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Committee Bias**



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The American Political Science Review, Vol. 84, No. 4. (Dec., 1990), pp. 1149-1166.

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THE COMMITTEE ASSIGNMENT PROCESS AND THE CONDITIONAL NATURE OF COMMITTEE BIAS

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The view that congressional committees tend to be biased subsets of their parent chambers provides the foundations for a substantial body of theoretical literature on distributive politics and legislative structure. More recent revisionist work suggests that committees composed of preference outliers are in fact rare. We reject the categorical account of preference outliers a priori and elaborate conditions under which committees should be unrepresentative of their parent chambers. We argue that the most widely available and frequently used data—floor roll call votes—are inappropriate to the task of assessing outlier predictions in any form. Finally, we conduct a differentiated set of hypothesis tests within one policy jurisdiction to illustrate the characteristics of evidence and analysis necessary to evaluate alternative theoretical accounts of legislative organization. The appearance of policy-relevant biases in congressional work groups, we conclude, is not so much rare as it is conditional, and we suggest several conditions on which future models of legislative organization should build.

Students of Congress have long held that the organizational structure of the institution is not politically neutral, that the committee system in particular produces systematic biases in congressional policy making. Within certain important limits, members have considerable influence in selecting the committees on which they serve, so that, over time, many committees are populated with legislators who have strong interests in matters within their work groups' jurisdictions and presumably strong attachments to the programs that these groups oversee. This account has attained the status in some quarters of a stylized fact and provides the foundations for a substantial body of theoretical literature on distributive politics and legislative structure (e.g., Shepsle and Weingast 1987; Weingast and Marshall

1987), as well as periodic calls for democratic reform (e.g., Ornstein 1974; Rieselbach 1986). More recent and more comprehensive empirical work suggests, however, that this account may be a stylized fiction—that committees composed of preference outliers are in fact rare (Krehbiel 1990, 159). To the extent that this revisionist view is correct, its implications are central to our understanding of decision making and political representation in what is, ostensibly, a majority-rule institution.

We contend that both the stylized account of committees as preference outliers and the revisionist view—that preference outliers are rare—mischaracterize the reality of legislative organization. Extending the literature on committee and subcommittee assignment processes, we establish the theoretical foundations for

rejecting the categorical account of preference outliers *a priori* and elaborate a more differentiated set of conditions under which we should predict committees to be unrepresentative of their parent chambers. Second, we argue that the most widely available and frequently used data—floor roll call votes—are inappropriate to the task of assessing outlier predictions in whatever form, demonstrating how problems in measuring preferences conspire to produce null results. Finally, we conduct a specific and differentiated set of hypothesis tests within one policy jurisdiction, thereby illustrating the characteristics of evidence and analysis necessary to evaluate alternative theoretical accounts of legislative organization. The appearance of policy-relevant biases in congressional work groups, we conclude, is not so much rare as it is conditional; and our analysis suggests several conditions on which future models of legislative organization should build.

The Conditional Nature of Committee Bias: Theoretical Foundations

Are committees composed of preference outliers? In addressing this question two possible tacks are available. One tack is to formulate the outlier hypothesis as a more or less categorical claim and test it for all committees. In some respects, this is not an unreasonable point of departure. Stylized accounts of committees have taken a central place in formal theories of legislatures; and there is some tendency among empirical scholars, in moments of summary description, to overstate the evidence of committee bias. In a recent and important paper, Krehbiel (1990) thus subjects the categorical claim to a systematic examination and finds it seriously wanting. In so doing, he provides a necessary corrective to unwarranted simplifications in both formal and empirical research.

Several arguments suggest that a more differentiated set of outlier hypotheses is important, however, if we are to move beyond the enterprise of accepting or rejecting stylized claims and develop a satisfactory theory of legislative organization.¹ First, is Fenno's introductory postulate on the study of committees, committees differ, and the implication he derives from it, that political scientists' search for explanation is subverted by "empirical generalizations . . . uttered as if [they] were equally applicable to all committees" (1973, xiii-xiv). Second is the empirical literature on committee bias that questions whether the stylized fact commonly employed in formal models of legislatures approximates what legislative scholars actually claim. Most statements of the outlier view only suggest that "*many committees are biased . . . in one way or another*" (Davidson 1981, 111, emphasis ours). Third and most importantly, the literature on committee and subcommittee assignment processes provides theoretical foundations for rejecting the categorical claims *a priori* in favor of more precise and more clearly justified conditional claims. While this literature does not provide a fully developed, condition-rich theory of preference outliers, it does (to paraphrase Fiorina) provide theoretical guidance regarding precisely what we expect to find and where we expect to find it (1974, 25). It thus permits us to push the empirical study of this important question beyond Krehbiel's useful beginnings.

We extend here the most sophisticated work in this area, Kenneth Shepsle's *Giant Jigsaw Puzzle* (1978; see also Rohde and Shepsle 1973). Three important components to Shepsle's theory bear reviewing here. First, committees are "*jurisdiction-specific* subunits of their parent chamber" (Shepsle 1978, 4; emphasis in original). Second, the specific nature of the "*jurisdictional distinctions*" leads members with different interests to evalu-

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ate the benefits of different assignments differently (pp. 4–5, 45). These subjective evaluations drive members' seeking of assignments and, given the tendency of the party committees on committees to accommodate member requests, produce a process that can be characterized, in the main, as self-selective. As Shepsle summarizes, "It is the jurisdictional characteristics which attract the distinctive membership" (p. 5). Third, Shepsle's is a theory of "constrained maximization in an institutional setting" (p. 7; emphasis ours). Self-selective efforts on the part of assignment seekers and accommodationist tendencies on the part of party leaders occur in the context of four significant procedural constraints: (1) House members are typically limited to no more than two major committee assignments; (2) while committee sizes are adjustable at the margin, the number of seats on each committee is limited by the majority party to some relatively small proportion of the House membership; (3) the committees on committees place limits on the number of representatives from a single state or zone that can sit on a particular committee; (4) most importantly, a long-standing House rule regarding party representation requires that with few exceptions, committees exhibit party ratios in rough proportion to the ratio evident in the parent chamber.

