Re-examining Legislative Committee Representativeness in the States

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ABSTRACT

I re-examine theories of legislative committee organization by using simulation to assess how representative state legislative committees are of their parent bodies. I find that clearly unrepresentative committees are rare and concentrated in a few chambers. I also find that comparing committee and chamber medians leads to very different conclusions about representativeness than does comparing means. My findings tend to confirm the informational model of committees and disconfirm the partisan model, but they cannot directly address the distributive model.

THE INFORMATIONAL, PARTISAN, AND DISTRIBUTIVE MODELS of legislative committee organization (Krehbiel 1991; Cox and McCubbins 1993; Weingast and Marshall 1988) can be compared and evaluated in part by examining the extent to which committees are representative of their chambers and their parties. Until recently, most of these comparisons have been in the United States House of Representatives, even though the theories themselves, which are based on game-theoretic formal models, should generalize to any legislature that generally comports to their formal models. This includes nearly all American-style legislatures.

But even if we knew with certainty that committees in the United States House were organized along informational or distributive principles, how would we know if this was a general process for legislatures of this structure? To answer this question, we can look to the state legislatures in a comparative context (Jewell 1981). Beyond merely being numerous, there is significant variation among state legislatures in their internal organization, membership characteristics, place in the political opportunity structure, and other factors that would allow for a robust test of these theories. Researchers have begun using state legislatures in this way, with Overby and Kazee (2000) using interest group scores to show that outlying committees in 12 lower state

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chambers are rare, and Overby, Kazee, and Prince (2003) extending this analysis to 45 lower chambers with similar results.

I use NOMINATE scores (Poole and Rosenthal 1987, 1997) and different methods than Overby, Kazee, and Prince in 11 state chambers to assess these theories of legislative committee organization. I also find that clearly outlying committees are rare, but I interpret their concentration in a few chambers differently than do Overby, Kazee, and Prince. In addition, I find that comparing committee and chamber medians, as substantive theory suggests, gives notably different results than comparing committee and chamber means, as Overby, Kazee, and Prince do. In most of the chambers I observe, committees are quite representative, which tends to confirm the informational model of committee organization. In four of the 11 chambers, the committee systems are far more representative than we could expect a random assignment process to create. Partisan contingents on the committees in my dataset are neither particularly representative nor unrepresentative, which tends to disconfirm the partisan model of committee organization. Limitations of NOMINATE scores do not allow me to address directly the distributive model of committee organization.

COMMITTEE MODELS AND EMPIRICS

Three theories of legislative committee organization dominate the scholarly literature – informational, partisan, and distributive. Here, I briefly describe each theory and highlight a single proponent. Interested readers should consult any of the recent stream of articles on the subject for a more thorough discussion of them (e.g., Hall and Grofman 1990; Groseclose 1994; Londregan and Snyder 1994; Maltzman 1997; Adler and Lapinski 1997; Adler 2000).

Proponents of the distributive model argue that legislative committees are fundamentally about facilitating vote trades. Weingast and Marshall (1988) write that a system where members self-select onto committees with gatekeeping power solves two problems in trading votes – it simplifies the exchange of power by consolidating it into one trade rather than many, and it prevents legislators from reneging on a successful trade. Such self-selection would result in committees that are unrepresentative of the entire chamber and consisting of high-demand legislators. For example, agriculture committees would work for the benefit of farmers and farming areas because they would be populated by rural legislators, merchant marines and fisheries committees would work for the benefit of fishermen and seacoast communities because they would be populated by legislators from the seacoast, and so on.

Proponents of the informational model (Krehbiel 1991) argue that committees are tools legislators use to gather useful, if biased, information in the legislative process. This allows a legislator to understand the links between legislative action and policy outcomes, helping him or her deliver outcomes voters approve, thereby gaining his or her re-election. Therefore, the incentive is for a chamber to develop committees that are representative of the entire chamber, thereby minimizing the principal-agent conflicts between chamber and committee and the resulting information bias.

However, a problem with testing the informational model is that it sometimes predicts unrepresentative committees and committees that are not clearly representative or unrepresentative (Krehbiel 1991, 135). A disproportionate number of highly representative committees in a legislature is relatively clear evidence for the informational model. However, unrepresentative committees or a mix of representative and unrepresentative committees are not unequivocal evidence disconfirming it. The informational model has two components working against each other with respect to committee representativeness: the information the committee delivers to the chamber, and the cost incurred by the chamber in persuading the committee to specialize and learn about its jurisdiction. More representative committees are more informative because of their reduced bias, but they are also more costly in this respect. This increased cost arises because it is easier to induce those already interested (and therefore biased) to specialize in a given policy area. While we might expect the tradeoff usually to be in favor of information, since legislators face only limited resource constraints, one cannot rule out the possibility that a given unrepresentative committee was set up by an informationally oriented legislature to maximize the net value of the committee (Gilligan and Krehbiel 1990). But while it is difficult to conclude that any particular committee is not informational, we can say that a committee system with few unrepresentative committees and many representative committees is probably good evidence for the informational model, since it corresponds to a clear prediction of the model that is not made by the competing models.

