Parties and Institutional Choice Revisited
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# *Parties and Institutional Choice Revisited*

Scholars of institutional change in Congress offer competing theoretical accounts of the accrual of procedural rights by House majority parties. One camp posits that the interests and capacities of political parties drive procedural change that affects agenda control. An alternative perspective offers a nonpartisan, median-voter account. I explore these two accounts, survey challenges involved in testing them, and determine the fit of the accounts to the history of procedural change in the House. I find that no single perspective accounts best for the pattern of rule changes affecting agenda control and that the median-voter model may be time-bound to the twentieth century—after partisan majorities had constructed the core partisan procedural regime of the House.

Today we know more than ever about the political dynamics that drive the development of the U.S. Congress. Studies of committee reform (Adler 2002), agenda control (Schickler 2000, 2001), partisan organization (Cox and McCubbins 2005; Smith and Gamm 2005), procedural rights (Binder 1997; Dion 1997), and more have offered both theoretical and empirical advances in our understanding of the forces that drive congressional change. Although much remains to be explained about the evolution of the House and Senate as political institutions, we now have competing accounts of the development of some central elements of the modern Congress.

In this paper, I take stock of two competing theoretical accounts of the accrual of procedural rights by House majority parties over the course of congressional history. One camp posits that changes in the rules that allocate agenda control are best explained by the interests and capacities of political parties (Binder 1997; Cooper and Brady 1981; Cox and McCubbins 1997; Dion 1997). The main alternative perspective is a nonpartisan account offered by Schickler (2000) based on median-voter theory (Black 1958). I articulate the grounds of both theoretical approaches and propose an empirical assessment of the fit

of the competing accounts. My findings suggest that, while it is premature to reject party-based accounts of procedural change, no single account best explains the politics of institutional change.

## Competing Accounts of Procedural Change

In recent years, many students of legislative politics have tackled questions about the dynamics of institutional change. Viewing institutions as endogenous to the preferences of legislators, these researchers have attempted to discern how preferences matter and, perhaps as important, *whose* preferences matter in shaping the timing and direction of procedural change. The majority of such authors favor a party-centric account, exploring how and why the preferences of the majority party shape institutional choices in Congress.[1]

As Cox and McCubbins (1997) point out, party-centric accounts embrace two separate theoretical claims. Most accounts focus on explaining "particular changes in the rules at particular times" (Cox and McCubbins 1997, 1376). The consensus view emerging from this work is that increases in the homogeneity of the majority party encourage rank-and-file party members to delegate more procedural powers to their leaders (Cooper and Brady 1981; Rohde 1991; Sinclair 1995). The collective interests of the majority party—whether defined in terms of policy or electoral goals—lead legislators to bolster the agenda-setting powers of their leaders.[2] Dion (1997), Fink (2000), and I (1997) have tested such accounts empirically by modeling changes in House rules that empower chamber majorities at the expense of the minority.

A second theoretical claim of party-centric accounts receives less attention. Party interests can affect the "whole set of rules . . . at any given time, and . . . [the] extent to which it favors the majority party" (Cox and McCubbins 1997, 1377). Because rule changes tend to outlive the coalitions that enact them, we should think of rule changes in legislative bodies as *cumulative*: although subject to change, they are inherited over time by subsequent majorities and give the chamber its "acquired procedural tendency" (Binder and Smith 1995). In other words, parties can help to shape specific episodes of change, as well as the core regime features of an institution. The Senate's core regime embraces individual and minority autonomy protected by the chamber's unique rules and practices that make it difficult for majorities to act by majority vote. The House's core regime features give considerable procedural advantage to the majority party, sparked initially by adaptation of the previous-question motion in 1811, which eased the

way for subsequent majorities to stack the rules to their advantage in the decades that followed (Binder 1997). More generally, party-centric accounts aim to explain the centralization of procedural powers in the hands of House majority parties and their leaders, as well as the specific adoption of rules that affect majority parties' control of the chamber agenda.

A key challenge to the party-based accounts comes from Eric Schickler's (2000) ideological balance of power model. In this nonpartisan alternative, the interests of the floor median dictate procedural change. As the floor median shifts closer to the majority party than to the minority party, the floor median favors rule changes that bolster the majority party's control of the agenda; when the floor median moves closer to the minority party median relative to the majority party median, the floor median favors rule changes that limit the agenda control of the majority party and boost the procedural rights of the minority. What matters in the nonpartisan model, in sum, is the ideological balance of power on the floor, "the closeness of the median voter on the floor to the two parties" (Schickler 2000, 270). Or, as Schickler and Rich (1997, 1387) summarize the approach, the model takes account of "the relative numbers of liberals and conservatives."