What, then, does committee assignment theory suggest in the way of hypotheses about preference outliers? First and most generally, nothing in this theory gives rise to the view that self-selection operates automatically and uniformly, notwithstanding the popularity of stylized accounts about homogeneous preference outliers. Second and more specifically, there is no theoretical reason to expect that self-selection will produce a policy bias in committees that lack a distinctive policy jurisdiction, such as House Administration, Rules, or Standards of Official Conduct. Tests for such commit-

tees are thus tests of predictions that committee assignment theory does not make. Third, Shepsle's theory implies that a self-selection-generated bias will be contingent on the breadth and diversity of a committee's jurisdiction. There are two reasons for this prediction. First, committees with broad, diverse jurisdictions necessarily evoke the interests of large numbers of members, leading to a substantial competition for available slots. Interest-driven self-selection thus plays a much smaller role in the determination of committee composition not because of any institutional unwillingness to accommodate member requests but because of the constraints imposed by the assignment preferences of other interested members. Throughout Shepsle's discussion of the assignment process, slot availability and competition are important and recurrent themes; and he suggests that they matter most for understanding assignment to exclusive, or otherwise broad-scope committees. The second reason for this prediction is simply that the more diverse a committee's jurisdiction, the less it can be characterized as the policy-specific unit that might generate an identifiable bias of interests. If a small subset of members seek assignment to House Energy and Commerce largely because of their district's interest in foreign trade, another subset because of their interest in clean air, and still others because of their interests in telecommunications policy, natural gas pricing, or health care, then the probability that the full committee will appear biased along any one of those dimensions is diminished.

Of course, how one discriminates between broad, heterogeneous jurisdictions and more narrow, homogeneous ones is not obvious; and Shepsle provides no systematic method for doing so (for one attempt, see Hinckley 1975). But while fine distinctions are not easy, several House committees clearly fall into the broad scope category: Appropriations, Ways

and Means, Budget, and Energy and Commerce. Indeed, Krehbiel (1990) takes note of Davidson's (1981) observation that the first three of these are exceptions to the outlier account; and various observers have emphasized the "octopus-like quality" of Commerce's jurisdiction (Shepsle 1978, 88; see also Stanfield 1988). Hence, if these committees fail to confirm the outlier hypothesis (as all of them do, according to Krehbiel's first results), they can hardly be taken as evidence that the theoretical account of self-selection is incorrect, though they do serve to remind us that stylized renderings of that account are, in fact, stylized. A theoretically more defensible formulation of the outlier hypothesis, on the other hand, replaces categorical with conditional claims: the second condition deducible from assignment theory is simply that a committee-level bias will depend on the jurisdictional heterogeneity of the committee.

This is not to say, however, that one can necessarily dismiss the matter of work group bias for committees that enjoy a policy jurisdiction broad in scope. When committee jurisdictions are diverse enough to attract different members pursuing different sorts of interests, the committee may differentiate itself in such a way that interested subsets of the committee form, and enjoy disproportionate influence over more discrete parts of the larger committee jurisdiction. Indeed, in the last two decades this has increasingly happened through the creation and institutionalization of subcommittees (Haeberle 1978), especially in the House (Smith and Deering 1984). Moreover, the subcommittee assignment process is governed by self-selection far less constrained than in the committee assignment process. Given that within-subcommittee seniority is not the basis for ascension to subcommittee leadership positions, members are not limited by the expected costs of queue switching and subcommittee sizes are far

more easily adjusted to bring the supply of seats in line with member demand (Hall 1990). Within committees, in turn, subcommittees tend to exert disproportionate influence over matters within their jurisdictions (Hall and Evans 1990).² In our view, then, a theoretically more plausible statement of the outlier account suggests a third condition: the appearance of a policy-relevant bias in jurisdictionally diverse committees is more apt to occur at subcommittee level than at full committee level.³

The case for focusing on subcommittees thus turns on the premise that one's analysis should be tailored to theoretically significant features of the legislative organization. But more generally, the committee-subcommittee distinction raises the matter of the level at which the analysis of bias is appropriately pitched. If, as Smith (1988) points out, significant policy biases vary with the issue, theoretical statements about work group bias that do not specify the issues for which the statement holds are as apt to mislead as to inform. Committee (or subcommittee) jurisdictions that include qualitatively different issues may be biased in predictable but different directions depending on the issue. Consider the case of the food stamps program in the House Agriculture Committee. Relative to the rest of the committee's jurisdiction, food stamps is the odd program out—a redistributive program for the urban poor in a committee "dominated by members with a primary interest in serving their rural constituents" (Ferejohn 1983, 137). As a consequence, one would not expect that committee members' preferences would be inherently more favorable toward the program than the parent chamber. To the extent that members' preferences regarding federal nutrition programs are based on constituency demand, in fact, one would be more likely to predict the reverse. This is not simply an ad hoc observation; the general principle at work in producing

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this hypothesis derives directly from the theory of self-selection, which posits that committees have "distinctive" jurisdictions. Programs that are relatively unrelated to the major or salient dimensions of the committee's jurisdiction (as perceived by interested assignment seekers) predictably will not enjoy a bias in their favor; indeed, findings of such null results should be counted as exceptions that prove the self-selection rule. If one is to assess the outlier hypothesis properly, then, an issue-specific analysis is almost certainly preferable and, on theoretical grounds, often necessary. Even (committee or subcommittee) jurisdiction-specific indicators, in other words, may mask significant biases in committee preferences with respect to specific but important issues, bills, or sub-jurisdictions.

To summarize, we strongly agree with Krehbiel's (1990) recent claim that the stylized account of preference outliers mischaracterizes the reality of congressional organization. But we also suggest that the study of committee bias must move beyond the testing of stylized claims in favor of a more differentiated and theoretically justified set of outlier hypotheses. To Fenno's admonition about committee-level generalizations, we would simply add that hypotheses uttered as if they were equally applicable to all subcommittees and all issues may also tend to mislead. In the next two sections, we shall show how attention to such distinctions directly affects the substantive conclusions one is likely to reach about committee bias.

