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Abstract

Balanced budget requirements lead to substantial pro-cyclicality in state government spending outside of safety-net programs. At the beginnings of recessions, states tend to experience unexpected deficits. While all states ultimately pay these deficits down, differences in the stringency of their balanced budget requirements dictate the pace at which they adjust. States with strict rules enact large rescissions to their budgets during the years in which adverse shocks occur; states with weak rules make up the difference during the following years. We use this variation to identify the impact of mid-year budget cuts on state income and employment. Our baseline estimates imply i) a state-spending multiplier of 1.7 and ii) that avoiding $25,000 in mid-year cuts preserves one job. These cuts are associated with shifts in the timing of government expenditures rather than differences in total spending over the course of the business cycle. Consequently, our results are informative about the potential gains from smoothing the path of state government spending. They imply that states could reduce the amplitude of business-cycle fluctuations by 15% if they completely smoothed their capital spending and service provision outside of safety-net programs.

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1. Introduction

Recent economic conditions have generated renewed interest in Keynesian fiscal policy. Interest among economists has been driven in part by resurgence in government use of active fiscal policy, embodied most tangibly by the passage of the $787 billion American Reinvestment and Recovery Act (ARRA) in February of 2009. In broader context, however, the ARRA is only the most recent episode in a trend that includes the Economic Growth and Tax Relief Reconciliation Act of 2001, the Jobs and Growth Tax Relief Reconciliation Act of 2003, and the ARRA's most immediate predecessor, the Economic Stimulus Act of 2008.

State governments, whose spending accounts for about 10% of GDP, play a major role in the fiscal policy landscape. Since almost all US states have formal balanced budget requirements, a large share of state spending fluctuates pro-cyclically. When states enter recessions, their tax bases contract and their safety-net expenditures expand. Consequently, compliance with balanced budget requirements typically entails significant reductions in capital expenditures and in spending on services including education, corrections, and health.[2] We illustrate the substantial pro-cyclicality of these expenditures in Figures 1 and 2, which we constructed using flexibly de-trended data on personal income and the relevant categories of government spending.[3] Figure 1 plots the

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[2] In this context, safety-net programs primarily include Unemployment Insurance (UI), cash welfare assistance, and Medicaid. Spending out of insurance trusts, which includes state UI programs, is not subject to state balanced budget requirements. Other insurance trust spending is dominated by pension plans for public employees. Non safety-net spending primarily involves spending on education, transportation, health, law enforcement (including corrections), and relatively minor categories including spending on utilities and public parks.

[3] Specifically, we regressed both non safety-net spending and personal income on state-specific quartic trends. Altering the number of terms in the polynomial does not significantly change the results, although a relatively flexible polynomial seems clearly preferable to a simple linear trend given the variety of changes a state's economic trajectory can make over the course of five decades.

means of these de-trended series across states from 1960-2006, while Figure 2 displays each state-year observation for the two series in scatter plot form. The best-fit line shows that when personal income is $1 below trend, non safety-net spending tends to be 8.9 cents below trend (with a standard error of 1.4 cents). Notably, roughly $250 billion was initially allocated by the ARRA for state and local governments with the express intent that they would not have to reduce such spending to comply with their balanced budget requirements (Government Accountability Office, 2009).[4] Our empirical work investigates the effects of the budget cuts these dollars aimed to prevent.[5]

Prior empirical work on fiscal policy has typically involved Structural Vector Autoregressions (SVARs),[6] from which both inference and extrapolation can be difficult (see, e.g., the discussion in Auerbach and Gale, 2009). Difficulties with inference based on SVAR studies relate to their source of identification. Such studies rely on the completeness of model specification for identification, giving identification a different meaning than that typically intended in empirical microeconomic studies. An examination of the time series for aggregate government spending quickly reveals that SVAR estimates will be based almost exclusively on shocks to defense spending, with the unexpected component coming largely from the World War II and Korean War build-ups (Ramey, 2009). Such spending makes inference difficult for two reasons. First, wars

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[4] GAO (2009) notes that among the funds disbursed through September 2009, approximately 62% was intended as support for state's Medicaid programs while 27% was meant for education and training. Since these funds did not exceed the amounts states would otherwise have spent on these programs, however, the additional federal funds amount to general budgetary support.

[5] Importantly, however, the natural experiment we study involves a smoothing of state government expenditures over the business cycle rather than the net increase in total expenditures made possible by federal aid in the form of grants. Our results speak more directly to the potential effects of federal loans that must be repaid during better economic times.

[6] Recent examples include Cogan, Cwik, Taylor and Wieland (2009), Ramey (2008) and Mountford and Uhlig (2009).

can be associated with other important shifts in economic policy.[7] Second, the onset and resolution of wars may have significant impacts on expectations for future tax and income streams, both being important determinants of current consumption behavior.

The difficulties with extrapolation from SVAR studies also relate to their reliance on defense spending. There are important differences between defense spending and the kinds of spending typically considered for stimulus purposes. Unlike defense spending, spending on infrastructure, health, and education may directly affect the economy's production possibilities by adding to physical and human capital. Spending on programs like Medicaid and UI will involve incentive effects that influence labor supply and hence the level of output associated with full employment in the long run. Finally, some war-related spending goes to wages for soldiers overseas, making it difficult for such spending to generate multiplier effects through subsequent consumption of US goods and services. In contrast, the budget cuts we study involve programs that have been and will be directly affected by past and future fiscal stimulus packages.

Our methodology involves exploiting a plausibly exogenous source of state-level variation for identification. Our strategy draws upon and extends previous work (in particular Poterba, 1994) on the effect of states' balanced budget requirements on their fiscal behavior. States have varying degrees of stringency built into the rules which govern the debt finance of general fund expenditures. During times of fiscal stress, states with relatively strict rules enact relatively large rescissions to their budgets in order to quickly narrow emerging budget deficits. Conditioning upon the size of the fiscal shock,

[7] During World War II, for example, the US economy underwent the imposition of rationing and price controls.

we use the spending cuts made by strict-rule states (in excess of those made by weak-rule states) to identify the effects of these budget cuts on state economic outcomes.

Having voiced concerns with the methods employed in past work, we would be remiss to not highlight problems with our own. First, as is common in much of the fiscal policy literature, precision is relatively low. We present our best reading of the available data and show that our results are robust to basic specification checks. We acknowledge, however, that our setting is one in which results change non-trivially with sufficient adjustment to the sample and specification. Second, we cannot fully rule out a potentially important threat to our identification strategy. Specifically, states with weak balanced budget rules rely somewhat more extensively than others on personal income taxation as a source of revenue. This has implications for our measure of deficit shocks which, for reasons elaborated below, could upwardly bias our estimates. These limitations suggest that further methodological innovations are needed to consolidate our knowledge of state governments' effects on the business cycle.

To summarize our results, we estimate i) a state-spending multiplier of around 1.7 and ii) that avoiding around $25,000 in cuts yields one job. A breakdown of the employment result between private and public establishments suggests that almost all of the jobs appear in the private sector, with public sector workers gaining moderately in the form of higher wages and salaries. Since a significant portion of state fiscal adjustment comes through deferred capital projects, much of this private employment may involve government contractors. The estimate of the state-spending multiplier is not very precise (e.g., a standard error of 0.93), while the estimate of the dollars-per-job figure is somewhat more so (e.g., a standard error of around $11,000). The potential upward bias noted above, along with evidence from robustness checks, suggests that our best estimate of the multiplier might be moderately below 1.7.

Our results do not apply to pure increases in government spending, but rather to shifts in its timing. Both weak- and strict-rule states ultimately adjust their budgets during downturns to pay off the unexpected deficits associated with fiscal crises. Our estimates pick up the relatively fast declines in personal income and employment associated with the relatively fast adjustments made by the states with strict budget rules. The results have implications for the extent to which business-cycle fluctuations could be lessened by smoothing the path of state government expenditures. We arrive at the relevant estimate by coupling our multiplier estimate with our estimate that spending tends to be 8.9 cents below trend when personal income is $1 below trend. The product of these estimates implies that the amplitude of state business cycles could be reduced by about 15% (1.7 x 0.089) if states completely smoothed their non safety-net expenditures.

Our employment evidence suggests a much lower dollars-per-job figure than that associated with general discussions of stimulus spending. This is at least in part due to the kind of spending we study. We study the effects of rescissions to pre-existing programs. Stimulus spending, on the other hand, often involves projects that have not begun or been fully planned. Additionally, the recent federal stimulus includes significant support for state Unemployment Insurance (UI) and Medicaid programs. These programs play important roles with respect to distributional and safety-net concerns, but may actually reduce employment on net due to their incentive effects. Further considerations highlight that dollars-per-job may serve better as a political metric than welfare metric. Job-creation programs significantly benefit those who obtain new jobs, while providing no benefit for those who remain unemployed. Conversely, safety-net spending should tend to diffuse across those hit hardest by a downturn.

The paper proceeds as follows. Section 2 illustrates our strategy for utilizing state-level variation to identify the impact of mid-year budget cuts on economic outcomes. Section 3 provides additional background regarding state balanced budget rules and our measure of deficit shocks. Section 4 presents the econometric model and describes the relevant data. Section 5 presents results and section 6 concludes.

2. An illustration of the estimation strategy

The following example illustrates our identification strategy. 1991 was a year of severe fiscal stress for the states. By our measure of fiscal shocks (adopted from Poterba, 1994, and discussed in the next section), Michigan and Mississippi experienced similar surprise deficits on the order of $150 per capita. Mississippi has a statutory requirement which prevents the government from carrying a general fund deficit through the end of a fiscal year (unless it takes actions to override the statute), making it a "strong-rule state" by our classification system. It cut spending aggressively in response to this shock, enacting rescissions amounting to $46 per capita.[8] Michigan, on the other hand, only requires unexpected deficits to be closed in the budget for the following fiscal year. This requirement has no bite in terms of the state's behavior in the year during which the shock occurs, making Michigan a "weak-rule state" by our classification system. As one would expect, Michigan cut spending less aggressively than Mississippi, enacting rescissions which amounted to $15 per capita.

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[8] Mississippi also reported taking actions including a hiring freeze, a travel freeze, layoffs, furloughs, and dipping into its rainy day fund during the recession (NASBO, *Fiscal Survey of the* States*: October 1991*).

