

Rainy Day Funds and State Government Savings

Brian Knight
University of Wisconsin, Madison
Madison, WI 53706

and

Arik Levinson
University of Wisconsin, Madison
Madison, WI 53706

May 17, 1999

Abstract

This paper examines the effect of rainy day funds on state savings behavior. We find that states with rainy day funds have higher total balances than states without such funds and also have higher balances after adoption than before adoption. Furthermore, rainy day fund deposits increase total balances dollar-for-dollar. While we cannot rule out that states planning future savings may adopt rainy day funds, our findings are robust to the inclusion of measures of savings preferences. In sum, these funds appear to belong to the growing set of fiscal institutions with real fiscal and economic consequences.

Introduction

During the last two decades, virtually all of the U.S. states have adopted 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 1984, 18 states had enacted rainy day funds, and by 1994, 45 states had them (ACIR, 1995). These accounts are designed to help state governments stabilize public spending over time by saving during booms and using the balances to cover revenue shortfalls during recessions. In 1996, rainy day fund balances averaged \$135 per capita, or 6 percent of total state expenditures.

The existence and significant size of these accounts do not necessarily imply that rainy day funds have increased total government savings because the revenue deposited into a rainy day fund may only represent funds that would otherwise have been saved in states' general funds. That is, to the extent that rainy day fund and general fund balances are fungible substitutes, deposits to rainy day funds may simply replace savings in general funds, resulting in no net increase in total state government savings.

While a number of previous studies have shown that various state fiscal institutions have real economic consequences,¹ few have examined rainy day funds in particular. Sobel and Holcombe (1996) show that states with rainy day funds suffered less fiscal stress during the 1990-1991 national recession, where fiscal stress is measured by how much states' expenditures fell below their long-run growth. While this suggests that the funds have had an effect on states' fiscal health, it may be that the states with rainy day funds are inherently savers, and would have saved enough to avoid fiscal stress in their general fund accounts even in the absence of special savings accounts. Similarly, Levinson (1998) shows that states with rainy day funds have

smoother business cycle fluctuations over the period from 1969 to 1995. While suggestive, this finding also cannot rule out that states with rainy day funds are inherently better at smoothing fiscal policy.

In this paper, we ask whether rainy day funds have increased state government savings above what they would have been in the absence of the funds. We use a panel of data describing states and their governments between 1984 and 1997, a period during which 27 states adopted rainy day funds. This variation within states over time allows us to use state fixed effects to control for any unobserved differences between states with and without rainy day funds.

Trends in State Government Balances

Figure 1 plots trends in average reaper-capita state rainy day fund balances and average per-capita total balances over time, where total balances are defined as general funds plus rainy day funds.² To allow for comparisons across years, all balances are presented in 1997 dollars. The upward trend in rainy day fund balances is especially pronounced in the years following the 1990-1991 national recession. Figure 2 plots these same balances as a percentage of total state expenditures. The upward trend in state balances is less apparent in Figure 2 due to the growth of state expenditures during the 1990s, though the patterns in both figures are roughly the same. Together these first two figures demonstrate a significant rise in both rainy day fund and total balances over this period. The recent and simultaneous increase in rainy day funds and total balances provides some initial evidence that rainy day funds have increased total savings. If rainy day funds had not increased total state savings, we would have expected to see no relationship between the two time series' depicted in Figures 1 and 2.

As cross-sectional evidence on the effect of rainy day funds, Figures 3 and 4 compare the 17 states with rainy day funds (RDFs) enacted prior to 1985 to the four states without rainy day funds for all years 1984-1997. In every year but 1990 and 1991, the two recession years, the RDF states had vastly larger total balances per capita than the non-RDF states. These differences suggest that rainy day funds have more than simply replaced general fund balances, and have actually increased total state government saving.

Both the time-series evidence from Figures 1 and 2 and the cross-sectional evidence in Figures 3 and 4 suggest that rainy day funds have increased state savings. However, none of these aggregate comparisons demonstrates that the relationship is necessarily causal. It may be, for example, that state governments that choose to adopt rainy day funds are systematically different from states that do not choose to do so. In econometric terms, states may be heterogeneous in ways that are correlated with both their propensity to save and their establishment of rainy day funds. If that heterogeneity is unobservable, then it will likely bias any assessment of the effect of RDFs on savings in favor of finding a large effect.

