National Tax Journal
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BALANCED BUDGETS AND BUSINESS CYCLES: EVIDENCE FROM THE STATES

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Abstract - *This paper presents evidence that stringent balanced budget requirements enforced in some U.S. states have exacerbated business cycles in those states. The effect is not apparent directly. However, among states where fiscal policy may have more macroeconomic consequences (large states), the difference in volatility between states with lenient and strict balanced budget rules is larger (more negative or less positive) than among states where fiscal policy may be less relevant (small states). Two implications are suggested: (1) states' fiscal policies have real macroeconomic consequences, and (2) strict balanced budget requirements increase business cycle volatility.*

"To keep the budget balanced would aggravate recessions."

—Petition signed by 1,100 economists, including 11 Nobel laureates, *NY Times* 2/3/97.

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INTRODUCTION

Proposals to amend the U.S. Constitution to require annually balanced budgets have gained support in recent years. In early 1997, such an amendment came one vote short of passage in the Senate. Opponents such as the 1,100 economists cited above argue that the amendment would force the government to operate procyclical fiscal policies and would exacerbate business cycle peaks and troughs. To balance its books, the government would have to increase taxes and decrease spending in recession years. Proponents of the amendment note that virtually every U.S. state has a balanced budget requirement (BBR) of some sort, and that state budget deficits have been proportionally smaller than the federal budget deficit has been. Therefore, the claim goes, the federal government should learn from the experience of the states and enact a BBR of its own (ACIR, 1987). This paper asks two related and inseparable questions: (1) can state fiscal policy affect state business cycles? and (2) do states with strict balanced budget rules have more volatile business cycles?

The conventional wisdom dictates that fiscal policy cannot be effective at the

National Tax Journal
Vol 51 no. 4 (December1998) pp. 715-32

state level (Oates, 1972; Fisher, 1996). On the surface, the analogy between state budgets and the federal budget is admittedly weak. States typically have more sophisticated budget processes than the federal government, including separate accounts for capital expenditures, public employee pensions, and social insurance programs. States are small and very open economies when compared to the entire U.S. economy. State debt is largely held externally, much of the incidence of state taxes and expenditures is borne by residents of other states, and state business cycles are closely related to national economic conditions (Gramlich, 1987).

There are, however, several reasons to question this conventional wisdom. For one, several individual states have very large economies, even on a global scale. California's and New York's economies are larger than most nations' economies. Moreover, state and local expenditures are large relative to federal expenditures. In 1997, the federal government spent $1,528 billion net of intergovernmental grants. State and local governments spent $983 billion, 64 percent as much (Council of Economic Advisers, 1998). Also, relative price changes such as the oil price shocks of the 1970s have had vastly different effects on different regions of the country. Similarly, variations in defense spending, international trade, and agricultural conditions all have different implications for different states. Evidence for these regional business cycles can be seen in the persistence of abnormally low or high state unemployment rates.

Another reason to question the conventional wisdom stems from the growing importance of nontraded services to the U.S. economy, shrinking the import and export propensities of states (Gramlich, 1987). Furthermore, in one important way, states may be *more* capable of fiscal policy than the federal government. Rational expectations models suggest that federal deficit spending will not affect behavior by life-cycle consumers anticipating future tax increases. At the state level, however, consumers can avoid future tax increases by leaving the state, a much less costly option than leaving the nation.[1] Finally, the growing internationalization of capital markets means that much of the national debt is held externally. While this does not increase the ability of individual states to enact fiscal policies, it does diminish the difference between state and federal governments in this respect.

There is wide variation in the degree to which state BBRs limit states' ability to conduct countercyclical fiscal policy. While only Vermont has no balanced budget rule, a number of states only require that their governors submit balanced budgets. What their legislatures do and what happens at the end of the year are unconstrained. At the other extreme, many states prohibit budget deficits from being carried over into the next fiscal year or biennium.[2] Only one paper has examined the correlation between these rules and state business cycles. Alesina and Bayoumi (1996) regress the standard deviation of annual gross state product (1965–1992) on an index of state balanced budget stringency, the average level of gross state product, the percentage of state product from mining, and an indicator for southern states. That paper finds a small, negative, and statistically insignificant coefficient on the index, and concludes that balanced budget rules have had no intensifying effect on business cycles. However, business cycle fluctuations are not the focus of that paper, so it does not address the possibility that unobserved

National Tax Journal
Vol 51 no. 4 (December1998) pp. 715-32

state characteristics are correlated with both business cycle fluctuations and the existence of state balanced budget rules. The rest of this paper is devoted to measuring the effect of balanced budget rules on business cycles using quarterly data, bivariate indicators of BBR stringency, and an explicit control for unobserved relationships between business cycle variability and balanced budget rules.

STATE BBRs

While virtually every U.S. state has some statutory or constitutional requirement that its budget balance, the requirements vary widely in effectiveness. The Advisory Commission on Intergovernmental Relations (ACIR, 1987) documents five types of BBRs: (1) the governor has to submit a balanced budget; (2) the legislature has to pass a balanced budget; (3) the state may carry over a deficit but must correct it in the next fiscal year; (4) the state may not carry over a deficit into the next budget period (often two years long); and (5) the state may not carry over a deficit into the next fiscal year. The first two BBRs are *ex ante* and impose no constraint on what happens at the end of the year if expenditures exceed revenues. The third BBR is also virtually irrelevant, because states with such requirements may continue to carry over deficits from year to year, as long as at the beginning of each year, revenues are forecast to match expenditures. Some states with BBRs of this type systematically overestimate future revenues and underestimate future expenditures (Briffault, 1996). The fourth and fifth BBRs are binding, at least in principle. They require that adjustments be made when tax revenues fall short of expenditures. The fourth BBR binds every budget period, which can be two years long for states with biennial budget cycles. The fifth binds every fiscal year, regardless of the length of the state's budget cycle. Intuitively, only the last two BBRs should matter to state fiscal policy, and this paper focuses attention solely on these most stringent BBRs.

