Does it Cost to be Virtuous? The Macroeconomic Effects of Fiscal Constraints

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First version, March 2004
This revision, June 2004

Abstract

We study whether and how fiscal restrictions alter volatilities and correlations of macrovariables for a sample of 48 US states. We also examine the "typical" transmission properties of fiscal disturbances and the implied fiscal rules of states with different fiscal restrictions. Fiscal constraints are characterized with a number of indicators. There are economic and statistical similarities in second moments and in the transmission properties of fiscal shocks across states with different fiscal constraints. The cyclical response of expenditure differs in size and sometimes in sign, but heterogeneity within groups makes point estimates statistically insignificant. Implications for the reform of the Growth and Stability Pact are discussed.

JEL classification numbers: E3, E5, H7

Key words: Budget restrictions, Fiscal policy transmission, Policy Rules, Dynamic Panels

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1 Introduction

The size of government deficits and the time path of debt are of central importance in the political discussions that shape economic policies in OECD countries. For example, in the US active fiscal policymaking has been limited by frequent disputes between the President and the Congress on how to reduce federal deficits. In Europe, the reform of the Growth and Stability Pact (GSP) has been a topic of intense debates in the last few years. In the past, membership to the EMU strongly depended on deficit policies, but initially virtuous countries such as France, Germany, and the Netherlands have joined ranks with initially less virtuous ones like Italy, Portugal and Greece in passing the upper bound set for the deficit to GDP ratio in 2003. Furthermore, in some of these countries, the net-of-interest debt to GDP ratio started growing again after the decline of the late 1990's. The implications of fiscal policy decisions for the maintenance of monetary stability have attracted the attention of central bankers and academics have started investigating how exuberant fiscal policy may affect local and union wide prices (see e.g. Canova and Pappa (2003)).

While the GSP is somewhat more flexible than the constitutional balance budget amendment recently proposed in the US, it has been criticized on a number of grounds. Critics often stress that fiscal constraints limit the ability of governments to react to fluctuations in the local economy. Two undesirable consequences may result. First, since government capability to stabilize the economy is reduced, the volatility of macrovariables could be increased. Second, since expenditures must follow the revenue cycle, budget restrictions may make expenditure procyclical. Hence, tight budget constraints may amplify fluctuations, turning slowdowns into deep recessions.

Despite the popular appeal of this argument, Canzoneri, Diba and Cumby (2002) suggest that fiscal policy in the US and Europe has hardly focused on macroeconomic stabilization over the last two decades. Two complementary reasons may account for this. First, given the lags in the legislative process, discretionary fiscal policy may be unable to counteract business cycle fluctuations. Second, since automatic stabilizers are roughly given at business cycle frequencies, and since their share in total expenditure is typically large, also the non-discretionary component of expenditure cannot vary substantially over the cycles. Hence, limiting fiscal actions cannot dramatically alter the magnitude and the shape of cyclical fluctuations.

Supporters of fiscal restrictions, on the other hand, suggest that the medium term benefits of limiting government actions dominate the short run costs incurred by the inability of fiscal policy to react to business cycle conditions (see e.g. Diaz Gimenez, et al. (2003), Andres and Domenec

(2002)). This argument is usually based on two principles. First, by limiting the ability of governments to run politically motivated deficits and unsustainable levels of debt, fiscal constraints make governments more credible, reduce the suboptimality of political games, and induce a smoother path for taxes, which is the optimal tax policy to follow in a number of theoretical models (see e.g. Alesina and Perotti (1996)). Second, since fluctuations in expenditure may have been themselves a source of undesirable fluctuations, restraining fiscal policy may actually stabilize the economy.

As for the first principle, the literature has made an important distinction between flexible rules, which allow for some sensitivity of deficit and debt to economic conditions, or apply to consumption but not to investment and infrastructure expenditures, and strict ones. On the other hand, the evidence on the contribution of fiscal shocks to macroeconomic fluctuations is contradictory. Standard dynamic general equilibrium models of fiscal policy (see e.g. King and Baxter (1993), Duarte and Wolman (2002) or Gali, Lopez Salido and Valles (2003)) have hard time to produce sizable fluctuations in response to fiscal disturbances in closed economy models calibrated to match salient features of OECD business cycles. Empirically, Perotti (2004), Mountford and Uhlig (2002) and Canova and Pappa (2003) have shown that expenditure shocks can at times produce economically significant output and employment multipliers.

Critics and supporters of the GSP however do agree on one fact: deficits and debts have distributional effects which may have long lasting repercussions in the economy. Borrowing, for example, reduces resources available to future generations and, if it is used to finance public consumption, it may induce a misallocation of resources. Therefore, if a reform of the GSP will be in the agenda of the EU, a careful balance of incentives and constraints, including intratemporal and intertemporal considerations, needs to be designed.

While there is evidence that fiscal restraints have provided some safeguard against the misuse of public funds (see e.g. Poterba (1994) and Bohn and Inman (1996); Von Hagen (1990) has an opposite view), very little is known about the macroeconomic consequences of imposing fiscal constraints. Gali (1994), Gali and Perotti (2003), Fatas and Mihov (2003), Lane (2003) and Sorensen, Wu and Yosha (2001) have examined some aspects of the relationship between fiscal variables and the macroeconomy, but to the best of our knowledge, no empirical study has simultaneously studied whether fiscal constraints alter (i) the business cycle features of macroeconomic variables, (ii) the transmission properties of fiscal shocks and (iii) the fiscal rules that governments follow. We can think of several reasons for why the literature has neglected this topic. First, it is difficult to find case studies where tight fiscal constraints have been imposed in countries which originally had no fiscal restrictions. Second, over the cross section, countries which have loose deficit restrictions typically have tighter debt constraints. Third, fiscal disturbances are difficult to identify since the systematic and the unsystematic component of policy are highly intertwined and "surprises" may induce macroeconomic changes before they are implemented. Fourth, fiscal rules may be subject to predictable changes at election times or at times of political turmoil. Last, but not least, cross-country data is typically short and hard to obtain at the quarterly frequency.

This paper studies how fiscal constraints affect the macroeconomy using data from 48 US states for the sample 1969-1995. First, we examine whether fiscal constraints alter the volatility and the comovements of state macroeconomic variables, grouping states with a number of indicators capturing different aspects of existing fiscal restrictions. Second, we examine the transmission properties of government expenditure disturbances (for two types of expenditure, one financed by debt and one by distortionary taxation) for a typical state with loose or strict fiscal restrictions. Finally, we back out the typical expenditure rules (one for each of the two shocks) for states with different fiscal restrictions and compare them. We use both asymptotic and small sample tests to measure the statistical significance of the difference in the statistics across groups and corroborate the analysis by evaluating the economic consequences of the differences we found.

Why use US states to assess the possible consequences of reforming the GSP? The EMU experience is unfortunately too short to shed any light on the relative merit of the two positions. The US experience, on the other hand, provides a natural laboratory to evaluate the consequences of weakening or strengthening the GSP for several reasons. First, as shown by Canova and Pappa (2003), the response of macroeconomic variables to fiscal shocks in the two monetary unions share a number of important similarities. Second, the cross section of US states is rich enough to include cases where rules are strict, others where they are somewhat loose and one case where no fiscal restrictions are in place (e.g. Vermont). Third, the available data covers a sufficiently long span of time (27 years) including both expansionary and recessionary periods.

Our results indicate that the macroeconomic consequences of fiscal constraints have been overemphasized. While point estimates and, at times, the sign of the statistics we compute for states with strict fiscal constraints differ from those of states with loose fiscal constraints, differences are statistically insignificant and, often, economically unimportant. This result holds regardless of how we define "loose" or "strict", of whether deficit, debt or institutional constraints are examined, of the type of statistical tests we employ and, to a large extent, the statistics we consider. For example, standard second moments that the literature has used to characterize business cycle fluctuations are similar in states with loose and strict restrictions. Likewise, reduced form macroeconomic relationships such as the comovements between the size of the government and the cyclicality of expenditure, or the volatility of business cycle and the cyclicality of expenditure are similar, on average, in the two groups of states. Furthermore, fiscal restrictions have little impact both qualitatively and quantitatively on how fiscal disturbances are transmitted to the real economy. Finally, the systematic response of expenditure to macroeconomic variables suggests that fiscal restrictions may not necessarily alter the ability of the government to respond to the state of the economy and only marginally explain the differences in fiscal rules across US states.

Why is it that fiscal constraints appear to make little macroeconomic difference? Do the results imply that the GSP should be scrapped or reinforced? Are EU politicians right when they disregard the warnings of Bruxelles about the dangers of exceeding deficit limits? To avoid unwarranted conclusions, several points should be stressed. First, it is well known that strict fiscal constraints are partially undone by creative accounting practices and by the presence of rainy days funds. Given that constraints apply only to a portion of the total budget, that no formal provision for the enforcement of the constraints exist and that rainy days funds play a buffer-stock role, it is not surprising to find that the macroeconomic consequences of the two regimes are not too far apart. Second, while the presence of strict fiscal constraints do not make an important difference for cyclical fluctuations, it is worth stressing that some fiscal restriction is present in all but one US states. If constraints are imposed to keep government behavior under control, tight restrictions may be the wrong way to go since they simply imply more creative accounting practices. That is to say, tight fiscal constraints are neither a necessary nor a sufficient condition for good government performance. On the other hand, if constraints are imposed to limit default probabilities or to reduce the effects that government spending has on average area wide inflation, tighter constraints could be envisioned Third, in the US, labor markets are sufficiently flexible, people move and other margins (such as relative prices) adjust to absorb macroeconomic shocks. Because Europe is very different in this respect, the imposition of tighter fiscal restrictions in the EMU may have completely different implications. Furthermore, since fiscal constraints in the US almost always exclude capital account expenditures, care should be exercised to extend the conclusions to situations where non-golden rule type of constraints are in place. Fourth, since our analysis has concentrated on business cycle issues, we can not exclude that distributional and long run effects could be different and significantly so in states where tight fiscal constraints are binding. Finally, while they may have little effects in "normal " times, fiscal constraints may have a much larger impact if imposed when runaway inflation or debt threaten macroeconomic stability.

The rest of the paper is organized as follows. The next section describes the model used in the exercises, explains our empirical methodology and compares it with those typically used in the literature. Section 3 presents the procedure used to identify fiscal shocks and to construct fiscal rules. Section 4 describes how indicators capturing deficit and debt restrictions are constructed. Section 5 presents the results and section 6 concludes.

