Essays on State and Local Fiscal Institutions

by Travis St.Clair

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Ed.M., June 2007, Harvard Graduate School of Education

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Joseph Cordes
Professor of Economics, of Public Policy and Public Administration,
and of International Affairs

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Essays on State and Local Fiscal Institutions

Travis St.Clair

Dissertation Research Committee:

Joseph Cordes, Professor of Economics, of Public Policy and Public Administration, and of International Affairs, Dissertation Director

Harold Wolman, Professor of Political Science, of Public Policy and Public Administration, and of International Affairs, Committee Member

Angela Gore, Associate Professor of Accountancy, Committee Member

Abstract of Dissertation

Essays on State and Local Fiscal Institutions

As of December 2008, 30 states faced a limit to the amount of money they could spend or to the level of revenue they could raise. All but one currently has in place some form of balanced budget requirement, and almost every state has imposed some sort of restriction on the ability of its local governments to raise property tax revenue (National Conference of State Legislatures, 2008; Mullins & Wallin, 2004). These limits exist despite the fact that traditional norms of public finance, with their view of a benevolent government that maximizes the public interest, see no need for them.

Much of the research on fiscal restrictions focuses on whether such mechanisms achieve their stated objectives: reducing spending, reducing debt, etc. A consensus seems to be emerging that institutions affecting a government's ability to manage its finances can be effective so long as they are well designed; poorly designed spending caps or budget stabilization funds will have no effect (Bohn & Inman, 1996; Kiewiet & Szakaly, 1996; Mullins & Joyce, 1996; Wagner, 2003). However, little attention has been paid to the unintended or ancillary consequences of these rules. This motivates the central question to be examined in this dissertation: what are the unforeseen consequences of fiscal institutions, and what sort of bearing do they have on the overall fiscal health of the state?

In order to address these questions, I focus on three of the major budgetary institutions in place at the state and local level in the United States: tax and expenditure limitations (TELs), budget stabilization funds, and debt restrictions. Each of these

institutions is the product of a complicated political process at the state and/or local level, and the presence of each has significant implications for public financial management. The essays that follow draw upon a variety of datasets, including data on local government finances in Colorado, a national panel of state pension contributions, and data on municipal debt issuance in California.

The findings are summarized below:

- An analysis of Colorado's Taxpayer's Bill of Rights suggests that TELs increase revenue and expenditure volatility, regardless of whether districts vote to override the restrictions imposed on them.

- The effective management of budget stabilization funds can assist states in meeting their pension obligations, yet strict withdrawal rules that make it more difficult for state legislators to access these funds may result in lower annual contributions.

- After the passage of Proposition 39 in 2000, which lowered the procedural hurdles required of school districts to issue general obligation bonds, school districts in California issued no more debt relative to other governments than they had previously and were no more responsive to changing credit conditions.

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Chapter 1 Introduction

1.1 Fiscal Institutions

"We are convinced that man can organize his political society better by putting checkreins on his behavior in advance."

-- Buchanan & Tullock, "The Calculus of Consent," (1967), p. 305

Ever since the 1840s, when the first debt restrictions were inserted into state constitutions, state and local governments have had to abide by certain fiscal "rules" when making taxing and spending decisions. Gradually new and different kinds of fiscal institutions took shape, including debt limitations, balanced budget requirements, rainy day funds, and tax and expenditure limits, to the point that nowadays such provisions are commonplace at both the state and local level (Wallis & Rodriguez Tejedo, 2010). All states but one face a balanced budget requirement. As of December 2008, 30 states faced a limit to the amount of money they could spend or to the level of revenue they could raise. In addition, almost every state has imposed some sort of restriction on the ability of its local governments to raise property tax revenue (National Conference of State Legislatures, 2008; Mullins & Wallin, 2004).

These limits exist despite the fact that traditional norms of public finance, with their view of a benevolent government that maximizes the public interest, see no need for them. For instance, Downs (1957) argues that public officials act to maximize the preferences of the median voter. Tiebout (1956) posits that individuals vote with their feet and will choose local communities that best meet their preferences for a particular set of taxes and services. Neither of these views is consistent with tax and expenditure

limitations (TELs) or other fiscal institutions, such as debt limits, that constrain the behavior of public officials.

In "The Calculus of Consent" and other works, public choice theorists such as Buchanan and Tullock proposed a new way of thinking about fiscal and political institutions. Given monopoly rights to tax and spend as well as informational advantages, government officials have an incentive to maximize public budgets and in doing so serve their own ends rather than the public interest. Under such conditions, voters may decide to establish institutions that will enable them to draw benefits from government while also placing restrictions on officials who would use their monopoly powers toward selfish ends. Indeed, the Leviathan model, proposed by Brennan and Buchanan, interprets TELs as citizens' attempts to exercise control over a wasteful and inefficient government (Brennan & Buchanan, 1979).

