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The Impact of Fiscal Governance on Bond Markets: Evidence from Late Budgets and State Government Borrowing Costs

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Abstract

Does fiscal governance affect government borrowing costs? We operationalize fiscal governance as the ability of governments to pass a budget on time and, using a unique data set on budget enactment dates, analyze the effect of such late budgets on government bond yield spreads. Based on a sample of 36 US states in the period 1988-1997, we estimate that a budget delay of 30 days has a long run impact on the yield spread between 2 and 10 basis points. States with sufficient liquidity in the form of large reserves face small or no costs from late budgets.

Keywords: Fiscal governance, political deadlock; late budgets; fiscal stalemate; Chubb relative value survey; debt cost; bond spreads

JEL classification: H72; H61; H63

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1. Introduction

How does fiscal governance, the way a government goes about its fiscal business, affect its borrowing costs? Recent events in a number of EMU member countries have revealed large difference in governance practices despite a reasonably common set of fiscal institutions, rules and regulations within the EMU, and the crises in state-level government finances in the US similarly suggests widely different governance practices emerging from seemingly similar institutional set-ups.

This paper investigates how fiscal governance affect financial market evaluations of governments' fiscal health. A large literature has investigated both economic and political determinants of differences in government bond yields, including both fiscal institutions and key government budget variables, both across countries and across subnational governments, but there exists no study that goes beyond looking at formal institions and instead at actual governance practices. We focus on one key element of fiscal governance, the (in)ability of political actors involved in the budget process to pass a timely budget. This was stressed by Putnam (1993) as a key variable on governance, and expert survey assessments of whether budgets are concluded in a timely fashion appear as a part of commonly used measures of governance, including the Pew Center's initiative on state government performance employed by Knack (2002). We investigate the extent to which such bad fiscal governance affects the costs of servicing government debt.

Our empirical analysis is carried out in the context of US state governments, and is based on a unique data set, collected by us, containing the dates of final passage of the budget, identified using legislative records, newspaper sources and a survey of state budget officers. These data show that negotiations over the state budget often drag on well beyond the beginning of the new fiscal year, which is generally recognized to be the final deadline for timely passage of a budget. In the sample we consider below, consisting of 36 state governments in the period 1988-1997, 29.7 % of state budgets were enacted after the beginning of the new

fiscal year, with the average delay equal to 25 days.

Late budgets are, moreover, not only a historical phenomenon. Recent experiences with late budgets in California and New York have, once again, emphasized the problem of budget gridlock in US states; for example, in 2009, California went 24 days into the new fiscal year with no reserves before a new state budget was agreed on, while in 2010 the budget negotiations have dragged on for a record setting three months so far. Also in 2010, citizens in New York had to wait 125 days beyond the fiscal year deadline before a new state budget was signed into law, with state officials beginning to prepare for the state's first-ever government shutdown, as political deadlock threatened to end the series of emergency budget bills the state relied on to stay in operation.

When a government does not have a budget in place by the beginning of the new fiscal year, the legal basis for government spending is jeopardized. This can, among other things, lead to disruptions in debt payments and can serve to highlight concerns about the sustainability of government finances also in the longer run, including an increased preceived risk of government default. During the fiscal crisis of state governments in the early 1990s, analyzed in Poterba (1994), Moody's Investor Services noted that "[*budget] delays are symptomatic of serious financial imbalances*" continuing that while "*[b]udget delays do not automatically lead to a long-term rating revision [...] the resulting pressures on a state's short-term liquidity position can trigger a review.*"[1] Similar concerns are often voiced in public debates on late budgets.

Late budgets can affect state borrowing costs through two channels, a liquidity premium on state bonds and by serving as a market signal. Regarding the former, states may not have the legal authority to make appropriations towards debt repayments without a budget in place. While some states have special provisions in place to avoid exactly this, and in general make debt payments one of their first priorities, a state government can simply find it self out of cash to spend on any provisions, including debt repayments,.if the budget

negotiations drag on for too long. The risk of this occurring increases with each day that passes by without a new budget in place. As investors observe such late budgets, they will require a higher premium for holding state debt.

The possibility of a late budget to be a market signal arises from the fact that bond market participants may not always have perfect information about the true fiscal position of the state government. Severe budgets delays are likely to arise when painful adjustments are needed to secure state solvency. Thus, the inability to pass the budget can provide a strong signal to the market about the presence of large unresolved fiscal imbalances, and, perhaps most importantly, that the political actors responsible for the budget lack the ability to deal with these problems in an appropriate and timely manner. As such, it is not the immediate consequences of the late budget itself that lead to higher borrowing costs; rather, it is the fact that it draws the market's attention to the state's fiscal problems, possibly triggering changes in the state's credit rating, that creates a causal link from a late budget to state borrowing costs.

We measure state borrowing costs by state general-obligation bond yields from the Chubb Insurance Company "Relative Value Survey," explained in detail below. Our data on late budgets begin in 1988 and the data series on the Chubb Relative Survey, available for 36 state governments, ends in 1997. We estimate the relationship between the number of days without a budget and state borrowing costs using a dynamic panel GMM-model, which explicitly recognizes the strong degree of persistence in our dependent variable and controls for fixed state characteristics. We find that budget delays significantly increase state government bond yields in a substantive way. These findings are robust to controlling for a host of other variables identified in the literature, including fiscal institutions, economic conditions and fiscal outcomes.

A number of previous papers have used the Chubb survey, as it is the only data set that provides comparable bond yields across state governments. Eichengreen (1992) considers the

effect of balanced budget requirements on bond yields and Bayoumi, Goldstein and Woglom (1995) examine in detail the effects of the size of government debt. Poterba and Rueben (1999, 2001) examine the effects of a broad range of fiscal institutions, and Lowry and Alt (2001) examine both the interaction between fiscal institutions and the economy as well as the role of political parties. A number of these topics have also been addressed in the cross-country literature on yield spreads and borrowing costs, including Codogno et al. (2003) on the EMU and Hallerberg and Wolff (2008), who look specifically at fiscal institutions within EMU member countries.

The paper proceeds in the following way: The next section describes our data and empirical strategy, while section 3 presents our empirical results and quantitative assesments. Section 4 concludes.