Furthermore, states that choose to adopt the funds may do so during the years in which they plan to save. In econometric terms, while rainy day funds may affect savings behavior, planned savings behavior may simultaneously affect the adoption of RDFs. Like the unobserved heterogeneity, this simultaneity will bias any attempt to measure the effect of RDFs on states' savings behavior in favor of finding a large effect. To address these issues of heterogeneity and simultaneity, in the empirical analyses that follow we control for as many observable state differences as possible and we employ a fixed-effects specification, taking advantage of the fact that from 1984 to 1997 RDFs were adopted by 17 states that did not previously have such funds.

Before describing those results, however, we must first detail the ways in which we characterize the existence, size, and stringency of states' rainy day legislation.

Measuring the significance of rainy day funds

To assess how rainy day funds have affected state savings, we use three alternative characterizations of state rainy day funds: whether states have adopted such funds; the size of the funds' balances; and the rules for contributions and withdrawals from the funds.

1) Rainy day fund indicators: 1984-1997

We begin by characterizing states as either having rainy day funds or not. The National Association of State Budget Officers (NASBO) published data on rainy day fund balances between 1984 and 1997. We define a rainy day fund as existing in the year in which a positive balance is first reported in these NASBO data.³ If rainy day funds increase state savings, states with rainy day funds will have larger total balances than states without rainy day funds. Specifically, in a regression of total state balances on a rainy day fund indicator and other state characteristics, including state fixed effects, the rainy day fund indicator coefficient will be large and positive. Alternatively, if rainy day funds merely substitute for savings that otherwise would have gone into general fund balances, the rainy day fund indicator coefficient will be zero.

2) Rainy day fund balances: 1987-1997

As a second test of the effect of rainy day funds on state savings, we will examine the *size* of state rainy day funds, again using the NASBO data on rainy day fund balances. For this approach, we are unable to use data for the first four years because rainy day fund balances were not reported separately from general fund balances for some states. If rainy day funds increase

state savings, states with larger rainy day fund balance should have larger total balances. Specifically, in a regression of total balances on rainy day fund *balances* and other state characteristics, including state fixed effects, the rainy day fund coefficient will be close to one. Alternatively, if rainy day fund contributions merely replace contributions to general funds, the rainy day coefficient will be close to zero.

3) Fund legal provisions

Finally, a third approach examines the *characteristics* of state rainy day funds, recognizing that not all rainy day funds are alike. The National Council of State Legislatures (NCSL) has surveyed states as to provisions of their funds. We use three aspects of these provisions, summarized in table 1.⁴ First, some state's laws mandate deposits to rainy day funds in certain years. For example, some states must deposit their fiscal-year-end surplus into the fund, while in other states, deposits are determined by a formula tied to the performance of the state economy. At the other extreme, some states only deposit funds into rainy day funds through occasional legislative appropriation. Second, some states have maximum limits, or caps, on fund sizes. These limits range from 2 percent of expenditures in three states to 25 percent of expenditures in Michigan. The most common limit is 5 percent, the generally accepted minimum level of total balances by credit rating agencies (Eckl, 1997), and the amount suggested by the NCSL (Sobel and Holcombe, 1996). Third, fund provisions differ in the availability of the balances for expenditure. Some states require only legislative appropriation for withdrawal, making the funds a politically attractive source of spending. Other states have provisions requiring that funds be used only in years of economic downturn (determined through formulas) or in the case of a revenue shortfall or a deficit. If the rainy day funds increase total savings, and

if the legal provisions of those funds matter, then we would expect rainy day funds with deposit and withdrawal formulas, and with large size limits, to have the largest balances.

Empirical model and results

Table 2 provides some initial summary statistics for three sets of states: those with rainy day funds in all years of the sample period, those without funds in any years of the period, and those with funds adopted during period. All monetary variables are presented in 1997 dollars. The 17 states with rainy day funds in 1984, in column (1), have significantly larger average balances over the period 1984-1997 than the other two sets of states. The four states without rainy day funds in all years tend to have lower per-capita incomes, higher unemployment rates, lower per-capita government expenditures and more elected officials from the Democratic party.