Schickler (2000, 270) recognizes that this model is not entirely nonpartisan, since "it accepts the notion that some allocation of agenda power to political parties is necessary for a legislature to function effectively." What is critical in distinguishing the two accounts, then, are the interests that drive the chamber's pursuit of rules that advantage the majority party. Party-centric accounts argue that the policy or electoral incentives of the majority party caucus are paramount in devising rule changes that centralize power in the majority party; the majority party's goals thus guide its deliberations over rule changes deemed necessary to secure the party's legislative and political goals. In contrast, in the nonpartisan account, the policy interests of the median voter are paramount: decisions to endow or strip the majority party of procedural advantage stem from the ideological goals of the median voter.

It is conceivable that no single model provides the best fit to the dynamics of institutional change in the House, even when the empirical focus concentrates on the allocation of procedural rights. Here, we are reminded of Cox and McCubbins's admonition to distinguish theoretically between the rules in place and changes to those rules. If House majority parties have at times been substantially advantaged by the rules base, then rule changes attempted at any given point in time may simply reflect marginal changes in the core regime. The ideological balance of power on the floor may indeed shift the rules periodically, but the core features of a party-majoritarian regime may remain in

place. We may need to integrate temporal variation into our explanations of procedural development in Congress and in other political bodies.

## Testing Competing Accounts

I propose to assess the fit of the party and nonpartisan accounts of procedural change, drawing on advances offered in previous studies of the development of the House. In this section, I consider matters of measurement and estimation, reviewing the range of empirical choices for devising a critical test of the alternative accounts.

### *Dependent Variable*

*Measurement.* How should changes in House rules be captured empirically? One approach conceptualizes a dichotomous dependent variable capturing whether or not a particular type of rule change was adopted in each Congress. I provided such an analysis in 1997, modeling the incidence of rule changes benefiting the majority party at the expense of the rights of the minority. The key alternative approach is offered by Schickler (2000), who creates a trichotomous dependent variable to capture rule changes that favored the majority party, favored the minority party, or were neutral in their partisan impact. Schickler proposes that we think of different types of rule changes as a set of ordered categories: Congresses with anti–majority party rule changes are coded as –1, Congresses with neutral or no rule changes are coded as 0, and Congresses with pro–majority rule changes are coded as 1. As I will later discuss, this categorization allows Schickler to estimate the likelihood of the different types of rule changes in a single estimation via ordered logit.

Which is the more-appropriate approach? For several reasons, a dichotomous dependent variable seems preferable.[3] First, when one includes both pro- and anti-majority rule changes in a single variable (thus modeling their incidence in a single model), one assumes that similar dynamics drive both the extension and retraction of minority rights. Several studies suggest, however, that different political dynamics are relevant in explaining the two sorts of changes. Fink (2000), for example, has shown that the type of partisan conflict that led to *restrictions* on access to the chamber floor in the early history of the House had little bearing on rule changes that *eased* access to the floor. Similarly, I have shown in a study of minority party rights (Binder 1997) that different dynamics drive the suppression and creation of minority rights in the House.

Second, as previously noted, a trichotomous dependent variable requires us to think about the rule changes as a set of ordered outcomes. In this particular context, the assumption may be problematic. When one estimates an ordered-logit equation, one assumes that the explanatory variables have roughly the same impact across all categories of the dependent variable (see Appendix for discussion). That is why we only estimate a single set of coefficients, even if we calculate different intercepts for each of the category equations. As I show in the Appendix, however, the three-category dependent variable in this context fails this proportional-odds assumption.

For both of these reasons—the differential dynamics behind different sorts of rule changes and the unordered character of such changes—it would appear that a dichotomous dependent variable is preferable. In the empirical analysis, I use two different dependent variables: one indicating whether or not pro–majority party rule changes were adopted in each Congress, and one indicating whether or not anti–majority party rule changes were adopted.[4]

*Time span.* To thoroughly test the competing partisan and nonpartisan models, we need a long time series. This requirement is more than a matter of adding observations to improve estimation; a long time series is necessary to capture the broadest array of rule changes affecting the majority's agenda powers. In particular, we need to capture variation in rules starting at the beginning of the House's history. By reaching back to the first Congress in 1789, the empirical model is better able to capture the dynamics of rule changes that produced the House's core procedural regime favoring the interests of the majority party. Truncating the time period increases the risk of misestimating party and median influences over the early period, during which the core features of the House were acquired. As I will later discuss, using a foreshortened time period increases our risk of failing to recognize the possibility that critical independent variables may vary as a by-product of the regime's development. For these reasons, the following empirical analysis relies on data I assembled for 1789 through 1990 (1st–100th Congresses; see Binder 1997).[5]

### *Independent Variables*

I draw the independent variables from existing tests of the partisan and nonpartisan accounts. To evaluate the median-voter account, I rely on Schickler's measure of the ideological power balance in each Congress. Schickler constructed the *Change in median* variable from first-dimension DW-NOMINATE scores, assuming a single ideological

dimension is primary to legislators' views about chamber rules. The variable taps change in the location of the floor median relative to the majority and minority party medians (as described in detail in Schickler 2000, 285).[6] Positive scores indicate a net movement of the floor median closer to the majority party median; negative scores indicate net movement toward the minority party median.