The partisan model echoes the logic of the informational model to the extent that it sees committees as largely subservient, but in the partisan model, committees are tools of the chamber party caucuses rather than the entire chamber (Cox and McCubbins 1993). Members of each party in the legislature are seen as acting to improve their collective reputation, thereby increasing the odds of their re-election. One way they do so is by controlling access to committees whose jurisdictions might help or harm the party's reputation. Hence, the partisan model implies that each committee par-

ty contingent will be representative of the parent caucus to the extent that the committee is important to the collective future of the party members.

Until recently, almost all testing of these three models of legislative committee organization has been conducted in Congress, mostly in the House (Hall and Grofman 1990; Krehbiel 1990; Groseclose 1994; Londregan and Snyder 1994; Adler and Lapinski 1997; Adler 2000). The results of these congressional tests have varied somewhat depending on the estimators of legislators' preferences and tests of representativeness being used, but they have not generally found an abundance of unrepresentative committees. Each model has found empirical support in at least one published study, but each also faces arguably disconfirming evidence in other studies. As a result, the congressional literature has shifted to two linked methodological disputes, one over how best to estimate legislators' preferences (Poole and Rosenthal 1987, 1997; Hall and Grofman 1990; Snyder 1992; Adler and Lapinski 1997; Adler 2000) and the other over how best to find unrepresentative committees (Weingast and Marshall 1988; Krehbiel 1990, 1991; Hall and Grofman 1990; Cox and McCubbins 1993; Londgren and Snyder 1994; Groseclose 1994).

Overby and Kazee (2000) have begun to take advantage of the empirical leverage the state legislatures provide for testing these models. They use interest group scores and two-sample t-tests of means to examine committee representativeness in 12 lower chambers. Overby, Kazee, and Prince (2003) expand this project to 45 lower chambers. Overby, Kazee, and Prince find that only 3.4 percent of these committees are unrepresentative at a statistical significance level of 0.05. They also find that 8.0 percent of Republican noncontrol committee delegations are unrepresentative of their parent party caucus, as are 11.0 percent of Republican control committee delegations and 9.0 percent of all Democratic delegations. Overby, Kazee, and Prince interpret their results as generally confirming the informational and partisan models and as weakly disconfirming the distributive model, although their full accounts are more nuanced.

I extend the efforts of Overby, Kazee, and Prince to test the models of legislative committee organization in the states. I use NOMINATE scores to estimate legislative preferences and Monte Carlo simulation to estimate the representativeness of committees. In my data from all 11 state legislative chambers, nine of 201 committees (4.5 percent) are 0.05–level outlying (unrepresentative) relative to the parent chamber when comparing medians, and 11 (5.4 percent) are 0.05–level outlying when comparing means. While these aggregate results are similar to Overby, Kazee, and Prince's, I interpret them somewhat differently.

DATA AND METHODS

I include 11 state legislative chambers in my study: the lower chambers of Connecticut, Georgia, Iowa, Louisiana, Maine, Minnesota, and Rhode Island; the upper chambers of New Hampshire, South Carolina, and Vermont; and the unicameral and nonpartisan Nebraska legislature. This is not a random sample of states, which would not have been feasible since not all states make their roll call votes available in a free and usable format. However, my sample of states is intended to reflect the regional, professionalism, size, and partisan control variation found in American state legislatures.

Estimating Legislator Preferences

To estimate legislator preferences in my sample of states, I generated unidimensional NOMINATE scores for all legislators for 1997–99. NOMINATE is a statistical method devised by Poole and Rosenthal (1987, 1997) for estimating a legislator's preferences through his or her roll call votes in the context of other legislators' votes.[1] To increase the number of states in the sample, in most cases the NOMINATE score is based on only the first 100 to150 votes in a session. The exact number of votes for each session varies because the scores were calculated from all votes cast through the legislative day in which the 100th vote took place. A potential problem with truncating the vote sample in this way is that if the out-of-sample votes differ from the in-sample votes, the NOMINATE scores will be biased. This worry is compounded by the fact that state legislatures, like Congress, have heavily back-loaded schedules with many votes being cast in the last few days or weeks of a session. However, experimentation with data from the United States House and the state chambers for which I entered all votes indicated little ill effect of this truncation in practice. Truncated and full-dataset NOMINATE scores were correlated at approximately 0.98, implying little room for bias. While end-of-session votes may be of greater substantive importance, they seem to create the same cleavages among legislators as do earlier votes.

NOMINATE scores are only estimates of ideal points based on specific observed votes, and since these votes vary from chamber to chamber, we cannot compare scores directly across chambers. For example, suppose we observe an agriculture committee in one chamber whose NOMINATE scores suggest that it is more representative of its chamber than the agriculture committee of some other chamber. This might reflect real, underlying differences in representation. On the other hand, both committees might be equally unrepresentative in their true preferences, but one chamber is more successful at preventing the floor votes that would show this. An additional

problem with NOMINATE scores is that they are not jurisdiction-specific, so I cannot straightforwardly test the distributive model of committee organization. Unidimensional NOMINATE scores seem to be closely linked to a general liberal-conservative continuum (Poole and Rosenthal 1987, 1997). But the distributive model does not predict that, for example, an agriculture committee will be more liberal or conservative than its parent chamber. Rather, it predicts that the committee will be more pro-farmer than the chamber. None of the dimensions NOMINATE recovers from roll call votes have been reported to resemble any dimension that parallels substantive committee jurisdictions. These problems notwithstanding, NOMINATE scores are still eminently useful in that they are easily generated from new data, they do not require a wealth of contextual data to create, and they have properties that are well-known among legislative scholars.

