

Political Polarization, Competition, and Fiscal Policy Outcomes in U.S. States

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Preliminary and Incomplete Version  
Please do not quote or cite

Prepared for the Duke University Workshop on Political Competition and Government  
Policy, May 4-5, 2007

This version: April 30, 2007

The paper presents and discusses different measures of state-level political polarization, both among politicians and voters, respectively. We argue that different kinds of polarization can influence political and economic outcomes in different ways and that these effects can differ across political settings. We show the effect of polarization on policy outcomes (public finances, in particular the political budget cycle) and on institutional change (budget transparency).

### **Theoretical framework**

We begin from a political agency model, originally due to Barro (1973) and Ferejohn (1986). These models adapt the principal-agent framework, in which the agent is better informed than the principal, to a political setting such that voters, as principals, elect politicians who, as agents, may enjoy an informational advantage over voters, and other politicians, and make policy choices that affect voters and opponents. Voters, in turn, control politicians by holding them accountable for their performance at election time. For an application to fiscal policy, see Persson, Roland and Tabellini (1997).

Political polarization can enter the political agency model at two levels, both at the elite level, between different parties and sets of activists, and at the level of citizens or voters. We explore the relationship between these different kinds of polarization and we argue that they may affect political and economic outcomes in different ways. Thus, for the purposes of this paper, political polarization is important only to the extent that it affects political behavior, either for parties and politicians or for voters.

In Alt and Lassen (2006), we consider the case of political polarization between political parties. Parties are concerned both about policy and about winning. Therefore, if parties polarize, the incumbent risks being replaced by an opponent whose policy is further away from his preferred policy, which, in this model, induces the incumbent to boost the political budget cycle with the aim of winning re-election.

At the same time, voters can be politically polarized. Think for a moment of political polarization simply as the absolute difference between platforms of the parties and, for a pure polarization effect, one that leaves the median voter's assessment of party platforms unchanged. As polarization increases it becomes more desirable for each party to stay in office, as the utility loss from seeing the opponent in office increases. Alt and Lassen (2006a) show that for both parties equilibrium debt increases as the degree of political polarization increases. That is because, as we elaborate in Alt and Lassen (2006a), if voters as having preferences both over fiscal policy and on a non-fiscal issue, then when considering vote choice, they can trade off fiscal policy and the perceived ability of the incumbent with their preference over the non-fiscal issue. In Alt and Lassen (2006b), conditioning on the degree of fiscal policy transparency, we showed that electoral cycles should be larger in more politically polarized electorates, and found empirically that, consistent with the theory, independent of other control variables, in a sample of OECD countries, various measures of political polarization did indeed positive affect the size of the electoral budget cycle in fiscal balance.

In this paper, for reasons which will be clear in a moment, we consider a variant of these models, due to Besley (2006). We define voters as polarized (or partisan) if they

vote for a particular party regardless of that party's performance on the valence issue.<sup>1</sup>

However, polarization in itself is not sufficient to affect policy. The impact of polarization depends both on the relative size of partisan voter shares (how they balance each other) and their absolute size (if both are small, it doesn't matter that one is bigger than the other). It also depends on the turnout propensity of the groups (which we probably don't know about), as well as the flow of information reaching (swing) voters (this could be a way to introduce media). We return to these issues below.

In theoretical analysis of political agency models with heterogeneous voters, it is, to get reduced form results, often necessary to make assumptions about the shape of the distribution. For example, in deriving the results in Alt and Lassen (2006a) described above, we assume that voters' preferences along the economic dimension are uniformly distributed, with the implication that, as party polarization increases, policy-motivated incumbents are willing to spend more resources winning over swing voters to avoid being replaced by an opponent. Similarly, Persson and Tabellini (2000) and Besley (2006) establish results under assumptions of uniform and unimodal distributions.

However, these distributional assumptions do not seem to fit American politics. DiMaggio, Evans and Bryson (1996) and Evans (2003) find that there is evidence of (increasing) polarization among citizens both in terms of variance (the dispersion of the distribution) and kurtosis (the bimodality of the distribution) for some groups; in particular, this is true along the ideological (liberal – conservative) dimension as well as in the politically important partisan self-identification, which are the dimensions of

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<sup>1</sup> (who introduced swing and partisan voters formally?). (something about recent work here, maybe Ashworth and BdM). Also, ref to bundling, incl. Lee and Roemer (JPubE, 2006).

polarization we examine below.<sup>2</sup> At the same time, McCarty, Poole and Rosenthal (2006) note that parties and/or politicians have polarized, increasingly organizing further away from the middle on the liberal-conservative axis.