When we partial out fixed effects and trends (upon which we condition in our regression analysis), we find that Mississippi had an income gap of \$54 per person, while Michigan was \$2 per person above its trend. Dividing this difference by the difference in their fiscal policies yields a multiplier estimate of $56/31 \approx 1.8$, similar to the results of our preferred specification. We construct similarly motivated estimates for the impact of fiscal policy on state employment.

The calculation in this example was simplified by the fact that Michigan and Mississippi had similarly sized fiscal shocks in 1991. In general this is not the case, making it essential for us to control for the main effect of the fiscal shock. One can at this point get a sense for the key identification assumption behind our estimation strategy. It is essential that a measured deficit shock in a weak-rule state carries the same economic content with respect to income and employment as a measured deficit shock in a strong-rule state. If a measured shock of \$150 per capita would (in the absence of state fiscal policy) be associated with a lower level of personal income (relative to trend) in a strong-rule state than in a weak-rule state, then our estimate of the multiplier would be biased upward. We would attribute too much of the strong-rule state's low level of personal income to its relatively aggressive budget rescissions.

The assumption that measured deficit shocks have the same economic content in weak- and strong-rule states corresponds to the exclusion restriction for our instrumental variables (IV) estimation strategy. We cannot explicitly test this restriction. In the text below, we consider potential sources of concern. Note that it does not matter if weak-rule states differ from strong-rule states along some fixed (or, for that matter, linearly trending) dimension. Since we have panel variation in our instrument, we always control for state fixed effects and trends. We typically make these controls specific to the episodes of fiscal stress in our dataset (i.e., the episodes surrounding the early 1990s and early 2000s recessions). These controls are important for the purposes of achieving both identification and precision. With their inclusion, differences between strong- and weak-rule states will only bias our estimates if they impact the economic content of our measure of deficit shocks.

3. Budget rules and deficit shocks

Budget rules

State balanced budget requirements play a central role in our identification strategy. Following the literature, we collect information on these requirements from a 1987 report by the Advisory Commission on Intergovernmental Relations (ACIR) and from various reports by the National Association of State Budget Officers (NASBO).

All states save Vermont have a formal balanced budget rule of one form or another. These rules vary substantially across states in terms of their stringency. For instance, some states require the governor to submit a balanced budget, the legislature to pass a balanced budget, or the governor to sign a balanced budget. This first set of rules applies solely to the enactment of the budget. We would not expect these rules to impact states' responses to deficits that emerge over the course of the fiscal year. The strictest rule (also known as the "no-carry" rule) prohibits carrying deficits through the next budget cycle altogether. This rule applies directly to the execution of the budget and would be expected to exert influence on states' behavior during the year in which the deficit emerges.

Rules can also differ in terms of how difficult it is for the government to override them. When the rules appear in statutes, they can be overridden by a simple majority vote. When they appear in the state constitution, on the other hand, a supermajority or statewide referendum may be required. This adds an additional dimension along which rules can be regarded as weak or strong.

Past studies of fiscal institutions have explored some of the consequences of these rules. Highlights include studies by Poterba (1997) and Bohn and Inman (1996), who examine the impact of different requirements on a broad range of budgetary outcomes, as well as Poterba and Reuben (2001) and Lowry and Alt (2001), whose work addresses the nexus between balanced budget requirements, state fiscal behavior, and interest rates on general-obligation debt. These studies find that requirements which apply to the budget's execution have greater impact than those that apply only to the budget's enactment. In particular, a rule requiring states to pay off unexpected deficits within the next fiscal year (the "no-carry" rule) drives relatively fast returns to fiscal balance following adverse shocks.

This paper links most directly to work by Poterba (1994). Using data available in semi-annual reports by the National Association of State Budget Officers (NASBO), Poterba constructs a measure of fiscal shocks which a) is driven by differences between budget forecasts and realizations and b) accounts for the budget adjustments made by states over the course of the fiscal year. He then shows that states take significant mid-year actions, in the form of spending rescissions and tax increases, to close unexpected budget deficits. States with relatively strict balanced budget requirements (primarily those with the No-Carry rule) enact significantly larger budget cuts than those without per dollar of fiscal shock.

There is enough variation in the effects of strong and weak budget rules to generate a reasonably powerful first stage. Using a standard approach from this literature, we divide states into weak-and strong-rule states (or weak-, medium-, and strong-rule states) using a stringency index provided by the ACIR (1987). Following Poterba (1994), our baseline regressions use the ACIR index to divide states into 2 categories of stringency, namely weak and strong. While Poterba designated states with scores greater than 5 as "strong-rule" states, we use a cutoff of 7 since there are relatively few states with scores less than or equal to 5. Our results are not sensitive either to this cut-off or to the division of states into strong, medium, and weak designations. We save a more detailed discussion of the breakdown of states across categories for our presentation of summary statistics in Section 4.[9]

Deficit shocks

Again following Poterba (1994), we quantify fiscal crises by the difference between forecasted and actual budgets:

$$Expenditure\ Shock_t = Outlay_{CL,t} - \mathbf{E}_{t-1}(Outlays_t)$$

[9] In addition to the ACIR and NASBO classifications of budget rules, a classification can also be found in a 1993 report by GAO. Differences between these classification systems are the subject of an exchange between Levinson (1998, 2007) and Krol and Svorny (2006). Our examination of the GAO report raises the possibility that four of our weak-rule states could be reclassified as strong-rule states. However, in the relevant table of the GAO report, each of these states are linked to a footnote which says "Although these states require year-end budget balance, carryover and/or borrowing to finance a deficit are allowed if necessary." The note implies that the requirement does not bind these states in any serious way, making us comfortable maintaining their "weak rule" classification. There are two additional states which are classified as weak-rule states by GAO and as strong-rule states by ACIR. Changing the classification of these two states (Iowa and Delaware) has little impact on our baseline results.

$$Revenue\ Shock_t = Revenue_{CL,t} - \mathbf{E}_{t\text{-}1}(Revenues_t).$$

The terms involving expectations are outlay and revenue forecasts, where the forecast is made at the end of the previous fiscal year. $Outlay_{CL,t}$ and $Revenue_{CL,t}$ are the *constant-law* levels of outlays and revenues: what would have prevailed in the absence of mid-year adjustments to the budget. The difference between these terms provides a true measure of expenditure and revenue shocks. We cannot directly observe constant-law outlays and revenues. However, we can recover them by subtracting mid-year changes (denoted as $\Delta Outlays_t$ and $\Delta Revenue_t$) from the final outlay and revenue outcomes for the fiscal year ($Outlays_t$ and $Revenue_t$). We then combine the revenue and expenditure shocks to form:

$$Deficit\ Shock_t = Expenditure\ Shock_t - Revenue\ Shock_t.$$

NASBO reports all the information required to construct these shocks in its semi-annual *Fiscal Survey of the States* series.

The validity of our research design depends in large part on the economic content of our measure of deficit shocks. Concerns may stem from the fact that measured deficit shocks result from forecasting errors. This raises two possible interpretations of the measured shocks. The first interpretation is that deficit shocks result from worse-than-expected economic environments in which expenditures on programs like Medicaid are larger, and the tax base smaller, than anticipated. In this scenario we would expect mid-year budget cuts to represent real cuts in spending.

The second interpretation is that measured deficit shocks result from bad forecasting. Forecasts might be skewed intentionally for political purposes, or just hastily done. In this scenario, our measured cuts might be more imaginary than real since they would be relative to an unrealistic baseline. This could lead us to identify the effects of accounting gimmicks (or errors) rather than actual reductions in spending.

Several pieces of evidence make it reasonable to accept the first interpretation. First, the mean deficit shock over the 1988-2004 period is $0.07 per capita, with a standard deviation of $91.70, suggesting these are truly mean-zero shocks. Second, as we demonstrate in a moment, positive deficit shocks are associated with downturns in personal income relative to its trend. Third, using more comprehensive budget data from the Census of Governments,[10] we have confirmed that a dollar of rescissions isolated by our instruments corresponds to a dollar less in state spending outside of insurance trusts and public welfare programs. Fourth, our baseline estimates change little when we replace the reported revenue and expenditure forecasts with simple econometric forecasts of our own.[11]

On the second point, consider the evidence in Figure 3 and Table 1. Figure 3 graphs national means (across the states) of deficit shocks and de-trended personal income per capita. The figure suggests that deficit shocks become large when an economy enters a recession; when de-trended personal income turns sharply downward, large, positive deficit shocks appear. We show this statistically in Table 1, which shows

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[10] The forecasts and budget outcomes in NASBO's series *Fiscal Survey of States* only account for spending out of states' general funds. This generally excludes spending out of revenue sources that are earmarked for specific purposes (as tends to be the case with charges and fees, a category including revenue sources like tolls and public university tuition). The Census of Governments' annual survey of state government finances provides a more comprehensive look at spending across categories. It does not, however, distinguish between earmarked revenue sources and sources destined to the general fund, making it impossible to reconstruct estimates of general fund totals from the variables provided.

[11] The forecasting element may also raise confusion regarding the persistence of the shocks. Since state forecasts incorporate new information about the state of the economy, deficit shocks tend not to be persistent. We have also found no significant difference in the persistence of deficit shocks in weak-rule states relative to strong-rule states.

results from regressions in which we use deficit shocks to predict changes in personal income.[12] We split the measure of deficit shocks into separate variables for its positive and negative values. This is standard throughout the paper (as well as in the previous literature) because balanced budget requirements only have binding implications for fiscal behavior when deficit shocks are positive. The results show that large, positive deficit shocks are associated with large downturns in personal income per capita across a range of alternative controls for fixed effects and time effects. It is less clear what negative deficit shocks imply about the state of the economy.

