A Online Appendix

A.1 Monte Carlo Experiments

A.1.1 Bridging Chambers using Bridge Voters

We used the NPAT to bridge different state legislatures. In particular, a legislator served as a bridge voter if he or she voted in the legislative chamber and also responded to the NPAT. We are likely to observe more individuals and fewer items in the NPAT relative to a session of a state legislature. Motivated by this, consider two chambers—the first with $N_1 = 1000$ individuals and $T_1 = 100$ items and in the second with $N_2 = 100$ individuals and $T_2 = 1000$ items. We assume that there are B bridge voters and we vary $B \in \{5, 10, 20, 100\}$, which represent state legislators who responded to the NPAT. We expect $B = 100$ to deliver good results—it represents the best case scenario we observe in the data—there are a large number of U.S. Representatives that responded to the NPAT in 2000. $B = 10$ represents the lower end of the typical cases we observe—in most chambers, there were at least 10 state legislators who responded to the NPAT in the time period we study.[1]

We generated the population parameters as follows. We drew $\alpha_n \sim Uniform(-2, 2)$ and we drew $a_t \sim N(0, 1)$ and $b_t \sim N(0, 1)$ independently. Since these represent the "true" population parameters, we held these fixed and drew $Y_{nt} \in \{0, 1, 2\}$ according to the item response theory model specification. We drew $R = 100$ replications of this data set. In experiments, we found that one parameter was important in obtaining the best possible results. When we observe many votes for a given voter, the penalty terms (which represent the precision of the prior) do not seem to have much of an effect. When, however, there is a group of voters for which we observe a relatively small number of votes, the voters tend to be more affected by the prior, so it is helpful to reduce the prior precision for these voters. We found that it was useful to set the ratio of precision equal to the ratio of votes for

[1] We report the actual number of bridge voters in Table 3 in Appendix A.3.

these two groups. Following this, we set the penalty term $\lambda_{\alpha,n} = \frac{100}{1000} = 0.1$ for individuals representing NPAT respondents and we set $\lambda_{\alpha,n} = 1$ for the individuals representing state legislators. Using a similar logic, we set $\lambda_{\delta,t} = 1$ for the items representing NPAT respondents and we set $\lambda_{\delta,t} = \frac{100}{1000} = 0.1$ for the items represent state legislative roll call votes. As this approach produced good results, we followed this approach when generating the actual estimates reported later in the paper.

The results are given in Figure 1. As can be seen, for all four values of B and for both groups of voters, we observe very little bias. Moreover, while the RMSE is smaller for the individuals representing state legislators (a consequence of the fact those individuals have more items), there are no detectable differences in the RMSE for different values of B. Evidentally, this indicates that even 5 bridge voters is enough to obtain good results. Our results here are consistent with the findings of Shor, McCarty and Berry (2008), who test similar methods and find that bridging using bridge voters is effective even when only a few bridge voters are available.

A.1.2 Bridging Chambers using Bridge Votes

The next set of experiments investigate whether a small number of bridge votes can accurately bridge together general population (NAES) and elite (NPAT) survey responses. Once again, we simulated two chambers. The first chamber had $N_1 = 10000$ individuals and $T_1 = 50$ items and the second chamber had $N_2 = 1000$ individuals and $T_2 = 200$ items. The sizes reflect the fact that the NAES has a larger number of respondents with fewer items and the National NPAT has a smaller number of respondents and a somewhat larger number of votes. We assume that there are B bridge votes and we vary $B \in \{5, 10, 20, 100\}$. We followed a similar procedure for setting the penalty terms—we set $\lambda_{\alpha,n} = 0.1$ for individuals representing NAES respondents and we set $\lambda_{\alpha,n} = 1$ for the individuals representing NPAT respondents. We set $\lambda_{\delta,t} = 1$ for the items representing NAES items and $\lambda_{\delta,t} = .25$ for the