To evaluate the fit of the partisan accounts, I draw from my previous analysis of the suppression of minority rights (Binder 1997, chaps. 4, 5), where the key explanatory variables are measures of partisan capacity and partisan need. *Partisan capacity* taps the relative ability of the majority and minority parties to muster winning coalitions, and I calculate it as the difference in majority party and minority party strength (itself a function of party size and voting cohesion):[7] the greater the majority party's advantage in partisan capacity, the higher the probability of rule changes enhancing majority power.

*Partisan need* captures the frequency of minority obstructionism during a Congress, under the assumption that the greater the obstructive tendencies of the minority party, the more likely the majority party will be to perceive a need for procedural changes that enhance majority party agenda control. Multiple measures of partisan need are necessary, because the dominant form of minority obstruction in the House changed over time after Republicans clamped down on the minority's favored dilatory tactics with readoption of the Reed rules in 1894. There was, in short, a sea change in the minority's abilities to obstruct the majority's agenda after 1894. As Cox and McCubbins (2005) observe, procedural changes after 1894 did not alter the structure of agenda power established by 1894. Thus, 1894 is the appropriate time point for distinguishing the two periods, and different measures are required to capture minority obstruction in the two periods.

For the first series, running from 1789 through the readoption of the Reed rules in 1894, I use the percentage of votes that were motions to adjourn in the previous Congress as a proxy for minority obstructionism and thus partisan need for limiting minority rights. For the second series, running from 1895 to 1990, and for models running the entire time span of Congress, I use a variable that captures whether or not a minority right was created in the previous Congress.[8] Creation of a minority right in one Congress predictably leads to higher levels of obstructionism in the subsequent Congress (Binder 1997). Because Schickler's (2000) comparison of the partisan and nonpartisan models does not control for partisan need, it remains an open question whether or not the median-based model provides a better fit to the pattern of rule changes than a fully specified party-based model.

Finally, I include two controls, one tapping the level of *Workload*, the other capturing any expected *Change in party control* of the chamber.[9]

Some further consideration of two of the key independent variables—partisan capacity and the ideological balance of forces—is important here. I have noted the theoretical distinction between the two concepts as sources of institutional change. These concepts also differ significantly in empirical terms. Schickler (2000, 275) appropriately notes that the concept of party capacity "has much in common with the notion of the ideological balance of power on the floor." Still, the two measures do not capture identical types of variation.[10] Most important, party capacity captures both voting regularity within each party as well as party size. In contrast, the measure capturing the ideological balance of forces captures net movement in ideological positioning along the left-right dimension. Given those differences, the two series need not move in tandem. For instance, party capacity can increase (say, with an increase in the size of the majority party) without any change in the location of the chamber or party medians.[11] Alternatively, cohesion can vary independent of movement in the ideological position of the chamber or party medians. Even if changes in voting cohesion reflect, in part, changes in the ideological distribution of party members around their respective median, the ideological position of the median voter and party medians need not change as well. The partisan and nonpartisan accounts thus differ fundamentally in both theoretical and empirical terms.

*Estimation Strategy*

A final empirical question relates to the methodology for testing competing explanations. Most often, competing accounts are tested in a "nested" manner, meaning that one model can be reduced to the other by imposing a restriction on one of the parameter estimates (Clarke 2001; Kennedy 1993, 81). Although this technique may be an appropriate strategy for many applications, the median and party-based models are, in fact, non-nested: they imply a set of nonoverlapping covariates. Combining the models into a single "supermodel," in Clarke's terms (2001, 730), amounts to "artificially nesting the two models in a single equation and treating the rival model as a control." This technique raises theoretical and methodological difficulties. First, we have no expectation that the adoption of pro-majority rule changes is a function of both party caucus and floor median interests. Thus, there is little theoretical basis for combining the key variables into a

single model. Methodological problems arise because traditional methods of discriminating between models (F-tests, likelihood ratio tests, and so on) will fail if applied to non-nested models (Clarke 2001; Kennedy 1993). Estimating the models separately and comparing the log-likelihoods is insufficient, since the number of variables in each model affects the size of the log-likelihoods. A methodology for testing non-nested hypotheses is thus required.