Table 3 estimates the relationship between total state balances per-capita and state rainy day funds, controlling for other state characteristics. It presents the results of both ordinary least squares (OLS) and fixed-effects regressions using the three alternative measures of the significance of rainy day funds. The regressions also control for per-capita income, state unemployment rates, per-capita government expenditures, legislative party control variables, and state governors' political party affiliations. The coefficient on per-capita income captures the state government's marginal propensity to save, while the unemployment rate captures business cycle effects on balances. The government expenditure variable is included because large state governments may desire to save more or less in absolute terms than smaller governments. The political variables attempt to measure differences across political parties in savings preferences. We exclude Alaska and Hawaii from all the regressions. Alaska's savings behavior is unlike the

rest of the states. It had balances of \$3.3 billion in 1997, or \$5,534 per capita. Minnesota, the next highest state, had a per-capita balance of \$550. In addition, Nebraska is excluded due to its unicameral, nonpartisan legislature.

For comparison with fixed-effects results, the first three columns present OLS regressions for the three alternative characterizations of the rainy day fund. Column (1) regresses total balances per-capita on state characteristics and an indicator equal to one for states with rainy day funds. The RDF indicator coefficient demonstrates that states with rainy day funds save more than states without these funds, although the standard errors are large. While the coefficient is statistically insignificant, its magnitude suggests that rainy day funds increase total balances by 20 percent, relative to the sample average of \$78 per-capita.

Low-income states save more than high-income states. States experiencing low unemployment rates increase their balances, while those experiencing high unemployment spend down those balances, as befits the expenditure-smoothing motive for savings. State governments with higher expenditures tend to save more. States with Republican legislatures and Democrat governors save more than those with Democrat legislatures and Republican governors, although these political coefficients are all statistically insignificant.

Column (2) employs the balance in the rainy day fund as the key independent variable. The coefficient on this variable, close to one and statistically different from zero at the 1 percent level, suggests that savings in rainy day fund balances do not replace savings in general fund balances. Rather, rainy day funds appear to increase total balances approximately dollar-for-dollar. The other variables have similar signs and magnitudes.

Column (3) of Table 3 uses measures of the rainy day fund legal provisions. Because we have only one year of NCSL data describing RDF provisions, there is no time variation in the seven fund descriptors. We have assumed that the details of state rainy day funds have remained constant over time. Therefore, the only within-state variation in the seven RDF provision dummy variables at the bottom of column (3) occurs in the 27 states that adopted rainy day funds, in the year in which they adopted their funds. Of course, there remains considerable cross-state variation between the 43 states that have RDFs at any point during our sample. Put differently, the seven RDF provision dummies are technically interactions between constant descriptive variables about each state and the RDF indicator at the top of column (3).

States with strict fund deposit provisions, those states with either required savings through a formula or a requirement of saving the year-end budget surplus, have larger balances than those with deposits only through appropriation, the omitted category. However, the year-end surplus variable is statistically insignificant. States with high fund balance limits or no limits save more than states with low limits, the omitted category. Funds with no limits save significantly more (\$64) than those with low limits (less than 5 percent). States with the strictest withdrawal provision, allowing access to rainy day fund balances only according to a formula measuring the state of the business cycle, save more than states that allow access to balances through appropriation, the omitted category, though this coefficient is statistically insignificant. The final variable, withdrawal allowed in the case of a revenue shortfall, has an unexpected negative sign although it is also statistically insignificant. The inclusion of these fund legal provision variables' causes the rainy day fund indicator coefficient (in the top row) to become

negative, suggesting that strict legal provisions, rather than the mere existence of an account, increase total savings.⁵

Columns (4) - (6) present the results of fixed-effects regressions. The only difference between these regression and those in the first three columns is that the fixed-effects regressions include 48 state-specific dummy variables. The coefficients on the RDF indicator in column (4) and on the associated balances in column (5) are smaller than the corresponding OLS coefficients. The point estimate of the RDF indicator coefficient suggests that states that adopt rainy day funds save 14 percent more than before they adopted those funds, again relative to the sample average of \$78 per-capita. The reduction in these coefficients may reflect the institutional endogeneity: if states with a strong preference for savings tend to adopt funds, the cross-sectional OLS results will be biased toward finding a positive effect of these funds. The fixed-effects regressions attempt to correct this endogeneity by controlling for time-invariant unobserved differences in savings preferences across states. However, even when state fixed effects are included, the coefficient on the RDF balance in column (5) is very close to one, suggesting that rainy day funds increase total savings dollar-for-dollar.