Problems of Data and Method in Testing Outlier Claims

The enterprise of testing alternative theoretical claims about committee bias is plagued by a number of complications. The unobservability of preferences requires that the researcher confront com-

plicated inferential problems when using behavioral data, and the development of appropriate statistical tests has received remarkably little attention. On both counts, Krehbiel's (1990) analysis of preference outliers provides important advances over existing work on the subject. We find especially important his argument that we distinguish between interest intensity and preference homogeneity. As Krehbiel points out, there are at least two plausible conceptions of committees as outliers. In one, the Niskanen (1971) model, committees are homogeneous, composed of those with relatively high demand for programs to serve their particularized needs; while in the other, we might merely expect to see those with relatively intense interests in an issue represented on the relevant committee, with no clear expectation that these interests would be mutually compatible. By elaborating the intensity-homogeneity distinction, Krehbiel provides an important guide to the empirical study of policy-relevant committee bias.

Important problems of data and method remain, however, and these problems conspire to produce no-difference conclusions. Our principal concerns follow directly from the use of roll call voting data to test differences in committee-chamber preferences. The first and most damaging problem is that not only are roll call votes error-filled measures of member preferences (Jackson and Kingdon 1990), but the measurement error is systematically related to the principal variable of interest—committee membership. Reconsider the outlier account in its most stylized form: (1) committee jurisdictions are sufficiently differentiated to permit a clear matching of member interests to assignment portfolios; and (2) members select their own committee assignments, so that committee members are significantly more likely to support programs within their jurisdictions and more likely to hold those preferences with

greater intensity. Under such a scenario the conditions for logrolling between committee members and nonmembers are ripe. Members sacrifice their weakly held preferences on matters outside their own committees' jurisdictions, taking their cues from relatively intense committee colleagues, in exchange for reciprocal treatment on matters before their own panels or other favors that may not involve votes (e.g., holding a field hearing, cosponsoring a bill, helping with a campaign). Indeed, such bargaining is often cited as an important means by which committees and other intense preference minorities build majority coalitions (e.g., Jackson 1974; Kingdon 1981, 100-101; Matthews 1973), despite the fact that incentives to defect from prearranged bargains render such coalitions somewhat fragile (e.g., Ferejohn 1983; Weingast and Marshall 1987). In addition, even where no implicit or explicit exchange operates, nonmember scrutiny of committee proposals entails time, staff, and information costs—costs that are less likely to be incurred by nonmembers expecting only limited benefits from pursuing preferences on what for them are relatively low salience issues. "Going along" on the floor, then, is not simply a norm that one abides in order to "get along"; it is the consequence of rational calculations by committee nonmembers about how to best spend their legislative time (Hall 1990).

In short, if one imagines a classical scenario under which committees are biased and powerful (i.e., committee members' preferences tend to be extreme and intense and their preferences are deferred to by nonmembers), we would expect to find nonmember voting patterns that mirror committee member patterns—a no-difference result! What Krehbiel (1990) cites as evidence that the stylized outlier account is wrong, in other words, is consistent with a stylized view that is right. For present purposes, however, one need not accept that intercommittee recip-

rocity is widespread (nor that existing theory adequately explains how the incentives to renege are overcome), only that its occurrence is nontrivial: to the extent that it occurs at all, roll call data will understate committee-chamber differences.

Even were roll call data appropriate to the task of measuring preferences, the use of interest group-generated indexes further inflates the likelihood of a no-difference finding. Such indexes are simply not well tailored to the jurisdiction-specific hypotheses being tested. This is inherently true of Americans for Democratic Action (ADA) scores. As we discuss above, an important foundation of the theory of self-selection is that committees are jurisdiction-specific units; it is the nature of a committee's jurisdiction that makes it more or less appealing to particular subsets of members. Committee assignment theory, therefore, does not lead one to predict that committees will tend to be biased along some scale that is not jurisdiction-specific—that in fact purposely collapses policy domains into a single dimension. Krehbiel's ADA-based analysis of committee-chamber differences for the House's 22 standing committees thus lacks grounding in the theoretical literature that gives rise to outlier predictions in the first place, a problem that cannot be addressed by recommending "extreme caution" in interpreting the results (Krehbiel 1990, 155).

While more defensible on theoretical grounds, more specialized group ratings are subject to similar validity problems. Even the best of these ratings are still not jurisdiction-specific. For example, the National Farmers Union (NFU) rating, which according to Krehbiel provides the "most jurisdiction-specific information of all ratings" (1990, 157), typically includes votes on topics that extend well beyond the jurisdictional boundaries of the agriculture committees, such as passage of omnibus appropriations bills, balanced

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Table 1. Committees As Voting Outliers Revisited: The Case of the Senate Committee on Agriculture, Nutrition, and Forestry, 99th Congress

Voting Index	Committee Median	Chamber Median	Difference in Medians	Committee Member Mean	Committee Nonmember Mean	Difference in Means
Americans for Democratic Action Index	20	45	-25	33.9	44.3	-10.4
National Farmers Union Index	58	58	0	49.8	52.9	-3.1
National Farmers Union Index "Corrected"	75	54	21	53.9	53.7	.2

Sources: Michael Barone and Grant Ujifusa, *The Almanac of American Politics 1988* (Washington: National Journal, 1987); "How Special Interest Groups Rated Senators," *CQ Weekly Report*, 22 November 1986 and 22 August 1987; "1985 Voting Record" 1986a; "1986 Voting Record" 1986b.

budget amendments, MX missile development, and social security financing (see "1981 Voting Record" 1981; "1985 Voting Record" 1986a; "1986 Voting Record" (1986b). Likewise, the AFL-CIO's Committee on Political Education score typically includes votes on such matters as federal food stamp benefits, natural gas decontrol, and voting rights—none of which fall under the jurisdiction of either the House or Senate labor committees (see e.g., Kirkland 1986a, 1986b). To the extent that these ostensibly policy-specific indexes are based on issues unrelated to the relevant committee's jurisdiction, they introduce measurement error that will systematically diminish the likelihood that a statistical test will establish a committee-chamber difference when one exists.

