The effects of state budget cuts on employment and income

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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

[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 declines by $1 relative to trend, non safety-net spending tends to decline by 8.9 cents relative to 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 requirements (Government Accountability Office, 2009).[4] Our empirical work investigates the effects of the budget cuts these dollars aimed to prevent.[5]

Past work, discussed in more detail below, has generally estimated fiscal policy multipliers using aggregate time series. Our methodological innovation 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, 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.

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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.

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. The estimate of the state-spending multiplier is not particularly precise (e.g., a standard error of 0.93), while the estimate of the dollars-per-job figure is more so (e.g., a standard error of around $11,000). The estimated employment effect is also a bit more robust across a variety of specification checks. That said, neither estimate shifts around more than one might expect on the basis of its standard errors. Evidence from robustness checks suggests that, if anything, our best estimate of the multiplier might be slightly below 1.7.

Crucially, 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 relative to trend falls by 8.9 cents for each $1 decline in personal income relative to trend. The product of these estimates implies that the amplitude of state-level business cycles would 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 should not be surprising given 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.

Our work complements, but also has some distinct advantages over, past work on fiscal policy. Prior empirical work leaves us well short of consensus regarding the likely effects of fiscal stimulus, particularly when it takes the form of government spending (see, e.g., the discussion in Auerbach and Gale, 2009). Research on tax-based stimulus has made more progress because tax cuts and tax rebates often involve useable sources of household-level variation. Household receipt of tax rebate checks in 2001, for example, was staggered across months in a predictable manner. Consequently, the rebate's implementation provided a relatively clean opportunity for estimating the household consumption responses at the root of the fiscal-multiplier process (Johnson, Parker, and

Souleles, 2006).[6] Neither the direct beneficiaries of stimulus spending nor the precise timing of its arrival, on the other hand, are so readily identifiable. Consequently, as mentioned above, estimation of Keynesian spending multipliers has tended to use macroeconomic time series. This typically involves the methods associated with Structural Vector Autoregressions (SVARs), from which both causal inference and extrapolation to actual policy options are difficult.[7]

The difficulties with inference based on SVAR studies of government spending 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 major reasons. First, wars can be associated with other important shifts in economic policy.[8] Second, the onset and resolution of wars may have significant impacts on expectations for future tax and income streams, both important determinants of current consumption behavior.

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[6] Work on tax rebates has progressed to include investigation of the conditions under which one might expect to observe relatively high or low marginal propensities to consume (MPCs). As theory would predict, empirical work finds that liquidity constrained households have relatively high MPCs out of tax rebates (Agarwal et al, 2009; Bertrand and Morse, 2009; Broda and Parker, 2008; Johnson, Parker, and Souleles, 2006), and that tax cuts that seem likely to be relatively long lasting are associated with relatively high MPCs out of the additional disposable income (see, e.g., Souleles, 2002).

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

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

The difficulties with extrapolation from SVAR studies also relate to their reliance on defense spending for identification. More specifically, there are important differences between defense spending and the kinds of spending typically under consideration for stimulus purposes. Spending on infrastructure, health, and education, for example, may directly affect the economy's production possibilities by adding to physical and human capital, while spending on a program like Medicaid may directly affect the welfare of individuals in the economy. Spending on programs like Medicaid and Unemployment Insurance 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.

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 discusses our 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.[9] 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.

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 the trend. Dividing this difference by the difference in their fiscal policies yields a multiplier estimate of about 56/31 = 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

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[9] 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*).

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 several potential sources of concern and present our basis for arguing that the condition is satisfied. Additionally, it is important to distinguish between genuine concerns about our exclusion restriction and a seemingly intuitive, but incorrect concern. In particular, 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. These requirements have been the subject of a significant amount of past research. Highlights include studies by Poterba (1997) and Bohn and Inman (1996), which 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 (commonly known as the No-Carry rule) drives relatively fast returns to fiscal balance following adverse shocks.

The current 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 1-10 stringency index provided by the ACIR (1987). The index allocates an initial point value ranging from 0 to 8 on the basis of a state's strictest rule. It adds 1 additional point for statutory rules and 2 for constitutional rules. Rules which apply to the budget's enactment phase receive lower scores than those that apply to the execution phase. The requirement that the governor propose a balanced budget, for example, receives a low initial score of 2. The prohibition against carrying a deficit into the next fiscal year receives the highest possible base score of 8.

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.[10]

Deficit shocks

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

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

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

[10] 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.

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* level 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. Unfortunately, we cannot directly observe constant-law outlays and revenues. However, we can recover them by subtracting mid-year changes (which we denote as $\Delta Outlays_t$ and $\Delta Revenue_t$) from the final outlay and revenue outcomes for the fiscal year ($Outlays_t$ and $Revenue_t$); this construction is further discussed in the next section.

We combine the revenue and expenditure shocks to form:

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

A large fiscal shock corresponds to a fiscal crisis in a state: expenditures are much higher than expected and revenues are much lower than expected. 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 higher, 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 sloppily done by insufficient staff. In this scenario, our measured cuts might be more imaginary than real since they would be relative to an unrealistic or phantom baseline. This could lead us to identify the effects of accounting gimmicks (or errors) rather than actual reductions in spending.

Four pieces of statistical analysis make it reasonable to accept the first interpretation over the second. First, the mean deficit shock over the 1988-2004 period is \$0.07 per capita, with a standard deviation of \$91.70 per capita, suggesting these are truly mean-zero shocks. Second, as we discuss 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,[11] we have confirmed that a dollar of rescissions isolated by our instruments does indeed correspond to a dollar less in state spending outside of insurance trusts (results not shown). Later, we also show that our own econometrically forecasted deficit shocks have similar properties as the standard measure constructed from the NASBO data.[12] This fourth piece of analysis will be more relevant following our formal exposition of the estimation strategy, hence we save it for then.

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[11] 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.

