Do balanced-budget rules increase growth?

Stone, Joe

University of Oregon

20 July 2014

DO BALANCED-BUDGET RULES INCREASE GROWTH?

Joe A. Stone
Economics
University of Oregon
jstone@uoregon.edu

July 2014

ABSTRACT.

This study tests the hypothesis that balanced-budget rules (BBRs) that restrict public borrowing to investments in public infrastructure increase growth by increasing the productivity of debt, either because investments in public infrastructure are more productive than other uses for which states borrow funds or because BBRs lower borrowing costs. Results are based on data at 5-year intervals for 49 US states over the period 1957-2007. The tests strongly support the hypothesis that BBRs increase growth by increasing the productivity of debt and withstand a variety of robustness checks, including alternative lags, exogeneity tests, GMM estimation, a placebo test, and the influence of outliers.

Keywords: balanced budget rule, infrastructure, fiscal policy, regional growth.

JEL categories: A10, E60, H00.

I. INTRODUCTION

Soaring levels of debt in Europe and in the United States have spurred interest in balanced-budget rules (BBRs). The primary economic objection to BBRs is that they overly restrict fiscal policy by preventing tax smoothing and impeding stable growth. Not surprisingly then, BBRs are rare among central governments. However, they are common at the sub-national level in the United States, where every state but Vermont has some type of BBR and restriction on debt. Paradoxically, BBRs do not necessarily balance state budgets, for reasons noted by Bohn and Inman (1995); Inman (1996); and Reuben and Poterba (1999).

Many BBRs for example, only apply ex ante, so officials tend to overestimate revenues and underestimate expenditures, reducing the extent to which the rules limit fiscal flexibility during a budget cycle; and even when BBRs apply ex post, officials often resort to temporary accounting manipulations to avoid a deficit. Moreover, most states only apply BBRs to current operations and exempt budgets for public infrastructure projects financed by long-term bonds. In addition, BBRs vary in whether they require the governor to submit a balanced budget to the legislature and whether require the legislature is required to pass a balanced budget. BBRs typically incorporate similar limitations for other public entities subject to state jurisdiction.

Evidence that BBRs do not necessarily balance budgets begs the question: Why did states put the rules in place? Wallis and Weingast (2006) (WW) argue that the primary purpose of BBRs is to restrict borrowing to growth-enhancing investments in infrastructure, not simply to balance budgets. WW offer historical and economic context to support their argument, but no study has linked the stringency of BBRs directly to higher growth through an increase in the productive effect of debt on growth, as is done here. This effect could work through either of two complementary channels: by redirecting borrowing from less to more productive expenditures or by lowering the cost of servicing debt.

Several attributes of BBRs, debt, and the hypothesized influence of BBRs on the growth effect of debt aid identification. 1) Most states lodged BBRs and debt-limitation rules in their constitutions long ago in the nineteenth or early twentieth century and now rarely make changes; 2) The stringency of state BBRs varies greatly. 3) The hypothesized effect is a nonlinear interaction between debt and the presence and stringency of BBRs, which permits the effect of the BBR-debt interaction to be separated from the direct effect of debt. Lastly, 4) the stock of state and local debt tends to accumulate slowly over time, so that both debt and BBRs are predetermined, if not strictly exogenous. We exploit these attributes using several alternative estimation strategies.

II. OTHER EFFECTS OF BBRS

Several other empirical effects of BBRs are already well documented. Reuben and Poterba (1999) find that balanced-budget and debt-restriction rules lead to lower taxes and lower debt, and Alt and Lowry (1997) find that more stringent BBRs are associated with lower borrowing costs. Despite concern that BBRs limit fiscal flexibility needed for counter-cyclical policies, Levinson (1997), Alesina and Bayoumi (1996), and Krol and Svorny (2007) provide mixed evidence to resolve the issue; Carlino and Inman (2013) find significant power for countercyclical deficits but do not focus on the role of BBRs, Both they and Eberts and Stone (1993) offer evidence that suggests an explanation for the conflicting results on whether or not BBRs increase the volatility of state and local economies: countercyclical deficits are less effective in the long run because they induce a subsequent reversal, offsetting earlier effects.

