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Theory Driven Bias in Ideal Point Estimates—A Monte Carlo Study

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This paper analyzes the use of ideal point estimates for testing pivot theories of lawmaking such as Krehbiel's (1998, *Pivotal politics: A theory of U.S. lawmaking*. Chicago, IL: University of Chicago) pivotal politics and Cox and McCubbins's (2005, *Setting the Agenda: Responsible Party Government in the U.S. House of Representations*. New York: Cambridge University Press) party cartel model. Among the prediction of pivot theories is that all pivotal legislators will vote identically on all successful legislation. Clinton (2007, Lawmaking and roll calls. *Journal of Politics* 69:455–67) argues that the estimated ideal points of the pivotal legislators are therefore predicted to be statistically indistinguishable and false when estimated from the set of successful final passage roll call votes, which implies that ideal point estimates cannot logically be used to test pivot theories. I show using Monte Carlo simulation that when pivot theories are augmented with probabilistic voting, Clinton's prediction only holds in small samples when voting is near perfect. I furthermore show that the predicted bias is unlikely to be consequential with U.S. Congressional voting data. My analysis suggests that the methodology of estimating ideal points to compute theoretically relevant quantities for empirical tests is not inherently flawed in the case of pivot theories.

1 Introduction

In the past two decades, unidimensional spatial models of lawmaking have come to play a central role in empirical studies of the U.S. Congress. Such models typically assume that legislators have (1) spatial preferences over a unidimensional set of policy outcomes, and (2) complete and perfect information about each others' preferences and the link between policy choices and outcomes. In a prominent subset of these models, the formal rules governing proposal and amendment procedures give rise to multiple *pivots*, or legislators whose consent is a necessary condition for policy change. Pivot models have proven extremely powerful for parsimoniously characterizing how the preferences of legislators interact with institutional arrangements to determine policy outcomes. In particular, they have played a central role in the ''parties vs. preferences debate'' (Krehbiel 1998; Cox and McCubbins 2005) over whether Congressional policy outputs are ''merely by-products of legislators' preferences'' or ''the result of strong partisan activities and pressures'' (Volden and Wiseman 2010).

Unidimensional spatial models are particularly amenable to empirical testing because each legislator's policy preferences are fully characterized by his most preferred policy or *ideal point*. Accordingly, political methodologists have developed several powerful statistical methods such as DW-NOMINATE (Poole and Rosenthal 1985, 1997) and IDEAL (Clinton, Jackman, and Rivers 2004) for estimating ideal points from Congressional roll call data. However, many scholars have cautioned against the use of ideal point estimates for testing lawmaking theories because the estimates are themselves ''endogenous to the legislative context'' (Shepsle and Weingast 1994). In other words, the theories make predictions about the lawmaking behavior generating the data from which ideal points are estimated that can

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contradict the assumptions underlying ideal point estimation. The difficulty is obvious in the case of theories that predict parties directly pressure members' votes; if such a theory is true, then the estimates used to test it are already biased by party influence (Groseclose and Snyder 2000; McCarty, Poole, and Rosenthal 2001).

Despite this difficulty, a now sizable literature uses ideal point estimates to construct competing empirical tests of nonpartisan and partisan pivot theories of lawmaking (e.g., Chiou and Rothenberg 2003; Covington and Bargen 2004)—in particular, Krehbiel's (1998) pivotal politics and Cox and McCubbins's (1993, 2005) party cartel models. The theories in these tests appear to be exempt from the endogeneity critique because they retain the prediction that legislators vote *sincerely*, that is, in accordance with their true policy preferences. In particular, the party cartel model assumes that party leaders shape policy outcomes by influencing the *agenda* on which legislators vote, rather than the votes themselves. Nevertheless, despite the apparent validity of the empirical tests and the starkly differing predictions of the models, the cumulative empirical evidence adjudicating between them remains mixed.

Recently, Clinton (2007) has proposed an interesting hypothesis for the failure of ideal point-driven tests to adjudicate between partisan and nonpartisan pivot theories. Clinton argues in effect that pivot theories predict legislative behavior that should result in estimated ideal points for pivotal legislators that are statistically indistinguishable, and therefore that pivot theories are also subject to the endogeneity critique. The reason, loosely speaking, is that if a pivot theory is true then votes that result in successful policy change should not divide pivotal legislators; if any pivotal legislator had indeed opposed what became successful legislation, he would have previously blocked it. This in turn appears to imply that the ideal point estimates extracted from these votes should not distinguish the preferences of the pivotal legislators. Clinton goes one step further and carries out the empirical test suggested by his argument for pivotal politics and the party cartel model. He estimates the ideal points of House members from a subset of votes he identifies as successfully resulting in policy change, finds that the estimated pivots implied by both theories are strongly statistically distinguishable, and concludes that neither one is supported by the data.

Although Clinton only formally tests pivotal politics and the party cartel model, the scope of his argument is significantly broader. If correct it implies that *no* pivot theory may be logically tested using ideal point estimates, a methodology that is now employed by a sizable body of empirical Congressional scholarship. In this paper I therefore evaluate the logical underpinnings of Clinton's argument. Specifically, I use Monte Carlo simulation to determine the conditions under which it is correct, and the extent to which those conditions hold for U.S. Congressional roll call data.

In the first portion of this paper, I argue that Clinton's claim that pivot theories imply incorrectly estimated ideal points is only trivially true in the sense that any model with perfect spatial voting is inconsistent with the assumption of probabilistic spatial voting underlying ideal point estimation. The probabilistic voting model assumes that legislators may make ''errors'' with respect to spatial voting due to idiosyncratic nonspatial factors that are uncorrelated across legislators and votes. I therefore argue that a fairer test of whether pivot theories predict incorrectly estimated ideal points is whether they do so when augmented with an assumption of probabilistic voting. For such a model, Clinton's claim is in fact false; the ideal point estimates of all legislators, including those who are pivotal, are consistent even when voting is nearly perfect and all pivotal legislators are arbitrarily likely to vote ''yea'' on all successful legislation. The reason is simple: when legislators vote probabilistically, consistency of ideal point estimates is independent of the agenda on which legislators vote (Lewis 2001).

In a pivot model augmented with probabilistic voting, the phenomenon that Clinton identifies is actually manifested as small sample bias, that is, in greater difficulty—rather than impossibility—in distinguishing the preferences of pivotal legislators. In the second portion of this paper, I therefore use Monte Carlo simulation to determine whether this *theory-driven bias* is likely to be consequential given the sample sizes from which ideal points are estimated and the observed pattern of voting in the U.S. Congress. Specifically, I simulate legislative sessions of the 106th House of Representatives using two different models of agenda formation. The first is a simple ''median proposer'' game in which amendments are unrestricted and the chamber median is the unique pivotal legislator (Krehbiel, Meirowitz, and Woon 2005). The second is a representative pivot model, Cox and McCubbins's (2005) party cartel game. The party cartel model assumes that the majority party can influence policy outcomes by ''gatekeeping'' legislation off the floor to prevent undesirable policy changes. In the model, both the chamber median and the majority party median are therefore pivotal. The median proposer game is used as a benchmark against

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which to compare the party cartel game because they generate identical distributions over the voting agenda but for the blocked votes. In both sets of simulations, I vary the degree to which voting is described by the unidimensional spatial model using σ^2, the variance of a normally distributed random utility shock that all legislators are subject to on all votes.