Column (6) estimates a fixed-effects version of column (3), with fund provision regressors. Because we have no intertemporal variation in fund provisions, with state fixed effects included there is no variation over time in the fund provisions that can be separately identified from fixed effects and from the existence of the RDFs in the first place. For example, the Deposit By Formula coefficient tells us that those states that adopted rainy day funds during our sample, and which required deposits to those funds based on economic formulas, had insignificantly higher balances per capita (\$0.137) after adopting their funds than before,

compared to the before-after difference for states that adopted funds with occasional legislative appropriations. Only six states fit this description, four of which adopted their funds in 1987 or earlier, the very beginning of our time series. Consequently, the Deposit By Formula coefficient is small and insignificant. The high limit states, those with a limit between 5 and 9 percent, those with a limit above 9 percent, and those with no limit, save more than states with a low limit, though only the limit above 9 percent coefficient is statistically significant. In contrast with the OLS results, states with the strictest withdrawal provisions, allowing access to funds only in recessions, save significantly more than states that can access funds through appropriation.

Taken together, these results suggest that rainy day funds and their associated balances increase total savings. While the methods for deposit do not seem to change total balances, states with high balance limits or no limits tend to save more than states with low limits. Similarly, states that provide access to the fund balances only during an economic downturn tend to have higher balances.

As a sensitivity check, table 4 presents the results including Alaska, Hawaii, and Nebraska and excluding the legislative variables. Though still insignificant, the coefficients on the RDF indicator in columns (2) and (5) are larger, reflecting the fact that Alaska has large balances and a rainy day fund. The coefficients on the RDF balance are smaller but still close to one. The limit variables are all positive, and a few are statistically significant, similar to those in table 3. Finally, the withdrawal variables are negative in the OLS results and positive in the fixed-effects results. The withdrawal through formula coefficient is large, although statistically

insignificant, in the fixed-effects case. Thus, the results are relatively insensitive to the exclusion of these three states.

Discussion and conclusion

We have found that not only do states with rainy day funds save more in total than states without such funds, but states that *adopt* rainy day funds save more than they did before they adopted the funds. Even more striking, balances saved in rainy day funds appear to increase total state savings *dollar-for-dollar*. We recognize that these findings do not comprise incontrovertible evidence that the rainy day funds themselves *cause* the savings. States that decide to save may simultaneously decide to adopt rainy day funds. However, because we have controlled for numerous state characteristics, including state fixed effects, we believe these results provide considerable evidence that the enactment of rainy day funds changes states' fiscal policies.

There are, however, several potential alternative explanations for our findings. One might ask for example, why the growth in rainy day funds has occurred only recently, mostly in the last two decades. One explanation is that California's Proposition 13 tax reforms in 1978 represented in large part a revolt against the high concurrent state budget surpluses (Gold,1983). It may be, therefore, that Proposition 13 made states leery of saving in general fund accounts, and they created rainy day funds to provide a politically acceptable means of savings.

We should also be careful not to interpret these results by themselves as implying that rainy day funds have smoothed states' fiscal policies. It might be, for example, that states without rainy day funds smooth expenditures over the business cycle by borrowing during

recession years. Of the 17 states that have had rainy day funds the longest, since before 1985, 11 have strict balanced budget rules that do not allow deficits to be carried over into subsequent fiscal years. And, of the ten states that have adopted rainy day funds most recently, or not at all, seven have strict balanced budget rules. However, Sobel and Holcombe (1996) and Levinson (1998) provide evidence that states with rainy day funds do experience less volatile fiscal cycles, which suggests that states without rainy day funds are not finding alternative means of smoothing expenditures. So though the finding in this paper do not alone show that rainy day funds smooth expenditures, in conjunction with the rest of the literature, they do have that implication.

