The Role of Fiscal Institutions in Analysis of Fiscal Policy

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Abstract

Balanced budget requirements lead to substantial pro-cyclicality in state government spending on infrastructure and public services. Differences in the stringency of states' budget rules drive the pace at which they must make these adjustments. We show that budget rules (and other fiscal institutions) generate variation in deficit-financed expenditures, which could be ideal for studying fiscal stabilization policy. In contrast, many alternative sources of variation in sub-national fiscal policy implicitly involve "windfall" financing, which will miss the effects of future debt on current consumption and investment behavior. Difficulties with our proposed identification strategy prevent us from cleanly estimating a multiplier.

1. Introduction

State governments, whose spending accounts for around 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 their spending fluctuates pro-cyclically. When states enter recessions, their tax bases contract and their safety-net expenditures expand. Compliance with balanced budget requirements can thus entail significant reductions in capital expenditures and in spending on publicly provided goods and services.[2]

It is difficult to rationalize pro-cyclical spending on infrastructure and service provision as serving any welfare-enhancing purpose. The consumption of public services, for example, would generate greater utility if these services flowed smoothly. Similarly, if capital expenditures must be conducted with any cyclical orientation at all, they would ideally be reserved for downturns as a source of "shovel ready" projects. The increase in business-cycle volatility that is induced by pro-cyclical state spending would thus appear to come without offsetting benefits.

The contribution of state governments to the business cycle depends on two parameters. The first parameter is a measure of how pro-cyclical their spending is in practice. Using state-level data on income and the relevant categories of government spending, we estimate this parameter in Section 2. For each $1 that income deviates from trend, state spending on infrastructure and service provision deviates from trend by 9 cents.

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

The second parameter of interest is the fiscal policy multiplier associated with state spending on capital infrastructure and public services. Estimating the causal effect of government spending on the economy requires isolating variation that is not, itself, driven by the business cycle. The fact that shifts in economic activity can be direct determinants of government spending makes isolating such variation difficult.

The empirical macroeconomic literature on fiscal policy has attempted to isolate exogenous shocks to government spending using a combination of Structural Vector Autoregressions (SVARs) and narrative histories.[3] The primary source of variation in this literature can make both inference and extrapolation difficult (see, e.g., the discussion in Auerbach and Gale, 2009). An examination of the time series for aggregate government spending reveals that the shocks isolated by both SVAR and narrative methods will stem largely from defense spending, with the unexpected component coming primarily from the World War II and Korean War build-ups (Ramey, forthcoming). Such spending makes inference difficult for two reasons. First, wars can be associated with other important shifts in economic policy.[4] Second, the onset and resolution of wars may have significant impacts on expectations for future tax and income streams, both of which are determinants of current consumption, investment, and labor supply decisions.

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[3] An early paper in this relatively recent literature is Blanchard and Perotti (2002). More recent examples include Ramey (forthcoming), Auerbach and Gorodnichenko (2010), and Mountford and Uhlig (2008).

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

Difficulties with extrapolation from the macroeconomic studies also relate to their reliance on defense spending. There are important differences between defense spending and the kinds of spending typically considered for stimulus purposes. Unlike defense spending, spending on infrastructure, health, and education affects the economy's production possibilities and may enter into individuals' utility functions. Also unlike defense spending, spending on programs like Medicaid and Unemployment Insurance involve incentive effects that influence labor supply.

A recent (and very much "in progress") literature estimates the effects of fiscal policy using plausibly exogenous variation in government spending at the sub-national level.[5] We show in Section 3 that this variation tends to depart in an important way from the ideal natural experiment for evaluating stabilization policy. Specifically, the spending is effectively "windfall" financed rather than deficit financed. This is common to sources of variation that involve federal financing. With federal financing, the tax burden is borne by both the "treatment" states that receive the spending and the "control" states that do not. Any tendency for deficit financing to crowd out private consumption and investment will thus cancel out within the estimation strategy. In a standard new-Keynesian framework, windfall-financed spending will tend to produce larger multiplier estimates than deficit-financed spending (see, e.g., the discussion by Cogan, Cwik, Taylor and Wieland, 2009).

We argue that differences in states' fiscal institutions, which affect their short-run savings behavior, have the potential to yield an ideal source of variation for studying

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[5] See, e.g., Council of Economic Advisers (2009), Cohen, Coval, and Malloy (2010), Serrato and Wingender (2010), Shoag (2010), and Nakamura and Steinson (2011).

fiscal policy. Spending variation generated by fiscal institutions (broadly taken to include balanced budget requirements, tax and expenditure limitations, and rainy day funds) affects states' long run budget constraints. Unlike windfall-financed spending, a spending increase induced by fiscal institutions is deficit financed. Furthermore, the variation will occur in precisely the categories of spending associated with stimulus-oriented policy decisions.

In sections 4 through 6 we explore an instance in which fiscal institutions produce an intuitively appealing source of variation in state fiscal policy. In doing so, we extend result from Poterba (1994) on the effect of states' balanced budget requirements on their responses to fiscal shocks. The relevant result uses the fact that states have varying degrees of stringency built into the rules which govern deficit financing of general fund expenditures. During times of fiscal stress, we see that states with strict rules enact relatively large rescissions to their budgets in order to quickly narrow emerging deficits. As part of this extension we make an updated version of the deficit shock series (including its component parts as reported by the National Association of State Budget Officers) available electronically through an online data appendix.

We explore the prospects for using 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. Two difficulties prevent us from taking a concrete stand on the size of the multiplier based on the evidence we present. First, states with weak balanced budget rules rely more extensively than others on personal income taxation as a source of revenue. Since personal income taxation is a relatively volatile source of revenue, states with weak budget rules will experience relatively large fiscal shocks in the face of an

economic shock of a given size. For reasons elaborated below, this violates the key identifying assumption associated with the proposed estimation framework. In principal, there are ways to adjust for this source of bias. However, efforts to make these adjustments reveal a troubling degree of sensitivity across specifications and sample periods. A general lack of precision (our second difficulty), coupled with sensitivity across specifications, makes it unwise to treat any particular result as a best estimate, or best reading, of the evidence.

2. Budget Rules and State Spending Over the Business Cycle

Balanced budget requirements dictate a tight relationship between state governments' tax collections and expenditures.[6] Absent significant saving through stabilization funds during expansions, states must choose between increasing tax rates and reducing expenditures during recessions. We save a discussion of differences in the stringency of states' balanced budget rules for section 5. At present, we note only that even the weakest rules require states to enter each fiscal year with the expectation that the year's general fund expenditures will not exceed revenues.[7] In this section we focus on the fact that, as a consequence of these requirements (coupled with generally limited saving in stabilization funds), states exhibit significant pro-cyclicality in their spending on capital infrastructure and service provision (or non safety net expenditures).

We illustrate the pro-cyclicality of non safety net expenditures in Figures 1 and 2,

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[6] To avoid unneeded confusion, we note here that we use the terms "spending," "outlays," and "expenditures" interchangeably.