My principal findings are two-fold. First, contrary to Clinton's claim I find that the estimated ideal points of the estimated pivots recovered from the party cartel model simulations are strongly statistically distinguishable, even with nearly perfect spatial voting. Thus, Clinton's proposal that pivot theories be tested by verifying whether estimated pivots are statistically indistinguishable is not robust to even a trivial introduction of voting error. Second, I find that there is indeed theory-driven bias in ideal points estimates from the party cartel simulations (relative to the median proposer simulations), but that the extent of the bias depends heavily on the degree of voting error. When the unidimensional spatial model nearly perfectly describes voting, the bias is severe; however, it diminishes rapidly as legislators become increasingly likely to make voting errors. Strikingly, when the degree of unidimensional spatial voting in the party cartel simulations matches that which is estimated for the 106th House by Clinton, the bias in recovered ideal points is virtually nonexistent. Moreover, when the simulation exercise is replicated with legislators voting via a linear probability model, the bias vanishes entirely.

Overall, my analysis belies the claim that pivot theories cannot be logically tested using ideal point estimates, and suggests that theory-driven bias is at worst a second-order problem in testing pivot theories. Furthermore, my use of Monte Carlo simulation to characterize the effect of the theory as a maintained hypothesis on the measures used to test it demonstrates an alternative methodology for accommodating the ''endogeneity'' of ideal point estimates to lawmaking theories, an endemic problem in the empirical study of Congress.

2 Pivot Theories

In this section, I discuss the basic properties of pivot theories and the predictions they make that are consequential for ideal point estimation.

Pivot theories are a subclass of unidimensional spatial models of lawmaking with complete and perfect information. Such models assume that (1) the set of available policies X is unidimensional, that is, $X \subset \mathbb{R}$, (2) associated with each legislator $i \in L$ is a most preferred policy or ideal point $x_i \in \mathrm{X}$ such that his utility for any other policy is strictly decreasing in its distance from x_i, and (3) legislators have complete and perfect information about the available policies, each others' preferences, and all other model primitives.

The distinguishing feature of pivot theories is that the formal rules governing proposal and amendment procedures give rise to multiple *pivotal legislators*, that is, legislators whose consent is a necessary condition for successful policy change.[1] In a typical pivot model, there are two pivots, labeled here L and R with corresponding ideal points x_l and x_r. For example, in Cox and McCubbins's party cartel model, L may be the chamber median while R is the majority party median. In the theory, both are pivotal because the majority party median can gatekeep any legislation off the floor and unilaterally retain the status quo policy, whereas the chamber median can amend any legislation that reaches the floor to reflect her own ideal point.

Because the consent of both legislators L and R is required for passage, the interval between them $[x_l, x_r]$ constitutes a *cutpoint deadzone*, that is, a region of the policy space within which no vote resulting in policy change will have a *cutpoint*. The cutpoint of a vote is the unique location of the policy space at which a hypothetical legislator would be indifferent between the vote succeeding and failing.[2] The reason that a cutpoint of a vote resulting in policy change cannot fall between the pivots is simple—it would imply that one of the two pivots had permitted a policy change to occur that he opposed. From the perspective of the econometrician, a pivot theory therefore implies that cutpoints between the pivots are *censored* from the set of votes that result in policy change.

Finally, when legislators' votes reflect their true policy preferences, that is, when they vote sincerely, the cutpoint of a vote also perfectly divides the ''yea'' votes from the ''nay'' votes. Because pivot theories

[1] For models with >2 pivots L and R may be thought of as the outermost pivots.

[2] With single-peaked preferences, unique cutpoints exist when legislators have identical (potentially asymmetric) spatial loss functions or differing but symmetric loss functions.

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predict that legislators will vote sincerely on any vote whose success will result in policy change, pivot theories therefore necessarily imply that all pivotal legislators, and all legislators between them, vote identically ''yea'' on all votes that result in a policy change. This prediction is the crux of the claim that pivot theories will result in estimated ideal points of pivots that are statistically indistinguishable when estimated from votes that resulted in policy change.

To illustrate this phenomenon, consider a hypothetical three member legislature whose members are evenly spaced on the interval $X = [-1, 1]$, ranked by increasing ideal point $x_1, \ldots, x_3$. Assume that the legislators have standard quadratic utility functions, so that a legislator with ideal point x_i receives utility from policy b equal to $-(x_i - b)^2$. Finally, suppose that the legislators will have the opportunity to sequentially revise two status quo policies $q^1 = -\frac{1}{2}$ and $q^2 = \frac{1}{2}$ corresponding to two different policy areas under consideration. We now consider policy proposals, outcomes, and votes under two simple games: a median proposer game in which no cutpoints are censored, and the party cartel game in which cutpoints between the chamber median and the majority party median are censored.

2.1 *Median Proposer Game*

In the median proposer game, the median legislator $x_2 = 0$ is assumed to be able to propose a bill $b^t \in X$ to be considered against each status quo policy q^t in a single up or down vote. The equilibrium of the game is simple. For each status quo q^t, the median proposes his own ideal point $b^t = 0$, and the legislation passes under a minimal majority vote and becomes the new policy.

Consequently, two votes will be observed—the first in which the median votes with legislator 3 to overturn q^1, and the second in which the median votes with legislator 1 to overturn q^2. On both votes legislators 1 and 3 vote differently, and each votes with the median once. The game is depicted in the leftmost column of Fig. 1. Each row $t \in \{1,2\}$ corresponds to the vote to dislodge status quo policy q^t. In each plot, the x-axis is the policy space, and the ideal points of the three legislators and the status quo policy are labeled. The y-axis plots the legislators' votes as a function of their ideal points, and the discontinuity corresponds to the cutpoint of the vote.

The key empirical implication of this example is that an observer who lacked knowledge of the legislator's preferences and the policies on which they voted, but who knew that the legislators had unidimensional spatial preferences and had voted in accordance with them, could discern the rank order of the legislators' ideal points. The reason is that a vote with a cutpoint dividing every adjacent pair of legislators is observed.

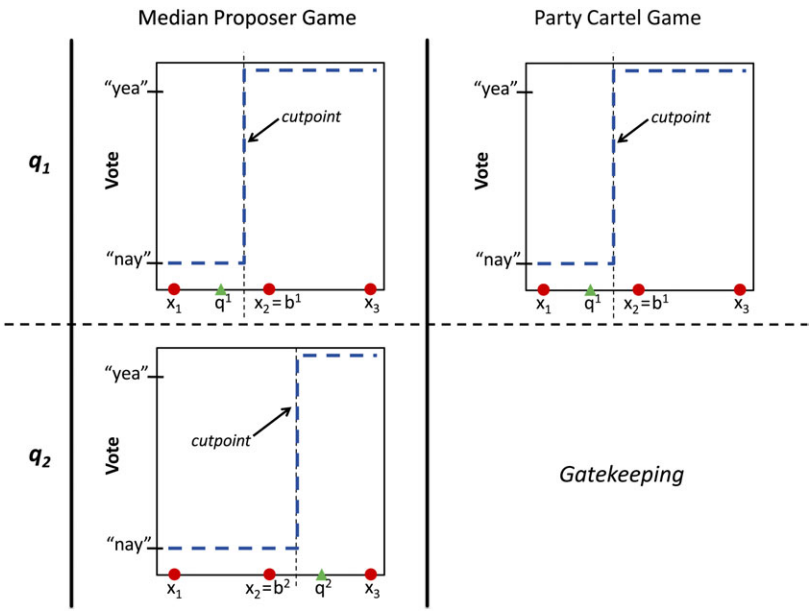


Fig. 1 Equilibrium votes in theoretical models.