2 The model and the methodology

The results presented in this paper are primarily obtained using VARs. While unconditional volatilities and correlations can be obtained without a VAR, we use such a model also for these statistics to unify our empirical analysis which also examines conditional comovements and distinguishes between systematic and unsystematic fiscal policy actions.

We have gathered annual data for 48 US states (DC, Alaska and Hawaii are excluded) for the period 1969 to 1995. The relative shortness of the data prevents us not only to study the transmission of shocks across states but also the estimation of a model which simultaneously includes a number of state and union wide variables. Given these limitations, we are forced to neglect possible neighborhood effects and choose, for each unit, five endogenous variables, four exogenous variables and a constant. The endogenous variables are: the log of the state to the union wide price level; the log of the state to the union wide real per-capita output; the log of the state to the union wide employment level; the log of state real government revenues and the log of state real government consumption expenditure, both in per-capita terms and deflated by state prices. Scaling state variables by their union wide level catches two birds with one stone: it transforms trending variables into stationary ones and it allows to directly control for fluctuations which are aggregate in nature. Note that we use total state and local expenditure to take into account possible off-budget activities where expenditures are shifted to less restricted part of the government whenever constraints become binding. The exogenous variables we employ are the area-wide nominal interest rate, the level of oil prices, the Federal aid to the states and the state debt to output ratio. The first three variables are used to control for aggregate area-wide supply and demand effects; local debt enters the specification following the suggestions of the fiscal theory of the price level (see Christiano and Fitzgerald (2001) for a survey), and the work of Canova and Pappa (2003). State debt includes both guaranteed and non-guaranteed debt to capture possible substitution effects induced by debt limits. The sources of the data and the exact definition of the variables are in the appendix. The Schwarz criteria indicates that only one lag of the endogenous variable suffices to capture the dynamics and exogenous variables are allowed to enter only contemporaneously in the system, except for debt which enters with one lag [1].

The literature has typically employed a two-stage strategy to analyze the effects that unit specific characteristics have on the dynamics of government finances, on the probability of (large) deficits and, in general, on the relationship between government expenditure and macroeconomic activity. In the first stage the time series dimension is employed to extract the information on relevant parameters and, in the second stage, the cross sectional dimension is used to explain the heterogeneity in estimated parameters using unit specific political, institutional or economic characteristics. For example, Bohn and Inman (1996) run a static first stage time series regression of the type $y_{it} = \varrho_i + \alpha x_{it} + e_{it}$ for each state, where $e_{it} \sim (0, \sigma_i^2)$, y_{it} is the state surplus and x_{it} a vector of macrovariables including output, employment, etc., and then run a cross sectional regression $\hat{\varrho}_i = z_i \gamma + v_i$ where z_i are observable state characteristics. Sorensen, Wu and Yosha (2001), Lane (2003) and Fatas and Mihov (2003), on the other hand, run a first stage regression of the type $y_{it} = \varrho_i + \alpha_i \Delta x_{it} + e_{it}$ where y_{it} is the budget surplus, the expenditure to output ratio, the revenue to output ratio or transformations of them, Δx_{it} includes contemporaneous, or contemporaneous and lagged macroeconomic variables and then attempt to explain differences in $\hat{\alpha}_i$ (or in $\hat{\sigma_i}$) with cross sectional regressions of the type $\hat{\alpha}_i = z_{1i}\gamma + v_i$ or $\hat{\sigma_i} = \sigma_0 + z_{2i}\delta + v_i$, where z_{1i} could be different than z_{2i}. While popular, these two-stage procedures produce incorrect estimates of γ or δ. In addition, it is hard to predict the direction of bias without knowing exactly what is the data generating process of the cross sectional dimension of the panel.

Intuitively, there are three problems. First, specifications like those of Bohn and Inman (1996) neglect slope heterogeneity: α_i may be different from α_j if unit i and j regressors are correlated with individual characteristics (which is likely to be the case if, e.g., x_{it} includes output and z_i labor market or other national regulations). Neglecting slope heterogeneities produces biased and inconsistent estimates of α and, given the structure of the resulting error term, an instrumental variable (IV) approach is unlikely to solve the inconsistency problem (see e.g. Pesaran and Smith (1995)). Second, specifications which allow for slope heterogeneities but exclude lagged dependent variables, like Sorensen, Wu, Yosha (2001), or Lane (2003), omit regressors which are, by construction, corre-

[1] We have examined variants of the model using e.g., revenues and expenditures measured in percentage of GSP; GSP per-capita and employment not scaled by union wide averages; state variables in growth rates (but not per-capita terms) and implicit price deflators instead of CPI prices. We have also run a model where instead of fiscal variables we use the residual of a preliminary regression of these variables on either union wide variables or the variables of the region where the state is located. The results we present are qualitatively invariant to all of these changes.

lated with the included ones whenever Δx_{it} is serially correlated. Lagged dependent variables are likely to be important in the first stage regression because all fiscal variables are serially correlated. Omission of lags of the left hand side variable produces biased and inconsistent estimates of the first stage parameters and therefore renders second stage regressions meaningless. Also in this case, an IV approach is unlikely to work since it is difficult to find instruments which effectively break the correlation between the regressors and the errors. Third, even when slope heterogeneity is accounted for and lagged dependent variables are included in the first stage regression (as in Fatas and Mihov (2003)), second stage estimates neglect the fact that α_i (or σ_i) have been estimated. Hence, estimates of γ (δ) may be artificially significant.

To illustrate these problems consider the model

$$y_{it} = x_{0it}\varrho_i + x_{1it}\alpha_i + e_{it} \quad (1)$$

$$\alpha_i = x_{2i}\gamma + v_i \quad (2)$$

where $i = 1, 2, \ldots, N$, x_{1it} is a $1 \times K_2$ vector of exogenous and lagged dependent variables, x_{2i} is a $K_2 \times K_3$ vector of time invariant unit specific characteristics, x_{0it} is a $1 \times K_1$ vector of unit specific variables (possibly depending on t) and γ is a $K_3 \times 1$ vector of parameters. We assume that $E(x_{1it}e_{it}) = E(x_{2i}v_i) = 0$ that $e_{it} \sim N(0, \sigma_i^2)$; that $E(e_{it}, e_{i'\tau}) = 0 \ \forall \ t \neq \tau$ and $i \neq i'$; and $v_i \sim N(0, \Sigma_v)$. Stacking the observations for each i and using (2) into (1) we get $y_i = x_{0i}\varrho_i + X_i\gamma + \epsilon_i$ where $X_i = x_{1i}x_{2i}$ is a $T \times k_3$ matrix, and $\epsilon_i = x_{1i}v_i + e_i$ so that $\text{var}(\epsilon_i) = x_{1i}\Sigma_v x'_{1i} + \sigma_i^2 I \equiv \Sigma_{\epsilon_i}$.

Given Σ_{ϵ_i} and γ the maximum likelihood estimator of ϱ_i is $\varrho_{i,ML} = (x'_{oi}\Sigma_v^{-1}x_{0i})^{-1}(x'_{oi}\Sigma_v^{-1}(y_i - X_i\gamma)$ and conditional on Σ_{ϵ_i}, the maximum likelihood estimator of γ is $\gamma_{ML} = (\sum_i X_i\Omega_i^{-1}X_i)^{-1}(\sum_i X_i\Omega_i^{-1}y_i)$ where $\Omega_i^{-1} = \Sigma_{\epsilon_i}^{-1} - \Sigma_{\epsilon_i}^{-1}x_{0i}(x_{0i}\Sigma_{\epsilon_i}^{-1}x_{0i})^{-1}x'_{0i}\Sigma_{\epsilon_i}^{-1}$. After some algebraic manipulations one obtains $\gamma_{ML} = (\sum_i x'_{2i}\mathcal{P}_i^{-1}x_{2i})^{-1})(\sum_i x'_{2i}\mathcal{P}_i^{-1}\hat{\alpha}_i)$ where $\mathcal{P}_i = (x'_{1i}x_{1i})^{-1}\Omega_i$. Hence, γ is a weighted average of the first stage estimates $\hat{\alpha}_i$ with weights given by $\mathcal{P}_i$.

When a two-step approach is used, second stage estimates of γ are $\gamma_{2step} = (\sum_i x'_{2i}\Sigma_v^{-1}x_{2i})^{-1}(\sum_i x'_{2i}\Sigma_v^{-1}\hat{\alpha}_i)$. Therefore γ_{2step} incorrectly measures the effect of x_{2i} on α_i for two reasons. First, suppose that $x_{i0t} = 0$, $\forall t$. Then the term $\sigma^{-2}(x'_{1i}x_{1i})$ is missing from the formulas of γ_{2step} and of its standard error $(\sum_i x'_{2i}\Sigma_v^{-1}x_{2i})^{0.5}$. This means that, while the weights used in γ_{2step} depend on Σ_v, those in γ_{ML} depend on Σ_i and on the volatility of the unit specific regressors $\sigma^{-2}(x'_{1i}x_{1i})$. Second, if $x_{i0t} \neq 0$, there are additional terms in Ω_i, measuring the influence that these regressors have on $\hat{\alpha}_i$, which are left out from γ_{2step}. Since the standard error of γ_{2step} is underestimated, a two-step regression gives an overoptimistic representation of the significance of the relationship. Moreover, if α_i is systematically larger when x_{1i} is more volatile, a positive γ_{2step} may be obtained even when the true effect is negative. These observations should be kept in mind when comparing our results with those existing in the literature. In fact, our methodology takes care of all of these problems. First, lagged dependent variables appear in the model for each state. Second, we allow for heterogeneity in regression coefficients and variances across units. Third, we construct maximum likelihood estimates of γ by plugging $\hat{\Sigma}_v = \frac{1}{N-1}\sum_{i=1}^{N}(\hat{\alpha}_i - \frac{1}{N}\sum_{i=1}^{N}\hat{\alpha}_i)(\hat{\alpha}_i - \frac{1}{N}\sum_{i=1}^{N}\hat{\alpha}_i)'$ and $\hat{\sigma}_i^2 = \frac{1}{T-dim(\alpha_i)}(y_i'y_i - y_i x_i \hat{\alpha}_i)^2$ into the relevant formulas. Our estimators are consistent when the number of units in each group is large (see e.g. Pesaran and Smith (1995)) and reproduce the random coefficient Bayesian estimators, when uninformative priors are used.

Since in our case x_{2i} are dichotomous variables, implementing γ_{ML} is equivalent to calculating the "typical" effect in states with loose and strict restrictions. Then the equality of the statistics across groups can be examined using asymptotic χ^2-tests or non-parametric devices (such as a small sample rank sum test).