One might fear that none of these BBRs is effective because states can use budgeting "gimmicks" to make unbalanced budgets appear balanced. Indeed, such techniques precipitated fiscal crises in Michigan and New York City, suggesting that credit markets impose more fiscal constraint than any constitutional or statutory provisions against deficit spending (Fisher, 1996). However, most state midyear budget gaps appear to be met by spending decreases or revenue increases, rather than interfund or intertemporal transfers that would be symptomatic of gimmickry (GAO, 1993). Moreover, this fear can be addressed empirically. If this paper were to conclude that BBRs have had no effect on business cycles, one explanation might be that the requirements themselves are ineffective.

Several studies have recently found that strong state BBRs have large effects on the size of state budget deficits and on states' propensities to run deficits. These papers are summarized in Table 1. The ACIR (1987) index is based on whether the state has one of the five types of BBRs, whether they are statutory or constitutional, and a subjective assessment of their importance by ACIR staff. Though the index ranges from zero to ten, most of the distribution is in the top few values. The average index value is 8.1, and the standard deviation is 2.6. Twenty-six states have ACIR indices of ten, and 42 states have indices of six or higher. The ACIR regressed measures of budget balances on their index values and other explanatory values, and concluded that states with strict rules

National Tax Journal
Vol 51 no. 4 (December1998) pp. 715-32

TABLE 1
STUDIES OF THE EFFECT OF BALANCED BUDGET RULES ON STATE BUDGET DEFICITS

Study	Years	Dependent Variable	Definition of Balanced Budget Stringency	Results

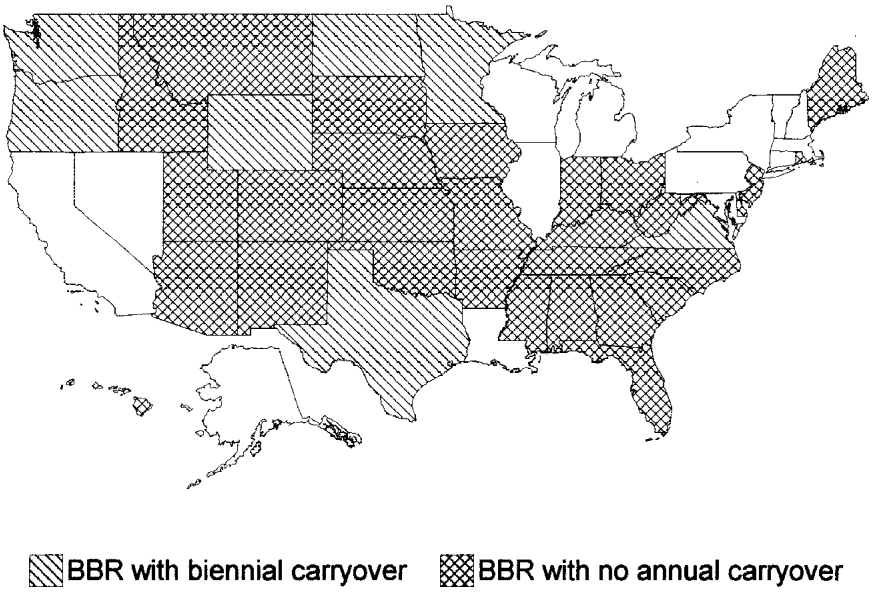
National Tax Journal
Vol 51 no. 4 (December1998) pp. 715-32

have lower deficits and lower state expenditures. Though the ACIR study uses a cross section of 1984 data, its conclusions are supported by the other papers listed in Table 1, all of which use panels of data.

The rest of the studies cited in Table 1 use the ACIR data on BBRs in various forms. Alt and Lowry (1994) estimate a simultaneous equations model of state revenues and expenditures, and find that states subject to BBRs eliminated deficits more quickly. Poterba (1994) shows that states with ACIR indices of six or greater enact larger expenditure reductions in response to unexpected short-run deficits. Alesina and Bayoumi (1996) find the value of the ACIR index to be positively correlated with states' primary budget surpluses. And Bohn and Inman (1996) find that states prohibited from carrying budget deficits into the next fiscal year have larger per-capita general fund surpluses, and are less likely to run deficits. They explore a number of budget provisions, including weak *ex ante* provisions, and find that the "no annual carryover" BBR is the best predictor of fiscal policy, and their result is apparent in bivariate correlations as well as in a more complex random effects model.

The papers in Table 1 thus provide ample evidence that BBRs have real effects on state fiscal policy, and in theory, there are reasons to believe that these requirements exacerbate business cycle swings. Drawing on the literature examining the effect of BBRs on state fiscal policy, this paper focuses attention on *ex post* rules, those that explicitly prohibit deficits from being carried over.[3] In particular, it uses the last two ACIR rules: deficits may not be carried over into the next biennium, and deficits may not be carried over into the next year. Figure 1 illustrates which states have each of the two BBRs. The 29

FIGURE 1. State Balanced Budget Requirements



National Tax Journal
Vol 51 no. 4 (December1998) pp. 715-32

darkly shaded states prohibit deficits from being carried into the next fiscal year, regardless of whether the state has an annual or biennial budget. This is the rule that Bohn and Inman (1996) find to be the best predictor of state budget deficits, and it corresponds to an ACIR index of nine or higher (out of ten). The seven lightly shaded states prohibit deficits from being carried into the next biennium. This second rule, represented by the total of the 36 shaded states, corresponds to an ACIR index of seven or higher. It only relaxes the balanced budget constraint for those states with biennial budget cycles. In sum, the 29 darkly shaded *no annual carryover* states are a subset of the 36 shaded *no biennial carryover* states.