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2.2 *Party Cartel Game*

To illustrate the phenomenon of cutpoint censoring when there are multiple pivotal legislators, suppose that the legislators instead play the party cartel game and that legislator 3 is the majority party median. Specifically, for each status quo q^t, legislator 3 first may decide whether to permit consideration of a bill to go forward. If consideration is permitted, only then can the median propose a bill.

The equilibrium of this game is similarly straightforward and solved by backward induction for each status quo. Whenever permitted to do so, the median will propose her own ideal point $b^t = 0$ knowing it will pass by majority vote. Anticipating this, legislator 3 allows revision of $q^1 = -\frac{1}{2}$ to $b^1 = 0$ to proceed but blocks $q^2 = \frac{1}{2}$ from consideration. The game is depicted in the rightmost column of Fig. 1.

In this game, only a single vote is observed–that which dislodged q_1 in which legislators 2 and 3 both vote ''yea'' against legislator 1. In contrast to the median proposer game, no cutpoint is observed between legislators 2 and 3 because revision of q^2 was blocked from consideration. Hence, an observer may discern that legislator 1 differs from the other two, but cannot identify which of legislators 2 and 3 is the median.

A key characteristic of the preceding example is that it is not dependent on a single vote remaining from which to discern preferences. For example, suppose instead that the legislature had the opportunity to sequentially consider 100 status quo policies, half located at $-\frac{1}{2}$ and half located at $\frac{1}{2}$. Then the preferences of legislators 2 and 3 would *remain* indistinguishable, since legislators 2 and 3 would vote together on the first half and legislator 3 would prevent the latter half from being considered. This final observation, however, depends crucially on the assumption that all legislators always vote perfectly in accordance with their spatial preference regardless of the number of votes considered. This assumption is at odds with the behavioral model assumed when estimating ideal points.

3 Estimation and Pivot Theories

Ideal point estimation augments the deterministic spatial model of preferences with a probabilistic component to account for voting data that cannot be perfectly described with the errorless model. In this section, I briefly review the quadratic-normal model of voting assumed by IDEAL and Poole's Quadratic-Normal (Poole 2001; Clinton et al. 2004) estimation procedures as a representative example of a probabilistic voting model. This review closely mirrors Clinton (2007).

Formally, $\{i = 1, \ldots, L\}$ legislators are observed to cast $\{t = 1, \ldots, T\}$ votes. Legislator i's utility from policy outcome x on vote t is assumed to be

$$U_i^t(x) = -(x_i - x)^2 + \varepsilon_{ixt}, \tag{1}$$

where ε_{ixt} is mean 0, variance $\frac{\sigma}{2}$, normally distributed random utility shock that is independent across legislators i, policies x, and time t.

The econometrician assumes that each observed vote represented a choice between a new policy b^t and a status quo policy q^t. Finally and crucially, it is assumed that on the observed votes, the legislators voted sincerely with respect to their *random* utility as described in equation (1). Legislator i is hence predicted to vote ''yea'' on vote t if and only if

$$U_i^t(b^t) - U_i^t(q^t) = ((x_i - q^t)^2 - (x_i - b^t)^2) + (\varepsilon_{ib't} - \varepsilon_{iq't}) \geqslant 0 \Leftrightarrow \tag{2a}$$

$$\eta_{it} = \varepsilon_{iq't} - \varepsilon_{ib't} \leqslant 2x_i(b^t - q^t) + (q^t)^2 - (b^t)^2, \tag{2b}$$

where the right-hand side of the inequality is legislator i's spatial utility difference for the bill over the status quo. Because $\eta_{it} = \varepsilon_{iq't} - \varepsilon_{ib't}$ is $\sim$N(0,σ^2) the probability that legislator i votes ''yea'' on vote t may be simply written as follows:

$$\Pr(\eta_{it} \leqslant 2x_i(b^t - q^t) + (q^t)^2 - (b^t)^2) = \Phi(\sigma^{-1}(2x_i(b^t - q^t) + (q^t)^2 - (b^t)^2)). \tag{3}$$

This completes the specification of the probabilistic voting model translating a legislator's ideal point x_i and policy alternatives b^t and q^t into a predicted probability distribution over votes.

The probabilistic voting model characterized by equation (3) has two notable features. First, it obviously differs from the deterministic voting model assumed by pivot theories by assuming that legislators

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may make voting ''errors'' with respect to their deterministic spatial utility. Formally, the aggregate propensity for error is governed by σ^2, the variance of the random utility shock difference $\eta_{it} = \varepsilon_{iq^t t} - \varepsilon_{ib^t t}$. Intuitively, the propensity for error may be thought of as the relative magnitude of stable unidimensional ideological preferences versus nonspatial idiosyncratic considerations, and a voting error as an instance in which nonspatial, idiosyncratic considerations override spatial, deterministic ones. Although it is intuitively appealing to interpret the errors as resulting from unmodeled structural features such as party influence or higher dimensional preferences, doing so is inconsistent with the statistical model's assumption that they be uncorrelated across legislators and votes. In other words, the errors are assumed to arise from truly idiosyncratic factors.

Second and crucially for my analysis, the characteristic property of the probabilistic model is that a legislator's propensity to vote ''yea'' on a given agenda (b^t, q^t) is a strictly increasing function of his spatial utility difference $2x_i(b^t - q^t) + (q^t)^2 - (b^t)^2$; intuitively, it reflects the *strength* of his deterministic spatial preference for the bill b^t versus the status quo q^t. For example, if $b^t > q^t$, then a legislator located at the cutpoint of the vote $x_i = \frac{b^t + q^t}{2}$ is equally likely to vote ''yea'' or ''nay,'' and becomes increasingly likely to vote ''yea'' as his ideal point moves rightward. In contrast, the purely deterministic model of voting lacks this critical identifying property because legislators on the same side of a cutpoint are predicted to vote identically.

Finally, a less obvious consequence of the difference in behavioral assumptions between deterministic pivot theories and the probabilistic estimation model is that translating the theories' predictions into statistical tests on estimated parameters requires bridging their assumptions. Formally, the analyst must specify the nature of the error that intervenes in the deterministic theoretical model to produce behavior consistent with the probabilistic estimation model. Otherwise, the assumptions are trivially inconsistent and the theoretical model being true implies that the estimates from the statistical model used to test it are false. The following section illustrates how building such a bridge for pivot theories rescues the ability to recover ideal point estimates.

3.1 *Cutpoint Censoring, Revisited*

To see how augmenting a pivot theory with probabilistic voting rescues the ability to distinguish the preferences of pivotal legislators, return to the party cartel example in the preceding subsection. Recall that status quo points at $q^2 = \frac{1}{2}$ are blocked, so that only votes between $q^1 = -\frac{1}{2}$ and $b^1 = 0$ with cutpoints located at $-\frac{1}{4}$ between legislators 1 and 2 are observed.

Now suppose that rather than voting perfectly in accordance with their deterministic spatial preferences, the legislators vote as characterized in the probabilistic model. Then on any observed vote with the preceding agenda, their respective probabilities of voting yea (in labeled order of legislator) are

$$\Phi\left(-\frac{3}{4}\sigma^{-1}\right) < \Phi\left(\frac{1}{4}\sigma^{-1}\right) < \Phi\left(\frac{5}{4}\sigma^{-1}\right). \tag{4}$$

The probabilistic model therefore predicts *distinct* voting behavior from legislators with distinct ideal points; in general, this holds regardless of (1) the location of the cutpoint and (2) the degree of voting error $\sigma^2 > 0$. Because the legislators' predicted behavior is distinct, in theory the econometrician can distinguish their preferences with a sufficient number of votes despite the absence of cutpoints between legislators 2 and 3. Formally, regardless of the agenda on which the legislators vote the probabilistic model of voting implies that the ideal point estimates of all legislators—including the pivots—are consistent given sufficient data (Lewis 2001).[3]

The preceding example also illustrates why the absence of cutpoints between two legislators generates small sample bias in estimated ideal points that is manifested as difficulty in distinguishing their

[3] A subtlety is that consistency requires both that the number of votes and the number of *legislators* tend to infinity; hence, it technically does not hold for a legislature of fixed size. The reason is that the number of ''incidental parameters'' (the bill and status quo locations) to be estimated increases with the number of observed votes (Neyman and Scott 1948). Absent an increasing number of legislators from which to estimate bill and status quo locations the latter cannot be estimated consistently, which in turn results in the former being estimated inconsistently (Londregan 2000).