3 Identifying Fiscal shocks

To examine the transmission of expenditure shocks and the systematic response of expenditure to macroeconomic fluctuations we need to identify fiscal shocks. Such an enterprise is typically complicated and this may explain why only a small number of studies have engaged in such an activity (see e.g. Ramey and Shapiro (1998), Edelberg, Eichenbaum and Fisher(1999), Mountford and Uhlig (2002), Blanchard and Perotti (2002), Burnside, Eichenbaum and Fisher (2002), Perotti (2002), Canova and Pappa (2003), Pappa (2004)).

Three features make fiscal shocks difficult to extract. First, fiscal policy is rarely unpredictable. A fiscal change is usually subject to long discussions and political debates before it is implemented. These delays make standard innovation accounting problematic: agents adjust their behavior to the new conditions when the old regime still prevails; macrovariables start moving before the shock occurs and no surprise is measurable at the time when the policy change actually takes place. This "non-fundamentalness" problem plagues fiscal shocks more than other types of policy disturbances. Second, even when the policy stance is unchanged, expenditures and revenues move with the state of the economy. Therefore, it is necessary to carefully distinguish exogenous policy shifts from endogenous reactions to business cycle conditions. Third, since fiscal and monetary policy actions may be related, identifying fiscal shocks in isolation may produce misleading results.

Our set up is designed to avoid, in principle, all these problems. First, because we consider a monetary union, we take monetary policy as given when examining state fiscal policy. We do this by imposing the exogeneity of the economy wide interest rate with respect to state variables. Second, since in the VAR all variables are endogenous and since we control for both the state of the local and of the aggregate business cycle, there is no need to produce cyclically adjusted estimates of fiscal variables. Third, since we precisely define the type of fiscal disturbances we are looking for and the timing of the responses of the endogenous variables is largely unrestricted, the non-fundamentalness problem is also considerably eased. In particular, we seek for expenditure shocks that produce positive comovements in states deficit and in state output(G); and shocks that leave state deficit unchanged and generate negative comovements with state output (BB).

The first type of expenditure shocks is the one usually encountered in macroeconomic text-books and in dynamic RBC models (see e.g. Baxter and King (1993) or Aiyagari, Christiano and Eichenbaum (1992)): an unexpected increase in spending, financed by bond creation increases, by definition, state deficits and boosts aggregate demand and output. In identifying this type of shocks we are agnostic about the behavior of revenues: they are allowed to stay unchanged or commove with expenditure as long as the correlation is not perfect. We are also agnostic about the timing of output responses (they could be contemporaneous, lagged or leading the shock).

The second type of shocks are budget-balanced shocks: expansionary disturbances are required to produce an instantaneous increase in revenues so as to leave state deficits unchanged, and to generate a fall in state output. These dynamics are standard in general equilibrium models of fiscal policy. For example, Baxter and King (1993) and Ohanian (1997) showed that in a RBC type model an increase in government spending, financed through labor taxation, temporarily decreases consumption and investment and has protracted negative output effects. While the sign of the output effect is robust across models, the magnitude of the fall depends on the source of financing (e.g. income taxes vs. sales taxes), on the elasticity of labor and capital supply to distortionary taxes and on whether a balance budget is imposed on a period-by-period basis or if some flexibility is allowed. Also in this case the timing of the output effect is unrestricted. Hence, anticipatory effects or future increases in distorting taxation of the type considered by, e.g., Dotsey (1994), are not a-priori ruled out. We summarize the identifying restrictions in table 1.

Table 1: Identification Restrictions

	Corr(G,Y)	Corr (T,Y)	Corr (G, Def)	Corr(T, Def)	Corr(G,T)
G shocks	> 0		> 0		≥ 0 but < 1.
BB shocks	< 0		$= 0$		$= 1$

Clearly, we do not expect G and BB shocks to be identified in all states. In theory, G shocks should be present only in those states which allow deficit carryover and BB shocks only in states with strict balance budget restrictions. However, balance budget legislation applies only to the general funds and there is no enforcement mechanism which insures that rules are not bent. Therefore, it is possible to have fiscal disturbances that look like G shocks even in states with tight balance budget rules. Conversely, debt restrictions may produce disturbances that look like BB shocks even in states with somewhat loose budgetary restrictions. Finally, one can easily conceive the situation where both type of shocks could be identified in one state (e.g. if different financing restrictions apply to different components of the budget). Rather than a-priori excluding these possibilities, we let the data tell us whether there are states which do not conform to the theoretical expectations and condition our analysis on the results of the identification exercise.

Since our identification procedure, which is based on the sign of the comovements of expenditure, deficit and output, differs from the one typically used in the VAR literature, it is useful to spend a few words highlighting the advantages of our strategy. The existing literature typically uses case study approaches, extraneous information or zero restrictions on the contemporaneous covariance matrix of VAR shocks to disentangle fiscal shocks from reduced form innovations. Case studies (see e.g. Ramey and Shapiro (1998) or Burnside, Eichenbaum and Fisher (2002)) are a powerful way to measure the effect of fiscal surprises if the changes are truly exogenous. As argued in Perotti (2004), exogeneity is dubious in two of the three typically studied episodes (Korean War, Vietnam war, Reagan buildup). The identification restrictions we use are theory based, while those employed in the literature are, to a large extent, conventional and hard to justify with low frequency data like ours. For example, assuming that it takes more than a period for government spending to respond to unexpected output movements (Blanchard and Perotti (2002)) is unappealing in annual data because of the presence of automatic stabilizers. Since we do not use zero restrictions, typical endogeneity and underidentification problems are considerably eased.

To recover shocks with the required characteristics we use the methodology of Canova and De Nicoló (2002). The approach starts from the eigenvalue-eigenvector orthogonalization of the variance covariance matrix of VAR residuals and proceeds examining the responses of the endogenous variables to each of the orthogonalized shocks. If we are unable to find expenditure shocks producing the required comovements in the variables, the eigenvalue-eigenvector decomposition is rotated by an orthonormal matrix $\mathcal{Q}(\theta)$, where θ measures the angle of rotation, and the comovements in response to the new set of shocks are examined. This search process continues, varying $\theta \in (0, \pi)$

or changing $\mathcal{Q}(\theta)$ for a fixed θ, until shocks with the required characteristics are found. [2]

4 Characterizing restrictions on government behavior

All US states, except Vermont, face some kind of deficit restrictions and the majority of them also face debt restrictions. However, deficit restrictions are at times loosely formulated; in some cases they are flexible enough to impose only weak constraints on spending behavior, and in others the debt limit is large enough to be hardly ever binding. Finally, the enforcement of budget and debt constraints varies across states. Hence, it is important to appropriately distinguish states where constraints are strict from those where they are loose.

As far as deficits are concerned, restrictions can be imposed ex-ante or ex-post. Ex-ante restrictions require the governor to present, or the legislature to approve, a balance budget. Submitting or passing a balance budget is a weak constraint since it does not exclude the possibility that, at the end of the year, the state will actually run a deficit if revenues fall below the expected values. When ex-ante restrictions are used, statutory or constitutional provisions for balancing the deficit may be used to prevent perpetual roll over into the infinite future. Therefore, the timing for balancing the budget can also serve as a constraint to induce fiscal discipline. With ex-post rules, the budget has to be balanced in each fiscal cycle (typically one, at times two years). This means that when economic activity falls short of expectations, state tax rates must be increased, expenditure cut or federal aid collected. If, despite the attempts, a deficit remains it is carried over but is required to be balanced by the end of the next year. Note that since ex-post rules apply only to the general fund balance [3], budget practices may still be unrestricted if it is possible to shift items across accounts or funds (see e.g. Poterba, 1995). Furthermore, the presence of rainy days funds, which can be accumulated in expansions and used to cushion unexpected shortfalls in revenues, may considerably ease the severeness of the constraints imposed by ex-post rules.

To account for these differences, we follow Bohn and Inman (1996), and construct three indicators capturing different aspects of deficit restrictions. In the first (Ex-ante) an entry of one is given to states where the governor must submit or the legislature must pass a balance budget and zero

[2] $\mathcal{Q}(\theta)$ is chosen from the class of rotation matrices, where two directions are rotated at one time. The grid of θ includes 500 values. More details are in Canova and De Nicoló (2002). By rotating more than two directions at a time, one can explore systematically the space of identification. Given the computational burden of such an approach and given that there are 48 states for which such a procedure needs to be run, we have only examined primitive bivariate rotations.

[3] Poterba (1995) reports that in one fourth of US states budget rules restrict less than 50% of the total budget.

to the others. In the second (Carryover), an entry of one is given to states which may not carry over a deficit for more than a year and zero to the rest. In the third (Ex-post), a value of one is given to states which are required to balance the budget within the current fiscal cycle and zero to the others (see first three columns of table 2). Here we do not distinguish between constitutional and statutory restrictions since we wish to measure the effects of fiscal constraints on state activity and not to design institutions which more effectively limit government actions.

In general, the information contained in the three indices overlaps. For example, among the 12 states with ex-ante budget restrictions, 9 are allowed to carry over deficits for more than one year. For reference, we also report in table 2 the ACIR (1987) index. This index, which rank states on the basis of the effectiveness of their deficit restrictions, and combines the information contained in our three indicators using grades from 0 to 10 (with ten being the most effective restrictions), is a popular choice in the literature. However, if we dichotomize it assigning a one to states with a grade of eight or above and a zero to states with a grade of six or below (as in Sorensen, Wu and Yosha (2001)), it becomes perfectly collinear with the Ex-post index. Similarly, it becomes perfectly collinear with the Ex-ante index if a grade of four is used as cut-off point.

As far as debt restrictions are concerned, constraints may refer to the total or only to the short run component of debt; they can be fixed in nominal terms, formulated in proportion of revenues, or the size of the states' general fund. To capture these differences, we construct three additional indicators. In the first (Debt1), a value of one is entered to states with some form of debt restriction and zero to the others. In the second (Debt2), a value of one is attributed to states which either prohibit guaranteed (full faith and credit) debt or allow a nominal amount below 200,000 dollars. A zero is given to all other states. In the third (Shortdebt), a one is given to states which prohibit short-term debt and a zero to the others (see columns 5-7 in table 2).