2. Data

2.1. The Chubb Relative Value Survey

Comparable market data on state bond yields are not readily available. Following Poterba and Rueben (2001) and Lowry and Alt (2001) we instead use data on state government bond yield spreads given from the "Chubb Relative Value Survey." This survey measures the bond yield for 39 states relative to New Jersey by asking roughly 25 sell-side bond traders to estimate the current yield, measured in basis points, on a hypothetical 20-year general obligation bond, relative to comparable bonds issued by the state of New Jersey.[2] Thus, differences in yields should only reflect differences in perceived riskiness of the state's general obligation debt, and not differences in maturity or other bond charachteristics.[3] The survey was conducted about every 6 months from July 1973 to January 1998. From 1976 to 1992, the survey was conducted in June and December.[4] In 1993 it was conducted in June, and beginning in 1994, the survey was done in January and July. Our dependent variable, $Chubb_{i,t}$, is constructed as the average of the summer (June/July) and winter

(December/January) Chubb surveys, such that $Chubb_{i,t}$ reflects survey answers given *after* the budget negotiations in the spring of year t, but before next year's budget negotiations commence. Thus, up to and including 1992, our dependent variable is given as the average of the June and December survey. Our 1993 observation is the average of the surveys from June 1993 and January 1994, and our 1994 observation is the average of the surveys from July 1994 and January 1995. The 1995 to 1997 observations are constructed in a similar manner as the 1994 observation.

2.2. Late budgets

What constitutes a late budget? In practice, budget processes vary considerably across US states. This complicates cross-state comparisons of budget timeliness, for investors and scholars alike, as there is no obvious, universal definition of when a budget is late. In this paper, we define budget negotiations to be concluded when the budget is finally enacted, typically by the governor signing the budget. [5] We compare this date to the date when the fiscal year begins and count the difference in days; we call this measure $days\_late_{i,t}$. Thus, if the budget for the fiscal year that starts in year t is signed into law 5 days after the end of the old fiscal year in state i, $days\_late_{i,t}$ takes the value 5. If the budget is signed into law 5 days before the end of the old fiscal year, it takes the value -5. The marginal effect on government yield spreads of using another day to finish the budget is likely to change dramatically once the fiscal year deadline is exceeded. To account for this, we separate $days\_late_{i,t}$ into two variables: $days\_late\_neg_{i,t}$, which is equal to $days\_late_{i,t}$ if $days\_late_{i,t}$ is negative, and zero otherwise, and the corresponding variable for positive values, $days\_late\_pos_{i,t}$.

The data for the budget enactment dates were collected from three sources: (i) State legislatures' websites; (ii) Archived newspaper articles; and (iii) a survey sent to state budget officers. Some state legislatures' websites have detailed information on the status and histories of all bills enacted in previous legislative sessions, including the budget bill(s). However,

most state legislatures' bill tracking tools only cover the most recent legislative sessions, if any. We therefore supplemented with information from archived (mostly state and local) newspaper articles accessed via Newslibrary.com.[6] Finally, we also sent a survey to state budget officers asking them to confirm the data we had collected ourselves as well as provide us with the information that we had not been able to find via any of the other sources. Out of the 48 mainland states, 19 responded to our survey. When overlapping, the data they reported were virtually identical to the data we collected ourselves.[7]

Figure 1 shows the distribution of budget enactment dates relative to the beginning of the fiscal year. A large share of budgets are enacted very close to the deadline, but many budgets are finalized after the deadline. For the years 1988-1997 we have recorded 79 cases where the budget was signed into law after the beginning of the new fiscal year, on average by 25.3 days. This amounts to 29.7 % of the budgets for which we have data.

[Figure 1 about here]

The distribution of late budgets is not even across states, as can be seen from Figure 2 showing the share of budgets that were late for the 36 states in our sample for the period we consider. The most important causes of late budgets are the presence of divided government and the state economic situation, measured by changes in the state unemployment rate; these causes are analyzed in detail in Andersen, Lassen and Nielsen (2010a), and included as controls in our analysis below.

[Figure 2 about here]

2.3. Econometric model and explanatory variables

Following Poterba and Rueben (2001), we can estimate state $i's$ spread to New Jersey (denoted with subscript NJ) in a given year t, $R_{i,t} - R_{NJ,t}$, as:

$$R_{i,t} - R_{NJ,t} = F(X^*_{i,t}, Z^*_{i,t}, Y^*_{i,t}) - F(X^*_{NJ,t}, Z^*_{NJ,t}, Y^*_{NJ,t})$$

where $X^*_{i,t}$ is a vector of economic and fiscal variables that are likely to affect the states probability of paying current and future interest obligations. We include our measure of late budgets in this category since, as argued above, late budgets are likely to increase the riskiness of interest payments. $Z^*_{i,t}$ is a vector of state fiscal institutions that are likely to affect government spending and its ability to collect revenues. No carry-over rules and tax and expenditure limits are examples of this. $Y^*_{i,t}$ proxies fiscal taste for debt repayment and other relevant political variables. Linearizing the above equation, the bond spread in state i in year t can be expressed as:

$$R_{i,t} - R_{NJ,t} = \beta_1^{'}(X_{i,t} - X_{NJ,t}) + \beta_2^{'}(Z_{i,t} - Z_{NJ,t}) + \beta_3^{'}(Y_{i,t} - Y_{NJ,t}) + \eta_i + \gamma_t + u_{i,t} - u_{NJ,t} \quad (1)$$

where β_j is $k_j \times 1$ vector of coefficients and $X_{i,t}$, $Z_{i,t}$ and $Y_{i,t}$ are our observed values of $X^*_{i,t}$, $Z^*_{i,t}$ and $Y^*_{i,t}$, respectively. η_i is an unobserved state fixed effect and γ_t measures aggregate shocks. $u_{i,t}$ captures omitted variables and errors from approximation. Since bond yields display a great deal of persistence, as noted by Lowry and Alt (2001), we also include lags of the dependent variable, making the model a dynamic panel data model.[8] Including L lags and defining $R_{i,t} - R_{NJ,t} \equiv Chubb_{i,t}$, equation (1) can be written as the following:

$$Chubb_{i,t} = \alpha_1 Chubb_{i,t-1} + ... + \alpha_L Chubb_{i,t-L} + \beta_1^{'} X_{i,t} + \beta_2^{'} Z_{i,t} + \beta_3^{'} Y_{i,t} + \eta_i + \lambda_t + \varepsilon_{i,t} \quad (2)$$

where we use that $\lambda_t \equiv \gamma_t - \beta_1^{'} X_{NJ,t} - \beta_2^{'} Z_{NJ,t} - \beta_3^{'} Y_{NJ,t} - u_{NJ,t}$ is constant across i in year t. We choose a value of L such that the error term, $\varepsilon_{i,t}$, displays iid properties. We estimate this dynamic panel data model using the GMM procedure developed in Arellano and Bond (1991), Arellano and Bover (1995) and Bond and Blundell (1998). However, as robustness checks we also estimate the model using the OLS and Fixed Effect estimators.

We now turn to the exact content of $X_{i,t}$, $Z_{i,t}$ and $Y_{i,t}$, the vectors of explanatory variables. For the sake of brevity, we limit ourselves to a short description of the variables here. Precise

definitions of all variables and their data sources can be found in the data appendix.