III. THEORETICAL FRAMEWORK

III.1 Endogenous-growth models

Endogenous-growth models along the lines of Barro (1989), Adam and Bevan (2005), Checherita, et al. (2012), Grenier (2013), and Greiner and Fincke (2012)—to mention only a few, have proven to be a useful framework for both theoretical and empirical studies of the effects of state fiscal structures on growth. Unlike traditional neoclassical models, endogenous growth models permit a permanent change in fiscal structure to have a permanent effect on growth rates. While providing valuable insights, these studies yield widely varying results for the link between growth and public debt—zero, positive, negative, and inverse U-shaped. Fortunately, the validity of the hypothesis tested here rests on whether BBRs make the effect of debt on growth significantly less negative or more positive, not on whether the direct effect of debt is zero, positive, or negative.

To apply closed-economy endogenous growth models to the open-economy environment of state and local economies we assume that states are quasi-open economies with goods and factors that respond sluggishly over our recursive lag interval (ten years). In a typical endogenous-growth model, output growth in the steady state depends only on structural parameters and fiscal structures, such as taxes and other elements of the government budget constraint. The stock of private capital is endogenously determined in these models, so it does not appear as an independent variable.

III.2 Neoclassical-growth models

Mutatis mutandis, the structural parameters and exogenous variables are common to both the neoclassical- and endogenous-growth models. We rely on the latter as the framework for our empirical specification, not because of differences in the set of variables relevant to the two models, but because the endogenous-growth models permit permanent changes in fiscal structure to have persistent effects, providing a rationale for our recursive structure with long-lags.

IV. DATA AND EMPIRICAL SPECIFICATION

Our baseline empirical specification is adapted from Bleaney and others (2001), Bania and others (2007), and Gray and Stone (2012). It is presented below as equation (1). We specify an equation incorporating fixed effects for state-specific growth common to all periods, period-specific factors common across states, as well as period-specific factors unique to each state. In this context, current and lagged unemployment rates are expedient because they are cyclically sensitive to state- and regional-specific factors but mean reverting. We employ an index constructed by the Advisory Commission on Intergovernmental Relations (ACIR, 1987) as a measure for the stringency of BBRs. We use this index rather than the measure constructed by the U.S. General Accounting Office because the former is almost universally adopted and semi continuous, rather than simply dichotomous (strict or not).[1] We rely on a long j-interval, recursive structure, where j alternately equals one (five years) or two (ten years), as well as an error-correction term.

IV.1 Baseline specification

Our baseline empirical specification is equation (1):

(1) $V_{it} = c + c_i + c_t + b_1 d_{it-j} + b_2 D_{i\,t-j} + b_3 ACIR_i * D_{it-j} + B_1 X_{i\,(t-j)} + B_2 Z_{it} + e_{it}$

[1] Krol and Svorny (2007) identify anomalies in the two measures, but they are not directly comparable: the KS index is dichotomous (strict or lenient), the ACIR index is semi continuous. Thus far, only KS have preferred the GAO measure.

Where (V_{it}) is the change in the log of real personal income per capita for state i in period t. (c) is a fixed intercept common to all states in all periods; (c_i) is a state-specific intercept common to all periods, and j is a discrete lag of j periods. (c_t) is a period-specific intercept common to all states. (d_{it}) and (D_{it}) respectively, are the budget deficit and the stock of state and local government debt, with coefficients b_1 and b_2.[2] Again, ($ACIR_i$) is a commonly used index (ranging from zero to ten) for the stringency of each state's balanced budget rule. Our focus is on (b_3) the nonlinear effect of ACIR on the productivity of debt. (B_1) is a vector of coefficients for other components of the state and local government budget constraint (denoted by $X_{i\,(t-j)}$). (B_2) is a vector of coefficients for period-specific factors unique to each state (denoted by (Z_{it}). All fiscal variables represent percentage points of state personal income, and (e_{it}) is the residual for state i in period t. The long lag length for the ACIR-debt interaction should yield a conservative test of the WW hypothesis. Nevertheless, we challenge the hypothesis using GMM estimation and several robustness checks, including a placebo test.

IV.2 Government budget constraint

For n elements of a government budget constraint, only n-1 elements are independent, so at least one element must be omitted in the estimation of linear fiscal effects. Bania and others (2007), Bleaney and others (2001), and Mofidi and Stone (1990) explain and illustrate the widely ignored empirical implication of this fact: the linear effect of a change in an element of X, the government budget constraint is *necessarily* relative to the effect of a compensating change in

[2] To construct a relative, non negative metric, we scale deficits by subtracting the smallest (most negative) deficit in the sample from each state's deficit, so that deficits are positive deviations from the most negative deficit [e.g. d - (-1)= d+ 1>0].

one or more omitted elements, so that the linear effect of each element may not be legitimately interpreted as an independent effect. In our regression specification of eq. (1), we include the lagged deficit, taxes, and federal inter governmental transfers but omit total expenditures and other residual revenue sources, so these budget elements become the reference category.