[7] As we show later, differences in state's budget rules dictate differences in how they respond to unexpected shocks.

which we constructed using flexibly de-trended data on personal income and the relevant categories of government spending.[8] The variables, as with all others in this study, are presented and analyzed in real, per capita terms. Figure 1 plots the means of these de-trended series (taken across states) over time from 1960-2006. Figure 2 displays each state-by-year observation for the two series in scatter plot form. The timing of the cyclical adjustments in state spending (as illustrated in Figure 1) is consistent with what one would expect due to balanced budget requirements. Spending tracks the business cycle with a lag of about one. The best-fit line in Figure 2 implies that when personal income is $1 below trend, spending outside of safety net programs tends to be 8.9 cents below trend (with a standard error of 1.4 cents).

The full effect of this pro-cyclical spending on the business cycle depends on the fiscal policy multiplier associated with these expenditures. Most directly, it depends on the multiplier that would be associated smoothing, or shifting the *timing*, of these expenditures. The implications for aggregate volatility could be substantial. If the relevant multiplier is as large as two (as estimated by Shoag, 2010), state governments would be responsible for nearly 20% of all business-cycle volatility. On the other hand, if shifts government expenditures displace contemporaneous private activity (as found by Cohen, Coval, and Malloy, 2010), state governments may cause a relatively small portion of volatility.

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[8] Specifically, we regressed both non safety net spending and personal income on state-specific quartic trends. Non safety net spending is defined as the sum of "capital" and "current" expenditures minus expenditures on "public welfare," which is dominated by Medicaid and cash welfare assistance. 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. We obtain similar results when running each state's income and spending data through the Hodrick-Prescott filter rather than de-trending with polynomials.

3. Differentiating between Sources of Variation in Fiscal Policy

In this section we lay out a framework for analyzing proposed sources of variation for estimating fiscal policy multipliers on sub-national government spending. A benefit of standard applied microeconomic approaches (in particular relative to VAR and SVAR methods) is that they make it possible to be very explicit about the source of the variation used for econometric identification.[9] We describe how alternative sources of variation can be mapped into distinct natural experiments in fiscal policy.[10] We focus, in particular, on differences in the implied financing of the relevant government spending.

Our characterization of sources of variation in sub-national fiscal policy involves tracking their impact on state spending, $G_{s,t}$, tax revenues, $T_{s,t}$, and the net present value of future liabilities, $NPVL_{s,t}$, at some time t.[11] A source of variation in fiscal policy can be thought of as an instrumental variable, $Z_{s,t}$, which is assumed to be exogenous with respect to some state-level economic outcome, $Y_{s,t}$, like income or employment. This exogeneity may require conditioning on a vector of covariates, $X_{s,t}$, which could include fixed effects, time trends and other relevant controls. To characterize the natural experiment associated with $Z_{s,t}$ one can run the following series of regressions involving the fiscal variables:

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[9] The explicit use of narrative shocks or some other clearly defined source of variation *within* a VAR or SVAR framework, as in Auerbach and Gorodnichenko (2010) and Giavazzi and Favero (2009), can also be an attractive source of relatively transparent identification.

[10] Natural experiments in fiscal policy can differ along several dimensions that are relevant for thinking about their associated multipliers. These include their impact on the economy's production possibilities, the extent to which they substitute for (or complement) private consumption, their timing, and their impact on incentives (e.g., for labor supply) in addition to how they are financed.

[11] In many applications *NPVL* could be proxied for by a measure of a states' current year deficit. When the financing of state (or local) expenditures takes place at the national level, a measure of each state's share of federal tax liabilities may be required.

$$G_{s,t} = \alpha^G + \beta_1 Z_{s,t} + \gamma^G X_{s,t} + \varepsilon^G_{s,t} \tag{1}$$

$$T_{s,t} = \alpha^T + \beta_2 Z_{s,t} + \gamma^T X_{s,t} + \varepsilon^T_{s,t} \tag{2}$$

$$NPVL_{s,t} = \alpha^{NPVL} + \beta_3 Z_{s,t} + \gamma^{NPVL} X_{s,t} + \varepsilon^{NPVL}_{s,t}. \tag{3}$$

Many sources of variation in state government spending involve changes in the distribution of an existing pool of federal funds. Examples include reallocations induced by changes in the structure of congressional power (see, e.g., Cohen, Coval, and Malloy, 2010) and reallocations brought about by updates to formulas that dictate the distribution of intergovernmental grants (see, e.g., Serrato and Wingender, 2010). In a very direct sense, these sources of variation amount to transfers from one state to another.[12] The recipients of these transfers incur no deficits as a result of the ensuing government expenditures. If the money is spent as intended, $Z_{s,t}$ could be scaled so that $\beta_1 = 1$ while $\beta_2 = \beta_3 = 0$. One would want to confirm, however, that the state does not offset the intended expenditures by decreasing other expenditures and lowering current or future taxes, in which case the natural experiment would involve $0 < \beta_1 < 1$, $\beta_2 < 0$, and $\beta_3 = \beta_1 - \beta_2 - 1$. It would be crucial in the latter case to realize that the increase in government spending would not fully characterize the natural experiment.[13]

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[12] As noted by Shoag (2010), the shocks to returns in states' pension funds which serve as his source of econometric identification are also equivalent to cross-state transfers.

[13] Implementation of such an approach can easily go awry when researchers focus exclusively on whether or not they have a "strong instrument" for predicting government expenditures. For example, suppose $\beta_1 = 0.75$ while $\beta_2 = 0.25$, both estimated with standard errors of 0.15. In this instance, the instrument could be characterized as being a strong predictor of spending while not having a statistically significant relationship with tax collections. Yet tax collections make up a non-trivial 25% of the total natural experiment in fiscal policy. Standard use of $Z_{s,t}$ as an instrument for government spending would lead to an inflated estimate of the government spending multiplier in standard Keynesian frameworks. With only one instrument, the

Other sources of variation in state government spending involve the distribution of a deficit- (surplus-) financed increase (decrease) in spending at the national level. A recent example involves formula-induced variation in the funds disbursed through the American Recovery and Reinvestment Act (Council of Economic Advisers, 2009). Another involves the differential impact of changes in defense spending on states with different exposure to defense-related industries (Nakamura and Steinson, 2011). If the residents of each state or locality pay an equal share of the federal tax burden, then these natural experiments become equivalent to changes in the distribution of an existing pool of funds. Suppose, for example, that a policy change results in an extra dollar of spending in half of the states and no change in the others, with all states facing an increase in their future tax liability with a present value of 50 cents. In comparing the impact of this policy change across regions, the future tax liabilities will difference out. This leaves the equivalent of a windfall-financed increase in spending in the states that received the federal funds. Econometrically, an instrument that perfectly predicts the $1 spending differential would be uncorrelated with a perfect measure of future tax liabilities. As before, $Z_{s,t}$ could be scaled so that $\beta_1 = 1$ while $\beta_2 = \beta_3 = 0$.[14]

Changes in government spending that are driven by states' fiscal institutions will differ from the spending changes just described. Such changes will not involve external

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multiplier on government spending cannot be independently identified. With a single instrument one can only estimate the effect of the entire natural experiment in fiscal policy. When spending and tax changes are both in play, the reduced form relationship between $Z_{s,t}$ and $Y_{s,t}$ will provide the only directly interpretable presentation of the results.