Following Raftery (1995), Long and Freese (2000), and Lawrence, Maltzman, and Smith (2006), I use the Bayesian Information Criterion (BIC) to compare models testing for the effects of the partisan and nonpartisan explanatory variables. The BIC values are essentially goodness-of-fit values that allow one to compare non-nested models with different numbers of parameters. BIC values are adjusted (or "penalized") by the number of estimated parameters and the sample size.[12] To generate a BIC statistic for each model, I test the party- and median-based accounts as non-nested logit models, using the BIC criterion to distinguish between the models.[13] The preferred model is that with the smallest value of the BIC statistic (Raftery 1995).[14]

## Results

In Table 1, I present the results for non-nested models to account for the timing of minority rights suppression. Each model consists of two estimations. The first estimates the party model for a specific time period; the second, the median-voter model for the same time period.

One can compare the paired estimations (model 1a versus 1b, model 2a versus 2b, and model 3a versus 3b) using the BIC statistic generated for each model, with the lower BIC statistic corresponding to the better-fitting model of the two paired estimations. The paired estimations can also be judged according to the magnitude of the difference in the BIC statistics, using guidelines reported by Raftery (1995). An absolute difference of 0 to 2 between two BIC statistics provides weak evidence in favor of the model with the lower BIC; a difference of 2 to 6 provides positive evidence; a difference of 6 to 10 provides strong evidence; and a difference greater than 10 yields very strong evidence in favor of the model with the lower BIC.

Of the pre-Reed comparison in models 1a and 1b, the most striking result is the robustness of the party model compared to the median-voter model.[15] Note first that the coefficient for change in median in model 1b is statistically insignificant, producing an insignificant $\chi^2$ for the model. The preferences of the floor median had little bearing on the adoption of rule changes advantaging the majority party during

TABLE 1
Non-nested Models Explaining the Suppression of Minority Rights (Logit Model Estimates)
(standard errors in parentheses)

| Theory | Variable | Model 1a PARTY Pre-Reed[a] | Model 1b MEDIAN Pre-Reed[a] | Model 2a PARTY Post-Reed[b] | Model 2b MEDIAN Post-Reed[b] | Model 3a PARTY Full series[c] | Model 3b MEDIAN Full Series[c] |
|---|---|---|---|---|---|---|---|
| Party | Partisan capacity | .137* | | .130* | | .110** | |
| | | (.066) | | (.067) | | (.040) | |
| | Partisan need | 25.396* | | 4.370** | | 2.412** | |
| | | (11.745) | | (1.748) | | (.843) | |
| | Change in party control | –.098 | | –.856 | | .290 | |
| | | (1.093) | | (1.355) | | (.735) | |
| | Workload | .174 | | 1.500* | | .096 | |
| | | (.553) | | (.859) | | (.308) | |
| Median-voter | Change in median | | –.888 | | 5.482* | | 2.512 |
| | | | (2.565) | | (2.618) | | (1.721) |
| | Constant | –5.549*** | –1.339*** | –4.826** | –1.641*** | –3.403*** | –1.451*** |
| | | (1.729) | (2.565) | | (.447) | (.785) | (.270) |
| | N | 49 | 49 | 46 | 46 | 95 | 95 |
| | LR $\chi^2$ | 17.97 | .12 | 15.64 | 5.30 | 19.33 | 2.25 |
| | Prob > $\chi^2$ | .0013 | .729 | .004 | .021 | .001 | .134 |
| | BIC | –139.623 | –133.447 | –127.141 | –128.279 | –334.103 | –330.682 |

[a]Drops 1st Congress because of inclusion of lagged independent variables; drops 19th Congress because of abrupt shift in legislative parties between 18th and 19th Congresses; and drops 30th and 46th Congresses because of simultaneous creation and suppression of rights in those Congresses.
[b]Drops 91st and 93d Congresses because of simultaneous creation and suppression of minority rights.
[c]Drops 1st, 19th, 30th, 46th, 91st, and 93d Congresses, as explained above.
* $p < .05$, ** $p < .01$, *** $p < .001$ (one-tailed tests).

the first half of congressional history. Second, according to the BIC statistics for models 1a and 1b, the party model outperforms the median-voter model, with the difference in BIC values indicating strong support for the party model.

The performance of the floor median in the post-Reed era is more consistent with the expectations of the ideological balance of power model, as seen in the results for models 2a and 2b. In the model testing for the impact of shifts in the position of the floor median (model 2b), the coefficient for the change in floor median is positive and statistically significant. This result suggests that a net movement of the median voter toward the majority party median helps to account for episodes of minority rights suppression in the twentieth century, after the majoritarian Reed rules were put in place by late nineteenth-century majorities. In the party-based model (2a), the coefficients for partisan need and partisan capacity are also statistically significant. The median model (2b), however, has a lower BIC statistic—although the small difference in the BIC statistics suggests that the median model only weakly outperforms the party model.[16]

