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A Bridge to Somewhere: Mapping State and Congressional Ideology on a Cross-institutional Common Space

Researchers face two major problems when applying ideal point estimation techniques to state legislatures. First, longitudinal roll-call data are scarce. Second, even when such data exist, scaling ideal points within a single state is an inadequate approach. No comparisons can be made between these estimates and those for other state legislatures or for Congress. Our project provides a solution. We exploit a new comparative dataset of state legislative roll calls to generate ideal points for legislators. Taking advantage of the fact that state legislators sometimes go on to serve in Congress, we create a common ideological scale. Using these bridge actors, we estimate state legislative ideal points in congressional common space for 11 states. We present our results and illustrate how these scores can be used to address important topics in state and legislative politics.

Following the contributions of Poole and Rosenthal (1985, 1991, 1997), the estimation of legislative ideal points has become an active and influential research agenda in political science. Quantities of interest in their own right, ideal point estimates have also become essential ingredients in studies of legislative politics.[1] More recently, Bayesian item-response theory models have come to complement the Poole-Rosenthal NOMINATE algorithm (Clinton, Jackman, and Rivers 2004; Jackman 2000, 2004; Martin and Quinn 2002).

Although initially focused on Congress, scholars have also applied ideal point analysis to comparative contexts (Poole and Rosenthal 2001), including U.S. state legislatures. A crucial limitation of existing state ideal point estimates is that they cannot be compared across states or with estimates for members of Congress, because each ideological space is defined solely within a single state.

For this article, we exploit a method for estimating ideal points of multiple state legislatures and Congress in a single, comparable common space. We use the voting records of bridge actors—legislators who "graduate" from a state legislature to Congress—to produce a universal spatial map for state and congressional politics. We then employ our new scores to consider several questions of longstanding interest to scholars of legislative politics. We also explore the underlying assumption of ideological consistency using a variety of methods.

Our method (and the comparable cross-state ideological measures generated by it) enables scholars to better address a host of substantive debates. For example, one of the perennial questions in state politics literature concerns the connection between elite ideology and policy choices. Likewise, past attempts to study the effects of party control on policy have been confounded by the difficulty of distinguishing legislator ideology from partisanship. Our new measures not only allow scholars to disentangle these relationships but also provide a superior alternative to existing measures, as we will show.

Moreover, unlike previous state ideology scores, our measures provide estimates for individual legislators, an advantage that opens up new avenues for research in state legislative politics. A whole range of questions that have occupied congressional scholars can now be brought to the state level. To give but a few examples: scholars have examined party polarization, the representativeness of committees, the size of gridlock intervals and the spatial locations of pivotal actors, and changes in voting patterns following redistricting—all of which require estimates of the spatial positions of individual legislators. Having comparable scores across states will allow scholars to investigate these questions using multiple legislatures, greatly expanding the potential for the states to be laboratories of democracy for political scientists.

The article proceeds as follows. We first describe previous efforts to measure state-level ideology using ideal point estimates and other methods. Next, we detail our methodology and discuss our data. We then present our major results. We analyze the dimensionality of state legislative politics, demonstrate how our new spatial map can be used to address issues of partisan polarization, and offer an assessment of the relationship between state legislatures and their respective congressional delegations. We conclude by evaluating the advantages and limitations of our approach and establishing an agenda for future research.

Measuring Ideology across States

A large portion of the state politics literature seeks to explain the relationships between the ideology of citizens, the ideology of their elected representatives, and the ideological orientation of resulting state policies. We focus on the second of these. Erikson, McIver, and Wright (1987; Erikson, Wright, and McIver 1993) were among the first researchers to attempt to estimate the ideology of citizens and government officials within states. Their measure of party-elite liberalism was based on surveys of congressional candidates, state legislators, county party chairs, and party convention delegates.[2]

Such estimates are important but limited by their inherently cross-sectional nature. Berry et al. (1998) produced the first annual estimates of citizen and government ideology for all 50 states. The Berry estimates of state government ideology rely on the interest group scores for the state's congressional delegation, derived separately by party and weighted to reflect the distribution of partisan control in the state's legislative and executive branches.

A notable limitation of both the Berry et al. (1998) and the Erikson, Wright, and McIver (1993) measures of government ideology is that they pertain to the state government writ large. They can tell us nothing about the ideology of individual legislators or the distribution of preferences more broadly. Nor does either approach yield an ideological measure for state government that can be meaningfully compared with standard congressional measures of ideology. The Berry et al. (1998) framework requires that ideological differences in parties across states are fully reflected in state congressional delegations. But, in fact, we find significant differences between the two as shown below. Meanwhile, Erikson, Wright, and McIver's (1993) measure relies on idiosyncratic and isolated surveys of state and local officials.[3]

Within the last few years, several scholars have estimated ideal points of state legislators by using roll-call data to produce NOMINATE scores. Aldrich and Battista (2002) produced NOMINATE scores for 11 state legislatures in the late 1990s to investigate party polarization and committee representativeness. Gerber and Lewis (2004) generated NOMINATE scores for the California state legislature. McCarty, Poole, and Rosenthal (2006) used NOMINATE scores to estimate polarization measures for 11 states. Kousser, Lewis, and Masket (2007) estimated NOMINATE scores for California Assembly members, to investigate whether legislators changed their voting behavior after the 2002 gubernatorial recall election. Jenkins (2006) estimated NOMINATE scores for 5 state legislatures and used an external survey to measure the effect

of parties on voting. To examine the effects of single-member versus multimember districts on legislator extremism, Bertelli and Richardson (2008) created NOMINATE scores for the Arizona legislature. Finally, Wright (2007) assembled roll-call data for all states for 1999 through 2000 and produced a comprehensive set of within-state NOMINATE scores. With a series of coauthors, Wright has also used state roll-call data to explore the extent to which legislative politics is one dimensional (Wright and Clark 2005; Wright and Winburn 2003), and the effects of parties on the structure of roll-call voting (Wright and Schaffner 2002).

Compared to the government ideology measures of Berry et al. (1998) and Erikson, Wright, and McIver (1993), the ideal point approach has the advantage of providing estimates of the ideological positions of individual legislators. Moreover, preference estimates are based on the actual behavior of state legislators rather than assumed correlations with the state's congressional delegation.

Two principal difficulties exist with ideal point estimation when it is applied to state legislatures. First, access to data on state legislative roll calls is sparse, to say the least. Worse, existing state-level ideal point analyses have been conducted one state at a time. Because the estimated latent ideological dimensions need not be on the same scale for separate roll-call matrices, the existing ideological scores are not directly comparable across states. Nor, for that matter, can existing state-level scores be directly compared with congressional scores. In the remainder of this article, we present a method for estimating ideological scores on a common dimension across states and Congress, and we offer preliminary comparative analyses using these new scores. Our results are agnostic with respect to estimation technology: we have used both NOMINATE and item-response models equally successfully.

Methodology

The need for comparable preference estimates across political institutions is hardly new. The existing literature includes, for example, efforts to produce common ideological scales for the U.S. House and Senate (Groseclose, Levitt, and Snyder 1999; Poole 1998), for presidents and Congress (McCarty and Poole 1995), for presidents, senators, and Supreme Court justices (Bailey and Chang 2001; Bailey, Kamoie, and Maltzman 2005), and for Supreme Court and Court of Appeals justices (Epstein et al. 2007). Indeed, connecting overlapping generations of political actors within a single institution over time presents similar challenges of estimating comparable ideal points for actors whose choices are not observed simultaneously (Martin and Quinn 2002;

Poole and Rosenthal 1997). To our knowledge, however, no one has attempted to put multiple state legislatures onto a common ideological map using ideal point techniques.

