

## Supporting Information Appendix

This appendix provides additional discussion of the data and results shown in the paper and also provides supplemental empirical analysis.

### *Coding of Pension Expansion and Reduction Bills*

As we describe in the paper, we used the NCSL database to identify all legislation from 1999 to 2011 that either expanded or reduced pensions for state and local government employees. We located over 600 bills in the NCSL database and then independently coded each one according to the changes the legislation made. We classified the following kinds of measures as changes to public pension benefits:

- Basic formula changes (e.g., changes to the multiplier or the retirement age)
- Changes in the tax status of pension benefits
- Changes in the length of employment that determines the pension payout
- Changes in the vesting period
- Changes in the compensation that is used to calculate pension payouts (e.g., sick leave)
- Changes in cost-of-living adjustments or COLAs (although, in general, the NCSL database excludes COLA legislation)
- Changes in the pension benefits available to surviving spouses or domestic partners
- Changes in employees' options for receiving benefits as a lump sum payment
- Changes in employees' options for purchasing service credit
- Changes in rules for receiving benefit payments early

For bills that met these criteria, we coded the change as an “expansion” if the bill made pensions more generous to government employees (such as lowering the retirement age), and we coded it as a “reduction” if it made pensions less generous to government employees (such as removing the option to purchase service credit for years not worked). We also coded employee contribution increases as pension reductions, including bills that eliminated or reduced employer “pickup” of employees' contributions.

There were several bills in the NCSL database that did not make pensions more or less generous to government employees. For example, some bills changed employers' contributions,

authorized pension obligation bonds, changed how funds were invested, or changed pension governance structures—but did not change the pension benefits for government employees. We excluded these bills from our analysis. We also excluded bills that only affected the pensions of state elected officials or judges, simply because those bills affect much smaller numbers of government employees than, for example, bills about pensions for teachers or police officers. As we mention in the paper, we also dropped 17 bills that included some form of both expansions and retrenchments. In the end, we had a dataset of 366 pension bills.

### *State Polarization*

As we explain in a footnote in the paper, one might be concerned that the post-recession spike in partisanship on pension bills is not specific to public pensions—that instead, votes on pensions reflect a sudden, across-the-board increase in partisanship in the state legislatures. To investigate this possibility, we downloaded Shor and McCarty’s (2011, 2013) estimates of state legislator ideology, which are constructed using data from state legislative roll call votes and the National Political Awareness Test. For the majority of state legislative chambers, the Shor and McCarty estimates are only available through 2008 and so do not allow us to compare pre- and post-recession trends. However, for 34 chambers, their dataset does contain estimates for years after 2008. For each of those 34 chambers, we calculated the distance between the ideology of the median Republican legislator and the median Democratic legislator in each year. Figure A1 plots the results; a vertical line in each graph denotes 2008.

Examining Figure A1, we see no evidence that polarization in the state legislatures was low until 2008 and then suddenly spiked in 2009. Some chambers, such as the Texas House, show a gradual increase in polarization over time. The lines for other chambers (such as the South Dakota House) are relatively flat, while still other chambers (such as the Ohio Senate)

show declines in polarization. Most importantly, no chamber has a trend similar to the one shown in Figure 2 of the paper: a sudden spike in 2009. This evidence is bolstered by Table A1, where we regress the difference in party medians for the 34 chambers on indicators for each year. Unlike in Table 1 of the paper, we do not find that overall polarization in 2009-2011 diverged dramatically from the earlier period. In an F-test of whether the coefficient on the year 2009 is equal to that of 2008, we fail to reject the null hypothesis. The same is true of a test of whether the coefficient on 2010 is equal to 2008. This suggests that the patterns we have found for public pensions are specific to pensions and not representative of a general trend in state legislatures.

#### *Distribution of Bills across States*

In Figure A2 below, we show how the 366 pension bills we analyze in the paper are distributed across states. We were able to collect individual state legislators' votes for 268 of the 366 bills; the black bars in Figure A2 show the number of bills in each state for which we could not obtain votes. As the figure shows, the only states for which we are missing all of the votes are Georgia (16 bills), Kansas (5 bills), Kentucky (6 bills), Massachusetts (7 bills), New Hampshire (7 bills), and West Virginia (6 bills). In all but three of the remaining 43 states (Maine, New York, and Oklahoma), our dataset includes votes on a majority of the pension bills. We have no reason to think that patterns of Democratic and Republican voting in the states with missing votes would be different than elsewhere in the country.