In models of government behavior, the Leviathan model and the model of a benevolent government that seeks to maximize the public interest can be seen as two ends of a spectrum (Brunori, Bell, Cordes, and Yuan, 2008). Some, such as Besley and Smart (2007), have devised game-theoretic models that incorporate aspects of both. However, other theories attempting to explain the existence of TELs have been posited as well, most notably fiscal illusion. For a variety of reasons, voters and taxpayers may not necessarily draw a connection between the taxes they pay and the government services they receive. While taxes are quite evident to those who pay them, some government expenditures produce services, such as planning and administration, that are hardly visible in the short-term, and others, such as road maintenance, that may only be noted in their absence. This would suggest that taxpayers support restrictions such as TELs

because they do not believe that the limits will have an impact on the services they receive.

As a consequence of this literature, and in particular the Leviathan model, much of the early work on fiscal institutions tended to focus on whether or not institutions were in fact successful at reining in legislators' self-interested behavior. That is, do institutions actually succeed in doing what it is they are intended to do: reducing debt, reducing spending, etc. One line of this research has involved examining the effect that institutions have on the distribution of responsibilities between state and local governments (Brunori, Bell, Cordes, & Yuan, 2008). A consensus seems to be emerging that institutions can be effective so long as they are well designed; poorly designed spending caps or budget stabilization funds will have no effect (Bohn & Inman, 1996; Kiewiet & Szakaly, 1996; Mullins & Joyce, 1996; Wagner, 2003).

However, a deeper understanding of budget rules requires taking a more comprehensive look at the budget process and examining some of the unanticipated impacts of fiscal institutions, effects that were unlikely to be considered at the time such institutions were adopted. After all, debt restrictions are not likely to affect debt levels alone; they most likely affect spending as well. Similarly, pension funding requirements will have consequences for rainy day funds and debt management. Insofar as the components of state and local budgets interact with each other, it is not enough to confine the analysis of fiscal institutions to the one part of the budget to which they most directly apply.

This motivates the central question to be examined in this dissertation: what are the unforeseen consequences of fiscal institutions, and what sort of bearing do they have

on the overall fiscal health of the state? This dissertation attempts to expand the existing literature on fiscal institutions by broadening the scope of the questions being asked. Rather than examining whether a limitation on one area effectively constrains government involvement in that area, it considers the budget in its entirety and in doing so develops insight into the consequences of limiting legislator discretion. Without an understanding of the effects that fiscal institutions have on the budgetary process as a whole, our understanding of fiscal institutions is bound to be incomplete.

The rest of this introduction provides short summaries of each of these essays, laying out the main research questions, methodological approaches, results, and key implications.

1.2 The Effect of Tax and Expenditure Limitations on Revenue Volatility

Much of the research on tax and expenditure limitations (TELs) focuses on the impact that limits have on the size of the public sector or the distribution of expenditures at the state and local levels. While these results shed light on the extent to which TELs succeed in reducing government spending, they don't have much to say about the impact of TELs on government budgeting or financial planning, despite the fact that voters support TELs in the hope of reducing government inefficiency (Courant, Gramlich, and Rubinfeld 1980; Ladd and Wilson 1982).

Instead of focusing on the level of government spending, this essay examines the effect of TELs on the stability of government revenues; sound tax policy entails controlling the volatility of revenues in order to plan more effectively for the future. A stable revenue stream facilitates planning and cost-savings. In contrast, volatile revenue

growth leads to unstable patterns of spending and infrastructure investment. While in theory tax and expenditure limitations should reduce revenue volatility by reducing the possible variance of revenue totals, TELs have historically led states to reduce their reliance on property taxes – a stable form of revenue – in favor of more income-elastic sources such as user fees and charges (McCubbins & Moule, 2010).

This essay tests the hypothesis that TELs impact revenue volatility by examining revenue data from Colorado before and after the state passed its Taxpayer's Bill of Rights (TABOR) in 1992, one of the nation's most restrictive TELs. It compares local government revenue data from Colorado with observations from several "control" states that were not subject to any TELs during this time period. An additional set of regressions examines expenditure data to determine whether the impact on revenue volatility translates into an impact on service provision. Furthermore, the analysis takes account of the fact that a large number of districts voted to override the law's restrictions and were therefore subject to the TABOR-limits for a brief period of time.

This study uses two different sources of data: the Colorado Division of Local Government and the Census Bureau's Annual Survey of State and Local Government Finances and Census of Governments. This essay measures volatility as the standard deviation in the annual percent change of real per-capita revenues.

The analysis takes the form of difference-in-difference estimation. Though obvious differences exist between the economies and populations of Colorado and the various states chosen for comparison, the advantage of difference-in-difference estimation is that it controls for unobserved heterogeneity between the states that is constant over time. That is, by focusing on the change in volatility that occurred, the

model sweeps away differences between the states that might result in different levels of volatility, such as different industrial structures or revenue structures.

Results show that the tax limitation enacted in Colorado in 1992, commonly known as TABOR, increased both revenue and expenditure volatility relative to local governments in other states over the same time period. This finding held even for local governments that successfully voted to override the law's requirements. The key implication of this finding is that, regardless of voters' preference for more limited government, tax and expenditure limitations appear to increase budget uncertainty and impede forecasting.

