but we will save on notational clutter by not noting this explicitly.
[3]. We put "average" in quote marks because we are not claiming that the estimated ideal points can be shown to be a closed-form function of the simple average of party pressures. Rather, we are claiming that the location of estimated ideal points is a monotonic function of the amount of party pressure applied on the

parties will vote more *differently* than would be expected on the basis of the estimated ideal points, yielding a higher-than-expected Rice index. When party pressure is well below-average, in contrast, the parties will vote more *similarly* than would be expected on the basis of the estimated ideal points, yielding a lower-than-expected Rice index. However, when party pressure is near average, then the Rice index will be close to what one expects on the basis of the array of ideal points.

All told, then, our test detects party pressures that differ significantly from the average pressure, Q, reflected in the estimated ideal points. For a single roll call, *the null hypothesis is that party pressures on that roll call are average:* $Q_j = Q$. For a set of roll calls, *the null hypothesis is that party pressures are constant*: $Q_j = Q$ for all j.

Note two things about these null hypotheses. First, rejecting the null entails rejecting the narrower hypothesis that the parties exert nil separating pressure ($Q_j = 0$). Second, rejecting the null does not entail rejecting the hypothesis, propounded by Krehbiel, that party pressures on each roll call balance, in the sense that the outcome of the roll call is the same as it would have been absent any pressure.[4]

Testing hypotheses drawn from procedural cartel theory

Our model can also sustain tests of procedural cartel theory (Cox and McCubbins 1993). To see how, note that there are two factors determining whether a given roll call will exceed, equal or fall short of the expected Rice index: the inherent variability of members' voting behavior; and the (unobserved) level of party pressure on the given roll call, relative to the mean party pressure across all roll calls. If party pressure is constant, the second factor is nil. Denoting the observed Rice index on roll call j by R_j, the deviation from

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roll calls scaled. As the average party pressure increases (or perhaps we would need a first-order stochastic shift up in party pressures), each Democrat's ideal point shifts left, each Republican's shifts right.

[4] For the record, we believe there is substantial evidence that the majority party has more resources with which to affect votes (see Aldrich and Rohde 2000). Moreover, political action committees say that majority status matters (Grenzke 1988) and they put large amounts of their money where their mouths are (Cox and Magar 1999; Ansolabehere and Snyder 2000). Also, the political parties themselves fight hard

expectation, $R_j - \mu_j$, should reflect only random variation around a mean of zero (with a "small" variance). If the alternative model holds, the second factor (party pressure) is a systematic omitted variable. The larger is the unobserved party pressure, Q_j, the larger will be $R_j - \mu_j$.

Under procedural cartel theory (Cox and McCubbins 1993), party pressure should be higher on procedural, organizational and label-defining votes, than on ordinary substantive votes. Higher pressure, in turn, should produce systematically higher values of $R_j - \mu_j$ on such votes. Thus, to test our predictions, we can regress $R_j - \mu_j$ on dummy variables indicating procedural, organizational and label-defining votes, as explained further below.

Other measures of party voting: Snyder and Groseclose

Snyder and Groseclose (2000) use only lopsided votes (where the winning side has more than 65% of the total) to scale legislators, then use the resulting "lopsided" ideal points and a party dummy to predict votes. The notion is that parties will not bother to exert pressure on lopsided votes, so that the resulting scale positions reflect only constituency and personal preferences. Using their technique, Snyder and Groseclose find many roll calls with significant party influence.

The main criticism of the Snyder-Groseclose technique focuses on the first stage of their procedure: the estimation of ideal points from lopsided votes. McCarty, Poole and Rosenthal (2001) note that the restriction to lopsided votes removes information needed to estimate the scale positions of moderate legislators, show that this can lead to a biased estimation of party effects, and consequently argue for the use of *all* roll calls in the first-stage estimation of ideal points.

Our criticism focuses instead on the second stage of the analysis: the assessment of party effects, given estimates of members' ideal points. Snyder and Groseclose use a linear probability model, estimated via ordinary least

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for majority status. All this suggests strongly that the majority party is better able to influence outcomes than the minority—which entails rejecting Krehbiel's balancing hypothesis.

squares regression, in the second stage. We prefer what is essentially a probit, albeit one implemented via a comparison of observed and expected Rice indices.

An example

To illustrate our criticism, suppose that the world is truly partyless and that all legislators vote according to the unidimensional quadratic utility model sketched in the first section of this paper. Suppose also that the analyst *knows* all legislators' true ideal points (x_i, i=1,…,435) and standard errors (s_i, i=1,…,435). In our example, we assume members' ideal points range from –1 to +1 and are distributed symmetrically about the median (zero). We also assume all members share a common standard error ($s_i = t$ for all i).

Now consider a roll call with cutpoint at the median ($c_j=0$) and with gap equal to the common standard error ($d_j = t$). In this case, the probability of voting "right" reduces to $\Phi(x_i)$. If one plots the probability of voting "right" against x_i, one recovers the curve in Figure 1. Generating simulated voting data from the model amounts to running an independent binomial experiment for each legislator, with probability $\Phi(x_i)$ of landing "right".

What happens if one regresses each member's simulated vote on his or her known ideal point, x_i? The *expected* regression line, given the symmetric distribution of the ideal points, is illustrated in Figure 1.

Note that the regression errors for this line correlate with party. The regression line in Figure 1 overstates the true probability of voting "right" (given by the curve in Figure 1) for all members with left-of-median ideal points, most of whom are Democrats. But the regression line understates the probability of voting "right" for all those with ideal points to the right of the median, most of whom are Republicans.

[Figure 1 about here.]

Thus, when one adds a party dummy variable to the regression, it may well have a significant estimated coefficient, because it correlates with the error term. In other words, the Snyder-Groseclose procedure can produce false positives in its test for party pressure, simply because it estimates nonlinear probability functions by a linear function.