IV.3 Data

Consistent with several prior studies, we rely on data at five-year intervals and omit Alaska due to the dominance of the Alaska pipeline and the consequent outlying variances in fiscal variables relative to other states. Use of five-year interval data allows a longer sample period from 1957 than the more limited and higher-frequency annual data, which for state and local public expenditures begin only in 1977. For present purposes, five-year intervals also have the advantage of suppressing short-term cyclical factors relative to low-frequency factors important to the intermediate- to long-run variations in growth relevant to our analysis. Use of five-year intervals has proven useful in this context in other studies, including Mofidi and Stone (1990), Bania and others (2007), Reed (2008), and Gray and Stone (2012). Of course a five-year interval would be too long if one focused primarily on short-term cyclical factors, as in Carlino and Inman (2013). Regional unemployment rates for example, tend to be strongly mean reverting by five to ten years (e.g. Eberts and Stone, 1992).

Data for state and local government fiscal variables are from the Census of Governments. Related economic, demographic, and other data for corresponding years are from the Bureau of Labor Statistics or the Department of Commerce (for personal income).

Table (1) reports summary statistics for the five-year-interval data used to estimate equation with the exception of the ACIR index, which is unchanged during the period. Values for ACIR range from zero to ten and have an average and median of about 8 and a standard deviation of about 3.

V. ESTIMATES

We begin with ordinary least-squares (OLS) estimates as our baseline estimates eq. (1). OLS estimates are reported in Table (2) for a lag of 2 periods (ten years), one period prior to the base year for growth, which yields a strictly recursive structure.

The R-squared of 0.61 in Table 2 is respectably large for a five-year growth interval, and the coefficient for the ACIIR-debt interaction (3.9) is significantly positive at the five percent level, consistent with the WW argument that BBRs increase growth by increasing the productive effect of debt by restricting public borrowing to investments in productive infrastructure. Coefficients for all other fiscal variables are insignificant at $p<.05$ at this long-lag interval, although lagged DEBT is significantly negative at $p<.10$. Carlino and Inman (2013) and Eberts and Stone (1992) also report insignificant effects for similarly long horizons. In this context, the significance of the ACIR-debt interaction is striking. Note, again, that ACIR cannot be included independently because it contains no variation independent from state and period fixed effects, so any direct effects of the ACIR index if any, are captured by the fixed effects. We turn next to the issue of whether the coefficient for the interaction is identified by endogenous or exogenous variation and then to issues of robustness and placebo regressions.

V.1 Exogeneity

To perform a standard Hausman test of the null hypothesis of exogeneity, we rely on three-period lagged values of the independent variables as instruments for the ACIR-debt interaction. That is, we use the ACIR-debt interaction lagged *fifteen* years and similarly lagged values of the other independent variables. The first-stage regression (not reported here) yields an R-squared of

0.83 and an F statistic of 32, well above the Stock-Yogo (2001) critical value for the null hypothesis of weak instruments.[3] Results for the Hausman test are reported in Table (3), where the p value for the coefficient for the first-stage residual (H-TEST) fails to reject the null hypothesis of exogeneity for the ACIR-debt interaction at the five percent level. Even so, we also report estimates based on the two-step, generalized method of moments (GMM) estimator in Table (4). The coefficient for the interaction term is significantly positive and rises insignificantly (based on the Hausman test) from the OLS estimate of 3.9 to 4.5; the Hansen's J statistic of 27.3 fails to reject exogeneity for the instruments at the five percent level;[4] the null hypothesis for an insignificant AR2 is not rejected; and the coefficient for lagged growth (-0.01) in Table (4) is notably small and insignificant. We have no reason thus far, to abandon the OLS estimates in Table (2), so we take the OLS estimate of 3.9 as our preferred estimate and report the GMM estimates in Table (4) merely for comparison.