This has consequences for models of political competition where swing voters are in play. Consider figure 1: The first panel (a) shows a stylized depiction of polarization in most OECD countries.

< insert figure 1 here >

In this case, the variance increases while the distribution remains unimodal and, as a result, swing voters remain in play. Contrast this with the second panel (b), which is a stylized depiction of polarization in an American setting: In this bimodal distribution, increasing dispersion and/or kurtosis pushes the two modes of the distribution further away from each other, resulting in less swing voters available for political persuasion. This means that an additional dollar spent on electioneering buys fewer votes than in the unimodal case, which can reverse the standard conclusion of the political agency models.

TBD further.

### *Polarization, the Design of Institutions, and Reform*

In addition to looking at the direct impact of polarization on policy outcomes (below), we also investigate whether polarization changes the institutional framework in which policy is formulated, proposed and adopted by looking in detail on reforms of fiscal transparency, extending the analysis in Alt, Lassen and Rose (2006) by looking at the act of reform rather than levels of transparency and by including improved measures of political polarization. In Alt, Lassen and Rose (2006), we survey the (small) literature on

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<sup>2</sup> As we note elsewhere, divisions in these two dimensions are increasingly collapsing into one.

endogenous budgetary institutions, with an explicit focus on transparency. In that paper, we argue that the degree of political party polarization can affect the trade-offs politicians face in choosing the level of transparency.

This can work in two directions. First, following from Hanssen (2004), increased political polarization can raise the cost of being replaced by the other party, if it means that opponents in low transparency environments will choose policies that are further from the incumbent's own ideal point than what would be chosen under high transparency, the logic being that in less transparent environments it is easier to divert funds to goals not shared by the parties. This suggests that polarization increases the benefits of tying the hands of a partisan adversary and, hence, we should see more transparency where polarization is higher. Second, and alternatively, it could be the case that some sort of agreement is needed in order for reform to take place. This possibility is reflected in the (informally formulated) hypothesis of North (1990, p.191) that "political institutions constitute *ex ante* agreements over cooperation among politicians." If this is true, a more polarized polity could be a hindrance, rather than a catalyst, to transparency. We will examine this issue empirically.

There exists no theory that we know of that relates voter polarization (through partisanship) to the choice of institutions. TDB.

## **Measurement**

### *Elite/Party Polarization*

To measure political polarization within a state, we follow Hanssen (2004) in using measures of ideology based on roll-call voting in the U.S. Congress, as no comparable

data on political polarization at the state level exists. The data are taken from Poole and Rosenthal (1997). The authors estimate the positions of members of Congress along two dimensions. Like Hanssen (2004), we use the first of these, the liberal-conservative axis, to calculate average ideology scores for each state's members of Congress. For each year and state, we measure policy distance by the absolute difference between average ideology scores for each party. In some cases, a state is represented by only one party. This can be the case for two reasons: Small states have fewer representatives, which reduced the chance of both parties being represented. Second, it can indicate extreme polarization. For states where a score is missing, we plan to construct it from past or neighboring states scores (following Berry et al. (1998) NOTE: Not yet implemented).

### *Voter polarization*

There are (at least) two ways of operationalizing voter polarization. One is to look at attitude polarization, as done by DiMaggio, Evans and Bryson (1996), updated in Evans (2003). DiMaggio et al. do not find much attitude polarization using various measures and attitudinal dimensions, but Evans documents that attitude polarization has been strongly increasing in new(er) data, in particular between partisan self-identifiers and along the liberal-conservative axis.

Alternatively, we can look directly at the shares of partisan voters. (There has to be some earlier, probably much earlier, work on this in American politics). In the context of a political agency model, Besley (2006, pp. 124-8) introduces the following notation: The share of partisan voters,  $\omega$ ; the difference in partisan voter shares,  $\eta$ ; and the utility partisan voters get from seeing their party in office,  $\phi$ .

If  $\phi$  is zero, there are no partisan voters. If  $\phi > 0$ , but small, party identifiers will still compare the performance of their party's candidate with that of the opponent party. However, if  $\phi$  is sufficiently big, partisan voters vote *only* on the basis of the party label, not on behavior or performance in office. Thus, in electorates with a greater number of partisan voters, fewer voters observe and act on performance in office. However, if the partisan groups are equal in size they effectively cancel each other, leaving competition for swing voters.