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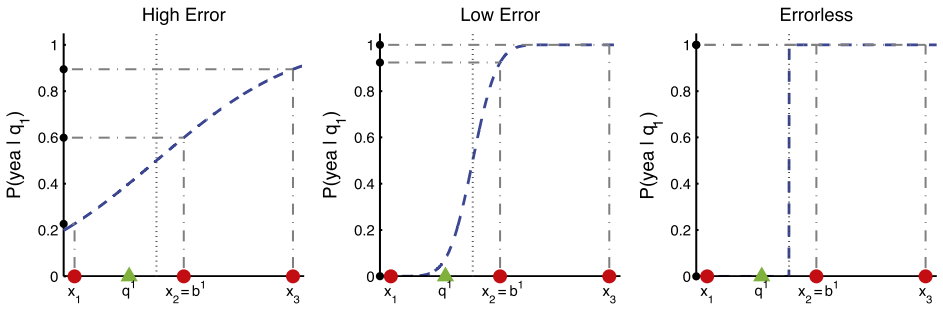


Fig. 2 Effect of voting error on conditional vote probabilities.

preferences. As the variance of the random utility shock σ^2 approaches 0, legislator 1's probability of voting "yea" tends to 0, that is, $\lim_{\sigma^2 \to 0}\{\Phi(-\frac{3}{4}\sigma^{-1})\} = 0$. However, the probability that legislators 2 and 3 vote "yea" both tend to 1, that is,

$$\lim_{\sigma^2 \to 0}\left\{\Phi\left(\frac{1}{4}\sigma^{-1}\right)\right\} = \lim_{\sigma^2 \to 0}\left\{\Phi\left(\frac{5}{4}\sigma^{-1}\right)\right\} = 1. \tag{5}$$

This implies that as behavior becomes increasingly well described by the spatial model, the degree to which preference strength is manifested in differential voting behavior by legislators 2 and 3 decreases, that is,

$$\lim_{\sigma^2 \to 0}\left|\Phi\left(\frac{1}{4}\sigma^{-1}\right) - \Phi\left(\frac{5}{4}\sigma^{-1}\right)\right| = 0. \tag{6}$$

The more general observation is that two legislators with the same spatial preference on a vote, that is, on the same side of the cutpoint, are predicted to vote increasingly similarly as $\sigma^2 \to 0$. This effect is depicted in Fig. 2; each plot is the probability a legislator votes "yea" on the vote to dislodge q^1 as a function of his ideal point. The rightward progression of plots visually demonstrates the effect of reducing σ^2 on the legislators' conditional vote probabilities.

As long as $\sigma^2 > 0$ the voting probabilities of legislators 2 and 3 will remain distinct, and their preferences will therefore be theoretically distinguishable. In this sense, the probabilistic spatial voting model with even a little error is unlike the fully errorless spatial model. However, as the probabilities approach 1 and each other, estimation will require a larger and larger number of votes in order to do so. This observation suggests that the salient question for empirical tests of Congressional lawmaking theories is whether pivot models *augmented* with probabilistic voting generate significant small sample bias in estimated ideal points, given the degree of unidimensional spatial voting in Congress and the sample sizes from which ideal points are typically estimated. To answer this question, I now turn to Monte Carlo simulations.

4 Monte Carlo Simulations

To characterize the degree of small sample bias in estimated ideal points implied by pivot theories, I generate two sets of simulated legislative sessions of the $L = 435$ legislators of the U.S. House of Representatives, labeled by rank order $\ell_1, \ldots, \ell_{435}$. The 106th Congress is used as a representative case, and the true underlying ideal points of the legislators are fixed at their first dimension DW-NOMINATE scores.[4] In one set of simulated legislative sessions the legislators play the median proposer game, and consequently no cutpoints are censored. In the second set the legislators play the party cartel game, and cutpoints between the chamber median and the majority party median are censored. The former game is used as a benchmark against which to compare the latter because the distribution of cutpoints it generates is identical but for the cutpoint censoring.

[4]The choice of both the 106th Congress and the values at which to fix the true ideal points is simply for illustrative purposes and have no substantive bearing on the results.

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Each legislative session begins with the exogenous selection of Q status quo points that are available for revision, drawn from a uniform distribution.[5] The expected number of successful votes in each legislative session is strictly less than Q. In the median proposer game, some status quo points will fail to be dislodged as a consequence of voting error. In the party cartel game, this will also occur because of gatekeeping.[6] Because the accuracy of ideal point estimates is sensitive to the number of roll calls observed, I fix Q so that the expected number of successful votes in each session is 241; this matches the number of successful final passage roll call votes in the 106th House by Clinton's (2007) criteria, and is comparable with other Congresses.[7] Note that 241 is significantly fewer than the number of roll calls used to calculate DW-NOMINATE scores, which use the full set of observed votes. I restrict the size of the simulated roll call matrix to match the number of successful final passage votes for the reasons identified by Clinton, who argues that pivot theories' predictions are restricted to votes that successfully result in policy change.[8]

After Q status quo points are exogenously selected, each individual status quo q^t is independently considered by the chamber in turn. In the median proposer game, q^t is pitted against a bill b^t located at the ideal point of the chamber median ℓ_m. In the party cartel game, the majority party median ℓ_M first chooses whether to permit consideration of a bill altering q^t or to gatekeep. If consideration is permitted, then the status quo is pitted against a bill $b^t = \ell_m$ as in the median proposer game. The well-known prediction of this model is that the majority party median chooses to gatekeep status quo points that fall in the interval $[\ell_m, 2\ell_M - \ell_m]$, termed the *blockout zone*.[9] Both models therefore generate the same distribution of cutpoints, but for the cutpoint deadzone located between the chamber median and the majority party median in the party cartel game.

Finally, once the bill prediction b^t is determined for each status quo point q^t, legislators cast their votes. When choosing, each legislator's utility difference is subjected to an additive random utility shock that is independently distributed across legislators and time. For each variant of the game, I run one set of simulations in which the shock is normally distributed with variance σ^2—as in Clinton et al. (2004) and Poole's (2001) Quadratic-Normal—and a second set of simulations in which the shock is uniformly distributed, as in Heckman and Snyder (1997).[10] With uniform shocks, a legislator's propensity to vote "yea" is a linear function of his ideal point.

Summarizing, the fixed parameters in each set of simulations are the legislator ideal points, the number of exogenous status quo points drawn Q, the game—party cartel or median proposer—and the distribution of the additive random utility shock—normally distributed with variance σ^2 or uniformly distributed.[11]

[5]The support of the distribution is set at $[2\ell_1 - \ell_m, 2\ell_{435} - \ell_m]$, where ℓ_m is the chamber median. This permits votes to occur on which every legislator's spatial preference is for the bill to pass. In the U.S. Congress, unanimous or "hurrah" votes are frequently observed.