1.3 The Impact of Budget Stabilization Funds on State Pension Contributions

As states have struggled to balance their budgets amidst the current economic crisis, numerous reports have expressed alarm at the projected shortfalls in public employee pension funds. The Pew Center on the States was one of the first organizations to provide a comprehensive portrait of state retirement systems nationwide (Pew Center on the States, 2010). Pew collected data on state-administered plans directly from the states' Comprehensive Annual Financial Reports (CAFRs), pension plan annual reports, and actuarial valuations. The report found that, as of fiscal year 2008, states' pension plans had roughly $2.3 trillion in assets and $2.8 trillion in liabilities, reflecting a gap of approximately half a trillion dollars.

While work such as Pew's has compared the funded status of different plans and focused on the role of discount rate assumptions in the measurement of liabilities, there is

little known about the effect of fiscal institutions on pension funding, despite the direct role that many institutions play in public financial management. Budget stabilization funds, otherwise known as "rainy day" funds, facilitate savings during years of flush revenues so that states will have resources to draw upon during economic downturns. Just as many states reduce their pension contributions during periods of fiscal stress, so too do they rely on budget stabilization funds as a countercyclical tool, drawing on their reserves to alleviate budget deficits when necessary.

This essay attempts to explain variation in the percent of the actuarially required contribution (ARC) that states have contributed to their pension funds over the period 1997-2008. The actuarially required contribution is the amount that actuaries have determined a state must contribute on an annual basis if it is to cover the benefits earned by current employees as well as unfunded liabilities that have accrued, liabilities that are typically amortized over a 30-year period. States are rarely *required* to pay the full amount of the ARC. A few states do face legal stipulations regarding their payment of the ARC, but even states facing constitutional provisions may amend their constitutions with a supermajority vote. Nonetheless, the ARC is a good benchmark to compare contributions against because it reflects the amount that a supposedly unbiased analyst believes that the state should contribute based on a number of factors, including the demographic make-up of the workforce.

There are a number of methodological challenges involved with trying to estimate the causal effect of fiscal institutions. Randomized experiments are of course rare in the public finance literature because of the obvious difficulty involved with manipulating taxing and spending decisions. As a result, researchers are typically forced to rely on

observational data. However, the use of panel data comes with the possibility of autocorrelation and improper standard error estimation. What's more, fiscal institutions tend to be relatively constant over time, precluding the use of fixed effects. To address these challenges, the prior year's ARC is included as a covariate to account for possible autocorrelation, and robust, clustered standard errors are also used to correct for heteroskedasticity and autocorrelation.

Fiscal institutions may also be endogenous. This is particularly true in the case of budget stabilization funds, as an omitted variable – a state's propensity for saving – may be influencing both the likelihood that a state has adopted a BSF as well as the amount that a state contributed to their pension funds. This potential endogeneity is a further reason why public finance researchers are often careful about inferring causality and in fact often operate from the assumption that OLS results suffer from bias. To address the omitted variable bias, this essay employs the Blundell and Bond (1998) system GMM estimator. The Blundell-Bond estimator is well-suited for the econometric problems at issue in this essay, as it was designed for 1) "small T, large N" panel estimation involving 2) a lagged dependent variable, 3) endogenous regressors, 4) unobserved time-invariant characteristics, and 5) heteroskedasticity and autocorrelation within individuals, all problems that are relevant to the case at hand.

Empirical results suggest that budget stabilization funds with strict deposit rules are associated with higher pension contributions, while strict withdrawal rules are associated with lower contributions. The conclusion regarding withdrawal rules is particularly significant as previous work on budget stabilization funds has tended to emphasize the benefits of strict withdrawal rules. Knight and Levinson (1999) show that

states with stricter withdrawal provisions tend to save more than states that allow access to balances through appropriation. Wagner (2004) finds that states with strict deposit and withdrawal rules see the greatest reduction in borrowing costs. Rose (2008) finds that requiring gubernatorial approval for withdrawal from budget stabilization funds may prevent political manipulation of the balances. The emphasis on building up BSF balances and ensuring that they are safe from political misuse may have distracted researchers from the fact that strict withdrawal rules can prevent lawmakers from accessing these same funds when the withdrawal is justified, as may be the case with pension contributions, which can be invested in the stock market and earn a return.

1.4 Procedural Debt Restrictions and California's Proposition 39

While many restrictions on taxes and expenditures were established during the Tax Revolt of the 1970s, limitations on bonded indebtedness date back to the previous century. As of 1996, there were only five states without constitutional debt limitations (Kiewiet & Szakaly, 1996). Moreover, there is considerable variety in the type and stringency of limitations that exist, with some pertaining only to full faith and credit debt and others applicable to both guaranteed and non-guaranteed debt.

Research on debt limits often focuses on the quantity of debt that states and localities issue and examines whether or not limits are able to effectively constrain borrowing. In recent years, the existence of debt limits has given rise to various alternative debt instruments, such as lease financing, that have been used to circumvent borrowing restrictions and have been cited by researchers as the main reason why debt limits must be well-designed if they are to be effective.