[12] Poterba (1994) uses a similar procedure as a check on the real nature of the deficit shocks.

For now, 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 negative, large, positive deficit shocks appear. We show this statistically in Table 1. Table 1 shows results from regressions in which we use deficit shocks to predict changes in personal income.[13] 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), since 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

Identification

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 that has power in predicting spending cuts. We run the following regressions:

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[13] 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.

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\text{1}^{\text{st}}\text{ Stage:}^{14}\quad
\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}\le 0\}} + \beta_3 Defshock_{s,t} * 1_{\{Defshock_{s,t}>0\}} \\
&+ \beta_4 Defshock_{s,t} * 1_{\{Defshock_{s,t}\le 0\}} + \delta_s + \delta_t + trend_t * \delta_s
\end{aligned}
$$

$$
\text{2}^{\text{nd}}\text{ Stage:}\quad
\begin{aligned}
&Y_{s,t} = \gamma_0 + \gamma_1 \Delta \hat{Outlays}_{s,t} + \gamma_2 Defshock_{s,t} * 1_{\{Defshock_{s,t}>0\}} \\
&+ \gamma_3 Defshock_{s,t} * 1_{\{Defshock_{s,t}\le 0\}} + \alpha_s + \alpha_t + trend_t * \alpha_s + \varepsilon_{s,t}.
\end{aligned}
$$

In these equations, $Y_{s,t}$ is a state-level outcome for state s during fiscal year t (in particular we use personal income and employment), $\Delta Outlays_{s,t}$ is the within-fiscal-year spending cut, $weakBBR_s$ 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-cycle specific fixed effects and trends. By cycles we refer to business cycle downturns, each of which can be viewed as a distinct episode of fiscal stress. We view the data set as consisting of two such episodes

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[14] 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.

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.

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 Δ*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. The more expansive set of fixed effects and trends helps us remove noise from *Y*.

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

$$\mathrm{E}(\mathrm{weakBBR_s} * \mathrm{Defshock_{s,t}} * \varepsilon_{\mathrm{s,t}}) = 0.$$

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

$$\begin{aligned}&(1 - p_{\mathrm{weakBBR}}) * \mathrm{E}(\mathrm{weakBBR_s} * \mathrm{Defshock_{s,t}} * \varepsilon_{\mathrm{s,t}} \mid \mathrm{weakBBR_s} = 0) \\ &+ p_{weakBBR} * \mathrm{E}(\mathrm{weakBBR_s} * \mathrm{Defshock_{s,t}} * \varepsilon_{\mathrm{s,t}} \mid \mathrm{weakBBR_s} = 1) = 0,\end{aligned}$$

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

$$p_{\mathrm{weakBBR}} * \mathrm{E}(\mathrm{weakBBR_s} * \mathrm{Defshock_{s,t}} * \varepsilon_{\mathrm{s,t}} \mid \mathrm{weakBBR_s} = 1) = 0$$

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 $\mathrm{E}(\mathrm{Defshock_{s,t}} * \varepsilon_{\mathrm{s,t}}) = 0.$ Consequently, we have that if $\mathrm{E}(\mathrm{Defshock_{s,t}} * \varepsilon_{\mathrm{s,t}}) = \mathrm{E}(\mathrm{Defshock_{s,t}} * \varepsilon_{\mathrm{s,t}} \mid \mathrm{weakBBR_s} = 1),$ the exclusion restriction is satisfied. In words, 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, then the restriction is satisfied. From this condition, we draw the interpretation that the exclusion restriction requires deficit shocks to have similar economic content in weak and strong budget rule states.

The traditional exogeneity concern in the budget rules literature is that the estimates would not pick up a causal effect of the rules, but merely an omitted variable somehow correlated with them, such as voter preferences. The literature tends not to believe this is the case for several reasons. First, some studies have explicitly controlled for political covariates. Second, most of the rules have existed unchanged for quite some time, with some constitutional rules dating back over one hundred years (see, e.g., Knight and Levinson, 2000 and Levinson, 1998).[15] Even if budget rules were initially correlated with voter preferences, the correlation between voters' preferences in the distant past and present is likely to be low. The more difficult the rules are to change the more plausibly exogenous is the variation. Most importantly, however, since we use an interaction between the budget rules and deficit shocks as our excluded instrument (rather than the budget rules themselves) and since we condition on a flexible set of fixed effects and trends, correlations between the budget rules and unobserved state characteristics per se are not threats to our identification strategy. Threats to our strategy would have to come from differences in how deficit shocks emerge in states with weak and strong budget

[15] Levinson (1998) reports that from 1969 to 1995, only Tennessee changed its budget rule regime (the switch being from weak to strong). The change occurred in 1977, or 11 years before our sample begins.

rules during an economic downturn of a given size. We devote considerable attention to this issue in the next sub-section.

We implement our regressions on the sample of states used by Poterba (1994). The sample excludes biennial budgeting states for two reasons. First, the implications of budget rules for the timing of fiscal adjustments is less clear in biennial states than in annual states. Second, for the 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 biennial budgeting states reduces first stage precision, raising statistical concerns regarding the extent to which our instruments satisfy the relevance criterion.[16] 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 definition of mid-year rescissions differs from that of other states.[17] This leaves a sample of 27 annual 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 annual budgeting states contains 8

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[16] When the biennial states are included in our baseline specification, the F-stat 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 along with other robustness checks.

[17] 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.

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. The fact that we can alter these divisions of the states with relatively little change in our results improves our confidence in our source of identification. Table 2 provides a breakdown of the states in each classification.[18]

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). We constructed both of the employment variables using data from the Bureau of Labor Statistics (BLS). Finally, we constructed all of the fiscal characteristics reported in the table using data available in semi-annual reports released by NASBO.