Estimating Committee Representativeness

Estimating the representativeness of a committee is a two-stage process. First, I need a way to use legislators' NOMINATE scores to assess the aggregate ideological position of the committee and the parent chamber (or caucus) and then compare these positions. Here, I compare the committee (party contingent) median of NOMINATE scores to the chamber (caucus) median, and also the mean score to the mean. Second, I need a way to make probability statements about how distinctive a committee is from its chamber. For this, I use Monte Carlo simulation.

Medians versus Means. Existing studies of committee representativeness have compared both median (Cox and McCubbins 1993; Groseclose 1994; Adler and Lapinski 1997) and mean (Weingast and Marshall 1988; Krehbiel 1990, 1991; Overby and Kazee 2000; Aldrich and Battista 2002; Overby, Kazee, and Prince n.d.) preferences. The theoretical logic behind comparing medians is well established. If legislators are assumed to have single-peaked preferences (as all models have assumed) and legislators are arrayed along only one dimension (also a common assumption), then the median voter theorem applies and the median preference is a Condorcet winner (Downs 1957; Black 1958). That is, the preferences of the committee collapse to its median legislator's preference, and the preferences of the chamber collapse to its median legislator's preference. Therefore, to compare the collective preference of a committee to that of its chamber, we need to compare median preferences.

Various reasons have been given to justify assessing representativeness by comparing committee and chamber means, rather than medians, but the primary reasons have to do with statistical tractability. In the 1980s, when

these models were developed and first tested, while computing a difference-of-means test statistic was trivial, comparing medians required methods relatively new to political science, such as the Wilcoxson rank-sum test. In 1994, Groseclose (1994, 446) assessed congressional committees with both medians and means and found that these tests yielded similar results (though means-based tests are more likely to find outlying committees), thus decreasing the urgency of using the theoretically more appropriate medians. Overby and Kazee (2000) cite Groseclose's results (among other justifications), noting that since they hypothesize that outliers are rare, the bias was against their expectations, making their tests conservative. Overby, Kazee, and Prince (2003) justify their use of means-based tests in part because it facilitates comparison with their earlier work. Thus, this use of difference-of-means tests is largely practical.[2] While a medians-based test would be better justified theoretically, means-based tests are easier to conduct. But as computing power has become less expensive and as political scientists have gained more facility with the necessary software and statistical approaches, the statistical-ease argument has become much less relevant.

Here, I contribute to this methodological debate by reporting results based on both means-based and medians-based tests. In doing so, one notable finding of my study is that means tests do not appear to be a reliable substitute for medians tests in comparing committees and chambers because their estimates of representativeness can differ wildly in either direction.

Testing for Representativeness. Assessing a committee's representativeness takes the form of asking how probable it would be to see a committee chosen at random from the parent group that was at least as different from the parent group as the actual committee in question. That is, where along the distribution of committees drawn randomly from the parent group does the actual committee fall? If it is near the center of that distribution, the committee is representative; if it is an outlier in one of the tails, the committee is unrepresentative. I use Monte Carlo simulation to estimate such distributions of random committees to make these assessments.

The primary advantage of using Monte Carlo simulation is that it allows me to avoid making restrictive assumptions about the distribution of estimated legislator preferences in the data. A difference-of-means test requires the assumption that preferences are distributed normally, an assumption usually violated in the distribution of actual legislator preferences. The Wilcoxson rank-sum test of medians requires the weaker assumption that the legislators' preferences are distributed symmetrically, but this is also routinely violated by the actual distributions. The distributions of NOMINATE scores

are usually either strongly multimodal with the modes far apart or roughly uniform but very noisy. Examples of distributions of NOMINATE scores that illustrate these assumption violations can be found in Aldrich and Battista (2002) and Poole and Rosenthal (1987, 1997), and at Poole's website (http://voteview.uh.edu, as of August 2003).

To estimate the distribution for a random committee paralleling each of the 201 committees in my sample, I created 10,000 committees of the same size as each actual committee with membership selected randomly from the entire parent chamber. Every member had an equal probability of being chosen, and sampling was without replacement in each simulated committee. It was then a simple matter to see where in this distribution the actual committee fell. For example, if only 490 of the 10,000 simulated committees were at least as far from the chamber median (or mean, depending on the test) as the actual committee, then that committee differed from the parent chamber at a statistical significance level of 0.049. I also followed this procedure with Democratic and Republican party committee contingents and their full chamber caucuses.