Table 1 illustrates this point. In order to show how even the most jurisdiction-specific ratings can bias the results in favor of the null hypothesis, we revisited the case of the Senate Agriculture Committee in the Ninety-ninth Congress. In addition to replicating Krehbiel's analysis for ADA and NFU scores, we created a "corrected" NFU score eliminating all votes on which the NFU rating was based that clearly did not deal with agriculture, nutrition, or forestry issues—fully one-third of the votes NFU selected for the

two-year period.⁴ The comparisons of committee and chamber means and medians are reported in Table 1. As a quick review of the table reveals, the committee-chamber comparisons look very different depending on whether the index is constructed to be jurisdiction-specific. Where the index includes votes that have little specific relevance to Agriculture's jurisdiction, one is led to infer that the committee is conservative with respect to agricultural programs, exhibiting a difference in median ADA scores of -25. When the index is based on votes drawn heavily but not exclusively from agricultural issues (the score that NFU reports), the committee and chamber medians are identical. Where the index is restricted solely to agriculture votes, however, the difference in medians jumps to 21, dramatically reversing Krehbiel's tentative inference that Agriculture is, if anything, more antiagriculture than the Senate as a whole (1990, 158). The pattern for the difference-in-means test is less striking, with the observed difference changing from clearly negative, using ADA scores, to slightly positive, using the "corrected" NFU score; but the results for both medians and means clearly demonstrate the no-difference bias inherent in indicators that are contaminated by votes unrelated (or only weakly related) to the relevant

committee's jurisdiction.

We conclude this section with a comment about the standards of demonstration appropriate to evaluating alternative theoretical claims about committee bias. At the core of Krehbiel's argument is a set of hypothesis tests that are ostensibly simple and convincing. In particular, he sets up a committee-chamber difference-in-means test, employs the conventional .05 significance level, and finds that the null hypothesis can be rejected in exceedingly few cases, regardless of the year or chamber being studied. Such findings, in turn, lead him to make the substantive claim that biased committees are rare (1990, 159) and to suggest that such findings provide encouragement to incomplete-information theories of legislative organization, which predict that Congress will prefer unbiased committees because of their greater willingness to share policy expertise (p. 160).

Based on Krehbiel's own results, neither point is justified. The problem derives from the fact that given his substantive thesis, Krehbiel minimizes the wrong type of statistical error. As Julnes and Mohr summarize the long recognized but often forgotten principle, "Whereas traditional conclusions of 'difference' seek to minimize Type I errors, no-difference findings need to minimize Type II errors (i.e., failing to reject the hypothesis of no difference . . . when it is in fact false" (1989, 629; see also Blalock 1979, 159-65). By setting the significance level at the conventional .05, in other words, Krehbiel unintentionally biases the interpretation of his results in favor of his principal argument and, by implication, an alternative theoretical perspective on legislative organization. Where one wishes to make a no-difference claim (more accurately, an "approximately-no-difference" claim), as Krehbiel does, the researcher must use a considerably larger alpha level; Julnes and Mohr recommend .25, one-tailed (1989, 641-44; see also Blalock 1979, 161). Had

he used this more appropriate standard (t -statistic $\geq .68$), Krehbiel's conclusions would have appeared very different: despite the no-difference bias associated with the voting indexes, seven of ten committees pass the threshold for at least one of the relevant "policy-specific" ratings that he employs. Hence, Krehbiel's conclusion that "few contemporary committees are composed predominantly of high-demand preference outliers" (1990, 159) is simply not supported by his results. More generally, the evaluation of theories of legislative organization that predict representative committees will require a different set of standards and a more complicated set of tests than those supporting conventional outlier predictions. Likewise, theories that suggest more conditional claims, including, say, hypotheses of difference for some committees and some issues and no-difference hypotheses for others, will require statistical standards that vary, test by test.

In summary, interest group indexes are not simply imperfect indicators, they suffer from measurement error that systematically understates differences between committee and chamber. More generally, they reflect the no-difference bias associated with any measure of preferences based on roll call data, thereby inflating the probability of retaining the null hypothesis when it is in fact false. Finally, Krehbiel's misapplication of conventional significance levels seriously inclines him to make errors of this kind. We shall show that that is precisely what Krehbiel has done in the case for which his evidence against committee bias appears strongest.

Testing for Preference Outliers: A Detailed Look at Senate Agriculture

Having taken issue in the last section with the data and specific tests that Krehbiel employs to investigate commit-

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tee bias, we shall show how different evidence and more appropriate tests suggest quite different substantive conclusions. While the type of data we use will not be appropriate to tests for all committees, our approach does reveal the characteristics of evidence and analysis that are necessary for one to determine whether committees are preference outliers in the ways that assignment theory predicts. The first and most important point is that one must be able to infer policy-relevant biases from the data without making theoretically unreasonable assumptions about measurement error. Although this is seldom easy to do, we believe that attention to this principle will prove central to future attempts to evaluate alternative theoretical accounts. Second, the data must be, at the very least, jurisdiction-specific and, where jurisdictions are particularly diverse, issue- or program-specific. Finally, one should pitch the investigation at the proper level of institutional analysis or, when in doubt, at each of several plausible levels in order to evaluate the robustness of the tests.

To illustrate our approach, we focus on the Senate Agriculture Committee for the Ninety-ninth Congress. In choosing this focus, we do not mean to suggest that Agriculture is a difficult test case for some general outlier prediction that might thereby serve to rehabilitate the stylized view of biased committees. As we have already suggested, we strongly believe that there are sound empirical and theoretical reasons for rejecting such an account a priori. In fact, Agriculture is one committee that, given the widely acknowledged constituency orientation of its members and the relative homogeneity of its jurisdiction, both empirical and theoretical scholars are apt to predict a finding of significant policy bias.