A simulation

To investigate whether our example generalizes, we have run a series of two-dimensional simulations in which we generate partyless data, then analyze it using the Snyder-Groseclose second-stage estimator. We are careful in our simulations to match the empirically observed classification error rates.[5] Nonetheless, we find that 209 of the 500 simulated roll calls, or 42%, exhibited false positives (significant party coefficients). Looking just at close votes, we find that 89 of 204, or 44%, register false positives.[6] Our simulations show that the Snyder-Groseclose second-stage estimator depends crucially on the error structure in voting actually being uniform, as they assume, rather than normal, as we assume.[7]

Real-world data

If the Snyder-Groseclose estimator really is biased toward finding party effects, then our method should find less party pressure than Snyder and Groseclose's when deployed on real data. To investigate this, we have reexamined the 95th Congress. We first estimate members' ideal points using only lopsided roll calls, as do Snyder and Groseclose.[8] We follow Snyder and Groseclose in their first stage not because we are entirely sold on the use of lopsided roll calls to estimate partyless ideal points, but rather in order to focus attention on our second-stage differences. Lopsided ideal points in hand, we then test for the existence of party effects, roll call by roll call, as do Snyder and

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[5]. Our simulation of the Snyder-Groseclose procedure, described in detail on the web site k7moa.uh.edu, is similar to that posed in an unpublished (but refereed) appendix to McCarty, Poole, and Rosenthal (2001), which is posted on their web sites. Our analysis differs in that we assume that we know the true legislator ideal points (thereby deflecting attention from the first stage of the Snyder-Groseclose procedure and focusing on the second), we allow for more than one dimension (matching an important strength of the Snyder-Groseclose method), and we use normally distributed error.

[6] Close roll calls are those in which the winning side has between 50% and 65% of the total vote. All other roll calls are lopsided.

[7]. This conclusion does depend on the overall classification error rate assumed in the simulation. The data-generating model is based on a normal distribution with standard deviation equal to s_i/d_j. Thus, as s_i/d_j grows, the normal better and better approximates the uniform distribution assumed by Snyder and Groseclose, and the second-stage estimator produces fewer and fewer false positives.

Groseclose (although we differ in using our test based on the difference between the actual and expected Rice index). For this Congress, it appears from Snyder and Groseclose's Figure 1 that 56% of all close roll calls had significant party effects. In contrast, we find that 28% of close roll calls exhibit party effects—or half the Snyder-Groseclose percentage.

Other measures of party voting: The two-cutpoints model

McCarty, Poole and Rosenthal (2001) compare two models, one in which each party has a separate cutpoint, one in which the two parties share a common cutpoint. If the restricted (one-cutpoint) model fits the data on a particular roll call more poorly than the unrestricted (two-cutpoint) model, then the null of no party influence is rejected.

The first thing to note about the McCarty-Poole-Rosenthal approach is that it is nearly identical *statistically* to ours. If a particular roll call has a surprisingly large Rice index, then left-of-cutpoint Republicans are voting right, right-of-cutpoint Democrats are voting left, or both. In this case, however, a model that allows separate cutpoints for each party will usually fit the data significantly better—and the Democratic cutpoint will be to the right of the Republican cutpoint. If a roll call has a surprisingly small Rice index, then right-of-cutpoint Republicans are voting left, left-of-cutpoint Democrats are voting right, or both. In this case, the Democratic cutpoint will tend to be significantly to the left of the Republican cutpoint.

Although our approaches are very similar statistically, we differ sharply on two points of theoretical interpretation that McCarty, Poole and Rosenthal denote as their hypotheses H1 and H5. Our critique here is essentially the same as that developed independently by Snyder and Groseclose (2001).

First, McCarty, Poole and Rosenthal assert that, if one assumes that parties exert significant pressure on a particular roll call, then one must also expect that classification error (incorrect predictions of individual roll call votes) on that roll call will diminish substantially with a two-cutpoint as opposed to a one-cutpoint model. We deny that partisan theories (e.g., Cox and McCubbins

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[8]. We estimate two-dimensional ideal points in the first-stage analysis, via Poole (2000).

1993) entail this. Suppose that both parties exert an identical (separating) pressure on all roll calls. Then members' ideal points, estimated from their roll call votes, would already reflect this constant pressure, and a one-cutpoint model would fit the data (nearly) as well as a two-cutpoint model. Only if party pressure varies across roll calls can one expect a two-cutpoint model to outperform a one-cutpoint model.

Second, McCarty, Poole and Rosenthal also assert that partisan theories imply that the Democratic cutpoint will be to the right of the Republican cutpoint (when there is any significant difference). We deny this too. Just as with our test (based on the Rice index), the two-cutpoint test will identify only the extremes of party pressure. To see why, recall that the location of legislators' ideal points will already reflect their parties' (average) pressures. Thus, when party pressure is well-above-average, the two-cutpoint test will find the Democrats' cutpoint significantly to the *right* of the Republicans', and the parties voting more *differently* than would be expected on the basis of the estimated ideal points. When party pressure is well-below-average, in contrast, the two-cutpoint test will find the Democrats' cutpoint significantly to the *left* of the Republicans', and the parties voting more *similarly* than would be expected on the basis of the estimated ideal points. Both sorts of divergence from expectation testify to the importance (and variability) of party pressure, contrary to the interpretation offered by McCarty, Poole and Rosenthal.

Measures of party voting: Two stages

In their first stage, Snyder and Groseclose use only lopsided roll calls to estimate legislators' ideal points. An advantage of this procedure is that, if the parties never pressure lopsided votes, then the second-stage analysis can test the null hypothesis that party pressure was nil on a particular roll call, hence estimate the percentage of pressured roll calls in a given Congress.

Procedures that use all (non-unanimous) roll calls to estimate ideal points cannot estimate the percentage of pressured roll calls but they can nonetheless support valuable tests. In particular, one can test whether the average pressure in a class of roll calls exceeds that in some baseline group.