Note that we study the impact of fiscal policy on personal income rather than GSP (the state equivalent of GDP). The vast majority of states have fiscal years which correspond to neither the calendar year nor the federal fiscal year. Most states begin their fiscal years in July. This makes it necessary to use variables which 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.[19]

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[18] 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.

[19] 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.

Our sample runs from 1988-2004. NASBO reports did not include all of the information needed to construct our measure of 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. 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 by annual and biennial states. Since the summary statistics look broadly similar across the two groups, we do not worry that our focus on annual 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 annual 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 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.[20] Figure 5 shows that mid-year spending cuts tend to occur at the start of recessions: just as personal income

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

begins to fall off, spending cuts rise. Spending cuts during the worst years are about 4% of general fund spending (for the hardest hit states), or about $200 per capita.

Table 4 breaks down the summary statistics by weak- and strong-rules states. Again, the demographic characteristics look broadly similar. The exception is that the weak-rule states do 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 with no impact on our results.

Perhaps of more concern are the observed differences between the average deficit shock observed in strong-rule states relative to weak-rule states. The difference is most noticeable when deficit shocks are positive. The average positive deficit shock in strong-rule states is about $39 per capita while the average deficit shock in weak-rule states is about $58 per capita. We note that the $19 difference between the two is actually quite small relative to the variable's standard deviation ($69 per capita in the case of strong-rule states and $79 per capita in the case of weak-rule states) and is statistically insignificant. This makes it possible that the observed difference comes from a combination of statistical noise and true differences in the economic shocks that affected the states. Nonetheless, the concern that deficit shocks are artificially larger in weak-rule states is a serious one since this would imply a violation of the exclusion restriction. We address this concern by constructing two additional deficit shock measures, both of which are designed to cleanse the standard measure of elements which might be subject to manipulations related to the budget-rule regimes.

The first measure eliminates cross-state differences that may occur as part of the forecasting process. Such differences may result from either direct efforts at manipulation or because forecasts get made at different points in time relative to the beginning of the fiscal year (implying different information content with respect to economic conditions for the coming year). We accomplish this by replacing the reported expenditure and revenue forecasts with simple econometric forecasts of our own. The econometric forecast involves predicting revenues and expenditures using single lags of revenues and expenditures themselves as well as single lags of state employment and personal income (our two outcome variables of interest), all in real per capita terms.

The second measure isolates the portion of the original deficit shock measure which can be explained using factors like lagged deviations of personal income and employment from their long-term trends. As reported in Table 1, there is evidence that positive deficit shocks emerge towards the end of booms. This guides our choice of variables for use in isolating the economic content of deficit shocks. We use two lags each of de-trended personal income (i.e., the lagged personal income gap) and de-trended employment. We also use a current value and two lags of the states' Bartik shocks, which pick up predictable local economic shocks resulting from states' industrial compositions.

A look at the summary statistics for each of the deficit shock measures reveals the same general pattern. We refer to the alternative measures as DEFSHOCK_B and DEFSHOCK_C respectively. For each measure, weak-rule states have larger mean positive deficit shocks, and the difference is never greater than one third of the mean for weak-rule states. It is always small as a fraction of the deficit shock's standard deviation.

We have investigated other aspects of the measured deficit shocks, finding, for example, that they are no more or less persistent in weak-rule states than in strong-rule states. We also address the concern that weak-rule states experience larger deficit shocks than strong-rule states because they choose relatively volatile tax bases and/or a relatively volatile set of expenditure programs. We investigate this using regressions presented in Table 5. The specifications entail regressing estimates of state-level coefficients of variation (CVs) for outlays and revenues on our indicator for weak budget rules. In columns 1 and 2, the CVs are constructed using each year in the 1988-2004 sample as observations. In these regressions the coefficient on the indicator for weak rules is quite close to 0 both statistically and economically. Note that the budget rules were binding for many states during several of the years used to construct these CV estimates. This would lead us, if anything, to overstate the impact of weak budget rules on the variance of revenues and outlays since strong-rule states take actions to counteract negative shocks to their revenues and positive shocks to their expenditures. However, the budget rules would not have been binding during the 1995-2000 boom period. Columns 3 and 4 present the results of regressions like those in Columns 1 and 2, but with the CV estimates constructed using 1995-2000 data only. In these regressions, the coefficients on the budget-rule variable remain economically and statistically close to 0, suggesting that outlay and revenue streams are not more volatile in weak-rule states than in strong-rule states. Taken together, the evidence makes us confident that the traditional measure of deficit shocks is, in fact, a result of economic shocks rather than an artificial outcome driven in part by the states' budget rules.

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.[21]

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

[21] Table A1 in the appendix 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.

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 problems.[22] 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 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, and marginally insignificant at 5% (the estimates in columns 1 and 3 of Table 7 have, respectively, *p*-values of 0.070 and 0.053). This lack of precision is likely due to the fact that variation in income is large relative to the variation in $\Delta Outlays$. The employment estimates are more precise, presumably because there is less volatility in employment than in income. With a baseline coefficient of 0.000040 and standard error of 0.000018, the employment results are relatively informative, making it possible to rule out non-trivial values at conventional levels of statistical significance. For the dollars-per-job metric, we use the delta method to obtain a standard error of $11,028 and a 95% confidence interval extending from $3,137 to $46,367. Comparing the results in

[22] 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.

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 8 we attempt to decompose the employment and income results across the public and private sectors. In particular, we estimate the impact of mid-year budget cuts on government employment and on government wages and salaries, and on aggregate employment net of government employment and aggregative income net of government wages and salaries.[23] We do this using two sources of information on 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 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

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

form of capital expenditures and other purchases of services and equipment which flow through privately owned firms.