Rather, the choice of Senate Agriculture is particularly appropriate for our purposes in that it is a case specifically biased against our critique of Krehbiel's

analysis and conclusions, most of which deal with House committees. First, by focusing on the Senate, we dramatically reduce what serves as the sample sizes for the hypothesis tests, given that the Senate membership is less than one-fourth that of the House and that the average committee membership in the Senate is less than one-half that in the House. (House Agriculture numbered 43 in the Ninety-ninth Congress, Senate Agriculture 17.) Hence, the ability to achieve statistical significance using Krehbiel's tests (even though we are dealing with the full populations of both chamber and committees) is necessarily diminished. Second, given that the Senate committee system constitutes a less differentiated division of the total policy domain (16 standing committees versus 22 for the House) and given that Senators' constituencies are clearly more heterogeneous, we should expect that an accurate matching of member interests to assignment portfolios will be inherently more difficult. Finally (and most importantly), Krehbiel specifically concluded that the Senate Agriculture Committee was "somewhat (but not significantly) more *anti-agriculture* than the Senate" (1990, 158; emphasis in original)—an anomalous result the likes of which he reported for no other committee. In short, should our evidence suggest that Senate Agriculture is significantly *pro-agriculture*, the validity of Krehbiel's tests for other committees, especially those in the House, will be called into serious doubt.

In order to investigate the possible policy biases of Senate Agriculture, we rely on data that reflect the economic interests of committee members' and nonmembers' constituents in committee programs (see also Arnold 1979, chap. 6; Cowart 1981). Employing such data requires that we make two explicit assumptions. First, we assume that differences in members' preferences are, in large part, based on differences in their geographic constituencies. While we do not believe that this is a

reasonable assumption for all issues in all committees, we do believe that it is relatively noncontroversial in the area of agricultural policy.

The second assumption has to do with the important distinction between preferences and interests. In general, indications of constituent interest in a program or jurisdiction mainly suggest that the member will be interested and therefore active (Hall 1987); they do not necessarily tell us what policy position he or she will prefer. Whether one can make inferences about preferences from constituency data depends upon the nature of one's indicator and the extent to which there is significant division of policy opinion among interested constituents or subconstituencies. For instance, one cannot assume that public lands acreage in a member's district necessarily evokes constituents' conservationist attitudes; it might reflect private development pressures on the member as well. More generally, it is not always obvious how members' complex perceptions of their constituencies (e.g., geographic, electoral, partisan) affect their preferences on specific issues (Fenno 1978). In order to make inferences about member preferences, therefore, one may require more refined measurement reflecting the balance of different interests or the inclination of the member to respond to one rather than another (Grofman, Griffin, and Glazer 1990).

Fortunately for our illustrative purposes, such complications are relatively modest in the agriculture case. We employ here two general indicators to capture senators' preferences for federal aid to agriculture: (1) total U.S. Department of Agriculture program payments to the senator's state measured as a percentage of gross state product and (2) state agricultural debt-asset ratios. The first reflects the stake of the state economy in the maintenance and/or expansion of federal crop subsidies and other agricultural programs; we assume that *ceteris paribus*,

senators from states affected by such programs will prefer proposals that provide more such benefits to those that provide less.⁵ The second captures the severity of the agricultural debt crisis—the most pressing issue on the agricultural agenda during the Ninety-ninth Congress. In using this indicator, we assume that the greater the severity of the debt crisis in a senator's state, the more willing he or she will be (*ceteris paribus*) to support federal aid to distressed farmers in the form, say, of more generous income maintenance, Farm Credit System relief, or institutional reforms to postpone or prevent farm foreclosures. In addition, we employ similar indicators of constituency interest in several of the specific programs that fall under the committee's jurisdiction, so that we can test for hypothesized subcommittee-specific and issue-specific biases that might be masked by the more general indicators. The program-specific indicators are federal feed grains program payments, wheat program payments, dairy production (wholesale sales), conservation expenditures, and food stamp program expenditures.⁶

In sum, our data should permit us to conduct a set of hypothesis tests tailored to the specific hypotheses and levels of analysis that committee assignment theory suggests, while avoiding the inherent biases of roll call-based measures. In Table 2, we use the constituency-based measures of preferences to replicate three of Krehbiel's tests: the percentage of members in the gap between committee median and chamber median (including committee members), the *t*-statistic for the difference between committee mean and chamber mean (excluding committee members),⁷ and whether or not the committee is *homogeneous* in the sense that the committee's Democratic and Republican median members are on the same side of the chamber median. As the table shows, the Senate Agriculture Committee is clearly *not* antiagriculture. Indeed, it

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**Table 2. Constituency-based Preferences and Committee Bias:
Evidence from the Senate Agriculture Committee**

Preference Indicator	Percentage in Gap between Medians	<i>t</i> -statistic for Difference in Means	Party Medians
General indicator			
Agricultural program payments	28	2.69	homogeneous
State debt-asset ratio	10	2.66	homogeneous
Program-specific indicators			
Feed grains payments	14	2.59	homogeneous
Wheat payments	6	1.87	homogeneous
Dairy production	6	.69	homogeneous
Conservation payments	18	2.02	homogeneous
Food stamp expenditures	14 ^a	.49	heterogeneous

Note: Indicators of committee member and nonmember constituency interests were calculated from state data reported by the Department of Agriculture and the Census Bureau for 1984. N of Senate Agriculture Committee for the Ninety-ninth Congress = 17.

^aThe panel rates *lower* than the chamber.

would require a strong repudiation of the constituency-representative connection to conclude that it was anything but *pro*-agriculture. Despite the small effective sample sizes, Senate Agriculture members are significantly more predisposed to support agriculture spending and crisis relief measures: the difference-in-means test reveals differences that are statistically significant at the .01 level for both of the general indicators, and both reveal the committee to be homogeneous according to Krehbiel's relatively demanding party medians test. While it is not easy to know what standard to use, there also appears to be a substantial number of members in the gap between committee and chamber medians. In interpreting the numbers in column 1, however, it is important to keep in mind that it is by definition impossible for more than 40% of the Senate's members to fall in this gap (roughly, half the total chamber membership minus half the committee membership).⁸ Given that upper limit, the fact that 28% fall in the gap between medians for the total program payments indicator is striking indeed.