[14] Note here that if states differ in the extent to which they are expected to bear the burden of future tax liabilities there may be a separate instrument that could be used to identify the effects of deficit financing. The availability of such a variable would make it possible, in principal, to recover estimates of the effects of fiscal policy associated with any degree of deficit finance.

sources of finance, so that any increase in spending (net of taxes) necessarily involves an increase in future liabilities. These institutions will not, in general, generate exogenous variation in the average level of government spending. They do, however, generate plausibly exogenous variation in how states respond to shocks. Budget rules, for example (as detailed below), dictate the pace at which states adjust in response to unexpected deficits. Similarly, savings devices like rainy day funds may generate differences in how states respond to the arrival of intergovernmental transfers.[15] In cases where fiscal institutions affect spending behavior without affecting revenue-raising behavior, $Z_{s,t}$ could be scaled so that $\beta_1 = \beta_3 = 1$ while $\beta_2 = 0$. This is precisely the natural experiment at the heart of stabilization policy. Distinguishing between this setting and settings where $\beta_3 = 0$ is crucial for testing the importance of traditional Keynesian mechanisms relative to the considerations associated with Ricardian equivalence, which incorporates rational expectations of future tax liabilities (Barro, 1974).[16] Any crowd out of private consumption or investment that results from deficit financing is implicitly netted out when the financing burden is spread equally across the treatment and control groups.

4. A Measure of Fiscal Shocks and its Relation to the Business Cycle

We now work through our attempt to estimate a fiscal policy multiplier using variation induced by fiscal institutions. We begin in this section by describing the

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[15] Suppose, for example, that a federal stimulus policy involved $10 billion transfers to each state and that states with rainy day funds save a greater share of these transfers than others. The difference in spending between the two sets of states would effectively be deficit financed; it would be associated with an equivalently-sized change in their future liabilities.

[16] Considerations driven by rational expectations are incorporated into both real business cycle and new-Keynesian models as discussed by Cogan, Cwik, Taylor and Wieland (2009).

construction of a measure of fiscal shocks and providing evidence on how these shocks

relate to business cycles. In Section 5 we provide evidence on how differences in states'

balanced budget requirements dictate the speed with which they respond to the shocks.

Since we do not, in the end, conclude that the evidence justifies a strong stance regarding

the size of the multiplier, we hold off on extensively detailing a variety of caveats that

arise in the construction of the deficit shocks and the coding of the budget rules. Readers

interested in further exploring such details or in using the deficit shock data should

consult the online data appendix.

Following Poterba (1994), we quantify fiscal shocks using the difference between

budget forecasts and realizations, which are expressed *without* mid-year adjustments as

described below:

$$\textit{Expenditure Shock}_t = \textit{Outlay}_{CL,t} - \mathbf{E}_{t\text{-}1}(\textit{Outlays}_t)$$

$$\textit{Revenue Shock}_t = \textit{Revenue}_{CL,t} - \mathbf{E}_{t\text{-}1}(\textit{Revenues}_t).$$

The terms involving expectations are outlay and revenue forecasts, where the forecast is

made at the end of the previous fiscal year. *Outlay*$_{CL,t}$ and *Revenue*$_{CL,t}$ are the *constant-*

law levels of outlays and revenues; they are what would prevail in the absence of mid-

year adjustments to the budget. The difference between these terms provides a true

measure of expenditure and revenue shocks.[17] We cannot directly observe constant-law

outlays and revenues. However, we can recover them by subtracting mid-year changes

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[17] The use of constant-law measures is crucial because mid-year adjustments to outlays and revenues will
tend to undo the appearance of fiscal shocks. Were mid-year adjustments to be complete, for example,
realized deficits would always equal zero assuming all states enter the fiscal year expecting the budget to
balance.

(denoted as $\Delta Outlays_t$ and $\Delta Revenue_t$) from the final outlay and revenue realizations for the fiscal year ($Outlays_t$ and $Revenue_t$). We compute the total shock by combining the revenue and expenditure shocks to form:

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

Beginning in 1988, the National Association of State Budget Officers (NASBO) reports all the information required to construct these shocks in its semi-annual *Fiscal Survey of the States* series.

Three features of the deficit shock series are worth addressing immediately. First, its dependence on forecasts raises issues associated with forecast manipulation. Forecast manipulation may be a significant concern when fiscal stress is particularly severe (as during the recent financial crisis).[18] We investigated the importance of forecast manipulation by replacing the forecasts reported by NASBO with simple econometric forecasts based of state income, revenues, and expenditures from the previous fiscal year. This change in the forecast series has essentially no effect on the deficit shock measure.

Second, the deficit shocks' relationship with forecasts makes them much less persistent than the economic shocks with which they are associated. State forecasters appear to be taken by surprise during the year in which an economic shock occurs. In forecasts for future years, however, the economic shock is taken into account.

Third, the NASBO reports include mid-year spending cuts, but not mid-year spending increases. This reflects some combination of institutional realities and

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[18] Rose and Smith (forthcoming) discuss a literature on the extent to which revenue forecast manipulation may be a more general phenomenon.

measurement error. The rules for changing appropriations in response to adverse shocks differ from those for changing appropriations in response to favorable shocks. Increases in appropriations require legislation. In the face of unexpected deficits, however, many state governors are constitutionally empowered to impose budget cuts unilaterally. Hence while the variable is indeed right-censored, the degree to which this reflects measurement error is unclear. Measurement error only arises if legislatures appropriate additional spending outside of the usual appropriations cycle. In the context of the NASBO data for 1988 to 1992, Reuben (1993) investigates the implications of making standard econometric corrections for censored data. She finds that these corrections have little impact on estimates of state responses to fiscal shocks.

Figure 3 graphs national means (across the states) of deficit shocks and de-trended personal income per capita from 1988 to 2004. The figure shows that deficit shocks become large when an economy enters a recession. When de-trended personal income turns sharply downward, large, positive deficit shocks occur. Deficit shocks tended to be small and negative during the expansionary years of the mid- and late-1990s. As reported in Table 1, the adverse shocks experienced at the beginnings of recessions and the favorable shocks experienced during expansions result in a mean shock that is fairly close to 0. Because deficit shocks occur close to the peak of a state's business-cycle, they are negatively correlated with *changes* in personal income and positively correlated with the *level* of personal income.

5. State Responses to Deficit Shocks During Two Recessions

In this section we analyze states' short-term responses to deficit shocks, focusing on the differential effect of budget rules with different degrees of stringency. We collect information on balanced budget 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). Rules can be differentiated in large part on the basis of whether they affect the *enactment* or *execution* of a state's budget. An example of a rule that applies to the budget's *enactment* is a rule requiring the legislature to pass a balanced budget. Such a rule does not force states to respond quickly to deficits that emerge over the course of the fiscal year. It requires only that the budget be balanced (in expectation) in the following fiscal year, i.e., that $E(G_{t+1}) < E(T_{t+1})$. Stricter rules apply more directly to the *execution* of the budget. The strictest rule (also known as the "No-Carry" rule) prohibits carrying deficits through the next budget cycle. This rule requires that if a deficit is incurred at time t, the budget for the following year must be such that $\text{Deficit}_t + E(G_{t+1}) < E(T_{t+1})$.[19]

The fiscal behavior of interest takes the form of mid-year tax increases and spending reductions that work to narrow unexpected deficits as they emerge. Since timing is central to this application, we follow Poterba (1994) in restricting our sample to a subset of 27 states with annual budgetary cycles and annual legislative cycles. We save

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[19] Past research has explored some of the consequences of these rules. Notable studies include work by Poterba (1997) and Bohn and Inman (1996), who examine the impact of different requirements on a broad range of budgetary outcomes. Highlights also include 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 confirm empirically that requirements which apply to the budget's execution have greater impact than those that apply only to the budget's enactment. Strict budget rules are associated with lower spending levels, modestly greater accumulation of surpluses in budget stabilization funds, and faster adjustment in response to fiscal shocks.

a detailed discussion of differences in the responses of states with alternative budgetary and legislative arrangements for the online data appendix.