Another question relevant to testing the legislative committee organization models is whether a given set of committees in a chamber has significantly more (or fewer) outlying committees than we would expect from a random assignment process. That is, a random assignment process is likely to produce about 5.0 percent of its committees being outliers at the 0.05 level, so what can we say about a chamber that has 8.0 percent of its committees outlying at that level? How probable would that be? Of course, there is no clear categorical distinction between outliers and inliers; most committees in most chambers will be neither extremely representative nor extremely unrepresentative. Arguments over what statistical significance level of preference differences makes a committee an outlier abound in the literature (Hall and Grofman 1990; Groseclose 1994). The main dispute is whether to use a strict standard, such as the 0.05 level, and fail to identify committees that are actually outlying, or to use a more generous, perhaps 0.25 level, standard and increase the chances of false positives. This is a tradeoff between Type-I and Type-II inferential error, and the appropriate balance is a subjective judgment for the researcher based on his or her null hypotheses and the consequences of each error. Since I am primarily concerned with testing the informational model, I rely primarily upon a 0.25 level since it biases results in the direction of my null hypothesis by making outliers easier to identify. However, since readers likely have their own preferences for a statistical significance level based on their own research interests, I report results based on 0.05–, 0.10–, and 0.25–level tests for outliers. These may be

interpreted as certain, probable, and possible outliers, respectively. Alternatively, they may be interpreted as severe, moderate, and mild outliers.

In addition, I assess how likely it would be for a random assignment process to produce very few committees outlying at even the 0.25 level. This gives me an estimate of whether a committee system is actually overrepresentative. A committee system with very few outliers would support the informational model of committee organization. To do this, I generated 10,000 random-assignment committee systems for each chamber with the observed number and size of committees in order to create a simulated density of the number of outliers at a given statistical significance level. One inferential problem with this is that the joint significance so determined depends critically on the statistical significance level used to define an outlier. It is possible for a chamber to have a significantly high number of 0.05–level outliers, a non-significant number of 0.10–level outliers, and a significantly small number of 0.25–level outliers. I discuss the implications of this as I discuss my results.

ARE STATE LEGISLATIVE COMMITTEES REPRESENTATIVE?

Are state legislative committees representative of their parent bodies? The short answer is, perhaps, the modal answer in social science: "It depends...." Overall, the committees in my sample do not tend to be clearly unrepresentative of their chambers. Only nine of 201 (4.5 percent) of these committees are 0.05–level outlying using medians-based tests, and 11 of 201 (5.4 percent) are outlying using means-based tests. At the 0.25 level, 40 committees (20.0 percent) are unrepresentative using medians-based tests, and 26 (12.9 percent) are so using means-based tests.

In general, these results are quite consistent with congressional studies and those of Overby and Kazee, and their expanded work with Prince, in state legislatures, but I interpret them somewhat differently than have previous researchers. Overby, Kazee, and Prince (2003) highlight the rarity of outlying committees. But these outliers are not simply rare. In fact, these unrepresentative committees are clustered in just a few chambers, with the other chambers being significantly more representative than we would expect by chance.[3] Outlying committees in my dataset are rare, not simply because most committees are inlying, but because most chambers have few, if any, outliers while a few chambers have more outliers than we would expect by chance. This same pattern is seen in Overby, Kazee, and Prince's (2003) data. Most of their chambers have either zero or one 0.05–level outlier, while a few have over 10 percent of their committees outlying at that level.[4] Thus, most cham-

bers establish highly representative committee suites, either by direct choice or through some indirect means, while other chambers do not. While Overby, Kazee, and Prince emphasize the rarity of outlying committees, I emphasize their uneven distribution among legislative chambers.

Table 1 displays the joint p-values of the committee systems of the 11 chambers for medians-based and means-based tests. High numbers indicate chambers with few outlying committees for a given statistical standard for defining outliers. Many entries are 1.000; these are chambers with no outliers at the indicated level of statistical significance. Only one chamber – the Rhode Island House – clearly has an abundance of unrepresentative committees. The South Carolina Senate and Georgia House committee systems wander in and out of 0.10–level statistical significance depending on the standard used to detect individual outliers. The Nebraska Legislature and Louisiana House are neither obviously representative nor unrepresentative. The remaining six chambers have highly representative committee systems, with the possible exception of the Connecticut House using the 0.25 standard to identify outliers. This lack of outliers is consistent with the informational and partisan models of committee organization, both of which predict few outlying committees. However, finding few committees that differ from their parent chambers at a statistical significance level of, say, 0.05 is also consistent with simple random selection.

Table 1. Probability of Observing at Least as Many Outliers by Chance, Committee vs. Chamber

		Jt. p-value (0.05)		Jt. p-value (0.10)		Jt. p-value (0.25)	
Year	Chamber	Mean	Median	Mean	Median	Mean	Median
1997	CT H	1.000	1.000	1.000	1.000	1.000	0.479
1998	GA H	0.216	0.446	0.648	0.199	0.726	0.069
1997	IA H	1.000	1.000	1.000	1.000	1.000	1.000
1999	LA H	0.583	0.228	0.089	0.536	0.241	0.271
1997	ME H	1.000	1.000	1.000	1.000	1.000	1.000
1997	MN H	1.000	1.000	1.000	1.000	0.995	1.000
1999	NE U	0.576	1.000	0.524	0.844	0.431	0.642
1997–98	NH S	1.000	1.000	1.000	1.000	1.000	1.000
1997	RI H	0.003	0.021	0.037	0.030	0.006	0.216
1997	SC S	0.036	0.578	0.189	0.810	0.543	0.334
1997–98	VT S	1.000	1.000	1.000	1.000	0.960	0.959

