Majority Status and Variation in Informational Organization

David Fortunato University of California, Merced

To maintain its policy cartel, the majority party requires information to recognize good policy and sufficient votes to realize it. There is ample research demonstrating the majority's ability to mobilize votes, but no empirical research examining its ability to facilitate efficient information transmission. Here, I investigate differences between the majority and minority in maintaining an informational organization (representative committee delegations that share their information with copartisans) and examine the effects of this organization on expressed legislative uncertainty. By analyzing comprehensive new data, I find that the majority is most often able to maintain representative committee contingents, but the minority is not. Further, the majority is able to induce its nonrepresentative delegations to be informative; however, certain types of outlying contingents significantly increase the minority's policy uncertainty. Finally, I conclude that the majority's informational organization is superior to that of the minority and speculate that this may have electoral implications.

n recent years, the partisan model of legislative organization has become the dominant framework through which scholars analyze the legislatures of the United States.¹ This framework has been applied to the House (e.g., Cox and McCubbins 2005) and Senate (e.g., Snyder and Groseclose 2000), as well as U.S. state chambers (e.g., Aldrich and Battista 2002), and even abroad (e.g., Jones and Hwang 2005).² These studies provide evidence that the majority party (or coalition) is able to maintain a monopoly over the policymaking process by controlling the outcome of legislative voting, either through negative agenda control or party discipline. Controlling votes, however, is insufficient to maintain this monopoly; the majority party also requires significant amounts of information to distinguish between harmful and beneficial policies. While we have substantial theoretical research demonstrating what motivates individual legislators to gather this information (e.g., Bendor and Meirowitz 2004; Gilligan and Krehbiel 1990), we have almost no empirical research investigating differences between

parties in their ability to provide policy information to their members.

This gap in the literature is puzzling given that the study of the other side of cartel maintenancevoting discipline-has already moved from whether discipline is maintained, to how discipline is maintained (Carroll and Kim 2010; Jenkins and Monroe 2012). In this article, I attempt to fill this gap by investigating the degree to which the majority and minority vary in their ability to maintain informative committees. To do this, I review the canonical model of informational organization and discuss its place within the partisan framework. I identify three testable hypotheses from this discussion: one regarding the composition of committees, one regarding their propensity to exacerbate policy uncertainty, and one regarding the general level of policy uncertainty displayed by representatives. Specifically, I argue that the majority party, as a function of its resource advantages, is more able than the minority to maintain representative committee delegations, a hallmark of informational organization.

The Journal of Politics, Vol. 75, No. 4, October 2013, Pp. 937–952 © Southern Political Science Association, 2013 doi:10.1017/S0022381613000935 ISSN 0022-3816

¹Support for this research was provided by the collaborative research center SFB 884 on the Political Economy of Reforms at the University of Mannheim (http://reforms.uni-mannheim.de), funded by the German Research Foundation (DFG). An online appendix for this article is available at www.journals.Cambridge.org/jop containing supplemental materials. Data and replication materials may be found at www.davidfortunato.com.

²This framework includes, but is not limited to, cartel theory (Cox and McCubbins 1993, 2005) and conditional party government (Aldrich and Rohde 2001). Cartel theory will be the focus here.

Further, I argue that when a representative committee contingent can not be formed, the majority is able to induce the outlying contingent to be informative, but the minority is not and, as a function of their higher quality organization, the majority will display less policy uncertainty than the minority. I motivate a measurement of expressed policy uncertainty (roll-call classification errors) and test my hypotheses with original data composed of over 92,000 jurisdiction-specific voting behaviors for 14 jurisdictions for the 84th through the 108th Houses. I present strong evidence that the majority party maintains an "informational organization"—i.e., representative committees that share their private information—that is beyond the capability of the minority party.

Informational Organization and Party Government

Since Shepsle (1979), legislative scholars have come to agree that the organization of committee systems and chamber rules is critical in determining the stability and content of policy. In the past 30 years, three models of legislative organization, each rooted in committee structure and purpose, have come to dominate American legislative scholarship. The distributive model (Weingast and Marshall 1988) holds that members self-select onto committees in order to secure policies that will aid reelection. The informational model (Gilligan and Krehbiel 1990) holds that committees are organized to provide the chamber with policy expertise. And, finally, the party government model (Cox and McCubbins 1993, 2005) maintains that committees are tools of the majority, used to prevent the passage of policies that may be harmful to the majority's brand and encourage the passage of policies that are beneficial to its brand.³ As noted above, the partisan model has come to dominate the literature.

To simplify the theoretical argument, prediction, and testing, this article takes a reduced view of distributive and informational models. Here, in nonpartisan terms for simplicity, it is assumed that in a House that is perfectly distributive in its organization, committees are composed of so-called "high demanders," representatives that require a certain type of policy to ensure reelection. By definition, these high demanders have policy interests that are significantly dissimilar (in either content, salience, or both) from the House as a whole, thus frequently resulting in outlying committees. Conversely, a House that is perfectly informational in its organization will have committees that are organized by the floor in order to provide it with expertise to limit its uncertainty over policy outcomes. The benefits of this organization fall to zero as the preferences of the floor and committee grow sufficiently dissimilar, thus outlying committees will be quite rare. Revisiting the informational model in more detail will help to flesh out these differences.

Gilligan and Krehbiel (1990) model a floor principal as risk averse over outcome uncertainty where the outcome realization x is a function of policy p and some random shock w. Committees organize to utilize expertise such that they may learn the realization of w and transmit this information to the floor to limit its utility loss in outcomes. An important implication of their model is that, because it is risk averse, the floor, "… always strictly prefers a *certain* outcome in the neighborhood of x to a lottery whose *expected* outcome is x" (1990, 537; authors' emphasis).⁴

This implies that as the variance of the shock term w increases, the floor will, to a degree, accept policies generating outcomes increasingly distant from its ideal point. More specifically, as Gilligan and Krehbiel demonstrate, when preference dissimilarity between the committee and the floor reaches a critical point $(x_c \ge 3\sigma_w^2)$, where x_c is the dissimilarity of the committee's ideal point from that of the floor, and σ_w^2 is the variance of the distribution from which the random shock w is drawn), the floor no longer tolerates the policy drift of the committee and terminates the delegation. In such instances, the floor receives no informational benefits from the committee's expertise, and committees derive no policy benefit from their specialization. However, within the range of dissimilarity $x_c < 3\sigma_w^2$ where the certain, somewhat dissatisfactory, outcomes delivered by the committee are preferable to the risk of some other, more dissatisfactory, outcomes, net gains from specialization are still positive for both entities. Because of this, Gilligan and Krehbiel predict that committees will be representative of the floor and that outliers will be rare.

While Gilligan and Krehbiel (1990) have designed a nonpartisan framework for their theory, the model

³See Maltzman (1995, 1997) for a rich discussion of floor and party principal models, as well as distributive or "independent committees" models of organization.