We believe the most conservative procedure when testing for party effects is to use all (non-unanimous) votes in the first stage. Hence, in this paper we have (for the most part) used all roll calls to estimate ideal points, rather than just the lopsided ones.

In contrast to our relatively permissive views on the first stage estimation problem, we reject Snyder and Groseclose's second-stage estimation procedure. Their linear probability model can in principle fail for the usual sorts of reasons that such models fail (per Figure 1). One cannot argue that the parameter values under which it does fail are safely distant from what is empirically relevant (as Snyder and Groseclose (2001) argue): our simulations match the empirically observed classification error rates and still produce unacceptably many false positives. Finally, changing just the second-stage estimation procedure (to, essentially, a probit) dramatically reduces estimates of party effects in real-world data. Hence, whether one uses a "zero pressure" baseline (e.g., lopsided votes) or not, one should use either two-cutpoint or Rice-based tests in the second stage. We prefer the latter due to its pedigree and greater intuitive content.

Empirical results: party effects in the 45th to the 105th Houses

Table 1 presents our first set of results. For each Congress, we display the total number of roll calls, the number of scalable roll calls, and the number of scalable roll calls with cutpoints between the party medians. Of the last set of roll calls, we calculate the percentage with surprisingly high Rice indexes, surprisingly low Rice indexes, and surprising Rice indexes (the sum of the high and low figures). If the null model works as expected, the percentage of surprisingly high Rice indexes should be roughly 2.5%, as should the percentage of surprisingly low Rice indexes.

[Table 1 about here.]

Looking at the actual results, note that the percentage of surprising Rice indices (those more than 1.96 standard errors from the expected value) is above what would be expected by chance (5%) in all but one Congress (the 59th)—and often well above. Using a more conservative approach, we take the number of roll calls with surprisingly high (or low) Rice indices, subtract twice the bootstrapped standard error for this figure, and recompute the percentage.[9] The resulting figures, which give a probable lower bound on the percentages of Rice indices that were high and low, are displayed in the last two columns of Table 1. By these figures, the data seem to fall into an early period, from the 45th to the 77th Congress, during which 22 of 33 Houses show some evidence of party effects; and a late period, from the 78th to the 105th Congress, during which 27 of 28 Houses show some evidence.

The first period can be further parsed into three subperiods. First, there are the 45th-63rd Congresses, during which the percent of high Rice indices exceeds the expected level (2.5%) in only 7 of 19 Houses, while the percent of low Rice indices exceeds expectations 15 out of 19 times. A second subperiod runs from the 64th to the 72nd Congress and exhibits relatively high percentages of surprising Rice indices in both directions (high and low). Finally, the New Deal period, from the 73rd to 77th Congress, again shows lower percentages of roll calls with surprising interparty differences.

It is important to note that, by the more conservative standard set in the last two columns, there is no evidence of party voting in the hey-day of czarism in the House (the 55th-60th Houses). Similarly, the early New Deal Houses, conventionally viewed as relatively partisan, also show less evidence of party voting. In the intermediate period between czar rule and the New Deal, one can easily and consistently reject the null of constant party pressure. Thus, there is a

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9. The bootstrap procedure has the following steps. (1) We use the first dimension W-NOMINATE coordinates as starting values (using only those roll calls with 2.5% or better in the minority). (2) We estimate the parameters of the one-dimensional quadratic utility model presented above—that is, the ideal point and standard error for each legislator, the cutpoint and gap for each roll call (via Poole 2001). (3) Using the parameters from step (2), for each roll call, we compute the mean and variance of D_L and R_L (via equations (1) and (2)). We then compare the actual Rice Index value for each roll call to see if it exceeds the critical value given by the mean μ_j plus 1.96 times the estimated standard error σ_j. (4) We run a bootstrap procedure 100 times to assess the variability of the number of roll calls above the critical values calculated in step 3. This fourth step involves the following substeps. 4a) Sample the roll calls with replacement. This yields a matrix of scalable roll calls of the same size as the original. 4b) Repeat steps (1) - (3) to get the number of roll calls above the critical value. 4c) Do (4a) - (4b) 100 times. From these results, calculate a "standard error" for the number of roll calls above the critical value.

clear inverse relationship between conventional measures of partisan voting and qualitative descriptions, on the one hand, and our percentages, on the other.

This inverse relationship is to be expected, because our test can only detect party pressures if they vary substantially across the observed roll calls in the sample. If party pressure in the czar-rule Houses was *consistently* high (cf. Brady 1973), then our test should not reject the null in these Houses. We stress that one would expect similar results for other highly partisan legislatures, such as the British House of Commons. Thus, one emphatically cannot take the percentage of roll calls with surprising Rice indices as an indicator of the mean strength of party pressures in a given Congress. It is much closer to being a measure of the variance in party pressure across roll calls.

All this reflects the point that our method tests the null that party pressure is constant, rather than that it is specifically zero. Thus, when we accept the null, this could mean consistently high pressures or consistently low pressures—we do not know which.

Testing procedural cartel theory

In this section, we test two hypotheses about party pressure drawn from procedural cartel theory (Cox and McCubbins 1993). First, party pressure should be higher on procedural and organizational votes (as compared to substantive votes). Second, party pressure should be higher on votes key to defining the parties' labels (as compared to those less electorally important). The first idea has been around for some time (e.g., Froman and Ripley 1965) but has been tested only with uncontrolled statistics. That is, scholars have found the parties to be more cohesive on procedural votes before (notably Rohde 1991, p. 53) but have not shown them to be any more cohesive than would be expected were all members simply voting their constituencies.

Operational approach

The dependent variable in our analysis is the difference between the actual and expected Rice index of party dissimilarity, $R_j - \mu_j$. If parties exert variable pressure across roll calls, $R_j - \mu_j$ will be a proxy for party pressure.