While we do not know the extent of potential swing voters, we can use existing data to get some idea of trends and developments in self-identified partisans. To construct measures of differences in partisan identifiers, we use data from Erikson, Wright and McIver (2005).<sup>3</sup> They present CBS/NYT polling data from 1976-2003, with yearly observations on all states, where people are asked to self-identify along both the partisan spectrum (Democrat, Independent, Republican) and the ideological spectrum (Liberal, Moderate, Conservative). We construct two measures of partisan bias. The broad measure defines as partisans voters that identify as Democrats or Republicans. The narrow measure defines partisan voters to be those who identify as either liberal Democrats or Conservative Republicans. Thus, we define  $\eta_b = \text{abs}(s_iD - s_iR)$ , where  $s_iX$  is self-identified Democrat and Republican, respectively. Below, we so far primarily use this measure, denoted APVD in the tables. Correspondingly,  $\eta_n = \text{abs}(s_iLD - s_iCR)$ . The share of partisan voters are  $\omega_b$  and  $\omega_n$ , respectively. Finally, we can combine these into a measure of polarization bias, defined as

$$\theta_b = \eta_b * \omega_b / (1 - \omega_b)$$

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<sup>3</sup> We are grateful to them for making the data available to us.

and similarly for the narrow measure. Theta is small if either the groups of self-identified partisans are of approximately equal size or if their share of the electorate is small.

The broad measure is probably too broad, as it includes both conservative Democrats and liberal Republicans, two groups that by most accounts belong among swing voters. On the other hand, the narrow measure may be too narrow. We have not (yet) experimented with augmenting the narrow measure with self-identified liberal or conservative independents.

EWM (2005) find an increase over time in the likelihood that a person identifying as a Republican also identifies as a Conservative and the self-identified Democrats increasingly identify as Liberals (this was also noted by Evans, above, though on a different data set). Further, the EWM data shows some, but not overwhelming, evidence that over time, SILs are more likely to identify as Democrats, while SICs are more likely to self-identify as Republicans. This is probably, though not exclusively, a product of the Southern Realignment. (This re-alignment of ideology and partisanship may also be related to Roemer's work on racism and redistribution.)

An alternative measure (ref:?) defines polarization as the sum of self-identified liberals and conservatives (or, conversely, as one minus the share of moderate voters).

### **What determines polarization?**

TBD.

### **Preliminary Results**

We examine the effects of polarization on public finance outcomes using a first difference GMM model. There is substantial inertia in public finances and as is well known, including lagged dependent variables in a fixed effects regression results in bias. However, first differencing corrects for this.

### *Public Finances and Political Budget Cycles*

*Variables.* We model state government surplus, expenditures, revenues and debt, all measured in real per capita terms, as a function of a set of standard controls, individual year effects (state fixed effects are differenced out) and polarization measures. The standard controls (see e.g. Alt and Rose, 2006) include population, dependency ratios, unemployment, federal grants received, income per capita, and Democratic control of the state legislature (Alt and Lowry, 2000). We include the NOMINATE polarization measure, including the correction indicator, as well as various measures of voter polarization and partisan imbalance. In addition, we include electoral year and pre-electoral year indicators, in order to examine the data for evidence of political budget cycles, and interact the election variables with polarization measures to examine interdependent effects on electoral cycles (see Alt and Lassen, 2006; Rose, 2006).

Alt and Rose (2006) did not examine polarization in their systematic review of institutional and strategic contextual factors conditioning the extent of political budget cycles, so we extend their study in that way. We also rely on that study to suggest further “institutional, structural, and strategic contexts in which elected, partisan incumbents make policy” (Franzese 2002: 370), which determine the circumstances under which

incumbents have greater incentives and ability to create political budget cycles. These include examining

(1) whether political budget cycles were less pronounced when U.S. state governors faced binding term limits;

(2) the competitiveness of elections, which could be institutional (differences in electoral rules or the relative strength of political parties make re-election more difficult in some places than others) or “strategic” (an election that is expected to be “closer” increases the value and thus desirability actions increasing the incumbent’s re-election prospects);

(3) shared policy control (like the presence of multiple “veto players”) hence the effect of divided government on the magnitude of the political business cycle;

(4) the extent of any information asymmetry between politicians and voters, like whether political budget cycles are smaller where fiscal institutions are more transparent or where there is a competitive media with substantial market penetration;

(5) finally, the effect of the existence of fiscal rules that limit or prohibit deficits.