Finally, we construct three indicators capturing political/legal characteristics which may influence the state fiscal stance. In the first (Veto), a value of one is given to all states where the governor has line item veto power on the budget and zero to the others; in the second (Court) a value of one is given to states where the Supreme Court is elected by voters and a value of zero if it is appointed by the Governor or the legislature and in the third (Constitution) a one is given to

Table 2: Budget Characteristics of US states

STATE	Ex-ante	Carryover	Ex-post	ACIR	Debt1	Debt2	Short Debt	Veto	Court	Constitution
AL	0	1	1	10	0	0	1	1	1	1
AZ	0	1	1	10	1	1	1	1	0	1
AR	0	1	1	9	1	0	1	1	1	0
CA	1	0	0	6	0	0	0	1	1	0
CO	0	1	1	10	0	0	1	1	0	1
CT	1	0	0	5	1	1	0	1	0	0
DE	0	1	1	10	1	0	0	1	0	0
FL	0	1	1	10	0	0	1	1	0	1
GA	0	1	1	10	1	0	0	1	1	1
ID	0	1	1	10	1	0	0	1	1	0
IL	1	0	0	4	1	0	0	1	1	0
IN	0	1	1	10	1	1	1	0	0	1
IA	0	1	1	10	1	1	0	1	0	0
KS	0	1	1	10	1	0	0	1	0	0
KY	0	1	1	10	1	1	0	1	1	0
LA	1	0	0	4	1	0	0	1	1	0
ME	0	1	1	9	0	0	0	0	0	0
MD	1	0	0	6	1	0	0	1	0	0
MA	1	0	0	3	1	0	0	1	0	0
MI	0	0	0	6	1	0	0	1	1	1
MN	0	1	1	8	1	1	0	1	1	0
MS	0	1	1	9	1	1	0	1	1	0
MO	0	1	1	10	1	0	1	1	0	0
MT	1	1	1	10	0	0	0	1	1	0
NE	0	1	1	10	1	1	1	1	0	0
NV	1	0	0	4	1	1	0	0	1	0
NH	1	0	0	2	1	0	0	0	0	0
NJ	0	1	1	10	1	1	1	1	0	0
NM	0	1	1	10	1	1	1	1	1	0
NY	1	1	0	3	0	0	0	1	0	0
NC	0	1	1	10	0	0	0	0	1	0
ND	0	1	1	8	1	0	1	1	1	0
OH	0	1	1	10	1	0	1	1	1	1
OK	0	1	1	10	0	0	0	1	1	0
OR	0	1	1	8	1	0	0	1	1	1
PA	1	0	0	6	1	1	0	1	1	1
RI	0	1	1	10	1	1	0	0	0	0
SC	0	0	1	10	1	0	1	1	0	0
SD	0	1	1	10	1	1	1	1	1	1
TN	0	0	1	10	1	0	1	1	1	0
TX	1	1	1	8	1	0	0	1	1	1
UT	0	1	1	10	1	0	0	1	0	1
VT	0	1	0	0	0	0	0	0	0	0
VA	0	1	1	8	1	1	1	1	0	0
WA	0	1	1	8	1	0	0	1	1	1
WV	0	1	1	10	1	0	0	1	1	1
WI	0	0	0	6	1	1	0	1	1	1
WY	0	1	1	8	1	1	1	1	0	0

states that need a constitutional amendment to be able to borrow and zero to the others.

As suggested by Bohn and Inman (1996) and Mitchell (1967) these characteristics may affect the fiscal stance for the following reasons. First, since State Courts are responsible for the enforcement of budget rules, it is conceivable that enforcement is less than perfect and monitoring looser whenever Courts are appointed by those who also legislate the budget. Second, since constitutional amendments are much harder to enact than referendums or simple legislative actions, states with such restrictions face considerable constraints in their ability to issue general obligation debt.

Finally, since fiscally conservative voters may hold Governors responsible for any marginal expansion of state budgets, governors seeking reelection may be more active in controlling spending and deficits. One way to exercise this control is to use the veto power. Hence, as suggested by Holtz-Eakin (1988) or Carter and Schap (1990), states where the governor has veto power may be less prone to run a deficit (see columns 8-10 of table 2).

5 The Results

5.1 Volatilities and Correlations

To begin with we examine whether basic, reduced form business cycle statistics are affected by the presence of fiscal restrictions. We summarize cyclical information through 9 statistics: the volatility of state expenditure, the volatilities of state output, prices and employment in deviation from their US counterpart; their correlation with per-capita real state consumption expenditure; the mean of the log consumption expenditure to output ratio and the mean of per capita output.

There are several ways of computing volatilities and correlations. For example, in the business cycle literature, is common to filter out long and short frequencies fluctuations and compute statistics for fluctuations within, say, 2 to 6 years. In cross unit comparisons, however, one has to worry about the fact that cycle length may differ in different units. In this latter case, it is more typical to compute statistics using growth rates of the variables. Here we present second moments obtained from the raw data and from the residuals of a VAR.

We prefer this approach for two reasons. First, given the short sample, the variability and correlation properties at business cycle frequencies may be poorly estimated with filtered data. Second, with the scaling we employ, variables are stationary so moments can be computed without the need of any further transformations. We present results for both raw data and the residuals of the VAR for robustness. In fact, in the first case predictable variations related to the presence of automatic stabilizers or, simply, serial correlation in the variables may be unaccounted for.

Table 3: Volatilities and Correlations

Index	Var(y)	Var(n)	Var(p)	Var(g)	Mean(pcy)	Mean(g/y)	Corr(y,g)	Corr(n,g)	Corr(p,g)	Joint
Asymptotic test: P-values for the null of equality of means across groups										
Ex-ante	0.79	0.90	0.92	0.98	0.48	0.81	0.98	0.62	0.68	0.61
Carryover	0.82	0.96	0.76	0.70	0.79	0.93	0.87	0.73	0.96	0.52
Ex-post	0.99	0.98	0.96	0.81	0.88	0.80	0.62	0.98	0.98	0.78
Debt 1	0.54	0.79	0.81	0.78	0.85	0.84	0.93	0.77	0.63	0.59
Debt 2	0.96	0.94	0.87	0.96	0.99	0.75	0.67	0.83	0.99	0.83
Short Debt	0.98	0.89	0.85	0.87	0.80	0.77	0.98	0.91	0.63	0.78
Veto	0.96	0.76	0.43	0.92	0.98	0.79	0.98	0.91	0.57	0.72
Supreme	0.99	0.82	0.97	0.83	0.82	0.80	0.98	0.99	0.86	0.86
Constitution	0.92	0.98	0.97	0.85	0.96	0.79	0.76	0.93	0.90	0.81
Rank sum test P-values for the null of equality of distributions across groups										
Ex-ante	0.06	**0.05**	0.84	0.20	0.58	**0.02**	0.96	**0.03**	0.90	
Carryover	0.12	0.90	0.90	0.23	0.83	0.90	0.96	0.96	**0.00**	
Ex-post	0.45	0.19	0.78	0.37	0.45	0.59	0.36	0.79	**0.03**	
Debt 1	0.22	0.70	0.78	0.44	0.49	0.78	0.14	0.20	**0.00**	
Debt 2	0.66	**0.02**	0.93	0.50	0.54	0.94	**0.00**	**0.04**	0.82	
Short Debt	0.77	0.21	0.76	0.43	0.84	0.62	0.34	0.14	0.57	
Veto	0.55	0.29	0.93	0.36	0.90	0.70	0.59	0.95	0.72	
Supreme	0.14	0.21	0.54	0.34	**0.00**	0.07	0.20	0.39	0.19	
Constitution	0.74	0.61	0.39	0.26	0.67	0.74	0.87	0.38	0.16	

Tables 3 and 4 report the p-values of two tests. The first is an asymptotic χ^2-test measuring the average differences in each of the statistics (or jointly) across groups of states with different fiscal restrictions. Since we have nine indicators of fiscal restrictions, different rows report the results obtained with different classifications. The second is a nonparametric rank sum test, measuring the difference in the distribution of each of the statistics across groups. Since with some classifications, the number of units in each group is small, since critical values of such a test have been tabulated for groups with as little as three units (see e.g. Hoel (1993)), and since the test examines the entire distribution, as opposed to the first moments, it may be more reliable to evaluate the statistical significance of the differences.

The message of table 3 is very clear: the presence of tighter budget, debt or institutional restrictions do not matter for business cycle fluctuations. This is true for the majority statistics we consider, for all classifications and for both tests. Mean differences are always insignificantly different across groups while distributions are occasionally different with some indicators (see, for example the volatility of employment or the correlations of employment and expenditure or prices and expenditure). However, also in these cases, the significance of the difference is often marginal.

Table 4: Volatilities and Correlations, VAR Residuals

Index	Var(y)	Var(n)	Var(p)	Var(g)	Corr(y,g)	Corr(n,g)	Corr(p,g)	Joint
Asymptotic P-values of equality across groups								
Ex-ante	0.89	0.81	0.94	0.74	0.88	0.93	0.97	0.67
Carryover	0.95	0.99	0.69	0.81	0.93	0.60	0.96	0.84
Ex-post	0.77	0.95	0.89	0.80	0.85	0.85	0.73	0.69
Debt 1	0.72	0.80	0.71	0.73	0.71	0.99	0.99	0.79
Debt 2	0.85	0.88	0.92	0.98	0.90	0.98	0.96	0.87
Short Debt	0.98	0.85	0.81	0.74	0.90	0.91	0.91	0.83
Veto	0.80	0.90	0.91	0.62	0.91	0.87	0.67	0.86
Supreme	0.91	0.99	0.77	0.85	0.91	0.91	0.97	0.91
Constitution	0.89	0.80	0.77	0.96	0.67	0.79	0.78	0.81
Rank test P-values of equality across groups								
ex-ante	0.88	0.47	0.52	0.66	0.43	0.29	0.88	
carry-over	0.34	0.43	0.21	0.81	0.96	0.81	0.85	
ex-post	0.34	0.43	0.21	0.81	0.96	0.81	0.85	
Debt 1	0.99	0.46	0.17	0.70	0.74	0.27	0.87	
Debt 2	0.94	0.68	0.22	0.21	0.45	0.99	0.73	
Short Debt	0.94	0.68	0.22	0.21	0.45	0.99	0.73	
Veto	0.53	0.61	0.24	0.26	0.43	0.88	0.57	
Supreme	0.83	0.14	0.06	0.50	0.12	0.95	0.60	
Constitution	0.63	0.08	0.71	0.38	0.47	0.75	0.89	

Table 4 has a similar flavor: when statistics are computed using the residuals of the VAR and an asymptotic test is used, differences across groups are insignificant, regardless of the classification employed to group states. When a small sample test is used, price volatility is marginally statistically different across groups when the Supreme Court indicator is used.