Hence, it should be larger on procedural, organizational and label-defining votes. In contrast, if parties exert constant pressure (perhaps zero) across roll calls, then $R_j - \mu_j$ will be white noise and unrelated to the various categories of vote.

To operationally define our vote categories, we proceed as follows. First, we distinguish two kinds of procedural votes. *Core procedural* votes include appeals of the Speaker's decisions on the floor, plus votes pertaining to special rules granted by the Rules Committee. Procedural votes that are not core are denoted "other procedural." Second, we distinguish three kinds of organizational vote: (1) the election of the Speaker; (2) votes on the staffing, funding and operation of the committees; and (3) votes on the adoption of rules that will govern House procedure. Third, we distinguish two kinds of label-defining vote, relating to the two issues that have most clearly distinguished the parties since the mid-1960s: taxes and welfare. A detailed listing of the votes falling into our various categories is given in an appendix available at k7moa.uh.edu.

To conduct our tests, we use a dataset compiled by David Rohde that classifies each roll call held in the 83rd – 105th Houses by the type of vote (see the appendix at k7moa.uh.edu for a listing of the categories). We split this sample of Houses into three periods, based on the likely strength of the majority party's procedural control: the 83rd-86th (before the packing of the Rules Committee); the 87th-92nd (after the packing of the Rules Committee but before the procedural reforms in the 93rd House); and the 93rd-105th (the post-reform House). The reforms we have chosen as demarcating our periods stand out in the previous literature as the logical choices. Before the Rules Committee was packed with additional liberal members after the 1960 election, standard sources view it as independent of the majority party (and, indeed, dominated by a conservative coalition of Southern Democrats and Republicans). Afterwards, the majority's control was improved but standard accounts stress that Rules continued to be largely independent of the majority party until further reforms in 1975 (see Rohde 1991, p. 25; Peabody 1963; Oppenheimer 1977). Finally, the procedural reforms of 1973, including the subcommittee bill of rights, were a true watershed—if one accepts the analysis in Rohde (1991).

Results

The results of our analysis (a regression of $R_j - \mu_j$ on variables indicating procedural, organizational and label-defining votes) are presented in Table 2. In the first period, 1953-60, the only significant effect is that elections of the Speaker do separate the parties more than would be expected on the basis of members' ideal points (and the roll call's cutpoint). The data, for the most part, do not conform to the predictions of procedural cartel theory.[10]

[Table 2 about here.]

In contrast, most of the theory's predictions are borne out in the second period, 1961-72. First, in the reference group of votes (i.e., votes that are neither procedural nor organizational nor label-defining), the actual Rice indices fall systematically short of the expected values—consistent with the hypothesis that party pressures tended to be below-average on such votes. Second, actual Rice indices on ordinary procedural votes systematically exceed their expected values. That is, the parties voted more differently than expected, consistent with the hypothesis that party pressures tended to be above-average on procedural votes.[11] Third, organizational votes pertaining to the Speakership and the adoption of House rules both show systematically greater party voting differences than expected, consistent with the hypothesis that party pressures are greater than average on such votes. Finally, votes pertaining to taxes also exhibit systematically higher Rice indices (greater party differences) than expected, again suggesting higher-than-average party pressure.

As a rough guide to interpreting how many votes are switching, we note that a change in the Rice index of .05 implies about 10 votes switching, when there are 240 Democrats and 195 Republicans in the House (a typical figure). Thus, the difference in the 1961-72 period between a substantive vote and an

[10]. Note that there were no votes on House rules during these Congresses. Thus, we cannot examine whether such votes are more highly pressured or not. Similarly, there were no votes on contested elections.

[11]. Actual Rice values tend to exceed expectations by an even larger margin for votes on special rules, although not consistently enough to attain statistical significance.

ordinary procedural vote is about eight votes switching due, presumably, to party pressures. Using the same rule of thumb, about twelve votes switch between otherwise identical substantive and special rule votes, and about 82 votes switch as between an ordinary substantive vote and the election of the Speaker. Note that the *size* of these effects is definitely affected by the assumption of unidimensionality. In only one dimension, there is substantial overlap between the two parties' distributions of ideal points, especially in this middle period. Thus, no cutpoint perfectly separates the two parties and party-line votes such as the Speakership election necessarily require many switches from expected behavior. Much smaller vote changes are implied in our two-dimensional analyses below, in which party-line votes can be much better approximated.

In the last period, 1973-1999, we find even stronger confirmation for the procedural cartel viewpoint. Interpreting larger-than-expected Rice values as higher-than-average party pressures, we find the following:

- Substantive votes tend to have below-average pressure.
- Ordinary procedural votes tend to have above-average pressure, altering about eleven votes from what would have been expected on a substantive vote with the same cutpoint.
- Votes on special rules have even higher pressure than do ordinary procedural votes, altering about fifteen votes from the baseline.
- Organizational votes—electing the Speaker, regulating committees, and establishing House rules—all have substantially higher-than-average pressure, altering roughly 20-40 votes (from what would have been expected on a substantive vote with the same cutpoint).
- Label-defining votes—on taxes and welfare—have higher-than-average pressure, altering three to nine votes.
- Finally, a handful of votes on contested elections, which one would presume to divide the parties as parties, also show higher-than-average pressure.

We have also ran our regressions separately for each Congress from the 83rd to 105th. The results, summarized in tables reported on the web at k7moa.uh.edu, show that the Congresses we have chosen as demarcating our early, middle and late periods are appropriate.

Summary

There are three summary conclusions we wish to stress. First, except in the first period, our results are as expected under procedural cartel theory.[12] The parties vote more as parties than would be expected on the basis of their members' ideal points (and other model parameters) on procedural, organizational and label-defining votes. This is inconsistent with the null model, hence with a partyless model.

Second, our results support Rohde's (1991) analysis of the 1973 procedural reforms in the House. Party effects in the post-reform House are generally larger and more precisely estimated.