In sum, state budget stabilization funds, or rainy day funds, appear to belong with a growing class of state fiscal institutions that have acknowledged real effects on state fiscal policy and hence on welfare.

Endnotes

The authors are grateful to Corina Eckl at NCSL, Stacy Mazer at NASBO, Daphne Kenyon, Leslie McGranahan, and Gary Wagner for helpful discussions and guidance.

1. For a general discussion of fiscal institutions, see Alt & Lowry (1994), or Poterba (1994, 1996). For specific examples, see Holtz-Eakin (1998), Carter and Schap (1990), Alm and Evers (1991), or Dearden and Husted (1993) on the line-item veto; see Levinson (1998) or Bohn and Inman (1996) on balanced budget rules; see Rueben (1995), or Elder (1992) on tax and expenditure limitations; see McGranahan (1998) or Poterba (1995) on state borrowing restrictions, see Knight (1998a) or Temple (1998) on supermajority rules for tax increases; see Matsusaka (1995) on voter direct legislation; and see Knight (1988b) on unfunded mandates.

2. All of the figures and tables exclude Alaska and Hawaii, except where noted.

3. An alternative definition would be the first year a state is listed with a balance, even if the balance is zero. Unfortunately, in some years of the NASBO survey, zeros are used to denote both states without funds and states with a zero balance rainy day funds. Gary Wagner, at West Virginia University, has conducted a phone survey of the states and found a close, but not exact, match between his findings and the NASBO data.

4. For the four states with more than one rainy day fund, the provisions from the stricter fund are used for the deposit and withdrawal method variables. Further, the fund balance limit for these states is the combined limit from the two funds.

5. Sobel and Holcombe (1996) found that the mere existence of rainy day funds during the 1990-1991 recession did not alleviate fiscal stress. Rather, those states with the strict deposit requirements were the ones with the reduced fiscal stress.

References

- Advisory Commission on Intergovernmental Relations (ACIR).** *Fiscal Discipline in the Federal System: Experience of the States*. Washington, DC: ACIR, 1987.
- Advisory Commission on Intergovernmental Relations (ACIR).** *Significant Features of Fiscal Federalism*. Washington, DC: ACIR, 1985.
- Alesina, Alberto and Tamim Bayoumi.** "The Costs and Benefits of Fiscal Rules: Evidence from U.S. States" NBER Working Paper No. 5614. Cambridge, MA: National Bureau of Economic Research, June, 1996.
- Alm, James and Mark Evers.** "The Item Veto and State Government Expenditures", *Public Choice* 68 No. 1 (January, 1991): 1-15.
- Alt, James E. and Robert C. Lowry.** "Divided Government, Fiscal Institutions, and Budget Deficits: Evidence from the States." *American Political Science Review* 88 No. 4 (December, 1994): 811-28.
- Bohn, Henning and Robert P. Inman.** "Balanced Budget Rules and Public Deficits: Evidence from the U.S. States" NBER Working Paper No. 5533. Cambridge, MA: National Bureau of Economic Research, April, 1996.
- Carter, John and David Schap.** "Line-Item Veto: Where is thy Sting?" *Journal of Economic Perspectives* 4 No. 2 (Spring, 1990): 103-18.
- Chari, V, Larry Jones and Ramon Marimon.** "The Economics of Split-Ticket Voting in Representative Democracies." *American Economic Review* 87 No. 5 (December, 1997): 957-76.
- Dearden, James and Thomas Husted.** "Do Governors Get What They Want?: an Alternative Examination of the Line-Item Veto." *Public Choice* 77 No. 4 (December, 1993): 707-23.
- Eckl, Corina.** "States Broaden the Scope of Rainy Day Funds." National Conference of State Legislatures. (November, 1997).
- Elder, Harold.** "Exploring the Tax Revolt: an Analysis of the Effects of State Tax and Expenditure Limitation Laws." *Public Finance Quarterly* 20 No. 1 (January, 1992): 47-63.
- Gold, Steven D.** "The Struggles of 1981: Budget Actions in the States." *State Legislatures* (July/August, 1981): 25.
- Gold, Steven D.** "Contingency Measures and Fiscal Limitations: The Real World Significance of Some Recent State Budget Innovations." *National Tax Journal* 37 No. 3 (September, 1984): 421-32.