To provide visual evidence for these results we graph in Figure 1 the estimated values of the 9 statistics for each of the 48 states when we use the Ex-post indicator to group states and the residuals of the VAR to compute second moments. A vertical bar in each graph cuts off the 13 states with loose restrictions from those with strict ones. Two features stand out from the figure. First, the mean difference in the statistics across groups is not only statistically but also economically small. For example, average relative output volatility in states with ex-post restrictions is only marginally higher than the average volatility in states with no ex-post restrictions (0.03 versus 0.02 percent), but the opposite is true for relative employment volatility. Second, there are considerable variations in the statistics within groups. For example, the correlation between per-capita real consumption expenditure and relative output ranges from -0.38 to 0.62 in states with loose fiscal restrictions and -0.53 and 0.41 in states with strict budget restrictions.

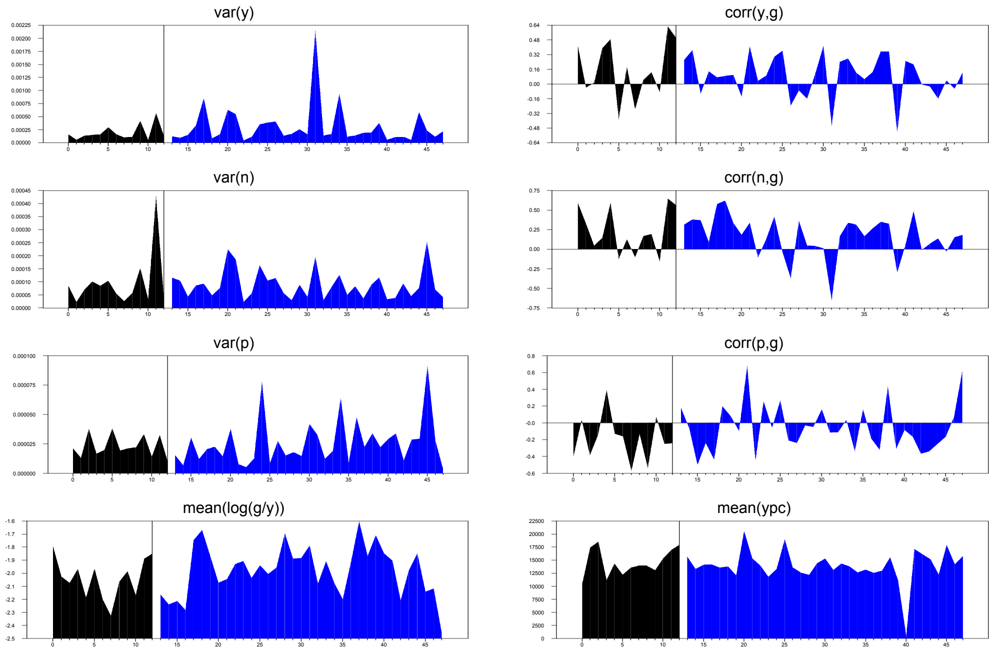

Figure 1: Moments using the Ex-post classification

While basic business cycle statistics are unaffected by tight budget, debt and institutional restrictions, important economic relationships could be altered. In fact, much of the discussion in the literature has not focused on business cycle moments but on the ability/inability of governments to respond to cyclical fluctuations in the economy.

There is some evidence in OECD countries that government expenditure plays a stabilizing role. For example, Gali (1994) and Fatas and Mihov (2001) found a significant negative relationship between output volatility and government size (measured by the average expenditure to output (G/Y) ratio) and/or level of development (measured by the per-capita GDP), while Lane (2003) found that more volatile economies tend to have more procyclical government expenditure. Similarly, there seems to be a tight relationship between expenditure volatility and macroeconomic volatility (e.g. Fatas and Mihov (2003)). Do US states conform to this evidence? Are the magnitude and the significance of these relationships altered by fiscal restrictions?

US states appear to be somewhat different from OECD countries but, to a large extent, the sign, the magnitude and the significance of the relationships are unaffected by fiscal restrictions, no matter what classification is used to group states. To illustrate this point, we present in Figure 2 scatter plots of four interesting relationships when the Ex-post indicator is used to group states and the residuals of the VAR are employed to compute variabilities and correlations. States without Ex-post restrictions appear with a star; states with restrictions with a diamond. Take, for example, the relationship between the variability of government consumption expenditure and variability of relative output. For the whole sample the slope of the relationship is negligible (-0.07); for the sample of states without ex-post restrictions it is -0.19 and for the sample of states with restrictions it is -0.09. Therefore, if there is any relationship between the two volatilities, it is smalland statistically similar across groups of states. Similarly, when we look at the relationship between government size and output volatility we find a strong non-linear pattern but the inverted U shape depicted in Figure 2 is independent of the presence of tight or loose budget restrictions.

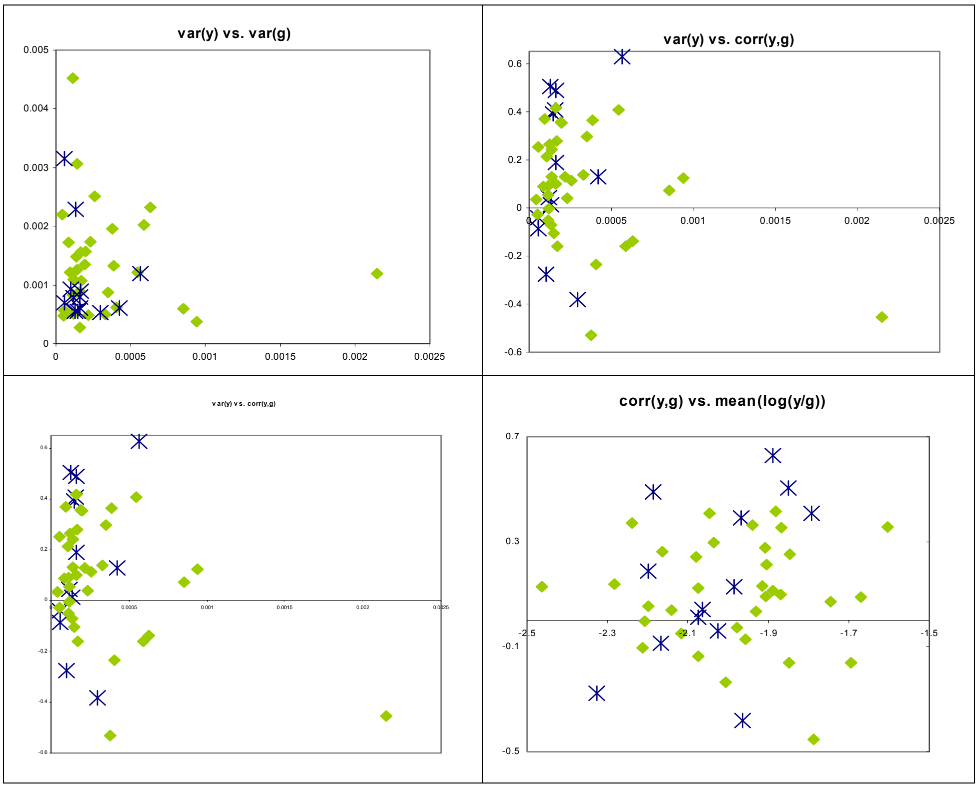

Figure 2: Macroeconomic relationships, Ex Post Classification

For the other two statistics some qualitative difference emerge. The relationship between output volatility and the correlation between expenditure and output is negative in the whole sample (point estimate -0.28), contrary e.g. to Lane (2003), suggesting that states where macroeconomic volatility is high do not necessarily display more procyclical expenditure. This estimate increases to -0.41 for states with ex-post budget restrictions but is positive for states with no ex-post budget restrictions (0.31). However, even these large slope differences are statistically insignificant.

A similar result obtains when we look at the relationship between size and cyclicality of expenditure. There is no relationship for the whole sample; a positive and significant slope is obtained for states with no ex-post budget restriction (0.48) and a negative one for states with ex-post restrictions (-0.15). However, the large heterogeneity within groups make these point estimates insignificantly different. Note that all of these conclusions are maintained when we substitute employment for output in all the scatter plots of figure 2.

There are many reasons which can explain the failure to find significant differences in the business cycle statistics we collected. One, often cited in the literature (see Milesi-Ferretti (2003)), is that state government use creative accounting to avoid constraints when they become binding. For example, governments may shift expenditure items off the budget or to less restricted branches (e.g. local governments) or use stabilization funds to limit the revenue crunch they may experience in recessions. Similarly, debt restrictions apply only to guaranteed debt. Hence, there could be an incentive for state governments to issue non-guaranteed (revenue) debt when the borrowing limit may be binding. Since our data includes both local and state expenditures and we consider total outstanding debt, we can study whether fiscal restrictions constraint government behavior or simply imply substitution toward less restricted accounts, bonds or practices.

Table 5, which reports first and second moments of the level of total state and local deficits and debt, of debt to output ratios and of the growth rate of non-guaranteed to guaranteed debt, supports the idea that more restricted governments tend to substitute across accounts to avoid the restrictions. In fact, the mean deficits appears to be different in strongly restricted vs. weakly restricted states only when the short-term debt indicator is used and the rank test is to evaluate the differences while the debt to output ratio is significantly different only when the Debt2 indicator is used. Perhaps surprisingly, we also find that the growth rate of non-guaranteed to guaranteed debt is not significantly different across groups of states with different debt constraints. While this appears to be in contrast with the substitution hypothesis, one should also notice that both types of states have substantially increased non-guaranteed debt financing over time and this may account for our failure to detect differences.