⁴It is fair to note that Bendor and Meirowitz (2004) demonstrate that risk aversion on the part of the principal is not required to favor delegation as uncertainty grows, merely that uncertainty grows in such a manner as to increase the distribution of possible outcomes away from the ideal point of the principal. Still, given that Gilligan and Krehbiel (1990) is the "industry standard," I continue in their language.

can be adapted to fit the party government framework if we replace the floor and the committee as a whole with the party and its committee contingent as principal and agent. Here, the signal from agent to principal may not be the policy itself (as in the original model), but the party contingent's evaluation of the policy. Indeed, this adaptation is, to a large degree, necessary for party government as understood in Cox and McCubbins (2005). As noted above, maintaining the policy cartel requires substantial expertise. Majority committee contingents must understand the electoral implications of policies and how they vary across the 435 House districts. Thus, party government requires committee contingents to not only accumulate substantial amounts of information but also to disseminate this information to each of their individual copartisans. In protecting the electoral prospects of individual party members, committee contingents protect the seat share of the party as a whole. However, the policies that are best for the party as a whole may not be the most preferable outcomes for the committee contingent. Thus, when committee contingent preferences grow sufficiently dissimilar from those of the party, the contingent becomes unwilling to subvert its own interests for those of the party and the net gains from this arrangement fall to nil (just as in Gilligan and Krehbiel). Because of this, Cox and McCubbins suggest that the majority will prefer committee contingents whose preferences are representative of the party as a whole and must supply members with some resource to make subversion of their private interests worthwhile.

The resource that makes party government possible, according to Cox and McCubbins (2005), is the party brand name. With this valuable, nonpolicy resource, Cox and McCubbins make a delegation model, similar to Gilligan and Krehbiel's model, more tractable by introducing something akin to a side payment. Recent scholarship has taken and tested Cox and McCubbins' admission that the brand name is necessary but insufficient to maintain the policy cartel. Carroll and Kim (2010) demonstrate empirically that the majority rewards its "policy losers" for continued support with targeted spending.⁵ Jenkins and Monroe (2012) find a similar relationship between policy loss and PAC contributions from party leadership to the policy losers. The theoretical work by Cox and McCubbins and empirical supplement by Carroll and Kim (2010) and Jenkins and Monroe (2012) provide evidence that

the majority utilizes nonpolicy side payments to maintain the cartel. This suggests that the majority is significantly advantaged by its status in inducing preferred behaviors such as disciplined voting, representative committee delegations, and the dissemination of information.

Inducing Preferred Behaviors

Both Gilligan and Krehbiel (1990) and Cox and McCubbins (1993) argue that a committee contingent's efficiency is a function of how representative it is of its (principal's) party's preferences. The more dissimilar the preferences of the party and its contingent are, the less benefit both entities extract from the contingent's expertise. Recent empirical work suggests that significant dissimilarity between these entities may be commonplace (e.g., Carroll and Kim 2010 and Carson et al. 2010 who show that there is substantial variation within parties in terms of policy preferences and loyalty, respectively).

Assume that there are several policy jurisdictions in which the preferences of the party and its committee contingent are substantially dissimilar. In Gilligan and Krehbiel's model, where payoffs to committee members are strictly a function of policy, neither the party nor the committee contingent benefit from the contingent's expertise. How then, is the majority to formulate its agenda? How are members to know how to vote? Under the parameters of the original model, where committee utility is purely a function of outcomes, the majority is at a loss. Cox and McCubbins (1993, 2005) suggest that this dilemma can be alleviated with a side payment of a valuable, nonpolicy goodbrand name in the original model, pork and PAC contributions in the empirical extensions of Carroll and Kim (2010) and and Jenkins and Monroe (2012).

What if these side payments may also be used to control committee composition—persuade majority members to take a less preferred committee assignment in a jurisdiction where they are more representative of the party as a whole—and informativeness—persuade outlying committee contingents to share their private information when a representative committee cannot be formed? Under these conditions, we may still observe some outlying committee contingents, however, in contrast to the original model, these outliers would *continue to be informative*.

In terms of inducing preferred behaviors, the minority is at a significant disadvantage. We know that the majority possesses more agenda control

⁵Carroll and Kim define "policy losers" as legislators that continue to support the majority despite frequently failing "to obtain their preferred policy outcomes" (2010, 34).

(e.g., Cox and McCubbins 2005), more campaign resources (e.g., Cox and Magar 1999), and greater control over directed government spending (Balla et al. 2002). Aldrich and Rohde (e.g., 2000a, 2000b) argue extensively that the majority is better able to influence its membership's voting, citing its institutional and resource advantages. Further, considering the odds stacked against them, the minority would rarely, if ever, attempt to influence the voting of its members, and as a rational party,

"... would not try to induce switches if they believed their side would win without them, nor would they do so if they believed they would lose even with the votes they could plausibly pick up." Aldrich and Rohde (2000a, 37)

The theoretical arguments imply that while majority members feel consistent pressure from their party, minority members may be considered free agents, often able to join either voting coalition where it behooves them individually. This is supported empirically by Aldrich and Rohde (2000a) and Roberts and Smith (2003) who present evidence that the minority demonstrates substantially less voting unity on key votes. I believe that the factors that influence the ability of parties to maintain voting unity (e.g., greater access to side-payment resources) should also affect their ability to maintain an informational organization, and we should observe differences between the majority and minority accordingly.

Hypotheses

The first implication of the discussion above concerns committee composition. As a committee's efficiency is a function of its representativeness, parties will prefer more representative contingents. The majority has more resources to induce a representative membership; thus, the expectation is a prevalence hypothesis that outlying contingents will be more common within the minority.

H1: Outlying contingents are more common among the minority than the majority.

The second implication is that, where outlying contingents are realized, the majority will be better able to induce them to reveal their information to their copartisans in order that they may vote correctly, leading to an informative hypothesis.

H2: Outlying majority contingents are more informative than their minority counterparts.

A third implication is that, because the majority is better able to maintain an informational organization, majority representatives should be better informed than their minority counterparts. This yields the status hypothesis.

H3: Majority representatives are better informed than their minority counterparts.

Measurement

In order to test these hypotheses, it is necessary to first identify outlying committee contingents. This is not an easy task. Several scholars have addressed the question of committee representativeness, and there is no shortage of disagreement amongst reported results. Sprague (2008), who provides a nice summary of the scholarship, suggests that this lack of consensus may be due to differences in methodology. She points out that scholars have found many outliers considering constituency characteristics (e.g., Adler and Lapinski 1997) but few outliers when considering voting behavior (e.g., Groseclose 1994). This incongruence may be the product of many factors, the two most likely being: (1) legislators identified as outliers according to the characteristics of their constituency do not vote differently from their peers, and (2) the voting behaviors we have considered are inappropriate, a notion most notably raised by Maltzman (1995).

I agree with Maltzman's (1995) concern that our roll-call-derived measurements have been inappropriate. Traditionally, scholars have used interest group scores, which rely on a few selected roll-call votes, or considered behavior on all votes by utilizing NOMINATE scores (Poole and Rosenthal 1985). Snyder (1992) provides a detailed rationale for the inappropriateness of interest group ratings for the analysis of committee composition. Specifically, he suggests that these groups have a tendency to select votes that result in artificially extreme distributions of legislator behaviors and identifies three possible problems that may arise because of this vote selection: (1) distributions may have enlarged tails; (2) the percentage of legislators between floor and committee medians may be artificially reduced; and (3) the differences between committee and floor legislators may be exaggerated. Snyder suggests that the first two issues bias against finding statistically significant differences between committee contingents and their parent parties, while the third would lead to an increased propensity for identifying false positives. This argument comes after the warning by Hall and Grofman (1990) that the actual votes selected by interest groups are often ill-suited for jurisdiction-specific testing.