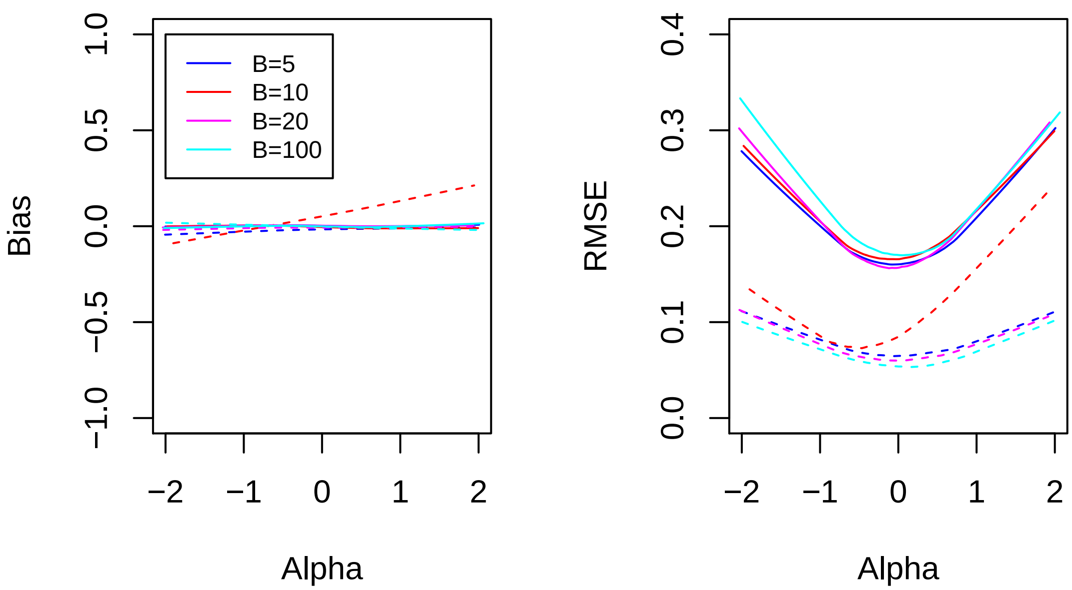

Figure 1: Bridge Voter Monte Carlo Experiment Results – Solid lines indicate the results for the larger chamber and dotted lines indicate the results for the smaller chamber.

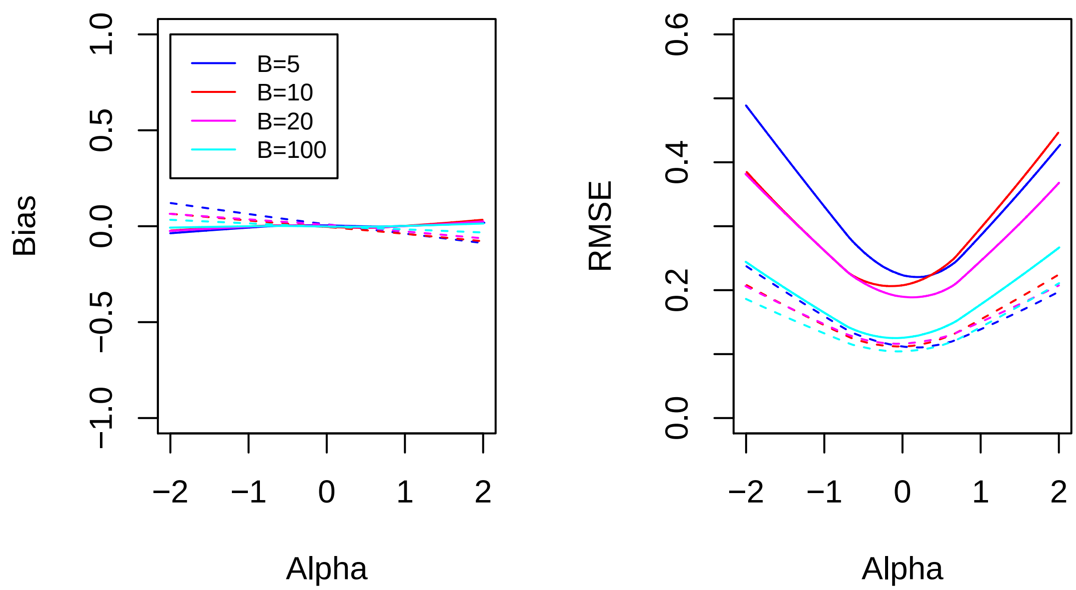

Figure 2: Bridge Vote Monte Carlo Experiment Results – Solid lines indicate the results for the larger chamber and dotted lines indicate the results for the smaller chamber.