4. Empirical Strategy

Specifications

The following first and second stage regression equations summarize our identification strategy. The strategy involves using the interaction of the deficit shock with an indicator for weak budget rules as an excluded instrument. We run the following regressions:

1st Stage:[13]

$$\begin{aligned}\Delta Outlays_{s,t} &= \beta_0 + \beta_1 * weakBBR_s Defshock_{s,t} * 1_{\{Defshock_{s,t}>0\}} \\ &+ \beta_2 * weakBBR_s Defshock_{s,t} * 1_{\{Defshock_{s,t}\leq 0\}} + \beta_3 Defshock_{s,t} * 1_{\{Defshock_{s,t}>0\}} \\ &+ \beta_4 Defshock_{s,t} * 1_{\{Defshock_{s,t}\leq 0\}} + \delta_s + \delta_t + trend_t * \delta_s\end{aligned}$$

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[12] We use the same sample in these regressions as in the later regressions in the paper. We explain the sample selection process in the following section.

[13] Results from Poterba (1994), who uses data from 1988-1992, motivate our first stage regressions. Poterba clarifies an important point regarding what might look like a simultaneity problem due to the appearance of $\Delta Outlays_{s,t}$ in the construction of the deficit shock (1994, pp. 809-810). In fact, a true simultaneity problem would result from failing to subtract $\Delta Outlays_{s,t}$. As Poterba notes, if one did not subtract $\Delta Outlays_{s,t}$, the resulting measure of the shock would equal the true measure of the shock plus $\Delta Outlays_{s,t}$. Hence regressing $\Delta Outlays_{s,t}$ on this incorrect measure would amount to regressing it on itself plus a random variable. Subtracting $\Delta Outlays_{s,t}$ yields an estimate of the true shock and eliminates the simultaneity problem.

2nd Stage: $$Y_{s,t} = \gamma_0 + \gamma_1 \Delta \widehat{Outlays}_{s,t} + \gamma_2 Defshock_{s,t} * 1_{\{Defshock_{s,t} > 0\}}$$
$$+ \gamma_3 Defshock_{s,t} * 1_{\{Defshock_{s,t} \leq 0\}} + \alpha_s + \alpha_t + trend_t * \alpha_s + \varepsilon_{s,t}.$$

In these equations, $Y_{s,t}$ is a state-level economic outcome for state s during fiscal year t, $\Delta Outlays_{s,t}$ is the within-fiscal-year spending adjustment, $weakBBR_s$ is an indicator equal to one if a state has weak balanced budget rules, and $Defshock_{s,t}$ is the measure of deficit shocks discussed above. As noted earlier, we follow the convention of including distinct variables for positive and negative valued deficit shocks. Since budget rules are only binding when deficit shocks are positive, failing to distinguish between positive and negative deficit shocks would involve a misspecification of the model. The α and δ terms represent state and year fixed effects.

In practice, most specifications will employ state fixed effects and trends that are business-cycle specific. Each business-cycle downturn constitutes a distinct episode of fiscal stress. We view the data set as consisting of two such episodes for each state, the first running from 1988-1994 and the second from 2001-2004. We specify the trend term so that it starts at 1 at the beginning of the relevant cycle. Since the $\Delta Outlays$ variable tends to be small relative to the size and variance of state level income and employment, we need to remove as much variation from Y as we can in order to obtain statistical power. Cycle-specific fixed effects and trends help us remove noise from Y. We discuss our result's sensitivity to adjustments in this specification in Section 5.

A caveat to note is that there may be error in the NASBO measure of budget cuts. If the reported cuts represent only a portion of the real cuts, our results will be biased upward, as they will also capture the effects of these unmeasured cuts. While this is a plausible case of measurement error which should be kept in consideration, we have no evidence suggesting that it is pervasive problem.

Identification

Ignoring for a moment that we include positive and negative deficit shocks as separate terms, we can write the exclusion (or orthogonality) restriction as follows:

$$E(\text{weakBBR}_s * \text{Defshock}_{s,t} * \varepsilon_{s,t}) = 0.$$

Noting that $weakBBR_s$ is binary, we can re-write this condition in two pieces:

$$(1 - p_{\text{weakBBR}}) * E(\text{weakBBR}_s * \text{Defshock}_{s,t} * \varepsilon_{s,t} \mid \text{weakBBR}_s = 0) \\ + p_{weakBBR} * E(\text{weakBBR}_s * \text{Defshock}_{s,t} * \varepsilon_{s,t} \mid \text{weakBBR}_s = 1) = 0,$$

where $p_{weakBBR}$ is the probability that a state has weak budget rules. The first piece of this expression automatically equals 0, however, since it is the piece for which $weakBBR_s$ always equals 0. Hence we are left with

$$p_{\text{weakBBR}} * E(\text{weakBBR}_s * \text{Defshock}_{s,t} * \varepsilon_{s,t} \mid \text{weakBBR}_s = 1) = 0$$

as our exclusion restriction, with $weakBBR_s$ always equal to one. Now note that since we include the main effect of the deficit shock in our regressions, it follows from the properties of ordinary least squares that $E(\text{Defshock}_{s,t} * \varepsilon_{s,t}) = 0.$ Consequently, we have that if $E(\text{Defshock}_{s,t} * \varepsilon_{s,t}) = E(\text{Defshock}_{s,t} * \varepsilon_{s,t} \mid \text{weakBBR}_s = 1),$ the exclusion restriction is satisfied. In words, the restriction is satisfied if the unconditional expectation of the deficit shock times the second stage error equals that same expectation conditional on a state having weak budget rules. We draw the interpretation that the exclusion restriction requires deficit shocks to have similar economic content in weak- and strong-rule states.

The primary threat to this condition stems from differences in the revenue bases utilized across states. States with weak budget rules make greater use of personal income taxes than states with strict budget rules. Personal income taxes tend to be more volatile than other revenue sources. Consequently, an economic shock of a given size may, all else equal, result in a relatively large deficit shock in the weak-rule states. If true, this would upwardly bias our multiplier estimates. Conditional on their deficit shocks, weak-rule states would have better performing economies than strong-rule states for reasons unrelated to their budget cuts. We return to this point when presenting summary statistics on state government budgets and economies.

Sample Inclusion Criteria

We implement our regressions on the sample of states used by Poterba (1994). The sample excludes biennially-budgeting states for two reasons. First, the implications of budget rules for the timing of fiscal adjustments are less clear in biennial states than in annual states. Second, for biennial states the NASBO reports leave uncertainty regarding the years in which forecasts were made and in which budget cuts were implemented. Inclusion of the biennially-budgeting states reduces first stage precision, raising statistical concerns regarding the extent to which our instruments satisfy the relevance criterion.[14] The sample also excludes Alaska, due to the uniquely prominent role of oil revenues in its finances, and Massachusetts, which engaged in budgetary shenanigans such that its

[14] When the biennial states are included in our baseline specification, the *F*-statistic on the joint test for significance of the excluded instruments is 5.17, which raises serious concerns about bias in both the coefficient and standard error estimates. Second stage results are in the same ballpark as the estimates in our preferred specification but have larger standard errors, particularly in the employment regression. GAO (1993) provides a classification of states that breaks biennial budgeting states into those that have annual and biennial *legislative* cycles. Adding the biennial budgeting states with annual legislative cycles to our sample does not substantially change our results. We report these results later while discussing robustness checks.

definition of mid-year rescissions differs from that of other states.[15] This leaves a sample of 27 annually budgeting states.

Data and Summary statistics

As noted earlier, we generate our measure of budget rules using a 1 to 10 index produced by the ACIR (1987). Using a cutoff of 7, there are 14 states with weak rules and 36 states with strong rules. Our sample of 27 annually budgeting states contains 8 states with weak rules, and 19 with strong rules. We also present results with the rules divided into 3 categories, with 19 states having strong rules, 4 medium, and 4 weak. Table 2 provides a breakdown of the states in each classification.[16]

Table 3 presents summary statistics for various demographic, economic, and fiscal characteristics across states. We constructed the demographic characteristics by tabulating individual level data from the Integrated Public Use Microdata Series (IPUMS) database for the Current Population Survey (CPS). We constructed the personal income variable using data from the Bureau of Economic Analysis (BEA) and the employment variables using data from the Bureau of Labor Statistics (BLS). Finally, fiscal characteristics come from semi-annual reports by NASBO and from the Census of Government's *Annual Survey of State and Local Government Finances*.

Note that we estimate the impact of fiscal policy on personal income rather than GSP (the state equivalent of GDP). Most states begin their fiscal years in July. This

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[15] This is quite explicit in the note for the Massachusetts entry in the 1988 NASBO report, raising concerns about the Massachusetts series for the length of the sample.
[16] Later, in Table 13, we present results where we include states that have biennial budget cycles but annual legislative cycles, changing the pool of weak- vs. strong-rule states; the results are very similar to our baseline specification.

makes it necessary to use variables that are reported at either a monthly or quarterly frequency. The BEA's state-level personal income series fits this description, while its GSP series does not.[17]

Our sample runs from 1988-2004. NASBO reports did not include the mid-year adjustments needed to construct deficit shocks until 1988. We view the sample as consisting of two periods of fiscal stress, namely 1988-1994 and 2001-2004. In general we omit 1995-2000 because this is a boom period, resulting in few positive deficit shocks and few mid-year budget cuts. Observations from this period thus couple minimal usable variation in our fiscal variable with substantial variation in our outcome variables.[18] We present robustness checks showing that our point estimates change little when we include these years, but that our standard errors become larger.

The data in Table 3 are broken down across annually- and biennially-budgeting states. Since the summary statistics look broadly similar across the two groups, we do not worry that our focus on annually-budgeting states limits the applicability of our results. In our sample, per capita general fund spending by state governments was on average around $1,680 in annually-budgeting states.

Recall Figures 1 and 2, which illustrate the relationship between de-trended personal income and de-trended state spending outside of safety net programs and

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[17] Those interested in a detailed look at the differences between BEA's personal income and GDP measures should see Table 1.7.5 from the National Income and Product Accounts. The main differences relate to cross-border income payments and capital depreciation.

[18] This choice is also driven by the fact that our fiscal variable (the reported mid-year budget cuts) is effectively right-censored. That is, NASBO does not report mid-year budget increases. Reuben (1997?) investigated this issue and found it to have no impact on analyses of the deficit shocks associated with the 1992 recession.

insurance trusts. The pro-cyclicality of such spending is clear. These figures cover the relatively long time horizon available using data from the Census of Governments. Figure 4 focuses on the data available through NASBO over the time period for which we can construct the required measure of deficit shocks. In this series as well, the pro-cyclicality of state government spending remains starkly apparent.[19] Figure 5 shows that mid-year spending cuts tend to occur at the start of recessions; just as personal income begins to fall off, spending cuts rise. Spending cuts in the worst hit states during the worst years are about 4% of general fund spending, or about $200 per capita.