All of the efforts to place multiple institutions in a common space rely, in varying ways, on bridge actors: political actors who make choices that can be construed as votes in more than one institutional setting. Common examples of bridge actors include members of Congress who serve multiple terms, members who migrate from the House to the Senate, solicitors general who advocate for one side in front of the Supreme Court, and presidents who express views on congressional bills.

We rely on bridge actors to make three types of connections within and between institutions. First, legislators who serve multiple sessions in the state legislature and Congress connect their respective institutions longitudinally. Second, legislators who move from the lower to the upper chamber of a state legislature connect those two institutions. And third, politicians who graduate from a state legislature to Congress connect the state and national ideological maps. Examples of these bridge actors include prominent politicians like Barack Obama and Rod Blagojevich of Illinois, David Vitter of Louisiana, Dennis Kucinich of Ohio, and Thomas McClintock of California. Although we never observe a bridge actor who serves in more than one state legislature, we can nevertheless place all the states on a unified spatial map through their common connection to Congress.

Gerber and Lewis (2004) are the sole authors to have placed a state legislature on a common scale with Congress. For a single state, California, Gerber and Lewis used interest group ratings of U.S. representatives and state Assembly and Senate legislators for the 1993–1994 period as their bridges. Specifically, the authors constructed phantom legislators to stand for the League of Conservation Voters, the Chamber of Commerce, and the AFL-CIO by referring to the votes that underlie these groups' respective ratings.[4] While an innovative approach, using interest group ratings may be problematic. Interest groups choose votes to score in a nonrandom fashion, and longitudinal and cross-chamber comparability is not always assured (Groseclose, Levitt, and Snyder 1999); groups may arbitrarily shift and stretch their scales. Finally, Gerber and Lewis did not tie any other state to Congress on a common space.

Poole (2005) provided an overview of methods for estimating a common spatial map across institutions using bridge actors. He suggests two approaches. The first, which we call *linear mapping,* extracts spatial maps for the two institutions separately and then connects them by

regressing the two sets of coordinates for the bridge actors. The second, which we call *pooled scaling,* combines the roll-call matrices across institutions into one large matrix. Using bridge actors as the glue, the researcher executes the scaling simultaneously for all the legislators across all chambers. In principle, the two methods should produce largely similar results. In practice, the vast size of a matrix containing all votes and legislators for all available time for all states and Congress renders the latter method computationally challenging.

Mapping and Scaling

For this article, we adopt both approaches but use them for different purposes. We apply pooled scaling to create comparable bichamber within-state scores, as well as to facilitate state-Congress comparisons. We pool roll-call voting decisions by state legislators across the entire time period and for both chambers. Thus, the data matrix includes rows for each state legislator who ever served anywhere or anytime in the legislature. The columns include all votes taken over the entire time period in both chambers. We mark votes as missing if the legislator was not present in a chamber at the time of the vote.

We repeat the exercise for the 103d through the 111th Congresses (1993–2009),[5] scaling congressional roll-call votes across time. This creates two separate sets of scores, one for the given state and another for Congress.

Finally, we use linear mapping to translate within-state, bichamber scores to congressional common space. We regress the within-Congress scores of each state's bridge legislators on their within-state scores, using bivariate ordinary least squares. We repeat this mapping for each state. Finally, we use the estimated coefficients from each regression to create predicted congressional common-space scores for the non-bridge legislators in each state. Because all predicted scores are now on the same scale, we can directly compare them across states (and with Congress itself).

To reiterate, our analysis relies on three sets of bridge actors. First, legislators who served in more than one session facilitate longitudinal comparisons. The vast majority of all legislators falls into this category. Second, legislators who served in both their state assembly and their state senate facilitate cross-chamber comparisons. There are dozens of these actors in our states. The last set of bridges—state legislators who went on to serve in Congress—allows us to rescale scores in cross-institutional common space. We typically have relatively few of these bridge actors because such transitions are infrequent and acquiring and

processing historical roll-call data from individual state legislatures is very costly. We explore the consequences of this data limitation later in the text.

Some might object that institutional differences across state legislatures cause great variation in agenda-setting power and observed voting behavior.[6] This variation might in turn lead to differences in what we infer from ideal point estimates. We do not consider this concern too troubling, for the following reasons. Majorities were rolled in all the states at rates not much different from those in the U.S. House, and party voting rates were actually smaller in the state legislatures than in Congress. More generally, because there is stochastic error in legislative voting decisions and agenda control is never perfect, roll-call voting data generally contain enough cutpoints to differentiate most legislators. Moreover, the simulations reported by McCarty, Poole, and Rosenthal (2006) suggest that distributions of roll-call votes are extremely insensitive to the distribution of cutpoints. This point is underlined by the fact that distributions of ideal points move in tandem between the U.S. House and Senate despite very different agenda-setting institutions (see McCarty, Poole, and Rosenthal 2009). In sum, variation in agenda control does not appear to be enough to imperil comparability of estimates across states.

We present results from a two parameter, one-dimensional Bayesian item-response model (Clinton, Jackman, and Rivers 2004; Jackman 2000, 2004, 2007; Martin and Quinn 2002) based on Markov Chain Monte Carlo (MCMC) methods.[7] We also experimented with estimating ideal points with W-NOMINATE (Poole 2007). In the interests of space, we have to omit the latter results from the present article. Nevertheless, estimates of ideal points via the two methods correlate extremely highly, confirming our suspicions that these scaling techniques yield similar results in data-rich environments (Carroll et al. 2009; Clinton and Jackman 2009).

Data

Wright (2007) has collected roll-call data on all 50 states, but only for the years 1999 and 2000. This dataset includes too few bridge actors for us to generate common-space scores. Sparse congressional turnover of incumbents means relatively fewer opportunities for ambitious state legislators to graduate to the House (much less the Senate, Barack Obama notwithstanding). We need a record of votes long enough to collect some minimal number of bridge actors to make our methodology work.

TABLE 1
State Data Description

State	Start	End	Roll Calls	Bridges
CA	1993	2008	16,632	20
CO	1996	2005	5,874	8
FL	1996	2005	2,013	15
IL	1996	2008	4,759	6
LA	1997	2006	3,292	5
MI	1996	2006	7,281	7
NC	1995	2006	2,889	6
NJ	1996	2005	1,778	5
NY	1996	2003	3,011	6
OH	1995	2008	1,416	10
PA	1996	2007	2,663	8

Consequently, we drew our state roll-call data for California from Lewis and Masket's (Masket 2009) work, and our data for the other states come from a state legislative voting data project funded by the Woodrow Wilson School and the Russell Sage Foundation. The state legislative journals of the other states for approximately the past decade were either downloaded from legislative websites or requested from the responsible state agencies. These journals, often thousands of pages in length, were laboriously disassembled, photocopied, and scanned. These scans were then processed with optical character recognition software. Finally, a set of data-mining scripts written in Perl were run to extract the roll-call information from the data files.

For this article, we analyze data from 11 states: California, Colorado, Florida, Illinois, Michigan, New Jersey, New York, North Carolina, Ohio, Pennsylvania, and Virginia. We chose these states for the number of their bridge actors, but a nice bonus is that the states are geographically diverse. Collectively, these 11 states account for 49.5% of the U.S. population in 2005. Following Poole's (2005) methods, we exclude near-unanimous roll calls (those on which the minority constituted less than 2% of the total vote). Table 1 describes the roll-call data and presence of bridge actors across states.

Scaling

Fits and Dimensionality

We begin by reporting the results from estimating within-state scores. The overall fit of the within-state scaling is shown in Table 2. Classification with one dimension averages 88.9%. Average proportionate reduction in error (APRE)—which measures the improvement in classification relative to a less naive null (for example, everyone voting with the majority)—averages 56.5%. The improvements in classification and APRE that we gain from moving up to two dimensions are 1.4% and 5.3%. These fit statistics are very comparable to those for the 103d-111th U.S. Congresses, which are 89.6% (0.8% better) and 71.7% (2.1% better), respectively. These results also comport nicely with some previous estimates. For example, Gerber and Lewis (2004) reported results for California of 89.7% correct classification and a 67% reduction in error for the first dimension.