[6]The simulation exercise implicitly assumes that the legislators' random utility shocks are unobservable to each other. An alternative approach would be to first subject legislator ideal points to idiosyncratic vote-specific shocks directly and then have legislators play the complete information game. This approach generates comparable results. The conventional assumption in econometric testing of complete information theories—that there are additive random utility shocks known to the players but not to the econometrician—is untenable in this context because it would generate policy preferences that are not single peaked.

[7]This requires calibrating Q separately for each specification of the simulations.

[8]Specifically, Clinton argues first that unsuccessful votes are not theoretically relevant because complete information pivot theories, in almost all cases, do not predict unsuccessful votes as a result of subgame perfection. Hence, some behavior outside of the theories' scope such as position taking must be taking place on unsuccessful votes. He furthermore argues that only successful *final passage* votes are theoretically relevant for two reasons. First, legislators will vote sincerely only if they do not anticipate any future rounds of gameplay, a questionable assumption for final passage votes (Volden 1998) but clearly indefensible for earlier stage votes. Second, complete information pivot theories typically assume that the median legislator proposes every bill. This assumption is an analytical shortcut that generates the same bill prediction as the consideration by the chamber under an unrestricted rule for the vote determining the final policy outcome. Because amendments and procedural votes are not votes determining the final policy outcome, they are outside the scope of both models.

[9]Cox and McCubbins (2005) present a model of this form (Appendix 9.A entitled "A Stochastic Spatial Model") to demonstrate that the prediction of the complete information model is retained in an incomplete information framework. This observation, however, only holds if the chamber median is assumed to propose non-strategically. To my knowledge an incomplete information lawmaking model with probabilistic unidimensional spatial voting and in which all legislators are fully strategic has yet to be analyzed; electoral models of this form, however, are common.

[10]Specifically, when $b_t \geqslant q_t$, the random utility shock difference η_{it} is uniformly distributed over $[2\ell_1 (b^t - q^t) + (q^t)^2 - (b^t)^2, 2\ell_{435} (b^t - q^t) + (q^t)^2 - (b^t)^2]$. When $q^t > b^t$, the positions of ℓ_1 and ℓ_{435} are reversed.

[11]The variance of the random utility shock cannot be varied freely in the linear probability model. The requirement that legislators' voting probabilities be strictly within the unit interval generates a lower bound on the variance of the uniform shock for each agenda.

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Table 1 Summary of simulations runs

	Game	
Shock distribution	Party cartel	Median proposer
Normal	Run 1 (σ^2)	Run 2 (σ^2)
Uniform	Run 3	Run 4

Table 1 summarizes and labels each parameter profile. The notation Run 1 (σ^2) denotes a Run 1 simulation in which the variance of the normally distributed random utility shock is σ^2.

For each profile of simulation settings, $Z = 2000$ legislative sessions are simulated. The sources of randomness are the set of status quo points subject to revision and the legislators' random utility shocks. For each simulated legislative session, unidimensional Heckman-Snyder ideal point estimates are recovered using principal components analysis,[12] and the parameter values of interest are the estimated ideal points and derived quantities such as the width of the cutpoint deadzone. A recovered parameter from each legislative session is a draw from its true distribution, conditional on the underlying game and the stochastic process governing the agenda and the random utility shocks. The empirical mean across sessions is therefore a numerical approximation of the expectation of the parameter estimate, and a kernel density estimate using all sessions is a numerical approximation of its true distribution.

Finally, note that unidimensional Heckman–Snyder scores are technically inconsistent when the underlying probability model is quadratic normal. Nevertheless, it is not clear that the use of another scaling method is preferable. Any scaling method that does not impose parametric assumptions on the distribution of the voting agenda will be inconsistent if the number of legislators is not taken to infinity (Lewis 2001). Although Heckman–Snyder scores may arguably be more problematic than NOMINATEs because they impose a linear probability structure on a nonlinear data generating process, this assumption is only burdensome in two cases: (a) the legislator in question has an extreme ideal point, and (b) voting approaches errorless. The former problem is inconsequential in my analysis since the legislators of interest—within the cutpoint deadzone—are centrally located in the policy space. The second problem plagues all random utility-based ideal point estimation procedures. Although Heckman–Snyder scores become distorted as voting approaches errorless because of the imposition of a linear model, NOMINATE scores perform similarly poorly because that procedure relies heavily upon voting error to identify the bill parameters. For this reason, I compare the estimates in the party cartel game to a benchmark median proposer game rather than their true values. This comparison identifies the *incremental* distortion generated by cutpoint censoring.

5 Simulation Results

I first analyze the simulation results from Runs 1 (σ^2) and 2 (σ^2) in which the random utility shocks are normally distributed. I focus on how the small sample bias generated by cutpoint censoring is mediated by the propensity for voting error as governed by the variance σ^2 of the random utility shocks.

Figures 3 and 4 plot the mean estimated ideal points from the simulated data as a function of the true ideal points from Runs 1 (σ^2) and 2 (σ^2). The cutpoint deadzone is identified by the two vertical lines. Within each figure, the variance is held fixed. Figure 3 plots the fully errorless voting case ($\sigma = 0$), and Fig. 4 plots a case in which legislators vote with error ($\sigma = .4$). In Fig. 3 the bias generated by cutpoint censoring is clear, and in the party cartel simulations the estimated ideal points of the legislators within the cutpoint deadzone are identical as hypothesized. In contrast, when legislators play the median proposer game the recovered ideal points are a strictly monotonic transformation of the true ideal points, with the severest distortions at the ends of the policy space due to the imposition of a linear probability model by the Heckman–Snyder estimation procedure. Once legislators vote probabilistically, however, the pattern looks

[12] As previously noted, in the party cartel game status quo points may fail to be dislodged both because they fall within the blockout zone and as a result of idiosyncratic errors in legislator voting. Although the underlying reasons for gridlock in these two cases are different, an econometrician who estimates ideal points from the set of successful final passage roll call votes will be filtering out both types of votes since there is no way to distinguish the two. Hence, in the simulated data votes that fail as a result of idiosyncratic error are also filtered out prior to estimation. The choice is made for conceptual clarity and has no effect on the qualitative findings.

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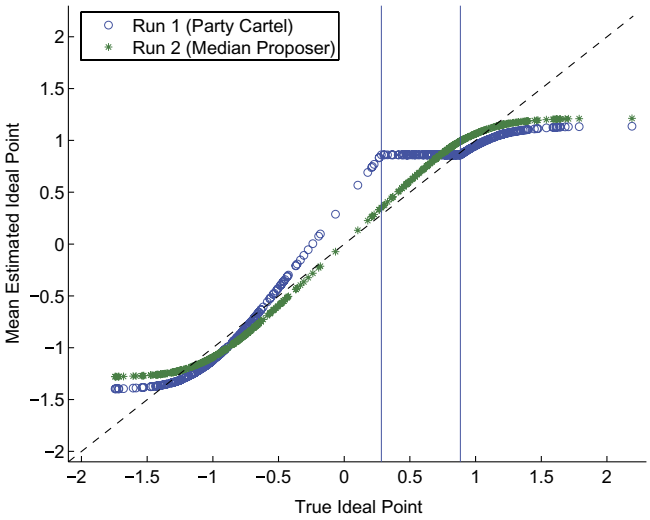


Fig. 3 Mean estimated versus true ideal points, normal errors, $\sigma = 0$.

quite different. In Fig. 4, where $\sigma = .4$, the distortion in the expected value of the estimates as a consequence of cutpoint censoring all but vanishes. The plot therefore demonstrates that even in small samples, the inclusion of error permits the estimation procedure to distinguish the ideal points of legislators within the deadzone.