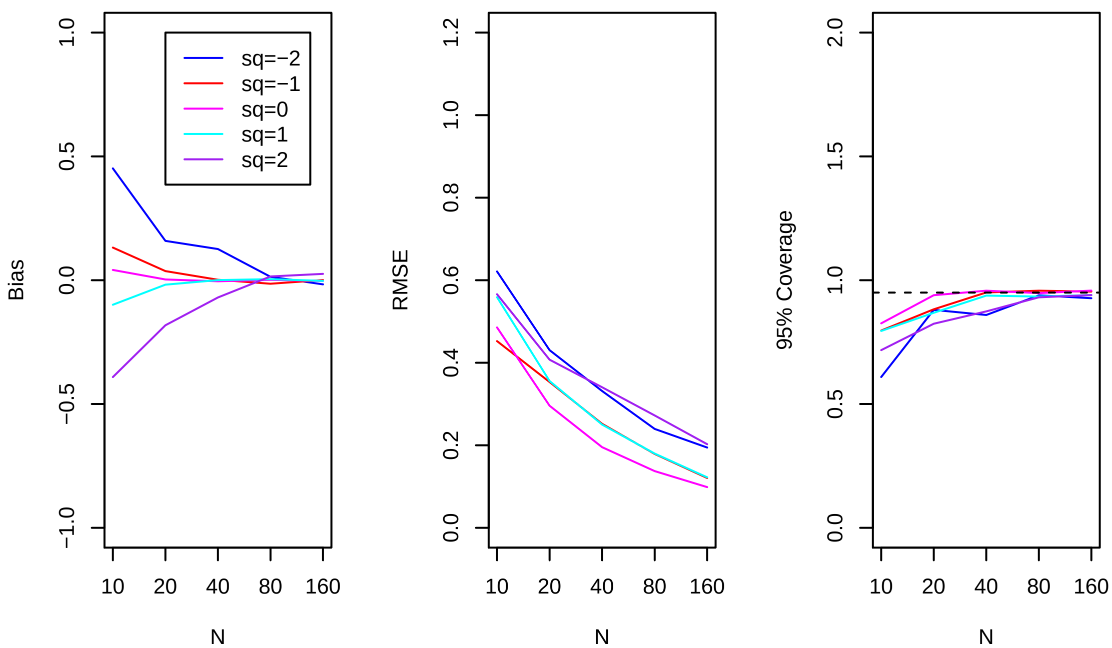

Figure 3: Policy Status Quo Location Monte Carlo Experiment Results.

items representing NPAT responses.

We again generated the population parameters as $\alpha_n \sim Uniform(-2, 2)$, $a_t \sim N(0, 1)$, and $b_t \sim N(0, 1)$ and drew $R = 100$ replicated data sets. The results are given in Figure 2. As can be seen there is little bias for both groups of voters even for small values of B. We again observe a larger RMSE for the set of individuals for which we observe few items (in this case, the individuals representing NAES respondents). Though there is some difference in the RMSE for different values of B, these differences are relatively small.

A.1.3 Estimating Policy Locations

The final set of simulations examine our ability to estimate policy locations on the basis of a small number of survey responses. Our estimation approach follows Richman (2011), who used the same technique to identify policy locations in the U.S. Congress, but an important difference is that the U.S. Congress is a much larger legislature than any of the state legislatures with the exception of New Hampshire. Consequently, the locations estimated by Richman often rely on a much greater number of NPAT respondents. In addition, response rates to the survey vary somewhat across states. At the upper end, we had 185 candidate responses with which to estimate the policy location of cigarette taxation in Maine in 2000. At the lower end, we had 19 responses with which to estimate the policy location of Louisiana property taxation in 2003. The minimal number of observations occurred rarely (only 3.5 percent of our estimated locations relied on fewer than 30 NPAT respondents. At the state-year level, the average number of candidate survey responses for policy locations in our analysis was 81.