NOMINATE scores (derived from all votes) were notably utilized by Cox and McCubbins (1993) and Kiewiet and McCubbins (1991). Unfortunately, these metrics are no more appropriate than interest group ratings for assessing the prevalence of outliers. Maltzman warns that,

"While NOMINATE ratings solve the problem of 'artificial extremism' by relying upon every vote, the use of every vote makes these data even more inappropriate for testing jurisdiction-specific hypotheses than are ratings from interest groups with a limited policy focus." (660, 1995)

This is logical. If we are interested in assessing the behavior of the Committee on Agriculture, why would we allow our inferences to be biased with votes on Foreign Policy legislation? Maltzman (1995) attempts to rectify this error by using only roll-call votes from the jurisdiction of interest, and I build on his contribution here. With the benefit of hindsight, we can identify three opportunities from Maltzman's approach to improve the quality of the data.

The data employed by Maltzman (1995) aggregate the roll-call votes of four congressional sessions (94th, 96th, 98th, and 100th) and produce ideal points for the 130 legislators serving in all four periods. This selection assumes that the most senior and safe representatives of the 100th House are a representative sample of all representatives in the 94th to 100th Houses. This is not the case. Indeed, Carson et al. (2010) suggest that the safest representatives should display the most partisan voting behaviors. Thus, I scale each jurisdiction of each session separately. Second, bills are selected on the criterion of having an amendment vote that was supported by less than 90% of the majority. While this is defensible for analysis of the majority alone, it excludes possibly meaningful data for analyzing minority and floor-wide behaviors. Third, classic nonparametric scaling is employed. This methodology discards important information. Specifically, the approach assumes that all votes are equally salient and therefore equally informative to the final ideal point estimates rather than modeling any variation across the roll calls. Here, I incorporate all votes for each jurisdiction into a parametric scaling model to address the final two points.6,7

My data begin with every roll call recorded in a given House session; the sample consists of the 84th through the 108th Congresses.⁸ These votes are sorted into jurisdictions representing 14 standing committees using Rohde's (2004) roll-call issue coding.9 Each jurisdiction is then scaled individually using W-NOMINATE (Poole 2005).¹⁰ All told, this procedure produces over 92,000 ideal point estimates for over 2,600 representatives. Committee members are identified using membership data collected by Nelson (2005) and Stewart and Woon (2011). As I will show below graphically, the distribution of jurisdictionspecific behaviors may, at times, look very similar or very different from the distribution of general behaviors gleaned from all votes or interest group ratings. However, the more nuanced information contained within those distributions is nearly always quite different. It should be noted that although I may refer to these behavioral estimates as "ideal points" (the preferred term in the literature), it is assumed that these estimates are a function of representative preferences, party pressures, and the informational environment and not estimates of true preferences.

Comparison of Jurisdiction-Specific and General Behaviors

In order to demonstrate the difference between jurisdiction-specific estimates and estimates generated from the universe of available roll calls, I perform a simple test. For each congress in my sample, I scale all roll calls to derive estimates of *general legislative*

⁶See Poole (2005) for a more detailed discussion of parametric and nonparametric scaling techniques.

⁷It is important to note the ideal points recovered will not be directly compared across jurisdictions or sessions; as such, it is not necessary to estimate a dynamic scaling model, i.e., DW-NOMINATE (Poole 2005), to establish a common scale across all jurisdictions and sessions.

⁸All roll calls for all Houses were collected by Poole (2013), Poole and McCarty (2013) and made available through Poole's Voteview website.

⁹These committees are Agriculture; Appropriations; Armed Services; Banking, Finance, and Urban Affairs; Budget; Education and Labor; Energy and Commerce; Foreign Affairs; Government Operations; Judiciary; Natural Resources; Public Works and Transportation; Science, Space, and Technology; and Ways and Means. Details on how Rohde's (2004) issue classifications are linked to specific committees can be found in the appendix.

¹⁰The roll calls were scaled in a single dimension using the chamber's most liberal northern legislator (as identified by Poole's (2013) first-dimension DW-NOMINATE estimations on Voteview) as the polarity setting for Democratic majority Houses and the most conservative southern legislator for Republican Houses (should this legislator fail to meet minimum vote requirements, the closest representative was tapped as replacement). Only jurisdictions with at least 20 votes were utilized for the primary analysis. However, for the coming median identification exercise, this threshold was increased to 40 in order to reduce convergence errors in the bootstrapping. Further, for the coming replication of Groseclose (1994), I reduced the vote minimum to 15 in order to compare as many jurisdictions as possible. More information on specification is available upon request.

behavior. I then identify four individuals of interest for each jurisdiction: the majority floor and committee medians and the minority floor and committee medians. Next, I scale each jurisdiction individually, deriving jurisdiction-specific ideal points, and assess the probability that the general estimates identify the same individuals of interest as the jurisdiction-specific estimates. In more substantive terms, for the case of Appropriations in the 99th House, this exercise estimates the probability that Representative Dante Fascell (Florida's 19th District), the legislator identified as the majority floor median by *all votes*, will also be identified as the majority floor median by *only Appropriations votes*.

This probability is estimated via parametric bootstrap following Lewis and Poole (2004). In short, using the initial estimates of the roll-call parameters and ideal points, we can specify a joint distribution from which we can draw simulated roll-call matrices. These simulated matrices are then scaled, and the process is repeated. For each iteration of the jurisdiction-specific bootstrap (of which there are 1,001), I record the individuals of interest.¹¹ The final probability of the two samples identifying the same individuals of interest is given simply by the number of identical identifications over the number of trials. For instance, for the substantive example above, the probability that Representative Dante Fascell (D, FL. 19), the majority floor median according to all roll calls, is the majority floor median for Appropriations voting is just 0.01.

This exercise reveals that the probability of all votes identifying the same individual of interest as a jurisdiction-specific subset of votes is quite low. It included 152 session jurisdictions across the 25 Congresses. Of the 608 medians evaluated, there were only two for which the probability of the total roll-call matrix recovering the same median as the jurisdiction-specific matrix was greater than 0.03. Indeed, the mean probability for each individual was less than 0.01.¹²

In reference to jurisdiction-specific behavior, this exercise provides evidence that past examinations of committee composition that have utilized behavioral estimates from all roll-call votes may not be valid. Quite simply, these analyses have been comparing different sets of individuals than they are theoretically interested in. Most often, a group median identified by all votes will not be the same individual that is identified by jurisdiction-specific votes.

Comparison to Interest Group Ratings

In 1994, Groseclose presented perhaps the definitive examination of committee composition utilizing interest group scores for the 99th House. He tests hypotheses derived from all three organizational models but finds little or no evidence to support any of them. He concludes, in fact, that his analyses imply that committee selection appears to be random. While the empirical tests were rigorous, it seems unlikely that the substantive conclusions presented could be valid. Indeed, these results are at odds with the overwhelming majority of anecdotal, qualitative, and empirical evidence we have regarding the manner in which committee seats are awarded and all of our theoretical expectations of committee behavior. Groseclose reinforces this skepticism when he remarks that committee assignment institutions indicate that seat allocation is "anything but random" (1994, 456).