2.3.1. Economic and fiscal variables

An obvious control variable is the level of state government debt. We scale the debt level relative to state GDP, because we want to measure the debt burden relative to the tax base. The state government budget surplus in the old fiscal year, i.e. the fiscal year that ends in year t, is another natural control. As in Lowry and Alt (2001), we therefore include a variable that is equal to zero in case of a deficit and otherwise equal to the (actual) surplus in percent of state GDP. We also include the corresponding variable for the deficit, thus allowing the effect of government net lending to differ depending on whether it is negative or positive. Like Lowry and Alt, we always include an interaction between our deficit variable and a dummy for whether the state has a no carry-over rule in place.

To control for the effect of business cycle fluctuations we include the change in the state unemployment rate as an explanatory variable.[9] Following Poterba and Rueben (2001), we also use the deficit shock variables originally developed in Poterba (1994) to control for fiscal shocks. Unlike Poterba and Rueben, however, we allow the effects of revenue shocks and expenditure shocks to differ by including a separate variable for each type of shock.

Government bond yields are likely to be sensitive to the liquidity position of the state government, and easy access to readily available funds is important for reliable debt service. We therefore include the (projected) end-of-year balance in the state's general fund and stabilization fund as an explanatory variable in our baseline specification.[10]

Finally, we include the change in the state's credit rating since the previous year as an explanatory variable.[11] We do this for two reasons: First, this may capture new information about the state's future ability to repay its debt obligations, which is known to rating agencies and investors, but unobservable to us (the researchers). Second, the credit rating itself can have an independent effect in an uninformed market if it influences investor sentiments, even

if the rating is not based on any fundamentals.

2.3.2. Political variables

Divided partisan control over the state government may potentially work as a check on new spending initiatives, thereby leading to lower borrowing costs. This is likely to be especially important in the absence of strict balanced budget rules. To account for this, we include a dummy for divided government, as well as its interaction with a dummy for no carry-over rules. Investor sentiments can also be influenced by the political preferences of the politicians in charge of fiscal policy. As a final control variable, we therefore include a measure of government ideology, taken from Berry et al (1998), where a higher value is associated with more liberal preferences.

2.4. Conditional effects of late budgets

The liquidity premium and market signal channels described above imply that the impact of late budgets on government bond yields depends on a number of observable characteristics. First, if a state ended the old fiscal year with a budget deficit and only few cash funds available in the general fund and stabilization fund, then we would expect a larger effect of late budgets through the liquidity premium channel, since such funds provide a safeguard against the risk that the state will run out of cash during a protracted political stalemate over a new budget. Second, a budget delay is likely to send a much more powerful signal about politicians' inability to deal with underlying fiscal imbalances when it is combined with a large deficit and a low end-balance in the fiscal year that just ended: If politicians can't agree on an answer to a state's fiscal problems when they are most pressing, and when the costs of inaction are likely to be highest, it seems unlikely that they ever will.[12]

A similar argument applies to the effect of election years: Andersen, Lassen and Nielsen (2010b) find that voters punish state legislators, and, to a lesser extent, governors, for late

budgets at the election polls. It seems plausible that this potential consequence of budget delays will assume a more prominent place in the minds of state lawmakers in election years than in off-election years. If state politicians are unable to pass a budget on time in an election year, despite the saliency of the potential consequences in such years, it may therefore send a stronger signal to financial markets about the state government's inability to deal with the fiscal challenges facing the state.

We test these hypotheses by estimating alternative versions of (2) that include interaction terms between our late budget variable $days\_late\_pos_{i,t}$ and each of the following: The size of the combined balance in the general fund and the budget stabilization fund at the end of the old fiscal year, the government surplus in the old fiscal year, and a dummy for state general election year. We then expect negatively signed coefficients on the first two interaction terms, and a positive coefficient on the interaction term involving the election year dummy.

3. Results

Table 1 shows the results from our baseline specification. Columns (1)-(3) reports estimates of equation (2) using the OLS, Fixed Effects and GMM estimators, respectively. We include four lags of our dependent variable to account for autocorrelation in the yield spread.[13] The estimated coefficients on the control variables are largely as expected. Larger debt, lower end-of-year balances and deteriorating credit ratings all increase the yield spread, with the effects being significant on a 1% level. Unexpected expenditure shocks also have a significant impact on the yield spread, but we do not find any significant effect from shocks to state revenue. Nor do we find any significant effects from yearly changes in the state unemployment rate.

The impact of the state government's fiscal balance in the old fiscal year depends strongly on whether this balance is positive or negative: Higher surpluses do not seem to affect yield

spreads. Deficits, on the other hand, have a strong impact. This is only true for states that allow deficits to be carried over to the next fiscal year, however: States that have a no-carry-over law in place see no effect on the yield spread from deficits, as can be seen by the negative coefficient on $gov\_deficit\_no\_carry_{i,t}$, which is significant and similar in magnitude to the coefficient on $gov\_deficit_{i,t}$.

More liberal government seem to pay a higher yield spread, but the effect is only borderline significant. Finally, having divided government tends to lower the yield spread in states that do not have a no-carry over law in place, whereas states with strict no-carry over laws see no effect of divided government.

We now turn to our main variables of interest, $days\_late\_neg_{i,t}$ and $days\_late\_pos_{i,t}$. The coefficients on both variables are positive, implying that longer negotiations over the budget are associated with higher yield spreads. The coefficient on $days\_late\_pos_{i,t}$ is highly statistically significant, and much larger than the coefficient on $days\_late\_neg_{i,t}$, which is very close to zero. We interpret these results as evidence in favor of our priors: Taking an extra day to finish the budget does not affect investors' requirements for state government bond yields much as long as the fiscal year deadline is not exceeded. Once the deadline has been passed, however, further delays in the budget process lead to significantly higher borrowing costs.

These results are robust in terms of sign and levels of significance to removing outliers, but if we remove the three observations where negotiations dragged on for more than 100 days, which is Wisconsin in 1997 and New York in 1996 and 1997, the coefficient on $days\_late\_pos_{i,t}$ more than quadruples, from .026 to .115, and is robust to further truncations of the sample; we return to this when we discuss the quantitative impacts below.

[Table 1 about here]

Next, we move on to include interaction terms between our late budget variable and the variables discussed in the previous section. Table 2 shows the results.[14] We start by

interacting $days\_late\_pos_{i,t}$ with the size of the end-of-year balance in the old fiscal year. The coefficient on $days\_late\_pos_{i,t}$ is positive, significant at a 1% level and almost twice as large as the estimate from column (3) in Table 1. The coefficient on the interaction term with the end-of-year balance is negative and also highly significant. Thus, larger end-of-year balances in the general fund and budget stabilization fund mute the impact of late budgets on state government borrowing costs. A back-of-the-envelope calculation based on the sizes of the estimated coefficients suggests that late budgets only cause yield spreads to rise in states where fund reserves amount to less than 2% of general fund expenditures. This applies to approximately a third of the observations in our sample.