Is 81 adequate to estimate policy locations? Is 19 adequate? To investigate, we ran a series of monte-carlo simulations in which we simulated survey responses and used the ordered probit model to estimate the policy location. In our simulations, we varied the number of respondents $N \in \{10, 20, 40, 80, 160\}$. The NPAT questions included 6 categories, but in our application, we collapsed these to three categories (*increase*, *remain the same*, and *decrease*). In principle, collapsing the response variable in this way may decrease efficiency, but using the original ordinal scale would have necessitated dropping additional observations due to perfect separation. The responses were characterized by two cutpoints, c_1 and c_2, as well as a slope parameter β. We set the slope parameter to 1 in our simulations and we varied the cutpoints (c_1, c_2) to be in $\{(-3,-1), (-2,0), (-1,1), (0,2), (1,3)\}$, with these corresponding to a "true" location in $\{-2,-1,0,1,2\}$. We drew random ideal points by first drawing a random party from each respondent and we assumed that the parties were of equal size. For each party, ideal points were drawn from the $N(-1,1)$ and $N(1,1)$ distributions, meaning that the ideal points were drawn from a bimodal distribution with mean zero and variance 2. According to this distribution, about 8% of the ideal points are less than -2 and 26% of the ideal points are less than -1. This means that a status quo of 0 is in the center of the chamber, a status quo of -1 or 1 is at the median of one of the parties, and a status quo of -2 and 2 is at the extreme of one of the parties.

The results are given in Figure 3. A few things are immediately apparent—for sample sizes 40 and greater, there is little bias in the estimates. The only significant bias that is observed is for $N = 10$ and for extreme status quos. In these cases, the estimated locations are biased towards the center. When N is at least 40, the root mean squared error is of a reasonable size and the size of the error is correctly diagnosed by the standard errors (as indicated by the coverage of a 95% confidence interval reported in the right-most panel of Figure 3). The bias observed when $N = 10$ comes from the fact that when the sample size is this low, a status quo often cannot be estimated due to perfect separation. Moreover, whether there was perfect separation depended on whether the error term happened to push members to vote in the moderate direction.

For the estimation of policy locations, we find that the estimation is accurate up until the point that observations are dropped due to perfect separation. This in turns occurs when the sample size is very small, and when the status quo is extreme. In this case, the bias is in a predictable direction, with locations appearing as somewhat more moderate than they actually are. Such extreme policies should be rare in theory—of the most common theories of lawmaking, none predict that the legislature should pass legislation that is more extreme than the majority party median.

A.2 Further Details on Bridging

In this appendix, we provide further details on the bridging techniques we use. When we collected the state-legislative and congressional NPATs, we generally collected surveys for the 1998 and 2000 elections. Six states did not fit this pattern—LA, MS, NJ, and VA hold elections in odd years and AL, LA, MD, MS only hold state legislative elections only every four years. In all states, we collected either the 1998 or 1999 NPAT (used to measure the status quo) and the NPAT for the subsequent election in 2000, 2001, 2002, or 2003 (used to measure the policy outcome). The NPATs used in our study is detailed in Table 1.

We note that this corresponds with Wright's roll call data—for states with odd-year elections, Wright collected roll call data for the 1999-2001 legislative term. To employ Wrights data, we assumed that the ideal points from the 1998 (1999) NPATs correspond with the roll call ideal points for the 1998-2000 (1999-2001) legislative terms. We did not constrain individuals to have the same ideal point in the 2000, 2001, 2002, and 2003 NPATs as they did for the 1998-2000 or 1999-2001 legislative terms (though we found little evidence of dramatic change).

To merge the NPAT with the NAES, we made use of 15 near identical items between these surveys. [2] For example, NPAT respondents were asked to agree or disagree with the statement "Federal government should adopt flat tax" and NPAT respondents were asked to agree or disagree with the statement "Do you support replacing the current U.S. income

[2]See Figure 3 in the body of the paper.