#### *Additional Discussion of Results in Table 2*

Next, we move to our analysis of individual legislators' votes, the results of which are presented in Tables 2 and 3 of the paper. Our discussion of that analysis focuses on Table 3—

the predicted probabilities generated from the logit models. Here, we discuss the hypothesis tests most relevant to our theory, which are presented in Table 2 of the paper.

In the simple model of pension expansion bills presented in column 1 of Table 2, the negative and significant coefficient on *Republican* tells us that prior to the recession, Republicans were less likely than Democrats to vote “yes” on pension expansion. However, that pre-recession inter-party difference was small in comparison to the gap between the two parties after the recession. As we can see from the positive coefficient on *Scope*, Democrats became *more* likely to vote “yes” on pension increases after the recession. Republicans, in contrast, became even less likely to vote “yes” than they had been before the recession, as shown by the significant negative effect of  $Scope + Scope*Republican$  at the bottom of column 1.

In column 2 of Table 2, we estimate the same model as in column 1 but allow our estimates to vary by whether the bills were enacted by Democratic unified governments or not. We first consider the voting patterns in states without Democratic unified governments. While Republicans were slightly less likely than Democrats to vote for retrenchment before the recession, the interparty difference was small relative to what it became with the onset of recession. The coefficient on *Scope* is negative and significant, which shows that Democrats became less likely to approve pension reductions after the recession. In contrast, as we show at the bottom of column 2, the sum of the coefficients on *Scope* and  $Scope*Republican$  is statistically indistinguishable from zero, which means that Republican support for retrenchment did *not* decline after the recession. Thus, as expected, voting on retrenchments became more partisan post-recession, and in non-Democratic unified governments, it became more partisan because Democrats became less likely to vote “yes” than before.

For retrenchment bills in Democratic unified governments, the patterns in voting are different. Before the recession, Republicans and Democrats were equally likely to vote “yes” on retrenchment: we cannot reject the null hypothesis that  $Republican + Republican*Democratic Unified Government$  equals zero. To test whether Republican voting on reform bills changed from before to after the recession, we test whether the combination of four coefficients— $Scope$ ,  $Scope*Republican$ ,  $Scope*Democratic Unified Government$ , and  $Scope*Republican*Democratic Unified Government$ —is equal to zero. At the bottom of column 2, we find that this estimate is negative and statistically significant, indicating that after the recession, Republicans became more likely to vote “no” on reform bills pushed through by Democrats. Democratic voting, however, did not change much: the combination of  $Scope$  and its interaction with  $Democratic Unified Government$  is negative and significant, but as Table 3 shows, the magnitude of that change was small (a shift from 95% voting “yes” to 92% voting “yes”).

#### *Results Using Ordinary Least Squares, Fixed Effects*

In a footnote of the paper, we explain that the findings we present in Tables 2 and 3 are substantively the same when we model legislators’ votes using ordinary least squares (OLS) rather than logistic regression. Table A2 presents the OLS estimates.

In column 1, we find that on expansion bills, the votes of Republicans and Democrats differed little before the recession: Republicans were only 5.6 percentage points less likely to vote “yes.” After the recession, Republican support for pension expansions plummeted by 24 percentage points (see the test of  $Scope + Scope*Rep. = 0$  at the bottom of column 1), while Democratic support for expansions actually increased slightly.

Our findings for reductions in column 2 also mirror those of Table 2. In non-Democratic unified governments, the parties voted together before the recession: Republicans were only 3.2

percentage points less likely to vote “yes.” After the recession, Democratic support dropped by 27 percentage points, while Republican support largely stayed the same. In Democratic unified governments, it was the Republicans whose support dropped after the recession (by 43 percentage points), while Democrats continued to vote “yes” at high rates (their support only decreased by 5 percentage points from before to after the recession). Thus, our conclusions from the OLS models are the same as those from the logit models.

In columns 3 and 4, we add state fixed effects, and our core findings are the same as those we present in the paper. The only substantive differences are the estimated effects of state debt per capita: we estimate a significant negative coefficient in the pension expansions model and an insignificant coefficient in the pension reductions model.