However, it is not entirely clear which of the above two cases is likely to prevail with U.S. Congressional data. What does it mean to have $\sigma = .4$, exactly? In the simulated data, it implies that approximately 89% of the individual votes made over the 241 roll calls are correctly predicted by the legislators' unidimensional spatial utility function. Although the percentage of ''correct'' votes in a Congressional session cannot be measured directly, the percentage of classification success of unidimensional scaling procedures is an indirect measure of the extent to which the unidimensional spatial model generates the observed data. For NOMINATE classification success is typically on the order of 80%–85% (Rosenthal 1992), and for the 241 final passage votes in the 106th House it is approximately 90% (Clinton et al. 2004). Hence, if classification success can be used as a proxy for the underlying percentage of correct votes under the maintain assumption that the statistical model is true, then the pattern of voting in the U.S. Congress is much closer to Fig. 4 than to Fig. 3.

Figure 5, which plots classification success and percentage error in the simulated data for the two models as a function of σ, suggests that classification success can indeed be used as a proxy. In the

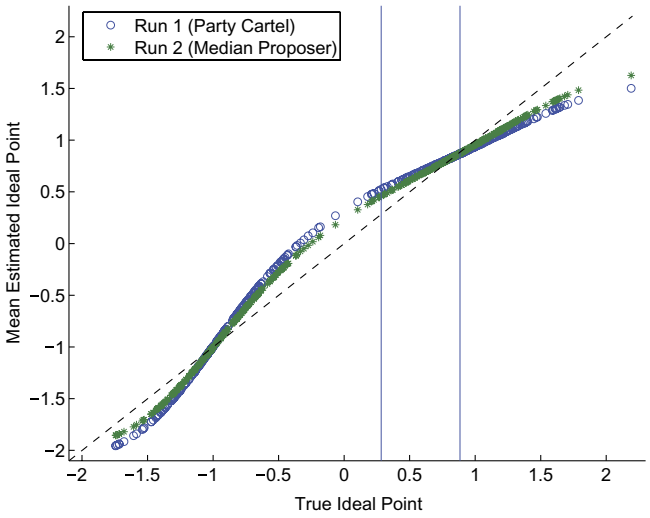


Fig. 4 Mean estimated versus true ideal points, normal errors, $\sigma = .4$.

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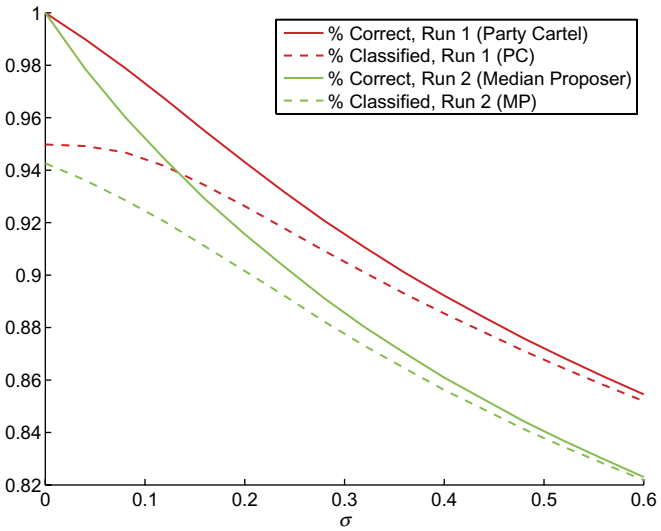


Fig. 5 Mean correct voting and classification success, normal errors.

simulated data, classification success and percentage of correct votes are similar for both the party cartel and median proposer games, although the gap between them grows as voting approaches errorless. For U.S. Congressional data the classification success of parametric scaling procedures rarely exceeds 90%, and in this range the gap between the two measures is approximately a single percentage point. The relationship is sufficiently robust to be worth exploring in future work, but here it is sufficient to observe that classification success can reasonably be used as a rough proxy for the percentage of correct votes if voting is as characterized by the probabilistic unidimensional spatial model.

Another more precise way of assessing the accuracy of the recovered ideal points is to compute the mean correlation coefficients between the estimated and true ideal points. This metric largely fails to capture monotonic distortions in the estimates, but it is difficult to be more precise in the absence of formal consistency. The use of correlation coefficients is also the modal technique for making comparisons across ideal point estimation procedures and assessing their accuracy (examples include Heckman and Snyder 1997; Clinton et al. 2004). Figure 6 plots the mean correlation coefficient of the recovered ideal point estimates with the true ideal points as a function of σ, restricted to the legislators located within the deadzone. The divergence between the median proposer and party cartel plots is a measure of the *incremental* distortion of the ideal point estimates of deadzone legislators as a result of the censoring.

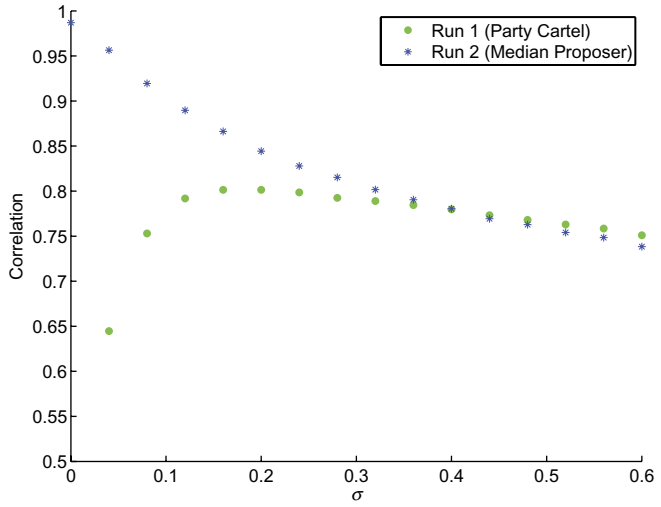


Fig. 6 Mean ideal point correlation of deadzone legislators, normal errors.

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The pattern is precisely as one would expect given the previous figures. In the median proposer simulations, the mean correlation coefficients are monotonically decreasing in σ. As legislators make more errors, the accuracy of the ideal point estimates decreases. In the party cartel model, in contrast, the pattern is nonmonotonic. As voting approaches errorless, the ideal points of the deadzone legislators become systematically distorted by cutpoint censoring and their ideal points indeed look too similar. Once a small degree of random utility error is introduced, however, the ideal points of these legislators appear to be accurately distinguished and ordered. With sufficient error, the correlation again begins to decrease.

Two aspects of Fig. 6 are striking. First, the positive effect of voting error on the ability to recover ideal point estimates in the presence of censoring occurs even at extremely low levels of error, indicating that extreme (small sample) version of the censoring bias conjectured by Clinton only holds in the extreme limit. Even at $\sigma = .08$, when 98% of the votes in the roll call matrix are correct and the classification success is 95%, the correlation exceeds .75. This is true despite the small sample size per legislative session (241 votes) as compared with DW-NOMINATE scores. Furthermore, there is little evidence that the degree of voting error in the U.S. Congress is anywhere near this low if the maintained assumption that voting is unidimensional is accurate. Even a *ten* dimensional model fails to approach this degree of classification success with Congressional data (Poole, Rosenthal, and Koford 1991).

The second striking aspect of Fig. 6 is that at $\sigma = .4$, the distortion in ideal points as measured by mean correlation vanishes entirely and estimation performs no worse in the presence of cutpoint censoring. At $\sigma = .32$, which produces an average classification success of 90% matching that of the 106th House, the presence of cutpoint censoring reduces the correlation by only about .013. Given the myriad problems posed by any number of questionable assumptions when ideal points are estimated and used in theory testing, it therefore seems difficult to argue that cutpoint censoring should be a principle concern.