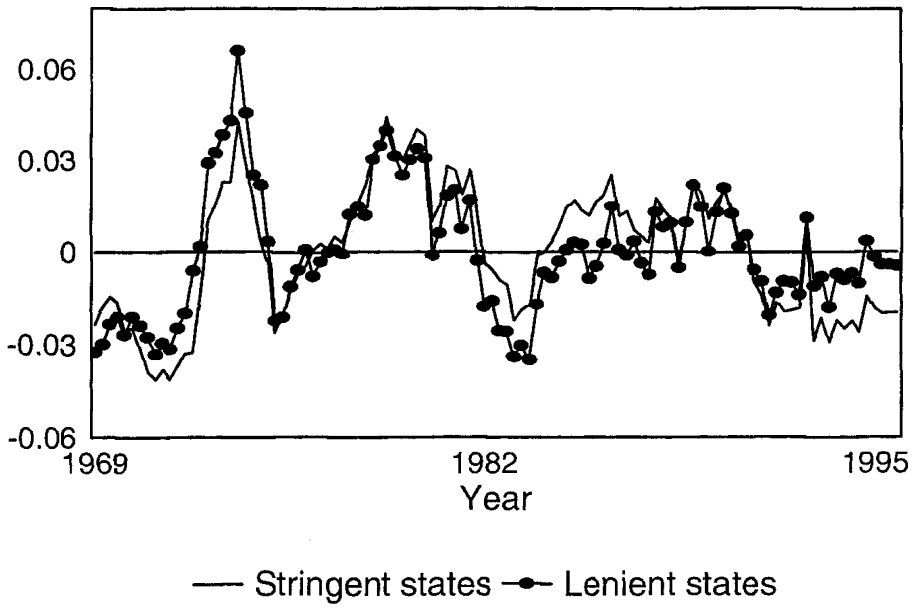
Except for Tennessee, which passed a strict constitutional BBR in 1977, all of the states' BBRs have been in place for many decades. Many belong to states' original constitutions and date from the 19th century (ACIR, 1987). Therefore, there has been virtually no change in these requirements over time, and any estimate of the effect of BBRs on state business cycles necessarily rests on the cross-sectional relationship between BBRs and cyclical fluctuations.

STATE BUSINESS CYCLE VOLATILITY

To quantify business cycle volatility, I use two measures calculated from a panel of quarterly data on the natural logarithm of personal income per capita in each state from 1969 to 1995.[4] For each state, I calculated the quarterly difference between the logarithm of per-capita personal income and its trend line. Each state's trend line was calculated from a simple regression of the log of personal income per capita on a time trend. Its slope is therefore the mean growth rate for the period.

Figure 2 depicts the average difference each quarter between actual and trend per-capita income, in logs, separating

FIGURE 2. Income per Capita. Deviation from Log Trend



National Tax Journal
Vol 51 no. 4 (December1998) pp. 715-32

the strict and lenient states as defined by the strictest no annual carryover rule. Five national recessions are represented during the period. One notable feature of Figure 2 is that both groups of states have similar aggregate fluctuations, with larger peaks and troughs in the 1970s than more recently. This is probably a result of the national economy receiving particularly severe energy price shocks during the 1970s. There were, however, relevant changes in fiscal policy between the 1970s and 1980s. For example, starting around 1980, many states began enacting budget stabilization funds, often called "rainy day funds," that allow them to save for unexpected revenue shortfalls. Prior to 1981, few states had such funds (Gold, 1981). By 1983, 19 states had rainy day funds in place (Gold, 1984), and by 1994, 45 states had them (ACIR, 1995). Also, in the latter period, many states' unemployment insurance trust funds borrowed from the federal treasury, repaying the loans from payroll taxes during more prosperous years (Burtless and Vroman, 1984). Because state BBRs only affect general funds, and unemployment benefits are paid from separate trust funds, these borrowing procedures could smooth business cycle fluctuations even in states with strict BBRs. In what follows, I will test the sensitivity of cyclical fluctuations to BBRs separately for the pre and post-1980 periods.

A second notable feature of Figure 2 is that there is not a large difference between the variability of stringent and lenient states. If anything, lenient states appear to have slightly *higher* peaks and slightly *lower* troughs. However, states differ in many regards, some of which may make them more or less likely to have volatile cycles, and some of which may make them more or less likely to have stringent BBRs. States whose economies are heavily dependent on one industry may have more volatile time-series patterns of personal income. Such states may be less likely to enact strict BBRs, knowing that they will only make their economic conditions more volatile. In that case, we would observe strict states having less volatile business cycles, not because BBRs ameliorate business cycles but rather because states with volatile business cycles are less likely to enact BBRs.[5]

To address these issues, in part by controlling for other characteristics of states, I use two quantitative measures of business cycle volatility. The first measure of business cycle volatility is the standard deviation over the 108 quarters of the difference between actual personal income per capita, in logs, and that predicted by the mean log growth rate. This measure is used by Romer (1986), and is intended to measure the *average* fluctuation of the state's macroeconomy over the period. The second measure of business cycle volatility is the difference between the largest peak over the period and the deepest trough. It is the largest positive difference between actual and predicted log personal income per capita, plus the absolute value of the largest negative difference between actual and predicted log personal income per capita. This second measure is intended to capture the *largest* business cycle fluctuation over the time period.[6]