Table 5: Means and Volatilities

Index	Mean(df)	Mean(Debt)	Mean(Debt/Y)	Mean(Δ NG/G)	vol(df)	vol(debt)	vol(Debt/Y)	vol(Δ NG/G)
Asymptotic test: P-values for the null of equality of means across groups								
Ex-ante	0.85	0.85	0.99	0.88	0.58	0.69	0.52	0.68
Carryover	0.86	0.99	0.85	0.98	0.96	0.98	0.89	0.89
Ex-post	0.49	0.87	0.97	0.80	0.96	0.95	0.96	0.91
Debt 1	0.88	0.98	0.92	0.91	0.73	0.78	0.95	0.67
Debt 2	0.92	0.93	0.63	0.85	0.82	0.84	0.87	0.74
Short Debt	0.81	0.85	0.99	0.74	0.56	0.56	0.99	0.91
Veto	0.82	0.94	0.97	0.77	0.95	0.97	0.62	0.68
Supreme	0.83	0.99	0.91	0.89	0.96	0.93	0.97	0.76
Constitution	0.92	0.92	0.86	0.78	0.87	0.90	0.80	0.64
Rank sum test P-values for the null of equality of distributions across groups								
Ex-ante	0.58	0.72	0.61	0.17	0.68	0.16	0.98	0.07
Carryover	0.96	0.57	0.60	0.57	0.11	0.98	0.87	0.76
Ex-post	0.51	**0.05**	0.88	0.81	0.96	0.27	0.79	0.53
Debt 1	0.55	**0.03**	0.66	0.83	0.40	0.36	0.91	0.97
Debt 2	0.29	0.77	**0.03**	0.96	0.68	0.94	0.43	0.31
Short Debt	**0.03**	0.66	0.82	0.94	0.62	0.29	0.94	**0.01**
Veto	0.24	0.79	0.97	0.46	0.97	0.12	0.66	0.08
Supreme	0.09	0.43	0.91	0.16	0.37	0.85	0.67	0.22
Constitution	0.59	0.45	0.86	0.82	0.22	0.75	0.80	0.87

5.1.1 Robustness

We have conducted a number of robustness checks to examine whether our conclusions may depend on some uncontrolled factors, or on our cross sectional method of analysis. For example, it could be that our fiscal indicators are endogenous: states which are more prone to large business cycle fluctuations (because of the composition of their output) may be less likely to impose fiscal restrictions than states where cyclical fluctuations are minor. Similarly, fiscal policy may be more restricted in states which are small and therefore open to movements of goods and people (since the effects of fiscal policy are likely to be small) than in states which are large and therefore relatively close. On the other hand, there has been a tendency of fiscal policy to become more restrictive in all states after the tax-revolt of the beginning of the 1980s and the widespread imposition of tax and expenditure limits (the so-called TELs). Therefore, it may not matter whether the constraints have taken the form of explicit or implicit constraints on government behavior. Along these lines, our sample contains an interesting case study where tight fiscal constraints were imposed in the middle of the sample (the case of Tennessee in 1977) and allows us to verify whether our cross sectional conclusions hold also over time. Finally, rainy days funds are not a prerogative of states with tight constraints and, in fact, all of the states with ex-ante restrictions had rainy days funds by the end of our sample.

While the reverse causality hypothesis does not seem to fit with the evidence we have collected, it is interesting to examine whether unaccounted heterogeneity can account for our inability to detect differences. Table 6 reports rank sum tests for the equality of selected government and macroeconomic statistics. There we present structural break tests for Tennessee using the samples 1969-1977 and 1978-1995; tests for equalities in the cross sectional distribution when we condition on the presence or absence of rainy days funds at the end of sample, or on the size of states (large states are those which are in the top quartile in terms of population); tests for equality of the distributions in states with tight and loose restrictions for the two subsamples 1969-1980 and 1981-1995; and tests performed when an alternative measure of volatility (inter-quartile range) is used. For the sake of space we only report results obtained with the ex-post and the short debt classification but none of the conclusions depend on the indicators used.

Table 6: P-values Rank Sum Test: Robustness

Index	vol(y)	vol(N)	vol(p)	corr(y,g)	corr(n,g)	corr(p,g)	vol(df)	vol(debt)	mean(Df/Y)	mean(Debt/y)
Rainy	0.41	0.70	0.50	0.91	0.50	0.43	0.03	0.76	0.31	0.97
Large	0.74	0.86	0.80	0.11	**0.00**	**0.05**	0.17	0.20	0.26	0.41
Before 1980										
Ex-post	0.90	0.57	0.94	0.48	0.60	0.51	0.74	0.06	0.94	0.92
Short Debt	0.26	0.74	0.79	0.36	0.68	0.46	0.66	**0.02**	0.16	0.56
After 1980										
Ex-post	**0.03**	0.17	0.32	0.29	0.39	0.50	0.64	0.07	0.47	0.21
Short Debt	0.66	0.93	0.09	**0.02**	0.19	0.77	0.32	**0.00**	0.98	0.84
Interquartile range										
Ex-post	0.56	0.79	0.06				0.47	0.50		
Short debt	0.93	0.14	0.36				0.46	0.24		
	y	p	N	deficit	Debt/Y					
Tennessee	0.11	0.48	0.11	**0.00**	**0.00**					

The table confirms to a large extent previous conclusions. First, it does not matter if we use inter-quartile ranges or variability to measure the size of fluctuations. Second, there are some differences in output volatility with the ex-post index after 1980 which were not present in the first subsample and, similarly, there are some differences in the correlation of output with expenditure in the second subsample when the short-term debt indicator is used. However, broadly speaking statistics for the two subsamples are similar and the same similarities across groups of states we noticed in the full sample also appear in the two subsamples. Third, there is little difference in the statistics of states with rainy days funds and without them or between large states and the others. These observations therefore contrast with the idea that large states or states with rainy days funds have different characteristics. Once aggregate cyclical fluctuations are taken into account, there are very little differences in the moments of macrovariables of different types of states. Finally, Tennessee data show little changes in the distribution of relative output, relative employment and relative prices across the two time periods. This however, come together with some significant change in the pattern of deficit (which show a declining trend) and in the debt to output ratio (the mean value is lower in the second part and less volatile). Hence fiscal constraints in Tennessee have restricted government behavior but left unaltered the time path of macroeconomic variables.

In sum, all the evidence indicates that business cycle statistics are largely unaffected by the presence of fiscal constraints. We find that this conclusions is independent to the classification used to define states with tight fiscal restrictions, of the procedure used to calculate business cycle statistics and, to a large extent, of the tests used to evaluate the mean differences across groups and of the statistics employed. We also argue that this pattern does not crucially depend on unaccounted factors or structural time series breaks. We argued that this in part is due to the fact that state governments have the ability to bend the rules and use creative accounting to avoid the constraints. The only available case study where fiscal constraints have changed over time confirms that local macroeconomic variables are not very much affected by fiscal constraints.

While the evidence overwhelmingly and robustly point in one direction, one important caveat needs to be mentioned: the conclusions we have drawn are so far based on "reduced form" statistics. Although volatilities and correlations are unaffected by budget restrictions, it is possible that the channels through which fiscal policy shocks are transmitted to the state economy could be significantly altered. In addition, tight budget, debt or institutional restrictions may imply different fiscal rules. Since our VAR model can exactly examine these issues, we next turn to a more structural evaluation of fiscal constraints.

5.2 The transmission of expenditure shocks

The identification of structural expenditure shocks roughly produced the expected results. Overall, we identify G disturbances in 36 states and BB disturbances in 12 states; in seven states (Connecticut, Iowa, Louisiana, Oklahoma, Rhode Island, Tennessee, Virginia) we fail to recover any expenditure shock and in seven states (Kansas, Maryland, Mississippi, South Carolina, Utah, Washington, West Virginia) we identify both G and BB shocks. We have already mentioned that since our data includes all state and local consumption expenditure, and since expenditure switching practices seem to be widespread, shocks in states with strict constraints may end up looking like G shocks. We find that this is the case in 25 states. We also mentioned the possibility that states with no strict budget requirement may nevertheless maintain close to a balance budget when manipulating the discretionary component of expenditure. This seems to be the case in Maryland and Pennsylvania. How is it that in some states both shocks are identified and in others no shocks satisfying the restrictions are found? We conjecture that structural instability is responsible for both these results. In fact, in states were no expenditure shock is identified, the comovements of expenditure, deficit and output are poorly estimated. On the other hand, the seven states where both shocks are identified are among the last to establish stabilization funds [4] and the variability of BB shocks in these states declines considerably in the last 10 years of the sample.

We measured the transmission of expenditure shocks to the local economy for a "typical" state with strict or loose budget restrictions using the one step methodology described in section 2. We computed "typical" responses grouping states with our nine indicators. Since the results are roughly similar, we present two outcomes: one obtained using the Ex-post indicator and one using the Debt2 indicator. Figure 3 plots the mean response and a 68% confidence band of relative output (first row), relative employment (second row) and relative prices (third row) following a G shock and Figure 4 the same information following a BB shock when the Ex-post index is used. Figures 5 and 6 plot bands for the two types of shocks when the Debt2 indicator is used to classify states.

Consider first the plots of Figure 3. Qualitatively speaking, the responses of the three variables to G shocks conform with theoretical expectations: expansionary expenditure shocks boost aggregate demand and increase, on average, relative employment in both groups of states. The pattern of relative price responses is slightly different: in fact, relative prices rise instantaneously when strict restrictions are in place, while are instantaneously insignificant in states with loose restrictions. However, in both cases responses are positive after two years and remain persistently and significantly above the trend for another 5 years.

[4] In Kansas stabilization funds were introduced in 1993, in Maryland in 1985, in Mississippi in 1982, in Utah in 1986, in West Virginia in 1981 and in Washington in 1981.

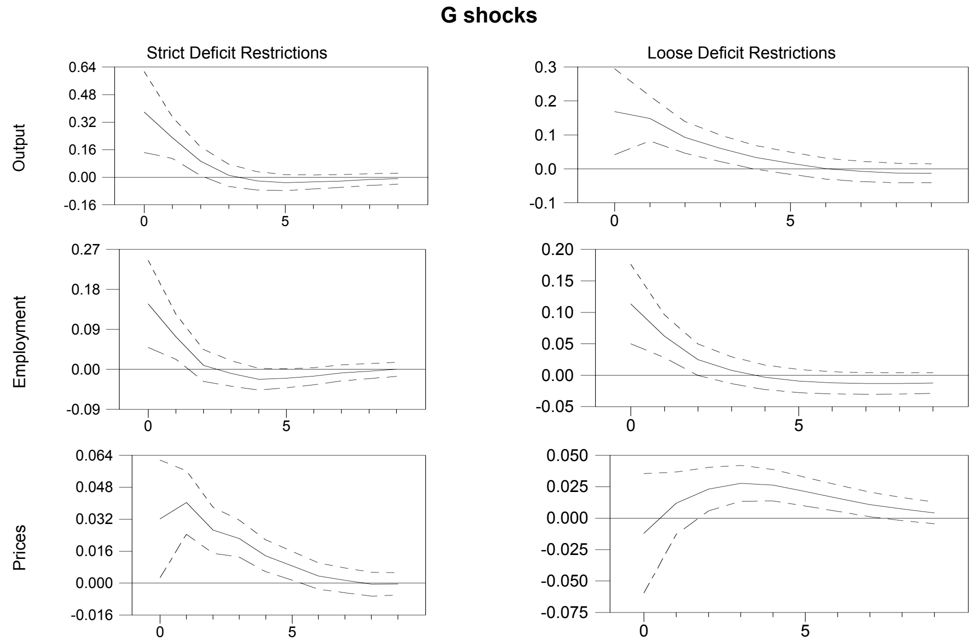

Figure 3: Responses of Macroeconomic Variables, Ex-Post Classification

A BB type disturbance, on average, significantly decreases relative employment in both groups of states. Also this pattern conforms to theoretical expectations since an expenditure increase, when financed by distortionary taxation, is expected to have contractionary effects on output. Relative price movements are insignificant over the first two years for both groups of states but then turn positive and slightly different from zero in states with strict deficit restrictions.