We generate our measure of budget rules using a 1 to 10 index produced by the ACIR (1987). We designate the 8 states with scores less than 7 as "weak-rule" states. This is the cutoff associated with the relatively crucial distinction between states with and without a rule that approximates the No-Carry rule. We consider a further sub-division of the weak-rule states into states with rules of weak- and medium-stringency.[20] Table 2 provides a breakdown of the states in each classification.[21] Table 1 reports summary statistics (separately for states with weak and strong rules) for the fiscal variables analyzed in the current section as well as for additional economic and demographic characteristics. The most striking demographic and economic differences between the groups is that weak-rule states tend to be large, highly populous, and have high incomes. They also, perhaps tellingly, include several of the states facing the worst budget crises during and after the recent recession.

We look to states' mid-year spending cuts and tax increases ($\Delta Outlays_t$ and $\Delta Revenue_t$), in particular to the extent that they are driven by differences in states'

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[20] 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). An alternative classification scheme, based on direct readings of statutes and constitutions across states, has also been recently produced by Hou and Smith (2006). The literature points towards the notion that state political culture may ultimately be as important as the actual content of the requirements themselves (NCSL, 2010). We focus on the ACIR classification system because of its power for predicting state's mid-year budget cuts. This is another case in which we would devote more time and space to robustness analysis if we were ultimately pushing a particular estimate of the multiplier on state government spending. Given that we have not settled on an estimate of the multiplier, however, we note only that robustness analyses along these lines, coupled with a compelling justification for the baseline specification, are crucial components of analyses that rely on particular schemes for classifying budget rules.

balanced budget requirements, as potential sources of variation of the sort described in equations (1) and (2). This leads us to estimate equations (4) and (5), which are similar to specifications implemented by Poterba (1994):

$$\Delta \text{Outlays}_{s,t} = \beta_1 weakBBR_s Defshock_{s,t} + \beta_2 Defshock_{s,t} + \delta_s + \delta_t + trend_t * \delta_s + \varepsilon_{s,t} \quad (4)$$

$$\Delta \text{Revenue}_{s,t} = \gamma_1 weakBBR_s Defshock_{s,t} + \gamma_2 Defshock_{s,t} + \alpha_s + \alpha_t + trend_t * \alpha_s + \mu_{s,t}. \quad (5)$$

In terms of the econometric framework laid out in section 3, the interaction between the deficit shock and the indicator for weak budget rules, $weakBBR_s Defshock_{s,t}$, is the instrument, $Z_{s,t}$, while the main effect of the deficit shock is an essential element of the vector of control variables, $X_{s,t}$. The reason for this will become clear in section 6 when we discuss the intuition behind the key identifying assumption associated with multiplier estimation. In the empirical implementation, we split the deficit shocks into distinct variables for their positive and negative values. Budget rules only have binding implications in the face of positive (i.e., adverse) deficit shocks, so that the relevant instrument is $weakBBR_s Defshock_{s,t} * 1_{\{Defshock > 0\}}$.

Tables 3 and 4 present results describing the behavior of state governments in the face of unexpected fiscal shocks from 1988 through 2004. Table 3 presents results using several categorizations of states on the basis of their balanced budget requirements. Columns 1 and 2 report results for mid-year budget cuts and mid-year tax increases, respectively, with all 27 states in the sample grouped together. These results show that states do little in response to negative deficit shocks while enacting fairly significant

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budget cuts and tax increases in response to positive deficit shocks.[22] The budget cuts

average 28 cents per dollar of deficit shock (estimated with a standard error of 6 cents)

while the tax increases average 7.5 cents per dollar of deficit shock (estimated with a

standard error of 2.3 cents). The estimated tax increases are for the calendar year during

which the shock occurs. Since mid-year tax increases will tend, in general, to be in effect

for fewer than 6 months out of the current fiscal year, the current-year collections can be

much smaller than the new collections scheduled for the following fiscal year.

Columns 3 through 6 of Table 3 divide states into classification by strong and

weak budget rules (columns 3 and 4) and by strong, medium, and weak budget rules

(columns 5 and 6). These columns show that the mid-year budget cuts are concentrated

in states with relatively strict balanced budget requirements. While strong-rule states

enact an average of 38 cents in budget cuts per dollar of deficit shock, weak-rule states

enact an average of 12 cents in such cuts. The standard error on the 26 cent differential is

6 cents, making the difference highly significant statistically. The strong, medium, and

weak classification yields quite similar results.[23]

The budget rules turn out not to predict mid-year tax increases to a degree that

could be regarded as either economically or statistically significant. This result is crucial

for characterizing the proposed natural experiment in fiscal policy (as discussed in

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[22] The absence of a mid-year spending response to negative deficit shocks may be driven by the fact that
NASBO only reports mid-year spending cuts, and not mid-year spending increases. As discussed in
Section 4, this may largely reflect institutional realities rather than measurement error. Outside of the
normal legislative cycle, it is more difficult for appropriations to be increased than for them to be rescinded.

[23] These results are quite consistent with results from Poterba (1994). Other results, which we do not
report, are also broadly consistent with Poterba's findings. This includes the finding that states with
relatively large balances in their stabilization funds enact less in the way of mid-year spending reductions
per dollar of deficit shock.

section 3). The result implies that the interaction between budget rules and deficit shocks

can be viewed as isolating a shock to spending that is deficit financed, with no current

change in tax revenues.

Table 4 breaks the sample down into groups of years, with 1988-1994

representing an initial period during which states experienced significant fiscal stress,

1995-2000 representing an expansionary period during which states experienced few

positive deficit shocks, and 2001-2004 representing a second period of fiscal stress. State

behavior during the two periods of fiscal stress is broadly similar. The principal

difference is that states enacted less in the way of mid-year tax increases during the 2001-

2004 period of stress (on the order of 6.6 cents per dollar of deficit shock versus 14.6

cents during the earlier period).

Differences between the expansionary period and the two periods of stress are

striking. Deficit shocks are generally un-predictive of state governments' mid-year

actions during the 1995-2000 expansion. None of the estimates for this period are

statistically significant and the interaction between budget rules and positive deficit

shocks yields an economically large, wrong signed, and highly imprecise coefficient in

the regression involving mid-year outlay changes. The imprecision is driven in large part

by the fact that there are very few observations involving positive deficit shocks in states

with weak budget rules during this period. These were also years when states were more

likely to have surpluses left over from prior years, making it possible for them to balance

their budgets with smaller mid-year spending reductions and tax increases. The

measurement of deficit shocks may also be more error prone during expansionary years

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due to the absence of reporting on mid-year spending increases.[24] For some combination

of these reasons, we observe that the budget rules lack predictive power during the

expansionary period. Consequently, we focus solely on the periods of fiscal stress in our

effort to estimate the effects of state budget cuts on state economies.