As we discuss in that same footnote, one might wonder whether the sudden post-recession divergence in Democratic and Republican votes is caused by changes in the composition of the legislatures or changes in the votes of legislators who were in office both before and after the recession. While our theoretical perspective does not imply that one type of change should be more important than the other,<sup>1</sup> it is interesting to ask whether individual legislators did, in fact, change their votes after the recession. We investigate this by replacing the state fixed effects with legislator fixed effects.

The legislator fixed effects partial out the effects of any unobserved characteristics of individual legislators that make them more or less likely to vote “yes” on a given type of bill, as long as those characteristics do not vary over time. However, including dummy variables for the

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<sup>1</sup> On pension expansion bills, for example, a change in voter awareness and interest group structure could have led to the election of more anti-pension Republicans, *or* it could have caused sitting Republicans to *become* more anti-pension. It is possible that both effects are at work.

thousands of individual legislators in our dataset places heavy demands on the estimation. It also has two important implications for how we interpret the estimated coefficients. First, the coefficients on the party variables (those not interacted with *Scope*) are based solely on legislators who switched parties. With so few cases of party switching, these coefficients do not merit discussion. As a result, the model provides no real insight into the question of whether the normal politics of pensions is partisan or bipartisan. Moreover, the estimates of the changes in Republicans' and Democrats' votes over time (i.e., *Scope* and the party variables interacted with *Scope*) are based only on the votes of legislators who were in office before and after the recession *and* voted on pension expansions (or reductions) in both time periods. Thus, the estimates from the legislator fixed effects models give only a limited picture of changes in partisan voting patterns over time.

We present the results of the legislator fixed effects models in columns 5 and 6. The coefficients and hypothesis tests show that many legislators *did* change their votes after the recession—and that the uptick in partisanship was not driven solely by changes in the composition of the groups of legislators voting on pension issues. In column 5, we find that Democrats became more likely (9 percentage points) to vote “yes” on expansions after the recession. Republicans, however, became 7 percentage points *less* likely to vote “yes” on pension expansions after the recession. In column 6, we find that in non-Democratic unified governments, neither Democrats nor Republicans became less likely to vote “yes” after the recession. In Democratic unified governments, Democratic voting on reductions did not change after the recession, but Republican voting did—by 24 percentage points. Thus, even in these models with legislator fixed effects, we find the same general patterns that we do in the paper.

*The Effect of Interest Group Pressure: Full Results*

In the final empirical section of the paper, we carry out tests of the proposed mechanism—the change in political pressure—that our theory suggests is at work. In the paper, our discussion focuses on the votes of Republicans, particularly on pension expansions and pension reductions in Democratic unified governments. In Table A3 below, we present the regression results for retrenchment bills in non-Democratic unified governments (which are not shown in the paper). In Table A4, we also present the complete set of predicted probabilities generated from Table 4 of the paper and Table A3 in this appendix. Our discussion here focuses on the probabilities that are not discussed in the paper: the probabilities of Republican “yes” votes on retrenchment bills in non-Democratic unified governments, and the probabilities for Democrats on all types of bills.

We focus first on Republicans’ votes on retrenchment bills in non-Democratic unified governments, presented in the top half of Table A4 in the middle column. During the pre-recession period, we expect highly cooperative voting, so we should not expect any of the interest group variables to have much impact on Republicans’ rates of voting “yes.” We find that this is the case: Republicans voted “yes” at high rates before the recession regardless of whether they had moderate or conservative donors, were with or without public sector union support, or had high or low levels of government employment in their districts.<sup>2</sup> Little changed after the recession. When Republicans are in control of at least one branch, and can thus veto any proposals that get made, virtually all of them can be expected to go along with whatever legislation actually gets passed. We find evidence in support of this in Table A4: after the

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<sup>2</sup> In Table A3, the pre-recession effect of union campaign contributions was positive and significant for Republicans, but as the predicted probabilities in Table A4 show, that effect is substantively small (only 4 percentage points).

recession, Republican support for retrenchment in non-Democratic unified governments did not vary substantially with our three interest group variables.<sup>3</sup>