As discussed earlier, our multiplier estimates apply to mid-year budget cuts which involve differences in the timing of government spending rather than differences in the total amount of government spending over the course of the business cycle. This multiplier is directly relevant for assessing the potential welfare gains from completely smoothing state governments' flow of expenditures on public goods and services and infrastructure investments. As we illustrated in Figures 1 and 2, such spending has historically been highly pro-cyclical, falling by about 8.9 cents for each $1 decline in personal income relative to trend. A smooth path of such expenditures would appear to be preferable in that would both smooth out the stream of public service consumption and reduce macroeconomic fluctuations. Applying our multiplier of 1.7, we estimate that a complete smoothing of these expenditures would reduce the amplitude of state business cycles by about 15% (0.089 x 1.7).

Robustness checks

Table 9 presents robustness checks across alternative instrument specifications. As discussed earlier, budget rules only have binding implications when deficit shocks are positive. Hence we run specifications like the baseline specifications, but only using the interaction between the budget rules and positive deficit shocks as excluded instruments (results in columns 2 and 4). This has a negligible impact on the results. Notably, with the removal of the "instruments" that have no explanatory power, our first stage F-statistics rise towards 20 in both cases.

Table 10 presents checks for the inclusion of various controls. Column 1 looks at demographic characteristics (in particular the shares of the population that are seniors and children), while column 2 controls for local labor market conditions. The Bartik shocks are constructed as in Bartik (1991) and Blanchard and Katz (1992): they represent local shocks to labor markets driven by national trends. The cross-sectional variation is induced by multiplying national movements to industry employment by local industry weights. To construct the shocks, we use state and national income from the BEA's State & Regional Accounts. Neither the demographic variables nor the Bartik shocks exert much influence on the results.

Column 3 of Table 10 conditions on the interaction between income levels and the mid-year spending cuts. In the summary statistics, income levels were the only main characteristic to substantially differ across weak- and strong-rules states. Conditioning on the interaction removes the concern that there is something about rich states that make them respond differentially. The inclusion of this control results in moderately smaller point estimates, with an estimated income multiplier of 1.4 and employment coefficient of 0.000032.

Column 4 of Table 10 includes a control for intergovernmental transfers from the federal government to each state government. The concern is that if such transfers are correlated with our instrument, they may bias our results. Including this control has no impact on the results. Taken together, the results in Table 10 provide evidence of stability with respect to the inclusion of additional economic and demographic controls.

In Tables 11 and 12, we explore the robustness of our results to changes to our sample. Table 11 involves a significant change in the sample of states. Specifically, 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.[24] 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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[24] 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 (of 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.

Table 13 explores robustness to using alternative measures of deficit shocks. The specifications in column 1 are the baseline specifications. The specifications in columns 2 and 3 involve the use of DEFSHOCK_B and DEFSHOCK_C. Recall that DEFSHOCK_B was constructed to purge measured deficit shocks of differences in forecasting behavior across states. DEFSHOCK_C was constructed to strip the original measure of deficit shocks down to its purely economic content (i.e., that predictable on the basis of current employment shocks and recent values of state employment and income). The specifications in Column 4 explore an additional issue related to the construction of deficit shocks. This issue relates to states' use of stabilization or rainy day funds during periods of fiscal stress. We construct a fourth deficit shock measure by adding the shock to the stabilization/rainy day fund to the standard deficit shock measure, where the fund shock is based on initial forecasts and eventual realizations of the fund balance. To the extent that weak- and strong-rule states differ in their use of these funds to smooth deficit shocks, this procedure may be an important check.[25]

[25] Evidence for differential usage of stabilization funds is not statistically significant by conventional standards, but it does lean in the direction which one might expect, namely that strong-rule states appear to build up larger fund balances and make moderately more use of these funds to smooth deficit shocks.

The employment results are fairly stable across this set of specification checks, making us confident that we have picked up something real about the relationship between mid-year budget rescissions and state employment levels. The personal income results are a bit less stable, as might be expected given the standard errors throughout. Nonetheless, the only aberrational results are the estimates involving the use of DEFSHOCK_C, which average 0.58 across the two specifications presented. Use of the alternative deficit shock measures always results in larger standard errors, significantly so for the specifications reported in columns 3 and 4. Taken together, this and previous tables suggest that a "best estimate" for the income multiplier based on the evidence in this paper might be moderately below the 1.7 baseline.

A key point to keep in mind in interpreting the results in Table 13 is that the substitution of new deficit shock measures involves new first stage regressions. One factor that leads us to view the baseline specifications as the most informative is that the first stage is more powerful when using the standard deficit shock measure than when using any of the alternatives. While the instruments almost always pass conventional tests for statistical significance, versions using the alternative deficit shock measures would regularly fall short of the more stringent standards for instrument relevance.

In unreported regressions, we have investigated the hypothesis that states with more trade will have smaller multipliers, as money that leaves the state will be absent from the successive rounds of consumption that drive a multiplier. This question was studied by following Fortin (2006) in using the sum of imports of mining, manufacturing and agricultural goods, scaled by state income as a measure of cross-state trade. The data came from the Bureau of Transportation's Commodity Flow Survey. The results were suggestive in that states with higher import propensities have lower multipliers, but were not precise enough to merit further study. The multiplier at the mean import propensity was similar to the baseline multiplier. Similar results were found for the employment estimates.

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 results apply to a particular form of fiscal policy, namely a smoothing of non safety-net spending by state governments over the business cycle. They imply that a complete smoothing of such expenditures has the potential to yield large welfare gains. Coupled with our finding that states reduce non safety-net spending by 9 cents for each $1 decline in income relative to trend, our income multiplier implies that smoothing these expenditures would reduce the amplitude of state business cycles by 15%.