V.2 Robustness

The OLS results in Table (2) are qualitatively invariant to several alternative specifications, including the addition of controls for the age composition of the population[5] or the addition of PROD, the lagged state income share invested in productive public infrastructure. The latter suggests that the coefficient for the ACIR-debt interaction is not significantly influenced by cyclical investments in public infrastructure. What aspect of the specification is not robust? Shortening the lag interval from two periods to only one (from ten to five years) disrupts the

---

[3] Individual unit roots are rejected for all variables.

[4] ($\text{Chi-square}_{/37\text{-}17/.05}$= 31.4).

[5] (percent of population 5-17, 18-64, and the implicit remainder for younger than 5 or older than over 64).

strictly recursive structure and yields an insignificant coefficient of (3.0) for the ACIR-DEBT interaction in Table (5). Not surprisingly, results in Table (6) for a Hausman test for the one-lag specification identified analogously to the longer two-period lag specification that is, with independent variables lagged j+1 (two) periods as instruments reject exogeneity at the five percent level at this shorter lag. Evidence that single-lagged values are endogenous is a useful finding, given that previously published studies have relied on single-period lags for identification. The two-period lag specification appears superior even for the five-year intervals. In light of the endogeneity present in the one-lag specification, we present GMM estimates for the one-period lagged specification with two-period lagged instruments. Table (7) reports these estimates, and the ACIR-debt coefficient is again significantly positive at (3.3). Unlike the two-lag GMM specification based on three-period lagged instruments, the one-period lag GMM coefficient for lagged growth in Table (7) is significantly positive at (0.19). Even so, an insignificant AR2 is not rejected, and the Hansen J-statistic of 27.3 fails to reject the null hypothesis of exogeneity for the instruments.[6] All estimates in Tables 2 through 7 for which exogeneity is not rejected yield a significantly positive coefficient for the ACIR-debt interaction, regardless of specification or estimator. The median across the range of these estimates (3.3 to 4.5) is 3.9, which coincides with the OLS estimate for the two-lag strictly recursive, specification—our preferred specification. We now turn to placebo regressions as another form of robustness check.

V.3 Placebo regressions

Placebo regressions are useful as a test for whether or not an effect is spurious because it is present where it should not be. An expedient choice for a placebo regression in the present

---

[6] chi $\text{square}_{/38\text{-}19/.05}$= 30.1)

context is to test whether the ACIR-debt interaction in one region has a significant effect in other regions, even though it should not, unless region spillovers are important. To test whether the ACIR-debt interaction in any of the nine Census regions has a significant effect on growth of other regions, we regress growth across all states in turn, on the ACIR-DEBT variable for each of the nine Census regions. Note that these placebo regressions are not a test for the general significance of regional spillovers from one specific region to another individual region. Instead, they are a test of whether regional spillovers or other factors spuriously common to states are responsible for the positive estimate for the effect of the ACIR-debt interaction in Table (2). Placebo regressions in this context are expediently conservative in the sense that regional spillovers will bias results toward a false placebo effect. Even so, we find no significantly positive effect for the ACIR-debt interaction in any of the nine regressions. To illustrate the results of the nine placebo regressions in a compact form, Table (8) presents results for the ACIR-DEBT interaction for Census region 4, which closely coincide with the median result across the nine placebo regressions. The next robustness check is to gauge the sensitivity of the results to outlying observations.

V.4 Outliers

To address the issue of sensitivity to outlying observations, residuals that are more than two standard deviations above or below the regression norm are identified, and dummy variables identifying these observations are added to the primary regression specification. Results from this augmented regression are equivalent and the correlations between the outlier residuals and the two key variables, growth and ACIR*DEBT are insignificant. The final robustness check is to determine the sensitivity of the standard errors to alternative correction procedures.

V.5 Standard errors

We explore the sensitivity of the standard errors by obtaining state-specific residual variances and period-specific residual variances from the OLS residuals from the specification in Table 2, including both state and period fixed effects. The variance across the state-specific residuals is 3.43, and the variance across the period-specific residuals is 3.59. The ratio of the latter to the former is 1.06, indicating slightly greater dispersion across periods than across states, which is why Table (2) reports period-weight panel corrected standard errors (PCSEs). If cross-section weight PCSEs are used instead, results are equivalent. The cross-section weight PCSE for the ACIR–DEBT coefficient is (2.12). We also calculate the period SUR (PCSE) for the ACIR-DEBT coefficient, and again the results are equivalent. The period SUR PCSE is (2.03).