However, states and localities are not only constrained in terms of the total amount of debt they can issue. Procedural hurdles exist that affect the process of debt issuance. Tejedo (2007) distinguishes between debt limits and debt restrictions, referring to debt limits as restrictions on the absolute amount of debt allowed or on the amount determined by a relative measure (eg. percent of assessed value). Procedural debt restrictions are restrictions on the process that is used to authorize the debt and can come in the form of a requirement for voter approval or a legislative majority. In some cases, debt limits are coupled with procedural restrictions.

Few studies, if any, have examined the effect that procedural debt restrictions have on the amount of new debt that local governments issue. This study exploits two developments in the municipal bond market to gain further insight into procedural debt restrictions. In 2000, California passed Proposition 39, which modified the normal referendum requirement of two-thirds voter approval for bond issues; so long as they meet certain requirements, school districts require only 55 percent of the local vote in order to issue a bond. Later in the decade, in 2009, Build America Bonds were introduced as part of the stimulus program, enabling state and local borrowers to issue debt at lower yields (U.S. Treasury Department, 2011).

This essay examines a panel of municipal debt issuance in California to investigate whether the lower supermajority requirement that school districts faced as a result of Proposition 39 caused them to 1) issue more new debt, and 2) respond more aggressively to the Build America Bond program. The data come from the California Debt and Investment Advisory Commission (CDIAC) and consist of all public debt issued in the state since 1984. In order to account for unobserved factors that impact

school debt levels but that are unrelated to procedural restrictions, this essay employs difference-in-difference estimation to compare the *change* in debt issuance for school districts and other local governments before and after both the passage of Proposition 39 as well as before and after the inception of the Build America Bonds program.

Results from probit and tobit estimations indicate that school districts were far more likely to issue general obligation debt throughout the time periods in question. However, there is no evidence that school districts saw any more of an increase in borrowing than other governments did in the years following 2000 and then again in 2009.

These findings suggest that debt restrictions have little to no effect on the level of debt or the elasticity of local government debt issuance with respect to credit conditions. However, as this essay exploits a relatively minor change in the debt process – lowering the supermajority vote required to issue debt from 2/3 to 55 percent – it is possible that more significant changes would have a more sizeable impact.

Chapter 2 The Effect of Tax and Expenditure Limitations on Revenue Volatility: Evidence from Colorado

2.1 Introduction

Following California's passage of Proposition 13 in 1978, a large number of states enacted tax and expenditure limitations (TELs) at both the state and the local level. A wide variety of analyses in the years since have focused on the impact TELs have had on the size of the public sector and the distribution of expenditures at the state and local levels. The conclusions from this body of work suggest that officials are often able to circumvent the limits proscribed by TELs; only when limits are carefully designed are they effective at reducing the size of government (Mullins & Joyce, 1996; Primo, 2007). While these results have shed light on the extent to which TELs succeed in reducing government spending, they have proved rather narrow in the scope of their analysis. Many voters who expressed support for TELs did so out of a desire for government to be more efficient (Courant et al., 1980; Ladd & Wilson, 1982), yet few researchers have attempted to identify the impact TELs have had on financial planning or budgeting – two activities central to the efficient provision of services.

Instead of the absolute level of government spending, this essay focuses instead on the stability of government revenues. Highly volatile revenues make it difficult for any organization to run effectively, while a stable revenue stream facilitates careful planning and cost-savings. Stability refers to a state's ability to maintain a consistent and predictable level of revenues over time. Since many public services are provided over an indefinite time frame, a consistent source of revenues is required to maintain the existing

level of service. Moreover, infrastructure investments require governments to be able to project future revenue streams with a degree of certainty. As Thompson and Gates put it, "volatile, unpredictable revenue growth causes all sorts of unpleasant responses on the part of governments, most commonly manic-depressive patterns of spending and taxing" (Thompson & Gates, 2007, p. 825).There is even some evidence that states can realize significant budgetary savings by reducing fiscal volatility (Crain, 2003, p. 108).

In practice, states attempt to increase stability by raising revenue through a mix of sources, some of which are more elastic in their response to economic conditions than others. Sales taxes on necessities tend to be insulated from economic swings, while income taxes fluctuate, producing more revenue in times of growth. On the one hand, it may be reasonable to assume that a limitation on the amount of revenue that a government can raise will necessarily decrease volatility by imposing a fixed ceiling on revenue totals, thereby reducing the possible variance. On the other hand, fiscal limits often target property taxes – one of the most stable forms of revenue – either alone or in conjunction with other revenue sources. If local governments are constrained in their ability to raise revenue through property taxes, they may turn to taxes and/or fees that are more income-elastic, thereby increasing volatility (McCubbins & Moule, 2010). Hence, the effect that TELs may have on revenue volatility is not immediately obvious.