Is it possible that these null results are a product of interest group scores being poor measures of jurisdiction-specific behaviors? To test this, I replicate a portion of Groseclose's (1994) analysis on the 99th House. Using my estimates described above, I retest Groseclose's first hypothesis: that committees are composed of outliers.¹³

I scale the individual jurisdictions of the 99th House for which my data and Groseclose's (1994) overlap: seven jurisdictions in all. After recovering the jurisdiction-specific estimates and identifying the committee and floor medians, I execute the Monte Carlo procedure described by Groseclose in order to estimate the probability that the committee is an outlier. Using the distribution of ideal points yielded by the scaling procedure, I construct a randomized committee by sampling without replacement from all

¹¹In cases where more than one legislator was identified as the median, I erred on the side of overestimating joint identification by recording all legislators and scoring a match where any legislator from the all votes group was also identified in the jurisdiction-specific group.

¹²The results of this exercise are presented in greater detail in the online appendix including statistics on how frequently the proper median would be identified by chance alone. In reference to the committee median, selecting individuals at random outperforms using general estimates from all votes in all but two cases.

¹³Here, I consider the committee as a whole to replicate Groseclose (1994). In the rest of the article, analysis will concentrate on the committee delegations of individual parties.

members of the 99th House.¹⁴ The median of the randomized committee is then identified and her ideal point is recorded. This process is completed 20,000 times in order to construct a distribution of committee medians. The probability that a given committee is an outlier is equivalent to the proportion of the simulated medians that are less extreme than the true median. The null hypothesis (that committees are not composed of preference outliers) is rejected if the true committee median is more extreme than at least 95% of the simulated medians. The results for this test are reported in Table 1 alongside the probabilities reported by Groseclose.¹⁵

The difference between the probabilities reported by Groseclose (1994) and the probabilities recovered here is pronounced. While the results for Appropriations, Armed Services, and Public Works voting are similar, the results for Agriculture, Budget, Education, and Foreign Affairs voting are quite different. Two of Groseclose's outliers—Education and Foreign Affairs—appear to be well-aligned with the floor (an outlier probability of 0.5 indicates the committee is perfectly aligned with the floor), while two of Groseclose's representative committees—Agriculture and Budget—appear to be outliers in this test. These findings seem to mesh better with our expectations for committee behavior than the results Groseclose reports.¹⁶

In addition to providing substantively different findings, these new data also stand up to Groseclose's (1994) secondary test better. Groseclose is careful to consider the possibility that, while he may find

	Pating	Recovered Probabilities				
Committee	Group*	Groseclose p	Current p			
Aging	NCSC	0.393	_			
Agriculture	NFU	0.285	0.000			
Appropriations	CCU.S.	0.020	0.000			
Armed Services	ASC	0.000	0.000			
Budget	BIPAC	0.431	0.001			
Education	COPE	0.077	0.237			
Foreign Affairs	ASC	0.068	0.411			
Interior	CCU.S.	0.177	_			
Public Works	PCCW	0.394	0.337			
Small Business	BIPAC	0.503	-			

Note: This table compares the results of Groseclose's (1994) first hypothesis test on the 99th House (that committees are composed of preference outliers) to the results of a replication of his analysis using the new data I have generated here.

*Rating group is relevant for Groseclose (1994) probabilities only.

statistically significant probabilities for some committees, the number of significant probabilities recovered may be insignificant. In his examination of 21 interest group ratings, he found two statistically significant outliers; yet, using traditional levels of significance, we would expect to find about one outlier by chance alone. To test that the number of outliers was significant, he generated a random committee membership for each rating and compared their medians to the distributions of randomized medians from the earlier analysis. He then recorded the number of outlier committees realized. This process was repeated 20,000 times in order to establish an index of false positive propensities. He then reported the number of outliers we would expect by chance alone, the number of outliers actually discovered, and the proportion of times at least this many outliers were recovered in the second Monte Carlo trials.¹⁷ The number of outliers found for these seven jurisdictions is quite significant. Indeed, the probability of finding four outliers by chance alone is approximately one in five thousand.¹⁸

The ideal point estimates scaled from all jurisdiction-specific votes are, according to Groseclose's (1994) criteria, more likely to reveal highly probable outlier committees, and these revelations are less likely to be the product of chance. The differences between

TABLE 1	Replication of Groseclose (1994)
	Junsdiction-Specific Estimates

¹⁴My approach varies from Groseclose's (1994) in one regard: while his randomized committees are truly random, my randomized committees honor the true partisan balance of each committee. If a given committee was truly composed of 25 Democrats and 20 Republicans, my randomized committee would be composed of 25 randomly selected Democrats and 20 randomly selected Republicans, whereas Groseclose's randomized committee would be composed of 45 randomly selected legislators. This decision was made to account for partisan stacking and bias results in favor of the null hypothesis, that committees are not composed of preferences outliers.

¹⁵Groseclose (1994) presents analysis of multiple interest group ratings for multiple committees. In the table, I present the results of analysis for only one interest group for each committee, choosing the rating that minimized the difference between Groseclose's findings and my own.

¹⁶It should be noted that, although there is no relationship between the number of votes scaled and the probability of finding an outlying committee in my analysis, my estimates were based on an average of 65 votes, while the interest group ratings (at least those for which I could find detailed documentation) are based on a range of 5–20 votes. Given Snyder's (1992) concern for the tendency for advocacy groups to cherry-pick votes such that polarization is increased, this difference is not insignificant.

¹⁷See discussion in Groseclose (1994, 447–48).

¹⁸A table comparing the expected number of outliers to the actual recovered outliers across several statistical criteria can be found in the online appendix.



FIGURE 1 Comparing Interest Group Scores

Note: Comparing interest group (dotted line), jurisdiction-specidic W-NOMINATE (solid line), and general W-NOMINATE estimates (shaded region). All estimates are mean centered and rescaled to have a standard deviation of 1 to facilitate visual comparison.

the results recovered from the interest group ratings and the results recovered from the more inclusive scaling procedure suggest that Snyder's (1992) concerns about using interest group ratings to assess committee composition were accurate. Figure 1 shows the distributions of preference estimates for two jurisdictions of the 99th House: Armed Services and Appropriations. In the figure, the distribution of the jurisdiction-specific W-NOMINATE scores (solid line) and the distribution of interest group scores (dotted line) are set against a shaded backdrop representing the distribution of W-NOMINATE scores recovered by scaling all roll calls in the 99th House.

Comparing the interest group ratings to the W-NOMINATE estimations shows that the selection bias inherent in the interest group rating process creates an artificially polarized, artificially bi-modal distribution of preference estimates, just as Snyder (1992) predicts. The figure shows that, at times, general W-NOMINATE estimates produce a distribution similar to the jurisdiction-specific estimates as in the Armed Services example in the left-hand pane. The graphic also shows that the match can sometimes be quite poor as the right-hand pane of Figure 1 shows. While the Groseclose replication examines only seven jurisdictions in a single House, the results gleaned from the different estimations are sufficiently different, and sufficiently in line with the Snyder's (1992) theoretical predictions, that they call into question the veracity of studies using interest group ratings as proxies for legislator behaviors just as the previous exercise calls into question the veracity of studies using estimates from the universe of roll calls.

Hypothesis Testing

To test my hypotheses, I extend the analysis executed on the seven committees of the 99th House to 14 standing committees in each House from the 84th through the 108th Congresses, collecting outlier probabilities for all committees and majority and minority committee contingents in that period. Of the 350 session jurisdictions over that period, 213 met the minimum vote criterion of at least 20 votes. Applying Groseclose's (1994) criterion for outlier identification, I find that the majority of committees, 59%, were, in fact, outliers in reference to the floor. A full 70% of minority committee contingents are classified as outliers along with 15% of majority committee contingents.¹⁹ All told, the minority has nearly five times the number of outliers as the majority. This very strong support for the prevalence hypothesis (H1) is summarized in Table 2.