Third, an important procedural watershed appears even earlier, with the packing of the Rules Committee. Our results suggest that the importance of this initial breaking of the Democratic logjam has been underestimated in the literature on congressional organization.

Extension to Two Dimensions

In this section, we investigate whether our results in Table 2 are affected by using a two-dimensional instead of a one-dimensional model. To replicate Table 2, we regress the difference between the actual and expected Rice index for a given roll call on various indicators describing that roll call (those used in Table 2). The only difference is in the dependent variable: we compute the expected Rice index based on the two-dimensional, instead of the one-dimensional, model. Our new results are presented in Table 3.

[Table 3 about here.]

For the 83rd to 86th Congresses, the sign and significance of all variables except one are preserved. The one exception is the variable indicating a Speakership election: although the sign of this variable continues to be positive—that is, party pressures tend to be higher on Speakership elections—the tendency is no longer statistically significant. Thus, with our two-dimensional model, there is no reason at all to reject the notion that party pressures are constant across the various categories of roll call identified in our analysis.

For the 87th to 92nd Congresses, we continue to find systematically greater party pressures on Speakership elections and on votes to adopt House rules. Our results regarding procedural votes and votes pertaining to special rules differ slightly. In the one-dimensional results, procedural votes are more highly pressured than the baseline votes but votes on special rules do not stand out clearly from the rest of the procedural votes. In the two-dimensional results, procedural votes are not significantly more pressured than the baseline votes but votes on special rules are significantly more pressured than both the baseline and the other procedural votes. Finally, our findings on tax votes are not robust: party pressures tend to be higher on tax votes but not significantly so.

In the 93rd to 105th Congresses, our results are qualitatively similar for all variables, except two. In the two-dimensional model, votes on committee organization and votes on welfare issues no longer stand out as more highly pressured than the baseline votes. Votes on procedure, on Speakership elections, on adoption of House rules, and on taxes all continue to exhibit systematically higher party separation than expected. Moreover, votes on special rules stand out, even among procedural votes, as especially high-pressure events. The coefficients in Table 3 imply that the two parties are from 4 to 12 votes more separated on the various categories of procedural and organizational votes, than on the baseline group—a considerably smaller set of estimates than in the one-dimensional model but still within the range of values

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[12]. In the first period, there is still substantial evidence of other sorts that the majority party operated a cartel—on which see Cox and McCubbins N.d.

suggested by the qualitative evidence on vest pocket votes (cf. King and Zeckhauser 1999).

All told, then, our two-dimensional results—though slightly weaker statistically than the one-dimensional results—continue to support strongly the notion that parties exert more pressure on procedural and organizational votes. The evidence that parties exert more pressure on label-defining votes is now confined to the last, post-reform period.

Conclusion

The study of party voting has been beset by both methodological difficulties and, partly for this reason, continuing substantive debates. The methodological issue is quite general: how best to detect the presence of party pressure in legislative voting analyses, given only members' recorded votes on each roll call? The trick is to identify systematic departures from the voting behavior one would expect on the basis of members' estimated ideal points alone, while recognizing that those ideal point estimates may already reflect party pressure. The substantive debate concerns whether legislative parties in the U.S. House of Representatives consequentially deflect their members' voting behavior from what would otherwise be expected.

Methodology

We have presented three reasons to reject the seminal Snyder-Groseclose (2000) estimator of party effects, all focusing on their second stage. First, they use a linear probability model which leads to overstating party effects for a wide range of parameter values. Second, in simulations with plausible parameter values the Snyder-Groseclose estimator detects party effects 42-44% of the time, when by construction none exist. Third, in analysis of real-world data, we find party effects about half as often as do Snyder and Groseclose.

While nearly identical to McCarty, Poole and Rosenthal's two-cutpoint test in terms of estimation and results, we differ sharply in the interpretation of those results. In particular, McCarty, Poole and Rosenthal argue that parties substantially affect members' ideal point locations (see also Hager and Talbert

2000; Nokken 2000) but that, once these locations are determined, further party effects are minimal. We agree that parties substantially affect the location of members' ideal points but find continuing party effects, even conditional on ideal point locations.

Results

We have presented two sorts of test. The results of our first test show that one can easily reject the null of "constant party pressures" for almost all postwar Congresses (and for about two-thirds prewar). These results, however, hinge on the assumption of unidimensionality.

Our second set of results is not dependent on using a unidimensional model. Even in two dimensions, we find the following patterns. First, before the packing of the Rules Committee in 1961, the two parties did not vote any differently than expected on procedural, organizational and label-defining votes. Second, after 1961, and especially after the procedural reforms in 1973, the two parties voted more dissimilarly than expected on procedural, organizational and label-defining votes.

These results support the general notion of parties as procedural cartels (Cox and McCubbins 1993). Moreover, they shed light on the partisan consequences of two watershed organizational episodes in House history. Finally, they help refute the widely-held "minimal party effects" thesis about roll call voting in the U.S. House. As articulated by Mayhew (1974, p. 100): "Party 'pressure' to vote one way or another is minimal. Party 'whipping' hardly deserves the name." This view of party strength is common—indeed, arguably dominant—in the literature of the 50s, 60s, 70s and 80s. Our results argue against it strongly. It is not just that there are statistically significant party effects. They are substantively significant, too. McCarty, Poole and Rosenthal (2001), Hager and Talbert (2000) and Nokken (2000) have shown that members who switch parties (and thus change the nature of party pressures by which they are influenced) exhibit big changes in voting behavior. In this paper, we have shown that hypothetically changing a roll call from a "low" pressure (e.g., a substantive vote) to a "high" pressure one (e.g., an organizational vote) would switch from

20-40 votes in the period 1973-1999, in a one-dimensional model, and 4-12, in a two-dimensional model. Thus, party pressure does affect a noticeable number of votes, even controlling for ideal points that themselves impound party pressures. Moreover, the pressured votes count because procedural and organizational decisions strongly influence substantive outcomes.