Note: "Year" denotes the years used to compute NOMINATE scores. "Chamber" lists the chamber with standard postal codes for the state, H for lower chambers, and S for upper. Nebraska's legislature is unicameral. Each set of "Jt. p-value" columns gives the joint probability value of each chamber's committees for the listed standard for identifying outliers, and is the probability of seeing at least as many outliers using a random assignment process as are actually present. The "mean" and "median" columns indicate whether means-based or medians-based comparisons are being made.

Further evidence supporting the informational model is seen in reversed joint statistical significance tests. This is a sharper test of the informational model. Table 1 shows that most chambers in my sample have a non-statistically significant number of outliers. But non-significance is not significance in the other direction, it is merely non-significance. However, a statistically significant paucity of outliers is strong evidence for the informational model of committee organization. That is, for those highly representative committee systems with only one or zero outliers at the 0.25 level for both means-based and medians-based tests, what is the probability of seeing so few outliers emerge from random selection of members into committees? Statistical significance suggests that there is some process actively creating representative committees rather than representativeness just resulting from random selection. Table 2 provides these probabilities. Note that using means-based tests, in three chambers (the Iowa and Maine Houses and the New Hampshire Senate), there were no outliers even at a standard of 0.50 (not shown), the probability of which by random assignment descends into the truly miniscule (less than 1 in 10,000). In four of the six chambers, there is clearly a highly statistically significant lack of outliers, far fewer than the occasional outlier a random process would likely produce. In the Connecticut House, statistical significance depends strongly on whether medians or means are compared, and in the Vermont Senate, the numbers of outliers we actually observe are roughly consistent with a random process. The existence of these overly representative committees clearly supports the informational model of committee organization, since it is a unique prediction of that model.

Thus, there are at least two dimensions to the representativeness of a committee system: a system of representative committees would have few

Table 2. Probability of Observing No More Than the Observed Number of Outlying Committees by Chance

	Means-based		Medians-based	
Chamber	0.25-Outliers/N	p-value	0.25-Outliers/N	p-value
CT H	0/22	0.001	6/22	0.479
IA H	0/16	0.000	0/16	0.008
ME H	0/23	0.000	0/23	0.001
MN H	1/18	0.004	0/18	0.004
NH S	0/17	0.000	0/17	0.005
VT S	1/11	0.197	1/11	0.191

Note: "Chamber" denotes chamber. "0.25-Outliers/N" reports the number of observed outliers using a 0.25 standard for identifying outliers relative to the total number of committees in that chamber. "P-value" reports the probability of obtaining no more than the observed number of outliers from a random assignment process. The means-based and medians-based sets of columns identify the type of comparison being conducted.

outliers, and it might also have an abundance of inliers, committees whose collective preferences are unusually close to the collective preferences of the entire chamber. That is, a representative committee system would have few committees with median NOMINATE scores far from their parent chamber's median and many committees with median scores near their chamber's median, and likewise for means-based tests. The most efficient way to see both of these dimensions of representativeness at the same time is graphically. Figures 1 through 3 display the simulated random and actual distributions of committee medians for selected chambers as deviations from the chamber median. I selected these three chambers as exemplars of three types of representativeness or unrepresentativeness. The 1997 Iowa House (Figure 1) and 1997 Maine House (Figure 2) illustrate systems of representative committees. Their figures show that they have fewer outlying committees and more inlying committees than a random process would produce, as the higher peaks at zero-deviation indicate. Deviations in both cases are toward the center of the preference space, but this is probably an artifact of the highly polarized parties in these chambers since there is little room to have significantly more extreme committees as unidimensional NOMINATE scores are bounded at -1 and 1 and legislators' scores often came near these bounds. The 1997 Rhode Island House (Figure 3) has an unrepresentative set of committees. The distribution of actual committee medians is flatter and wider