Estimates of the welfare costs of business cycles vary widely within the literature. Recent work (e.g., Krusell et al, 2009), which accounts for factors like the relatively weak labor-force attachment of the poor and the incompleteness of unemployment insurance, estimates that the US population would be willing to sacrifice around 1% of all future consumption to eliminate all future business cycles. Chauvin, Laibson and Mollerstrom (2009) highlight the importance of asset bubbles for welfare, estimating that the asset bubbles associated with the two most recent recessions had welfare costs equivalent to 3.9% of future consumption. These estimates differ by an order of magnitude from seminal work by Lucas (1987). Lucas arrived at small estimates due largely to his use a representative agent framework. If we place the cost of fluctuations at 2% of future consumption and assume a utility function such that costs rise with the square of the business cycle's amplitude, our estimates imply that smoothing state government spending could reduce the welfare cost of business cycles by an amount equivalent to 0.6% of all future consumption.

Our personal income results, while imprecise, are moderately robust, and our employment results are relatively precise and robust across a variety of specification checks. 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.[26]

Our results highlight the importance of state government budgets in the broader context of business-cycle fluctuations and stabilization policy. State-government spending accounts for 10% of GDP, making it a nontrivial component of both total government spending and the broader economic landscape. Consequently, the pro-cyclicality of state spending outside of safety-net programs emerges as an important contributor to business-cycle fluctuations.

[26] 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.

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).[27] 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 following three differences between the spending cuts we study and the spending often associated with fiscal stimulus. First, these cuts apply to previously existing programs that were presumably better planned than hastily organized stimulus projects which must quickly squeeze through administrative bottlenecks. Second, other spending which gets tagged as "stimulus spending" might more accurately be called "safety-net spending" and may have very different effects, in 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

[27] This argument naturally requires that states' policies are not perfectly correlated in a cross-section.

effects on incentives for job search. Third, the spending we study involves a shift in the timing of spending rather than an increase in total spending over the course of the business cycle. 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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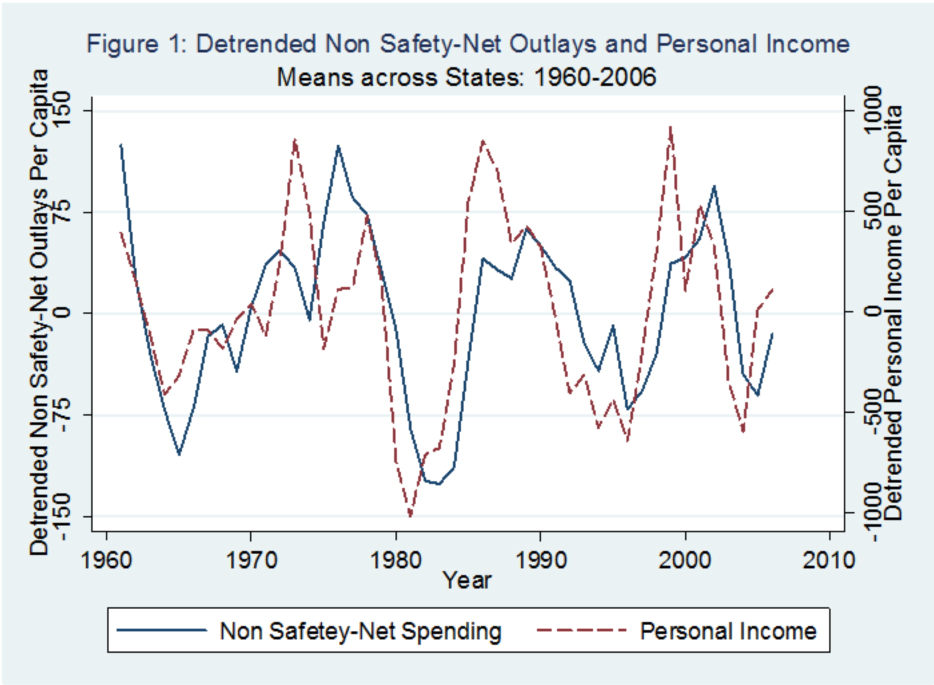


Note: Figure 1 plots the unweighted means (across states) of de-trended personal income and state government spending outside of insurance trusts and safety-net programs on a per capita basis. Detrending was conducted using state-specific quartic polynomials. Personal income data come from the Bureau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