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Table 1:

Party Effects for Houses 45 - 105

Congress	RCs	Scaled	Between Medians	High[a]	Low[b]	Percent High	Percent Low	Lower Bound Percent High[c]	Lower Bound Percent Low[d]
45(1877-8)	377	362	293	18	36	6.143	12.287	1.386	7.094
46	439	425	355	3	33	0.845	9.296	-0.782	5.268
47	349	288	238	0	14	0	5.882	-1.734	2.625
48	334	312	229	7	31	3.057	13.537	0.134	9.162
49	306	291	204	4	18	1.961	8.824	-0.351	4.118
50(1887-8)	320	272	181	3	13	1.657	7.182	-0.717	2.966
51	587	539	481	1	27	0.208	5.613	-0.733	2.813
52	304	271	135	31	20	22.963	14.815	14.601	8.967
53	373	284	202	30	42	14.851	20.792	8.389	16.036
54	162	158	127	2	14	1.575	11.024	-1.981	5.035
55(1897-8)	183	172	152	1	7	0.658	4.605	-1.859	0.746
56	149	141	123	0	11	0	8.943	-4.488	4.153
57	185	160	128	2	7	1.563	5.469	-0.463	3.166
58	87	84	78	0	4	0	5.128	-1.813	-7.403
59	136	125	101	0	4	0	3.960	-3.521	0.905
60(1907-8)	312	231	178	0	9	0	5.056	-1.004	1.416
61	202	193	167	9	20	5.389	11.976	0.620	5.938
62	262	249	173	5	14	2.890	8.092	-0.661	4.121
63	282	268	182	10	19	5.495	10.440	0.665	6.429
64	157	148	91	15	11	16.484	12.088	6.114	3.743
65(1917-8)	266	217	119	4	9	3.361	7.563	-1.748	1.884
66	339	271	159	4	8	2.516	5.031	-0.182	-1.517
67	362	325	230	24	18	10.435	7.826	5.481	3.849
68	179	164	108	21	24	19.444	22.222	11.804	14.180
69	114	106	55	14	11	25.455	20.000	13.229	6.898
70(1927-8)	72	68	39	5	8	12.821	20.513	0.728	2.821
71	103	98	66	9	14	13.636	21.212	4.406	8.212
72	123	115	79	12	19	15.190	24.051	8.463	10.684
73	143	137	109	2	8	1.835	7.340	-8.462	0.110
74	212	206	132	13	9	9.848	6.818	1.762	0.200
75(1937-8)	158	153	113	10	10	8.850	8.850	1.811	2.444
76	227	216	166	6	8	3.614	4.819	-2.884	1.433
77	152	126	72	4	8	5.556	11.111	-5.289	1.236
78	156	135	75	14	14	18.667	18.667	7.600	6.669
79	231	206	113	37	34	32.743	30.089	26.207	19.575
80(1947-8)	163	146	73	5	6	6.849	8.219	-2.945	1.011
81	275	245	137	41	45	29.927	32.847	22.926	23.655
82	181	159	108	33	43	30.556	39.815	19.394	30.841
83	147	120	63	19	15	30.159	23.810	16.248	8.797
84	149	121	66	12	13	18.182	19.697	6.803	5.803

85(1957-8)	193	172	96	41	35	42.708	36.458	33.825	24.654
86	180	162	101	43	36	42.574	35.644	31.980	25.751
87	240	202	119	40	41	33.613	34.454	25.197	24.798
88	232	200	126	38	42	30.159	33.333	23.100	25.059
89	394	304	194	75	67	38.660	34.536	32.348	27.806
90(1967-8)	478	389	182	71	66	39.011	36.264	32.186	29.980
91	443	353	137	48	62	35.037	45.255	25.966	33.518
92	649	527	236	88	78	37.288	33.051	30.847	25.860
93	1078	918	403	116	148	28.784	36.725	24.738	31.091
94	1273	1065	556	164	184	29.496	33.094	25.951	28.649
95(1977-8)	1540	1217	606	185	177	30.528	29.208	26.808	25.394
96	1276	1067	575	138	160	24.000	27.826	21.054	23.809
97	812	679	329	85	81	25.836	24.620	22.537	17.936
98	896	784	492	97	124	19.715	25.203	16.748	20.943
99	890	777	543	79	95	14.549	17.495	12.436	14.497
100(1987-8)	939	771	552	80	100	14.493	18.116	12.623	14.626
101	879	752	496	79	94	15.927	18.952	12.936	15.419
102	901	780	563	78	68	14.029	12.230	11.414	9.045
103	1094	969	715	87	88	12.168	12.308	9.837	9.119
104	1321	1176	920	131	139	14.239	15.109	11.230	11.895
105(1997-8)	1166	946	643	107	94	16.641	14.619	13.617	10.197

[a] Number of roll calls with actual Rice indices more than 1.96 standard errors *above* the expected Rice index.
[b] Number of roll calls with actual Rice indices more than 1.96 standard errors *below* the expected Rice index.
[c] Number of roll calls with high party pressure minus two times the bootstrapped standard error divided by the number of roll calls between the party medians. Expressed as a percentage.
[d] Number of roll calls with low party pressure minus two times the bootstrapped standard error divided by the number of roll calls between the party medians. Expressed as a percentage.

Table 2:

Rice indices are higher than expected on procedural, organizational and label-defining votes

Independent variables	83^{rd} – 86^{th} Houses	87^{th} – 92^{nd} Houses	93^{rd} – 105^{th} Houses
Constant	–.003	–.017**	–.035**
	(–0.25)	(–2.02)	(–23.07)
Procedural: All	0.003	0.040**	0.057**
	(0.13)	(3.10)	(20.27)
Procedural: Special rules only	–0.018	0.022	0.018**
	(–0.53)	(0.84)	(4.44)
Organizational: Speakership	0.159**	0.410**	0.214**
	(1.98)	(6.04)	(8.87)
Organizational: Committees	.018	.062	.116**
	(.26)	(1.33)	(13.22)
Organizational: House rules	__	.200**	.174**
		(2.50)	(7.47)
Label-defining: Taxes	.001	.125**	.046**
	(.03)	(5.32)	(11.32)
Label-defining: Welfare	–.162	–.000	.015*
	(–1.02)	(–.01)	(1.87)
Contested election	__	__	.186**
			(4.88)
Number of observations	307	956	7057
R^2	.02	.07	.12

Notes: One asterisk indicates statistical significance at the .10 level in a two-tailed test. Two asterisks indicate significance at the .05 level.