Poole and Rosenthal (1991, 1997) famously found that Congress showed low ideological dimensionality. The one dominant dimension consistently present over the course of American history is traditionally conceptualized as liberalism-conservatism. Occasionally, a second dimension appears, but in recent history, it has receded into insignificance. This finding has been echoed in diverse institutional settings, overseas and in the United States (Poole and Rosenthal 2001).

We might have reason to suspect that there may be more than one dimension present in the American states. The policy issues in conflict at the state level may well load onto other dimensions because of state heterogeneity. For example, consider urban-rural conflict in states with dominant central cities: the Chicago-downstate conflict in Illinois[8] or the New York City-upstate conflict in New York. There is also the intrastate conflict between state natives and out-of-state migrants in rapidly growing states like Florida and Nevada.

On the other hand, we may expect that the dominant left-right dimension explains nearly all political conflict in state legislatures. State parties are increasingly organizational and ideological franchises of the national parties. Although historically heterogeneous across states, state parties have drifted into ideologically distinct camps in recent times (Erikson, Wright, and McIver 2006; McCarty, Poole, and Rosenthal 2006).

Ultimately, the question of dimensionality is an empirical one. The tools we use to assess dimensionality include observations of fit improvement and skree plots[9] when we increase the number of

TABLE 2
Within-state Fit Statistics:
Average Correct Classification Percentage
and Average Proportionate Reduction in Error

	Class% 1	Class% 2	Class% Change	APRE 1	APRE 2	APRE Change
CA	92.4	93.4	1.0	69.6	73.5	3.9
CO	87.0	87.9	0.9	52.9	56.2	3.3
FL	90.1	91.0	0.9	64.6	67.8	3.2
IL	87.8	90.8	3.0	57.9	68.2	10.4
LA	82.4	84.3	1.9	24.4	32.6	8.2
MI	90.4	91.6	1.2	70.7	74.4	3.6
NJ	92.5	93.3	0.8	69.3	72.6	3.3
NY	91.2	92.2	1.1	52.0	57.8	5.8
NC	87.0	88.5	1.5	47.0	53.0	5.9
OH	88.3	90.0	1.7	58.5	64.4	5.9
PA	88.7	89.8	1.1	54.1	58.7	4.6
Average	88.9	90.2	1.4	56.5	61.8	5.3
Congress	89.6	90.3	0.8	71.7	73.8	2.1

dimensions. We have already shown that fit improvements for the states in the study are modest, and comparable to those for Congress. If additional dimensions were present, then such improvements would be considerably larger. Illinois is a genuine outlier, however, with a small but noticeably larger improvement in fit gained when we include a second dimension.

Even if one dimension accounts for nearly all political conflict in any given state, that dimension may be different between the states or from Congress's dominant dimension. An urban-rural divide may be dominant in some states, a water-usage dimension in another, and so forth. If Congress was primarily organized on a left-right ideological scale while states were primarily organized along different first dimensions, then our project of rescaling nonbridge actors into common space might be in peril. But if that were the case, then within-state and within-Congress ideal point estimates for our bridge actors would not be correlated very much. And, in fact, the correlations of these scores are uniformly high. In our 11 states, the correlations average 0.95 and fall no lower than 0.90 (all p-values are less than 0.05).[10]

Linear Mapping

Since the first dimension so dominates the second, we drop analysis beyond the first. We regress the U.S. Congress scores for our bridge actors in each state on those legislators' within-state scores. These regressions produce a set of intercept and slope coefficients mapping scores from state space to congressional common space. Fits are extremely good, and, as we expected, more conservative (liberal) bridge legislators in the states are more conservative (liberal) in the Congress, too.[11]

We use these coefficients to generate predicted scores for the nonbridge legislators from each state. We plot these predicted scores in common space with a boxplot, shown in Figure 1 (density curves for each state are omitted for brevity). We then directly compare the results from different states with each other, as well as with results for the U.S. Congress. We first compare the range of ideological preferences in each institutional setting. California and Ohio stand out with the widest ranges. Pennsylvania, Louisiana, and New Jersey have the smallest ranges.

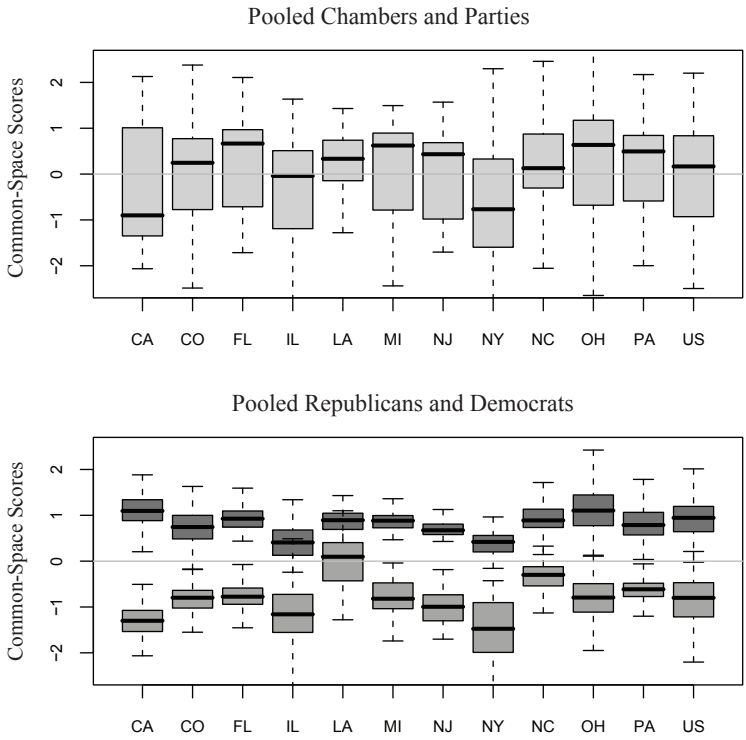
Second, we compare medians of each state's pooled scores with each other and with the pooled 103d–111th Congresses. California and New York have the most liberal legislative medians. Florida, Michigan, and Ohio have fairly conservative medians over their respective time periods. Third, we compare the party medians. Ohio and California Republicans stick out by being quite conservative. Illinois and New York Republicans are notable for their liberalism. California and New York's Democrats, on the other hand, are very liberal. The most conservative Democrats are from Louisiana and North Carolina.

What if we had failed to rescale the within-state scores? How much of a difference does our mapping strategy make? The left and middle boxplots in Figure 2 tell the story. The short answer is that the difference rescaling makes varies state by state and party by party. Republicans and Democrats might be erroneously viewed as more or less extreme than they really are.

Another interesting exercise is to compare state congressional delegations to the state legislatures themselves. We have good reason to expect that the distributions of these two sets of preferences are not independent. First, both are anchored by state public opinion (Erikson, Wright, and McIver 1993). Second, as the presence of bridge actors so prominently emphasizes, members of Congress are drawn from a pool of state elites that also supplies state legislators.