The program-specific indicators pro-

vide similar evidence of committee bias, yet they also reveal the importance of developing and testing program-specific hypotheses even for committees that, like Agriculture, exhibit relatively little jurisdictional diversity. The differences in committee-chamber means are statistically significant at .05 for three of the four agricultural program indicators, and they reveal the committee to be homogeneous without exception. As we note above, however, the food stamp program stands out as the principal exception to a jurisdiction that is generally concerned with programs for rural areas; committee assignment theory therefore does not predict that food stamps will enjoy pro-program bias. The results of Table 2 bear this out. Committee members' interests along that policy-specific dimension appear heterogeneous; and, using an appropriately higher alpha level (e.g., .25, *t*-statistic = .68) to assess a negligible-difference hypothesis, we find that the hypothesis is supported. Judging from a comparison of committee and chamber medians, in fact, there appears to be some constituency-based committee prejudice *against* the food stamp program. To the

extent that this is true, at least, any summary measure of Agriculture Committee program preferences that combines, say, food stamps and feed grains is apt to hide biases that may have important consequences for committee deliberations about one or both programs.

Table 3 explores the robustness of our results at other levels of institutional analysis. Despite the smaller effective

sample sizes (ranging from 5 to 13), such tests provide additional evidence of policy bias on Senate agriculture panels. The evidence is particularly striking for the Senate Agriculture Subcommittee on Production, Marketing, and Price Stabilization—the subcommittee with jurisdiction over all of the commodity programs and most of the committee's discretionary budget authority. Each of the relevant in-

**Table 3. Constituency-based Preferences and Subcommittee Bias:
Evidence from Senate Agriculture-related Panels**

Panel and Preference Indicator	Percentage in Gap between Medians	t-statistic for Difference in Means	Party Medians
Agriculture			
Production, Marketing, and Prices Subcommittee (N = 12)			
Agricultural program payments	34	3.65	homogeneous
Feed grains payments	34	3.14	homogeneous
Wheat payments	22	2.48	homogeneous
Dairy production	20	1.52	homogeneous
Credit and Rural Development Subcommittee (N = 7)			
State debt-asset ratio	6	1.63	homogeneous
Research, Conservation, and Forestry Subcommittee (N = 10)			
Conservation payments	16	1.08	homogeneous
Nutrition Subcommittee (N = 7)			
Food stamp expenditures	22 ^a	1.93 ^a	homogeneous ^a
Appropriations			
Subcommittee on Agriculture and Related Agencies (N = 13)			
Agricultural program payments	28	4.13	homogeneous
State debt-asset ratio	18	2.74	homogeneous
Small Business			
Subcommittee on the Family Farm (N = 5)			
Agricultural program payments	2	.63	heterogeneous
State debt-asset ratio	28	2.30	homogeneous
Membership on any agriculture-related panel (N = 32)			
Agricultural program payments	28	4.06	homogeneous
State debt-asset ratio	14	4.82	homogeneous

Note: Indicators of senators' constituency-based preferences were calculated from state data reported by the Department of Agriculture and the Census Bureau for 1984.

^aThe panel rates *lower* than the chamber.

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dicators reveals that the subcommittee is homogeneous; that a substantial number of members fall between the subcommittee and chamber medians; and with the possible exception of dairy (statistically significant at .10), that there is a substantial difference in panel-chamber means. The evidence for Agriculture subcommittees likewise reinforces our argument that any analysis that fails to distinguish between programs within particular committee jurisdictions stands to mislead us: unlike any other program in Agriculture's jurisdiction, the Nutrition Subcommittee is, in terms of consistency-based predilections, homogeneous and biased against its principal program.

In addition to the evidence for Senate Agriculture subcommittees, we also note that two other standing committees have subcommittees whose jurisdictions deal almost exclusively with agriculture issues. Hence, the failure of a member to seek or receive assignment to Senate Agriculture does not necessarily mean that a self-selection-driven, proagriculture bias does not materialize elsewhere. The two general indicators reveal the Agriculture Appropriations Subcommittee to be a homogeneous high-demander with respect to agriculture programs. In addition, the indicator that reflects constituency interest in the single issue of greatest importance to family farmers during the mid-1980s—the severity of the agricultural debt crisis—suggests that the Small Business Subcommittee on the Family Farm was also homogeneous and biased. Finally, when one tests for the bias evident in the subset of members who receive assignment to one or more of the several agriculture-related panels, the results overwhelmingly support the self-selection account.

In sum, we provide consistent evidence at several levels that the Senate Agriculture Committee and the several subcommittees with agriculture-related jurisdictions are homogeneous and biased in

ways that are likely to affect specific committee decisions, evidence that stands in stark contrast to Krehbiel's roll call-based analysis of the same committee during the same period.⁹ The strength of our evidence, of course, is itself dependent on contestable assumptions. One possible objection is that similarities in constituents' stake in particular programs may mask differences in what they think should be done (and hence in representatives' perceptions of constituent opinion). Both within and across, say, corn-producing districts, there can be substantial differences within the agricultural community on questions ranging from payment limitations to conservation compliance. We do not wish to claim otherwise, and we thereby recognize that such indicators necessarily contain measurement error. Even so, they should still capture well the member's predisposition to value federal actions or expenditures providing net benefits to agricultural producers (and to discount the net costs to taxpayers or consumers); and these predispositions can affect any number of minor and major, visible and invisible choices made during committee deliberations. To the extent the reader finds this assumption implausible, however, our results will be less convincing, and the prospects for using similar data in the study of committee bias less promising.

A second possible objection to our data owes to the fact that even on agriculture issues, preferences can be based partly on nonconstituency considerations, a general point that we also concede. Following Arnold, we adopt the view that the member's "true preference" is based on both general-benefit and local-benefit evaluations (1979, 39). By relying on data that tap district economic stake in a program, we measure only the local-benefit component of the member's preference.¹⁰ Three arguments suggest that this limitation does not seriously undermine our specific conclusions, however. First, our