In column (2) we interact $days\_late\_pos_{i,t}$ with the old fiscal year's government surplus (relative to state GDP).[15] The coefficient on $days\_late\_pos_{i,t}$ is positive, large and significant, while the coefficient on the interaction term with the government surplus is negative and highly significant. Thus, late budgets have a smaller effect on government yield spreads if they are accompanied by a sufficiently large surplus, but severe if accompanied by a deficit: At a deficit of 1% of general state government spending, the impact of a budget delays is almost three times larger than when the last fiscal year ended in balance ($gov\_spl_{i,t} = 0$). In column (3), we include both interactions terms simultaneously. The coefficients on the interactions terms decrease slightly compared to columns (1) and (2), but they are both still negative and highly significant.

In column (4), we interact $days\_late\_pos_{i,t}$ with a dummy for gubernatorial election year. We find that the marginal impact of late budgets in non-election years is around the same as what we found in our baseline specification in Table 1. In contrast, the effect is about four times larger in election years.[16]

To sum up, the results found here broadly confirm our hypotheses about the conditional effects of late budgets on government bond yield spreads: The availability of previously accumulated reserves dampen the impact of late budgets on state government borrowing

costs, while fiscal imbalances and the proximity of upcoming elections magnify it.

[Table 2 about here]

3.1. Quantifying the Effect of Late Budgets

How large are the effects of late budgets on state government borrowing costs? In the following, we use our estimates from the previous section to calculate the impact on yield spreads of a hypothetical 30-day budget delay. While delays of this length are in most states not everyday news, they do occur: Of the 266 budget adoption processes in our sample, 79 were delayed beyond the beginning of the new fiscal year, and 23 of those, almost 10 percent of the sample, by 30 days or more.

Using our baseline estimates of the coefficient on $days\_late\_pos_{i,t}$ from Table 1, we see that that the immediate impact of a 30-day delay is about 1 basis point. That is, for every 10,000 dollars of debt issued, the state must now pay an extra dollar in interest. Taking the persistence in the yield spread into account, we can calculate a "long run" impact on total interest payments. To do this, we consider a thought experiment in which a state issues new debt (or refinances existing debt) for a fixed amount each year. In this situation, the total effect of a late budget, measured in basis points and summed over all future years, can be calculated by multiplying the immediate impact with the long run impact factors reported in the bottom of tables 1 and 2. Our baseline estimates in Table 1 thus suggest that the total long run impact of a 30-day late budget is in the range of 2 basis points (when using the lower bound estimates given by the GMM and Within estimators) and 5 basis points (using the upper bound estimate given by the OLS estimator).[17] As noted above, these results are, however, significantly influenced by three outliers where budget negotiations dragged on for more than 100 days past the deadline. In the robust sample without these outliers, effects are considerably larger: The immediate impact of a 30-day delay is 3.5 basis points based on the GMM estimator, with a corresponding total long-run impact equal to 10.0 basis points.

For both the full and the robust samples, the estimates reported in Table 1 and above reflect unconditional average effects across all observations in our sample. As shown in Table 2, the impact of late budgets differs substantially depending on the economic and political circumstances under which they occur. In a state where fiscal reserves are approaching zero, our results indicate an immediate impact of 1.4 basis points following a 30-day delay, and a long run impact of 4.2 basis points. Similarly, conditioning on the size of the budget deficit in the old fiscal year reveals a substantial variation in the effect of a 30-day delay: For a state than ran a deficit equal to 0.34% of state GDP (the average deficit among all deficits in our sample), we find immediate- and long run impacts of 3 and 9 basis points, respectively, while the corresponding numbers are 1.8 and 5.6 basis points for a state with a balanced budget, and 0.7 and 1.9 basis points for states with a surplus equal to 0.37% of state GDP (the sample average of all surpluses). Furthermore, the results in column (4) of Table 2 suggest that the impact of a 30-day late budget is more than 4 times larger in election years than in non-election years (the long run impact rising from 2.3 to 9.4 basis points). Finally, the coefficient estimates in column (5), where we include all interaction terms simultaneously, show that a 30-day delay has a long run impact of 14.2 basis points when we condition on an end-of-year balance at zero, a deficit at 0.34% of GDP, and a gubernatorial election coming up.

Whether these numbers are "small" or "large" is not obvious. For comparison, the average spread to New Jersey in our sample is 8.5 basis points with a standard deviation of 15 basis points. One way to assess the magnitude of the impact of late budgets is to compare it to the impact of other economic factors. Imagine a state (without a no-carry-over requirement) ending its fiscal year with a budget deficit equal to 0.34% of state GDP (the sample average) and a timely adopted budget for the new fiscal year. In this situation, going from an on-time budget to a 30-day delay has the same impact on the yield spread as a three-fold increase of the deficit, or as a 14% unexpected increase in general fund expenditures (as measured

by the deficit shock variable $exp\_shock_{i,t}$).[18] Thus, when it comes to the impact on state government borrowing costs, lengthy delays in the budget adoption process are comparable to sizeable, adverse fiscal shocks.

3.2. Robustness Analysis

In Table 3 we consider a number of robustness issues. Columns (1) to (3) replicate Table 1, but with $days\_late\_pos_{i,t}$ and $days\_late\_neg_{i,t}$, our variables of primary interest, replaced with two new variables, $days\_delayed\_pos_{i,t}$ and $days\_delayed\_neg_{i,t}$. Rather than counting the number of days from the end of the fiscal year until the budget is signed into law, these variables focus on legislative budget delays by measuring the number of days from the legislature's deadline for passing the budget until the legislature actually passes the budget in its final form (see Andersen, Lassen and Nielsen (2010a) for details). Using these alternative measures does little to change our conclusions, although the estimated effects of budget delays are now somewhat smaller.[19]

A key theme in Poterba and Rueben (1999, 2001) is how the effect of fiscal shocks on borrowing costs depends on state fiscal institutions. Our baseline estimation results reported above do not allow for such interactions. We compensate for this in the estimations reported in Table A1 in the appendix. Here we interact dummy variables for no carry-over rules as well as tax- and expenditure limits (TELs) with each of the expenditure shock and revenue shock variables. The results broadly confirm the conclusions found in Poterba and Rueben (2001): Having a no carry-over rules in place seems to neutralize the impact of expenditure shocks. The same is the case for expenditure limits. There does not seem to be much effect of expenditure limits on the impact of revenue shocks. Tax limits appear to have a clear effect on expenditure shocks but not on revenue shocks. Most importantly for our purposes, however, the inclusion of these interaction terms does in no case change the sign, magnitude or statistical significance of the late budget variable $days\_late\_pos_{i,t}$.