This essay tests the hypothesis that TELs impact revenue volatility by examining revenue data from Colorado before and after the state passed its Taxpayer's Bill of Rights (TABOR) in 1992, one of the nation's most restrictive TELs (Bradley & Lav, 2005; McGuire & Rueben, 2006). It compares local government revenue data from Colorado with observations from "control" states that were not subject to any TELs during the time

period. The focus of the analysis is whether or not TELs improve revenue stability and thus the budgeting capacities of local governments. An additional set of regressions examines expenditure data to determine whether the impact on revenue volatility translates into an impact on service provision.

This essay proceeds as follows. Section two will review prior research on TELs. Section three will discuss measures of volatility and highlight the importance of revenue stability to financial performance. Section four discusses Colorado's Taxpayer's Bill of Rights, a constitutional amendment that the state passed in 1992. Results from a difference-in-difference analysis of local government revenue and expenditure data are presented in part five. Section six concludes.

2.2 Research on Tax and Expenditure Limitations

Much of the early work on TELs was focused on the question of whether or not TELs succeed in reducing spending. The early consensus was that TELs do not in fact significantly impact state fiscal policy outcomes. Bails (1982) found that TELs in their current form were not effective at restraining growth in the public sector. Examining a variety of constitutional provisions, Abrams and Dougan (1986) found that most constraints fail to influence the budgetary process. Kenyon and Benker (1984) discussed TELs in the context of a movement toward greater discipline and also found that TELs are ineffective at constraining growth. In 1990, Bails re-evaluated his earlier results and reached the same conclusion: TELs have virtually no effect on the growth of state revenues or expenditures (Bails, 1990).

While most of the early studies concluded that TELs have little to no effect on state fiscal policy, more recent research began to find evidence of a modest impact. King-Meadows and Lowery (1996) examined state and local revenues as a percentage of state personal income to determine whether tax-revolt era state fiscal caps effectively reduced the size of government. Their analysis built on earlier research by Cox and Lowery (1990) that studied three pairs of matched states and found no evidence that TELs affect the total level of state and local expenditures. By extending the time series data of Cox and Lowery's earlier work to include the recession years of 1987-1991, the authors did ultimately find weak evidence that fiscal caps modestly reduce the size of government.

However, their work underscores some of the difficulties involved in studying TELs, which have been enacted in various forms at different points in time. Research has often focused on particular states (Reid, 1988; Susskind, 1983) or has not looked over a broad enough period of time. Results have demonstrated the importance of including recession years so as to study the impact of TELs at a time when state and local finances face the greatest strain. More recent research by McCubbins and Moule (2010) found greater revenue declines during recessions in states with property tax limitations.

While some TELs are enacted only at the state or the local level, limitations in one area can be alleviated by increased spending in the other, something that studies focused only on state-level revenue or spending fail to capture. Shadbegian (1996) looked at state expenditures per capita over the years 1972-1987 and found that while TELs restricted the growth and size of government in states with below average income, in general they had no effect. His finding, however, failed to account for the fact that states

may have increased the size of their local governments in response to restrictions on state budgets.

New (2001) looked at the change in per capita state and local expenditures for 49 states from 1972 to 1996. Unlike earlier work, he identified different categories of TELs and examined how each type of TEL impacts budgets differently, finding that TELs passed by citizen initiative were more likely to reduce total expenditures than TELs passed by state legislatures. He also found that TELs which include immediate taxpayer refunds or surpluses and those which hold spending increases to the inflation rate plus population growth are more effective. On the other hand, Kousser, McCubbins, and Moule (2008) showed that legislators can circumvent citizen initiatives by raising fees.

More recent work has confirmed that the design of limitations matters and that different institutional variables interact with one another to determine their effect on spending. Primo (2006) used 32 years of state budget data to demonstrate that spending limits can be effectively designed to reduce spending, though his focus was on balanced budget requirements as caps on spending rather than TELs. One of the few to investigate the effect of TELs on revenues and to include a measure of volatility in his models, Crain (2003) sampled 47 states for the years 1970-1998 and controlled for the effect of other fiscal institutions such as balanced budget requirements and biennial budget cycles. Though his focus was only on state revenues, he found that TELs did reduce total state per-capita revenue as well as per-capita tax revenue.

Other researchers have branched out to investigate the impact of TELs on the centralization of revenues and the mix of revenue sources. Mullins and Joyce (1996) looked at the shifts in reliance among revenue sources and the changes in the state share

of total state and local revenue and expenditures. Consistent with other findings, they concluded that state limits must be coupled with local limits in order to see any sort of reduction in the size of government. State TELs do not decrease the relative importance of state taxes, but local TELs do lead to less use of broad-based taxes at the local level. TELs also lead to increases in user charges since public officials are forced to find alternative sources of revenue. More importantly, TELs lead to increased centralization; states collect a larger share of total state and local revenues, and as a result their responsibility for expenditure decision-making expands. The authors concluded that TELs have vested more power at the state level and reduced the ability of local governments to respond to the needs of their citizens.