Table 4 compares summary statistics across weak- and strong-rules states. Again, the demographic characteristics look broadly similar. The exception is that the weak-rule states tend to be larger, and wealthier. As discussed above, cross-state differences are not an identification concern in and of themselves because we control flexibly for state fixed effects and trends. We also experiment with including an interaction between income and deficit shocks as a control variable; it has a modest impact on our results.

Two notable differences arise in comparing fiscal variables across states, one of which threatens our identification strategy while the other does not. State government budgets are larger in weak-rule states than in strong-rule states. This may appear to threaten our identification strategy. Expressed as shares of state income, however, the governments in weak- and strong-rule states are of the same size. All else equal, economic shocks of a given size will thus result in similarly sized deficit shocks.

[19] More specifically, the figures plot the residuals of income and general fund expenditures from regressions which include state fixed effects and quadratic state trends.

Differences in state revenue bases raise a genuine concern, as revenue bases with different elasticities (with respect to income) can lead a given economic shock to result in deficit shocks that differ across states. Weak-rule states make moderately, but systematically, greater use of taxation than strong-rule states. Specifically, taxation accounts for 51 percent of general revenues in weak-rule states and 45 percent in strict-rule states, with personal income tax revenue accounting for almost the entire difference. Strict-rule states make up this difference through a combination of charges, fees, intergovernmental transfers, and other miscellaneous revenues. This raises concern because personal income taxes tend to be more volatile than other revenue sources.

A variety of issues complicate direct estimation of differences in the volatility of revenue sources across states. One set of complications arises from the mid-year adjustments of rates and fees made by states to counteract shocks as they occur. These changes must be netted out in order to obtain a true estimate of a tax base's volatility. Another is driven by the fact that personal income taxes themselves can vary significantly across both time and space in key features like the extent of progressivity. States that utilize relatively volatile tax bases may compensate by designing the relevant taxes to reduce the volatility of collections. They may also make their non safety-net spending programs more volatile so that revenues and expenditures adjust together with the state of the economy, yielding no net difference in the implied deficit shocks.

Table 5 presents estimates of the volatility of revenues and expenditures with a breakdown between weak- and strong-rule states. The coefficients show the relationship between a change in personal income and changes in revenues and expenditures. These changes are expressed in logs in Panel A and in levels in Panel B. The measures of general fund revenues and expenditures have been adjusted to net out states' mid-year tax and expenditure changes. As one might expect, given their greater use of personal income taxes, revenue appears to be more volatile in weak-rule states. However, expenditures are also relatively volatile in weak-rule states, implying that the difference in the volatility of deficits is less than the difference in the volatility of revenues. Expressed in logs, the difference continues to imply greater volatility in weak-rule states. Expressed in levels, however, the difference nets out to exactly zero.

The pro-cyclicality of expenditures is driven in part by balanced budget requirements. To the extent that revenue changes are anticipated, planned expenditures must be adjusted in order for a state to submit a balanced budget. All else equal, however, this would lead one to expect greater volatility in the expenditures of states with strict budget rules. As a test for differences in the volatility of state budgets, the evidence in Table 5 is not clean. This is because states' mid-year actions, driven by differences in their balanced budget requirements, are a determinant of personal income. The absence of a clean test relates to the fact that one cannot, in general, directly test the exclusion restriction associated with an instrumental variables framework. We cannot rule out the possibility that our estimates biased upward by weak-rule states' choice of relatively volatile tax bases, but the volatility of their expenditures appears to mitigate this concern.

5. Results

First stage and baseline

Table 6 presents our first stage results. The specifications incorporate year effects, cycle-specific state effects, and cycle-specific state trends. Columns marked

"Poterba sample" only use data from 1988-1992. The "extended sample" includes 1988-1994, and 2001-2004. The results show that Poterba's findings are not sensitive to extending the sample. They show that for each dollar of deficit shock, strong-rule states tend to enact around \$0.47 in mid-year budget cuts. States without strong rules tend to cut between \$0.33 and \$0.40 less than strong-rule states (depending on the sample and the classification of rules used in the regression), for net cuts ranging from \$0.07 to \$0.14 per dollar of deficit shock. Results in Appendix Table A1 show that, as also found by Poterba (1994), budget rules are unrelated to state responses to deficit shocks along the tax margin.[20]

The first stage partial *F*-statistic on the instruments is 10.89 using the weak/strong classification, and 12.01 using the weak/medium/strong classification. Our instruments thus satisfy the Staiger and Stock (1997) rule of thumb calling for an *F*-statistic greater than 10, and there is little concern that our two-stage least squares (2SLS) coefficient estimates will be significantly biased. Nonetheless, Stock and Yogo (2004) present evidence suggesting that 2SLS estimates of standard errors can suffer from significant downward bias even in the presence of moderately strong instruments, potentially leading to flawed statistical inference. Their results show that standard error estimates using limited information maximum likelihood (LIML) are much less sensitive to such

[20] Table A1 presents results similar to those in the first columns of Table 4 from Poterba's 1994 paper, which do not use fixed effects, time effects, or trends. The results using the 1988-1992 sample come quite close to replicating Poterba's work even with our adjustment to the threshold for distinguishing between strong- and weak-rule states. Poterba describes the construction of the ΔTAX and ΔTAXNEXT variables, which capture state response to fiscal crises on the revenue side of the ledger in the current and next fiscal year. For our purposes, what is important to note is that while our instruments *do* explain a significant amount of the variation in outlays, they *do not* explain variation in tax revenues; the difference between the responses of weak- and strong-rule states to fiscal shocks occurs in terms of spending, not taxes. Consequently we do not worry that tax changes correlated with the outlay adjustments picked up by our instrument are a source of bias in our results. In our IV specifications, conditioning on these tax changes does not affect the results.

problems.[21] Consequently we present our preferred specifications using both 2SLS and LIML to demonstrate that the LIML standard error estimates differ little from the 2SLS standard error estimates.

Table 7 presents results which provide a sense for how the mid-year cuts driven by strong budget rules play out in state budgets. The expenditure results confirm that a when weak-rule states cut a dollar less in spending (per dollar of deficit shock), they spend about one dollar more than strong-rule states across the budget. As would be expected, this number increases moderately (to about 1.2) if public welfare expenditures are excluded (result not shown). In the following year, weak- and strong-rule states have roughly equalized their expenditures, and two years out the weak-rule states make up their deficits by spending somewhat more than one dollar less than strong-rule states. The revenue results suggest that there is no separation between weak- and strong-rule states in the year of the shock, that weak-rule states collect about 70 cents less in revenues in the year after the shock, and that collections are again comparable two years out. Over the course of the three years, the implied difference in deficits nets out to almost exactly zero. While imprecision is a problem throughout, the point estimates match quite closely with received wisdom regarding the effects of weak and strong budget rules. Weak-rule states bring their spending in line with strong-rule states in the year following the shock and they make up their deficits shortly thereafter. The results support the interpretation that the estimated effects of mid-year budget cuts will reflect

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[21] See Tables 1, 2, and 4 from Stock and Yogo (2004). Table 2 implies that in cases involving 1 endogenous regressor and 2 or 4 instruments (corresponding, respectively, to our specifications involving the weak/strong and weak/medium/strong classifications) our F-statistics leave open the possibility of substantial downward bias in our standard error estimates when using 2SLS. Table 4, on the other hand, implies that we have little reason to worry about downwardly biased standard errors when using LIML.

the effects of shifts in the timing of expenditures rather than an increase in total expenditures over the course of the business cycle.

Table 8 presents our baseline specifications. Our baseline multiplier estimates on personal income are around 1.7 whether we instrument using the weak/medium/strong classification or the weak/strong classification. We invert the coefficient on employment to produce an estimate of dollars per job "preserved" or "created." Using both budget rule classification systems, the implied cost is about $25,000. The multiplier estimates are not very precise. This lack of precision is due to the fact that variation in income is large relative to the variation in Δ*Outlays*. The employment estimates are somewhat more precise, presumably because there is less volatility in employment than in income. For the dollars-per-job metric, we use the delta method to obtain a standard error slightly larger than $11,000 and a 95% confidence interval bounded by $3,100 to $46,400. Comparing the results in Panel A to the results in Panel B, one can see that neither the coefficient estimates nor the standard errors change substantially when we use LIML rather than 2SLS.

In Table 9 we attempt to decompose the employment and income results across the public and private sectors. We first estimate the impact of mid-year budget cuts on government employment and government wages and salaries. Then we estimate impacts on aggregate employment net of government employment and aggregative income net of government wages and salaries.[22] We do this using two sources of information on

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[22] Personal income and wages and salaries are not directly comparable, although wages and salaries is the largest component of personal income. Similar results could be obtained by showing a breakdown of total wages and salaries into its private and public components. We show results for personal income net of

government employment and wages. These include a) the Quarterly Census of Employment and Wages' (QCEW) breakdown of firms by ownership, which includes private and various levels of government, and b) the Census Bureau's annual report of the employment and wages of state government employees.

The results from both sources suggest that little of our observed employment effect comes through government employment (see columns 2 and 3 of Panel A). Mid-year budget cuts do appear to come in the form of lower wages for government employees. However, our sources disagree on the extent to which this is the case. The results in columns 2 and 3 of Panel B imply that for each extra dollar that a state government spends (or dollar of rescissions that it avoids), it will tend to spend either $0.14 (QCEW) or $0.46 (Census of Governments) on wages and salaries for government employees. The results are not precisely estimated in either case. Taken at face value, they imply that most of these dollars are spent in the form of capital expenditures and other purchases of services and equipment which flow through privately owned firms.

Robustness checks

We have run a variety of specification checks to explore the sensitivity of our results. We discuss two sets of regressions for which we also present tables, after which we provide a qualitative assessment of our additional checks. The results presented explicitly involve relaxation of our sample inclusion criteria.

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government wages and salaries because personal income (as opposed to wages and salaries) is the variable of interest in our main specifications.