5.1 *Measuring Deadzone Width*

One of Clinton's key contributions is to propose a new theory-driven test of pivot theories based on cutpoint censoring. He argues that because the ideal point estimates of the deadzone legislators should be statistically indistinguishable when recovered from the set of successful final passage votes, whether the estimated width of the deadzone implied by a particular pivot theory is statistically distinguishable from 0 actually constitutes a test of that theory. The preceding results cast doubt on the validity of Clinton's statistical test. However, the issue is worth exploring in further detail because the ability to recover estimates of the deadzone width is a critical assumption in several analyses performing comparative tests of lawmaking theories. If a small sample version of Clinton's conjecture were even partially true, these analyses would be rendered highly suspect.

Clinton's conjecture is inspected in Fig. 7. Each subplot contains a nonparametric density estimate of the full distribution of the estimated deadzone width holding σ fixed, and σ is varied from 0 to .32 across the subplots.[13] The extent to which the density plot from the party cartel simulations (Run 1) deviates from the plot from the median proposer simulations (Run 2) captures the degree to which cutpoint censoring downward biases the estimated deadzone width.[14] The first panel of Fig. 7 is the fully errorless voting case, and here Clinton's argument is correct. When legislators play the party cartel game, the distribution of the estimated deadzone width is a point mass at 0, and it is far away from 0 when legislators play the median proposer game. The second subplot of Fig. 7 illustrates the effect of introducing a very small degree of random utility error ($\sigma = .04$, 99% correct votes, and 95% classification success). Here, Clinton is both right and wrong. The estimated width of the deadzone in the party cartel case is packed tightly around a mean of .05. The gray region under the curve indicates that the parameter estimate will lie in the associated interval 95% of the time. However, even when $\sigma = .04$, this region does not overlap with zero, and Clinton's statistical test is hence inappropriate when even a trivial degree of random utility error is introduced.

Clinton, however, is correct in the sense that when voting is near errorless, the estimated deadzone width in the party cartel simulations is biased severely downward relative to the median proposer simulations. The distortion from cutpoint censoring is not eliminated when *any* error is introduced, although

[13] $Z = 2000$ legislative sessions are simulated for each value of σ to compute the density.

[14] The true value of the deadzone width is represented by the dotted line. The downward distortion common to both sets of simulations arises from the use of the Heckman–Snyder method as previously noted.

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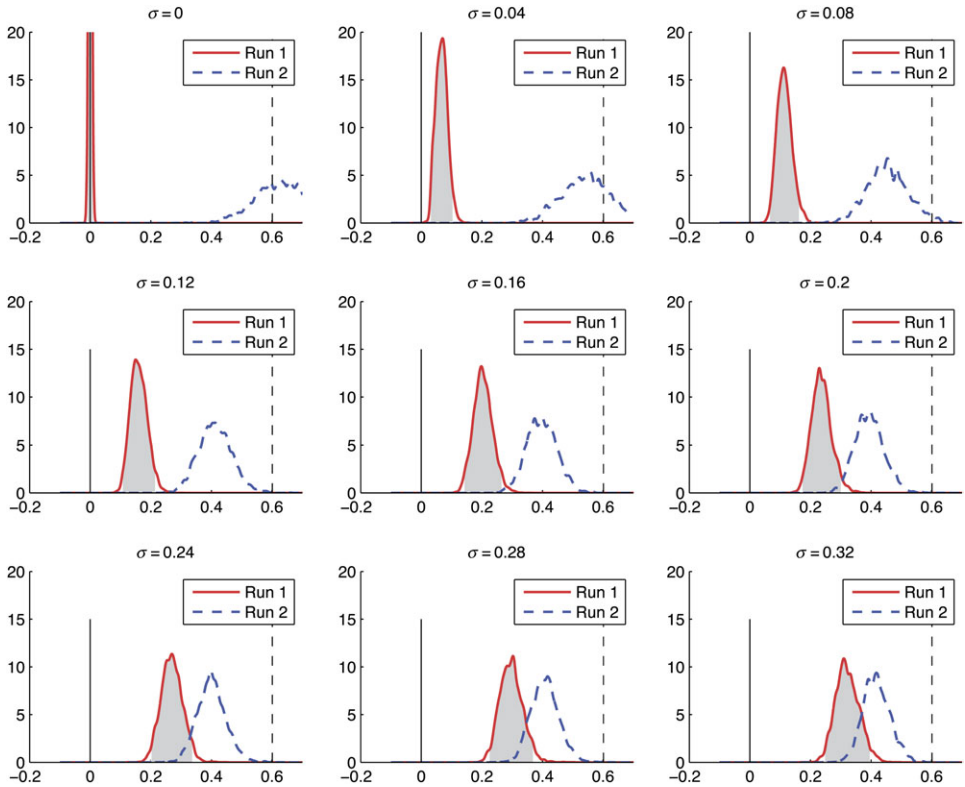


Fig. 7 Distribution of estimated deadzone widths, normal errors.

even a little significantly mitigates the problem. The progression of plots in Fig. 7 visually demonstrates how the downward bias in estimated deadzone width is increasingly attenuated as more voting error is introduced. At $\sigma = .32$ and 90% classification success, the two distributions appear quite similar and are well away from zero. Moreover, their 95% probability regions overlap. Figure 7 thus confirms the previous findings; even a little error eliminates the most extreme form of the bias, and at levels of error matching, the pattern of Congressional voting the censoring bias is minor or nonexistent.

5.2 *Uniform Errors*

For completion, in Runs 3 and 4 I replicate the preceding analysis for an additional set of simulations in which the legislators' random utility shocks are uniform. Although normally distributed shocks establish the pattern of the bias as a function of the degree of error, more econometric precision is possible with uniformly distributed shocks because the recovered ideal point estimates are actually consistent. Note, however, that with uniform shocks only about 70% of votes are correct in the simulated data, which appears to be somewhat below what must prevail in Congressional voting if the unidimensional probabilistic model is correct.

In the uniform case, there does not appear to be any effect whatsoever of cutpoint censoring on the estimates. The accuracy of recovered ideal points for the deadzone legislators as measured by correlation coefficients is nearly indistinguishable between the party cartel and median proposer simulations, which produce mean correlation coefficients of .67 and .68, respectively. More strikingly, the full distribution of estimated deadzone widths between the two models is virtually identical; this can be seen in Fig. 8, which replicates Fig. 7 with uniformly distributed errors.[15] In addition, Fig. 9 plots both the mean of the estimated

[15]Note that both the median proposer and party cartel distributions are biased slightly upward from the true value as indicated by the dark vertical line. This bias is therefore not a consequence of cutpoint censoring, but rather small sample bias produced by simultaneously estimating the identities of the pivots and their ideal points. When the average size of each legislative session is increased this bias diminishes accordingly.