Table 3:

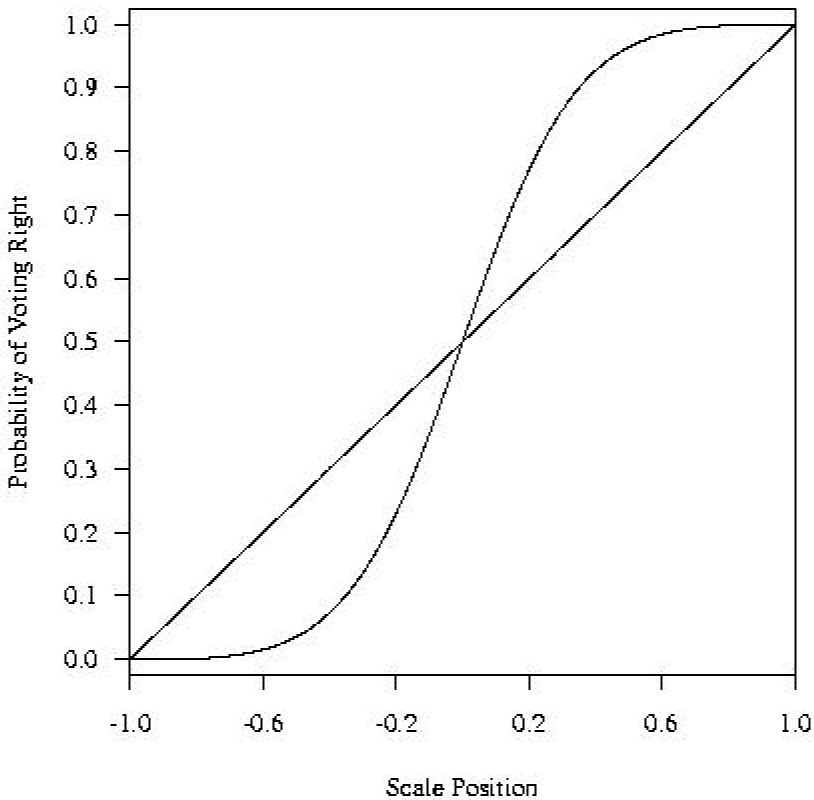
Replicating Table 2 with two-dimensional preferences

Independent variables	83^{rd} – 86^{th} Houses	87^{th} – 92^{nd} Houses	93^{rd} – 105^{th} Houses
Constant	–.003	.003**	–.006**
	(–0.86)	(2.06)	(–10.36)
Procedural: All	0.005	-.000	0.012**
	(1.06)	(-.05)	(10.74)
Procedural: Special rules only	–0.006	0.013**	0.009**
	(–0.74)	(3.21)	(5.61)
Organizational: Speakership	0.023	0.025**	0.062**
	(1.20)	(2.46)	(6.51)
Organizational: Committees	-.006	.001	-.001
	(-0.34)	(0.10)	(-.40)
Organizational: House rules	—	.021*	.037**
		(1.77)	(4.02)
Label-defining: Taxes	.011	.004	.004**
	(1.10)	(1.20)	(2.73)
Label-defining: Welfare	–.042	–.005	.000
	(–1.14)	(–1.01)	(.02)
Contested election	—	—	.057**
			(3.80)
Number of observations	307	956	7057
R^2	.02	.02	.05

Notes: One asterisk indicates statistical significance at the .10 level in a two-tailed test. Two asterisks indicate significance at the .05 level.

Figure 1:

An example of how scale position translates into a probability of voting "right"



Appendix: Categorization of votes

Votes included in the analysis
(with number of observations in parentheses)

Substantive votes

Final passage votes (1643)

Passage of a Bill (942)
Final Passage of Conference Report (331)
Final Passage of Resolution (235)
Final Passage of Joint Resolution (84)
Adoption of Concurrent Resolution (45)
Adoption of First Part of Resolution (4)
Adoption of Second Part of Resolution (2)

Votes on amendments (3718)

Straight Amendments (includes en bloc & substitute amendments) (3292)
Amendments to Amendments (390)
Substitute (35)
Amendment to Substitute (1)

Core procedural votes

Votes on special rules (894)

Passage of Special Rules (649)
Previous Question on Special Rules (245)

Votes relating to the speakership (25)

Appeal of the Chair's Ruling (3)
Election of Speaker (22)

Other procedural votes (2037)

Motion to Postpone (5)
Motion to Rise from the Committee of the Whole (67)
Demand for a Second (15)
Motion to Adjourn (80)
Motion to Resolve into the Committee of the Whole (9)
Motion to Table (198)
Motion to Proceed (1)
Motion to End Debate (27)
Motion to Order Previous Question (116)

Dispense with Further Proceedings with Quorum Call (11)
Motion to Discharge (2)
Miscellaneous (120)
Motion to Agree (22)
Motion to Delete (8)
Motion to Disagree (8)
Motion to Recede (45)
Motion to Commit (16)
Motion to Consider (13)
Motion to Refer (10)
Motion to Strike (28)
Vote to Approve House Journal (372)
Motion to Recommit (670)
Motion to Instruct Conferees (87)
Motion to Recede and Concur (107)

Votes excluded from analysis

Vetoes, treaties, constitutional amendments

Amendments to the Constitution
Passage over Presidential Veto
Treaty Ratification

Suspension of the rules

Suspension of Rules for a Bill
Suspension of Rules for a Joint Resolution
Suspension of Rules for Concurrent Resolution
Suspension of Rules for a Resolution
Suspension of Rules to Amend Bill
Suspension of Rules for Conference Report
Motion to Suspend the Rules and Concur

Simulated data and the Groseclose-Snyder method

In this appendix to our publication (Gary W. Cox and Keith Poole. N.d. “On measuring partisanship in roll call voting: The U.S. House of Representatives, 1877-1999.” American Journal of Political Science. Forthcoming.), we detail the procedure by which we generated simulated data to test the Groseclose-Snyder method.