Alt and Rose found that, on the one hand, the expected closeness of the upcoming election, measured by moderate gubernatorial job approval, was associated with larger pre-election surges in spending. Low newspaper circulation was also associated with substantially larger budget cycles. Other results (divided government, transparency) did not come out clearly, as they had in some cross-national studies. The clearest effect, however, was that of balanced budget laws: states that restrict politicians’ ability to issue debt to cover spending shortfalls simply do not exhibit political budget cycles. So we

start by looking at whether the effects of polarization are affected by this factor, and then work back through the others.

*Specification.* A standard estimation strategy, used by Alt and Rose (which we follow with one minor exception) is the general method of moments estimator developed by Arellano and Bond (1991). This uses first differences instead of fixed effects and includes a lagged dependent variable. The use of first differences means that we analyze changes in spending, surplus, and other fiscal conditions, but also that each individual state-level fixed effect drops out.<sup>4</sup> In more technical terms, we use the following regression specification:

$$y_{it} - y_{i,t-1} = \beta C_{it}(E_{it} - E_{i,t-1}) + \lambda(X_{it} - X_{i,t-1}) + \gamma(y_{i,t-1} - y_{i,t-2}) + (\delta_t - \delta_{t-1}) + (\mu_{it} - \mu_{i,t-1}).$$

where  $y_{it}$  is the fiscal quantity (like per capita spending) in state  $i$  and year  $t$ ,  $E_{it}$  is a vector of dummy variables representing the years before and containing the gubernatorial election,  $C_{it}$  is a vector of contextual dummy variables (approval, term limits, divided government, transparency, media, or fiscal rules),<sup>5</sup>  $X_{it}$  is the vector of control variables representing state political and economic conditions,  $\delta_t$  are year effects, and  $\mu_{it}$  is a disturbance term. Since the equation is in first differences, the dependent variable

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<sup>4</sup> This method has a problem of correlation between lagged values of the dependent variable and the lagged disturbance. To remove this correlation between  $y_{i,t-1}$  and  $\mu_{i,t-1}$ , the lagged levels of the dependent variable from period  $t-2$  and earlier ( $y_{i,t-2}, y_{i,t-3}, \dots$ ) are used as instrumental variables for the difference ( $y_{i,t-1} - y_{i,t-2}$ ). Arellano and Bond show that these lagged levels are suitable instruments if the disturbance term  $\mu_{it}$  has mean zero and is not serially correlated, in which case the first difference of the disturbance term ( $\mu_{it} - \mu_{i,t-1}$ ) has zero covariance with the levels of the dependent variables from period  $t-2$  and earlier. Diagnostic tests can tell us how serious a problem this is.

<sup>5</sup> We do not include the full set of years in the election cycle, unlike Alt and Rose, and so we cannot use their device for displaying the magnitude of the cycle. Our measures of fiscal rules do not vary over time, and so the contextual variable is  $C_i$  in these cases rather than  $C_{it}$ .

measures the annual change in, e.g., spending, and all time-varying variables on the right-hand-side are also in annual changes. Variables that are time-invariant (like state-level fixed effects) disappear (since they do not change) except where they are interacted with variables that do vary over time. The (differenced) year effects control for confounding due to accidental concentrations of elections in particular years.

*Results.* We find consistently that party polarization improves fiscal balance by decreasing government expenditures and increasing the surplus. These effects take place predominantly through reducing the sensitivity of public finances to electoral effects. In the spending and surplus regressions (Table 1, columns 1 and 2), both election and pre-election year indicators suggest strong political budget cycles by increasing expenditures and decreasing surpluses. Effects on revenues are smaller and insignificant. However, party polarization significantly reduces the amplitude of these cycles: every coefficient in the expenditures (surplus) regression in Table 1 that relates to party polarization is negative (positive) and the independent effects of party polarization on outcomes are generally significant. In a typical regression, increasing party polarization by one standard deviation reduces the election year effect on the surplus by a third – a substantial effect. We stress that this is the opposite of what we found in OECD countries, and so it constitutes an interesting further conditionality to investigate: why polarization is positive for the political budget cycle in some contexts and negative in others. At the same time, we find no consistent effect, direct or indirect, of party polarization on public sector revenues (or debt, not shown).

[Table 1 about here]

[Discussion of citizen results to follow.] Effects of partisan imbalance (APVD) are similar to those of party polarization, but weaker and generally insignificant in spending regressions. Generally, citizen polarization tends to increase revenues (also a finding of Besley, Persson and Sturm though in the context of political competition), but not in election years, and possibly to decrease debt. We could think of a Ferejohn-style model where citizens determine how much revenues to allocate to politicians, while politicians make decisions on expenditures and intertemporal allocations of the budget. The relationship to the electoral cycle suggests an interpretation that works through confidence in the party securing re-election, however.]