The absence of predictive power during expansions need not pose a problem for

efforts to estimate the effect of government spending "on impact" during recessions.

However, it does raise problems for efforts to estimate multipliers in models with lag

structures. The absence of a clean, extended time series is not conducive to efforts to

account for complex dynamics.

6. Budget Rules as a Source of Variation for Estimating Fiscal Policy Multipliers

We now explore the prospects for using equation (4) as the first stage in an

instrumental variables strategy for estimating the effects of budget cuts on economic

outcomes. The estimating framework is summarized below:

$$\hat{G}_{s,t} = \beta_1 weakBBR_s Defshock_{s,t} + \beta_2 Defshock_{s,t} + \delta_s + \delta_t + trend_t * \delta_s$$

$$Y_{s,t} = \gamma_1 \hat{G}_{s,t} + \gamma_2 Defshock_{s,t} + \alpha_s + \alpha_t + trend_t * \alpha_s + \varepsilon_{s,t},$$

where $G_{s,t}$ represents the mid-year outlay changes from section 5 and where, again as in

section 5, the specifications are estimated with distinct variables for the positive and

negative values of the deficit shocks. Before presenting the estimates we devote the next

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[24] As discussed in an earlier footnote, the absence of reporting on mid-year spending increases may reflect the fact that, institutionally, increases in appropriations require legislation and are hence unlikely to be occur outside of the normal budgetary process.

sub-section to describing the economic intuition behind the key identifying assumption. We also discuss the principal threat to that assumption in detail.

Identification

The key identifying assumption, often called the exclusion or orthogonality restriction, can be written as follows:

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

In short, the excluded instrument, $weakBBR_s Defshock_{s,t}$, must be uncorrelated with the second stage error term.

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

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

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

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

as the exclusion restriction, with $weakBBR_s$ always equal to one. Since we include the main effect of the deficit shock in our regressions, it follows from the properties of ordinary least squares that $E(\text{Defshock}_{s,t} * \varepsilon_{s,t}) = 0$. Consequently, we can write that if $E(\text{Defshock}_{s,t} * \varepsilon_{s,t}) = E(\text{Defshock}_{s,t} * \varepsilon_{s,t} \mid \text{weakBBR}_s = 1)$, then

$E(\text{Defshock}_{s,t} * \varepsilon_{s,t} \mid \text{weakBBR}_s = 1) = 0$. In words, the restriction is satisfied if the unconditional expectation of the deficit shock times the second stage error equals that same expectation conditional on a state having weak budget rules. We interpret this as requiring that the deficit shock variables have similar economic content in weak- and strong-rule states.

The primary threat to this condition stems from differences in the revenue bases utilized across states. Revenue bases with different elasticities (with respect to economic conditions) can lead a given economic shock to result in deficit shocks that differ across states. As reported in Table 1, taxation accounts for 55 percent of general revenues in weak-rule states and 51 percent in strict-rule states, with personal income tax revenue accounting for the entire difference. Strong-rule states make up this difference through a combination of charges, fees, intergovernmental transfers, and other miscellaneous revenues. This raises concern because personal income taxes tend to be more volatile than other revenue sources. Consequently, an economic shock of a given size may, all else equal, result in a relatively large deficit shock in the weak-rule states. If true, this would upwardly bias our multiplier estimates. Conditional on their deficit shocks, weak-rule states would have better performing economies than strong-rule states for reasons unrelated to their budget cuts.

We explore two ways of investigating and attempting to account for bias that may result from this difference in revenue bases. First, we directly allow the relationship between deficit shocks and income to vary with the share of revenues raised through taxation. We do this by controlling for an interaction between the tax share and the main effects of the deficit shock variables. Second, we explore the precise timing with which

our initial estimates of the effect of mid-year outlay changes on income arrive. If the avoidance of mid-year budget cuts is truly improving a state's economic performance, the improvement should arrive during the latter half of the fiscal year. It is only during this latter portion of the year that the fiscal policies of states with different balanced budget requirements would diverge.[25]

The evidence presented below suggests that weak-rule states have better economic performance than strong-rule states in the first quarter of the fiscal years in which shocks occur. This is consistent with the concern that there is, in fact, a difference between the economic content of deficit shocks in the two types of states. We attempt to account for this by controlling directly for the performance of the economy during the first quarter of the fiscal year. The inclusion of this control substantially reduces the size of the estimated multipliers. Unfortunately, a lack of precision makes it difficult to convincingly distill a "best estimate" of the multiplier from this collection of specifications. Low precision and instability across specifications lead us to conclude that this particular strategy for using fiscal institutions to estimate fiscal policy multipliers is unsatisfactory.

Results

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[25] In some settings the anticipation of a change in fiscal policy may lead a response to occur before the fiscal policy actually arrives. Such anticipation effects do not seem particularly plausible in this instance for two reasons. First, to the extent that these deficit shocks are genuinely unexpected, the anticipation effect would have to occur remarkably quickly between early-year recognition of the shock and mid-year implementation of the budget cuts. Second, the limited available evidence suggests that the fiscal policies of weak- and strong-rule states do not differ for long. Weak-rule states appear to pay down their unexpected deficits within as few as two years. Anticipation of the avoidance of budget cuts during the year of the shock would thus have to be coupled with a failure to anticipate the deficit-reducing budget cuts of the following years. In our setting, plausible explanations of the multiplier must rely primarily on the potential contemporaneous effects of government spending (which could include shifts in consumption due to the relaxation of liquidity constraints and various channels through which government spending might crowd in or crowd out contemporary private activity).

Table 5 presents the most basic set of second stage results. We present results separately for the 1988-1994 (columns 1 and 2) and the 2001-2004 (columns 3 and 4) periods of fiscal stress. For each period we show results both with (columns 1 and 3) and without (columns 2 and 4) the inclusion of state-specific trends. In both periods, the results with state-specific trends imply relatively large multipliers while the results without trends imply multipliers much closer to, or even below, one. None of the results are very precisely estimated.

In results not reported, we found that the coefficients in Table 5 are robust to changes in the specification of the instruments (e.g., to only using the interaction between *positive* deficit shocks as an instrument or to dividing the budget rules into the weak, medium, and strong classification), to the use of Limited Information Maximum Likelihood estimation rather than Two Stage Least Squares estimation, and to an expansion of the sample that incorporates states with biennial budget cycles and annual legislative cycles. We also obtain similar results (with the benefit of less sensitivity to the exclusion of fixed effects and trends) when we run each state's personal income series through the Hodrick-Prescott filter prior to estimation. In the reported results we focus on dimensions of the robustness analysis that highlight problems with the estimation strategy.

Table 6 presents results from specifications in which we include controls for interactions between the share of a state's general revenues that come from taxation and the measures of deficit shocks. This specification allows the relationship between deficit shocks and income to vary with the tax share, which would be expected given that taxes exhibit greater volatility than other sources of revenue. The inclusion of these controls

decreases the size of the multiplier estimated for the 1988-1994 period while increasing the size of the multiplier estimated for the 2001-2004 period. The interactions themselves yield coefficients that are imprecisely estimated and that change substantially across the two samples.