The middle and right boxplots in Figure 2 show the results of our comparison of party medians for state legislatures and congressional

FIGURE 1
Estimated Congressional Common-Space Scores for 11 States Compared with Scores for the U.S. Congress



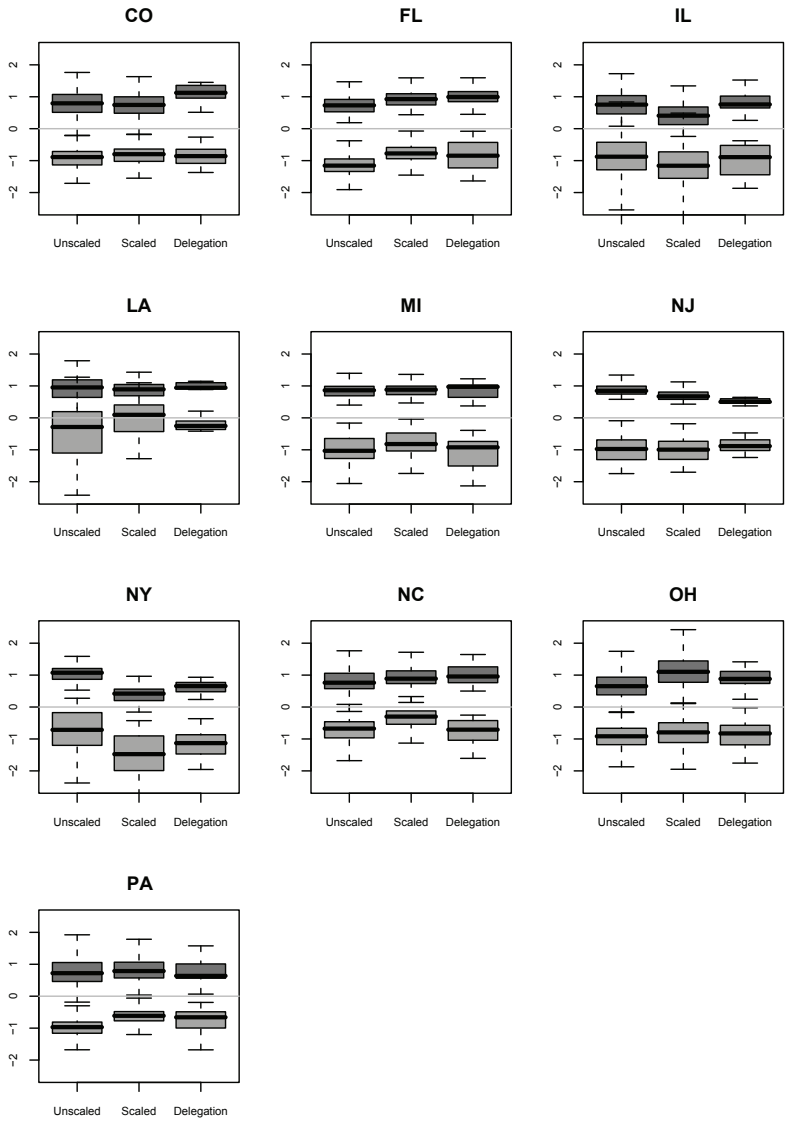
Note: The top plot of Figure 1 pools both parties, while the bottom plot separates them (Republicans represented by darker gray).

delegations. The distribution of ideology in state congressional delegations can be fairly different from such distributions in state legislatures. Thus, it would seem problematic to use state delegation scores as proxies for state legislatures, as done by Berry et al. (1998).

Disaggregating Scores by Chamber and Year

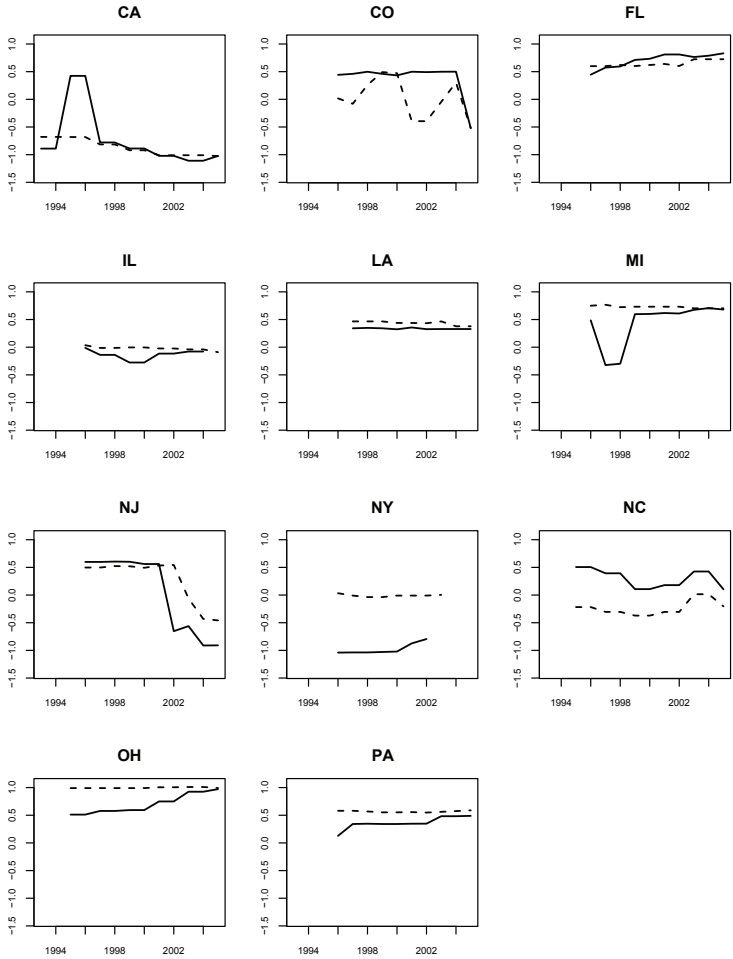
Because estimated congressional common-space scores are fixed (by an assumption we justify elsewhere in this article) over the course of each legislator's career, the only way we observe shifts in aggregate

FIGURE 2
Comparison of Ideological Distributions of State Legislatures



Note: Figure 2 compares the ideological distributions of state legislatures in within-state space (unscaled), state legislatures in common space (scaled), and congressional delegations in common space. Republicans are represented by darker gray.

FIGURE 3
Chamber Medians over Time



Note: The solid line is for the lower chamber, and the dashed line is for the upper chamber.

chamber ideal points is through replacement and party switching. We measure the chamber medians to get a sense of the aggregate distribution of ideology in the state legislature. Figure 3 shows these medians for the upper and lower chambers of each state.[12] States are quite different from each other in this regard. Louisiana's chamber median barely budges over the time period, while the medians for Colorado, New Jersey,

and North Carolina change rather rapidly. Michigan's results reflect changing partisan control for the state House in successive elections in 1996 and 1998, as do results for California's Assembly in 1994 and 1996 and New Jersey in 2001.

Comparing Scores

To what degree are the congressional common-space scores for the state legislatures in this article consistent with other measures of state ideology? Berry et al.'s (1998) state elite scores are publicly available for the 1968–2002 period. Berry et al. derive these scores from a formula that is a weighted average of party proportions in both chambers multiplied by state delegation congressional ideology.[13]