Other work has confirmed that property tax limits, the main form of limit imposed on local governments, result in changes to local government fiscal structure, increasing government's reliance on sales taxes, user charges, and off-budget activities. In an early piece of work on the subject, Bennett and DiLorenzo (1982) contended that property tax limits lead to increases in off-budget spending, as well as the creation of special districts that can be used to avoid the limits, a contention that was supported with more recent work by Bowler and Donovan (2004). Lewis (2001) pointed to California in the wake of Proposition 13 as evidence that sales tax revenue often replaces property tax revenue. McCubbins and Moule (2010) showed evidence that property tax limits are associated with an increased reliance on user charges and fees.

2.3 Measures of Volatility

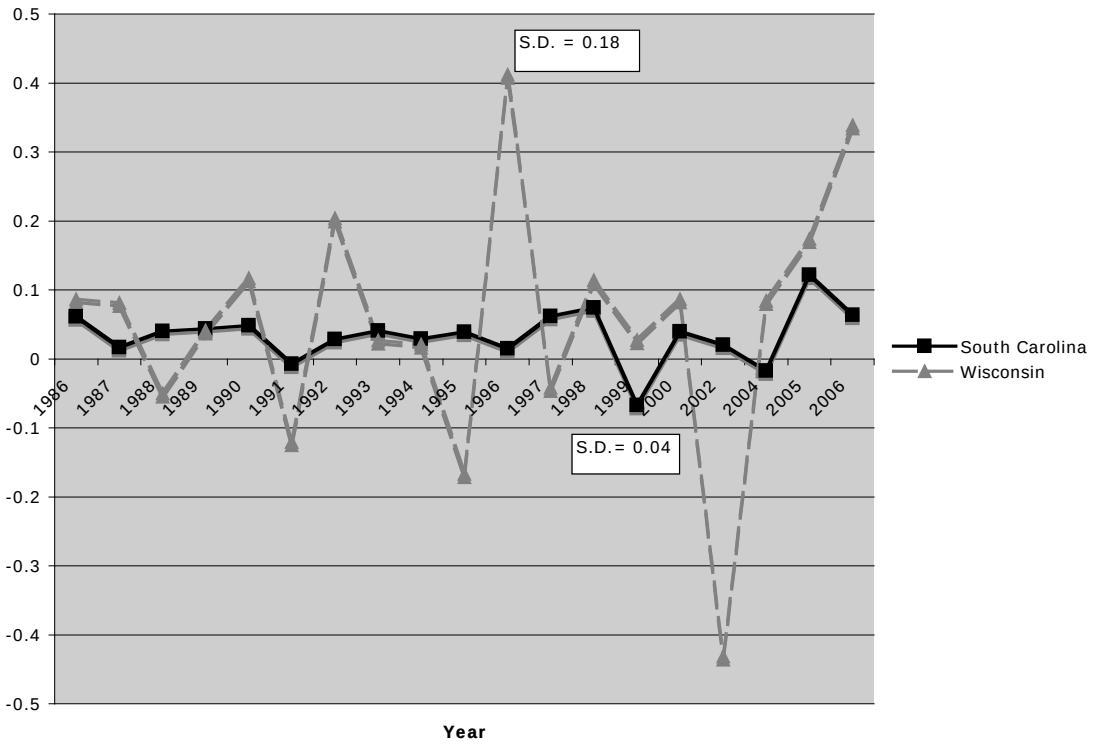
Volatile revenues can be a source of trouble for governments for various reasons. This is especially true in the case of state and local governments that face a strict balanced budget constraint. Unexpected revenue growth can lead to spending increases or tax cuts that are unsustainable once a recession arrives, leading governments to make painful cuts that only exacerbate the business cycle (Levinson, 1998). While rainy day funds can be of some assistance, lawmakers must manage the funds properly and allow them to build up balances before they can be of sufficient size to close budget holes. Indeed, one of the major pieces of advice dispensed by financial management experts, in addition to the careful management of rainy day funds, is that state and local governments diversify their revenues sources in order to reduce volatility (Thompson & Gates, 2007, p. 826). What's more, researchers increasingly look to volatility as a key measure of economic performance, as modern financial theory suggests that absolute revenue levels, without risk being taken into account, only tell part of the performance story (Crain, 2003, p. 31).

Figure 2.1 displays the year-over-year annual percent change in revenues for two states – South Carolina and Wisconsin – over the period 1986 to 2006. These two states illustrate the distinction between a state with a relatively stable revenue stream and one that is more volatile. As the figure illustrates, per-capita revenues in South Carolina generally increased only slightly from year to year, with few large peaks and valleys. On the other hand, Wisconsin saw significant variation in the changes from year-to-year, which is likely to have made budgeting a more formidable process for state lawmakers.

A common measure of volatility is a standard deviation, which captures average variation around the average. This essay measures volatility as the standard deviation of

the annual percent change of real per-capita revenues (in 2005 dollars).[1] As Figure 2.1 illustrates, a revenue stream with greater variability has a larger standard deviation. The annual percent changes in South Carolina's revenues have a standard deviation of 4.0 percentage points. That is, approximately 68 percent (or roughly two-thirds) of the state's revenue shifts fall into a band with a width of +/- 4 percentage points (or a total of 8 percentage points). On the other hand, the standard deviation of the annual percent change in Wisconsin's per-capita revenues is equal to 18 percentage points; two-thirds of the revenue shifts between 1986 and 2006 fall within a band that is of a width equal to +/- 18 percentage points. Measuring revenue in real, per-capita terms removes the effect of population growth and inflation in order to focus on the underlying trend.