VI. DISCUSSION

Given the regression estimates in Table (2), we are now in a position to discuss whether or not BBRs change the effect of debt on growth, and if so, how. The significantly positive coefficient for the ACIR-debt interaction suggests that the answer to the first question is 'yes': more stringent BBRs make debt more productive, consistent with the WW view that these rules increase the productiveness of debt by restricting borrowing to productive public infrastructure. This effect could work either directly through the greater productivity of investments in public infrastructure or more indirectly by assuring lenders that future borrowing will be limited to public infrastructure, resulting in lower costs of borrowing. Both channels are consistent with the evidence in Alt and Lowry (1997) and others that borrowing costs are lower for states with more stringent budget rules.

How large is the effect of BBRs on the productivity of debt in terms of growth? With no BBR in place (i.e., with an ACIR index of zero) and evaluated at sample means, the coefficients in Table (2) indicate that a one standard-deviation increase in the stock of debt (an increase of

5.6 percent of personal income) decreases the steady-state (five-year) growth rate by 17.4 percent of real income—just over 3 percent per year. However, with a strict BBR in place (i.e., with an ACIR index of 10), the coefficients predict instead an increase in the steady-state (five-year) growth rate of about 15 percent for a one-standard-deviation increase in debt.

VII. POLICY CONSIDERATIONS

Evidence elsewhere indicates that balanced-budget rules lower borrowing costs and restrain levels of state and local debt. Our evidence also indicates that high levels of debt can slow growth, but our unique contribution is to provide an arguably well-identified test of the hypothesis that state-level balanced-budget restrictions in the U. S. increase growth by restricting borrowing to productive public infrastructure. Evidence that state balanced-budget restrictions increase growth via this channel provides the first formal test of the WW hypothesis and adds a new perspective to the effects of balanced-budget rules by suggesting that the benefits of tax smoothing and fiscal flexibility (permitted in the absence of a balanced-budget restriction) may come at the expense of lower growth. We close however, with caveats; we provide evidence relevant to the WW hypothesis, not a comprehensive analysis of the merits of balanced-budget and other fiscal restrictions. In particular, the context for fiscal policy differs in obvious and important ways for countries and sub-national states. Auerbach (2007) for example, analyzes federal fiscal policy 'rules' and issues for recent decades.

REFERENCES

Adam, C. S. and Bevan, D. L. (2005). "Fiscal Deficits and Growth in Developing Countries." *Journal of Public Economics*, 89: 571-597.

Advisory Commission on Intergovernmental Relations. 1987. Fiscal Discipline in the Federal System: national reform and the experience of the states. Washington, DC: ACIR.

Ash, M. Herndon, T. and Robert Pollin (2013). 'Does High Public Debt Consistently Stifle Economic Growth? A Critique of Reinhart and Rogoff' working paper, U. Mass.

Alesina, A, and Bayoumi, T. (2006). "The Costs and Benefits of Fiscal Rules: Evidence From U.S. States," NBER working paper.

Auerbach, A. J. (2000). "Formation of Fiscal Policy: The Experience of the Past Twenty-Five Years", Federal Reserve Bank of New York *Economic Policy Review*, April, 1-15.

Bania, N., Gray, J. A. and Stone, J. A. 2007. "Growth, Taxes, and Government Expenditures: Growth Hills for U.S. states," *National Tax Journal*, 60:193-204.

Barro, R. J. (1989). "Government Spending in a Simple Model of Endogenous Growth," Journal of Political Economy, 98: S103-26.

Bleaney, M., Gemmell, N., and Kneller, R. (2001). "Testing the Endogenous Growth Model: Public expenditure, taxation, and growth over the long run," *Canadian Journal of Economics*, 34: 36-57.

Carlino, G. and R. Inman (2013). "Local Deficits and Local Jobs: Can States Stabilize Their Own Economies? NBER working paper number 18930.

Checherita-Westphal, C., A.H. Hallett and P. Rother (2012) "Fiscal Sustainability using Growth-Maximising Debt Targets." ECB working paper series number 1472.

Eberts, R. W. and J. A. Stone (1992*).* *Wage and Employment Dynamics in Urban Labor Markets,* Kalamazoo: W.E. Upjohn Institute for Employment Research.

Gray, J. and J. Stone (2012). "Nonlinearities in the Effects of Debt and Fiscal Policy: Evidence from the States," *Economic Research Guardian* 2:2: 152-159.