Comparing these results to past examinations exposes pronounced differences in the distribution of outlying committees and committee contingents. For example, Maltzman (1995, 1997) finds that over half of the majority's committee contingents in his sample are significant outliers. I find that only the majority's contingent on the Committee on Armed Services is an outlier at least half the time. Further, in reference to the floor, I find substantially more outlying committees than the vast majority of previous studies.

¹⁹Tables detailing the presence and directionality of outliers across parties, jurisdictions, and sessions are available in the online appendix.

	Jurisdiction*														
	Agr	Арр	Arm	Ban	Bud	Edu	Ene	For	Gov	Jud	Nat	Pub	Sci	Way	Total
Total Scaled	9	24	18	12	15	18	19	20	16	19	9	7	4	23	213
							Prope	ortion (Dutliers						
Chamber	0.78	0.75	0.67	0.33	0.47	0.44	0.95	0.55	0.56	0.47	0.67	0.14	0.00	0.61	0.59
Majority	0.44	0.12	0.50	0.08	0.00	0.11	0.05	0.15	0.12	0.21	0.11	0.14	0.00	0.00	0.15
Minority	0.78	0.75	0.72	0.58	0.73	0.67	0.95	0.90	0.69	0.63	0.78	0.00	0.00	0.65	0.70

 TABLE 2 Proportion of Outlying Committee Contingents by Jurisdiction

Note: Groseclose's (1994) outlier criterion applied to each jurisdiction in each House for the 84th through the 108th Congresses for which there are enough votes to scale. The figures reported are number of outliers identified for each jurisdiction over the number of times that jurisdiction was scaled.

*Agr: Agriculture; App: Appropriations; Arm: Armed Services; Ban: Banking, Finance, and Urban Affairs; Bud: Budget; Edu: Education and Labor; Ene: Energy and Commerce; For: Foreign Affairs; Gov: Government Operations; Jud: Judicial Affairs; Nat: Natural Resources; Pub: Public Works and Transportation; Sci: Science, Space, and Technology; Way: Ways and Means.

I attribute these differences to the improved measures I utilize here.

Are Outliers Informative?

As discussed above, parties prefer representative committees as representativeness engenders informational efficiency. When a party is unable to organize a representative contingent, it may attempt to induce the outlying contingent to continue to disseminate its information to its copartisans, subverting its own policy interests for those of its party. The majority has substantial resource advantages as a function of its status, thus the expectation is that when outliers are realized, majority outliers will not exacerbate policy uncertainty, while minority outliers will. To test the informative (H2) and status (H3) hypotheses, a measure of *expressed legislative uncertainty* is needed.

Recently, Richman (2008) and Yoshinaka and Grose (2011) have employed roll-call classification errors as measures of voting uncertainty. Roll-call classification errors represent the inability of the scaling model to correctly "predict" the legislator's vote as a function of her ideal point and the estimated parameter of the roll call in question. These classification errors, according to Brady and Rohde, are a product of characteristics of "the time period, individual members, or the roll call being predicted" (2007, 1). Richman (2009) writes that classification errors are "a function of the information environment. When legislators are less informed, they should make more errors" (2009, 331). He further suggests that "such errors reflect legislators' uncertainty about the consequences of their actions" (331). As I argue above that committee contingents must transmit information relating how bills will translate into policy and what the electoral implications of these policies

will be, roll-call classification errors appear to be a nearly ideal proxy for expressed uncertainty. I employ these errors as my dependent variable, but I first discuss them in more substantive terms and address difficulties analyzing them.

Substantively, we could conceptualize roll-call classification errors as representing a temporary disruption to the typical voting coalitions we expect given a particular policy. That is, some legislator votes "yea" when her previous behaviors and the behaviors of her neighbors predict that she will vote "nay." The natural response of most probabilistic vote-scaling models (like W-NOMINATE) is to rearrange the legislators in the voting space to correct such disruptions. Thus, classification errors are only realized in instances where the voting coalition that is broken is so consistent that rearranging it to correct the break would reduce the overall model fit for all legislators or if there is no alternate rank ordering that can be established to correct the error.²⁰

A legislator's error rate may not be considered in a vacuum. There is another component that needs to be considered in tandem with error rates when utilizing them as the dependent variable—the dimensionality of the data. Dimensionality must be brought in for both theoretical and empirical reasons. Voting errors may occur for two reasons: The first is legislator uncertainty, the concept I am trying to capture here; legislators vote incorrectly when they misunderstand the implications of their decision. The second reason for an error is dimensionality. Here, the erring legislator is simply casting her vote on some dimension orthogonal to that being considered by the majority of

²⁰This explanation is expanded upon in the online appendix with the help of an illustration from Poole (2005).

her peers. Where voting data are scaled in a single dimension, the proportion of higher dimensional voting errors is a function of the variation not explained on this first dimension. Thus, accounting for the variation explained on the first dimension helps isolate the portion of voting errors I am substantively interested in, those driven by uncertainty, from the voting errors I am not interested in, those driven by dimensionality.

As error rates are reflective of the scaling model's ability to correctly classify all votes in an allocated number of dimensions-most often one or twoholding the scaling model's number of allocated dimensions constant, as the "natural" dimensionality of the data increases so will the number of classification errors as a function of the model's fitting algorithm. Here, as I scale the roll-call votes in a single dimension, I use the ratio of first-dimension variance to all higher dimensional variance as the control.²¹ Including this term is critical because higher data dimensionality will necessarily lead to higher error rates as a function of the estimation process. Statistically, this is akin to including an exposure variable in the estimation of count data where the number of possible event realizations is limited. Neglecting to include this term would induce omitted variable bias and potentially lead to misestimation of the parameters of interest. With this in mind, the informative (H2) and status (H3) hypotheses can be operationalized:

H2: (*operational informative hypotheses*): All else equal, outlying majority contingents increase error rates less than outlying minority contingents.

H3: (*operational status hypotheses*): All else equal, majority representatives have smaller error rates than their minority counterparts.

To test these hypotheses, I calculate the error rate for each representative in each jurisdiction in my sample: 213 in total. The error rate is simply the number of incorrectly classified votes over the total number of votes classified. These measures become the dependent variable in a maximum-likelihood model that regresses the error rate on the representativeness of the individual's committee contingent. The representativeness measure is the proportion of simulated committee contingents that are more moderate than the true committee contingent. The measure is bounded [0, 1], where 0 indicates a perfectly extreme outlier (a contingent for which every simulated contingent was more moderate), .5 indicates a perfectly representative contingent, and 1 indicates a perfectly moderate outlier. The model includes a squared term to allow a curvilinear relationship. This construction will facilitate a detailed interpretation, allowing for committee contingents to differently affect the voting certainty of their copartisans according to both the direction and degree to which they are outliers. As I am substantively interested in the difference between committee members and their copartisans, I include a dummy variable indicating whether or not an individual sits on the committee controlling the jurisdiction at hand. This variable, the outlier terms, and a dummy for majority are interacted in order to test Hypothesis 2.

I include several control variables as well. As Lauderdale (2010) finds that more unpredictable legislators tend to have more moderate ideal points, I include the rank-order distance between each member and the chamber median. To account for differences in committee resources and the division of committee resources, I include the proportion of congressional staff the committee employs and share of committee seats controlled by the majority and interact these terms with majority status. Unfortunately, detailed records regarding the distribution of congressional staff across committees was unavailable for the entirety of the sample. Therefore, I imputed the missing values from a distribution defined by the data I was able to find. Discussion of how this error is modeled is below, and details on the imputation are available in the online supplemental materials.