The typical responses of output and employment to both shocks in the two groups are also quantitatively similar. Take, for example, G shocks. Here the maximum difference in the output and employment responses for the two groups are 0.12 and 0.06, respectively. But the mean response of the two variables for the typical state with strict restrictions is inside the band obtained for the typical state with loose restrictions and the bands for the two groups of states largely overlap. Furthermore, the qualitative difference in relative price responses we have noted, wash out once standard errors are accounted for.

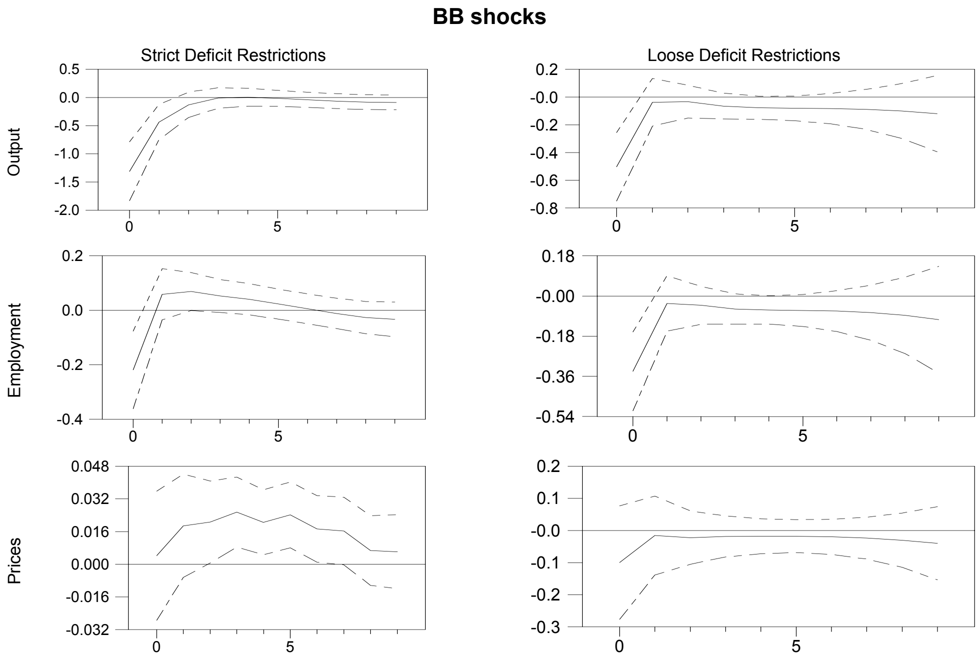

Figure 4: Responses of Macroeconomic Variables, Ex-Post Classification

Two other interesting features of figures 3 and 4 need to be emphasized. First, the timing of the responses is largely unaltered by the presence of strict budget restrictions: the largest response of relative output and relative employment is always instantaneous, while the response of relative prices is slightly humped shaped. Second, the persistence of the responses looks similar across groups for both types of shocks. For example, the half-life of the output responses to G shocks is about two years for both groups while it is one year for both groups with BB shocks.

Is there any possibility that, although statistically insignificant, difference across groups are economically relevant? Figures 3 and 4 are not very informative on this issue. For example, comparing point estimates it looks as if cumulative one-year output multipliers for both types of shocks are about 20% larger in states with strict fiscal restrictions. However, any meaningful attempt to explain this difference (for example, noting that large fiscal shocks are less likely to occur when strict fiscal constraints are in place) comes against the fact the uncertainty around point estimates is sufficiently large to make the size of the two multipliers indistinguishable.

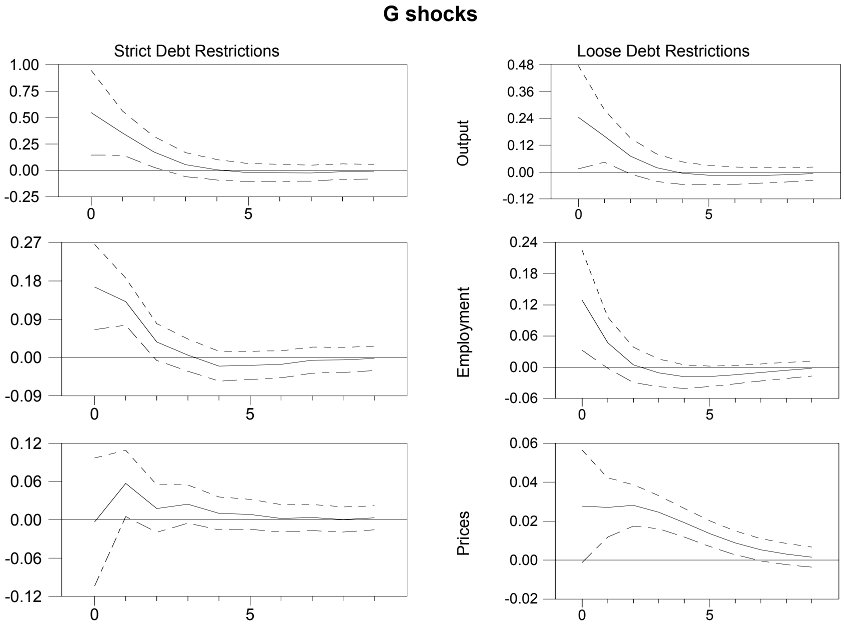

Figure 5: Responses of macroeconomic variables, Debt2 Classification

Figures 5 and 6 confirm these conclusions. The only noticeable difference across states with strict/ loose debt restrictions concerns the behavior of employment with BB shocks. In fact, it appears that employment is better shielded from the adverse economic effects of balance budget shocks when loose debt constraints are in place. Also in this case, standard error bands largely overlap at several horizons making differences statistically insignificant.

To summarize, the transmission of fiscal disturbances to the real economy is both qualitatively and quantitatively unaltered by the presence of strict budget or debt constraints. Some qualitative differences emerge when we look at relative price responses, but also in this case differences are statistically insignificant. It is important to stress that not only the magnitude of the responses but also their shape and their persistence are unaffected by the restrictions. Why is it that we fail to find differences across groups of states? Once again, part of the explanation has to do with the fact that it is relatively easy to circumvent fiscal constraints. In fact, the response of deficits to G and BB shocks look very similar across groups of states. Another part of the explanation has to do with the fact that flexible labor markets may compensate for the inflexibility of fiscal policy. This flexibility may be the crucial difference one should expect to encounter when trying to extend the conclusions for US states to Euro area countries.

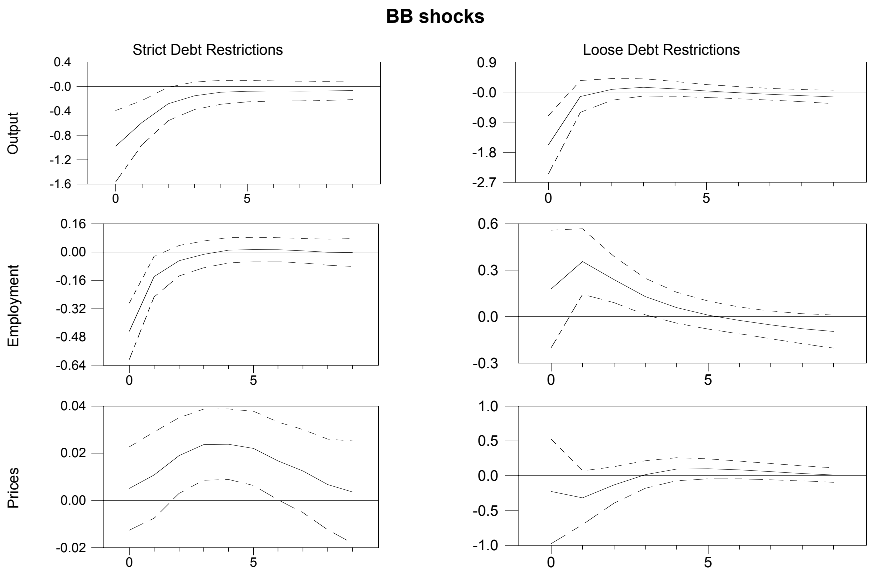

Figure 6: Responses of macroeconomic variables, Debt2 Classification

5.3 Fiscal rules

To analyze the systematic component of expenditure we compute the contemporaneous policy rules implied by our structural VAR estimates for each group of states [5]. We report in table 5 average point estimates of the coefficients on output, employment, prices and debt/output ratio for each of the indicators used to group states. For interpretation purposes coefficients are normalized so that expenditure appears on the left hand side with a unitary coefficient.

Several interesting features emerge from the table. First, it appears that different type of shocks imply different expenditure rules. With G shocks, expenditure is generally leaning against relative output, relative employment and debt while it is roughly unresponsive to relative prices. When BB shocks are considered, expenditure follows relative output, leans against relative prices while it is roughly unresponsive to the other two variables.

[5] This is achieved computing the policy rule for each state given the identification scheme. Then the policy rule for a typical state of each group is calculated weighting each state's coefficients by their standard deviations as described in section 2.

Second, there are some changes in the sign of the output coefficient across groups of states. However, in many cases, only magnitude differences are present.