Greiner A. and B Fincke (2012) *Public Debt and Economic Growth,* Berlin and Heidelberg: Springer-Verlag Press.

Greiner, A. (2013)"Debt and growth: Is There a Non-Monotonic Relation?" *Economics Bulletin*, 33: 340-47.

Krol, R. and S. Svorny (2007). "Budget Rules and State Business Cycles," *Public Finance Review*, 35: 530-544.

Poterba, J. and K. Reuben. eds. (1999). *Fiscal Institutions and Fiscal Performance*: U. Chicago.

Bohn, H. and R. Inman (1995). "Constitutional Limits and Public Deficits: Evidence From the U.S. States," *Carnegie-Rochester Conference Series on Public Policy*, 45: 3-76.

Capeci, J. (1994). "Local Fiscal Policies, Default Risk, and Municipal Borrowing Costs," *Journal of Public Economics*, 53: 73-89.

Inman, R. (1996). "Do Balanced Budget Rules Work? U.S. Experience and Possible Lessons for the EMU," NBER Working Paper no. 5838.

Levinson, A. (1997. "Balanced Budgets and Business Cycles: Evidence from the States," Dept. of Economics, U. Wisconsin, photocopy.

Lowry, R. and J. Alt. 1997). "A Visible Hand? Bond markets, Political Parties, Balanced Budget Laws, and State Government Debt," Dept. of Government, Harvard, photocopy.

Mofidi, Ala and Joe Stone (1990). "Do State and Local Taxes Affect Economic Growth?" *The Review of Economics and Statistics* 72: 686-691.

Poterba, J. (1989). "Tax Reform and the Market for Tax-Exempt Debt," *Regional Science and Urban Economics*, 195: 37-62.

Poterba, J. (1994). "State Responses to Fiscal Crises: The Effects of Budgetary Institutions and Politics," Journal *of Political Economy*, 102: 799-8.

Reed, R. (2008). 'The Robust Relationship between Taxes and U. S. State Income Growth,' *National Tax Journal*, 61:5-80.

Reinhart, C. and K. Rogoff (2010). Growth in a Time of Debt. American Economic Review, 100, 2: 573-8.

Wallis, J. and B. Weingast (2009). "Dysfunctional or Optimal Institutions? State Debt Limitations, the Structure of State and Local Governments and the Finance of American Infrastructure" *in Fiscal Challenges: An Interdisciplinary Approach,* Elizabeth Garrett, Elizabeth Graddy, and Howell Jackson, eds. Cambridge: Cambridge U. Press.

TABLE 1 Summary Statistics (49 states 1957-2007)

	GROWTH	DEFICIT*	DEBT	TAXES	FED	UR
Mean	11.76	8.33	17.27	10.07	3.57	5.93
Median	11.39	8.32	16.63	10.01	3.37	5.58
Maximum	30.14	11.59	42.69	17.75	7.68	15.45
Minimum	-9.93	0.00	4.52	7.13	0.91	2.00
Std. Dev.	5.56	0.99	5.61	1.33	1.27	2.11
Obs	441	441	441	441	441	441

GROWTH is the log change in real personal income per capita (five-year intervals).

Fiscal data are percentage points of state personal income. See text for data.

*scaled to non negative by subtracting the smallest deficit. The raw mean is (-0.09).

TABLE 2 Two-Lag OLS Growth Estimates
49 states 1957-2007
no. obs. 391)
(period weight PCSEs)

Variable	Coeff	Std. Error	Prob.
C/100**	2.740	0.861	0.000
DEFICIT-2	0.226	0.256	0.378
DEBT-2*	-20.406	10.727	0.058
ACIR*DEBT-2**	3.878	1.230	0.002
ACIR*YEAR**	-0.014	0.004	0.001
TAXES-2	0.106	0.340	0.755
FED-2	0.209	0.529	0.692
UR**	-2.105	0.234	0.000
UR-2**	0.459	0.126	0.003
State and period fixed**			
R-squared	0.611		

**p< .05 *p<.10
See text for data

TABLE 3 Hausman Test, Two-Lag Estimates
(no. obs. 391)
(period weight PCSEs)

Variable	Coefficient	Std. Error	Prob.
C/100**	2.52	1.05	0.02
DEFICIT-2	0.01	0.30	0.97
DEBT-2	-9.61	18.41	0.60
ACIR*DEBT-2*	3.84	2.26	0.09
ACIR*YEAR**	-0.01	.001	0.02
TAXES-2	0.38	0.29	0.19
FED-2	0.45	0.40	0.26
UR**	-2.08	0.17	0.00
UR-2*	0.28	0.16	0.09
H-TEST*	-2.25	1.29	0.08
State fixed**			
Period fixed**			
R-squared	0.58		

** p<.05, * p<.10
See text for data

TABLE 4 Two-Lag GMM Growth Estimates

Notes:
**p<.05
*p<.10

See text
for data

(No.obs. 391)
White period instrument weighting matrix
(White-period std.err.)