The first row of Table 2 presents data on the first measure of cyclical fluctuations, the standard deviation of the deviation from the trend in log personal income per capita. For the 21 states with lenient BBRs (according to the strictest no annual carryover rule), the average standard deviation is 0.0442. For the 29 strict states, the average is 0.0335. The difference is 0.0107 and confirms the

National Tax Journal
Vol 51 no. 4 (December1998) pp. 715-32

TABLE 2
AVERAGE STATE CHARACTERISTICS BY PRESENCE OF A BBR:
STRICTEST BBR—MAY NOT CARRY OVER ANNUAL DEFICIT

	Lenient States	Strict States
Number of States	21	29
Standard deviation from log trend	0.0442 (0.0058)	0.0335** (0.0019)
Maximum difference from log trend	0.1966 (0.0321)	0.1571 (0.0125)
Population in thousands (1969)	5,600 (1,251)	2,860* (435)
Import propensity	0.4644 (0.0160)	0.5060 (0.0116)
Growth rate of personal income/capita	0.0047 (0.0002)	0.0048 (0.0001)
Personal income/capita, 1969 ($1,000s)	13.45 (0.41)	11.45* (0.35)
Percent of gross state product from agriculture	2.39 (0.10)	2.53 (0.08)
Percent of gross state product from mining	7.39 (2.83)	4.33 (1.18)
Percent of gross state product from services	14.18 (1.22)	12.48 (0.39)
Percent of gross state product from manufacturing	19.47 (1.97)	20.96 (1.55)

*Difference is statistically significant at 5 percent.
**Difference is statistically significant at 10 percent.

visual examination of Figure 2, that lenient states have more volatile business cycles on average. The second row of Table 2 presents data on the second measure of cyclical fluctuations, with similar results: lenient states have larger business cycle fluctuations. One explanation for this counterintuitive result is that states have characteristics that are correlated with both mild business cycles *and* strict BBRs.

The remainder of Table 2 contains other state characteristics. States with strict BBRs tend to have smaller populations, import more goods and services as a fraction of total consumption, have lower personal income per capita, and derive less of their gross state product from mining and services, relative to agriculture and manufacturing. To account for these correlates, as well as potentially unobserved correlates, in what follows, I employ an empirical strategy that controls for these observable state characteristics and takes advantage of the difference between large and small states to account for unobserved state characteristics.

DIFFERENCES OF MEANS

One way to control for unobserved state characteristics correlated with both BBRs and volatility would be to exploit the time-series nature of the data. In principle, one could examine volatility under different regimes in different periods, controlling for state characteristics with fixed effects. In practice,

National Tax Journal
Vol 51 no. 4 (December1998) pp. 715-32

TABLE 3
COMPARING TENNESSEE TO THE REST OF THE U.S., BEFORE AND AFTER 1977

	1969–77	1978–95	Difference
Standard deviations:			
Tennessee	0.0264	0.0248	0.0017
Other 49 states	0.0389	0.0354	0.0035
Difference	–0.0125	–0.0106	**–0.0018**
Maximum differences:			
Tennessee	0.0898	0.1018	–0.0120
Other 49 states	0.1450	0.1419	0.0031
Difference	–0.0553	–0.0401	**–0.0151**

Columns and rows may not sum due to rounding errors.

however, during the entire period examined (1969–95), only one state switched its status: Tennessee in 1977, from lax to stringent. Though more anecdotal than systematic, Table 3 presents data comparing Tennessee to the other 49 states both before and after 1977.

The first row of Table 3 compares the standard deviation of the deviations from the trend in Tennessee's personal income. It shows that the variability of income decreased after 1977, when Tennessee's strict BBR took effect. This decrease must be viewed in light of Figure 2, however, which depicts the national decline in volatility. The second row of Table 3 shows that the other 49 states displayed an even larger decline after 1977. The difference between Tennessee and the other 49 states (–0.0018) suggests that Tennessee's business cycle volatility did not decrease as much as it might have had the state not enacted a stringent BBR. A similar calculation with the alternative measure of volatility, the maximum peak-trough difference, is presented in the lower half of Table 3. Here, Tennessee's measured volatility *increased* after 1977, while the other 49 states experienced decreases, on average. Again, one interpretation of these results is that, without its stringent BBR, Tennessee's business cycle swings might be smaller.

While suggestive, these results must be viewed as anecdotal because they rely on only one state and one policy change. A more systematic approach necessarily relies on cross-sectional comparisons of cyclical fluctuations in states with strict BBRs to those in states with lenient BBRs. However, comparing these two groups directly is not likely to be informative, judging from Figure 2 and the first rows of Table 2. Instead, I compare the two groups indirectly, by examining their cyclical fluctuations relative to a second state attribute that is exogenous (beyond the control of state governments) and correlated with the ability of state fiscal policy to affect business cycle fluctuations. That second attribute is the size of the state, as measured by its population.

The conventional wisdom that states cannot conduct independent fiscal policies rests on arguments that also suggest that smaller (less populous) states are less able to conduct fiscal policy than larger states. Two pieces of empirical evidence support this contention. First, relatively more workers commute into or out of small states each day to their jobs. In 1990, in the smallest 25 states, 4.5 percent of working residents commuted across state borders, while in the largest 25 states, 3.2 percent of workers commuted interstate.[7] The correlation

National Tax Journal
Vol 51 no. 4 (December1998) pp. 715-32

between the percent of workers commuting interstate and the population of the state is –0.22. Though the differences are small, it appears that a greater fraction of the economic incidence of taxes or subsidies in small states is likely to travel across state borders with those commuting workers, reducing the ability of small states to affect statewide economic conditions with fiscal policy.