For the full time period comparison in model 3, the party model (3a) receives stronger support than the floor median model (3b). As shown by model 3b, movement of the floor median over the course of congressional history appears to have, at best, a marginal impact on the adoption of rule changes advantaging the majority party ($p$ value = .07, one-tailed test), with the overall model $\chi^2$ statistically insignificant. In contrast, party incentives and capacity for adopting rule changes appear strongly related to the incidence of rule changes advantaging the majority, as shown by the significant parameter estimates for partisan capacity and need in model 3a. A comparison of the BIC statistics for models 3a and 3b also provides positive support for the party model. In sum, the BIC statistics indicate stronger support for the party-based model in two of the three estimations, with the party-based model strongly outperforming the median-voter model for the nineteenth century and for the model encompassing the 1st–101st Congresses. Only in the post-Reed period does the nonpartisan model (weakly) outperform the party model.

How well do the party and nonpartisan models fare in explaining the incidence of rule changes creating minority rights? In Table 2, I show the results of two non-nested models of the likelihood of minority rights creation. The parameters of interest in each model are statistically significant. In the party account (model 1a), slumps in partisan capacity lead to the adoption of new minority rights; in the median-voter model (1b), net movement of the chamber median toward the

TABLE 2
Non-nested Models Explaining the Creation of Minority Rights
(Logit Model Estimates)
(standard errors in parentheses)

| Theory | Variable | Model 1a PARTY (1789–1990) | Model 1b MEDIAN (1789–1990) |
|---|---|---|---|
| Party | Partisan capacity | –.101* (.055) | |
| | Change in party control | –1.072 (1.140) | |
| | Workload | .183 (.425) | |
| Median-voter | Change in median | | –9.839** (3.486) |
| | Constant | –1.354* (.608) | –3.346*** (.658) |
| | N | 95 | 95 |
| | LR $\chi^2$ | 4.50 | 12.79 |
| | Prob > $\chi^2$ | .212 | .000 |
| | BIC | –368.920 | –386.319 |

*Note:* N = 95, with the 1st, 19th, 30th, 46th, 91st, and 93d Congresses dropped (see notes to Table 1).
* $p < .05$, ** $p < .01$, *** $p < .001$ (one-tailed tests).

minority party makes adoption of rule changes limiting majority party control more likely.[17] According to the BIC statistics, the median-voter estimation is more robust than the party account.

How should these results be interpreted? In truth, it is quite difficult to assess the fit of the party and nonparty accounts in explaining the dynamics of rights creation. The difficulty lies in the expectations of the two accounts. Both perspectives identify cross-party coalitions as the engine of rights creation, and thus rights creation is a poor dynamic for distinguishing between the two accounts. They are, in short, observationally equivalent. In the party account, lower values of partisan capacity occur when the strength of the minority party increases relative to the strength of the majority party. As I have shown in detail elsewhere (1997), cross-party coalitions are most likely to emerge and

demand procedural concessions from the majority party when the parties are at near parity in strength. Schickler (2000, 270) advances a similar explanation, noting that when the median voter moves toward the minority party, "she will favor rules changes that limit the majority party's agenda control and that instead create more opportunities for the minority party and cross-party coalitions." Ideological affinity of the median voter for the positions of the minority party, in short, makes cross-party coalitions more likely. Again, the observational equivalence of the party and median-voter accounts in predicting rights creation makes a meaningful test of the fit of the two models unlikely in this context. To distinguish between the party and median-voter accounts, we need to focus on cases of minority rights suppression. These are the cases for which the two competing explanations are not observationally equivalent.

## Discussion

Perhaps the most striking finding is the time-bound character of the median-voter model. What might explain this limitation? As Schickler (2000, 276) notes, the ideological balance of power model assumes a single, primary ideological dimension. Lacking a single dimension, the median voter cannot be assured of shaping the chamber's choices without risk of cycling. There is some support in the historical record for inferring the lack of a single dimension across the pre-Reed period. Poole and Rosenthal (1997), for example, point out that the spatial model breaks down during two periods in the nineteenth century: between 1815 and 1825, and again in the early 1950s. More generally, the fit of the spatial model varies more in the nineteenth century than in the twentieth (Poole and Rosenthal 1997, 32). One might reasonably argue that there is a strong theoretical basis for the time-boundedness of the median-voter model, given the time limitations reflected in the pre-Reed analysis here.

Still, we need to more carefully assess the degree to which the necessary conditions for applying the ideological balance of power model hold over the period before 1867 (when Schickler begins his analysis). First, for the nonpartisan model to fit, there must have been only two primary parties competing along a single underlying dimension. Second, because the median-voter model argues that changes in the location of the floor median are critical, we need to know for how many adjoining Congresses two parties competed along a single dimension (that is, at both time $t$ and $t-1$). Of the 39 transitions between adjoining Congresses before 1867, 10 transitions lacked two parties or

proto-parties competing over a dominant single dimension—just over a quarter of the period.[18] Although the pre-1867 period might not provide a perfect proving ground for assessing the fit of the ideological power balance model, there remain ample periods of time over the nineteenth century for which we can test the fit of the nonpartisan account.