Table 11 presents results in which we significantly expand the sample of states. Using information provided by GAO (1993), we can divide the set of biennial budgeting states into those with annual and biennial legislative cycles. Though they are excluded in Poterba's work on state responses to deficit shocks, as well as from our main results, the states with annual legislative cycles are plausibly comparable to those with annual budget cycles (both in terms of their behavior and their data reporting). The results in Table 11 involve re-running the specifications from Panel A of Table 7 with the inclusion of this additional set of states. Among this set of states, Wyoming and Vermont may be viewed as outliers: Wyoming due to its reliance on natural gas and intergovernmental revenues and Vermont due to its standing as the only state without a formal balanced budget requirement of any kind.[23] Consequently, we report two sets of results. In Panel A we report results excluding Wyoming and Vermont from the set of additional states. In Panel B we report results with their inclusion. Both the employment and personal income results in Panel A are quite similar to our baseline results (the personal income multiplier is around 1.8 and the employment coefficients are 0.000038 and 0.000039). Adding Wyoming and Vermont to the sample has almost no effect on the personal income results, but pushes the employment coefficient down to 0.000025, implying that around $40,000 in rescissions must be avoided to generate a job.

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[23] While a typical U.S. state relies on intergovernmental revenues for about 25% of total revenues and on traditional tax bases (i.e., sales, individual income, and corporate income) for close to 2/3 of its total revenues, Wyoming relies on intergovernmental revenues for nearly 40% of total revenues and on traditional tax bases for only 11% of total revenues. It relies much more heavily on "other taxes," charges, and miscellaneous revenues, with these accounting for nearly 30% of total revenues relative to 15% in the average state. Wyoming's state government is also much larger than the typical state government, with total revenues amounting to $10,200 per capita in 2004 relative to $5,400 for the average state.

In Table 12 we bring the 1995-2000 period into the sample. This period has previously been excluded since it constitutes a boom during which there is little variation in our fiscal variables. As expected, the inclusion of these observations results in an increase in our standard errors (by on the order of 20-30%). The point estimates change very little, however, with estimated personal income multipliers of 1.55 and 1.59 and employment coefficients of 0.000035 and 0.000046.

In unreported regressions we have inspected sensitivity to several additional forms of specification changes. These include the addition of potentially relevant controls, alteration of the manner in which the instruments are specified, and adjustments to the manner in which the fixed effects and trends are specified.

We have controlled for factors including the fraction of the population in the labor force, economic shocks due to shifts in the national industry mix (see, e.g., Bartike, 1991 and Blanchard and Katz, 1992), and the amount of federal grant dollars flowing into each state. These controls have essentially no effect on the results. We have also controlled for interactions between deficit shocks and state income (due to the concern that the relatively wealthy weak-rule states may respond differently to deficit shocks through channels related to income). This moderately reduces the multiplier estimate to 1.4.

Altering the specification of the instruments has essentially no effect on our results. Specifically, since budget rules only bind when deficit shocks are positive, the interaction between budget rules and negative deficit shocks is superfluous to the identification strategy. Dropping these instruments yields first-stage F-statistics close to 20 and has essentially no impact on either the income or employment estimates.

Specification sensitivity emerges when we consider adjustments to the manner in which we specify fixed effects and trends. We have experimented with a variety of specifications involving replacement of our cycle-specific state effects with state effects and our cycle-specific state trends with linear, quadratic, or cubic state trends. Specifications that look most like our baseline specifications (e.g., cycle-specific state effects and quadratic state trends) yield results almost identical to our baseline results. Other specifications yield results which can be either substantially higher or lower than our baseline estimates. In this context, our results emerge as what they are, namely the central tendency of a modestly powered analysis.

Our baseline specifications are our preferred specifications for three reasons. First, they are, roughly speaking, the central tendency of this battery of specification checks. Second, they are more precisely estimated than specifications with sparser sets of fixed effects and trends. Third, as shown in Table 8 they map consistently into aggregate budget data; a dollar in spending cuts predicts a dollar less in aggregate expenditures and weak-rule states adjust their budgets to pay down deficits over a short time horizon.

6. Conclusion

We have exploited plausibly exogenous variation in state budget cuts in an attempt to identify parameters of interest for evaluating counter-cyclical fiscal policy. Our estimates apply to mid-year budget cuts which effectively shift the timing of government spending over the course of the business cycle. This makes them relevant for assessing the welfare effects of smoothing the flow of state expenditures on goods, services, and infrastructure. A complete smoothing of such expenditures could yield large welfare gains. We find that state spending tends to be 9 cents below trend for each \$1 that personal income falls below its trend. If states kept their spending on trend, our multiplier estimate implies that income would be \$0.09 x 1.7 ≈ \$0.15 closer to trend. Smoothing state expenditures could thus reduce the amplitude of state business cycles by about 15 percent.

Estimates of the welfare costs of business cycles vary widely within the literature. Seminal work by Lucas (1987) arrived at a very small estimates of these costs, while more recent work (e.g., Krusell et al, 2009, and Chauvin, Laibson, and Mollerstrom 2009), has arrived at estimates equal to or in excess of 1 percent of all future consumption. It is beyond the scope of this paper to determine precisely how state government spending feeds into the mechanisms associated with the welfare costs highlighted in past work (e.g., the asset bubbles studied by Chauvin, Laibson, and Mollerstrom). If business cycles are as costly as this work finds, the welfare gain from eliminating the contribution made by state governments could reach 0.5 percent of future consumption.

Our estimates suggest that \$1 in mid-year budget cuts reduces state income in that year by around \$1.70 and that \$25,000 in cuts result in the loss of a job. To the extent that federal aid to states prevents budget cuts of a similar nature, we would expect this aid to have similar effects on state economies. For federal aid to replicate the natural experiment we study, however, it would need to take the form of a loan to be repaid during better economic times. Such a loan would generate the required shift away from pro-cyclical expenditures rather than leading to higher total spending over the business cycle.[24]

In evaluating the policy implications of our results, one should consider potential differences between the effects of state and federal stabilization policy. On this topic, Gramlich (1987) notes that state stabilization policy may, contrary to conventional wisdom, have greater impact than federal stabilization policy. He points out that since labor is fairly immobile in the short run, and since services and non-tradables comprise a large share of Gross State Product (GSP), factor and consumption leakages do not pose serious threats to state stabilization policy. He also notes that state stabilization policies will be less offset than federal policy by feedback through the exchange rate (sometimes known as a Mundell-Fleming effect).[25] Ultimately, the relationship between multipliers on state spending and federal spending is *a priori* ambiguous. Cross-state leakages suggest that state multipliers will be smaller; but factor mobility and Mundell-Fleming effects could push in the other direction.

One should also note the important differences between the spending cuts we study and the spending often associated with fiscal stimulus. These cuts apply to previously existing programs that were presumably better planned than hastily organized stimulus projects which must quickly squeeze through administrative bottlenecks. Additionally, other spending which gets tagged as "stimulus spending" might more accurately be called "safety-net spending" and may have very different effects, in

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[24] Additionally, the possibility of federal grants to debt-laden states creates a source of moral hazard which may impact other aspects of state fiscal behavior.
[25] This argument naturally requires that states' policies are not perfectly correlated in a cross-section.

particular with regards to employment. Spending through UI and cash welfare will contribute to income in the form of transfer payments and may "stimulate" when targeted at individuals with high marginal propensities to consume, but may actually reduce employment due to their effects on incentives for job search. These considerations highlight the need for fiscal policy research to focus on key differences between broad fiscal policy categories even while trying to maintain a degree of generality.

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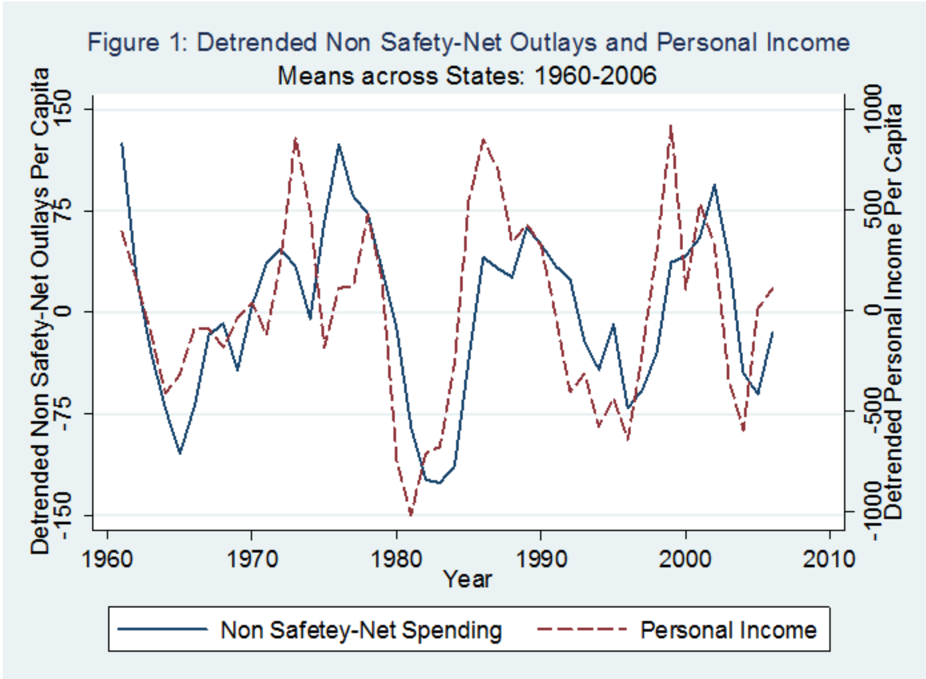


Note: Figure 1 plots the unweighted means (across states) of de-trended personal income and state government spending outside of insurance trusts and safety-net programs on a per capita basis. Detrending was conducted using state-specific quartic polynomials. Personal income data come from the Bureau of Economic Analysis (BEA) and state government spending data come from the Census of Governments (COG).