A second piece of evidence that small states have less local fiscal impact involves their propensity to import goods and services. A 1981 study (RSRI) of the 1977 Commodity Flow Survey conducted by the U.S. Department of Transportation calculated each state's "propensity to import." The import propensity is the fraction of the dollar value of goods and services consumed by each state that comes from outside the state. These range from a low of 0.33 for California to a high of 0.65 for North Dakota (omitting Alaska and Hawaii). The largest 24 states' average import propensity was 0.45, while the smallest 24 states' average was 0.53. The difference between these averages is statistically significant at five percent. As with the commutation patterns, though the differences in import propensities are small, they do suggest that large states have more ability to affect their local economies through fiscal policy. A greater share of the economic incidence of small states' taxes and expenditures will be borne by other states' residents. Any constraint on fiscal policy, such as a BBR, will affect the economies of large states more than the economies of small states.

If state government deficits or surpluses matter more in large states than small states, then the difference between fluctuations in lenient and strict states should be larger (more negative or less positive) for large states than for small states. As an extreme example, suppose small states' cyclical fiscal policies have absolutely no macroeconomic effects, and that large states' policies have some. Then the lenient-strict difference in cyclical fluctuations for the small states is purely a function of other things, and the lenient-strict difference for the large states will be a function of the other things *plus* the effect of the strict BBRs. If the other effects are additive, then the difference between the lenient-strict difference for large states and the lenient-strict difference for small states will be a function of the BBRs alone.

Table 4 presents evidence supporting this conjecture. The first row of Table 4 shows the difference between the lenient and strict states for the largest 25 states only. That difference is 0.0055. The second row presents the lenient-strict difference for the smallest 25 states, and that difference is 0.0282. For the large states, the difference is less positive than for the small states, suggesting that fiscal policy is more effective for large states and reduces business cycle volatility. The difference between these two differences (–0.0228) is statistically significant at ten percent.

There is an alternative way to look at the data in Table 4. Small states have more volatile business cycles than large states. This may be due in part to their economies being less diverse and therefore more sensitive to adverse shocks affecting one industry or sector. Looking at the lenient states only, for the large lenient states, the measure of cyclical volatility is 0.0339, and for the small lenient states, it is 0.0649. The difference is –0.0311. Looking at the strict states only, the large-small difference is –0.0083. Thus, large states in general have smoother business cycles, and for lenient states, the large-

National Tax Journal
Vol 51 no. 4 (December1998) pp. 715-32

TABLE 4
STATE CYCLICAL FLUCTUATIONS: STANDARD DEVIATION FROM TREND:
STRICTEST BBR—MAY NOT CARRY OVER ANNUAL DEFICIT

	Lenient States	Strict States	Difference
Largest 25 states:			
Average	0.0339	0.0284	0.0055**
N	14	11	25
Standard error	0.0026	0.0019	0.0032
Smallest 25 states:			
Average	0.0649	0.0367	0.0282*
N	7	18	25
Standard error	0.0129	0.0025	0.0131
Difference (large-small):	–0.0311*	–0.0083*	**–0.0228****
Standard error	0.0132	0.0031	0.0135

*Statistically significant at 5 percent.
**Statistically significant at 10 percent.

TABLE 5
DIFFERENCES-IN-DIFFERENCES: ALTERNATIVE SPECIFICATIONS

	Strictest BBR: May Not Carry Over Annual Deficit (1)		Less Strict BBR: May Not Carry Over Biennial Deficit (2)	
	Dependent Variable		Dependent Variable	
Independent variables	Standard Deviation (1a)	Maximum Difference (1b)	Standard Deviation (2a)	Maximum Difference (2b)
All 50 states	–0.0228**	–0.1130	–0.0108	–0.0222
	(0.0135)	(0.0820)	(0.0182)	(0.0798)
Without Tennessee	–0.0231**	–0.1133	–0.0111	–0.0227
	(0.0136)	(0.0822)	(0.0182)	(0.0799)
Omitting the 20 midsized states	–0.0259**	–0.1245	–0.0074	0.0187
	(0.0151)	(0.0940)	(0.0193)	(0.0915)
Difference based on "import propensity"[a]	–0.0140	–0.0624	0.0073	0.0652
	(0.0109)	(0.0718)	(0.0078)	(0.0394)
Pre-1980 only	–0.0349**	–0.1140	–0.0156	–0.0259
	(0.0205)	(0.0803)	(0.0282)	(0.0844)
Post-1980 only	–0.0124	–0.0753*	–0.0064	–0.0121
	(0.0087)	(0.0354)	(0.0105)	(0.0289)
Post-1980, strict BBRs without rainy day funds	–0.0156	–0.0391	—	—
	(0.0120)	(0.0453)	—	—

Standard errors in parentheses.
*Statistically significant at 5 percent.
**Statistically significant at 10 percent.
[a]Omits Alaska and Hawaii.

small difference is even more pronounced. A potential explanation for the difference in these differences (–0.0228) is that the large states have smoother business cycles in part because they are able to have effective countercyclical fiscal policy. When large states have stringent BBRs, they are less able to deficit spend, and their business cycles look more like those of small states.