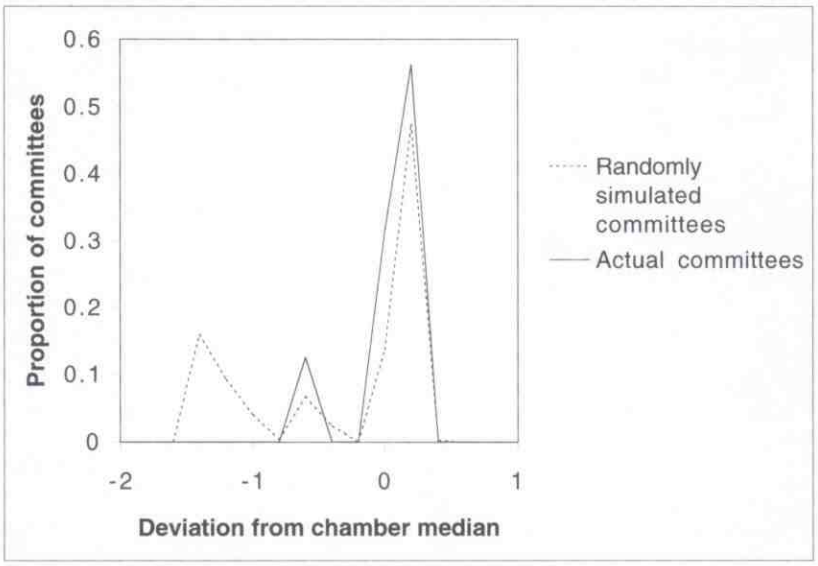

Figure 1. Distribution of Median Committee Preferences, 1997 IA House

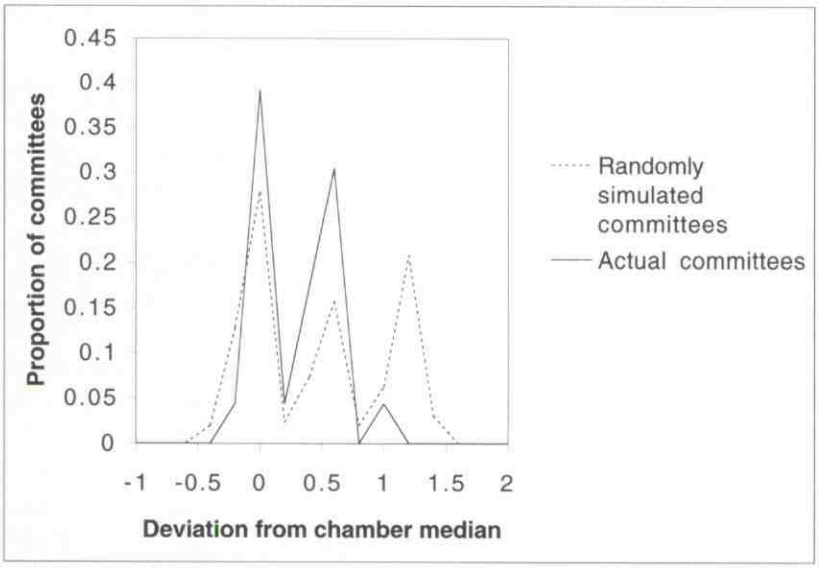

Figure 2. Distribution of Median Committee Preferences, 1997 ME House

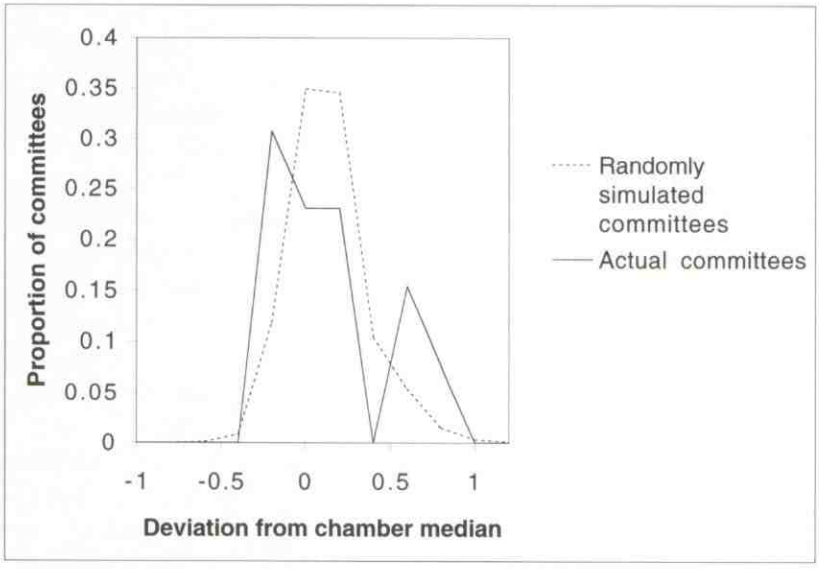

Figure 3. Distribution of Median Committee Preferences, 1997 RI House

than the distribution of simulated medians, showing more outliers and fewer inliers than a random assignment process would produce.

On the other hand, an analysis of the representativeness of party contingents on committees tells a different story. Tables 3 and 4 present the joint p-values for Democratic and Republican contingents relative to their chamber caucuses.[5] In eight of these 20 partisan caucuses there are significantly high numbers of outlying partisan contingents for at least one outlier level and method of testing. In no chamber are there significantly fewer outlying contingents than a random assignment process would produce, for either party and using either method of testing (results not shown). While there are chambers with no outlying contingents at a given statistical significance level, the probability of this occurring by chance is greater than the traditional significance level of 0.05. Thus, most partisan contingents are not particularly representative or unrepresentative.