Table 7: Government expenditure rules

Index		Output	Prices	Employment	Debt
		G Shocks			
Ex-ante	loose restrictions	-0.24	0.93	-2.41	-0.12
	strict restrictions	-0.21	0.00	-0.12	-0.82
Carry-over	loose restrictions	-0.14	-0.03	-0.03	-0.23
	strict restrictions	-0.31	1.25	-3.20	-0.45
Ex-post	loose restrictions	-0.16	0.01	-0.08	-0.47
	strict restrictions	-0.38	2.21	-5.53	-0.03
Debt 1	loose restrictions	-3.70	10.64	2.94	-2.82
	strict restrictions	0.16	-0.52	-2.21	-0.06
Debt 2	loose restrictions	-1.92	1.83	0.16	-0.59
	strict restrictions	1.80	-0.81	-3.88	-0.05
Short Debt	loose restrictions	-0.24	0.57	-1.01	-0.26
	strict restrictions	1.81	-0.81	-3.76	-0.05
Veto	loose restrictions	-0.08	0.003	-0.06	-0.06
	strict restrictions	-0.41	1.38	-3.59	-0.68
Court	loose restrictions	-0.12	-2.75	0.43	0.73
	strict restrictions	-0.20	-0.30	-1.09	-0.05
Constitution	loose restrictions	-0.32	-1.15	0.43	0.73
	strict restrictions	-0.28	-0.88	0.09	0.18
		BB Shocks			
Ex-ante	loose restrictions	4.92	-5.42	0.05	0.10
	strict restrictions	8.29	-0.15	-6.13	-0.47
Carry-over	loose restrictions	17.42	0.29	-8.98	-1.11
	strict restrictions	5.48	-4.64	-0.40	0.14
Ex-post	loose restrictions	19.94	-0.41	-15.09	-1.41
	strict restrictions	3.88	-4.25	0.03	0.11
Debt 1	loose restrictions	0.15	0.01	-0.01	0.16
	strict restrictions	7.34	-4.64	-2.38	-0.13
Debt 2	loose restrictions	8.09	-3.58	-2.48	-0.01
	strict restrictions	2.55	-4.02	-1.03	-0.19
Short Debt	loose restrictions	8.32	-0.00	-3.52	-0.12
	strict restrictions	0.36	-12.74	1.85	0.03
Veto	loose restrictions	5.97	-3.75	-1.93	-0.08
	strict restrictions	-0.12	-2.75	0.43	0.73
Court	loose restrictions	7.34	-8.88	1.71	-0.19
	strict restrictions	5.06	-0.34	-4.36	-0.00
Constitution	loose restrictions	1.32	-0.69	-0.12	-0.06
	strict restrictions	2.28	-0.93	-0.61	-0.18

For example, for G shocks, expenditure is always leaning against relative output movements when loose restrictions are in place and it is following relative output movements when strict restriction are in place only with the three debt classifications. Expenditure also follows relative output movements for both groups of states when BB shocks are considered with eight of the nine indicators and it is only with the Veto indicators that strict restrictions imply countercyclical responses.

Third, the signs of employment and price coefficients depends, to a large extent, on the classification used and magnitude variations are considerable. For example, with G shocks the average relative price coefficient for states with loose restrictions runs from -2.75 to 10.64 and the one for states with strict restrictions runs from -0.81 to 2.21.

Fourth, expenditure systematically responds in a stabilizing fashion to debt/output ratio in both groups of states when G shocks are examined with all but one indicator. The magnitude of the average estimated elasticity ranges in this case from 0.05 to a large 2.82, but it is not necessarily true that states with strict fiscal rules react differently, on average, to debt. A more mixed pattern instead emerges when BB shocks are considered since the signs change across classifications in a somewhat unpredictable manner and no pattern is detectable.

But perhaps more importantly, regardless of the classification used to group states and of the type of shocks considered, and even in those cases when sign switches are present, differences across groups of states are statistically insignificant. This is true both when average coefficients are significantly different from zero and when they are not. This occurs because policy rules within groups are very heterogeneous. As an illustration, take the Ex-post classification. The average relative prices and relative employment coefficients are equal to -0.08 and 0.01 respectively, (with standard errors equal to 1.82 and 1.32) when no restrictions are in place, implying, for example, that a one percent movement in state employment above the national level make per-capita expenditure fall by less than 0.1 percent. Expenditure becomes strongly countercyclical with respect to relative employment movements and turns procyclical with respect to relative prices movements, on average when restrictions are in place (coefficients are -5.53 and 2.21, respectively). However, standard errors are large also in this case (equal to 2.70 and 1.61, respectively). Hence, confidence bands around the mean largely overlap.

What is the large heterogeneity telling us about fiscal rules? It appears that deficit, debt and political restrictions only marginally account for the differences in expenditure responses to business cycle conditions across states. To put this result in another way, the R^2 in a typical two-stage regression where fiscal dummies are used to explain differences in the first stage slope estimates is negligible. This suggest that other state characteristics (e.g. their location, the composition of output or the trade pattern with neighboring states) could be more important to explain differences in the cyclical responses of state expenditure to movements in macro variables.

6 Conclusions

This paper analyzed whether tight fiscal constraints affect the macroeconomic performance of 48 US states for the period 1969-1995. First, we studied the volatility and the comovements of a number of state variables and a few important macroeconomic relationships. In each case we construct a mean estimator for group of states with different fiscal constraints and evaluate the statistical and economic significance of the difference. Second, we examine the differences in the transmission properties of two types of expenditure disturbances (one financed by debt and one by distortionary taxation) for a typical state with or without fiscal restrictions. Finally, we back out expenditure rules (one for each of the two shocks) for states with loose and strict restrictions and compared them.

Our results indicate that the macroeconomic consequences of fiscal constraints have been overemphasized. While the sign and the magnitude of point estimates are, at times, different, these differences are statistically insignificant and economically unimportant. Our conclusions are robust in a number of dimensions, and in particular, do not depend on the way we define "loose" or "strict", on whether deficit, debt or institutional constraints are in place, on the type of statistical tests we employ and, to a large extent, on the statistics we consider.

Does the experience of US states provide any reliable indication on the possible consequences of reforming the GSP? Do the results imply that the GSP should be scrapped or reinforced? Are EU politician right when they disregard the warnings of Bruxelles about the dangers of exceeding deficit limits? The answers to these questions clearly have important policy implications, which we briefly discuss next.

First, the EMU experience is still too short to shed a clear light on the macroeconomic consequences of imposing tight budget restrictions, but the experience of US states has important information on the expected magnitude of the gains and looses in the EMU. In fact, the two monetary unions display responses of macroeconomic variables to fiscal shocks which share a number of important similarities; the cross section of US states is rich enough to match the heterogeneities present in Europe; the available data covers a sufficiently long span of time with both expansionary and recessionary periods.

One should also remember that even the strictest fiscal constraints apply only to portions of the budget and do not exclude the use of stabilization funds for rainy days purposes. It is well known that such funds play the same buffer-stock role that debt has for smoothing the path of taxes, or of precautionary savings to ease the needs of credit constrained consumers. Since a prudent use of these stocks may help to implement optimal allocations in environments where aggregate uncertainty is modest or insured via some sharing mechanism, it is perhaps not surprising to find that macroeconomic performance is broadly independent of the tightness of the fiscal regime.

Second, while strict fiscal constraints do not make an important difference, it is worth stressing that some fiscal restriction is present in all but one US states. If these restrictions were designed to e.g. reduce the probability of debt default, the fact that they do not affect too much the macroeconomy is good and, in this sense, strict rules are preferable to weak ones. On the other hand, if they were imposed to keep government behavior under control, tight restrictions, may be not be needed since, lacking enforceability, they simply imply more creative accounting practices. Also, it is important to stress that while our results do not suggest that the GSP, or other fiscal restraining mechanisms should be abandoned, the lack of homogeneity we found indicates that good macroeconomic performance may have little to do with fiscal constraints.

Third, in the US labor markets are sufficiently flexible, people move and other margins (such as relative prices) adjust to absorb macroeconomic shocks. The lack of flexibility in the EMU may therefore render the consequences of imposing budget and debt rules both statistically and economically more significant. Furthermore, since fiscal constraints in the US almost always exclude capital account expenditures, care should be exercised to extend the conclusions to situations where non- golden rule type of constraints are present. Fourth, since our analysis has concentrated on business cycle issues, we cannot exclude that distributional and long run effects could be different and significantly so in states where tight fiscal constraints are binding. Finally, while they may make little difference in "normal " times, fiscal constraints may have a much larger impact if imposed when runaway inflation or debt threaten macroeconomic stability.

Appendix A: Data sources and definitions

US data are annual from 1969 to 1995, real, seasonally adjusted, per capita data. U.S. Census Bureau is the source unless it is otherwise indicated.

State Population: total state population in thousands.

Gross state product (in constant 1982 prices): Obtained from Bureau of Economic Analysis (BEA) from 1977; before 1977 we used the series from Oved Yosha's US State-Level Macroeconomic Databank (www.tau.ac.il/yosha).

State revenue: total revenue.

State expenditure: Total state current expenditure measures all expenditures other than expenditures on capital outlays. It includes both state and local expenditures and covers all funds available to the state government.

State debt: total debt outstanding at the end of the fiscal year. It includes short-term debt and long run guaranteed and non-guaranteed (revenue bonds) debt. The decomposition of long run total debt into two components is available only from 1977.

State employment: total full and part time state employment (from BEA).

State Prices: State prices are from Del Negro (1998). The price level for state i is computed as: $P_{it} = w_i^u P_{it}^u + (1 - w_i^u) P_{it}^R$ where P_{it}^R denotes the price level in rural areas of state i and comes from the Monthly Labor Review data of the Bureau of Labor Statistics (after 1978) and the "cost of living for intermediate level budget" from the same source (before 1978). w_i^u measures the fraction of population living in rural areas of state i and comes from the Statistical Abstract of the US. P_{it}^u is constructed as $P_{it}^u = \sum_{k=1}^K \omega_i^k P_{it}^k + (1 - \sum_{k=1}^K \omega_i^k) P_{it}^B$ where P_{it}^k is the CPI in metropolitan area k obtained from the ACCRA (American Chamber of Commerce Realtors Association) and the Bureau of Labor Statistics data on CPI for Urban Consumers (CPI-U) and CPI by Regions and by Urban Population and ω_i^k is the percentage of urban population living in metropolitan area k obtained from the Bureau of Economic Analysis site. P_{it}^B is the CPI in other urban areas taken from the Monthly Labor Review data of the Bureau of Labor Statistics. State CPI is normalized so that in each year their population average coincides with the US CPI.

Federal Aid Total: State transfers provided by the Federal Goverment.

State GDP Deflators: Computed from real and nominal GSPdata

US aggregate data for real GDP, interests rate, CPI and oil prices come from the Federal Reserve Bank of St. Louis FREDII data bank.

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