analysis need not assume here that constituency considerations push out all others, only that they constitute an important variable in the preference calculation; that is, we simply assume that members' preferences are a function of some set of variables, one of which is district economic interests, and that the coefficient on that variable is positive and significant. This we take to be a more-than-reasonable assumption. Indeed, a wide range of other accounts reveal that the preeminence of constituency considerations seems especially applicable to the agriculture area (e.g., Jones 1961; Kingdon 1981, 123, 232; Smith and Deering 1984, 105-6, 116). Even, say, the atypically pure ideologue Jesse Helms strongly supports heavy-handed federal interventionism in the tobacco economy, where the financial interests of his constituents are at stake.¹¹ What we have shown, then, is that the values on the constituency variable for committee members are significantly different from the values of committee nonmembers, applying the appropriately demanding thresholds of statistical significance. Second, the homogeneity tests reported in Tables 1 and 2 demonstrate that members of both parties (and hence quite different ideological dispositions, regions, and group affiliations) appear biased on constituency grounds—that is, even were the chamber to be composed solely of Republicans, there would exist strong constituency pressures for committee members, relative to nonmembers, to pursue policies generous to agriculture interests. Third, we repeated the chamber-committee difference-in-means tests by party and found that for every indicator, both general and program-specific (except food stamps), the respective party means on the committee were greater than the party means off the committee and that in almost every case, both party's committee-chamber differences proved statistically significant at the .05 level. In short, the constit-

uency component of members' policy preferences is significantly biased with respect to committee membership even if the institution does constrain committees—through party ratio requirements—to have ideological diversity that tends to approximate the chamber.

The defense of our assumptions in the Agriculture case is not intended to recommend the indiscriminate use of similar data to test for committee bias, however. If anything, it illustrates how difficult it is to infer members' preferences from publicly accessible data; Senate Agriculture was convenient for our check of Krehbiel's analysis precisely because of the highly plausible connection between member preferences and available constituency data. At the same time, such data partly capture an outcome-relevant dimension of committee-chamber differences that Krehbiel discusses but voting data do not tap—preference intensity. The extent of the district's stake in a particular issue or jurisdiction is likely to affect not only members' policy positions but also the vigor with which they pursue their preferred outcomes in the legislative process. In fact, such differences in intensity are likely to affect forms of participation far more important (and far less observable) than the casting of roll call votes (Hall 1987, 1990). Specifically, such differences will affect the relative willingness of members to expend time, staff, and political capital in setting the legislative agenda, developing amendments, building coalitions, and a wide range of other activities at the committee, floor, and postfloor stage. Differences in preferences, in short, are not all that we need to know.¹²

Conclusion

The idea that committees tend to be unrepresentative of their parent chambers is central to much of what legislative schol-

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ars write and teach about Congress, to the strategies that interest group lobbyists and agency officials develop to pursue their legislative objectives, and to proposals long advocated by democratic reformers. The recent work of Krehbiel (1990) thus stands as an important challenge to conventional ways of thinking about Congress and, if correct, should significantly reorient the agenda for both empirical and theoretical research.

We have reinforced Krehbiel's challenge, arguing on theoretical grounds that the stylized account of committee bias mischaracterizes the nature of legislative organization. At the same time, we contend that Krehbiel's revisionist view—that preference outliers are rare—does the same. We elaborate a more differentiated set of conditions under which we should predict committees to be unrepresentative of their parent chambers; challenge the data and methods used to assess outlier claims; and, providing a detailed examination of a single jurisdiction for which we predict specific and variable policy-relevant biases, illustrate the characteristics of evidence and analysis necessary to evaluate alternative theoretical accounts of legislative organization.

Neither our general critique of Krehbiel's approach nor our own empirical analysis, then, is intended to suggest that all committees will be composed of preference outliers—much less for all issues within their jurisdictions. A number of long-standing institutional constraints limit committee self-selection; and these constraints are consistent with the important and theoretically innovative perspective being developed by Gilligan and Krehbiel (1989, 1990) and Krehbiel (1989), which suggests that a majority-rule institution achieves important informational gains by limiting committee bias. Indeed, once such constraints are in place, the majority cannot only allow the interest-driven initiation of assignment requests by individual members, it can

also allow advocacy and accommodation to characterize the behavior of zone representatives, members of the committees on committees, and party leaders—and committee bias is *still* going to be kept within some bounds. However, the theoretically significant question then becomes, To what extent, in what particular ways, and under what conditions do significant work group biases appear, given the constraints?

Our analysis suggests that several factors, often acting in combination, will affect the extent to which preference outliers will dominate specific panels on specific issues. In particular, three deserve greater attention in future research: (1) the extent to which the panel's policy jurisdiction is narrow and homogeneous, (2) the identification of specific issues within the panel's jurisdiction that evoke the concerns of a mobilized or otherwise visible constituency, and (3) the identification of issues within the panel's jurisdiction that are perceived as having concentrated benefits and dispersed costs. The first two of these follow from the extensions of committee assignment theory we have developed. The third follows from what we believe to be a necessary modification of Niskanen's stylized view of legislatures, that "advocacy is concentrated and opposition is diluted" (1971, 139). This characterization, we believe, should not be an unqualified assumption but a variable, the value of which will affect whether a committee will be biased and, if so, for which specific issues. To the extent that Niskanen's premise holds (as it does for the most part in the area of agriculture policy making), self-selection should produce a policy bias. Where both advocacy and opposition are concentrated (as in the case of, say, energy policy), self-selection is more prone to produce a committee that is roughly representative of the parent chamber.

Thus, the collection and analysis of better data relevant to other committees

should not lead to findings of substantial bias for all committees and all issues. But our critique does suggest that where committee bias exists, Krehbiel's approach will either fail to find it or understate it. In so doing, we fear that it pushes the study of legislative organization away from one caricature, that biased committees are ubiquitous, toward another, that biased committees are rare. At the same time, however, Krehbiel's challenge provides a badly needed reminder (one needed in areas well beyond the study of Congress) that stylized accounts can distort as much as inform, that strong tendencies to reify these accounts are evident in the literature, and that such reification can misguide theoretical inquiry.

In sum, while we recognize that stylized facts are necessary simplifications in the formal modeling of institutions, especially in the early stages of theory development, the next important step is to build on more sophisticated and systematic empirical work to inform the modeling enterprise. We have focused attention on party ratio requirements and other institutional constraints that limit self-selection, suggested conditions under which bias is likely to appear, and addressed significant problems of data and method in the hope of aiding future efforts in that direction.