State	1998	1999	2000	2001	2002	2003
AK	X		X			
AL	X				X	
AR	X		X			
AZ	X		X			
CA	X		X			
CO	X		X			
CT	X		X			
DE	X		X			
FL	X		X			
GA	X		X			
HI	X		X			
IA	X		X			
ID	X		X			
IL	X		X			
IN	X		X			
KS	X		X			
KY	X		X			
LA		X				X
MA	X		X			
MD	X				X	
ME	X		X			
MI	X		X			
MN	X		X			
MO	X		X			
MS		X				X
MT	X		X			
NC	X		X			
ND	X		X			
NE	X		X			
NH	X		X			
NJ		X		X		
NM	X		X			
NV	X		X			
NY	X		X			
OH	X		X			
OK	X		X			
OR	X		X			
PA	X		X			
RI	X		X			
SC	X		X			
SD	X		X			
TN	X		X			
TX	X		X			
UT	X		X			
VA		X		X		
VT	X		X			
WA	X		X			
WI	X		X			
WV	X		X			
WY	X		X			

Table 1: Years of State Legislative NPAT Survey Used

tax structure with a flat income tax?". The bridge items we used covered a wide variety of issues including taxes, social security, education spending, school vouchers, health-care, guns, missile defense, campaign finance, welfare, and regulation.

We note that we use the 2000 NAES. The time period for the NPAT surveys we use varies—to estimate the status quos, we use NPATs from either 1998 or 1999. To estimate policy outcomes, we use the NPATs from 2000, 2001, 2002, or 2003. The appropriate year with which to collect state voter ideology depends on the analysis we are performing. If we are studying policy representation, the relevant year would be the year of the later NPAT survey which is used to measure the policy outcome. In all but 6 cases, this is 2000. If we are seeing how well state legislative elections lead to good representation, the relevant year would be the year of the earlier NPAT survey. In all but 4 cases, this is 1998. Rather than complicate the analysis by using different years of data for different analysis, we used the 2000 measures of state opinion throughout. We would argue that this is a reasonable choice because state opinion varies very little over time. For example, Erikson, Wright and McIver (1993) report yearly estimates of state ideology using aggregate survey data. We report the correlations between various years and 2000 in Table 2. The correlations reported correct for attenuation due to measurement error. We find very strong correlations with the 2000 estimates. Similarly, we find that the mean and standard deviation of opinion did not change much over this time period.[3] When we compared the right-wing presidential vote share, we found that the 2000 and 2004 estimates correlated at 97%, again suggesting that state opinion changes slowly over time. As an additional check, we replicated the results of this paper by mapping the yearly Erikson, Wright and McIver estimates onto our scale using the 2000 data and found that this did not effect any results significantly.

[3]The measure of state opinion is the percentage of conservatives minus the percentage of liberals in the states.

Year	Corr. w/ 2000 State Opinion	Mean of State Opinion	Std. Dev. in State Opinion
1998	88%	0.13	0.10
1999	99%	0.13	0.09
2000		0.12	0.10
2001	90%	0.14	0.11
2002	86%	0.11	0.12
2003	94%	0.15	0.10