Table 2 shows the results of breaking out the regressions of Table 1 according to whether a state had a binding nocarryover law or not. Without undue elaboration, it appears the political budget cycle in spending is very much inhibited by the presence of binding balanced budget laws (see column 2) so that there is not much left for polarization to do (or maybe not many degrees of freedom left to do it with). However, the party polarization results in column 1 (state does not have a binding law) resemble those of Table 1. It is harder to evaluate the effects of these laws on the cycle in fiscal balance (columns 3 and 4): the pattern of signs seems to be broadly preserved, except that polarization does appear to have some effects in those states where there are binding laws. Table 3 repeats the analysis, but now broken out by contexts of higher as opposed to low or medium budget process transparency. We cannot reject the conjecture that the electoral cycle is bigger in less transparent contexts (echoing Alt and Lassen 2006 but not found by Alt and Rose 2006), but it is again difficult what consistent effect this control has on the effect of polarization on the political budget cycle.

[Tables 2 and 3 about here]

In results not reported, we also investigate the effect of party polarization on the degree of pro-cyclicality in state government finances. While most economic models recommend the fiscal policy follows a counter-cyclical pattern,<sup>6</sup> fiscal policy is often pro-cyclical in practice, increasing expenditures in booms and decreasing them in recessions. American state governments are no exception: Direct state government expenditures are highly pro-cyclical, in particular in booms, as a positive output gap increases expenditures significantly. Consistent with the results on expenditures and government fiscal balance reported above, however, party polarization significantly reduces the degree of pro-cyclicality in expenditures in good times by reducing the effect of a positive output gap approximately by fifty percent. [Interact with election effects: is fiscal policy more pro-cyclical in election years? Probably].

#### *Polarization and institutional choice*

In Alt et al. (2006) we considered a model in levels, relating the levels of transparency to levels of the independent variables in a distributed lags model. In this paper, we take a different approach. We focus on trying to explain changes in transparency, rather than the evolution of transparency levels, which are in part historically determined in times when public sector governance was not high on the agenda.

*Variables and specification.* Our dependent variable takes on the value 1 in years when a change in the transparency index is registered, and zero at all other times. This implies that our dependent variable registers a change only between 6 and 8 percent of all cases,

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<sup>6</sup> Ref in Rev. of Ec. and Stat that this is indeed rational behind grant system from fed govt.

depending on the sample. We analyze the causes of transparency in a panel data structure, using both a linear panel data probability model and a logit panel data model; the differences are not large, but as twelve states experience no change in transparency over our period of investigation (1976-99), they are excluded from the logit model by construction. In addition, the linear model facilitates interpretation of the results.

Our independent variables attempt to capture the economic and political environment of the state. We include past fiscal policy (we experiment with levels of and growth in expenditures, revenues, debt, and fiscal balance), party polarization, voter polarization, and political variables including whether governor was a Democrat, whether the house was controlled by Democrats, the degree of state legislature political competition, and whether the governor was constrained by term limits. Finally, we also include a variable meant to capture a fact that popped up in our case studies of major fiscal transparency reforms reported in Alt, Lassen and Rose (2006): Reforms are often undertaken by newly elected governors (also, ref to comparative literature, in particular NZ experience). We include a variable that takes the value one if a governor is newly elected and faces a unified state legislature controlled by the party of the governor (we also experimented with different combinations of newly elected governors and state house configurations, but this was the only one significantly different from the rest).

*Results.* The results are reported in Table 4. First, we note (not reported) that year fixed effects are insignificant and therefore excluded, and that state fixed effects are borderline significant and kept in the model. In general, the model does not predict changes in transparency well, which is unsurprising as the changes in transparency are few and far between. Second, we observe some support for the argument that elite

political polarization increases the probability of observing an increase in transparency significantly. At the same time, citizen polarization, here measured by partisan voter bias, also seems to increase transparency, though this variable is not consistently significant. [We still need to look at partisan configurations here; is bias pro or against current governor?].