Variable	Coefficient	Std. Error	Prob.
GROWTH-1)	-0.013	0.042	0.743
DEFICIT-2)	0.138	0.148	0.353
DEBT-2)**	-26.361	7.134	0.000
ACIR*DEBT-2)**	4.535	0.898	0.000
TAXES-2)	0.086	0.141	0.540
ACIR*YEAR**	-0.013	0.002	0.000
FED-2)**	0.396	0.254	0.119
UR**	-1.936	0.125	0.000
UR-2)**	0.478	0.081	0.000
State and Period fixed**			
R-squared	0.522		
AR2 (p=0.433)			
J-statistic	27.3	Instrument rank 37	

TABLE 5 One-Lag OLS Growth Estimates

(No. obs. 489)

(period weight PCSEs)

Variable	Coefficient	Std. Error	Prob.
C/100**	2.945	84.133	0.005
ACIR*DEBT-1	2.976	1.927	0.123
DEFICIT-1	-0.069	0.285	0.806
DEBT-1	-30.583	16.918	0.071
ACIR*YEAR**	-0.013	0.005	0.009
TAXES-1	0.291	0.264	0.272
FED-1**	0.994	0.364	0.006
UR**	-2.070	0.160	0.000
UR-1**	0.662	0.159	0.000
Cross-section fixed**			
Period fixed**			
R-squared	0.583		

**p<.05 *p<.10

See text for data

TABLE 6 Hausman Test One-Lag Estimates

(No. obs. 391)

(period weight PCSEs)

Variable	Coefficient	Std. Error	Prob.
C/100**	2.369	0.997	0.027
ACIR*DEBT-1	-0.091	2.085	0.965
DEFICIT-1	-0.223	0.290	0.442
DEBT-1	-15.425	17.875	0.388
ACIR*YEAR**	-0.012	0.006	0.043
TAXES-1	-0.073	0.288	0.798
FED-1**	1.539	0.392	0.000
UR**	-2.112	0.160	0.000
UR-1**	0.578	0.171	0.000
H-TEST**	2.265	0.821	0.006

State and Period fixed**
**p< .05, *p<.10
R-squared 0.615
see text for data

TABLE 7 One-Lag GMM Growth Estimates

(No. obs.489) period weight instrument matrix)
White-period standard errors)

Variable	Coefficient	Std. Error	Prob.
GROWTH-1**	0.199	0.062	0.001
DEFICIT-1	0.114	0.363	0.752
DEBT-1	-14.390	13.498	0.287
ACIR*DEBT-1**	3.273	1.526	0.032
ACIR*YEAR**	-0.009	0.002	0.000
TAXES-1**	-0.734	0.258	0.004
FED-1**	1.890	0.213	0.000
UR**	-2.107	0.114	0.000
UR-1**	1.141	0.246	0.000
State and Period fixed**			

**p< .05. *p<.10
See text for data.

AR2 p=0.141 J-statistic 31.4 Instrument rank 38

TABLE 8 Placebo Regressions: Median Result for Census Regions (Census Region 4)
Dependent Variable: GROWTHREG4
Total panel (unbalanced) observations: 440
White-period standard errors)

Variable	Coefficient	Std. Error	Prob.
C/100**	0.767	0.385677	0.0470
ACIR*DEBT-2	0.320	0.540276	0.5538
DEFICIT-2	0.171	0.140513	0.2231
DEBT-2	-3.881	4.266604	0.3635
ACIR*YEAR*	-0.004	0.002400	0.0568
FED-2	0.228	0.168553	0.1760
UR	0.032	0.044967	0.4749
UR-2	0.053	0.086337	0.5395
TAXES-2	-0.216	0.251445	0.3907
Cross-section fixed**			
Period fixed**			
R-squared	0.846		

**sig at .05 see text for data and variables.