Table 2: Persistence of State Opinion

A.3 Additional Figures and Tables

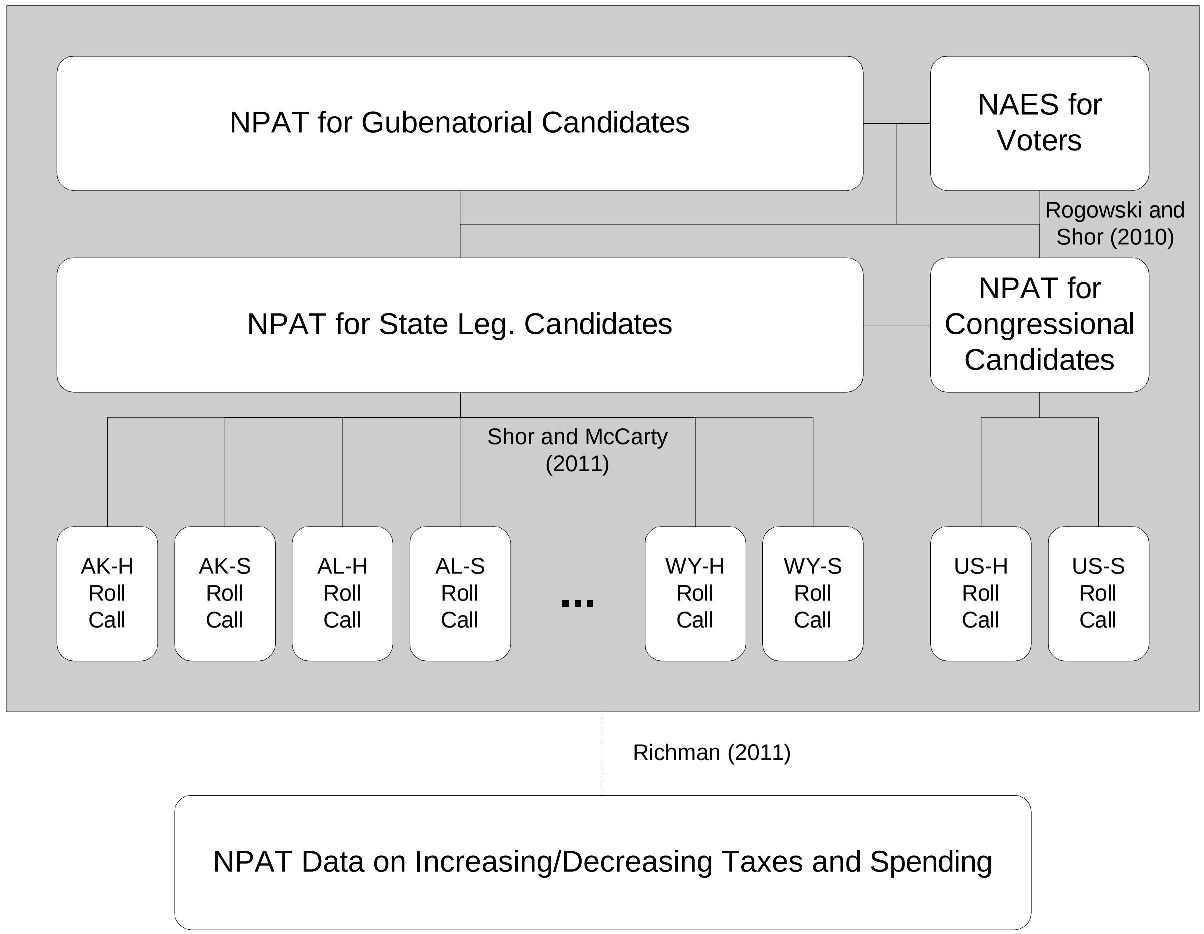

Figure 4: Summary of Data Used to Generate Common Space Estimates – Lines denote bridging observations between surveys and techniques used for creating bridging observations.

References

Erikson, Robert S., Gerald C. Wright and John P. McIver. 1993. *Statehouse Democracy: Public Opinion and the American States.* Cambridge: Cambridge University Press.

Richman, Jesse. 2011. "Parties, Pivots, and Policy: The Status Quo Test." *American Political Science Review* 105:151–165.

Shor, Boris, Nolan McCarty and Christopher Berry. 2008. "Methodological Issues in Bridging Ideal Points in Disparate Institutions in a Data Sparse Environment." Working Paper.