Table 7 presents an investigation of the timing of the income gains that were estimated in columns 1 and 3 of Table 5. If these income gains are truly caused by the avoidance of mid-year budget cuts, then they would be expected to appear during the latter two quarters of the fiscal year. The results in Table 7 show that this is not the case. The income gains appear to be equally spread across the fiscal year for the 1988 to 1994 sample. For the 2001 to 2004 sample the income gains actually appear to occur primarily during the first two quarters of the fiscal year. While the results are not estimated, they raise serious questions about the validity of interpreting any of the estimated effects of mid-year outlay changes on income as unbiased estimates of fiscal policy multipliers.

Table 8 completes the investigation by re-estimating the specifications reported in Table 5, but with the inclusion of state income during the first quarter of the fiscal year as a control. This is intended as a way to control for differences in the performance of state economies prior to the portion of the fiscal year during which balanced budget requirements would induce a divergence in fiscal policy. As expected, given the results from Table 7, the inclusion of this control tends to substantially reduce the size of the multiplier estimates. The estimates presented in Table 8 would, if anything, be relatively consistent with the hypothesis that state government spending crowds out contemporaneous private activity.

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6. Conclusion

We have studied the behavior of state governments over the course of the business cycle. A key feature of this behavior is the substantial pro-cyclicality of expenditures on capital infrastructure and service provision. These expenditures tend to scale up and down proportionately with the size of the state's economy. When state income is one dollar below trend, these non safety net expenditures are, on average, 9 cents below trend.

The pace of state adjustment to fiscal shocks is driven in part by the stringency of their balanced budget requirements. Unfortunately, our exploration of this source of variation in fiscal policy did not yield reliable estimates of fiscal policy multipliers. The shocks were insufficiently large to generate a reasonable degree of precision. We showed further that the second stage results are sensitive to several important specification checks. Most notably, this included sensitivity to efforts to account for bias due to the relatively extensive reliance of states with weak budget rules on personal income taxation.

Analysis of fiscal policy has produced a literature in which, even when an econometric strategy appears robust, results tend not to differentiate between theories of the business cycle (and by extension of stabilization policy) with significant statistical power. A neutral prior for analyses of the spending multiplier is one. A multiplier of one implies neither Keynesian crowding in nor Ricardian crowding out of private sector activity. The null of one (or, for that matter, other uncontroversial priors) could rarely be rejected in the literature, raising questions regarding how much we have learned

empirically about the absolute size of fiscal policy multipliers. Advances in this literature have tended to be methodological and qualitative (or conceptual) in nature.

We show that variation in sub-national fiscal policy will not, in general, yield estimates that directly support predictions of the effects of stabilization policy. When variation stems from changes to the distribution of federally financed expenditures, the financing burden is shared by treatment and control states alike. Consequently, any adverse effects of deficit financing (including reductions in consumption due to wealth effects and reductions in investment due to expected increases in interest rates) will net out to zero within the estimation framework. These sources of variation provide direct evidence on government's ability to increase economic activity in one region relative to another. They do not, however, demonstrate the effect of stabilization policy on activity aggregated across the fiscal union.

Direct tests of the effects of stabilization policy require differences in the savings behavior of similarly situated governments. Fiscal institutions such as balanced budget requirements, stabilization funds, and tax and expenditure limits are potentially attractive drivers of the needed differences in savings behavior. The shortcomings of the strategy investigated here are unfortunate because it had the potential to yield estimates with desirable applications. They would apply most directly to estimating the effect of deficit-financed stabilization policy conducted at the state level. This would, in turn, provide a platform for extrapolating towards estimates of the effects of such policies at the national level.[26] They could also be used to estimate the impact of pro-cyclical state

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[26] The recent fiscal policy literature, and work going back to Gramlich (1987), have dwelt extensively on potential differences between multipliers at the national and sub-national levels, highlighting issues

expenditures on the volatility of the business cycle, with potential for estimating this cyclicality's welfare costs.[27] The relationship between fiscal institutions and stabilization policy should thus remain an active area of research.

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associated with "consumption leakages" and the implications of labor mobility across state borders. Consumption leakages would tend to reduce sub-national multipliers relative to national multipliers while factor movements could have the opposite effect.

[27] Estimates of the welfare costs of business cycles vary widely within the literature. Seminal work by Lucas (1987) arrived at a very small estimates of these costs, while more recent work (e.g., Krusell et al, 2009, and Chauvin, Laibson, and Mollerstrom 2009), has arrived at estimates equal to or in excess of 1 percent of all future consumption. It is beyond the scope of this paper to determine precisely how state government spending feeds into the mechanisms associated with some of the welfare costs highlighted in this literature (e.g., the asset bubbles studied by Chauvin, Laibson, and Mollerstrom).

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Figure 1: Detrended Non Safety-Net Outlays and Personal Income
Means across States: 1960-2006
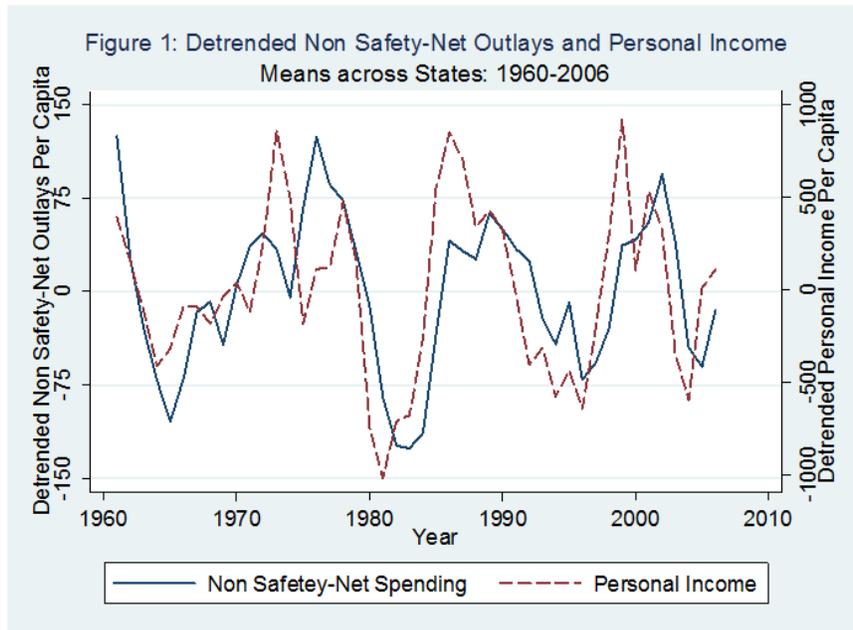
Note: Figure 1 plots the unweighted means (across states) of de-trended personal income and state government spending outside of insurance trusts and safety-net programs on a per capita basis. Detrending was conducted using state-specific quartic polynomials. Personal income data come from the Bureau of Economic Analysis (BEA) and state government spending data come from the Census of Governments (COG).