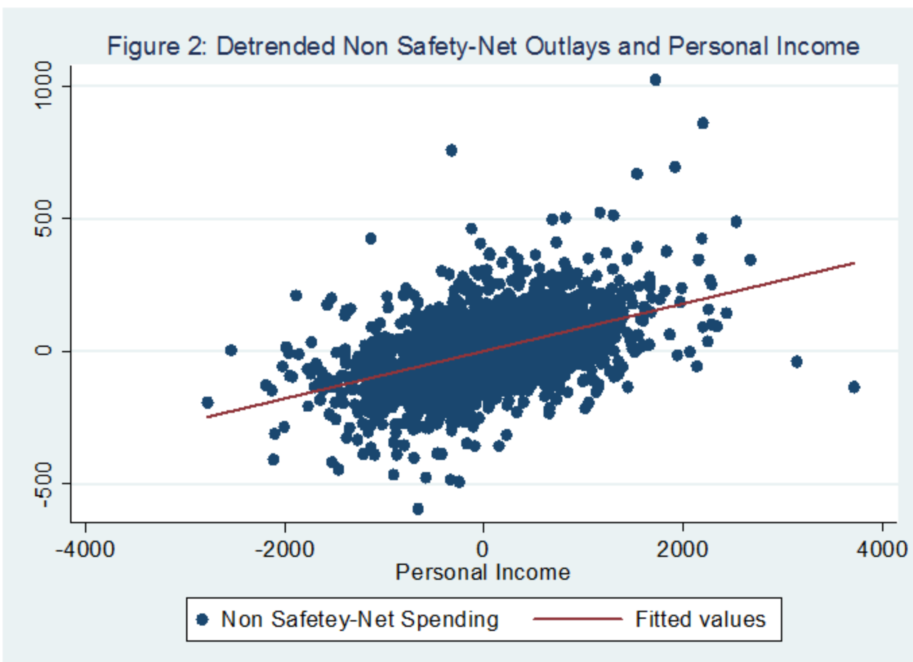
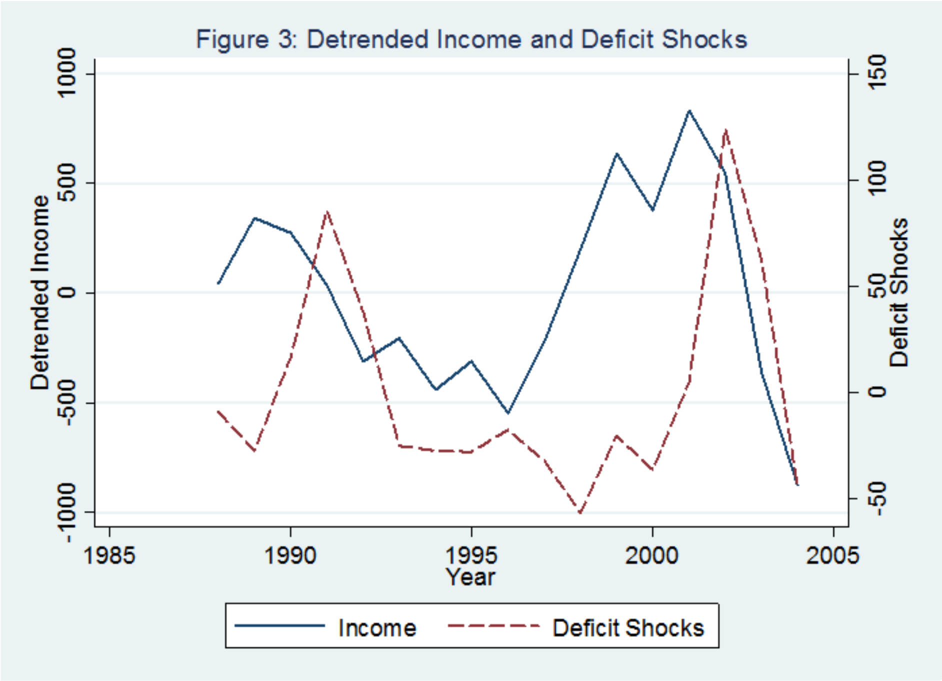
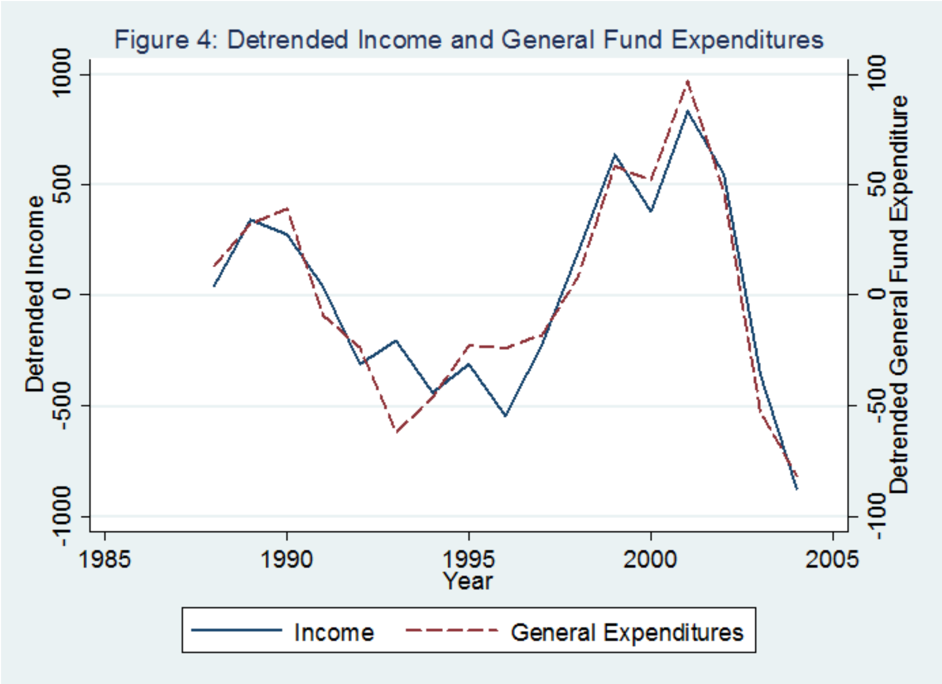


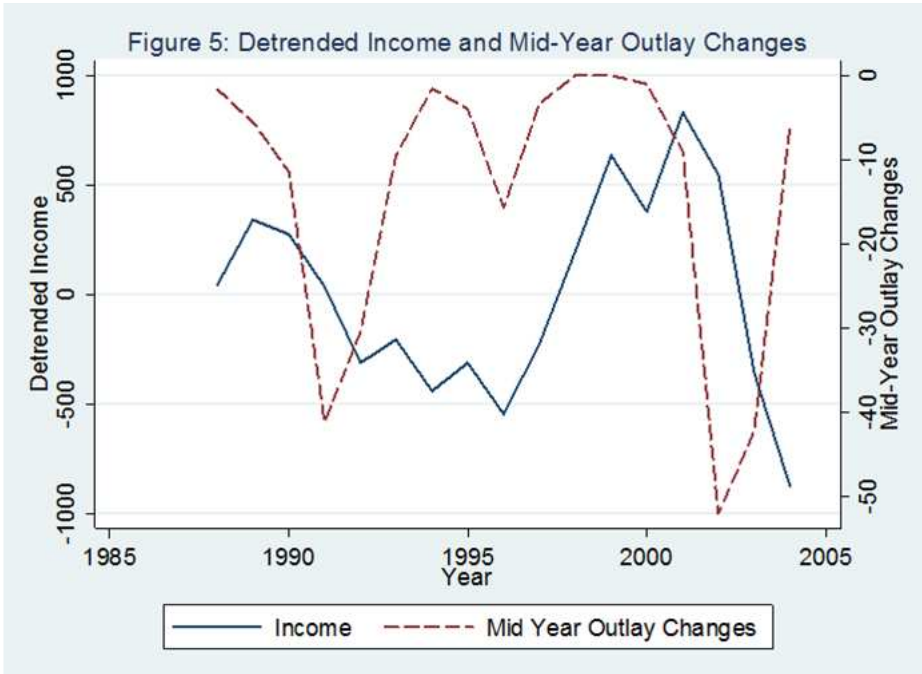
Figure 2 plots state-year observations of de-trended personal income and state government spending outside of insurance trusts and safety-net programs on a per capita basis. The best-fit line has a slope of 0.089 (standard error of 0.014) and the regression yields an r-squared of 0.22. Detrending was conducted using state-specific quartic polynomials. Personal income data come from the BEA and state government spending data come from the COG.



Note: Figure 3 graphs deficit shocks per capita and de-trended personal income per capita. The deficit shocks were constructed using data from semi-annual reports by the National Association of State Budget Officers (NASBO). Personal income data come from the BEA.



Note: Figure 4 graphs personal income and general fund expenditures per capita, de-trended and averaged across states. Personal income data come from the BEA. State general fund expenditures come from NASBO's semi-annual reports.



Note: Figure 5 graphs annual means (across states) of mid-year budget cuts (taken from NASBO's semi-annual reports) and de-trended personal income per capita.

Table 1

Deficit Shocks and Changes in Per Capita Personal Income

	ΔPersonal Income	ΔPersonal Income	ΔPersonal Income	ΔPersonal Income
DEFSHOCK*1{DEFSHOCK > 0}	-2.006***	-1.051**	-1.669***	-0.981
	(0.32)	(0.42)	(0.56)	(0.59)
DEFSHOCK*1{DEFSHOCK < 0}	-0.519	-0.131	-0.245	0.127
	(0.54)	(0.42)	(0.59)	(0.53)
Cycle Specific State Effects?	No	No	Yes	Yes
Year Effects?	No	Yes	No	Yes

Note: ***, **, and * indicate statistical significance at the .01, .05, and .10 levels respectively. Deficit shocks were constructed using data from the National Association of State Budget Officer's (NASBO) Fiscal Survey of the States series. Changes in state level personal income were calculated using data from the Bureau of Economic Analysis (BEA), and were calculated on a state fiscal year basis. Both series are in terms of real 2004 dollars per capita. The sample includes observations from 27 of the 29 states documented as having annual budget cycles by NASBO. Massachusetts and Alaska were excluded due to their status as fiscal outliers. The sample covers the years 1988-2004. Several additional observations are unavailable due to incomplete data reporting, leaving a final sample of 448 observations.