Before estimating the model, it is important to consider the structure of the data. These data present in a complex hierarchical structure where sessions, jurisdictions, and representatives combine to create seven levels of nested and crossed variation.²² Previous work in this literature has confronted potential influence from this type of structure by estimating fixed effects. However, fixed effects models suffer from generalizability issues. Further, I would have to estimate several thousand parameters in order to account for representative effects making estimation inefficient and interpretation onerous. Thus, I estimate a hierarchical random effects model. Unfortunately, even in the error components framework, modeling each of

²¹This figure is calculated from an eigenvalue decomposition of the agreement score matrix. More discussion on this procedure, as well as classification errors, can be found in Poole (2005).

²²There are (1) session effects that are constant over jurisdictions and members, (2) jurisdiction effects that are constant over sessions and members, (3) member effects that are constant over sessions and jurisdictions, (4) session-jurisdiction effects are constant over members, (5) session-member effects that are constant over jurisdictions, (6) jurisdiction-member effects that are constant over sessions, and (7) session-jurisdiction-member effects, or the residual error.

the seven levels is not feasible. However, I can estimate a model in which error is allowed to vary at three crossed levels. Here, the logical levels to estimate are the session,²³ jurisdiction, and representative levels. This approach assumes that session effects are constant over jurisdictions and representatives, jurisdiction effects are constant over sessions and members, and member effects are constant over sessions and jurisdictions.

Though this is not ideal, there is precedent for such assumptions (i.e., the fixed-effects specifications in Yoshinaka and Grose 2011). Further, the error clustering exercise detailed in Fortunato and Stevenson (2013) suggests that these are the three most salient levels that can be simultaneously accounted for. The model results are presented in Table 3.²⁴

Evaluating Hypothesis 2 from the parameters in Table 3 is difficult as the hypothesis requires interpretation of interaction terms and quadratic relationships. Further, as mentioned above, the estimates in Table 3 are derived from data with error. Before evaluating the informative hypotheses, it is important to take this uncertainty into account. To model this uncertainty, I specify a normal distribution of the uncertain data, draw new values of the covariates, estimate the statistical model, and specify a multivariate normal distribution with means equivalent to the parameter estimates and variance equivalent to the variance-covariance matrix of the model. I then take 100 draws of the parameter estimates from this distribution and begin the process again. This is iterated 1,000 times resulting in a distribution of 100,000 parameter estimates capturing the uncertainty of both the statistical model and the data that it draws from.²⁵

The distribution is used to build Figure 2 which plots the difference in committee and floor error rates over the range of committee contingent representativeness by averaging over the sample for both the majority and minority parties to evaluate the Hypothesis 2. This approach to the hypothesis holds committee members as the informational baseline and assumes variation from the baseline is a function of the level of information provided by the committee contingent

²³The portion of seats controlled by the majority is nested within session.

²⁴Descriptive statistics for the data may be found in the online appendix.

TABLE 3	Hierarchical Linear Regression of
	Individual Level Error Rates

Variable	Parameter	Standard Error
Intercept	0.2020	(0.0183)
Committee Member	0.0024	(0.0022)
Representativeness	0.4993	(0.0085)
Representativeness Squared	-0.4401	(0.0102)
Committee Member *	0.0210	(0.0262)
Representativeness		
Committee Member *	-0.0429	(0.0316)
Representativeness		
Squared		
Committee Staff	0.1703	(0.0405)
Majority Committee Seat Share	0.0432	(0.0162)
Majority	-0.0797	(0.0100)
Majority * Committee Member	0.0031	(0.0044)
Majority * Representativeness	-0.1866	(0.0105)
Majority * Representativeness	0.0947	(0.0118)
Squared		
Majority * Committee Member	-0.0397	(0.0322)
* Representativeness		
Majority * Committee Member	0.0555	(0.0366)
* Representativeness		
Squared		
Majority * Committee Staff	-0.3012	(0.0248)
Majority * Majority	0.1053	(0.0161)
Committee		
Seat Share		
Rank-Order Distance from	0.0000	(0.0000)
Median		
Data Dimensionality	-0.0407	(0.0089)
Random Effects		
Congress (Intercept)	0.0088	(0.0293)
Congress (Majority Strength)	0.0211	(0.1451)
Jurisdiction (Intercept)	0.0010	(0.0315)
Representative (Intercept)	0.0009	(0.0293)
Residual	0.0118	(0.1086)
		(002000)
N(Congress)		25
N(Jurisdiction)		14
N(Representatives)		2611
N(Observations)		92699
AIC		-145304

to their copartisans. That is, more or less outlying committees may arise in sessions or jurisdictions where agendas are more or less uncertain, and both committee members and floor members are more or less likely to err. What I am interested in here is not the baseline uncertainty but the quality of information transmission, given that uncertainty. Therefore, the quantity of interest is the divergence between committee and floor members. That there is no point in which floor

²⁵A more detailed reporting of the results of this process is available in the online appendix. These results will be useful for those who may be interested in variation in voting error across jurisdictions or congressional sessions. For example, representatives are least likely to err on Appropriations and Ways and Means voting and most likely to err on Science and Public Works legislation.



FIGURE 2 Committee Representativeness and Member Error Rates

Note: This figure shows the difference between committee and floor member error rates over the range of committee representativeness. Values above the zero-line indicate that floor members are more error-prone than committee members. The bands represent a 95%, two-tailed confidence interval. The figure shows that majority committee contingents never increase the uncertainty of their co-partisans, but that very moderate minority contingents do.

members are significantly less error-prone than committee members supports this approach.

The figure shows that over the entire range of representativeness, from extreme outlying to perfectly representative to moderate outlying, there is no statistically significant difference in the propensity to err between majority floor and committee members. This is not the case for the minority. While minority contingents that are extreme outliers or representative of their caucus continue to be informative, moderate outliers are quite disruptive. Indeed, once a moderate minority committee contingent reaches a traditional level of statistical significance in a single-tailed outlier test, they are increasing the error rates of their copartisans by over 0.015. This is not an insignificant increase. For example, in the 108th Congress, changing all minority contingents from representative (or extreme outliers) to moderate outliers would result in about 3,380 *additional* errors for the minority. This is approximately 16 additional errors per representative, and 16 additional attacking points for each representative's majority opponent come campaign season.

Evaluating Hypothesis 3 (the status hypothesis) is much easier. One can simply look at Table 3 to see that the coefficient on majority status is both in the predicted direction and highly statistically significant. Indeed, by comparing the raw majority and minority error rates within session jurisdiction, the data show that minority members err at a rate 15% higher than the majority (p = 0.003). Taking advantage of the distribution of parameter estimates drawn above and holding all covariates constant at their mean, the data show a much greater effect of status. Once all other factors are taken into account, the data show that minority members display an error rate 37% higher than majority members.²⁶

The analysis provides strong evidence for both Hypothesis 2 (the informative hypothesis) and Hypothesis 3 (the status hypothesis). Figure 2 shows that under no circumstances does the composition of the majority contingent significantly increase the policy uncertainty of the floor vis-á-vis committee members. The figure also shows that, for the minority, moderate outlying contingents cause a substantial increase to the policy uncertainty of their copartisans. Indeed, moderate minority contingents need not even reach traditional levels of statistically significant dissimilarity in order to disrupt the voting of their caucus. This finding is novel in that it is the first robust empirical evidence that outlying committee contingents matter. Further, the data also show that, on average, majority representatives demonstrate substantially less policy uncertainty than minority members. In sum, this analysis has found very strong evidence that the capacity of the majority to maintain an informational organization is significantly greater than the minority, both statistically and substantively.