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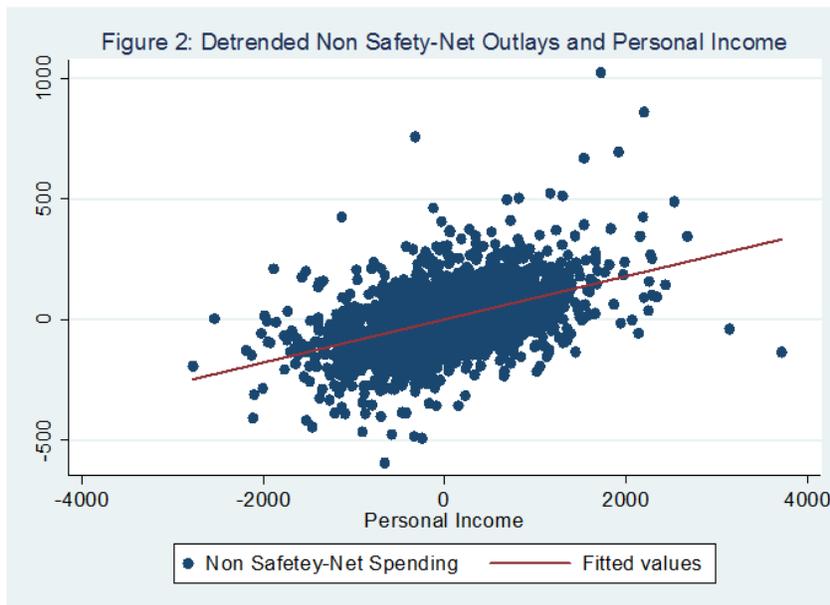
Figure 2 plots state-year observations of de-trended personal income and state government spending outside of insurance trusts and safety-net programs on a per capita basis. The best-fit line has a slope of 0.089 (standard error of 0.014) and the regression yields an r-squared of 0.22. Detrending was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

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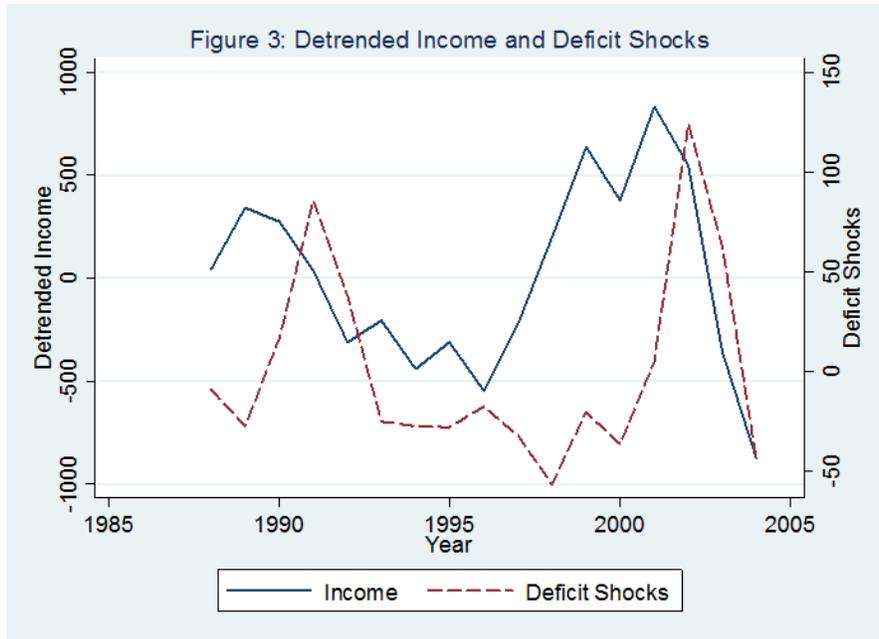
Note: Figure 3 graphs deficit shocks per capita and de-trended personal income per capita. The deficit shocks were constructed using data from semi-annual reports by the National Association of State Budget Officers (NASBO). Personal income data come from the BEA.

Table 1

Summary Statistics: Strict vs. Weak Budget Rules States (1988-2004)

Variable	Mean	Std. Dev.	Mean	Std. Dev.
	Strict		Weak	
Fiscal Variables ($ per capita)				
State General Fund Expenditures	1612	498	1926	650
State General Fund Revenues	1620	503	1925	657
Total Taxes as Share of Gen. Rev.	0.51	0.05	0.55	0.06
Pers. Inc. Taxes as Share of Gen. Rev.	0.19	0.07	0.24	0.05
DEFSHOCK	-5	89	13	98
ΔREVENUE	9	51	8	70
ΔOUTLAYS	-14	31	-13	30
Economic Variables				
Personal Income ($ per capita)**	26699	4414	32342	5139
Employment per capita	0.43	0.04	0.43	0.03
Demographic Variables				
State Population	3264722	2070425	12100000	8974343
Drop Out Fraction	0.18	0.04	0.17	0.03
High School Grad Fraction	0.27	0.03	0.27	0.04
Some College Fraction	0.18	0.04	0.17	0.03
College Plus Fraction	0.15	0.03	0.17	0.04
Medicaid Fraction	0.10	0.04	0.11	0.03
Senior Fraction	0.12	0.02	0.12	0.02
Child Fraction	0.28	0.03	0.27	0.02
Observations	317		131	

Note: ** and * indicate statistically significant differences between the means for weak- and strong-budget rule states at the .01 and .05 levels respectively. The combined sample includes data on the 27 annually budgeting states listed in Table 2 for the period 1988-2004. Several additional observations are unavailable due to incomplete data reporting, leaving a final sample of 448 observations. Columns 1 and 2 report data for the states identified in Table 2 as having "Strong" budget rules while columns 3 and 4 report data for states identified in Table 2 as having "Weak" or "Medium" budget rules. Fiscal variables were generated using data from the Census of Government's (COG) *Annual Survey of State and Local Government Finances* and the National Association of State Budget Officer's (NASBO) semi-annual series *Fiscal Surveys of the States*. Income data come from the Bureau of Economic Analysis, employment data come from the Bureau of Labor Statistics, and demographic data come from the March Demographic Supplements to the Current Population Survey.

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. 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 5 or 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.

Table 3

State Responses to Deficit Shocks: 1988-2004

	(1)	(2)	(3)	(4)	(5)	(6)
	ΔOutlays	ΔRevenues	ΔOutlays	ΔRevenues	ΔOutlays	ΔRevenues
DEFSHOCK*1{DEFSHOCK > 0}	-0.283**	.075**	-0.382**	.080*	-0.383**	0.080*
	(0.062)	(0.023)	(0.049)	(0.031)	(0.049)	(0.032)
DEFSHOCK*1{DEFSHOCK < 0}	0.003	0.023	0.027	0.0021	0.028	0.021
	(0.016)	(0.012)	(0.017)	(0.017)	(0.018)	(0.017)
Weak Rules*DEFSHOCK*1{DEFSHOCK > 0}			0.256**	-0.015	0.203**	-0.010
			(0.061)	(0.033)	(0.045)	(0.030)
Weak Rules*DEFSHOCK*1{DEFSHOCK < 0}			-0.057	0.004	-0.043	0.012
			(0.034)	(0.232)	(0.042)	(0.030)
Medium Rules*DEFSHOCK*1{DEFSHOCK > 0}					0.291**	-.016
					(0.078)	(0.040)
Medium Rules*DEFSHOCK*1{DEFSHOCK < 0}					-0.076	-0.010
					(0.034)	(0.028)
State Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes
State Specific Trends?	Yes	Yes	Yes	Yes	Yes	Yes
Year Effects?	Yes	Yes	Yes	Yes	Yes	Yes
Observations	448	448	448	448	448	448

Note: ** and * indicate statistical significance at the .01 and .05 levels respectively. Standard errors, calculated allowing for arbitrary correlation at the state level, are in parentheses beneath each point estimate. The sample is as described in the note to Table 1. The classifications of budget rules are as described in the note to Table 2. The measure of deficit shocks was constructed using data from NASBO as described in the text. ΔOutlays and ΔRevenues are mid-year changes to outlays and revenues. ΔOutlays is reported directly by NASBO while ΔRevenues is constructed using NASBO's reports of changes in tax law that were enacted by the states during each fiscal year. All fiscal variables are expressed in terms of 2004 dollars per capital.