[Table 3 about here]

4. Conclusion

This paper estimates the impact of bad fiscal governance in the form of late budgets on state government borrowing costs in the US. We find that late budgets significantly increase the yield spread on hypothetical 20-year general obligation bonds. Our results provide clear evidence that bond market investors bad fiscal governance as a reason for concern when it comes to the prospect of uninterrupted repayments on state debt, and they strongly suggest a link between late budgets and higher state borrowing costs.

The average, unconditional long-run cost of a 30-day late budget is an increase in the yield spread fo about 10 basis points based on the robust sample. The effect varies greatly depending on the state's economic and political situation, however. Easy access to liquidity, for example in the form of previously accumulated reserves, reduces investors' fear of payment disruptions in case of a late budget, thereby lowering the premium paid for budgets delays. In contrast, the impact of a month-long delay is much higher if the state has run out of reserves. Markets also punish late budgets much more harshly if they occur during times of fiscal stress. In such times, a late budget sends a powerful signal about politicians' (lack of) ability to address fiscal imbalances, and investors react more sharply. A related effect is present in election years: When an election is approaching, the personal costs to state politicians of a late budget are presumably higher, and delays in the budget process demonstrate more clearly that politicians are incapable of reaching fiscal compromises. Indeed, our results show that the reaction of bond market participants to late budgets is 4 times stronger in election years than in non-election years.

Our estimates should be seen as a lower bound on the economic costs of late budgets. Costs related to the disruption of state government services and payments, difficulties in fiscal planning in state agencies and local governments, and the uncertainty facing state gov-

ernment employees and citizens are likely to be substantial, but are practically impossible to measure.[20] With the lower bound being positive, our results provide a strong rationale for state governments to avoid lengthy delays in the budget process. And, perhaps equally as important, they provide voters with a rationale for holding their elected politicians accountable when they fail to deliver a state budget on time.

Notes

[1]Quoted from *The Bond Buyer*, July 2, 1992
(http://www.highbeam.com/doc/1G1-12436969.html)

[2]States excluded from the Chubb survey are: Arizona, Arkansas, Colorado, Idaho, Indiana, Iowa, Kansas, Nebraska, South Dakota and Wyoming. Since our data set on late budgets does not include Alaska and Hawaii, and since our sample for Montana starts after the end of the Chubb survey, our effective sample consists of a total of 36 states (not counting New Jersey).

[3]See the discussion in Poterba and Rueben (1999, 2001).

[4]The exact time of the survey varied slightly before 1976.

[5]There are a number of exceptions to this general definition: for example, if the governor vetoes the entire budget, the legislature can in most states override the veto by some super majority vote in both chambers, and the budget then becomes law without the governor's signature; alternatively, governors may in some states let the budget become law without signing it, simply by letting the deadline for gubernatorial vetoes run out.

Our measurement is further complicated by the fact that some states do not pass a single, all-encompassing budget bill. Instead, their budgets consist of several individual appropriation bills. In such cases we do not consider the budget fully enacted until the last appropriation bill for state operations has been enacted. Also, state governments sometimes react to unexpected developments in state government finances by passing within-fiscal year supplementary appropriation bills. We do not view such supplementary budget bills as part of the budget adoption process that we are interested in, however, and we therefore restrict our attention to the budgets as originally enacted. See Andersen et al. (2010a) for a thorough discussion of these and other related issues.

[6]Newslibrary.com is an online newspaper archive that covers more than 2,500 news sources across the United States. We also used The New York Times online archive on several

occasions to access relevant news articles. In many cases, these newspaper accounts contained additional information helpful in handling uncertain cases. All articles used in constructing the data set is on file with the authors.

[7]The instructions for the survey are available from the authors upon request. Table A.1 gives details on the coverage of and sources of information on late budgets for each state.

[8]Using the Dicky-Fuller test for unit roots in heterogeneous panels, as proposed by Im, Pesaran and Shin (2003), we can clearly reject the presence of a unit root in our spread variable.

[9]We include the unemployment rate in first differences, but obtain very similar results when using the level of the unemployment rate.

[10]The end-of-year balance is measured in percent of proposed general fund expenditures (see data appendix for more details).

[11]We use the first difference of Moody's rating, where positive values imply an improved rating.

[12]The lack of immediately available funds were a major factor leading to California's decision to issue IOUs when faced with a late budget in 2009. In New York in 2010, government workers where given furlough notices as a way of reducing expenditures during a severe budget delay that prevented a more permanent deficit-reducing solution from being implemented. Thus late budgets seem more likely to lead to particularly disruptive outcomes when they occur along with government deficits and low government savings.

[13]We also tried including a fifth lag, but this was never significant. Our results are not sensitive to the exact number of lags. Testing for autocorrelation in all our GMM estimations revealed no signs of second order or higher autocorrelation, which suggests that the GMM procedure is indeed valid. Note that our sample size is not reduced when introducing more lags of the dependent variable, since we have data for the yield spread available well before 1988.

[14]As in the analyses above, the coefficient on $days\_late\_neg_{i,t}$ is very small in all cases, so for simplicity we impose a zero restriction on it in the analyses presented in this section. Relaxing this restriction does not change the results for the interaction terms involving $days\_late\_pos_{i,t}$.

[15]We do not separate surpluses from deficits here. Deficits thus appear as negative values.

[16]As shown in column (5) of Table 2, including all three interaction terms simultaneously does not alter the estimated coefficients much compared to when we include them separately, and all coefficients are significant on a 1% level.

[17]Assume that the state needs to finance a fixed amount X , where X is measured in ten thousands of dollars. A 1-day late budget in year t then gives rise to extra interest costs worth $\beta_L X$ for debt issued in year t, $\alpha_1\beta_L X$ for debt issued in year $t+1$, $(\alpha_1^2+\alpha_2)\beta_L X$ for year $t+2$, etc., where β_L is the coefficient on $days\_late\_pos_{i,t}$ and α_i is the coefficient on the i'th lag of the dependent variable. Applying the formula for an infinite geometric series then gives us a long run impact of $(1-\alpha_1-...-\alpha_L)^{-1}\beta_L$, where the first term represents the long run impact factors reported in Table 1 and Table 2.

[18]These calculations are based on the estimation results of the specification in Table 2, column (2). The estimated coefficients on $gov\_deficit_{i,t}$ and $exp\_shock_{i,t}$ (not reported in Table 2) are 4.402 and 0.207, respectively. The implied effects on yield spreads are comparable in size to those found in Lowry and Alt (2001) and Poterba and Rueben (2001).