Turning to the lower half of Table A4 (panels 4 through 6), we find that Democrats almost universally supported pension increases throughout this time period, regardless of whether they had moderate or liberal donor bases, contributions from public sector unions, or high or low levels of government employment in their districts. To the extent we see any sizeable effects of the interest group variables on Democrats' votes, it is for votes on retrenchment bills. On pension reductions in non-Democratic unified governments, we again see cooperative voting prior to the recession. After the recession, Democrats with liberal donor bases and Democrats with financial backing from public sector unions were less likely to support retrenchment than Democrats with more moderate donor bases or no union support. (We do not find the same pattern using the government employment measure, however.) By contrast, when Democrats are in full control of state government, and can push forward their own versions of pension retrenchment, we see a different pattern: nearly all Democrats get on board, regardless of their interest group constituency, both before and after the recession. The one exception here is in the third column of Table A4, which uses donor conservatism as the measure of interest group activity: there, we find that after the recession, Democrats with more moderate donor bases were less likely to support Democratic pension reduction bills than Democrats with more liberal donor bases.

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<sup>3</sup> The one interest group effect that is statistically significant in this case, shown at the bottom of Table A3, is that of donor conservatism: Republicans with conservative donors were more likely to vote "yes" on retrenchment in non-Democratic unified governments than Republicans with more moderate donors. However, in Table A4, we can see that this, too, is a substantively small effect.

**Table A1: Polarization**

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2000	0.007 (0.010)
2001	0.026 (0.029)
2002	0.048 (0.043)
2003	0.07 (0.042)
2004	0.088* (0.046)
2005	0.108* (0.053)
2006	0.113* (0.054)
2007	0.154** (0.058)
2008	0.164** (0.059)
2009	0.188*** (0.059)
2010	0.172** (0.074)
2011	-0.164 (0.208)
Constant	1.178*** (0.098)
R-squared	0.02
N	408

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*Notes:* Standard errors clustered by state in parentheses. Dependent variable is ideological distance between the party medians in a state legislative chamber.

\* p<0.1; \*\* p<0.05; \*\*\* p<0.01

**Table A2: OLS Models, State Fixed Effects, and Legislator Fixed Effects**

	<i>Expansions</i>	<i>Reductions</i>	<i>Expansions</i>	<i>Reductions</i>	<i>Expansions</i>	<i>Reductions</i>
	(1)	(2)	(3)	(4)	(5)	(6)
Scope	0.007*	-0.27***	0.13***	-0.183***	0.092***	-0.034
	(0.004)	(0.013)	(0.008)	(0.019)	(0.011)	(0.032)
Republican	-0.056***	-0.032***	-0.054***	-0.034***	0.026	0.027
	(0.004)	(0.010)	(0.004)	(0.011)	(0.078)	(0.121)
Scope*Republican	-0.251***	0.254***	-0.271***	0.278***	-0.163***	0.01
	(0.014)	(0.015)	(0.013)	(0.015)	(0.028)	(0.032)
Democratic Unified		-0.009		0.001		0.067*
		(0.012)		(0.017)		(0.036)
Republican*Democratic Unified		0.02		0.03		-0.193***
		(0.020)		(0.020)		(0.057)
Scope*Democratic Unified		0.218***		0.214***		0.101*
		(0.018)		(0.025)		(0.052)
Scope*Republican*Dem. Unified		-0.633***		-0.659***		-0.32***
		(0.033)		(0.032)		(0.086)
Debt per capita	-0.0002	0.016***	-0.058***	-0.023	-0.063***	-0.045*
	(0.001)	(0.003)	(0.004)	(0.015)	(0.007)	(0.025)
Previous Expansion	-0.019***		-0.027***		-0.024***	
	(0.004)		(0.005)		(0.006)	
Previous Reduction		-0.091***		-0.071***		-0.058***
		(0.010)		(0.013)		(0.020)
Constant	0.981***	0.926***				
	(0.003)	(0.009)				
R-squared	0.08	0.13	0.17	0.22	0.54	0.72
Observations	21,245	10,688	21,245	10,688	21,245	10,688
Fixed effects	None	None	State	State	Legislator	Legislator
<b>Additional hypothesis tests</b>						
Scope + Scope*Rep.	-0.244***	-0.016*	-0.142***	0.095***	-0.071**	-0.024
	(0.014)	(0.009)	(0.015)	(0.015)	(0.029)	(0.024)
Scope + Scope*Rep. + Scope*Dem. Unif.		-0.43***		-0.349***		-0.243***
+ Scope*Rep.*Dem. Unif.		(0.027)		(0.033)		(0.071)
Scope + Scope*Dem. Unif.		-0.052***		0.031		0.067
		(0.014)		(0.026)		(0.050)