Table 4

First Stage Regressions: Period by Period

	(1)	(2)	(3)	(4)	(5)	(6)
	ΔOutlays	ΔRevenues	ΔOutlays	ΔRevenues	ΔOutlays	ΔRevenues
	1988-1994		*1995-2000*		*2001-2004*	
DEFSHOCK*1{DEFSHOCK > 0}	-0.447**	0.146*	-0.068	-0.038	-0.404**	0.066
	(0.056)	(0.058)	(0.086)	(0.045)	(0.099)	(0.060)
DEFSHOCK*1{DEFSHOCK < 0}	0.074	-0.039	-0.023	0.019	0.167	0.112
	(0.051)	(0.039)	(0.022)	(0.040)	(0.128)	(0.084)
Weak Rules*DEFSHOCK*1{DEFSHOCK > 0}	0.332**	-0.075	-0.951	-0.119	0.434**	-0.009
	(0.077)	(0.071)	(0.656)	(0.138)	(0.111)	(0.075)
Weak Rules*DEFSHOCK*1{DEFSHOCK < 0}	-0.046	0.046	0.072	0.175	-0.289	-0.071
	(0.051)	(0.044)	(0.075)	(0.109)	(0.154)	(0.086)
State Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes
State Specific Trends?	Yes	Yes	Yes	Yes	Yes	Yes
Year Effects?	Yes	Yes	Yes	Yes	Yes	Yes
Observations	182	182	160	160	106	106

Note: ** and * indicate statistical significance at the .01 and .05 levels respectively. Standard errors, calculated allowing for arbitrary correlation at the state level, are in parentheses beneath each point estimate. The sample is as described in the note to Table 1. Specifications are equivalent to those estimated in columns 3 and 4 of Table 3, but with the samples restricted to the years listed in the current table's column headings.

Table 5

Initial Estimates of the Effects of Mid-Year Outlay Changes on State Income

	(1)	(2)	(3)	(4)
	Personal Income	Personal Income	Personal Income	Personal Income
Sample	1988-1994		2001-2004	
ΔOUTLAYS	2.33	0.46	1.60	1.20
	(1.83)	(2.20)	(0.88)	(1.20)
DEFSHOCK*1{DEFSHOCK > 0}	1.30	0.70	1.01	1.19
	(0.81)	(0.70)	(0.33)	(0.40)
DEFSHOCK*1{DEFSHOCK < 0}	-0.24	1.77	0.25	0.59
	(0.49)	(0.55)	(0.37)	(0.92)
State Fixed Effects?	Yes	Yes	Yes	Yes
State-Specific Trends	Yes	No	Yes	No
Year Effects?	Yes	Yes	Yes	Yes
Observations	182	182	106	106

Note: This table contains results from the second stages of IV regressions of personal income on mid-year budget cuts. The sample used in columns 1 and 2 is the same as the sample used in columns 1 and 2 of Table 4. The sample used in columns 3 and 4 is the same as the sample used in columns 5 and 6 of Table 4. Standard errors, calculated allowing for arbitrary correlation at the state level, are in parentheses beneath each point estimate. The excluded instruments in all specifications are the interactions between an indicator for weak budget rules and the main effects of the two deficit shock variables.

Table 6

Impact of Controlling for the Share of Each State's Revenues Generated by Taxation

	(1)	(2)	(3)	(4)
	Personal Income	Personal Income	Personal Income	Personal Income
Sample	1988-1994		2001-2004	
ΔOUTLAYS	0.64	0.29	2.98	2.98
	(2.77)	(3.87)	(1.02)	(1.87)
State Fixed Effects?	Yes	Yes	Yes	Yes
State-Specific Trends	Yes	No	Yes	No
Year Effects?	Yes	Yes	Yes	Yes
Observations	182	182	106	106

Note: This table contains results from the second stages of IV regressions of personal income on mid-year budget cuts. The sample used in columns 1 and 2 is the same as the sample used in columns 1 and 2 of Table 4. The sample used in columns 3 and 4 is the same as the sample used in columns 5 and 6 of Table 4. Standard errors, calculated allowing for arbitrary correlation at the state level, are in parentheses beneath each point estimate. The excluded instruments in all specifications are the interactions between an indicator for weak budget rules and the main effects of the two deficit shock variables. The specifications also included controls for interactions between an estimate of the share of a state's general revenues that come from taxation interacted with the main effects of the deficit shock variables. The tax share was estimated using the average value of total tax revenues divided by total general revenues for each state across the full 1988 to 2004 sample period. The second stage coefficients on these interactions were imprecisely estimated and instable across specifications.

Table 7

Estimates on a Quarter-by-Quarter Basis

	Personal Income, Q1	Personal Income, Q2	Personal Income, Q3	Personal Income, Q4
Panel A		1988-1994		
ΔOUTLAYS	2.47	2.30	2.10	2.44
	(2.26)	(2.41)	(1.43)	(2.66)
Panel B		2001-2004		
ΔOUTLAYS	2.90	3.44	-1.11	1.18
	(0.87)	(1.97)	(3.95)	(3.46)
State Fixed Effects?	Yes	Yes	Yes	Yes
State-Specific Trends	Yes	Yes	Yes	Yes
Year Effects?	Yes	Yes	Yes	Yes

Note: This table contains results from the second stages of IV regressions of personal income on mid-year budget cuts. The specifications reported across Panel A are equivalent to the specification reported in column 1 of Table 5, but with the dependent variable representing personal income for a single quarter of the fiscal year (as labeled in the column headings). Panel B was constructed similarly, but with the specifications being equivalent to those from Table 5's column 3.

Table 8

Impact of Controlling for Income in the First Quarter of Each State's Fiscal Year

	Personal Income	Personal Income	Personal Income	Personal Income
Sample	1988-1994		2001-2004	
ΔOUTLAYS	0.41	-0.47	1.83	-.44
	(0.53)	(0.69)	(1.62)	(0.91)
State Fixed Effects?	Yes	Yes	Yes	Yes
State-Specific Trends	Yes	No	Yes	No
Year Effects?	Yes	Yes	Yes	Yes
Observations	182	182	106	106

Note: This table contains results from the second stages of IV regressions of personal income on mid-year budget cuts. The sample used in columns 1 and 2 is the same as the sample used in columns 1 and 2 of Table 4. The sample used in columns 3 and 4 is the same as the sample used in columns 5 and 6 of Table 4. Standard errors, calculated allowing for arbitrary correlation at the state level, are in parentheses beneath each point estimate. The excluded instruments in all specifications are the interactions between an indicator for weak budget rules and the main effects of the two deficit shock variables. The specifications also included a control for the level of personal income during the first quarter of each fiscal year.