[19]In additional robustness analyses, we restricted our sample to states the experienced a late budget at least once in the period for which we have data. This does not affect results.

[20]As noted in a New York Times editorial in the run up to a possible shutdown in 2010: "[*A*] *shutdown would disrupt millions of lives, cost millions of dollars and leave state officials scrambling to operate prisons, the State Police and the Metropolitan Transportation Authority* [...]". (New York Times editorial on June 12, 2010, http://www.nytimes.com/2010/06/12/opinion/12sat1.html?hp)

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5. Appendix

[Table A1 about here]

[Table A2 about here]

[Table A3 about here]

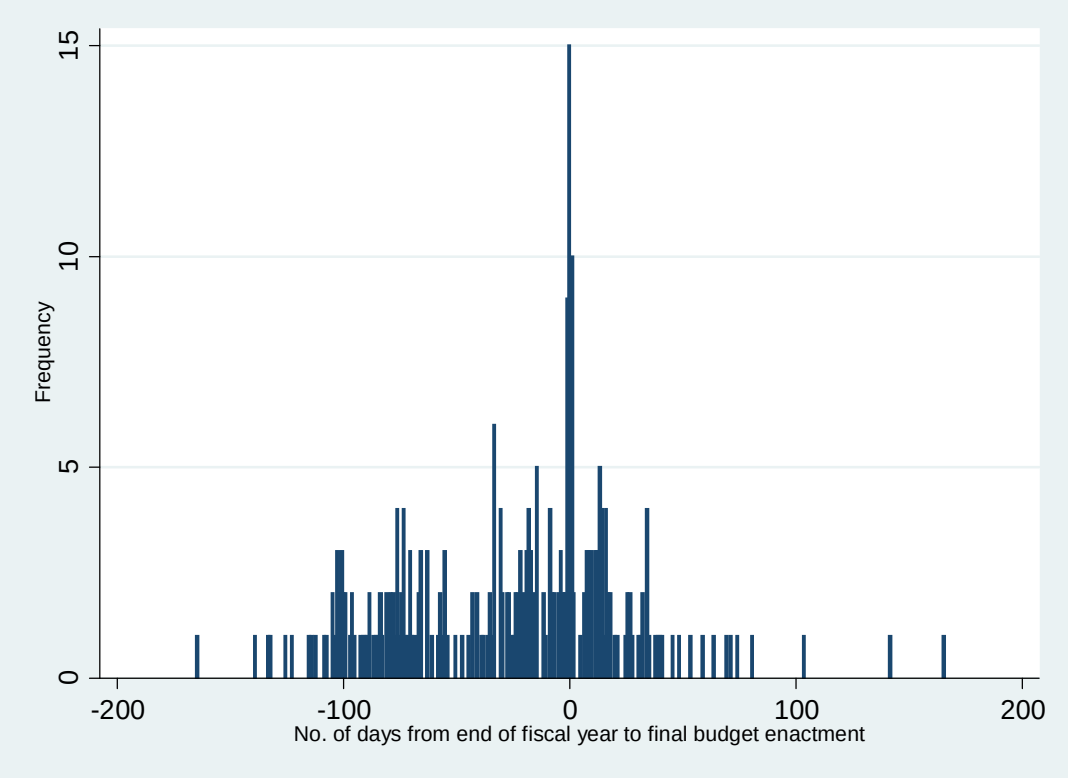

Figure 1: The date of budget completion relative to the beginning of the fiscal year, 36 states, 1988-97.

Note: The sample of states excludes AK and HI as well as states that are not rated in the Chubb Relative Survey, as noted in the text. We do not have information for all states for all years, see Table A3 for details.

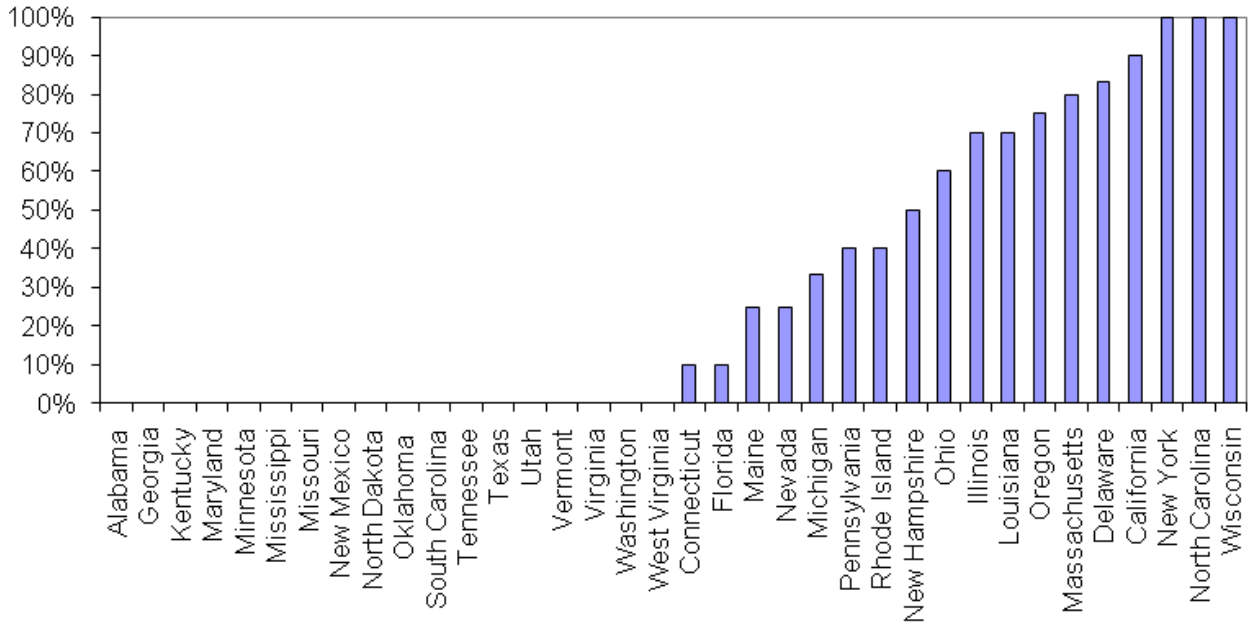

Figure 2: The share of budgets passed after the beginning of the new fiscal year, 36 states, 1988-97.

Note: The sample of states excludes AK and HI as well as states that are not rated in the Chubb Relative Survey, as noted in the text. We do not have information for all states for all years, see Table A3 for details.