Notes: Standard errors clustered by legislator in parentheses. In columns 1, 3, and 5, Scope + Scope\*Rep. tests whether Republicans' rates of voting "yes" were the same before and after the recession. In columns 2, 4, and 6, Scope + Scope\*Rep. tests whether Republicans' votes were the same before and after the recession in non-Democratic unified governments, and Scope + Scope\*Rep. + Scope\*Dem. Unif. + Scope\*Rep.\*Dem. Unif. is the corresponding test for Democratic unified governments. Scope + Scope\*Dem. Unif. tests whether Democratic voting was the same before and after the recession in Democratic unified governments. All hypothesis tests are two-tailed. \* p<0.1; \*\* p<0.05; \*\*\* p<0.01

**Table A3: The change in interest group pressure, pension reductions in non-Democratic unified governments**

	(1)	(2)	(3)
Scope	-2.15*** (0.162)	-1.003*** (0.270)	-3.072*** (0.349)
Republican	-0.857*** (0.259)	-0.162 (0.272)	-1.841*** (0.437)
Scope*Republican	1.802*** (0.317)	1.145*** (0.312)	3.05*** (0.518)
Conservative donors	-0.182 (0.184)		
Scope*Conservative donors	0.417** (0.199)		
Republican*Conservative	0.625** (0.306)		
Scope*Rep.*Conservative	-0.247 (0.375)		
Previous reduction	-0.879*** (0.083)	-0.862*** (0.081)	-0.832*** (0.078)
Debt per capita	0.166*** (0.023)	0.16*** (0.024)	0.134*** (0.022)
Union		1.055*** (0.298)	
Scope*Union		-1.74*** (0.333)	
Republican*Union		-0.316 (0.372)	
Scope*Rep.*Union		0.929** (0.419)	
Government employment			-4.442** (1.726)
Scope*Government			4.624** (1.889)
Republican*Government			7.611*** (2.626)
Scope*Rep.*Government			-5.365* (3.242)
Constant	2.708*** (0.157)	2.023*** (0.245)	3.605*** (0.324)
Observations	7,877	7,449	8,087
Pseudo R-squared	0.1232	0.1284	0.1211
Interest group effect	-0.483 (0.413)	0.740*** (0.223)	-0.418 (0.263)
for Republicans, before			
Interest group effect	0.581*** (0.225)	-0.072 (0.143)	-0.321 (0.239)
for Republicans, after			

Notes: Standard errors clustered by legislator in parentheses. The interest group hypothesis tests at the bottom of the table show the effect of conservative donors in column 1, the effect of union contributions in column 2, and the effect of high government employment in column 3. Hypothesis tests are two-tailed. \*p<0.1, \*\*p<0.05, \*\*\*p<0.01