Table 2

Rules Classification: Weak/Medium/Strong

Weak Rules	Medium Rules	Strong Rules
CONNECTICUT	CALIFORNIA	ALABAMA
ILLINOIS	MARYLAND	ARIZONA
LOUISIANA	MICHIGAN	COLORADO
NEW YORK	PENNSYLVANIA	DELAWARE
		GEORGIA
		IDAHO
		IOWA
		KANSAS
		MISSISSIPPI
		MISSOURI
		NEW JERSEY
		NEW MEXICO
		OKLAHOMA
		RHODE ISLAND
		SOUTH CAROLINA
		SOUTH DAKOTA
		TENNESSEE
		UTAH
		WEST VIRGINIA

Note: The table contains a classification of the 27 states with annual budget cycles that are included in our final sample as described in the note to Table 1. States were ranked according to a stringency index found in Table 3 of ACIR (1987). States with an index value less than 5 are classified as weak, an index equal to 6 as medium, and an index exceeding 6 as strong. When we classify states as strong or weak, the states classified as medium are shifted into the weak classification. In Table 11, we expand the sample by 11 additional states which, although they have biennial budgeting cycles, have annual legislative cycles.

Table 3

Summary Statistics: Annual Vs. Biennial Budgeting States

Variable	Mean	Std. Dev.	Mean	Std. Dev.
	Annual		Biennial	
Demographic Variables				
State Population	5759126	6441555	4526764	4691658
Drop Out Fraction	0.18	0.043	0.166	0.04
High School Grad Fraction	0.274	0.034	0.282	0.033
Some College Fraction*	0.174	0.036	0.186	0.041
College Plus Fraction	0.152	0.038	0.151	0.033
Medicaid Fraction	0.103	0.035	0.091	0.031
Senior Fraction	0.122	0.018	0.121	0.018
Child Fraction	0.278	0.032	0.272	0.025
Economic Variables				
Personal Income ($ per capita)	28460	5509	28132	3681
Employment per capita*	0.423	0.039	0.438	0.037
Bartik Shock	0.018	0.016	0.019	0.023
Fiscal Variables ($ per capita)				
State General Fund Expenditures	1679	572	1579	559
State General Fund Revenues	1674	569	1564	548
DEFSHOCK	17.9	101.3	6.6	120.8
ΔTAX	2.8	12.8	4.1	15.3
ΔTAXNEXT (next fiscal year)	22.5	62.9	21.1	57.8
ΔOUTLAYS	-19.1	33.8	-21.2	38.5
Observations	288		230	

Note: ***, **, and * indicate statistically significant differences between the means for annual and biennial budgeting states at the .01, .05, and .10 levels respectively. The 288 observations for annual states correspond to the sample initially described in the note to Table 1. The 230 observations for biennial states include all observations for the 21 biennial budgeting states (as categorized by NASBO) for which the data required to construct the deficit shock measure are available in the years 1988-1994 and 2001-2004. State population and personal income data come from BEA. Other demographic variables were estimated using data from the Current Population Survey (CPS). Employment data come from the Bureau of Labor Statistics (BLS). The employment data were also used to construct the Bartik Shock variable, which is described in detail by Bartik (1991) and Blanchard and Katz (1992). All fiscal variables are either taken directly from, or calculated by the authors using, data in various issues of NASBO's *Fiscal Survey of the States*, and are expressed in real 2004 dollars per capita.

Table 4

Summary Statistics: Strict vs. Weak Budget Rules States

Variable	Mean	Std. Dev.	Mean	Std. Dev.
	Strict		Weak	
Demographic Variables				
State Population*	3235591	2060689	12000000	8911843
Drop Out Fraction	0.18	0.05	0.18	0.03
High School Grad Fraction	0.27	0.03	0.28	0.04
Some College Fraction	0.18	0.04	0.17	0.03
College Plus Fraction*	0.15	0.04	0.17	0.04
Medicaid Fraction	0.10	0.04	0.11	0.03
Senior Fraction	0.12	0.02	0.12	0.02
Child Fraction	0.28	0.03	0.27	0.02
Economic Variables				
Personal Income ($ per capita)***	26816	4644	32519	5397
Employment per capita	0.42	0.04	0.43	0.03
Bartik Shock Growth	0.02	0.02	0.02	0.02
Fiscal Variables ($ per capita)				
State General Fund Expenditures	1584	512	1912	645
State General Fund Revenues	1585	512	1896	641
DEFSHOCK	10	97	36	110
ΔTAX	2	13	5	13
ΔTAXNEXT (next fiscal year)	20	56	28	77
Total Taxes as Share of Gen. Rev.	0.45	0.05	0.51	0.07
Pers. Inc. Taxes as Share of Gen. Rev.	0.14	0.07	0.19	0.05
Corp. Inc. Taxes as Share of Gen. Rev.	0.02	0.01	0.03	0.01
Sales Taxes as Share of Gen. Rev.	0.23	0.06	0.23	0.04
Intergov. as Share of Gen. Rev.	0.35	0.06	0.32	0.06
ΔOUTLAYS	-20	37	-17	25
Observations	83		205	

Note: ***, **, and * indicate statistically significant differences between the means for weak- and strong-budget rule states at the .01, .05, and .10 levels respectively. The combined sample of 288 observations was determined as initially described in the note to Table 1. Data sources for variables that also appear in Table 3 are as described in the note to that table. The additional fiscal variables, which describe various revenue sources as shares of total general revenues were taken from the 2004 Census of Government's *Annual Survey of State and Local Government Finances*.

Table 5

Cyclicality of General Fund Revenues and Expenditures

	(1) All States	(2) Strong-Rule States	(3) Weak-Rule States	(4) All States	(5) Strong-Rule States	(6) Weak-Rule States
Panel A: logs						
	Change in constant law ln(General Revenues)			Change in constant law ln(General Expenditures)		
Change in ln(Personal Income)	0.606***	0.533***	0.772***	0.999***	0.962***	1.084***
	(0.140)	(0.182)	(0.187)	(0.151)	(0.194)	(0.217)
Observations	426	304	122	426	304	122
Panel B: levels						
	Change in constant law General Revenues			Change in constant law General Expenditures		
Change in Personal Income	0.029***	0.021	0.043***	0.062***	0.053***	0.075***
	(0.010)	(0.013)	(0.011)	(0.009)	(0.011)	(0.011)
Observations	426	304	122	426	304	122

Note: ***, **, and * indicate statistical significance at the .01, .05, and .10 levels respectively. Standard errors, calculated allowing for arbitrary correlation at the state level, are in parentheses beneath each point estimate. In column 1 the sample consists of observations for the 27 annually budgeting states listed in Table 2. In column 2 the sample is restricted to states designated as having strong budget rules. In column 3 the sample contains states with either weak or meadium budget rules. The measure of general fund revenues was constructed by taking NASBO's reports of actual general fund revenues and adding back the measure of mid-year tax changes that can be cconstructed using data on tax changes that are also included in the NASBO reports.

Table 6

First Stage Regressions: Baseline Specification

	(1)	(2)	(3)	(4)
	Poterba Sample	Extended Sample	Poterba Sample	Extended Sample
Weak Rules*DEFSHOCK*1{DEFSHOCK > 0}	0.346***	0.401***	0.383***	0.386***
	(0.081)	(0.10)	(0.082)	(0.057)
Weak Rules*DEFSHOCK*1{DEFSHOCK < 0}	-0.103*	-0.148	-0.102*	-0.106
	(0.051)	(0.11)	(0.051)	(0.11)
Medium Rules*DEFSHOCK*1{DEFSHOCK > 0}			0.334***	0.404**
			(0.082)	(0.15)
Medium Rules*DEFSHOCK*1{DEFSHOCK < 0}			-0.100	-0.159
			(0.10)	(0.11)
DEFSHOCK*1{DEFSHOCK > 0}	-0.478***	-0.473***	-0.476***	-0.470***
	(0.057)	(0.049)	(0.058)	(0.048)
DEFSHOCK*1{DEFSHOCK < 0}	0.0422	0.0682	0.0352	0.0525
	(0.048)	(0.091)	(0.049)	(0.092)
Cycle Specific State Effects?	Yes	Yes	Yes	Yes
Cycle Specific State Trends?	Yes	Yes	Yes	Yes
Year Effects?	Yes	Yes	Yes	Yes
Observations	129	288	129	288
R-squared	0.69	0.76	0.69	0.76

Note: ***, **, and * indicate statistical significance at the .01, .05, and .10 levels respectively. Standard errors, calculated allowing for arbitrary correlation at the state level, are in parentheses beneath each point estimate. In all columns, the sample contains 27 annual budgeting states as first described in the note to Table 1. In columns 1 and 3, the years of the sample are 1988-1992, roughly replicating the sample used by Poterba (1994), but with two additional observations missing due to incomplete or questionable data. In columns 2 and 4 the sample includes data from 1988-1994 and from 2001-2004. Cycle specific state effects means that there are two dummy variables included for each state, one equal to 1 for the years 1988-1994 and the other equal to 1 for the years 2001-2004. Similarly, cycle specific state trends mean that each state has two trend variables, one set equal to 1 in 1988 and rising to 7 in 1994, but equal to 0 thereafter, and one equal to 0 prior to 2001, then equal to 1 in 2001 and rising to 4 in 2004. In columns 3 and 4, the states are categorized according to their budget rules as listed in Table 2. In columns 1 and 2, both the weak and medium rule states from Table 2 are categorized as weak-rule states.

Table 7

Relationship between Mid-Year Budget Cuts and the Evolution of Total Revenues and Expenditures

Panel A	(1)	(2)	(3)
		Expenditures	
	Year t	Year t+1	Year t+2
ΔOUTLAYS (year t)	1.036	0.008	-1.717
	(0.642)	(1.121)	(0.816)
Panel B			
		Revenues	
	Year t	Year t+1	Year t+2
ΔOUTLAYS (year t)	-0.226	-0.728	-0.052
	(0.587)	(0.860)	(0.398)
Observations	288	288	288

Note: ***, **, and * indicate statistical significance at the .01, .05, and .10 levels respectively. This table contains results from the second stages of IV regressions of state government expenditures and revenues on mid-year budget cuts. The sample is as described in the note to Table 1. Expenditures are constructed as the sum of real per capita current and capital expenditures from the Census of Governments. Revenues are constructed as real per capita general revenues from the Census of Governments.