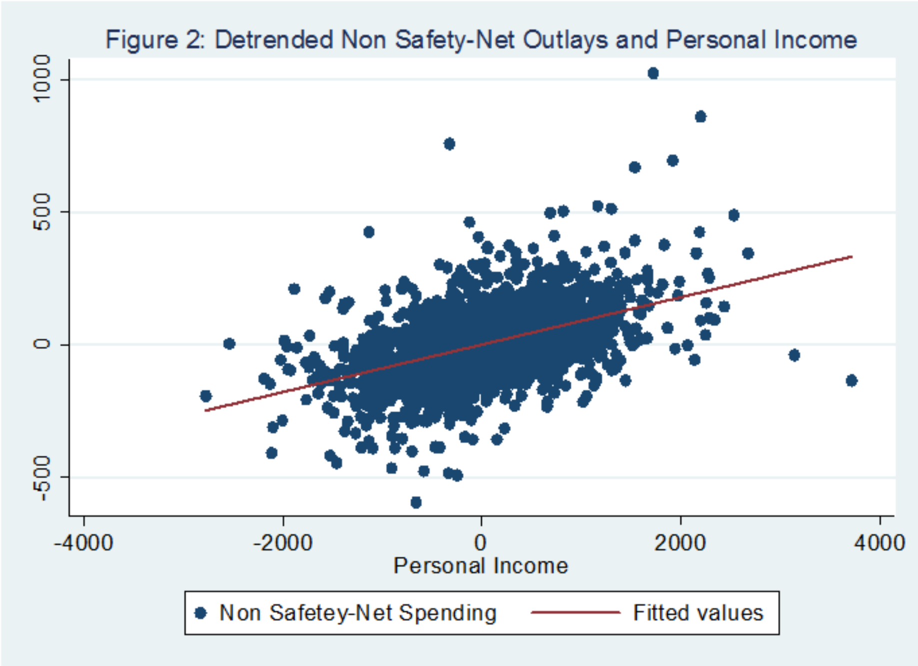
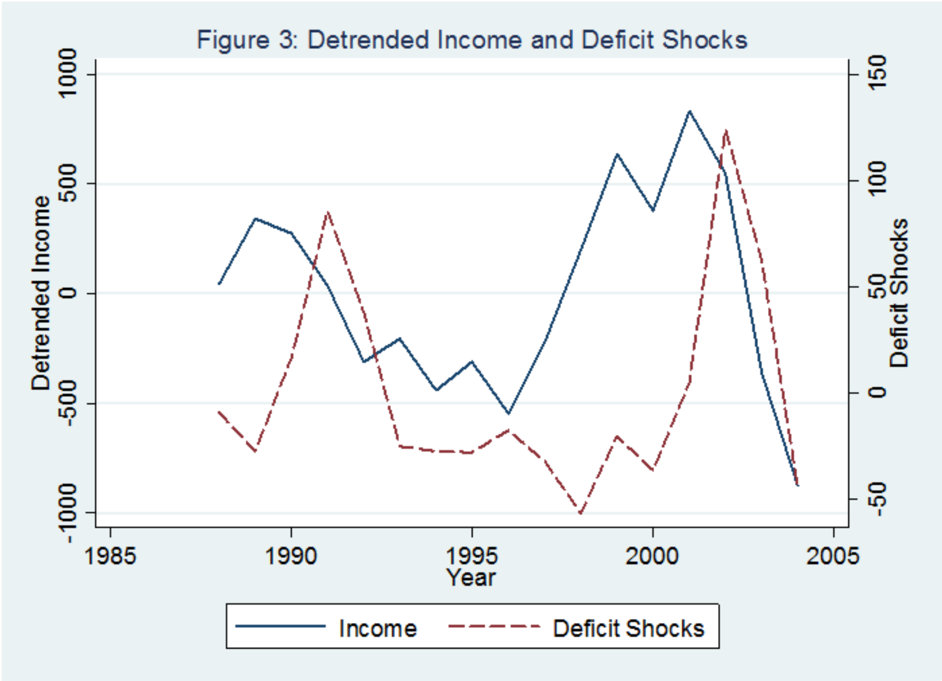
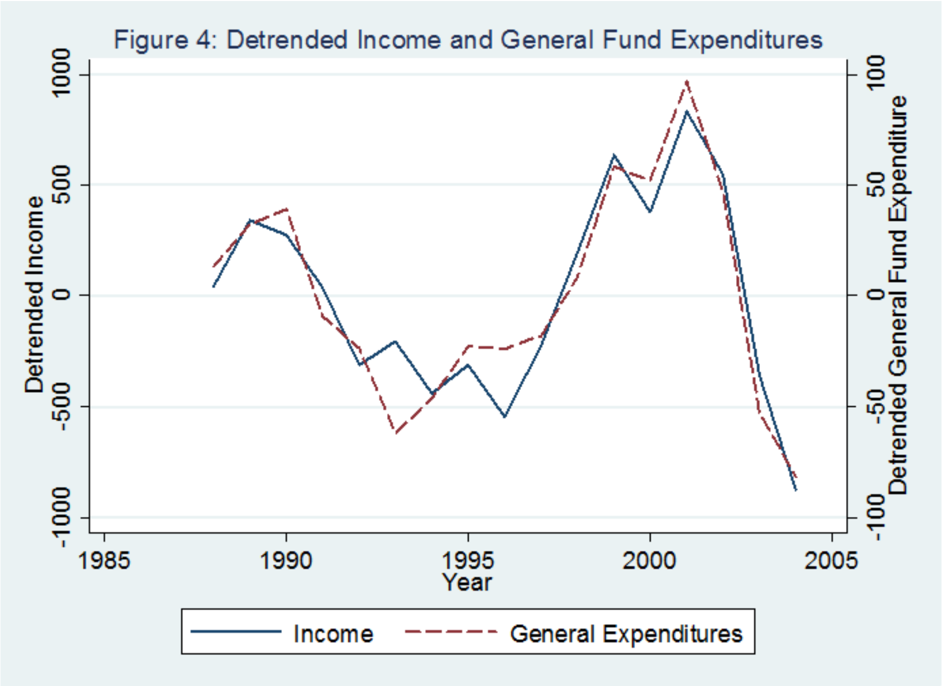


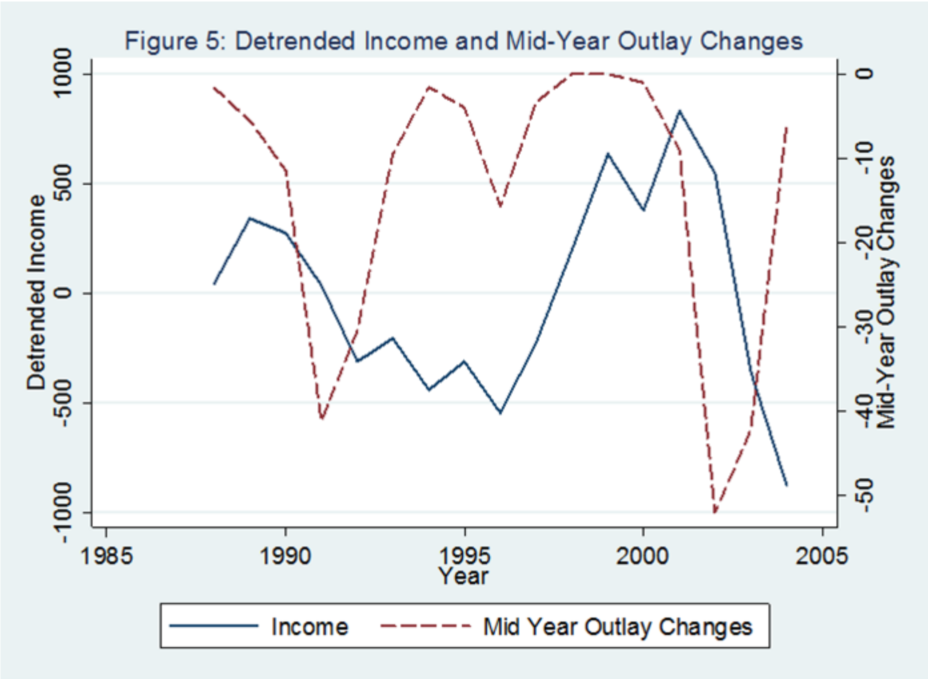
Figure 2 plots state-year observations of de-trended personal income and state government spending outside of insurance trusts and safety-net programs on a per capita basis. The best-fit line has a slope of 0.089 (standard error of 0.014) and the regression yields an r-squared of 0.22. Detrending was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