Holtz-Eakin, Douglas. "The Line Item Veto and Public Sector Budgets: Evidence from the States." *Journal of Public Economics* 36 No. 3 (August, 1988): 269-92.

Knight, Brian. "Supermajority Voting Requirements for Tax Increases: Evidence from the States." University of Wisconsin, Madison. Mimeo, 1998a.

Knight, Brian. "Fiscal Effects of Unfunded Mandates: Evidence from State Reimbursement Requirements." University of Wisconsin, Madison. Mimeo, 1998b.

Levinson, Arik. "Balanced Budgets and Business Cycles: Evidence from U.S. States." *National Tax Journal* 51 No. 1 (December, 1998): 715-32.

McGranahan, Leslie. "State Debt Referenda and the Composition of State Indebtedness and Spending." Federal Reserve Bank of Chicago. Mimeo, 1998.

Matsusaka, John. "Fiscal Effects of Direct Legislation: Evidence from the Last 30 Years." *Journal of Political Economy* 103 No. 3 (June, 1995): 587-623.

National Association of State Budget Officers (NASBO). *Fiscal Survey of the States*. Washington DC: NASBO, 1984-1997.

Poterba, James M. "State Responses to Fiscal Crises: The Effects of Budgetary Institutions and Politics." *Journal of Political Economy* 102 No. 4 (August, 1994): 799-821.

Poterba, James M. "Capital Budgets, Borrowing Rules, and State Capital Spending." *Journal of Public Economics* 56 No. 2 (February, 1995): 165-87.

Poterba, James M. "Budget Institutions and Fiscal Policy in the U.S. States." *American Economic Review: Papers and Proceedings* 86 No. 2 (May, 1996): 395- 400.

Rueben, Kim. "Tax Limitations and Government Growth: the Effect of State Tax and Expenditure Limits on State and Local Government." MIT. Mimeo, 1995.

Sobel, Russell S. and Randall G. Holcombe. "The Impact of State Rainy Day Funds in Easing State Fiscal Crises During the 1990-1991 Recession." *Public Budgeting and Finance* 16 No. 3 (Fall, 1996): 28-48.

Temple, Judy. "State Revenue and Supermajority Limitations, Unobserved State Effects, and State Revenue Growth." Northern Illinois University. Mimeo, 1998.

Table 1: State Rainy Day Fund Details as of April 1995

State	Deposit method	Limit (as % of expenditures)	Withdrawal method	Year started (first balance)
Alabama	formula	2%	appropriation	1988
Alaska	appropriation	no limit	appropriation	pre-1985
Arizona	formula	5%	formula	1994
Arkansas				
California	year-end surplus	no limit	revenue shortfall	pre-1985
Colorado	formula	2%	revenue shortfall	pre-1985
Connecticut	year-end surplus	5%	revenue shortfall	pre-1985
Delaware	year-end surplus	5%	revenue shortfall	pre-1985
Florida	formula	20%	revenue shortfall	pre-1985
Georgia	year-end surplus	3%	appropriation	pre-1985
Hawaii				
Idaho	appropriation	no limit	appropriation	pre-1985
Illinois				
Indiana	formula	7%	formula	1985
Iowa	appropriation	10%	appropriation	pre-1985
Kansas	appropriation	no limit	appropriation	1993
Kentucky	appropriation	no limit	revenue shortfall	1987
Louisiana	appropriation	no limit	revenue shortfall	never balance
Maine	year-end surplus	4%	appropriation	1985
Maryland	formula	no limit	appropriation	1987
Massachusetts	year-end surplus	5%	revenue shortfall	1987
Michigan	formula	25%	formula	pre-1985
Minnesota	year-end surplus	5%	revenue shortfall	pre-1985
Mississippi	year-end surplus	7.5%	revenue shortfall	1985
Missouri	appropriation	5%	revenue shortfall	1992
Montana				
Nebraska	year-end surplus	no limit	revenue shortfall	pre-1985
Nevada	formula	8%	revenue shortfall	1987
New Hampshire	year-end surplus	5%	revenue shortfall	1987
New Jersey	year-end surplus	5%	revenue shortfall	1988
New Mexico	appropriation	no limit	revenue shortfall	pre-1985
New York	year-end surplus	2%	revenue shortfall	pre-1985
North Carolina	year-end surplus	5%	appropriation	1990
North Dakota	year-end surplus	no limit	revenue shortfall	1989
Ohio	appropriation	4%	appropriation	1985
Oklahoma	year-end surplus	10%	revenue shortfall	1988
Oregon *				1994
Pennsylvania	appropriation	3%	revenue shortfall	1986
Rhode Island	appropriation	3%	revenue shortfall	1985
South Carolina	appropriation	5%	revenue shortfall	pre-1985
South Dakota	year-end surplus	5%	appropriation	pre-1985
Tennessee	appropriation	no limit	revenue shortfall	pre-1985
Texas	year-end surplus	10%	revenue shortfall	1990
Utah	year-end surplus	8%	revenue shortfall	1987
Vermont	year-end surplus	5%	revenue shortfall	1988
Virginia	formula	10%	appropriation	1985
Washington	formula	no limit	revenue shortfall	1989
West Virginia	year-end surplus	5%	revenue shortfall	1994
Wisconsin	appropriation	no limit	appropriation	1993
Wyoming	year-end surplus	no limit	appropriation	pre-1985