An important distinction in American legislatures is that between control and non-control committees. Control or power committees (Fenno 1973), such as the House Appropriations, Ways and Means, and Rules committees in Congress, are committees that constrain or coordinate the actions of other committees. They generally play an important role in bringing the decisions of more policy-oriented committees into some semblance of a coherent policy. Because of their greater power, an unrepresentative control committee could do more harm than a non-control committee, and Cox and McCubbins (1993) predict that control committees should have tightly controlled, representative partisan contingents. While it can be difficult to determine which committees in a given chamber are truly control committees, taxing and spending committees are typically among the most important committees. The Georgia House has majority taxing and spending committee contingents that are 0.05–level outliers using a medians-based test, and the Louisiana House's majority contingent on its Appropriations committee is arguably unrepresentative, with a p-value of 0.062 using a medians-based test. Similarly, no taxing or spending committee has a majority contingent that is unusually representative (i.e., with only 5 percent or fewer of the simulated contingents being closer to the actual caucus median or mean). Thus, there are unrepresentative party contingents on control committees, if only a few, and there is no sign that the parties are actively making their control-committee contingents overrepresentative of their caucuses. This clearly does not reflect the Cox and McCubbins (1993) model of parties controlling access to committee assignments, especially on control committees. While partisan contingents are not obvious outliers, there is little evidence to suggest any active process to prevent outlying partisan contingents from developing.

Table 3. Probability of Observing at Least as Many Outliers by Chance, Democratic Committee Contingents vs. Caucus

		Jt. p-value (0.05)		Jt. p-value (0.10)		Jt. p-value (0.25)	
Year	Chamber	Mean	Median	Mean	Median	Mean	Median
1997	CT H	0.691	0.270	0.672	0.364	0.069	0.020
1998	GA H	0.005	0.020	0.040	0.040	0.157	0.041
1997	IA H	0.051	0.540	0.071	0.433	0.029	0.314
1999	LA H	0.002	0.070	0.005	0.098	0.040	0.050
1997	ME H	1.000	1.000	1.000	0.942	0.544	0.871
1997	MN H	0.615	0.244	0.105	0.105	0.061	0.056
1997–98	NH S	1.000	1.000	1.000	1.000	0.936	1.000
1997	RI H	0.002	0.141	0.007	0.039	0.089	0.223
1997	SC S	0.171	0.519	0.182	0.831	0.762	0.698
1997–98	VT S	1.000	1.000	0.697	1.000	0.555	0.825

Note: Each set of Jt. p-value columns gives the joint probability of observing at least as many outlying Democratic contingents on committees using a random assignment process as are actually present. The mean and median columns indicate whether means-based or medians-based comparisons are being made.

Table 4. Probability of Observing at Least as Many Outliers by Chance, Republican Committee Contingents vs. Caucus

		Jt. p-value (0.05)		Jt. p-value (0.10)		Jt. p-value (0.25)	
Year	Chamber	Mean	Median	Mean	Median	Mean	Median
1997	CT H	0.322	0.264	0.191	0.148	0.500	0.262
1998	GA H	0.194	0.169	0.162	0.147	0.287	0.019
1997	IA H	0.574	0.548	0.819	0.843	0.200	0.569
1999	LA H	0.561	0.623	0.800	0.846	0.613	0.833
1997	ME H	0.108	0.692	0.415	0.905	0.093	0.517
1997	MN H	0.228	0.219	0.564	0.257	0.700	0.236
1997–98	NH S	1.000	0.542	0.240	0.715	0.655	0.200
1997	RI H	0.397	1.000	0.337	0.439	0.241	0.529
1997	SC S	0.572	1.000	0.797	0.746	0.915	0.518
1997–98	VT S	0.002	1.000	0.020	0.802	0.124	0.963

Note: Each set of Jt. p-value columns gives the joint probability of observing at least as many outlying Republican contingents on committees using a random assignment process as are actually present. The mean and median columns indicate whether means-based or medians-based comparisons are being made.

MEDIANS AND MEANS REVISITED

Choosing means-based or medians-based tests has a clear effect on the conclusions one draws from these data, and there are lessons that the field can draw in this respect from my analysis. Comparing whole committees to their parent chambers, the means-based tests give estimates of representativeness (p-values) that are, on average, 0.088 higher than those of the medians-based tests, contrary to Groseclose's (1994) findings in the United States House. But,

this summary figure hides significant variability. The standard deviation of the difference between means-based and medians-based p-values in this dataset is 0.313, which is substantial given that these p-values are bounded between zero and one. These means-based and medians-based estimates of representativeness are correlated at only 0.387. The findings are similar for partisan contingents. For Democrats, the standard deviation of the difference between means-based and medians-based p-values is 0.295, and the correlation between them is 0.565. For Republicans, the standard deviation is 0.279, and the correlation is 0.576.

This variability indicates that the statistically tractable means-based test is not a reliable substitute for a medians-based test, which is generally regarded as theoretically preferable. While they are usually in the same range on average, knowing that a specific committee is outlying at a 0.05 level with a means-based test carries no real information about whether it would be outlying using a medians-based test. Thus, the difference between the means- and medians-based tests becomes especially important when an analyst assesses only a few committees, such as control committees or committees with a substantive jurisdiction of interest to the analyst. Scores can vary wildly between these tests on a single committee. This does not mean that the means-based test is necessarily wrong, but it does imply that the choice of test in a given analysis should be justified strongly on theoretical grounds and not on the basis of computational convenience.