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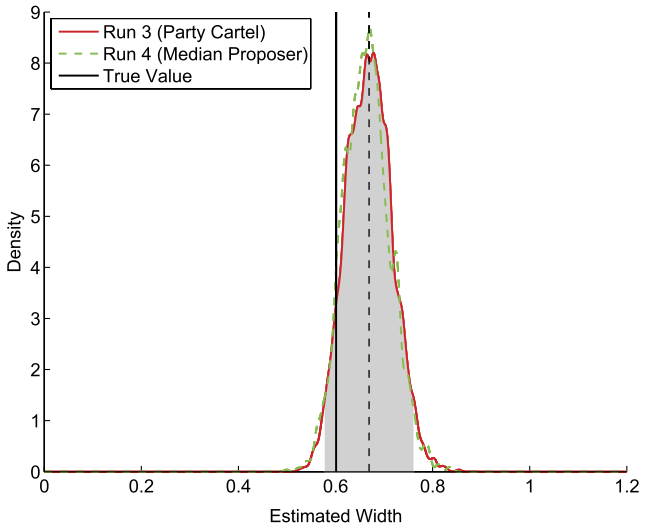


Fig. 8 Distribution of estimated deadzone widths, uniform errors.

ideal points and the true ideal points as a function of true rank from the party cartel simulations; the two can be directly compared in the linear probability case because the estimates are formally consistent. The two plots are identical, and there is no observable bias in the recovered estimates. Figure 9 conclusively demonstrates that cutpoint censoring has no effect in the case of uniform shocks.

Finally, in Fig. 9 the dashed lines above and below the plot indicate the interval within which the ideal point estimate of that legislator falls 95% of the time. The 95% probability interval shows that the estimates of the legislators in the deadzone are no noisier than those outside the deadzone. In addition, the standard deviation of the estimates is approximately constant across all legislators. This belies the claim that the estimates of legislators located far away from cutpoints will be less precise (Stiglitz and Weingast 2010), although the effect may be specific to the linear probability model.

6 Conclusion

To conclude, the cumulative simulation evidence strongly suggests that in practice the bias in estimated ideal points implied by pivot theories is likely to be small or nonexistent with Congressional voting and the sample sizes from which ideal points are typically estimated. The broader implication of my findings is

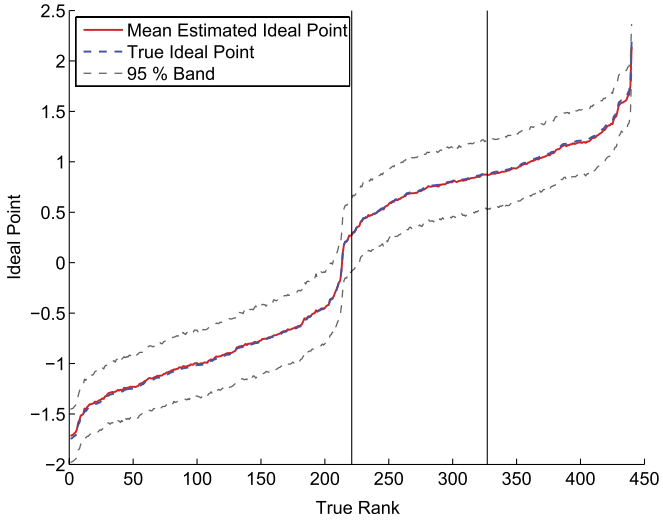


Fig. 9 Estimated ideal points, Run 3 (party cartel), uniform errors.

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that, contrary to Clinton's claim, the practice of testing pivot theories by estimating ideal points and ''calculating gridlock intervals for use in a regression analysis'' is not at all ''of uncertain worth'' (p. 461). To state the conclusion less strongly, although the practice may indeed be of uncertain worth, the reason is unlikely to be the cutpoint censoring predicted by the theories.

Nevertheless, it remains true that cutpoint censoring implies some degree of bias in estimated ideal points. Although the calibration exercise herein demonstrates that the magnitude of the bias would likely be minimal for Congressional roll call data, it is impossible to know as a general proposition whether it is truly inconsequential for theory testing. Clinton's work is therefore significant in that it identifies a new source of theory-driven bias to which a broad class of theories are subject. In future work, scholars should therefore be cognizant of, and preferably account directly for, this bias when testing pivot theories.

Although the best technique to account for the bias is unclear, the logic underlying the approach suggested by Clinton—to test ''the theories' predictions in terms of the distribution of estimated ideal points'' themselves—is dubious. The statistical properties of hypothesis tests on estimated ideal points only hold if the assumed probabilistic model of voting is true. But Clinton in effect suggests testing restrictions on the estimated parameters that could hold if the probabilistic model is false. Clinton's approach therefore has its own endogeneity problem—if the null is true, then the test itself is false!

A superior approach would appear to be that suggested by Clinton himself in a pathbreaking 2003 paper ''Integrating Voting Theory and Roll Call Analysis: A Framework'' (with A. Meirowitz). The approach is sufficiently well articulated that I reproduce it here verbatim.

> 1. constructing an account of the legislative history that isolates two sets of hypotheses: those that are *to be maintained* and those that are *to be tested*
>
> 2. translating hypotheses into constraints on parameters of the statistical model
>
> 3. estimating the model assuming that the identifying constraints implied by the *maintained hypotheses* are true
>
> 4. testing the constraints corresponding to the *to be tested hypotheses* (p. 382)

Clinton and Meirowitz's (2003) approach provides a simple recipe for testing unidimensional pivot theories. The random utility model of voting and a normalization on the ideal points constitute the ''maintained assumptions'' identifying the model's remaining parameters. The constraints on bill and status quo locations implied by each pivot theory constitute the ''to be tested hypotheses.'' Estimation and hypothesis testing then proceeds simply using either a maximum likelihood or Bayesian framework. The key benefit of this integrated approach is that the theory-driven bias present with pivot models is *automatically accounted for* by the likelihood function used in the statistical test. Integrating theory and estimation in precisely the fashion advocated by Clinton in earlier work can therefore account for the bias he now identifies. To my knowledge, a straightforward competing test of partisan and nonpartisan models using this integrated approach has yet to be performed.[16]

A final more general point illustrated by my analysis is that integrating theoretical and measurement models when testing lawmaking theories is significantly more nuanced than it may sometimes appear. Consider the following two predictions of gatekeeping theories articulated by Clinton as equivalent:

> 1. Gatekeeping theories predict we should never observe a cutpoint in the interior of $[x_m, x_g]$ resulting in policy change
>
> 2. ... g and m should never vote differently on votes enacting a policy. (p. 462)

With perfect and sincere spatial voting, the preceding two statements are indeed equivalent. However, *any* integrated model that generates behavior consistent with the assumptions underlying ideal point estimation cannot generate the second prediction because of the nature of probabilistic voting. Similarly, any integrated model that makes the two preceding predictions requires an alternative statistical model for estimation and hypothesis testing.

The approach of this paper has been to simulate a specific integrated model of the party cartel theory that generates predictions on the agenda consistent with the complete information model, and voting

[16] Stiglitz and Weingast (2010) use recoved cutpoint estimates from IDEAL to test the predictions of partisan and nonpartisan lawmaking models. However, their hypothesis tests on cutpoints are performed with second-stage regressions that do not account for the preestimation of cutpoints.

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behavior consistent with the ideal point estimation model. This is sufficient to explore the small sample bias arising from cutpoint censoring, but for more complex games the approach is wanting. What are ''errors'' in voting? Are they truly idiosyncratic and consistent with the measurement model, or are they instead unmodeled systemic features of the political environment, unmodeled higher dimensions, or something else? Are the errors unknown to both the econometrician and the legislators? In other words, are we actually estimating a game of incomplete information? If so, then the nature of strategic interaction is likely to be quite different than in a complete information model for all but the simplest games. Alternatively, are these errors known to the legislators but unknown to the econometrician? Then the assumptions underlying ideal point estimation are almost certainly false, and more complex estimation procedures are needed.

In all cases, the details of how we integrate theory and measurement models matter crucially for both the estimation methods we employ and the predictions we make. An important challenge for future work in Congressional scholarship is therefore to take a much more explicit a stand on the details of the integrated model used to link theory and measurement, and extract testable predictions accordingly.

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