Notes

We have benefited from the comments of John Chamberlin, Doug Dion, John E. Jackson, David C. King, John Kingdon, Keith Krehbiel, Larry Mohr, and Steve Smith and the research assistance of Gary Levenson and Gary McKissick.

1. It is important to note that in related work with Gilligan (Gilligan and Krehbiel 1989, 1990; see also Krehbiel 1989, chap. 3) Krehbiel develops an informational theory of legislative organization predicting that legislatures will generally prefer unbiased committees because of their relative willingness to share policy expertise but also that a legislature will permit outlying committees under one exceptional condition. The revisionist empirical work with which we take issue here (Krehbiel 1990) is more directly concerned with testing the *conventional*

stylized account of committee bias, not Gilligan and Krehbiel's alternative theoretical perspective.

2. It is also the case that the effective work groups that shape policy are often biased through a process of selective participation, which may or may not correspond to formal jurisdictional boundaries (Hall 1990). We confine our attention to the formal panels here, however, since they are the focus of existing institutional analysis though the examination of committee or subcommittee memberships is not dispositive with respect to the larger issue of work group bias.

3. In a larger study, Krehbiel focuses his tests at the subcommittee level for one of his broad scope committees, House Appropriations, but does not generalize this research strategy to any other committees (1989, chap. 4).

4. Although only votes involving issues *clearly* beyond the boundaries of the Agriculture Committee were eliminated, these accounted for 8 of the 24 votes that NFU selected: 3 of the 12 in 1985 and 5 of 12 in 1986. The votes eliminated for 1985 were a resolution dealing with MX missile appropriations, a joint resolution to raise the debt limit and approve the Gramm-Rudman balanced budget amendment, and adoption of the conference report to raise the debt limit and approve Gramm-Rudman. The votes eliminated for 1986 were passage of a joint resolution to propose a balanced budget amendment to the U.S. Constitution; an amendment to approve aid to the Nicaraguan Contras; an amendment to prevent the deregulation of old natural gas; an amendment supporting the deductibility of state and local taxes for federal tax purposes; and an amendment to reform campaign finance regulations for congressional campaigns. See "1985 Voting Record" 1986a and "1986 Voting Record" 1986b.

5. We use data on federal program payments on the assumption that they best capture the state's stake in the program and hence indicate the member's likely "demand" for program benefits. An alternative approach is to use general and crop-specific measures of agricultural production. We replicated our tests using such data; and in every instance those results provided evidence of a pro-agriculture bias at least as strong as those we report in Tables 2 and 3. By using the program payments data, then, we employ an indicator that we believe makes theoretical sense; but in any case, the choice of program over production data does not affect any of our general or specific substantive conclusions. On the contrary, the fact that the two different kinds of data produce similar results reinforces our principal conclusions.

6. Data on agricultural debt-asset ratios, total federal program payments, and (with the exception of food stamps) the program-specific indicators are based on data from *Economic Indicators of the Farm Sector, State Financial Summary* for 1984, the year immediately preceding the period during which

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committee assignments for the Ninety-ninth Congress were made. Data on state food stamp expenditures were taken from "Federal Expenditures by State" in the Bureau of the Census: *Governments Annuals and Biennials*. We use dairy production data rather than dairy program payments data to assess the extent of a prodairy bias because, due to the indirect nature of dairy subsidies, data on payments going to state dairy producers are not available. With the exception of agricultural debt-asset ratios, each indicator was defined as a percentage of gross state product, the data for which were taken from "Gross State Product by Industry, 1963-1986," in the Bureau of Economic Analysis' *Current Periodicals* for 1988.

7. Given that we intend to draw a conclusion of *difference*, it is incumbent on us to adopt a demanding statistical standard, i.e., a low alpha level. We focus at the .05 level, one-tailed, which requires a *t*-statistic of approximately 1.66, though we also note when key results pass either the .01 or .10 thresholds (*t*-statistics of 2.36 and 1.29, respectively).

8. For the House, the maximum percentage in the gap averages closer to 45%.

9. In addition to the tests reported here, we also conducted tests for committee chamber differences for several more minor programs. Even using these indicators, additional evidence of a self-selection bias appeared. For instance, the difference in means between chamber and committee was statistically significant at .10 for both rice and cotton programs, while the difference in means between the chamber and the subset of members who held assignment to any agricultural panel was statistically significant at .05 for these same two programs. (However, the difference was correct in sign but statistically insignificant for Wool Act payments, regardless of the subset analyzed.) In addition, comparable tests using agricultural production rather than program payment data produced statistically significant differences between chamber and agricultural panel members for feed grains, rice, wheat, cotton, peanuts, and soybeans, as well as for all agricultural crops combined. The only major agricultural products subsidized by the federal government (and for which we had production data) that failed to exhibit a statistically significant panel-Senate difference were wool and tobacco.

10. In addition, our constituency indicator fails to capture constituents' preferences for public goods (Jackson and King 1989). With the exception of nutrition policy (which evokes some constituents' preferences for public attention to basic needs and other constituents' preferences regarding governmental waste, fraud, and abuse) and certain environmental issues with Agriculture's jurisdiction (e.g., pesticide use, groundwater pollution), public goods preferences are seldom prominent in the dialogue between the member and the subconstit-

uency that takes an interest in his or her actions in the agriculture area.

11. Note that the Helms example provides us not simply with anecdotal evidence but a limiting case: if the prototypical ideologue suspends his ideological predispositions when constituents' agricultural interests are salient, it is relatively safe to assume that other members do as well.

12. We thus reaffirm Krehbiel's emphasis on the direction of preference/intensity of preference distinction; but we strongly disagree with his assertion that "if preference outliers . . . do not exist . . . then the existence of intense or high salience outliers would be inconsequential for the ultimate distribution of policy benefits" (1990, 5). In fact, this assertion holds only in the unusual case in which the distribution of intensities mirrors the distribution of preferences; that is, in the determination of policy outcomes the teeter-totter principle holds: the outcome depends on the ratio of weight times distance and weight times distance. Krehbiel asserts that only distance matters. Physical science holds otherwise, and political science suggests that, in the United States at least, intense minorities' preferences usually rule, not majorities' (Dahl 1956, chap. 5).

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