	House			Senate		
	Num. Bridges	ρ	Per. Corr.	Num. Bridges	ρ	Per. Corr.
AK	19	0.863	0.901	5	0.958	0.880
AL	28	0.842	0.837	8	0.719	0.851
AR	31	0.894	0.831	4	0.591	0.816
AZ	27	0.919	0.834	12	0.284	0.893
CA	26	0.943	0.933	6	0.981	0.941
CO	28	0.948	0.878	5	0.932	0.888
CT	44	0.658	0.869	8	0.834	0.885
DE	9	0.713	0.854	7	0.720	0.784
FL	29	0.845	0.916	3	0.979	0.885
GA	46	0.789	0.852	15	0.692	0.903
HI	23	0.691	0.915	6	0.894	0.909
IA	43	0.708	0.915	11	0.953	0.892
ID	20	0.924	0.825	13	0.183	0.839
IL	20	0.247	0.840	5	0.748	0.866
IN	30	0.864	0.903	9	0.939	0.869
KS	21	0.560	0.885	9	0.668	0.886
KY	29	0.713	0.836	3	1.000	0.887
LA	8	0.674	0.808	3	0.972	0.824
MA	44	0.371	0.916	4	0.858	0.924
MD	21	0.809	0.874	11	0.958	0.876
ME	58	0.848	0.848	16	0.867	0.854
MI	50	0.851	0.925	21	0.784	0.964
MN	45	0.896	0.886	16	0.859	0.857
MO	43	0.907	0.881	5	0.898	0.838
MS	20	0.777	0.841	10	0.957	0.839
MT	39	0.922	0.869	10	0.968	0.846
NC	24	0.642	0.859	9	0.846	0.916
ND	35	0.669	0.853	15	0.707	0.851
NE				11	0.614	0.780
NH	79	0.820	0.800	7	0.783	0.834
NJ	12	0.928	0.935	13	0.645	0.875
NM	20	0.885	0.890	10	0.590	0.874
NV	12	0.913	0.884	4	0.878	0.859
NY	47	0.738	0.906	13	0.918	0.932
OH	30	0.894	0.849	6	0.972	0.939
OK	28	0.822	0.875	4	0.338	0.860
OR	42	0.809	0.882	11	0.768	0.868
PA	37	0.843	0.867	7	0.556	0.908
RI	17	-0.574	0.854	8	0.507	0.850
SC	27	0.683	0.846	7	0.834	0.854
SD	31	0.750	0.816	21	0.557	0.840
TN	27	0.775	0.818	7	0.820	0.819
TX	32	0.874	0.866	4	0.968	0.862
US	188	0.912	0.910	13	0.908	0.917
UT	20	0.901	0.841	7	0.946	0.848
VA	26	0.826	0.876	7	0.881	0.850
VT	29	0.871	0.855	4	0.606	0.866
WA	21	0.864	0.895	7	0.957	0.894
WI	24	0.852	0.912	4	0.838	0.936
WV	22	0.537	0.874	8	0.064	0.826
WY	25	0.877	0.818	6	0.728	0.800

Table 3: Bridging the State Legislatures – *Num. Bridges* refers to the number of legislators in the chamber that responded to the NPAT. ρ is the correlation between the NPAT ideal points and roll call ideal points for the chamber. *Per. Corr.* is the percent of roll call votes correctly predicted by a one-dimensional model in the chamber.

	House	Senate	Governor
AK	49	23	1
AL	116	39	1
AR	124	17	3
AZ	96	52	4
CA	230	62	4
CO	160	33	
CT	157	40	3
DE	44	18	2
FL	189	21	3
GA	147	44	2
HI	78	26	1
IA	86	36	2
ID	89	43	3
IL	87	12	2
IN	132	27	7
KS	109	73	3
KY	100	17	
LA	65	26	9
MA	126	26	3
MD	110	48	1
ME	250	82	4
MI	227	53	2
MN	223	108	4
MO	190	30	2
MS	67	32	2
MT	154	38	1
NC	137	43	4
ND	120	46	
NE		49	1
NH	314	33	4
NJ	141	74	
NM	128	78	2
NV	64	23	1
NY	201	82	4
OH	181	29	4
OK	97	27	2
OR	124	35	8
PA	244	37	5
RI	72	34	2
SC	137	47	3
SD	123	64	4
TN	117	22	5
TX	117	16	4
US	1549	205	
UT	105	36	7
VA	100	17	
VT	190	54	4
WA	129	36	4
WI	92	17	4
WV	192	41	9
WY	87	19	3

Table 4: NPAT Respondents — The number of respondents from House candidates, Senate candidates, and gubernatorial candidates in each state. Responses come from the 1998 and 2000 NPATs in most states, with exceptions listed in Table 1