If conditions were largely (albeit not entirely) ripe for the ideological balance of power model to hold in the pre-Reed period, why do the results here lend such strong support to the party-based account in this period? Why might party interests drive the emergence of a party-majoritarian House in the nineteenth century and then recede in importance to the median voter in the twentieth? The answer likely lies in the character of the House over its first one hundred years. For much of that period, simple majorities were powerless to select favored rules, given chamber rules that required a supermajority vote when rule changes were proposed midstream during a Congress. In that institutional context, the preferences of the floor median were unlikely to have been pivotal in shaping the selection of rules, even if a single ideological dimension prevailed. A simple majority was able to put rule changes onto the floor agenda at any time only after a century of rule changes that culminated in the Reed rules in the 1890s (Cooper and Young 1989). Consequently, we should not expect to find empirical support for the median-voter account until the late nineteenth century, certainly by the time of the Reed Rules. This pattern is precisely what I find when I reestimate the party and floor median accounts in Table 1.

It is helpful here to think about Cox and McCubbins's (1997) argument regarding the need to distinguish theoretically between two types of procedural deck-stacking: one concerning the accumulated set of rules in place at any given time and a second involving marginal changes in those rules. As Cox and McCubbins observe, "It is possible that the majority party could lose several battles on the margin, but still benefit substantially from the rules as a whole" (1997, 1385). Cox and McCubbins crystallize the argument in their more-recent analysis of party government in the House (2005, 55): "Rule changes subsequent to 1894 [when the Reed rules were readopted] did not alter the fundamentals of the system that Reed established."

The empirical results here suggest that Cox and McCubbin's theory holds true over the course of House history, at least as it applies to the extension and retraction of minority party procedural rights. Development of the House's core regime—deck-stacking that gradually enhanced the power of the partisan majorities to set the chamber's agenda with little influence from the minority—appears to have taken place during the pre-Reed period. Most important, that deck-stacking

appears to have been driven by the interests of the majority party; the ideological balance of forces on the floor seems not to have been relevant in this period for the adoption of rules that advantaged the majority party, whether because of the prevailing rules and practices that determined the threshold for successful procedural change or because of the lack of a single ideological dimension.

Once such a set of pro-majority rules was put into place, subsequent majority parties could weather marginal changes in the rules that reallocated some powers to the minority party. Moreover, the post-Reed procedural regime ensured that a simple majority could control the agenda—ironically empowering the floor median to successfully pursue such changes (for example, by changing the discharge process to make it easier to circumvent majority party leaders). Still, even when pro-minority rule changes made it to the floor's agenda in the twentieth century, none of those rule changes restored the procedural advantages enjoyed by the minority party in the pre-Reed period. When liberal, cohesive majorities began to emerge in the late twentieth century, they inherited a core procedural regime that could be exploited by simple—but now partisan— majorities to influence the shape of the floor agenda and, presumably, control salient policy outcomes (see Binder 1997, 82; Cox and McCubbins 2005, chap. 4).

## Conclusion

No single model appears well suited to explaining the procedural development of the House. Although previous studies have shown the value of both partisan and nonpartisan accounts, the methodological approach applied here brings nuance to existing studies. Claims of the applicability of a party-based account across the history of the House need to be tempered: a median-voter model makes a significant contribution in explaining marginal changes in House rules over the course of the twentieth century. This ideological balance of power model seems to be time-bound, however. A party-based account better explains the development of majority party procedural rights over the House's first century.

Still, this study raises additional questions about the procedural development of the House. One key assumption among scholars is that the set of rules adopted over the course of the nineteenth century significantly advantaged subsequent partisan majorities: the core procedural regime of the House made party government a future possibility. If so, that scenario suggests that changes in House rules after the Reed rules revolution may represent marginal changes in the

rules, compared to the change of the previous century. Distinguishing between rule changes that grant significant agenda control to the majority party and those that marginally shift power without undoing the majority party's potential to control the agenda will be critical for fully explaining the dynamics of institutional change in the House.

More generally, considering all rule changes equally salient and structuring empirical tests accordingly ignores the possibility that different rule changes are of varying importance to the control of the House agenda. Moreover, the hard work of connecting procedural change to the types of resulting policy outcomes remains to be done.[19] With a firmer grasp on the incentives that drive procedural change over the course of House history, our reasonable next step is to determine the ability of winning coalitions to exploit new rules to their policy and political advantage. To be sure, determining which policy alternatives might have been chosen had the rules not been changed is a tricky endeavor. But a greater effort to connect procedural incentives and battles to policy alternatives and outcomes remains an important challenge for students of institutional choice and change, particularly scholars seeking to pinpoint the foundation and emergence of partisan rule in the House.