The average size of business cycle fluctuations over all 50 states, as

National Tax Journal
Vol 51 no. 4 (December1998) pp. 715-32

measured by the standard deviation from the trend in log personal income per capita, is 0.0389. The differences-in-differences reported in Table 4 is –0.0228, a large fraction of overall cyclical variation. The implications therefore are significant empirically as well as statistically: in large states, where fiscal policy matters most, the absence of a strict balanced budget requirement is associated with cyclical fluctuations that are less than half as volatile as the national average.

The calculations in Table 4 can be repeated for a variety of definitions of BBRs, business cycle volatility, and subsets of states. Table 5 presents the differences-in-differences for alternative versions of the calculations in Table 4. Column (1) contains the calculations for the strictest category of BBRs—deficits may not be carried into the next fiscal year. Column (2) contains the calculations for the less stringent version that relaxes the constraint slightly for states with biennial budget processes. Table 5 also compares the two measures of business cycle volatility: standard deviation and maximum difference. For the strictest BBR, both volatility measures are always negative and sometimes statistically significant. The maximum difference is always larger (as expected).

The first row of differences in Table 5 contains the calculations for all 50 states, including a repetition of the Table 4 difference (–0.0228). In the second set, I drop Tennessee from the sample because it only enacted its strict BBR in 1977. The results are the same. Third, I omit the middle 20 states, ranked by their populations. Comparing the largest 15 states to the smallest 15 states sharpens the large-small distinction and increases the absolute size of the relevant differences-in-differences for the strictest BBRs. Next, I use the RSRI (1977) measure of import propensity rather than size of states. In other words, the numbers reported are the lenient-strict difference for low-import states, minus the lenient-strict difference for high-import states. If low-import states are similar to large states in that their fiscal policies have more local incidence, then these numbers can be interpreted similarly to the rest of Table 5, as an indication that strict BBRs exacerbate business cycle fluctuations.

Finally, at the bottom of Table 5, I examine the data for the period prior to 1980 and after 1980 separately. This division is motivated by Figure 2, which shows that business cycle swings were larger prior to 1980. The differences-in-differences are larger before 1980, and smaller after 1980. One potential explanation for the change in volatility after 1980 is the adoption of rainy day funds by many states. These funds allow states that cannot carry over deficits to spend down previously accumulated surpluses. Sobel and Holcombe (1996) examine rainy day funds in detail, and find that states that mandate contributions to rainy day funds during non-recession years experienced fewer fiscal disruptions during the 1990–91 national recession.

To test the importance of these rainy day funds, I calculated versions of the differences where the definition of strict BBRs is limited to those states that cannot carry over annual deficits and that had not established rainy day funds as of 1989 (NASBO, 1989).[8] For these ten states, the difference-in-differences for the standard deviation from trend was –0.0156. While this is larger than the value that includes states with rainy day funds, suggesting that rainy day funds smooth out cyclical variation or weaken the effect of strict BBRs, the

National Tax Journal
Vol 51 no. 4 (December1998) pp. 715-32

result is not statistically significant. Furthermore, when the same calculation is done using the maximum peak-trough difference, the difference is *smaller*.

Though suggestive, the rainy day fund results deserve extreme caution. First, only two states (Texas and North Carolina) are large and had no rainy day fund as of 1989. By 1992, even these two states had adopted rainy day funds, rendering the analogous analysis impossible. Second, these funds differ in scope, flexibility, and levels of funding. Clearly, the existence of rainy day funds is meaningless if their balances are small. In sum, while the post-1980 decrease in the measured effect of BBRs may be due to rainy day funds, one cannot rule out the simultaneous state unemployment trust borrowing or the diminution of aggregate national shocks relative to those of the 1970s.

In every case explored in Table 5, the differences-in-differences for the first definition of BBRs is negative and sometimes statistically significant. For the second BBR, the differences are usually negative, but never statistically significant. The difference between columns (1) and (2) suggests that only the strict version of BBRs matters for macroeconomic cycles, which would be expected given Bohn and Inman's (1996) finding that only the strict version matters for fiscal policy. Alternatively, it may be that adding seven states to the strict category leaves too few (14) states in the lenient category to draw statistical inference from the difference between large lenient and small lenient states. Nevertheless, for a variety of definitions of key variables and subsets of the data, the negative differences-in-differences in column (1) suggest that strict BBRs may increase cyclical volatility. However, the differences are not precisely estimated, and are rarely even marginally significant statistically. To estimate the effect of BBRs on business cycles more precisely, it is necessary to control for other characteristics of states that may be correlated with business cycle volatility.

A REGRESSION APPROACH

To include other characteristics of states, I estimate versions of the following equation:

1

$$V_s = \theta X_s + \beta_0 + \beta_1(Lenient_s) + \beta_2(Large_s) + \beta_3(Lenient_s \text{ and } Large_s) + \varepsilon_s$$

where V_s is volatility, X_s is a vector of state characteristics, $Lenient_s$ is an indicator equal to one if state s has a lenient BBR, $Large_s$ is an indicator equal to one if state s is large, and e_s is an error term. The coefficient of interest is that on the interactive term (β_3), equal to one for states classified as both lenient and large. If other characteristics, X_s, are not included, then the ordinary least-squares estimate of β_3 is mathematically equal to the difference-in-difference values reported in Tables 4 and 5. The regression framework, however, allows other characteristics to be included, albeit at the usual costs of parameterization assumptions.