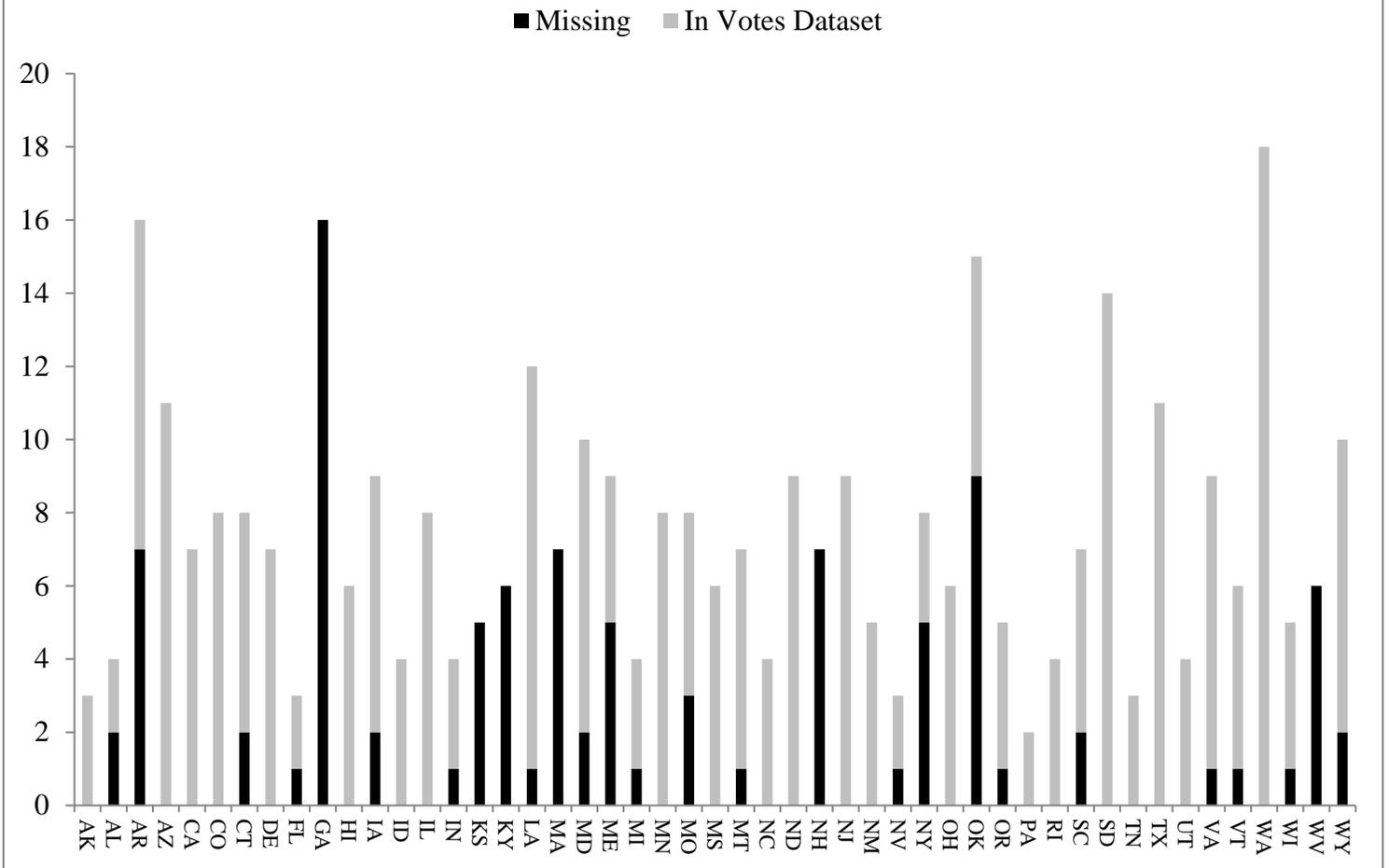
**Table A4: Predicted probabilities of voting "yes" on pension bills**

	Increases	Reductions, Non-Democratic Unified Governments	Reductions, Democratic Unified Governments
<b>Republicans</b>			
1.	Before, Moderate Donors	0.929	0.896
	Before, Conservative Donors	0.926	0.973
	After, Moderate Donors	0.739	0.586
	After, Conservative Donors	0.646	0.240
2.	Before, With Union Support	0.928	0.831
	Before, Without Union Support	0.915	0.894
	After, With Union Support	0.759	0.469
	After, Without Union Support	0.610	0.324
3.	Before, High Government Employment	0.949	0.906
	Before, Low Government Employment	0.878	0.889
	After, High Government Employment	0.828	0.441
	After, Low Government Employment	0.597	0.525
<b>Democrats</b>			
4.	Before, Liberal Donors	0.978	0.582
	Before, Moderate Donors	0.975	0.993
	After, Liberal Donors	0.987	0.945
	After, Moderate Donors	0.978	0.621
5.	Before, With Union Support	0.976	0.899
	Before, Without Union Support	0.975	0.963
	After, With Union Support	0.985	0.913
	After, Without Union Support	0.982	0.911
6.	Before, High Government Employment	0.970	0.914
	Before, Low Government Employment	0.958	0.919
	After, High Government Employment	0.987	0.925
	After, Low Government Employment	0.981	0.904

Figure A1: Polarization Over Time



Figure A2: Number of bills by state



## REFERENCES

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