Table 8

Baseline Second Stage Specifications

	(1)	(2)	(3)	(4)
Panel A		*2SLS*		
	Personal Income	Employment	Personal Income	Employment
ΔOUTLAYS	1.758*	0.0000402**	1.681*	0.0000404**
	(0.91)	(0.000019)	(0.93)	(0.000018)
DEFSHOCK*1{DEFSHOCK > 0}	1.106***	0.00000653	1.084***	0.00000657
	(0.37)	(0.0000044)	(0.38)	(0.0000041)
DEFSHOCK*1{DEFSHOCK < 0}	-0.0594	0.00000709	-0.0565	0.00000708
	(0.31)	(0.0000064)	(0.31)	(0.0000064)
Panel B		*LIML*		
	Personal Income	Employment	Personal Income	Employment
ΔOUTLAYS	1.763*	0.0000408**	1.682*	0.0000406**
	(0.91)	(0.000019)	(0.93)	(0.000018)
DEFSHOCK*1{DEFSHOCK > 0}	1.108***	0.00000668	1.084***	0.00000664
	(0.37)	(0.0000045)	(0.38)	(0.0000042)
DEFSHOCK*1{DEFSHOCK < 0}	-0.0596	0.00000707	-0.0565	0.00000707
	(0.31)	(0.0000064)	(0.31)	(0.0000064)
Excluced Instruments	Medium and Weak Budget Rules*DEFSHOCK (above and below zero)		Weak Budget Rules*DEFSHOCK (above and below zero)	
Cycle Specific State Effects?	Yes	Yes	Yes	Yes
Cyle Specific State Trends?	Yes	Yes	Yes	Yes
Year Effects?	Yes	Yes	Yes	Yes
Observations	288	288	288	288

Note: ***, **, and * indicate statistical significance at the .01, .05, and .10 levels respectively. This table contains results from the second stages of IV regressions of personal income and employment on mid-year budget cuts. The sample is as described in the note to Table 1. Standard errors, calculated allowing for arbitrary correlation at the state level, are in parentheses beneath each point estimate. Results in Panel A were estimated using the Two-Stage-Least-Squares procedure while results in Panel B were estimated using Limited Information Maximum Likelihood. Fixed effects and trends are as described in the note to Table 6. The excluded instruments in columns 1 and 2 correspond to the 4 interaction variables used in columns 3 and 4 of the regressions presented in Table 6. The excluded instruments in columns 3 and 4 correspond to the 2 interaction variables used in columns 3 and 4 of the regressions presented in Table 6. The partial F-statistic on the excluded instrument exceeds 10 in all specifications.

Table 9

Employment and Personal Income/Wages by Public and Private

	(1)	(2)	(3)
Panel A			
	Total Employment	Government Employment (QCEW)	Government Employment (Census of Govs.)
ΔOUTLAYS	0.0000404**	0.0000020	0.000000922
	(0.000018)	(0.0000046)	(0.0000027)
Panel B			
	Total Personal Income	Gov. Wages and Salaries (QCEW)	State Gov. Wages and Salaries (Census of Govs.)
ΔOUTLAYS	1.681*	0.136	0.461
	(0.93)	(0.204)	(0.49)
Panel C			
	Private Employment (QCEW)	Personal Income net of Gov. Wages and Salaries (QCEW)	Personal Income net of State Gov. Wages and Salaries (Census of Govs.)
ΔOUTLAYS	0.0000384**	1.546*	1.220
	(0.000017)	(0.93)	(1.18)
Excluded Instruments	Weak Budget Rules * DEFSHOCK * 1{DEFSHOCK above and below 0}		
Cycle Specific State Effects?	Yes	Yes	Yes
Cyle Specific State Trends?	Yes	Yes	Yes
Year Effects?	Yes	Yes	Yes

Note: ***, **, and * indicate statistical significance at the .01, .05, and .10 levels respectively. Standard errors, calculated allowing for arbitrary correlation at the state level, are in parentheses beneath each point estimate. Specifications take the same form as the specifications from columns 3 and 4 of Panel A in Table 7. In Column 2 the dependent variable in Panel A is total government employment as taken from BLS's Establishment Survey and the dependent variable in Panel B is total government wages and salaries as taken from the same source. In Column 3 the dependent variable in Panel A is total state government employment as taken from the Employment section of the annual survey of state governments taken by the Census Bureau, and the dependent variable in Panel B is the wages and salaries total as taken from the same source. Panel three shows results from specifications in which government employment or wages and salaries are subtracted from the relevant sum of private and public income or employment, with the sources as indicated in the table.

Table 10

Robustness to Inclusion of Additional States with Annual *Legislative* Cycles

Panel A	(1)	(2)	(3)	(4)
	Personal Income	Employment	Personal Income	Employment
ΔOUTLAYS	1.831*	0.0000380**	1.835*	0.0000391***
	(1.01)	(0.000015)	(1.07)	(0.000015)
Observations	407	407	407	407
Panel B				
ΔOUTLAYS	1.874*	0.0000249	1.874*	0.0000247
	(0.98)	(0.000018)	(1.01)	(0.000018)
Instruments	Medium and Weak Budget Rules*DEFSHOCK (above and below zero)		Weak Budget Rules*DEFSHOCK (above and below zero)	
Cycle Specific State Effects?	Yes	Yes	Yes	Yes
Cyle Specific State Trends?	Yes	Yes	Yes	Yes
Year Effects?	Yes	Yes	Yes	Yes
Observations	429	429	429	429

Note: ***, **, and * indicate statistical significance at the .01, .05, and .10 levels respectively. This table contains results from the second stages of IV regressions of personal income and employment on mid-year budget cuts. Standard errors, calculated allowing for arbitrary correlation at the state level, are in parentheses beneath each point estimate. Specifications in Panel A correspond to those in Panel A of Table 7, but with the addition of 11 states that have biennial *budget* cycles and annual *legislative* cycles. These states are: Florida, Indiana, Maine, Minnesota, New Hampshire, Ohio, Virginia, Washington, Wisconsin, Nebraska, and Hawaii. Among these additional states, Wisconsin and New Hampshire qualify as weak-rule states and the remainder as strong-rule states. Specifications in Panel B add Vermont (weak rules) and Wyoming (strong rules) to the sample used in Panel A. These states are added with caution due to their atypical fiscal traits, which are described in the text.

Table 11

Robustness to Inclusion of the 1995-2000 Boom

	(1)	(2)	(3)	(4)
	Personal Income	Employment	Personal Income	Employment
ΔOUTLAYS	1.554	0.0000353*	1.592	0.0000463**
	(1.17)	(0.000021)	(1.34)	(0.000023)
Instruments	Medium and Weak Budget Rules*DEFSHOCK (above and below zero)		Weak Budget Rules*DEFSHOCK (above and below zero)	
Cycle Specific State Effects?	Yes	Yes	Yes	Yes
Cyle Specific State Trends?	Yes	Yes	Yes	Yes
Year Effects?	Yes	Yes	Yes	Yes
Observations	448	448	448	448

Note: ***, **, and * indicate statistical significance at the .01, .05, and .10 levels respectively. This table contains results from the second stages of IV regressions of personal income and employment on mid-year budget cuts. The sample is as described in the note to Table 1. Standard errors, calculated allowing for arbitrary correlation at the state level, are in parentheses beneath each point estimate. Specifications correspond to those in Panel A of Table 7, but with the addition of 11 states that have biennial *budget* cycles and annual *legislative* cycles. These states are: Florida, Indiana, Maine, Minnesota, New Hampshire, Ohio, Virginia, Washington, Wisconsin, Nebraska, and Hawaii. Among these additional states, Wisconsin and New Hampshire qualify as weak-rule states and the remainder as strong-rule states.

Appendix

Table A1

Poterba Regressions with Balanced Budget Requirement Variables

	(1)	(2)	(3)	(4)	(5)	(6)
	Poterba Sample			Extended Sample		
	ΔOUTLAYS	ΔTAX	ΔTAXNEXT	ΔOUTLAYS	ΔTAX	ΔTAXNEXT
DEFSHOCK*1{DEFSHOCK > 0}	-0.454***	0.120**	0.554**	-0.416***	0.0687**	0.240**
	(0.061)	(0.044)	(0.21)	(0.034)	(0.027)	(0.10)
DEFSHOCK*1{DEFSHOCK <= 0}	-0.0358	0.0141	0.159	-0.0350**	0.0233	0.118**
	(0.022)	(0.024)	(0.12)	(0.016)	(0.014)	(0.050)
Weak Rules*DEFSHOCK*1{DEFSHOCK > 0}	0.274***	-0.0381	-0.138	0.254***	-0.0131	0.0354
	(0.074)	(0.047)	(0.25)	(0.052)	(0.028)	(0.14)
Weak Rules*DEFSHOCK*1{DEFSHOCK < 0}	0.0300*	-0.0120	-0.0177	0.0179	-0.0269*	0.0210
	(0.016)	(0.022)	(0.10)	(0.015)	(0.013)	(0.051)
Observations	129	129	129	288	288	288
R-squared	0.52	0.22	0.19	0.59	0.15	0.12

Note: ***, **, and * indicate statistical significance at the .01, .05, and .10 levels respectively. Standard errors, calculated allowing for arbitrary correlation at the state level, are in parentheses beneath each point estimate. In all columns, the sample contains 27 annual budgeting states as first described in the note to Table 1. In columns 1, 2, and 3, the years of the sample are 1988-1992, roughly replicating the sample used by Poterba (1994), but with two additional observations missing due to incomplete or questionable data. In columns 4, 5, and 6 the sample includes data from 1988-1994 and from 2001-2004. ΔOUTLAYS represents the mid-year budget rescissions made by states in real per capita terms. ΔTAXNEXT is the value of tax collections expected in the following fiscal year as a result of tax increases enacted during the current fiscal year. ΔTAX is an estimate of the value of enacted tax increases in the current fiscal year. Estimating ΔTAX requires making use of the enactment dates provided by NASBO in the *Fiscal Survey of the States* series. Like the specifications in Poterba (1994) these specifications include neither time effects nor state fixed effects.