Our procedure is as follows. First, we stipulate some ideal points. In particular, we take each member’s two-dimensional W-NOMINATE score for the 90th Congress (as estimated by Poole and Rosenthal, 1985, 1997) as his or her true ideal point. Second, we randomly generate cutlines until we exactly match the empirically observed vote margins in the 90th House.[13] Third, we select various possible values for the gap parameter, so as to match the empirically observed classification error rates for the 90th Congress.[14] Fourth, given the model parameters, we calculate each legislator’s theoretical probability of voting “right”, p_{ij}, then draw each legislator’s vote as an independent binomial with probability p_{ij}. Fifth, we regress the simulated votes on a constant, the first and second coordinates of each member’s true ideal point, and a party dummy variable. Sixth, we compute the percentage of all roll calls that generate false positives—i.e., a significant coefficient on the party dummy variable. Note that *any* significant coefficient on the party dummy will be a false positive, as there are no party pressures in the simulation.

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[13]. Specifically, we divided the majority vote margins between 50 and 100 percent into ten 5 percent intervals and replicated the percentage of roll calls in each interval. Following Poole and Rosenthal (1997), we discarded all roll calls with majority margins greater than 97.5 percent.

[14]. For the postwar Congresses under study here, the typical classification error rates for the NOMINATE procedure are between 10% and 15% (see Poole and Rosenthal, 1997, 2001). That is, 10-15% of all vote choices are such that a member whose ideal point is on one side of the cutline nonetheless votes as if it were on the other. For our simulation, we know the true ideal points and the true cutlines and can thus compute an *expected* vote for each member (e.g., a member to one side of the cutline is expected to vote with that side). We also know the *simulated* vote for each member. We can thus compute a simulated classification error rate, as the number of disagreements between the expected and simulated vote, divided by the total number of votes to be predicted (435×number of roll calls). We choose the gap parameter so that the simulated error rate matches the empirical error rate.

Running the simulation just described, we find that 209 of the 500 simulated roll calls, or 42%, exhibited significant party coefficients. Looking just at close votes, we find that 89 of 204, or 44%, register false positives.[15] These are the numbers reported in our publication.

[15] Close roll calls are those in which the winning side has between 50% and 65% of the total vote. All other roll calls are lopsided.

Appendix:

Congress-by-Congress estimates of party effects

In this appendix, we report the results of regressions identical to those reported in our publication (Gary W. Cox and Keith Poole. N.d. "On measuring partisanship in roll call voting: The U.S. House of Representatives, 1877-1999." American Journal of Political Science. Forthcoming.), except that the regression is run separately for each Congress.

In the 83rd – 86th Congresses, there were no significant effects of any kind in the single-Congress regressions. The significant effect for the Speakership elections reported in Table 2 arise from pooling a series of relatively weak statistical effects across the four Congresses.

[Table A about here.]

The first significant effect in a single-Congress regression arises in the 87th, where the parties are unusually distinct in the election of the Speaker. The first Congress with multiple results that conform to procedural cartel theory is the 88th, in which the parties are systematically more distinct on procedural, Speakership and tax votes than on the baseline group of substantive votes. After a lapse in the 89th Congress—the parties are distinct only on the Speakership election—the 90th shows the parties systematically more different than expected on procedural, Speakership, committee organizational, and tax votes. The 91st House shows unexpected differences between the parties on the Speakership election and taxes. Finally, the 92nd House finishes the middle period with the first evidence of systematically greater party pressures on the adoption of House rules, in addition to higher pressures on special rules, Speakership and tax votes.

In our last period, the two parties are *always* systematically more different than expected on procedural votes, with special rules often being especially partisan. Among the organizational votes, the parties are almost always systematically more different than expected on votes affecting the organization of committees; and are more often than not unusually distinct on Speakership and House rules votes. Finally, the partisan battle over taxes is evident in all but three of the post-reform Houses.[16]

Table A:
Congress-by-congress evidence on Rice indices

Congress	Proced.: ordinary	Proced.: Special rule	Org.: Speaker	Org.: Comm.	Org.: House rules	Label-defining: taxes
83					NA	
84					NA	
85				NA	NA	
86					NA	
87			*		NA	
88	**		**		NA	**
89			**	NA	NA	
90	**		**	**		**
91			**			**
92		*	**		**	**
93	**		**	**	**	**
94	**	**	**	**	**	
95	**		**	**	**	**
96	**	**	**	**	**	**
97	**		**	**	NA	**
98	**	**	**	**	NA	**

16. Table 3 does not report the results for the welfare variable—never significant except in the 105th Congress—and the contested elections variable—where typically there is not a relevant vote (when there is, it is significant in three of four Houses).

99	**			**	*	**
100	**		**	**	**	**
101	**	**	**	NA	NA	
102	**	**				**
103	**			**		**
104	**			**		**
105	**	*		**		

Notes: A cell entry of “NA” indicates that there were no observations of the particular type of vote (indicated in the column heading) in the relevant Congress (indicated in the row number). A blank entry indicates that there was no significant coefficient for the relevant variable and Congress. Finally, a single asterisk indicates a positive coefficient significant at the .10 level in a two-tailed test, while two asterisks indicate a positive coefficient significant at the .05 level in a two-tailed test.