[Table 4 about here]

The control variables are also of some interest. Democratic governors tend to reduce the probability of increasing transparency, while Democratically-controlled state houses increase it. More political competition at the level of the state legislature increases transparency. Among the economic controls, recent expenditure growth increases probability of transparency reform, while there is no effect of past debt or past fiscal balance. Finally, there is some merit to the argument about new brooms. Newly elected governors enjoying their first term in office and cooperating with a state legislature controlled by the same party are significantly more likely to initiate reform by increasing transparency, and the effect is identical for the two parties. At the same time, lame ducks increase transparency, is only weakly so. We interpret this as evidence that incumbent governors are significantly more likely to increase transparency when blame can be apportioned elsewhere (newly elected governors) or when reelection is not a concern and informational rents therefore less important.

## **Discussion**

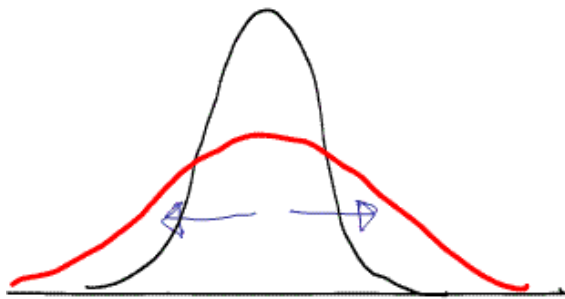
To be added

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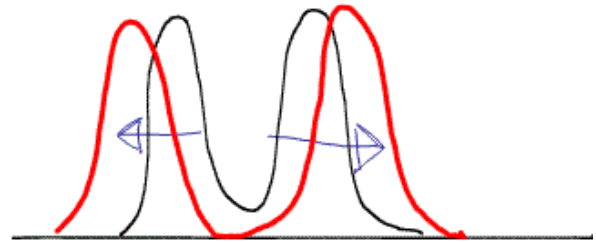
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Figure 1:

### Political Polarization in uni- and bi-modal distributions



(a) Increasing polarization in a unimodal distribution



(b) Increasing polarization in a bimodal distribution

Table 1: Polarization and Public Finance Outcomes

	(1) Expenditures	(2) Surplus	(3) Revenues
Lagged endogenous variable	0.839*** [0.0476]	0.825*** [0.0397]	0.825*** [0.0441]
Election year	75.44** [26.20]	-74.13* [30.72]	19.35 [26.30]
Pre-Election year	67.00* [29.10]	-49.64 [28.39]	26.53 [28.86]
Abs. partisan voter difference (APVD)	-0.238 [0.342]	0.734* [0.371]	0.696* [0.344]
APVD*Election year	-0.0847 [0.462]	-0.443 [0.483]	-0.904* [0.457]
APVD*Pre-Election year	-1.036 [0.605]	0.256 [0.513]	-0.833 [0.570]
Party Polarization (POLAR)	-82.90 [62.29]	67.98 [60.23]	-4.871 [54.92]
One party state (OPS)	-57.29 [41.65]	17.76 [41.97]	-49.37 [35.31]
POLAR*Election year	-91.08** [33.62]	87.37* [42.97]	-24.28 [38.57]
POLAR*Pre-Election year	-64.99 [36.82]	52.57 [40.54]	-26.89 [42.91]
OPS*Election year	-24.98 [26.92]	71.43* [34.70]	36.45 [35.12]
OPS*Pre-Election year	-18.79 [28.18]	59.60 [45.42]	31.29 [46.65]
Population (mill.)	25.41* [12.49]	-16.35 [16.86]	16.51* [7.885]
Unemployment	-26.81*** [5.396]	7.898 [5.070]	-12.34 [7.371]
Dependency ratio	-2721.4** [972.1]	651.6 [525.1]	-1726.0 [1126.0]
Grants received per cap	0.302** [0.112]	0.0785 [0.0857]	0.302*** [0.0672]
Real income per cap	0.0329*** [0.00823]	0.00537 [0.0143]	0.0516* [0.0206]
Observations	1008	1008	1008
No. of states	48	48	48