Table 1. The Effect of Late Budgets on Yields Spred on 20-year GO Debt

	Yield spread vs. New Jersey on 20-year GO debt		
	(1)	(2)	(3)
Days_late_neg $_{i,t}$	0.001	0.003	0.004
	(0.006)	(0.011)	(0.015)
Days_late_pos $_{i,t}$	0.035***	0.025**	0.026***
	(0.008)	(0.009)	(0.009)
Unempl_change $_{i,t}$	0.252	0.051	-0.06
	(0.404)	(0.484)	(0.413)
Endbalance $_{i,t}$	-0.168***	-0.192***	-0.315***
	(0.057)	(0.058)	(0.057)
D_Moodys $_{i,t}$	-5.798***	-6.435***	-6.766***
	(1.509)	(1.760)	(1.639)
Debt $_{i,t}$	0.110**	0.274	0.375***
	(0.044)	(0.274)	(0.143)
Rev_shock $_{i,t}$	-0.038	-0.024	-0.041
	(0.047)	(0.053)	(0.069)
Exp_shock $_{i,t}$	0.196***	0.176**	0.224***
	(0.063)	(0.066)	(0.082)
Gov_surplus $_{i,t}$	1.257**	0.528	0.603
	(0.618)	(0.746)	(1.260)
Gov_deficit $_{i,t}$	7.141***	3.7	4.640**
	(1.624)	(2.492)	(1.878)
Gov_deficit $_{i,t}$ x *No_carry* $_i$	-8.007***	-5.066*	-6.379***
	(1.979)	(2.607)	(1.670)
Divided_gov $_{i,t}$	-0.45	-1.488	-1.721*
	(0.797)	(0.888)	(0.919)
Divided_gov $_{i,t}$ x *No_carry* $_i$	0.239	0.844	1.255*
	(0.487)	(0.520)	(0.653)
Ideology_gov $_{i,t}$	0.006	0.027	0.051
	(0.011)	(0.018)	(0.031)
No_carry $_i$	0.073		
	(0.425)		
Estimator	OLS	FE	GMMSYS
Time dummies	Yes	Yes	Yes
Number of lags of dependent variable	4	4	4
Sample	1988-1997	1988-1997	1988-1997
Long-run impact factor	4.957	2.686	2.925
Number of states	36	36	36
Observations	266	266	266

*** p<0.01, ** p<0.05, * p<0.1

Standard errors reported in parentheses. Cluster adjusted standard errors used in colums (1) and (2). Robust standard errors used in colum (3).

GMMSYS estimates are obtained using the dependent variable lagged twice or more as instruments in the differenced equation. The level equation uses the lagged first difference of the dependent variable as instrument. See Table A2 for variable description

Table 2. Interacting Late Budget with Endbalances, Government Surplus and Election Dummy

	Yield spread vs. New Jersey on 20-year GO debt				
	(1)	(2)	(3)	(4)	(5)
Days_late_pos $_{i,t}$	0.047***	0.061***	0.066***	0.026***	0.065***
	(0.013)	(0.023)	(0.022)	(0.009)	(0.021)
Days_late_pos $_{i,t}$ x *Endbalance* $_{i,t}$	-0.027***		-0.021**		-0.018***
	(0.010)		(0.008)		(0.007)
Days_late_pos $_{i,t}$ x *Gov_spl* $_{i,t}$		-0.111***	-0.071**		-0.074***
		(0.029)	(0.030)		(0.026)
Days_late_pos $_{i,t}$ x *Elex* $_{i,t}$				0.081***	0.068***
				(0.018)	(0.022)
Estimator	GMMSYS	GMMSYS	GMMSYS	GMMSYS	GMMSYS
Time dummies	Yes	Yes	Yes	Yes	Yes
Number of lags of dependent variable	4	4	4	4	4
Sample	1988-1997	1988-1997	1988-1997	1988-1997	1988-1997
Long-run impact factor	2.965	3.065	2.994	2.939	2.995
Number of states	36	36	36	36	36
Observations	266	266	266	266	266

*** p<0.01, ** p<0.05, * p<0.1

Robust standard errors in parentheses

Same explanatory variables as in Table 1 included in all estimations. See notes in Table 1 for more details.

Table 3. Robustness: Using Legislative Delays as indicator for Late Budgets

	Yield spread vs. New Jersey on 20-year GO debt		
	(1)	(2)	(3)
Days_delayed_neg $_{i,t}$	-0.006	0.012	0.014
	(0.006)	(0.016)	(0.016)
Days_delayed_pos $_{i,t}$	0.027***	0.013	0.028***
	(0.010)	(0.008)	(0.009)
Estimator	OLS	FE	GMMSYS
Time dummies	Yes	Yes	Yes
Number of lags of dependent variable	4	4	4
Sample	1988-1997	1988-1997	1988-1997
Long-run impact factor	4.899	2.709	2.918
Number of states	36	36	36
Observations	262	262	262

*** p<0.01, ** p<0.05, * p<0.1

Robust standard errors in parentheses in colums (2)-(6). Cluster adjusted standard errors used in colum (1)

Same explanatory variables as in Table 1 included in all estimations. See notes in Table 1 for more details.

GMMSYS estimates are obtained using the dependent variable lagged twice or more as instruments in the differenced equation. The level equation uses the lagged first difference of the dependent variable as instrument.

Table A1. The Effect Fiscal Rules on Yield Spreads

	Yield spread vs. New Jersey on 20-year GO debt		
	(1)	(2)	(3)
Days_late_neg $_{i,t}$	0.004	0.005	0.003
	(0.015)	(0.015)	(0.016)
Days_late_pos $_{i,t}$	0.026***	0.026***	0.019*
	(0.010)	(0.009)	(0.010)
Rev_shock $_{i,t}$	-0.155	-0.062	-0.045
	(0.125)	(0.078)	(0.064)
Rev_shock $_{i,t}$ x *No_carry* $_{i}$	0.22		
	(0.148)		
Rev_shock $_{i,t}$ x *Spending_limit* $_{i,t}$		0.167	
		(0.134)	
Rev_shock $_{i,t}$ x *Revenue_limit* $_{i,t}$			-0.043
			(0.160)
Exp_shock $_{i,t}$	0.339***	0.251***	0.011
	(0.122)	(0.089)	(0.078)
Exp_shock $_{i,t}$ x *No_carry* $_{i}$	-0.411**		
	(0.183)		
Exp_shock $_{i,t}$ x *Spending_limit* $_{i,t}$		-0.306	
		(0.198)	
Exp_shock $_{i,t}$ x *Revenue_limit* $_{i,t}$			0.332***
			(0.127)
Estimator	GMMSYS	GMMSYS	GMMSYS
Time dummies	Yes	Yes	Yes
Number of lags of dependent variable	4	4	4
Sample	1988-1997	1988-1997	1988-1997
Long-run impact factor	2.805	2.895	2.929
Number of states	36	36	36
Observations	266	266	266

*** p<0.01, ** p<0.05, * p<0.1

Robust standard errors in parentheses

Same explanatory variables as in Table 1 included in all estimations. See notes in Table 1 for more details.