Table 6 presents OLS estimates of equation 1. The average growth rate 1969–95 is included as a regressor on the theory that faster growing states may have different levels of income volatility. Personal income per capita is included because income may be correlated with volatility. To capture the effect of industrial composition on volatility, the shares of four major

National Tax Journal
Vol 51 no. 4 (December1998) pp. 715-32

TABLE 6
CONTROLLING FOR OTHER STATE CHARACTERISTICS

	Strictest BBR: May Not Carry Over Annual Deficit (1)		Less Strict BBR: May Not Carry Over Biennial Deficit (2)	
	Dependent Variable		Dependent Variable	
Independent Variables	Standard Deviation (1a)	Maximum Difference (1b)	Standard Deviation (2a)	Maximum Difference (2b)
Constant	0.0682**	0.6363*	0.0653	0.5629*
	(0.0362)	(0.2385)	(0.0424)	(0.2781)
Lenient dummy	0.0222*	0.1279*	0.0177*	0.0733
	(0.0052)	(0.0345)	(0.0077)	(0.0505)
Large state dummy	–0.0013	–0.0004	–0.0035	–0.0150
	(0.0046)	(0.0300)	(0.0049)	(0.0323)
Lenient × Large	–0.0210*	–0.1130*	–0.0156**	–0.0631
	(0.0062)	(0.0406)	(0.0082)	(0.0536)
Growth rate	–3.4993	–40.26**	–2.99	–34.99
	(3.3474)	(22.03)	(3.86)	(25.33)
Personal income per capita (1969)	0.0026*	0.0015	0.0026**	0.0032
	(0.0012)	(0.0081)	(0.0014)	(0.0092)
Percent GSP agriculture[a]	0.1502**	0.8773	0.1498	0.9396
	(0.0889)	(0.5850)	(0.1024)	(0.6721)
Percent GSP mining	0.0265	–0.1925	0.0478	0.0021
	(0.0406)	(0.2670)	(0.0469)	(0.3076)
Percent GSP services	–0.2046*	–1.1164*	–0.2024*	–0.9761**
	(0.0675)	(0.4446)	(0.0822)	(0.5395)
Percent GSP manufacturing	–0.1161*	–0.8114*	–0.1168*	–0.7735*
	(0.0381)	(0.2511)	(0.0447)	(0.2935)
Northeast	0.0063	0.0604	0.0077	0.0694
	(0.0060)	(0.1394)	(0.0070)	(0.0458)
Midwest	–0.0049	0.0009	–0.0018	0.0187
	(0.0054)	(0.0357)	(0.0061)	(0.0403)
West	–0.0169*	–0.1094*	–0.0143*	–0.0947*
	(0.0056)	(0.0370)	(0.0064)	(0.0421)
N	50	50	50	50
R^2	0.80	0.73	0.74	0.65

Standard errors in parentheses.
*Statistically significant at 5 percent.
**Statistically significant at 10 percent.
[a]GSP: gross state product.

industries in gross state product are included as regressors. Finally, to control for regional differences in volatility, three of four census region indicators are included. For the strictest BBR, in column (1), the coefficient on the interactive term (*Lenient* × *Large*) is negative and statistically significant (–0.0210), suggesting that large states have milder business cycles than small states and that this large-small difference is greater for fiscally lenient states than it is for states with strict BBRs.

National Tax Journal
Vol 51 no. 4 (December1998) pp. 715-32

Column (1b) of Table 6 contains a version of equation 1 in which the measure of *volatility* is the maximum difference between peak and trough over the 108 quarters. Here, the interactive coefficient (–0.1130) is also statistically significant, whereas the simple differences-in-means in Table 5 were rarely statistically significant. Columns (2a) and (2b) of Table 6 contain versions of equation 1 using the less strict BBR. Here, the interactive coefficients are also negative, but they are smaller and less statistically significant. Again, one interpretation of Table 6 is that only the narrow annual definition of fiscal stringency matters. Alternatively, it may be that adding seven states to the stringent category leaves too few states in the lenient large and lenient small categories to draw statistical inferences.

Other correlates in Table 6 are unsurprising. High-income states tend to have slightly more volatile economies. State economies that are more dependent on services and manufacturing are less volatile than those dependent on mining and agriculture. And western states appear to have the least volatile economies.

Table 7 presents the interactive coefficients from a variety of alternative specifications of equation 1. Dropping Tennessee has little effect on the results. In its second row, Table 7 contains a version where I omit the 20 middle states, ranked by population. As with the simple comparisons of means in Table 5, omitting the midsized states sharpens the large-small difference and increases the magnitude of the interactive coefficient for the strictest BBRs. Next, in the third row of Table 7, I include a version of equation 1 in which a dummy for states with small import propensities has been substituted for

TABLE 7
ALTERNATIVE SPECIFICATIONS: LENIENT × LARGE INTERACTIVE COEFFICENTS

	Strictest BBR: May Not Carry Over Annual Deficit (1)		Less Strict BBR: May Not Carry Over Biennial Deficit (2)	
	Dependent Variable		Dependent Variable	
Independent Variables	Standard Deviation (1a)	Maximum Difference (1b)	Standard Deviation (2a)	Maximum Difference (2b)
Without Tennessee	–0.0204*	–0.1076*	–0.0151**	–0.0594
	(0.0063)	(0.0413)	(0.0082)	(0.0540)
Omitting the 20 midsized states	–0.0230*	–0.1269**	–0.0126	–0.0321
	(0.0102)	(0.0672)	(0.0125)	(0.0817)
Interact *Lenient* with *Import Propensity* rather than *Large*[a]	–0.0136*	–0.0773**	–0.0047	–0.0078
	(0.0067)	(0.0443)	(0.0081)	(0.0531)
Pre-1980	–0.0317*	–0.1077*	–0.0258*	–0.0674
	(0.0092)	(0.0396)	(0.0120)	(0.0516)
Post-1980	–0.0108**	–0.0711*	–0.0054	–0.0142
	(0.0056)	(0.0200)	(0.0069)	(0.0288)
Post-1980, strict BBRs without rainy day funds	–0.0123	–0.0442	—	—
	(0.0087)	(0.0360)	—	—

Standard errors in parentheses.
*Statistically significant at 5 percent.
**Statistically significant at 10 percent.
[a]Omits Alaska and Hawaii.