DISCUSSION AND CONCLUSIONS

Consider what these analyses suggest about the three models of legislative committee organization discussed at the outset of this article. First, my results fail to support the partisan model of committee organization (Cox and McCubbins 1993). Committee party contingents of legislators are not disproportionately representative of their caucuses, and there is little sign that majority parties are taking active steps to prevent outlying contingents. On the other hand, my results provide some support for the informational model of committee organization (Krehbiel 1991). At least five of my 11 chambers had significantly overrepresentative suites of committees, suggesting that those chambers organized their committees to retrieve and filter unbiased information to the chamber as a whole. In the other five or six chambers, I do not find support for the informational model. But the results in those chambers are not necessarily inconsistent with an informational committee system. More representative committees in these chambers might require excessively high transfers from the chamber to the committee as an induce-

ment to specialize. In such a case we might observe informational but unrepresentative committees (Gilligan and Krehbiel 1990). Finally, my results can neither confirm nor disconfirm the distributive model of committee organization, since they are based on NOMINATE scores. While it might be tempting to consider those chambers with unrepresentative committees to be distributive, such an inference would be invalid. A clear test of the distributive model of committee organization must wait for data that can assess the specific forms of policy-based committee unrepresentativeness that the model predicts.

The committee systems in the state legislative chambers in my sample fall into three categories of representativeness: clearly representative committee systems, those that are no more representative than those that would be the result of a random assignment process, and those that are clearly unrepresentative. However, at the same time, we see committee party contingents throughout these chambers that resemble closely the distribution of randomly selected contingents. That these two patterns could exist simultaneously should not be surprising. If a chamber chooses to have approximately the same ratio of party members on a committee as in the parent chamber (as is typical), it is choosing a certain level of representativeness even if the parties themselves were to assign their members to these committees randomly. The equal-party-ratio choice eliminates most of the possible committees that are extremely unrepresentative, if the parties are polarized in a manner consistent with conditional party government theory (Rohde 1991; Aldrich and Rohde 1998; Aldrich, Berger, and Rohde 1999). While I omitted the figures for brevity, the actual committee densities in my dataset and the densities simulated from random selection and the actual party ratios are very similar. Thus, it is entirely possible for even a random selection process at one level to lead to representativeness at a higher one.

In summary, no single model of legislative committee organization is clearly right to the exclusion of the others. The informational model explains some committees and committee systems, but not others. This mixed finding should not be surprising. There are simply too many good reasons for a legislature to divide itself into committees for any single theoretical explanation of committee organization to explain all committee composition. Furthermore, state legislatures differ widely. They are at different points in their states' opportunity structures, which themselves are quite variable. They exist in constitutional structures and social settings that differ greatly from one another. While legislatures deal with many policy areas in common, they also must deal with their states' unique social and policy problems. The legislators themselves range from professional politicians at the height of their

careers to homemakers, retirees, and students. Given all of these differences in the legislatures and their states, it makes sense that they would organize themselves in different ways to meet their own particular needs.

This mixed finding creates a great opportunity for future research. Instead of a world where committees all fit neatly into one analytical pigeonhole, we find a world where different legislatures organize their committees along different lines and for different reasons. This variation cries out for theoretical and empirical explanation. We should devise theories that predict which kinds of committees will be more or less representative of their parent chamber. Cox and McCubbins (1993) took an important step in this direction with their theoretically derived prediction that control committees should have more representative majority party contingents than committees that serve a narrower clientele. We should also develop theories of systematic chamber-wide variation in committee representativeness. Overby, Kazee, and Prince (n.d.) began doing just this by regressing the proportion of committees that are unrepresentative at a 0.05 level against a set of regressors including party competitiveness and professionalization, but they found that their models performed poorly. In addition to empirical models, future work should focus on theoretical questions. What underlying institutional structures, if any, are influencing legislatures' various committee system choices? If the committee party ratio is one such structure, why do chambers choose varying party ratios? Do the observed differences in representativeness ultimately spring from differences in constitutions and legislative rules, from social, economic, and demographic differences among the states, or from something else? And what are the effects of all this variation in committee representativeness? Do chambers with different committee make-ups enact different policies or behave in different ways? While my data have provided no definitive answer for how legislative committees are organized, the variation I have exposed provides political scientists with a wealth of new puzzles to solve.

ENDNOTES

1. I use the W-NOMINATE executable available on Poole's web site, http://voteview.uh.edu (as of publication).

2. Not all studies that compare means do so for practical reasons. Aldrich and Battista (2002) argue that comparing means is a better theoretical approach because medians-based tests are only appropriate if the legislature's preferences are unidimensional.

3. Overby, Kazee, and Prince (2003) are clearly aware of this, but they place less emphasis than I do on their concentration into a few chambers.

4. Overby, Kazee, and Prince (2003) assemble an ordinary least squares regression model to explain the percentage of 0.05–level outliers in each chamber, but it does not perform well, insofar as a low R^2 indicates poor performance. However, since most of their chambers have zero or very few outliers, a linear model may not be appropriate.

5. Democrats were the majority party, except in the Iowa House and New Hampshire Senate. The Nebraska legislature is not included in these tables because it is nonpartisan.

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