Robust standard errors in brackets

Significance levels: \* p&lt;0.05, \*\* p&lt;0.01, \*\*\* p&lt;0.001

Table 2: Polarization, Outcomes, and Budget Rules

	Expenditures		Surplus		Revenues	
	(1)	(2)	(3)	(4)	(5)	(6)
Lagged endogenous variable	0.831*** [0.0521]	0.761*** [0.0456]	0.798*** [0.0322]	0.834*** [0.0619]	0.825*** [0.0307]	0.801*** [0.0287]
Election year	94.51* [36.99]	66.38 [40.58]	-72.25 [46.09]	-59.95 [37.13]	26.26 [32.84]	-14.79 [38.30]
Pre-Election year	114.7** [41.47]	28.59 [40.86]	-24.12 [36.01]	-62.66 [31.97]	81.40* [36.39]	-54.42 [48.29]
Abs. partisan voter difference (APVD)	0.508 [0.418]	-0.724 [0.646]	0.0103 [0.547]	2.070*** [0.465]	0.582 [0.389]	1.420* [0.573]
APVD*Election year	-0.0979 [0.625]	-0.360 [0.800]	0.268 [0.705]	-1.222 [0.829]	-0.0523 [0.420]	-1.513*** [0.451]
APVD*Pre-Election year	-1.303 [0.805]	-0.459 [0.652]	0.431 [0.620]	-0.195 [0.959]	-0.874 [0.559]	-0.369 [0.998]
Party Polarization (POLAR)	-107.3 [61.11]	46.96 [82.44]	145.8 [85.57]	-56.39 [83.86]	17.68 [70.61]	-54.27 [53.96]
One party state (OPS)	-87.38 [51.86]	11.92 [58.23]	47.51 [53.89]	-28.73 [56.67]	-67.95 [57.65]	-39.68 [42.00]
POLAR*Election year	-104.1* [48.19]	-97.70* [48.82]	54.78 [61.97]	101.8* [51.83]	-47.97 [46.27]	31.22 [55.14]
POLAR*Pre-Election year	-122.0* [50.23]	-24.15 [54.37]	5.730 [53.94]	81.62* [41.21]	-101.7 [56.00]	79.59 [59.96]
OPS*Election year	-41.82 [39.08]	-33.06 [40.30]	69.14 [50.11]	35.64 [31.51]	24.59 [40.39]	26.54 [36.83]
OPS*Pre-Election year	-70.06 [39.90]	3.521 [46.93]	34.81 [49.48]	54.81 [30.89]	-24.84 [50.74]	78.16 [45.05]
Population (mill.)	30.21* [14.68]	-35.16 [57.05]	-4.335 [12.15]	1.668 [26.86]	19.29 [10.18]	-38.87 [51.05]
Unemployment	-30.82*** [7.632]	-10.73 [5.703]	8.776 [8.504]	3.500 [3.084]	-16.52 [11.20]	-1.941 [3.226]
Dependency ratio	-4636.9*** [743.7]	-1393.3** [529.6]	809.1 [1065.9]	1059.9* [426.4]	-3831.3* [1491.8]	-32.64 [325.7]
Grants received per cap	0.170 [0.115]	0.554*** [0.124]	0.0116 [0.0821]	0.166 [0.127]	0.163* [0.0809]	0.666*** [0.140]
Real income per cap	0.0220** [0.00687]	0.0433*** [0.0111]	0.00774 [0.0148]	-0.00917 [0.0110]	0.0485* [0.0234]	0.0444*** [0.00995]
Observations	630	378	630	378	630	378
No. of states	30	18	30	18	30	18
Sample: Nocarry over laws	Not binding	Binding	Not binding	Binding	Not binding	Binding