National Tax Journal
Vol 51 no. 4 (December1998) pp. 715-32

the size dummy. For the strict BBR, the interaction coefficient remains negative and statistically significant, though slightly smaller.[9]

Finally, examining only the pre-1980 years yields larger (negative) coefficients, while the post-1980 difference-in-difference estimates are smaller. As in Table 5, in the last rows of Table 7, I estimate specifications defining stringent states as those that cannot carry over annual deficits and had no rainy day funds as of 1989. Similar results appear: standard deviations are larger, maximum differences are smaller, and neither is statistically significant. As with the uncontrolled differences in Table 5, one cannot rule out other concurrent changes as explaining the apparently diminished importance of BBRs after 1980.

Tables 6 and 7 show that large states have milder business cycles than small states, and that for states with lenient BBRs, this large-small difference is even greater. This difference-in-differences is statistically significant, and approximately stable in magnitude for a variety of sensitivity tests. One explanation for these findings is that BBRs force states to operate procyclical fiscal policies, thereby amplifying their business cycles. However, due to the difference-in-difference approach, and the assumptions inherent in that approach, it is difficult to draw precise conclusions about the magnitude of the effect.

The size of the coefficient on the *Lenient* × *Large* interactive term for the specification with standard deviation as the dependent variable is approximately –0.02. Across all states, the average value of the standard deviation (over time) of the cyclical fluctuations in log per capita income is 0.039. The coefficient thus suggests that the standard deviations of cyclical fluctuations are about half as large in large lenient states. For the specification with the maximum difference as the dependant variable (column (1b) of Table 7), the coefficient is approximately –0.11. Across all states, the average value of the maximum difference is 0.17. Thus, for this specification also, the coefficient suggests that large states without strict BBRs have fluctuations that are significantly less severe.

Conclusions

This paper presents evidence from U.S. states that strict BBRs exacerbate business cycle fluctuations. The effect is not apparent directly—those states with strict requirements do not on average have larger cyclical fluctuations. However, among states more able to implement countercyclical fiscal policy (large states), the difference in volatility between states with lenient and strict balanced budget rules is larger (more negative or less positive) than among states less able to implement fiscal policy (small states). These results rest on several admittedly strong assumptions. First, they rely on a cross section of 50 states. Second, they rely on a difference-in-difference analysis that assumes (a) any bias resulting from the simultaneous determination of business cycle volatility and balanced budget rules is additive, and (b) fiscal policy matters more for large states than small states. The results, however, are apparent for numerous subsets of the 50 states, two subsets of the years 1969–95, two measures of cyclical variability, two measures of state fiscal sensitivity, and simple difference-of-means comparisons as well as regressions that include other covariates.

Setting aside the caveats for the moment, the results have remarkable

National Tax Journal
Vol 51 no. 4 (December1998) pp. 715-32

implications. First, they imply that fiscal policy in general has real macroeconomic consequences, a subject of longstanding academic debate. Second, they imply that state governments in particular have the ability to affect their local macroeconomic conditions, a subject of debate among state and local public finance economists. Third, the results indicate that strict BBRs exacerbate business cycle fluctuations, a finding that highlights one important cost of the proposed constitutional amendment to balance the federal budget.

ENDNOTES

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[1] If states' citizens own immobile factors (land) and the present discounted value of future taxes is capitalized into those factors, then rational expectations models may still find fiscal policy irrelevant. However, for mobile citizens who rent, rather than own, state fiscal policy may be more relevant.

[2] Excellent summaries of these various state requirements can be found in ACIR (1987), Poterba (1994), and Bohn and Inman (1996).

[3] Both Alt and Lowry (1994) and Bohn and Inman (1996) demonstrate that the weak *ex ante* BBRs have no effect on states' actual deficits or surpluses.

[4] Quarterly, seasonally adjusted, total state personal income figures are from the Bureau of Economic Analysis at the Commerce Department. State populations, however, are only available annually. The denominator of personal income per capita, therefore, is based on a linear interpolation of the annual state population totals.

[5] The fact that many BBRs date from the 19th century certainly limits the possibility that their enactment and current business cycle fluctuations are determined simultaneously. However, states may have attributes that are correlated with both the existence of BBRs and with business cycle volatility, and it may not be too far-fetched to imagine that some of these attributes have remained stable for long periods of time (Poterba, 1996).

[6] Romer (1986) uses the peak-to-trough change, a slightly different measure. I rely on the maxima and minima to avoid having to identify quarterly peaks and troughs of business cycles for each state. Romer's work uses annual data and focuses on the aggregate U.S. economy, for which generally accepted peak and trough dates are available.

[7] Author's calculations from 1990 U.S. Census data.

[8] These states are Arizona, Arkansas, Hawaii, Kansas, Montana, North Carolina, Oregon, South Dakota, Texas, and West Virgina.

[9] To ensure that the interactive coefficients are not being generated by some unforseen statistical artifact, I also estimated a version of equation 1 where the large-small indicator was replaced by a randomly generated binomial variable. Unsurprisingly, the coefficient on the interaction between the *Lenient* indicator and the random binomial was small and never statistically significant for either measure of volatility or BBR stringency.

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