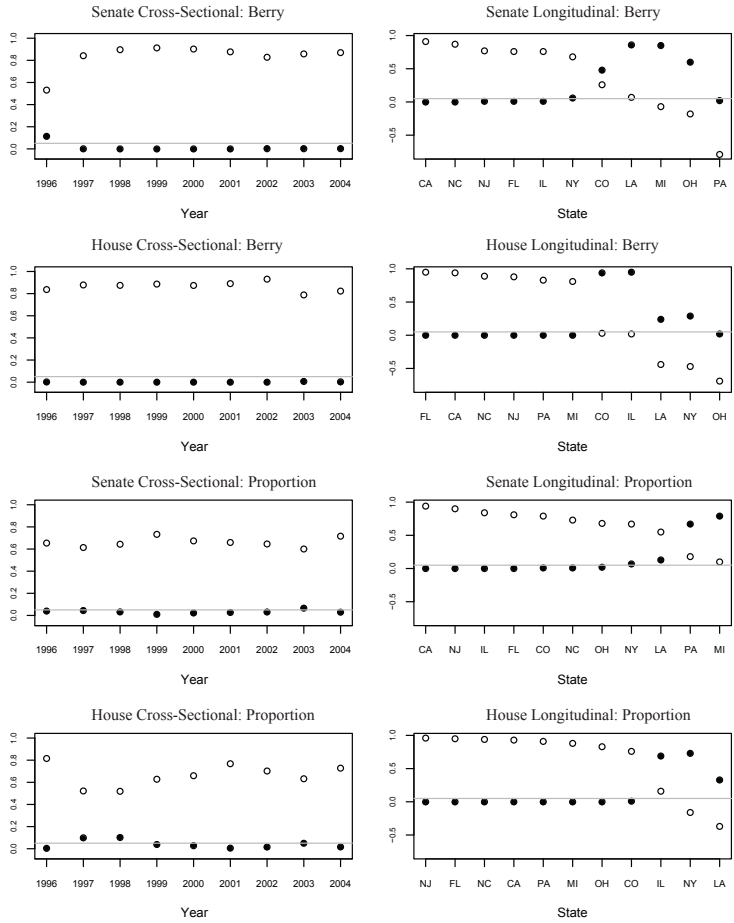
We replicate the Berry scores but made some slight modifications, for two reasons. First, we need to excise the inferred gubernatorial ideology, because we do not have common-space scores for governors to compare. But because the governor's position for Berry et al. is itself merely the average of own-party ideology, we consider it only a reweighting of the inferred legislative ideology. We also separate the component calculations for the upper and lower chambers to achieve a more fine-grained comparison between the two series of scores. We thus generate what we call "Berry component scores."[14] Finally, we want to extend the comparison beyond 2002.

As we have shown, congressional delegations are not a perfect proxy for state legislatures. What consequence follows from this? We investigate this question longitudinally and cross-sectionally. That is, we asked, to what degree are the Berry component scores correlated with congressional common-space chamber medians within each state (or year)?

The performance of the Berry scores is very uneven. The left side of Figure 4 shows that their cross-sectional performance is fine (although not perfect). The correlation coefficient averages 0.82 and 0.85 for the upper and lower chamber, but it falls as low as 0.53 for the upper chamber and 0.70 for the lower chamber. The *p*-values are at 0.11 or below. If we compare party proportions in the state legislatures to common-space scores, then we find similar correlations, averaging 0.69 for both chambers, but with 1996 insignificantly correlated in both.

The longitudinal performance, on the other hand, is often wrong, as the right side of Figure 4 shows. Longitudinal correlations between the Berry component scores and common-space chamber medians were insignificant ($p > 0.10$) or incorrectly signed in 10 of 22 chambers. In some cases (the Pennsylvania Senate and the Ohio House), the Berry component scores were correlated *negatively,* and significantly so, with

FIGURE 4
Correlation of Berry et al. (1998) Component Scores
and Party Proportions for State Upper and Lower Chambers
with State Average Congressional Common-Space Scores



Note: The correlation coefficient is the empty circle; the associated *p*-value is the filled circle. The left column is cross-sectional by year; the right column is longitudinal by state.

chamber medians recovered by our procedure.[15] Using simple party proportions improves matters, yet the correlation remained insignificant or incorrectly signed in 6 of the 22 chambers.

How does this magnitude of error occur? We can examine Ohio more closely for an example. The failure is the consequence of two

simultaneous trends: a leftward tilt by Ohio congressional Democrats that was not matched by their state counterparts, and a strong rightward drift by state Republicans not matched by their copartisans in the congressional delegation. Because the Berry scores cannot directly reflect these trends, they provide an incorrect estimate of Ohio legislative ideology, one that implies more liberalism in the legislature from 1995 to 2005. In contrast, our common-space median measure correctly registers the overall ideological shift in the Ohio legislature over this decade: the chamber median increases in a conservative direction, from 0.75 to 0.98.

The Berry scores, then, perform relatively well in assessments of state legislative ideology across states within a given year, but they function quite badly in assessments of ideological change within states across time. This result should be disquieting for researchers in state politics. Lacking scores for all 50 states for a long time period, what are they to do when working with the time-series cross-sectional data for all the U.S. states that is so prevalent in the subfield? On the basis of this analysis, we would urge fellow researchers to use raw party proportions instead of the Berry scores.

Applications

Our estimates of congressional common-space scores allow us to weigh in on a number of debates in the legislative and state politics literatures, such as those regarding partisan polarization and state policy differences.

Polarization

Studies of the U.S. Congress have found that parties have become highly polarized in Congress in recent years (Layman, Carsey, and Horowitz 2006; McCarty, Poole, and Rosenthal 2006; Poole and Rosenthal 1984). Plagued by insufficient data, scholars have not been able to ascertain if such a trend manifests at the state level. Aldrich and Battista (2002) investigated single sessions of single chambers of several states. They found examples of polarized state legislatures in more-or-less competitive states and unpolarized state legislatures in essentially one-party states.

We examine partisan polarization across states and over time in the 11 states. Because scholars have heretofore been unable to place state legislatures on a single scale, their comparisons of legislative polarization have had to rely on scales invariant to linear transformations of the ideal points. These scale-invariant measures come in two classes.

The first scale-invariant measures depend only on the ordinal properties of the ideal point measures. A commonly used measure of this class is party overlap, which researchers generally measure by the number of Democrats to the right of the most liberal Republican and the converse. The key problem with this measure is that it is not very robust to outliers. A single conservative Democrat can depress this polarization measure for an entire chamber. This specific obstacle can be mitigated if authors use some other percentiles to compute the overlap region, such as the number of Republicans to the left of the 90th centile Democrat. There are two problems with such an approach, however. First, the measure ignores all information about partisan distributions other than the density of their moderate tails. Second, the more the Democratic centile is raised and the Republican centile is lowered, the more likely the overlap measure is to be 0, independently of actual polarization. Making the measure more robust thus makes it less informative.

The second class of scale-invariant measures requires that the measures be normalized. The researcher assumes the ideal point measures to be identified up to a linear transformation $x^* = a + bx$, where x^* is the estimated measure and x is the true measure. So if one has estimates of b for each state, one can compute comparable differences-in-means and differences-in-medians across states by simply dividing each sample measure by the state's b. Of course, one does not observe b. So some authors use the fact that $se(x^*) = b \cdot se(x)$ and normalize by $se(x^*)$. But this method assumes that $se(x)$ is constant across states, which is a very strong assumption—especially when one wants to measure differences in polarization across states.[16]

Because we can estimate state legislators on a common scale, we can avoid many of the problems associated with the scale-invariant measures. We are able to use not only cardinal and ordinal information, but also measures that take into account the full distribution of each party's positions. And because all states are on the same scale, there is obviously no reason to normalize.

Two such measures that are sensitive to scale are reported in Table 3. The first is a simple difference in party medians. Note that Aldrich and Battista (2002) employed this comparative measure, using *unscaled* within-state scores. The second is a party-free average distance between every possible pair of legislators.