Table A2. Variable definitions and sources

Variable	Description	Source
$Chubb_{i,t}$	Average of summer and winter observation of the "Chubb Relative Value Survey", given as the surveyed yield spread on 20-year general obligation bond relative to New Jersey	Lowry and Alt (2001) and Poterba and Reuben (2001)
$Days\_late_{i,t}$	Number of days from end of fiscal year to budget signed into law	Andersen, Lassen and Nielsen (2010a)
$Days\_delayed_{i,t}$	Number of days from legislative deadline to legislative budget passage	Andersen, Lassen and Nielsen (2010a)
$Late\_budget_{i,t}$	Dummy variable equal to 1 if budget was signed into law after end of fiscal year	Andersen, Lassen and Nielsen (2010a)
$Delayed\_budget_{i,t}$	Dummy variable equal to 1 if budget was passed by legislature after legislative deadline	Andersen, Lassen and Nielsen (2010a)
$Days\_late\_pos_{i,t}$	Equal to $days\_late_{i,t}$ x $late\_budget_{i,t}$	Andersen, Lassen and Nielsen (2010a)
$Days\_late\_neg_{i,t}$	Equal to $days\_late_{i,t}$ x (1-$late\_budget_{i,t}$)	Andersen, Lassen and Nielsen (2010a)
$Days\_delayed\_pos_{i,t}$	Equal to $days\_delayed_{i,t}$ x delayed_$budget_{i,t}$	Andersen, Lassen and Nielsen (2010a)
$Days\_delayed\_neg_{i,t}$	Equal to $days\_delayed_{i,t}$ x (1-delayed_$budget_{i,t}$)	Andersen, Lassen and Nielsen (2010a)
$Unempl\_change_{i,t}$	Change in unemployment rate since previous year	Bureau of Labor Statistics
$Gov\_spl_{i,t}$	General Government surplus relative to GDP	US Census Bureau
$Gov\_surplus_{i,t}$	*gov_spl* if *gov_spl* >0, and zero otherwise	US Census Bureau
$Gov\_deficit_{i,t}$	-*gov_spl* if *gov_spl* <0 and zero otherwise	US Census Bureau
$Debt_{i,t}$	Debt at the end of fiscal year scaled relative to GDP	US Census Bureau
$Moodys_{i,t}$	Moodys credit rating on 20-year GO bonds, ranging from 4 to 1, where Aaa=4, Aa=3, A=2, Baa=1	Alt and Lowry (2001)
$D\_Moodys_{i,t}$	$Moodys_{i,t}$ -$Moodys_{i,t-1}$	Alt and Lowry (2001)
$Divided\_gov_{i,t}$	Dummy variable equal to 1 if either *i)* both legislative chambers controlled by other party than governor's, or *ii)* two chambers controlled by different parties	http://www.ipsr.ku.edu/SPPQ/journal_datasets/klarner.shtml
$Elex_{i,t}$	Dummy variable equal to 1 in years with a gubernatorial election	Book of the States, various editions.
$Population_{i,t}$	State population (in millions of people)	U.S. Census Bureau
$GDP_{i,t}$	State GDP	Bureau of Economic Analysis
$Endbalance_{i,t}$	End-of-year balances in the general fund and stabilization fund, as projected in executive budget proposal. Measured in percent of proposed general fund expenditure	National Association of State Budget Officers: *The Fiscal Survey of States* , various editions
$Ideology\_gov_{i,t}$	Score of government (governor and two major party delegations in house and senate) ideology. Ranges from 0 to 100, with 0 being the most conservative value and 100 the most liberal position.	Berry et al (1998)
$Supermajority_{i}$	Dummy variable equal to 1 if a supermajority vote is required to pass each budget	National Conference of State Legislatures
No_carry_{i}	Dummy variable equal to 1 if the state law does not allow a budget deficit to be carried over to the next fiscal year	Bohn and Inman (1996)

Table A2. Variable definitions and sources (continued)

Revenue_limit $_{i,t}$	Dummy equal to one if a revenue limit is in place	Poterba and Rueben (2001)
Spending_limit $_{i,t}$	Dummy equal to one if a spending limit is in place	Poterba and Rueben (2001)
Exp_shock $_{i,t}$	Percentage deviation of actual general fund expenditure from original projections, net of the effect of within-year tax changes	Data provided by Kim Rueben. See Poterba and Rueben (2001)
Rev_shock $_{i,t}$	Percentage deviation of actual general fund revenue from original projections, net of the effect of within-year tax changes	Data provided by Kim Rueben. See Poterba and Rueben (2001)

Table A3. Late budgets data by state

	Information on late budget available for [1]	Responded to survey	Number of late budgets observed
Alabama	1989, 1991-1997	No	0
California	**1988-1997**	Yes	9
Connecticut	**1988-1997**	Yes	1
Delaware	1992-1997	No	5
Florida	**1988-1997**	Yes	1
Georgia	**1988-92**, *1993* , **1994-1997**	Yes	0
Illinois	**1988-1997**	Yes	7
Kentucky	1988-1997	No	0
Louisiana	1988-1997	No	7
Maine	**1991-1997**	Yes	1
Maryland	1991-1997	No	0
Massachusetts	**1988-1997**	Yes	8
Michigan	1988-89, 1991, 1995-1997	No	2
Minnesota	1988-1997	No	0
Mississippi	**1997**	Yes	0
Missouri	1988-1997	No	0
Nevada	1991-1997	No	1
New Hampshire	1991-1997	No	2
New Mexico	*1988-94* , **1995-1997**	Yes	0
New York	1988-1997	No	10
North Carolina	1988-1997	No	10
North Dakota	**1988-1997**	Yes	0
Ohio	1988-1997	No	3
Oklahoma	1993-1997	No	0
Oregon	1988-90, 1992-1997	No	3
Pennsylvania	**1988-1997**	Yes	4
Rhode Island	*1988-1997*	Yes	0
South Carolina	**1988-1997**	Yes	0
Tennessee	1995-1997	No	0
Texas	1988-1997	No	0
Utah	1989-1997	No	0
Vermont	1988-1997	No	0
Virginia	**1988-1997**	Yes	0
Washington	**1988-1997**	Yes	0
West Virginia	*1988-92* , **1993-1997**	Yes	0
Wisconsin	1988-1997	Yes	5

Notes:

(1) Normal font indicates that authors' own data collection is the only source of information. Italics indicate that the survey sent to state budget offices is the only source of information. Bold indicates that information is available from both sources.