Figure 2.1 Annual Percent Change in Real Per-Capita Revenue



[1] See Boyd (2008).

Although other measures of volatility have been adopted, such as using variations in the residuals from a time-series forecasting equation in order to control for factors that influence changes in macroeconomic conditions (Ramey & Ramey, 1995), this essay uses the more straightforward approach throughout in order to more easily draw comparisons between different data sources and between volatility at the state and local level. Another alternative would be to use the coefficient of variation, the ratio of the standard deviation to the mean. However, the coefficient of variation is sensitive to small changes in the mean when the mean is close to zero. As the year-to-year change in per-capita revenues is sometimes close to zero, the standard deviation is preferred.

Revenue volatility will naturally be higher in some states than in others, due in part to industrial structure, demographics, and the mixture of revenue sources. This makes assessing the impact of a particular legislative change – in his case, the enactment of a tax and expenditure limitation – on revenue volatility particularly challenging. National economic conditions may influence revenue volatility at the state level and local level. There are potential endogeneity concerns; it is possible that states enact TELs because they previously experienced high levels of volatility. Furthermore, as many states have enacted more than one form of TEL, it can be difficult to isolate the effect of one specific set of limitations, a difficulty that plagues all studies of TELs.[2] For these reasons, the focus in this essay will be on the *change* in volatility following a TEL.

Figure 2.1 displays revenues at the state level. Given that the focus in this essay is the impact of revenue volatility on government services and planning capabilities, isolating the effect of a particular fiscal limitation requires measuring its effect on

[2] Even California, the supposed progenitor of the tax revolt with Proposition 13, had previously passed a general revenue limit in 1972 that applied only to school districts.

individual government entities. Since various states have enacted TELs at different points in time, it is easier to measure this effect by examining revenue volatility at the local level, particularly in a state where all local governments were subject to the same state-wide limitation.

Although a number of states passed both general revenue as well as property tax limitations in the years following Proposition 13, Colorado was the only state to pass a general revenue limitation that affected all levels of government in the 90s (Mullins & Wallin, 2004). What's more, the state did not repeal the law soon thereafter, as some others did. It also passed one of the most restrictive TELs in the nation, making the finances of local governments in Colorado a particularly apt subject for study. The next section focuses on Colorado's Taxpayer's Bill of Rights (TABOR) and examines local government data in Colorado before and after the legislation was enacted.

2.4 The Case of Colorado

Passed in 1992, Colorado's Taxpayer's Bill of Rights (TABOR) was a constitutional amendment applying to all taxing districts in the state that limited the growth rate of tax revenues. It has been described as the most restrictive TEL in the nation, in part because it restricted general revenues to the prior year's revenues, allowing adjustments only for population growth and inflation, but not personal income growth (McGuire & Rueben, 2006). In addition to overall spending and revenue limits, TABOR also specifically limited the annual growth of property tax revenue and prevented local governments from raising property tax rates without voter approval. TABOR required voter approval for any policy change that would result in a tax increase, including

renewal of expiring taxes, and because the law was passed as a constitutional amendment, it could only be amended by popular vote. Excess revenues were to be refunded to taxpayers through any reasonable method, including temporary tax credits or rate reductions.[3] The law explicitly prohibited the implementation of certain types of taxes and placed strict restrictions on the ability of the state legislature to pass emergency overrides (McGuire & Rueben, 2006).[4] Since its passage, numerous reports have cited TABOR as an example of a TEL that has been especially effective at curbing the growth of government (New, 2010; The Bell Policy Center, 2003).

The strictness of TABOR's provisions makes Colorado an attractive state to study. Unlike TELs in other states, there were few opportunities for legislatures to avoid the law's restrictions, and because the law applied to all taxing jurisdictions in the state, special purpose governments were not shielded from its effects. Table 2.1 lists the revenue volatility for 319 local governments in Colorado before and after TABOR, as well as the percent of tax revenues that were collected through property taxes. Volatility was calculated for the 7 years before and after the law was enacted: 1986-1992 and 1994-2000.[5,6] This choice of time frame was selected as a compromise between shorter time frames that may have failed to see the limit fully implemented and longer time frames

[3] Districts have used various mechanisms for returning refunds to taxpayers. Because rebates can take the form of temporary rate reductions, they are sometimes reflected in the revenue numbers of the following fiscal year. Alternatively, the refund may appear as an expenditure. For instance, Colorado's 1999 CAFR shows a $563.2 million expenditure related to the refund for fiscal year 1997-1998.

[4] In 2005, the state voted to suspend TABOR.

[5] As TABOR took effect on December 31, 1992 (halfway through fiscal year 1993), the first full year of implementation did not take place until fiscal year 1994.