Considering a Competing Explanation

Anecdotal evidence documenting the organizational changes employed by Republicans of the 104th House is ubiquitous. Newt Gingrich consolidated the power of the chair by altering committee-assignment processes and eschewing seniority norms in designation

of committee chairmanships, as well as making other changes to increase leadership control over chamber proceedings (Aldrich and Rohde 2000b). Such changes in organizational style and perceived leadership strength conjure a simple counter to the argument I have presented. Differences in observed committee behavior between the majority and minority are not truly a function of majority resource advantage. These differences may instead simply be an artifact of differing internal organizational strategies employed by the Democrats and Republicans. Given the extent to which my data are dominated by Democratic majorities, it is important to address this competing explanation.

To investigate this competing explanation, I execute a final test. If observed differences between the majority and minority in my data are a function of the differing organizational preferences of Republicans and Democrats rather than differing access to inducement resources as a function of status, then Republican behavior prior to the 104th House should be a good predictor for Republican behavior after the 1994 elections. Conversely, if the observed differences are a function of the majority advantage, as I have argued, then the behavior of the Republican majority from the 104th House on should be better predicted by the behavior of the preceding Democratic majority. Here, I test the following organizational hypothesis:

H4: Republican majority behavior will be better predicted by Democratic majority behavior than by Republican minority behavior.

I test this hypothesis with an out-of-sample prediction exercise. First, I divide the data between the Democratic and Republican eras. I reestimate the model above using only data from the Democratic era, the 84th through 103rd Houses, then repeat the error modeling procedure above generating a new distribution of parameter estimates. These parameter estimates from the Democratic-era are then combined with the actual covariate values from the Republican majority for the 104th through 108th Houses to generate two sets of predicted values: one using the Democratic-era minority parameters and one using the Democratic-era majority parameters. I then simply compare how well these predicted values match the actual observed values. My argument is supported if the majority parameters from the Democratic era better predict Republican majority error rates than the minority parameters.

The Democratic majority $(84^{th}-103^{rd})$ parameters predict modern-era Republican majority $(104^{th}-108^{th})$ error rates more accurately with probability 1. That is, there is not one single parameter draw in which

²⁶This difference has a 0.95 confidence interval of (33%, 42%).

Republican-Democrat differences trump minoritymajority differences. This out-of-sample prediction exercise provides powerful evidence for the organizational hypothesis and powerful evidence that findings presented here are not being driven by the internal organizational preferences of Democrats and Republicans, but rather by the effects of majority status.

Discussion

This article investigated, for the first time, whether majority advantages in inducing preferred voting behaviors extend to maintaining an informational organization. In investigating this open question, I discussed the canonical models of informational and partisan legislative organization (Gilligan and Krehbiel 1990; Cox and McCubbins 1993, 2005, respectively) and argued that the former was pivotal to the latter. Substantial information must be gathered and disseminated in order that legislators may make the correct vote for their party and constituency and continue to win reelection. Thus, an efficient informational organization is a necessary condition to maintain the policy cartel. However, maintaining such an organization is difficult when the preferences of party members are dissimilar. Building upon recent empirical literature, I argued that the majority party, as a function of its myriad status advantages, should be better able to maintain an informational organization than the minority. This argument led to hypotheses regarding the prevalence and effect of outlying committee contingents, as well as the overall capability of the parties to maintain an informational organization: outlying committee contingents are more common among the minority than the majority, outlying committee contingents in the majority are more informative than their minority counterparts, and majority representatives are better informed than their minority counterparts.

In order to test these hypotheses, it was necessary to identify outlying committee contingents. To this end, I argued the necessity of new data by building on past research by Maltzman (1995, 1997), Snyder (1992), and others. The comprehensive new data are composed of measures of voting behavior for all House members in up to 14 jurisdictions for the 84th through 108th Houses. In examining these data, I found that, in the case of committees as a whole, more often than not, committees are, in fact composed of preference outliers. Indeed, this study identified a higher proportion of outlying committees than nearly every past effort examining voting behavior. In the case of minority committee contingents, outliers were even more prevalent; my analysis finds that minority contingents are outliers 70% of the time. On the other hand, majority contingents were most often aligned with their party, as predicted by cartel theory and in support of the prevalence hypothesis (H1) articulated here.

Perhaps more important than the discovery of differing numbers of outlying committee contingents was the analysis of the effects these outliers had on their copartisans. Over the entire range of committee representativeness, whether the committee was perfectly representative of its party, an extreme outlier, or a moderate outlier, majority floor representatives are never more likely to err than their committee contingents. Thus, we can conclude that, even when the majority is unable to organize a representative committee contingent, it is still able to compel its delegates to inform their copartisans as to the policy implications of roll-call votes.

Minority contingents are another matter. The empirical analysis suggests that outlying minority contingents can have quite an effect on their copartisans in some cases. More specifically, as minority contingents become more and more moderate, more like the opposition and less like the core of their party in their voting behavior, they become less informative and increase the propensity of their copartisans to err. In contrast, extreme outlying contingents had no discernible effect on the policy uncertainty of their copartisans, suggesting that, as Maltzman (1995, 1997) argued, extreme, or hyperpartisan outliers are likely to be effective delegates. Further, I provided evidence that the differences uncovered between the majority and minority are much more likely to be a function of status and quite unlikely to be a function of the internal organizational preferences of Republicans and Democrats.

These findings are quite novel. As discussed in the introduction, there has been much research and debate on the prevalence of committee outliers over the past few decades; however, there has not been any empirical research investigating whether or not outliers actually have an effect on other members of the chamber. This article takes an important step forward in this regard. Beyond offering another answer to the question, "are committees outliers?"—this article offered an answer to the question, "do outliers matter?" In terms of policy uncertainty, as measured by the prevalence of roll-call classification errors, the answer is, yes, outliers do have an effect within the minority, so long as they are moderate in nature. However, extreme minority outliers and majority outliers of any kind do not adversely effect the voting of their copartisans on the floor.

Differences between the majority and minority do not stop at the organization of committee contingents and their informativeness. The data show that, even when the representativeness of the committee contingent is controlled for, minority members are substantially more likely to err than majority representatives. This suggests that the majority is better able to limit the policy uncertainty of its caucus beyond the organization of its committee contingents. These differences highlight yet another self-perpetuating advantage of majority status; if majority members are significantly less likely to err, then they are also likely to have stronger voting records to campaign on than their minority counterparts.

The implication of this difference is that majority members should enjoy a greater incumbency advantage as a function of their party's superior informational organization. Indeed, this remains an untested assumption of cartel theory: that majority members, who have presumably subverted their individual interests for the good of the party, are rewarded with a greater degree of electoral security. Cox and McCubbins (1993, 2005) argued that the party brand name would deliver that electoral advantage. The analysis here implies that there are advantages beyond the party brand name to a cartelized policymaking process-in exchange for providing policy information in their own jurisdiction, majority partisans are rewarded with policy information in alternate jurisdictions which they may use to better represent the interests of their constituents.