Louisiana stands out for being relatively nonpolarized. In contrast, California is extremely polarized. Jacobson (2004) used indirect methods to measure the ideological polarization of the California legislature and arrived at a similar conclusion. If we had not rescaled the estimates of state legislator scores that came from a purely within-state

TABLE 3
Scale-variant Polarization Measures

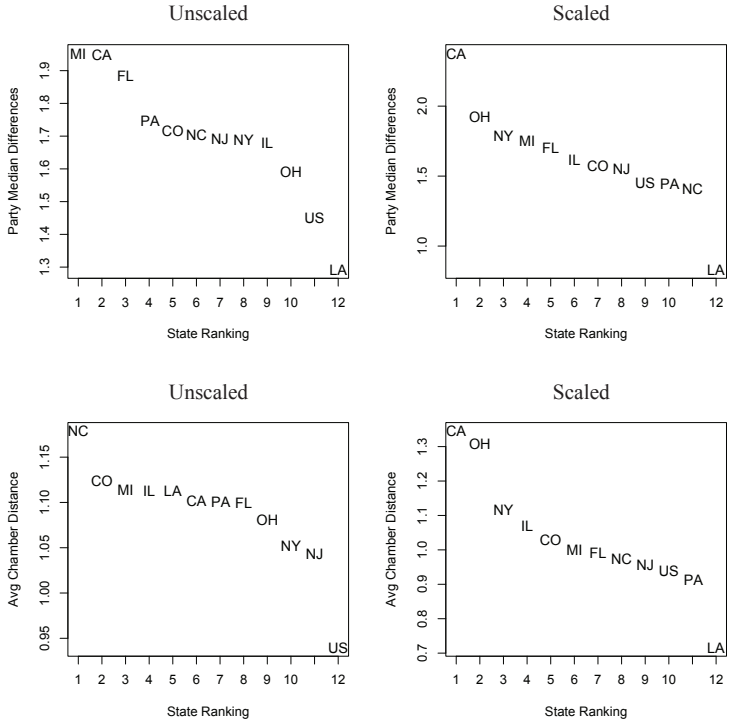
Legislature	Party Differences	Average Distance
LA	0.83	0.72
NC	1.41	0.97
PA	1.45	0.91
U.S. Congress	1.45	0.94
NJ	1.55	0.96
CO	1.57	1.03
IL	1.61	1.07
FL	1.70	0.99
MI	1.76	1.00
NY	1.79	1.11
OH	1.92	1.31
CA	2.38	1.34

Note: The first column is the common-space ideological distance between Democrat and Republican party medians in the legislature. The second column is a party-free average distance between every possible pair of legislators.

analysis, then we would have drawn wrong conclusions regarding the extent of polarization in each state relative to the others and to Congress as a whole. Figure 5 graphically illustrates these differing conclusions. Notice substantial differences, for example, in the relative positions of Congress, Michigan, and Ohio.

Where does legislative polarization originate? We can imagine two sources: the electorate itself, or the party system in a state. That is, voters choose more or less extreme politicians and we observe polarization between parties in some states and some years but not others. McCarty, Poole, and Rosenthal (2006) have argued, in the context of the United States as a whole, that increasing income inequality (itself driven partly by immigration) leads politicians in both parties to polarize, motivated by the increased electoral returns gained from specializing by ideology. Although this scenario offers a longitudinal story for the United States, we can see if the argument holds up in cross section. Figure 6 provides some evidence in the affirmative. Highly polarized states, like California, also exhibit very high levels of income inequality. This inequality is, in large part, driven by the state's very high level of immigration. Ohio and Pennsylvania, on the other hand, show far less polarization and inequality.

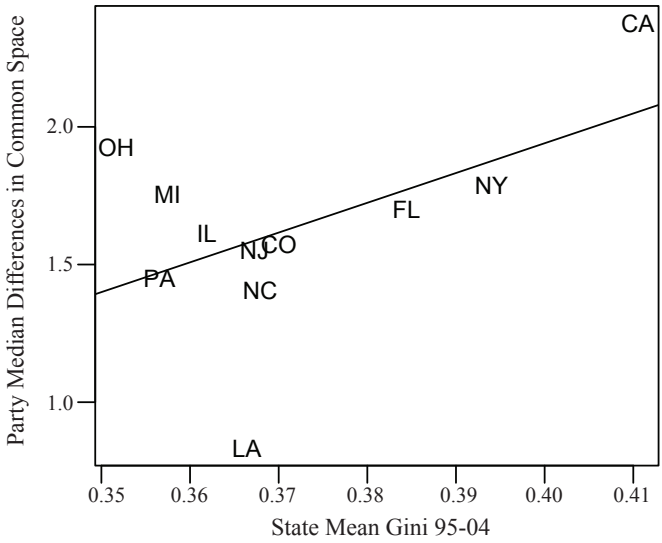
FIGURE 5
Polarization of State Legislatures as
Measured by the Differences between
Party Medians (top) and Average Chamber Distance (bottom)



Note: In the graphs on the left, the measures are derived from the unscaled within-state scores. In the graphs on the right, the measures are derived from the mapped, congressional common-space scores. Note the substantial differences derived from mapping.

Alternately, polarization may emanate from political parties themselves. Fiorina (2005) has posed one version of this argument, noting that, given a binary voting choice, moderate voters have no true alternatives. Democrats and Republicans are free to drift in more-extreme directions. Aldrich and Battista (2002) have posited another version of this argument, claiming that the presence of a competitive party system is a necessary condition for polarization; their claim follows in the spirit of Key's (1949) observation that one-party Southern states lacked debate on ideological grounds. We did not, however, find such

FIGURE 6
Party Difference and Inequality



Note: This figure depicts differences in pooled state legislative party medians and average ideological distance plotted against state-level income inequality as measured by the Gini coefficient.

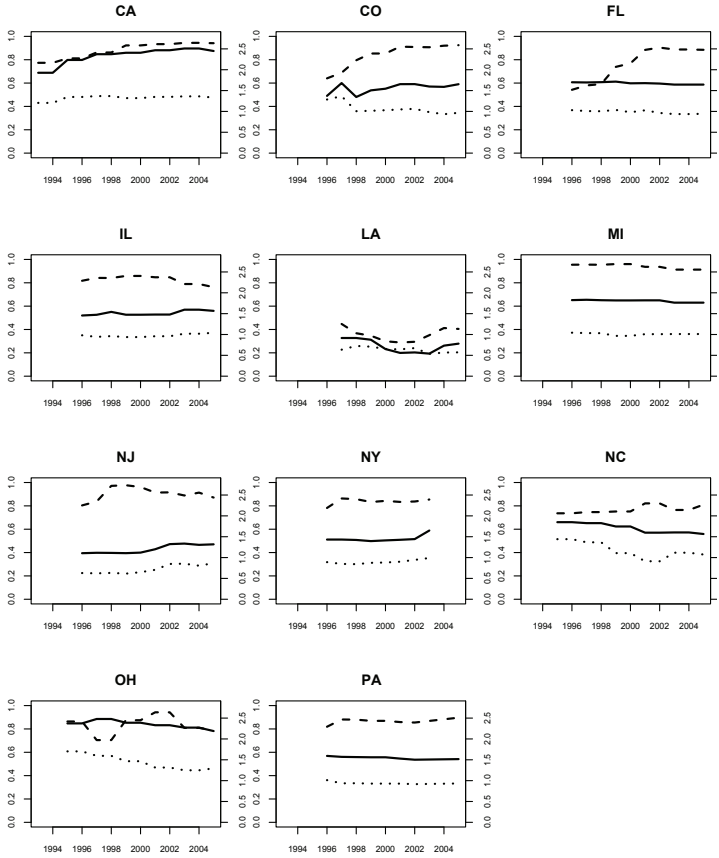
a relationship. The 11 states we examined differ only very slightly in the overall competitiveness of the two parties.[17]

So far we have looked at cross-sectional variation in polarization. Our new data allows us to examine longitudinal variation, as well. Figures 7 and 8 plot party polarization over time in state upper and lower chambers. In most of the states, polarization appears to have increased over time or remained stable. Nevertheless, the majority of variation in polarization seems to be primarily cross-sectional rather than longitudinal.