Source: Eckl (1997).

*The publication did not include Oregon in its list of states with rainy day funds.

Table 2. Summary statistics (monetary variables in 1997 dollars)

Description		States with RDF in 1984 (17 states)	States w/o RDF in all years (4 states)	States adopting RDF in sample (27 states)	Source
		(1)	(2)	(3)	
Total balance per capita	Per-capita general fund and RDF balances	102.38 (133.19)	31.28 (59.05)	69.39 (67.06)	NASBO Fiscal Survey
RDF indicator	Rainy day fund indicator	1.00 (0.00)		0.68 (0.47)	NASBO Fiscal Survey
Per-capita income	Total income per-capita (thousands)	21.89 (3.70)	19.43 (3.16)	21.26 (3.36)	Bureau of Economic Analysis
Unemployment rate		5.67 (1.61)	7.24 (1.78)	5.93 (1.81)	Bureau of Labor Statistics
Government expenditures	Total general fund expenditures	1310.17 (473.33)	1084.93 (255.17)	1217.90 (367.26)	NASBO Fiscal Survey
Both chambers Democrat		0.48 (0.50)	0.71 (0.46)	0.56 (0.50)	Book of the States
Both chambers Republican		0.25 (0.43)	0.07 (0.26)	0.21 (0.41)	Book of the States
Governor Democrat		0.55 (0.50)	0.57 (0.50)	0.56 (0.50)	Book of the States
RDF balance per capita	Rainy day fund balance per-capita	42.85 (51.68)		22.51 (26.38)	NASBO Fiscal Survey
Deposit by formula	Money deposited into RDF by formula	0.13 (0.34)		0.17 (0.37)	NCSL / Eckl
Deposit of year- end surplus	Year-end surplus deposited into RDF	0.58 (0.50)		0.31 (0.46)	NCSL / Eckl
Limit 5-9% indicator	RDF cap 5-9% of expenditures	0.29 (0.46)		0.30 (0.46)	NCSL / Eckl
Limit > 9% indicator	RDF cap greater than 9% of expenditures	0.18 (0.38)		0.08 (0.27)	NCSL / Eckl
No limit indicator	No cap on RDF balances	0.35 (0.48)		0.14 (0.35)	NCSL / Eckl
Withdrawal by formula	Withdrawal by formula only	0.06 (0.23)		0.04 (0.21)	NCSL / Eckl
Withdrawal of shortfall	Withdrawal allowed if revenue shortfall	0.60 (0.49)		0.42 (0.49)	NCSL / Eckl