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## APPENDIX
## Choice of an Estimator

How should the likelihood of different types of procedural change be estimated? Schickler (2000) classifies each Congress according to the type (or absence) of rule changes affecting majority party control of the agenda. In doing so, he conceptualizes a hierarchy of procedural change. Congresses with anti-majority rule changes are coded as –1, Congresses with neutral or no rule changes are coded as 0, and Congresses with pro-majority rule changes are coded as +1. Because Schickler considers this a set of ordered categories, he appropriately models the likelihood of rule change via ordered logit. In this appendix, I review the central problem that arises in the use of ordered logit, and I suggest an alternative modeling strategy that may be more appropriate.

A key assumption in our use of ordered logit is that the explanatory variables have a consistent, uniform effect on the different levels of the dependent variable. (Thus, ordered-logit routines generate a single set of coefficients.) If the "proportional odds" assumption—also known as the parallel-regression assumption (Long and Freese

2003)—does not fit, then ordered logit is not the best estimator for exploring the impact of the explanatory variables. A generalized ordered logit would be preferable, as it relaxes the proportional-odds assumption and allows explanatory variables to have different effects on the likelihood of the different levels of the dependent variable (see Fu 2001).

To test the proportional-odds assumption, I estimated non-nested versions of the basic partisan and nonpartisan accounts, converting my (1997) minority rights creation and suppression data into a three-category dependent variable (–1 = rights created, 0 = no net change, +1 = rights suppressed). The Brant test provides the most direct method of testing the proportional-odds assumption (Long and Freese 2003), comparing slope coefficients of the binary logits implied by the ordered-logit estimation. As shown in Table A1, Brant tests allow me to reject the proportional-odds assumption for the ordered-logit estimation for both the party and median models.[20]

Since there are strong theoretical reasons for estimating separate models of creation and suppression of minority rights, the additional information that the parallel-regression assumption is violated leads me to suggest that the likelihood of rule changes affecting minority and majority power should be estimated in separate logit equations.

TABLE A1
Testing the Proportional-Odds Assumption of Ordered Logit
(standard errors in parentheses)

| Theory | Variable | Model 1a PARTY (1791–1990) | Model 1b MEDIAN (1789–1995) |
|---|---|---|---|
| Party | Partisan capacity | .069** | |
| | | (.027) | |
| | Partisan need | 2.008** | |
| | | (.725) | |
| Median-voter | Change in median | | 3.051* |
| | | | (1.395) |
| | Intercept 1 | –1.811 | –2.558 |
| | | (.461) | (.377) |
| | Intercept 2 | 2.394 | 1.290 |
| | | (.487) | (.244) |
| | N | 100 | 104 |
| | LR $\chi^2$ | 14.52 | 4.89 |
| | Prob > $\chi^2$ | .001 | .027 |
| | Brant test $\chi^2$ for violating variable | 5.02* | 4.22* |

*Note:* Party test begins in 1791 since partisan need is based on lagged variable.
*$p < .05$; **$p < .01$; ***$p < .001$ (one-tailed tests).

## NOTES

I appreciate the comments and suggestions of Brian Humes, Eric Lawrence, Forrest Maltzman, Eric Schickler, Steven Smith, and participants in the Congress and History workshop held at Columbia University (June 2002).

1. Work focused on House rules includes Cox and McCubbins 1997, Dion 1997, Fink 2000, and Schickler and Rich 1997. Senate rule changes are explored in Binder and Smith 1997 and Brady, Brody, and Epstein 1989, among others.

2. The debate continues over the relevance of collective electoral and party goals. See, for example, Aldrich and Rohde 1997–98, Cox and McCubbins 1997, and Smith and Gamm 2005.

3. One might object that the dependent variable should be conditioned on attempting to change the rules in the first place, assuming that majority parties have multiple strategies for combating minority parties. Here I assume that strategic legislators in pursuit of policy change understand the general odds against successful institutional change and thus do not seek to change the rules absent a strong chance of winning. Thus, models reported here (and elsewhere, e.g., Dion 1997 and Schickler 2000) estimate the conditions fostering successful change in the rules.

4. In his Appendix A, Schickler (2000) notes that he reran the analysis using my dichotomous variable for minority rights suppression as the dependent variable. He found no appreciable differences across the two estimations.

5. One prominent alternative offered by Schickler (2000) begins with the 40th Congress (1867). Starting with the 1st, rather than 40th, Congress allows me to capture the adoption of the previous-question motion in 1811, a change to the House suspension rule in 1822, the one-hour debate limit imposed on individual floor speeches in 1841, and further changes to the previous-question motion adopted in 1837 and 1880.