Policy

State legislative ideology should be related to state-level policy outcomes, since ideology presumably measures policy preferences. We compared our scores to widely used measures of policy liberalism. These composite scores were first discussed by Erikson, Wright, and McIver (1993)[18] and updated by Gray et al. (2004) to reflect state policy choices relevant as of 2000.[19] As we expected, state policy liberalism for the year 2000 correlated very highly with the cross-sectional state legislative

FIGURE 7
Measures of Polarization in the State Senates

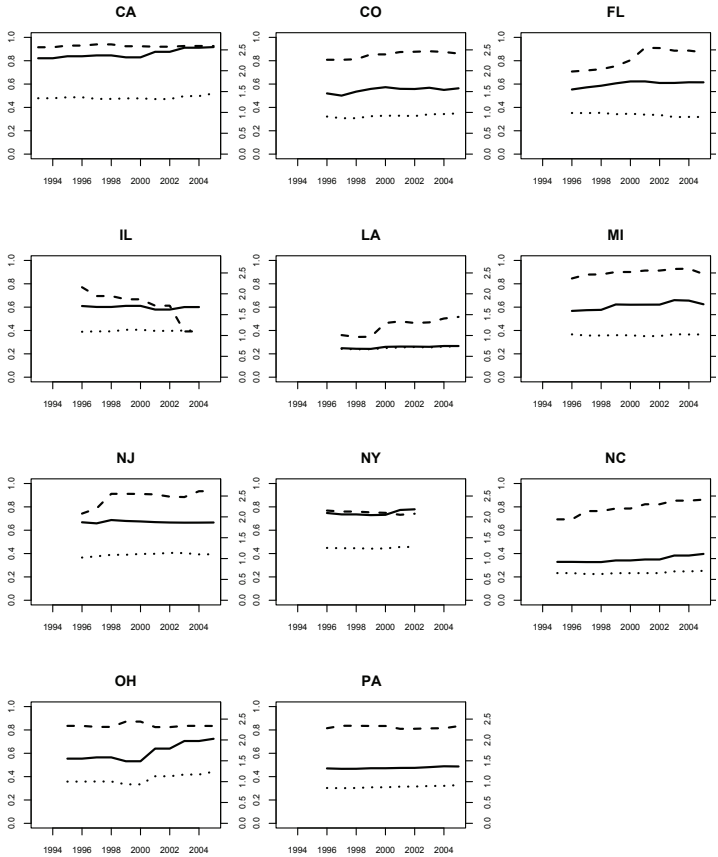


Note: Dashed line is R^2 from a regression of party on ideology, dotted line is the average distance between members, and the solid line is differences in party medians.

medians. Figure 9 shows that more conservative state legislatures are associated with more conservative policy outputs.

We now turn to an example of a situation in which using the Berry et al. (1998) legislative ideology scores leads to conclusions that are likely to be substantively wrong. Fellowes and Rowe (2004) sought to explain the variation in welfare policy after the enactment of the Temporary Assistance for Needy Families (TANF) program. The authors tested a whole host of predictors—including the Berry elite ideology scores—on 47 states for the 1997–1999 time period.[20]

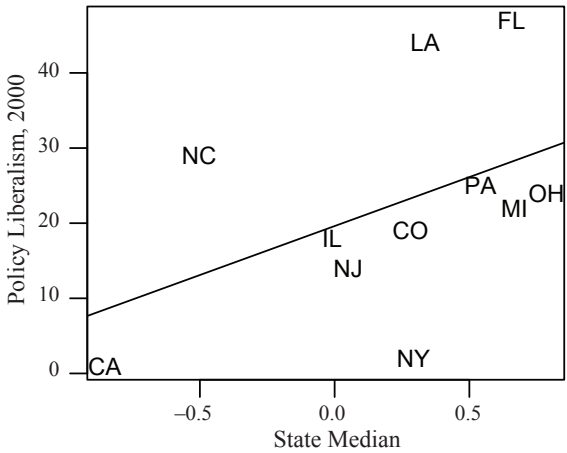
FIGURE 8
Measures of Polarization in the State Houses



Note: Dashed line is R^2 from a regression of party on ideology, dotted line is the average distance between members, and the solid line is differences in party medians.

Investigating the size of the TANF benefits for a family of three, Fellowes and Rowe expected to find that more liberal state legislatures were associated with larger cash benefits. Instead, using the Berry scores, they discovered the opposite. Although the Berry scores predictor was signed incorrectly, at least it was statistically insignificant. In contrast, when Fellowes and Rowe tested an alternative measure of legislative ideology—legislative party composition—they found a significant effect in the presumed wrong direction: more conservative legislatures were linked to more generous welfare benefits.

FIGURE 9
State Medians and Policy



Note: This figure is a scatterplot of the cross-section of pooled state legislative medians for the entire time period (x-axis) against the 2000 policy index from Gray et al. (2004).

We replicated Fellowes and Rowe's analysis using our data. We constructed a time-series, cross-sectional dataset for our 11 states over the 1997–2005 time period. Like Fellowes and Rowe (2004), we used the Urban Institute's *Welfare Rules Database* (Rowe and Versteeg 2005). We employed state and year fixed effects to control for omitted variables that vary at the state and year levels. We also modeled a panel-specific AR1 process to allow for time trends that vary by state. Our primary predictors were the average of the chamber medians for a given state in a given year (*Chamber Conservatism*), along with a composite *Berry Legislative Conservatism* score and average *Proportion Republican*.

We replicate part of Fellowes and Rowe's (2004) results, as shown in the Model 1 column in Table 4. We found both Berry Legislative Conservatism and Proportion Republican to be signed incorrectly.[21] Using congressional common-space scores (see Model 2 column), we found conservative legislatures (with conservatism measured by ideology, not party) to be associated with less generous welfare benefits, not more.[22] Although this is not a definitive test, it does illustrate the consequences of employing a relatively weaker proxy for state legislative ideology, especially in the context of panel data. Admittedly, the use of such proxies is often unavoidable, but scholars should be mindful of their flaws.

TABLE 4
Models of Welfare Policy and Legislative Ideology in the States

Variables	Model 1 (1)	Model 2 (2)
Chamber Conservatism		–53.0 (19.4)***
Berry Legislative Conservatism	38.0 (41.2)	
Proportion Republican	94.1 (81.1)	454.0 (134.3)***
Republican Governor	4.3 (3.2)	3.8 (2.7)
Unified Government	10.1 (4.1)**	15.3 (5.2)***
Legislative Professionalism	17.9 (53.4)	13.9 (63.1)
N	93	93

Note: The dependent variable is inflation-adjusted TANF benefit for a family of three, 1997–2005. Coefficients on fixed effects for states and years are omitted. Panel-corrected standard errors appear in parentheses.

***$p < .001$; **$p < .01$; *$p < .05$.

Bridging Concerns

Because we estimated state legislative scores after pooling across chambers and sessions, we assume individual state legislator scores are constant throughout the legislators' careers. Aggregate ideological change over time comes from replacement, not conversion or adaptation.[23] Furthermore, we assume the state legislators—our bridge actors—who go on to Congress will have consistent ideologies across the institutional divide. This identifying assumption allows us to rescale state legislative ideal points to congressional common space.

But is this assumption justified? We argue it is, on two separate grounds. First, we agree with Poole that political elites exhibit both coherent belief systems (Converse 1964) and ideological intensity (Poole 2003). Issue positions are related, even if philosophically (or logically) such positions may not necessarily cohere (for example, positions regarding environmentalism and gun control). These relationships are anchored by the ardent passion of ideologues. Second, parties structure voting agendas in legislatures and constrain individual ideological drift (Jenkins 2000). In the electoral arena, parties weed out nonconformists as they develop and promote candidates.