Acknowledgments

I would like to thank Ben Bishin, Royce Carroll, Keith Krehbiel, James Lo, Lanny Martin, Randy Stevenson, Georg Vanberg, Rick Wilson, and three anonymous reviewers for constructive criticism and helpful feedback. All remaining errors are, of course, my own.

References

- Adler, Scott, and John Lapinski. 1997. "Demand-Side Theory and Congressional Committee Composition: A Constituency Characteristics Approach." American Journal of Political Science 41 (3): 895–918.
- Aldrich, John. 1995. Why Parties? The Origin and Transformation of Political Parties in America. Chicago: University of Chicago Press.
- Aldrich, John, and James Battista. 2002. "Conditional Party Government in the States." American Journal of Political Science 46 (1): 164–72.

- Aldrich, John H., and David W. Rohde. 2000a. "The Consequences of Party Organization in the House: The Role of the Majority and Minority Parties in Conditional Party Government" In *Polarized Politics: Congress and the President in a Partisan Era*, eds Jon R. Bond and Richard Fleisher. 31–72. Washington, DC: CQ Press.
- Aldrich, John H., and David W. Rohde. 2000b. "The Republican Revolution and the House Appropriations Committee" *Journal of Politics* 62 (1): 1–33.
- Balla, Steven J., Eric D. Lawrence, Forrest Maltzman, and Lee Sigelman. 2002. "Partisanship, Blame Avoidance, and the Distribution of Legislative Pork." *American Journal of Political Science* 46 (3): 515–25.
- Bendor, Jonathan, and Adam Meirowitz. 2004. "Spatial Models of Delegation." American Political Science Review 98 (2): 293–310.
- Brady, Michael C., and David W. Rohde. 2007. "When Good Predictors Go Bad: Vote Context, Win Margins, and Misclassified Votes in the 75th to 108th Congresses." Duke University, Unpublished manuscript. (SSRN abstract 1019255)
- Carroll, Royce, and Henry Kim. 2010. "Party Government and the 'Cohesive Power of Public Plunder'." *American Journal of Political Science* 54 (1): 34–44.
- Carson, Jaime, Gregory Koger, Matthew Lebo, and Everett Young. 2010. "The Electoral Costs of Party Loyalty in Congress." *American Journal of Political Science* 54 (3): 598–616.
- Cox, Gary W., and Eric Magar. 1999. "How Much is Majority Status in the U.S. Congress Worth?" American Political Science Review 93 (2): 299–309.
- Cox, Gary W., and Matthew D. McCubbins. 1993. *Legislative Leviathan: Party Government in the House*. New York: Cambridge University Press.
- Cox, Gary, and Matthew D. McCubbins. 2005. Setting the Agenda: Responsible Party Government in the U.S. House of Representatives. New York: Cambridge University Press.
- Fortunato, David, and Randolph T. Stevenson. 2013. "Perceptions of Partisan Ideologies: The Effect of Coalition Participation." *American Journal of Political Science* 57 (2): 459–77.
- Gilligan, Thomas, and Keith Krehbiel. 1990. "Organization of Informative Committees by a Rational Legislature". *American Journal of Political Science* 34 (2): 531–64.
- Groseclose, Tim. 1994. "Testing Committee Composition Hypotheses for the U.S. Congress." *Journal of Politics* 56 (2): 440–58.
- Hall, Richard, and Bernard Grofman. 1990. "The Committee Assignment Process and the Conditional Nature of Committee Bias." *American Political Science, Review* 84 (4): 1149–66.
- Jenkins, Jeffrey A., and Nathan W. Monroe. 2012. "Buying Negative Agenda Control in the U.S. House." *American Journal of Political Science* 56 (4): 897–912.
- Jones, Mark P., and Wonjae Hwang. 2005. "Party Government in Presidential Democracies: Extending Cartel Theory Beyond the U.S. Congress." *American Journal of Political Science* 49 (2): 267–82.
- Kiewiet, D. Roderick, and Matthew McCubbins. 1991. *The Logic* of Delegation: Congressional Parties and the Appropriations Process. Chicago: University of Chicago Press.
- Lauderdale, Benjamin E. 2010. "Unpredictable Voters in Ideal Point Estimation" *Political Analysis* 18 (2): 151–71.
- Lewis, Jeffrey B., and Keith T. Poole. 2004. "Measuring Bias and Uncertainty in Ideal Point Estimates via the Parametric Bootstrap." *Political Analysis* 12 (2): 105–1275.

- Maltzman, Forrest. 1995. "Meeting Competing Demands: Committee Performance in the Post-Reform House." *American Journal of Political Science* 39 (3): 653–82.
- Maltzman, Forrest. 1997. Competing Principals: Committees, Parties, and the Organization of Congress. Ann Arbor: University of Michigan Press.
- Nelson, Garrison. 2005. Committees in the U.S. Congress, 1947-1992: House, 83-102, accessed on April 14, 2013.
- Poole, Keith T. 2005. *Spatial Models of Parliamentary Voting*. New York: Cambridge University Press.
- Poole, Keith T. Roll Call Matrices for Congresses 1–101, accessed on April 14, 2013.
- Poole, Keith T., and Nolan McCarty. Roll Call Matrices for Congresses 102–108: House, 102–108, accessed on April 14, 2013.
- Poole, Keith T., and Howard Rosenthal. 1985. "A Spatial Model for Legislative Roll Call Analysis." *American Journal of Political Science* 29 (2): 357–84.
- Richman, Jesse. 2008. "Uncertainty and the Prevalence of Committee Outliers." *Legislative Studies Quarterly* 33 (2): 323–47.
- Roberts, Jason M., and Steven S. Smith. 2003. "Procedural Contexts, Party Strategy, and Conditional Party Voting in the U.S. House of Representatives, 1971-2000." American Journal of Political Science 47 (2): 305–17.
- Rohde, David W. 2004. Roll Call Voting Data for the United States House of Representatives, 1953-2004. Compiled by the Political Institutions and Public Choice Program, Michigan

State University, East Lansing, MI, 2004, accessed on April 14, 2013.

- Shepsle, Kenneth A. 1979. "Institutional Arrangements and Equilibrium in Multidimensional Voting Models." American Journal of Political Science 23 (1): 27–59.
- Snyder, James M., Jr. 1992. "Artificial Extremism in Interest Group Ratings." Legislative Studies Quarterly 17 (3): 319–45.
- Snyder, James M., Jr., and Tim Groseclose. 2000. "Estimating Party Influence in Congressional Roll Call Voting." American Journal of Political Science 44 (2): 193–211.
- Sprague, Mary. 2008. "The Effects of Measurement and Methods Decisions on Committee Preference Outlier Results." *Political Research Quarterly* 61 (2): 309–18.
- Stewart, Charles, III, and Jonathan Woon. Congressional Committee Assignments, 103rd to 110th Congresses, 1993–2007.
- Weingast, Barry R., and William Marshall. 1988. "The Industrial Organization of Congress; or, Why Legislatures, Like Firms, Are Not Organized as Markets." *Journal of Political Economy* 96 (1): 132–63.
- Yoshinaka, Antoine, and Christian R. Grose. 2011. "Ideological Hedging in Uncertain Times: Inconsistent Legislative Representation and Voter Enfranchisement." *British Journal* of Political Science 41: 765–94.

David Fortunato is an Assistant Professor at the University of California, Merced, CA 95340.