6. More specifically, Schickler calculates his measure as follows: Calculate the absolute distance between the minority party median and the floor median, and subtract the absolute distance between the majority party median and the floor median. Then assess the change in that variable from one Congress to the next.

7. I determined party size from Martis 1989, dropping all nonmajor party members. I used Rice cohesion scores to tap party cohesion, calculated over the set of roll-call votes generated each Congress. Rice scores capture the voting regularity of party members and, as such, do not expressly measure party members' preferences. Nevertheless, high cohesion scores are likely to reflect a strong degree of shared policy views. Rice scores may be influenced by the agenda and by party discipline, but alternative measures, such as Poole and Rosenthal's (1997) DW-NOMINATE scores, also incorporate, to some degree, party and agenda effects (see McCarty, Poole, and Rosenthal 2001). The results reported in Table 1 are robust to measures of party advantage that are not based on roll-call-voting patterns, such as a simple difference in the percentage of seats held by the majority and minority parties. The greater the majority's advantage is, the higher the likelihood that minority rights will be suppressed. (Results available from the author.)

8. Because Reed's rules shut down the use of repeated dilatory motions, the measure of adjournment motions cannot be replicated for Congresses after the 1890s.

9. I measured workload with a factor score for each Congress derived from factor analysis of the number of laws enacted each Congress, the number of days in

session, and the number of House members. For the post-Reed analysis, I dropped the number of House members, since it does not vary after reaching 435 in 1912. I measured expected change in party control as actual change in party control with each Congress. Measurement justification appears in Binder 1997, Appendix 2.

10. Over the time series from the 1st–100th Congresses, the Pearson's *r* correlation is .35. The correlation is quite low for the nineteenth century, potentially due to the breakdown in the unidimensional assumption at critical points in that century (on the fit of the single dimension, see Poole and Rosenthal 1997). The correlation is much higher in the twentieth century.

11. For example, the floor median moved closer to the minority party median after the elections of 1934, 1936, 1940, and 1956, all of which produced seat gains for the Democratic majority party. As noted by Wiseman and Wright (N.d., 13), the heterogeneity of the Democratic party during the heyday of the conservative coalition helps account for the weakened relationship between the position of the floor median and electoral change benefiting the majority party.

12. Weakliem (1999) challenges BIC.

13. I estimated the models with the *logit* routine in Stata 8.0. The minor differences between Schickler's parameter estimates and the ones reported here are likely attributable to different statistical software (Schickler estimates his models in SAS) and possibly to the source used to measure party size.

14. I generated and compared BIC statistics using Long and Freese's (2000) *fitstat* routine in Stata 8.0.

15. I checked each of the independent variables for evidence of a unit root, and I rejected the null hypothesis of a unit root for each of the variables except workload. Rerunning the analysis without the workload variable does not appreciably change the results, although it does strengthen the BIC comparison in favor of the party model. I rejected the null hypothesis of a unit root for the workload scores for the second period (model 2) and the entire series (model 3).

16. The $\chi^2$ is actually better for the party model than the median model, in contrast to the conclusion suggested by BIC comparison. This anomaly probably results from the fewer parameters in the median model.

17. Note that the skewedness of the dependent variable (seven Congresses with rights creation compared to 87 without) may affect the robustness of the logit estimation.

18. I arrived at the count of 10 poorly fitting congressional transitions in the following way. First, I used Martis 1989 to determine the Congresses in which the two major parties were challenged by a third, or more, organized coalition(s). The 18th and 34th Congresses most clearly fit this description, meaning that four congressional transitions (17th–18th, 18th–19th, 33d–34th, and 34th–35th) lack the characteristics necessary for the median-voter account to hold. Second, I used Poole and Rosenthal 1997 to determine the Congresses in which there was not a strong first dimension. Poole and Rosenthal (1997, 71) identify four Congresses in particular for which the geometric mean probabilities fall below .6: the 14th, 15th, 17th, and 32d Congresses. Thus, we lose six additional congressional transitions (13th–14th, 14th–15th, 15th–16th, 31st–32d, and 32d–33d Congresses); I had already counted the 17th–18th Congress transition.

19. Cox and McCubbins offer a roll-call-based approach to modeling the direction of policy change (2005, chaps. 4, 9).

20. In the median model, we can reject with confidence that the coefficients for the change-in-median variable across the two binary regressions are alike; in the party model, we can reject with confidence that the coefficients for partisan need across the two binary regressions are alike. Substituting Schickler's (2000) three-category dependent variable yields comparable results for the partisan model (again rejecting the proportional-odds assumption for the partisan need variable) but acceptable results for the median model ($p = .158$ for Brant test statistic).

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