Robust standard errors in brackets

Significance levels: \* p&lt;0.05, \*\* p&lt;0.01, \*\*\* p&lt;0.001

Table 3: Polarization, Outcomes, and Fiscal Transparency

	Expenditures		Surplus		Revenues	
	(1)	(2)	(3)	(4)	(5)	(6)
Lagged endogenous variable	0.879*** [0.0329]	0.685*** [0.0432]	0.797*** [0.0367]	0.803*** [0.0417]	0.863*** [0.0336]	0.713*** [0.0306]
Election year	83.85** [28.49]	30.62 [46.28]	-63.30 [34.27]	-64.75 [59.37]	7.328 [31.94]	36.57 [53.90]
Pre-Election year	94.73* [38.88]	7.191 [42.34]	-69.24* [32.46]	-9.112 [45.85]	2.993 [34.99]	60.45 [59.16]
Abs. partisan voter difference (APVD)	0.524 [0.395]	-0.655 [0.455]	0.203 [0.487]	1.215** [0.444]	0.528 [0.557]	0.804 [0.558]
APVD*Election year	-0.653 [0.537]	0.884 [0.729]	-0.201 [0.678]	-0.426 [0.457]	-0.823 [0.618]	-0.402 [0.628]
APVD*Pre-Election year	-1.393 [0.891]	0.0631 [0.614]	0.123 [0.672]	-0.253 [0.391]	-0.912 [0.816]	-0.341 [0.736]
Party Polarization (POLAR)	-26.66 [52.58]	-66.87 [80.51]	81.01 [85.63]	-44.60 [87.77]	32.24 [88.57]	-32.72 [66.89]
One party state (OPS)	-28.16 [38.07]	-74.20 [60.86]	23.89 [55.17]	-19.26 [52.36]	-52.10 [58.48]	-56.04 [38.85]
POLAR*Election year	-73.34* [35.79]	-82.98 [60.22]	48.33 [40.99]	111.1 [87.60]	-10.01 [45.68]	-63.08 [72.25]
POLAR*Pre-Election year	-84.08 [47.41]	-22.73 [59.86]	67.70 [44.80]	31.80 [67.92]	7.680 [51.36]	-82.64 [75.11]
OPS*Election year	-46.97 [30.15]	31.56 [51.65]	82.85* [33.40]	18.95 [62.35]	44.31 [34.32]	-7.694 [56.07]
OPS*Pre-Election year	-61.36 [34.73]	41.98 [48.72]	110.0* [47.06]	-26.96 [55.34]	62.80 [53.26]	-37.63 [63.54]
Population (mill.)	21.09 [11.06]	26.13 [47.38]	2.450 [10.04]	-3.285 [41.85]	16.98 [13.88]	-15.76 [69.10]
Unemployment	-25.10*** [7.519]	-15.86* [6.736]	-3.529 [6.711]	19.39** [7.361]	-15.79 [8.836]	8.634 [4.914]
Dependency ratio	-2243.1* [922.0]	-4170.0*** [1244.5]	675.1 [530.3]	1147.9 [1031.8]	-1333.9 [912.8]	-1758.8 [1056.8]
Grants received per cap	0.235* [0.104]	0.628*** [0.131]	0.0715 [0.0838]	-0.0148 [0.114]	0.210** [0.0802]	0.580*** [0.133]
Real income per cap	0.0367*** [0.0101]	0.0329*** [0.00959]	0.00638 [0.0191]	0.000812 [0.00736]	0.0669* [0.0314]	0.0527*** [0.0105]
Observations	672	336	672	336	672	336
No. of states	32	16	32	16	32	16
Sample: Fiscal Transparency	low/medium	high	low/medium	high	low/medium	high

Robust standard errors in brackets

Significance levels: \* p&lt;0.05, \*\* p&lt;0.01, \*\*\* p&lt;0.001

Table 4: Polarization as a Cause of Fiscal Transparency

	Increase in Transparency			
	(1)	(2)	(3)	(4)
Party Polarization (POLAR)	0.235*	0.252*	0.245*	0.260*
	[0.0928]	[0.109]	[0.0931]	[0.110]
One party state (OPS)	0.152*	0.162*	0.158*	0.164*
	[0.0607]	[0.0739]	[0.0610]	[0.0748]
Abs. partisan voter difference (APVD)	0.000522	0.000720	0.000401	0.000479
	[0.000945]	[0.00128]	[0.000952]	[0.00132]
Democratic Governor	-0.0384*	-0.0474	-0.0425*	-0.0550*
	[0.0183]	[0.0241]	[0.0169]	[0.0218]
Democratic controlled State House	0.0800**	0.103**	0.0817**	0.106**
	[0.0286]	[0.0329]	[0.0292]	[0.0342]
Gov. debt per cap. (1000\$), lagged	-0.0245	-0.0273	-0.0263	-0.0302
	[0.0150]	[0.0179]	[0.0144]	[0.0169]
Gov. exp. per cap. (1000\$), lagged	0.0541*	0.0653*	0.0560*	0.0687*
	[0.0254]	[0.0310]	[0.0254]	[0.0310]
Governor cannot run again	0.0232	0.0345	0.0383	0.0563
	[0.0226]	[0.0297]	[0.0260]	[0.0343]
Divided government	-0.00709	-0.00953		
	[0.0176]	[0.0228]		
New governor*unified government			0.0359	0.0510
			[0.0210]	[0.0278]
Election year	0.00487	0.00918	0.00518	0.0103
	[0.0235]	[0.0314]	[0.0233]	[0.0312]
Election year - 1	-0.0192	-0.0223	-0.0197	-0.0222
	[0.0187]	[0.0241]	[0.0188]	[0.0241]
Election year - 2	-0.00481	-0.00542	-0.00548	-0.00642
	[0.0178]	[0.0238]	[0.0178]	[0.0238]
Observations	1008	777	1008	777
No. of states	48	37	48	37
Sample: States with reform	All	Reform	All	Reform

Robust standard errors in brackets

Significance levels: \* p<0.05, \*\* p<0.01, \*\*\* p<0.001