Empirical research on Congress has largely confirmed the consistency assumption. Poole and Rosenthal (1991) and Poole (2003) have argued that ideological change in Congress has been almost entirely driven by replacement.[24] On the other hand, there has been almost no research on the ideological consistency of state legislators. We assume that the same ideological and institutional factors constrict the change of state legislators' policy positions, but we have little evidence in the literature on this score.[25]

We therefore proceed with an analogue of an empirical test for ideological consistency suggested by Poole (2005). The iterative procedure involves removing one bridge actor at a time and treating that legislator as two separate people with two distinct ideal points (one each for the state and federal levels). If the two points match, according to the information gained solely from the other bridge actors, then that particular actor is ideologically consistent.

We implement a version of this procedure for our linear-mapping approach. For each state, we loop through each of the bridge actors, dropping each in turn from the linear regression of the bridge actors' state scores on their congressional scores. There are as many separate regression estimates as there are bridges, and therefore as many sets of predictions for all the state legislators. We use the prediction for each of the dropped bridged actors and compare it with that legislator's true Congress score.

If our bridge actors are perfectly consistent, then our predicted score should exactly match the true Congress score, and if the set of our bridges is consistent, then we could plot the scores and draw a 45-degree line right through them. We omit the plot because of space considerations, but the results show almost exact adherence to the diagonal in all the states.

Even if our bridge actors are, in fact, ideologically consistent as they move from state legislatures to Congress, do we have enough of them for proper analysis? Common-space scores created to bridge the U.S. House and Senate, for example, exploit large numbers of bridges. But we do not yet have a good methodological rule of thumb regarding the lower bound of bridges necessary to construct a common space. Knowing the minimum is important, because we are not likely to see huge numbers of bridge actors in U.S. states, given the data limitations.

To answer this question, we conduct a Monte Carlo analysis of the performance of NOMINATE and item-response model ideal point estimation techniques under a variety of theoretical conditions (including the number of bridges, the size of the chambers, the party proportions, and the mean and variance of true ideological positions). We run this simulation for a large number of iterations, creating unique sets of legislators and roll-call votes. We then estimate ideal points from

the revealed vote decisions of the legislators we create. Comparing these points with the known, true ideal points of these individuals gives us a barometer to determine how well we are doing.

Our basic finding is that a low number of bridge actors does not harm the overall fitness of estimated ideal points.[26] In fact, even with as few as four bridge actors, bridging via linear mapping does very well. We recover true ideal points much better than when we naively scale chambers individually for very different chambers (the best case for rescaling), and we rarely err drastically when the chambers are exactly alike (the worst case). The latter result is impressive because—even when no rescaling is necessary at all—introducing a small number of noisy bridge actors does little to no harm. Nor does lack of representativeness of the bridge actors matter; as long as they vote in an ideologically consistent manner, we can recover the true ideal points very well.

Conclusion

This article should be viewed as testimony to the wisdom of the dictum "compare, but carefully." The absence of data and the right method has prevented legislative scholars from making valid comparisons of state legislative ideological preferences. We remedy this problem by using connections between ambitious politicians' ideology in their state legislatures and in Congress. Of course, generating results for 11 states is merely suggestive of a true cross-state common space. More extensive data collection remains to be done.

Nevertheless, the extensive use of ideal points in studies of legislative organization and policymaking in Congress points the way forward for studying state legislatures. Indeed, the inherent heterogeneity of state legislatures should generate even more promising territory for testing theory. Perhaps more important, this article underscores the potential of using a common space to analyze the American political system. For the first time, a common ideological scale allows researchers to compare otherwise disparate institutions and draw inferences about their consequences.

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NOTES

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1. Poole (2005) provides an introduction and survey of ideal point estimation.

2. Erikson, McIver, and Wright originally developed cross-sectional measures but have recently produced longitudinal estimates of state opinion liberalism and public partisanship—although not party elite ideology—for the 1977–2003 period (Erikson, Wright, and McIver 2006).

3. Since the same survey was administered across states, however, cross-state comparisons of partisan elites are obviously valid.

4. Interestingly, Gerber and Lewis also put voters on the same space by scaling ballots of Los Angeles County voters, treating their voting decisions on initiatives as legislative votes.

5. Voting data on the 111th Congress was current as of July 2009.

6. We thank a reviewer for making this point. See Snyder 1992 for a formal argument to this effect, but also see Rosenthal 1992.

7. Bafumi et al. (2005) discuss the practical issues involved in this estimation strategy.

8. Much of downstate Illinois is Republican, but there are several highly Democratic counties bordering Missouri.

9. Skree plots graphically summarize the sizes of the eigenvalues from a NOMINATE scaling. A rule of thumb for inferring dimensionality from such plots is to look for an "elbow" where subsequent dimensions show rapidly diminishing eigenvalues. For multiple examples of skree plots, see Keith Poole's "Voteview" website (Poole 2003). Also see the site for an extended discussion of dimensionality in the context of the Supreme Court. To conserve space, we do not present skree plots here, but they confirm the presence of a single dominant dimension, with the potential presence of additional dimensions in a few states.

10. It is possible these high correlations may conceal substantively different left-right dimensions, but this variation would not undermine our methodology of translating within-state scores to a common space.

11. A very few outliers exist, primarily in Illinois. The biggest of these is David Phelps (D-IL), who voted as a moderate "Blue Dog" Democrat in the U.S. House following his 1998 election. Phelps was eventually ousted by conservative Republican John Shimkus, another incumbent, following the 2002 redistricting, which radically redrew Phelps's downstate Illinois district into an even more Republican direction. Yet Phelps was far more conservative when serving in the Illinois state legislature than in Congress, a pattern out of line with the consistency assumption.

12. We omit the equivalent plot of party medians in each chamber for brevity's sake, but we can provide those figures upon request.

13. Berry scores are weighted 25% for each chamber. Gubernatorial ideology is assumed to be the average of the own-party ideology (e.g., the congressional delegation) and is weighted 50%.

14. This Berry is no relation to any of us!

15. We also average the component scores together as Berry et al. (1998) did, but the results hardly changed.

16. Measures based on such normalizations are not meaningless. They simply need to be interpreted appropriately as measures of partisan differences relative to total variation in ideal points.

17. Louisiana does have the least competitive party system and is also the least polarized, but apart from that state, polarization varies dramatically while variation in competitiveness is far more muted.

18. These composites included measures of education, Medicaid, Aid to Families with Dependent Children (AFDC), consumer protection, criminal justice, legalized gambling, ratification of the Equal Rights Amendment, and tax progressivity.

19. The updated composites reflect state policies on gun control, abortion, right-to-work laws, tax progressivity, and stringency of welfare-eligibility rules.

20. Fellowes and Rowe excluded Alaska, Hawaii, and Nebraska.

21. By *incorrectly,* we mean relative to our intuition that conservatives and Republicans should be less, not more, likely to award generous welfare benefits to qualifying families.

22. Note that the positive coefficient for proportion Republican is not as straightforwardly interpretable, given the inclusion of chamber conservatism. See, also, Jordan and McCarty 2010 for additional discussion of ideology and welfare policy.

23. Party switchers are the exception, and we treated them as if they were two separate individuals with two distinct scores.

24. "Since World War II, individual movement has been virtually nonexistent. . . . Politically, selection is far more important than adaptation" (Poole and Rosenthal 1991, 256–57). And, according to Poole (2003), "members of Congress die in their ideological boots. That is, based upon the roll-call voting record, once elected to Congress, members adopt an ideological position and maintain that position throughout their careers—once a liberal or a conservative or a moderate, always a liberal or a conservative or a moderate." A legislator's ideology remains consistent even when that politician is planning retirement (Poole and Rosenthal 1997), preparing to run for higher office (Poole and Romer 1993), or facing the consequences of redistricting (Poole and Romer 1993).

25. Kousser, Lewis, and Masket (2007) have, however, documented an exogenous shock (the California gubernatorial recall election) that led to small but significant ideological change in a state legislature.

26. See Shor, McCarty, and Berry 2008 for more details.

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