Table 3. Rainy Day Funds' Effects on State Government Savings

Dependent variable is total balances per capita	OLS	OLS	OLS	Fixed Effects	Fixed Effects	Fixed Effects
	(1)	(2)	(3)	(4)	(5)	(6)
RDF indicator	15.664 (14.228)		-35.969 (21.838)	11.165 (9.692)		-21.284 (27.563)
Per-capita income (thousands)	-6.810* (2.827)	-5.117* (2.245)	-7.029* (3.008)	-4.750 (3.273)	-3.167 (3.259)	-2.853 (3.378)
Unemployment rate (percentage points)	-10.034* (4.861)	-10.809* (3.356)	-9.691† (4.828)	-12.507* (2.344)	-14.918* (2.281)	-12.038* (2.367)
Government expenditures	0.083† (0.042)	0.045 (0.030)	0.086* (0.040)	0.060* (0.024)	0.055* (0.023)	0.053* (0.025)
Both chambers Democrat	-35.332 (23.540)	-22.175 (17.362)	-36.964 (23.853)	-21.733* (10.658)	-12.236 (9.394)	-20.671† (10.928)
Both chambers Republican	13.200 (33.569)	1.253 (17.094)	1.998 (24.955)	8.479 (11.730)	11.801 (9.763)	7.665 (11.718)
Governor Democrat	6.636 (12.841)	4.549 (8.846)	5.992 (12.698)	4.373 (6.610)	2.861 (5.970)	6.448 (6.737)
RDF balance		1.370* (0.158)			1.137* (0.084)	
Deposit by formula			34.724† (17.325)			0.137 (23.020)
Deposit of year-end surplus			32.251 (28.089)			-17.933 (23.353)
Limit 5-9% indicator			44.970 (27.911)			42.134 (32.420)
Limit > 10% indicator			29.727 (21.086)			102.238 (39.489)*
No limit indicator			63.916* (30.851)			40.486 (28.920)
Withdrawal by formula			5.262 (27.529)			78.386† (45.807)
Withdrawal of shortfall			-15.199 (22.766)			-14.305 (18.866)
R-squared (excluding FE)	0.1423	0.4914	0.1931	0.0888	0.4133	0.1112
Years	1984-1997	1987-1997	1984-1997	1984-1997	1987-1997	1984-1997

Standard errors in parentheses - adjusted to reflect within-state correlation for OLS.

* significant at 5% level.

† significant at 10% level.

Table 4. Robustness Checks.

Dependent variable is total balances per capita	OLS	OLS	OLS	Fixed Effects	Fixed Effects	Fixed Effects
	(1)	(2)	(3)	(4)	(5)	(6)
RDF indicator	30.379 (25.248)		7.222 (59.692)	28.191 (22.492)		-35.635 (65.429)
Per-capita income (thousands)	-9.544 [†] (4.942)	-6.871* (2.606)	-4.784 (8.840)	38.993* (6.068)	-2.908 (3.508)	42.068* (6.194)
Unemployment rate (percentage points)	-7.375 (10.190)	-22.551* (5.886)	-5.267 (11.653)	-5.987 (5.264)	-22.727* (2.610)	-5.327 (5.329)
Government expenditures	0.172* (0.014)	0.076* (0.018)	0.154* (0.022)	-0.422* (0.022)	-0.007 (0.020)	-0.429* (0.022)
Governor Democrat	-13.417 (28.910)	19.525 (12.258)	-19.980 (30.824)	-18.705 (15.116)	3.674 (7.006)	-16.653 (15.466)
RDF balance		0.952* (0.012)			0.890* (0.019)	
Deposit by formula			20.787 (58.665)			-62.811 (54.089)
Deposit of year-end surplus			-12.940 (55.370)			-47.414 (54.968)
Limit 5-9% indicator			76.444 (47.834)			82.210 (76.841)
Limit > 10% indicator			29.579 (35.027)			236.214* (92.757)
No limit indicator			148.465 [†] (76.967)			67.743 (68.377)
Withdrawal by formula			-21.850 (59.987)			97.893 (108.430)
Withdrawal of shortfall			-88.561 (68.376)			6.335 (44.771)
R-squared (excluding FE)	0.181	0.932	0.219	0.397	0.917	0.406
years	1984-1997	1987-1997	1984-1997	1984-1997	1987-1997	1984-1997

Standard errors in parentheses - adjusted to reflect within-state correlation for OLS.

[†] significant at 10% level.

* significant at 5% level.