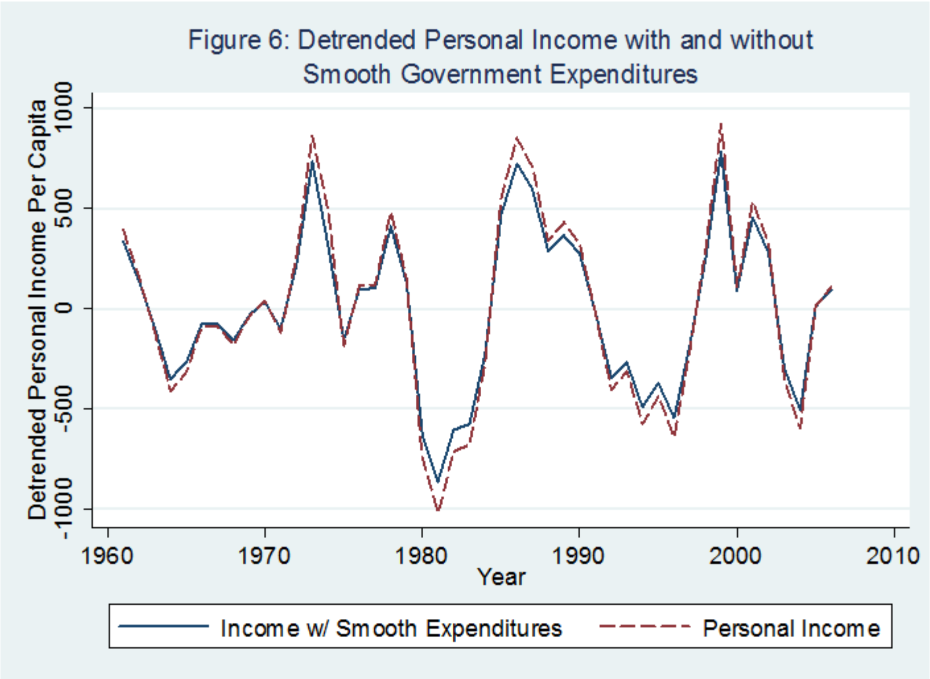
Note: Figure 3 graphs deficit shocks per capita and de-trended personal income per capita. The deficit shocks were constructed using data from semi-annual reports by the National Association of State Budget Officers (NASBO). Personal income data come from the BEA.



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.



Note: Figure 6 graphs the de-trended personal income series from Figure 1 alongside a simulated de-trended income series illustrating the potential impact of smoothing non safety-net state government expenditures. This series was constructed by simply multiply the original de-trended income values by 1 – 0.15, then taking the mean across states for each year. The 0.15 adjustment comes from the product of our estimated multiplier of 1.7 and the observed relationship between de-trended income and de-trended non safety-net expenditures (i.e., the slope of the best fit line in Figure 2, namely 0.089).

Table 1

Deficit Shocks and Changes in Per Capita Personal Income

	ΔPersonal Income	ΔPersonal Income	ΔPersonal Income	ΔPersonal Income
DEFSHOCK*1{DEFSHOCK > 0}	-2.228***	-1.599***	-2.371***	-1.297*
	(0.46)	(0.47)	(0.69)	(0.67)
DEFSHOCK*1{DEFSHOCK < 0}	-0.299	0.434	-0.0845	0.917*
	(0.52)	(0.39)	(0.65)	(0.5)
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 1988-1994 and 2001-2004, which account for the two periods of fiscal stress in our 1988-2004 dataset. Several additional observations are unavailable due to incomplete data reporting, leaving a final sample of 288 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	0.182	0.046	0.175	0.033
High School Grad Fraction	0.273	0.033	0.275	0.038
Some College Fraction	0.176	0.038	0.169	0.031
College Plus Fraction*	0.146	0.036	0.167	0.037
Medicaid Fraction	0.101	0.036	0.108	0.032
Senior Fraction	0.122	0.019	0.121	0.017
Child Fraction	0.281	0.034	0.271	0.023
Economic Variables				
Personal Income ($ per capita)***	26816	4644	32519	5397
Employment per capita	0.419	0.042	0.43	0.03
Bartik Shock	0.018	0.017	0.018	0.015
Fiscal Variables ($ per capita)				
State General Fund Expenditures	1584	512	1912	645
State General Fund Revenues	1585	512	1896	641
DEFSHOCK	10.4	96.8	36.4	110.2
DEFSHOCK_B	23.8	88.7	30.3	98.5
DEFSHOCK_C	16.2	51.6	29.6	56.4
ΔTAX	2.1	12.9	4.6	12.5
ΔTAXNEXT (next fiscal year)	20.4	56.1	27.7	77.3
ΔOUTLAYS	-19.9	36.9	-17.1	24.6
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 all variables are as described in the note to Table 2. DEFSHOCK_B is shock relative to a deficit forecast from single lags of revenues and expenditures as well as single lags of state employment and personal income. DEFSHOCK_C is a shock relative to a deficit forecast from two lags of de-trended employment, two lags of de-trended personal income, and current values and two lags of Bartik shocks

Table 5

Volatility of fiscal policy

	(1)	(2)	(3)	(4)
	Coeff. varn. of revenue stream	Coeff. varn. expenditure stream	Coeff. varn. of revenue stream	Coeff. varn. expenditure stream
Weak budget rules	0.004	-0.002	0.004	-0.001
	(0.014)	(0.013)	(0.012)	(0.009)
Constant	0.091***	0.092***	0.049***	0.051***
	(0.006)	(0.006)	(0.005)	(0.004)
Observations	27	27	27	27
R-squared	0	0	0.01	0
Sample	1988-2004	1988-2004	1995-2000	1995-2000

Note: ***, **, and * indicate statistical significance at the .01, .05, and .10 levels respectively. Heteroskedasticity-robust standard errors are reported in parentheses beneath each point estimate. In columns 1 and 2, the coefficients of variation for state revenue and expenditure streams were calculated as the standard deviation of general fund revenues and expenditures divided by their means (as reported by NASBO) from 1988-2004 in constant dollars per capita. In columns 3 and 4, the sample for calculating the coefficient of variation was restricted to 1995-2000, which corresponds to the boom years during which very few states experienced fiscal stress.

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

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 8

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 9

Robustness Across Instrument Specifications

	Personal Income			
ΔOUTLAYS	1.681*	1.712*	1.758*	1.694*
	(0.93)	(0.89)	(0.91)	(0.88)
	Employment			
ΔOUTLAYS	0.0000404**	0.0000442**	0.0000402**	0.0000435**
	(0.000018)	(0.000018)	(0.000019)	(0.000018)
Excluded Instruments	Weak * DEFSHOCK (above and below zero)	Weak * DEFSHOCK (above zero only)	Medium and Weak * DEFSHOCK (above and below zero)	Medium and Weak * DEFSHOCK (above zero only)
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. The entries in each row labeled ΔOUTLAYS constitute results from a distinct regression. The results in columns 1 and 3 under the heading "Personal Income" correspond to the results from columns 1 and 3 in Panel A of Table 7. The results in columns 2 and 4 are similar, but do not include the interactions between the budget rule variables and the variable for deficit shocks which are less than 0. Similarly, the results in columns 2 and 4 under the heading "Employment" correspond to the results from columns 2 and 4 in Panel A of Table 7. The main effects of the deficit shock variables are also included as controls, but the results are not reported.

Table 10

Robustness To Inclusion of Controls

	Child and Senior Shares	Bartik Shocks	Income *ΔOUTLAYS	Federal Grants
Personal Income	1.777*	1.629*	1.387	1.788*
	(1.04)	(0.91)	(0.95)	(0.96)
Employment	0.0000413**	0.0000409**	0.0000320**	.0000389**
	(0.000017)	(0.000019)	(0.000014)	(0.000019)
Excluded Instruments	Medium and Weak Budget Rules * DEFSHOCK{above and below 0}			
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. Each entry in the rows labeled "Personal Income" and "Employment" corresponds to the coefficient on ΔOUTLAYS from a separate regression. Each personal income specification corresponds to that in column 1 of Panel A of Table 7, but with an added control variable as labeled in the column headings. Similarly, the employment specifications correspond to that in column 2 of Panel B of Table 7. Column 1 includes separate controls for the share of the population that is either under 18 years of age and that is above 65 years of age as estimated using the CPS. Column 2 controls for Bartik shocks, defined as in Bartik (1991) and Blanchard and Katz (1992) and constructed using establishment data from BLS. Column 3 controls for an interaction between ΔOUTLAYS and personal income per capita. Column 4 controls for intergovernmental grants from the federal government to the state governments, again in per capita terms (data on intergovernmental grants come from the Census of Governments' surveys of state government finances). The main effects of the deficit shock variables are also included as controls, but the results are not reported.

Table 11

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 12

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.

Table 13

Robustness Across Alternative DEFSHOCK Measures

Panel A	(1)	(2)	(3)	(4)
	Baseline	Using DEFSHOCK_B	Using DEFSHOCK_C	Fund Shock Inclusive DEFSHOCK
Personal Income	1.758*	2.081**	0.680	1.698
	(0.91)	(0.97)	(1.95)	(1.69)
Employment	0.0000402**	0.0000486***	0.0000402	0.0000313
	(0.000019)	(0.000017)	(0.000030)	(0.000021)
Excluded Instruments	Medium and Weak Budget Rules * DEFSHOCK * 1{DEFSHOCK above and below 0}			
Panel B				
	Baseline	Using DEFSHOCK_B	Using DEFSHOCK_C	Fund Shock Inclusive DEFSHOCK
Personal Income	1.681*	2.033*	0.478	1.745
	(0.93)	(1.08)	(2.28)	(1.65)
Employment	0.0000404**	0.0000467***	0.0000414	0.0000327
	(0.000018)	(0.000017)	(0.000030)	(0.000021)
Excluded Instruments	Weak Budget Rules * DEFSHOCK * 1{DEFSHOCK above and below 0}			
Cycle Specific State Effects?	Yes	Yes	Yes	Yes
Cyle Specific State Trends?	Yes	Yes	Yes	Yes
Year Effects?	Yes	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. Regressions in Panel A are estimated by two-stage least squares, while regressions in Panel B are estimated by limited information maximum likelihood. In column 2, the standard measure of deficit shocks is replaced with DEFSHOCK_B, which is the shock relative to a forecast constructed from single lags of revenues and expenditures as well as single lags of state employment and personal income. In column 3, the standard measure of deficit shocks is replaced with DEFSHOCK_C, which is the shock relative to a forecast constructed from two lags of de-trended employment, two lags of de-trended personal income, and current values and two lags of Bartik shocks. In column 4, the deficit shock measure is augmented to include any unforecasted flows into or out of each state's budget stabilization fund.

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.