[6] Since each observation is a year-over-year change, the periods 1985-1992 and 1994-2000 represent six observations each.

that would be subject to a wider range of confounding factors.[7] The data come from the Colorado Division of Local Government and consists of local government finances for 336 counties and municipalities from the period 1975-2004.[8] Seventeen governments – all municipalities – that did not have a full set of observations for the years 1986-2000 were excluded.

Table 2.1 shows that of the 319 local governments for which data were collected, 166 (52 percent) saw an increase in volatility following TABOR, while 153 saw a decrease. On average, local governments in Colorado saw their revenue volatility – as measured by the standard deviation of the annual percent change in real per-capita revenues – increase by 2 percentage points.

Table 2.1 also breaks down local governments into two categories: those that overrode the limitations imposed on them by TABOR, and those that did not. In the years following TABOR, numerous districts held local elections in which citizens voted to override the limits imposed on them by TABOR, thereby allowing their districts to retain revenues that were in excess of the TABOR limits. Such elections, commonly referred to as "de-Brucing" elections after Douglas Bruce, the original sponsor of TABOR, have been widespread in Colorado in the years since TABOR passed. According to the Colorado Legislative Council, as of 2010, voters in 76 percent of municipalities, 81 percent of counties, and 98 percent of school districts had voted to

[7] The difference-in-difference models presented in section five were re-estimated for time periods of different length, and the signs of all coefficients proved to be robust to the choice of different time frames.

[8] A few key distinctions exist between the data presented in Table 2 and the census data on local government finances (presented in the next section). The data set prepared by the Colorado Division of Local Government consists only of government funds, however it includes transfers between enterprise funds and government funds. For this reason, the focus is on total revenues. It also excludes funds that reflect the activity of public authorities and component units. In contrast, the census data utilized in the next section includes school districts and special districts.

override the limits imposed on them by TABOR, at least temporarily (Legislative Council of the Colorado General Assembly, 2010).

De-Brucing elections have varied greatly in their scope. While some districts have elected to de-Bruce all revenues from all sources, others have elected only to de-Bruce a particular revenue stream, such as property tax revenues. Similarly, voters in some districts have approved indefinite overrides, while others have approved overrides lasting a certain number of years. Such differences make it challenging to perform a "clean" analysis of de-Brucing elections, as does the lack of a comprehensive data source with information on special purpose governments. Data on de-Brucing elections come from the Colorado Municipal League and Colorado Counties Inc. respectively. A local government is coded as having de-Bruced if it passed a broad spending limit waiver between 1993 and 1999 exempting all revenues from TABOR indefinitely.

Table 2.1 Revenue Volatility of Local Governments in Colorado

	All Districts	Districts that De-Bruced	Districts that did not De-Bruce	*Difference* *(Standard Errors in Parentheses)*
Average Volatility Change	.021	.037	.004	*.033 (0.064)*
% of Districts with Increased Volatility	52.0	56.9	46.7	*10.2** (5.6)*
% Total Revenue from Property Taxes 1986	20.2	21.0	19.3	*1.7 (1.4)*
% Total Revenue from Property Taxes 2000	15.3	15.5	15.1	*0.4 (1.2)*
Number of Observations	319	165	154	*11*

** $p < .05$ in a one-sided test

Table 2.1 highlights an interesting pattern within the overall volatility data. More governments saw an increase in volatility than saw a decrease (166 to 153), a trend that was particularly significant in the case of local governments that successfully de-Bruced. In fact, the percentage of de-Bruced governments that saw an increase in volatility was statistically significant from 50% at the 5 percent level in a one-sided t-test. Moreover, the difference between governments that de-Bruced and those that did not was also statistically significant at the 5 percent level in terms of the percentage that saw an increase in volatility. The average volatility change was significantly larger for governments that de-Bruced (an increase of 3.7 percentage points) than for governments that did not (essentially zero), although the difference was not statistically significant. Table 2.1 also shows that, as Brown (2000) has demonstrated, local governments in Colorado decreased their reliance on property taxes following TABOR, from an average of 20.2 percent of total revenues in 1986 to an average of 15.3 percent in 2000.

The fact that revenues became more volatile in districts that de-Bruced during this period is unsurprising, as these districts would have been subject to the limits for a short period of time, after which point they would have been permitted to grow again at an unconstrained rate. In addition, property taxes declined significantly among all governments as a percent to the total, leaving districts to rely on a more volatile mix of revenue sources. While governments that did not de-Bruce were subject to a revenue cap for the entire period, they too saw their property taxes decline; it is plausible that these two offsetting effects may have cancelled each other out.

The data presented in Table 2.1 are significant because, unlike the census data presented in the next section, Table 2.1 includes data on the smallest municipalities, and

as a result represents a more comprehensive sample of general-purpose governments in Colorado during the periods before and after TABOR was enacted (though it does exclude certain types of districts; see footnote 8).

2.5 Difference in Difference Analysis

The results from Table 2.1 suggest that, while tax and expenditure limitations may have little effect on the revenue volatility of local governments subject to the limitation, they can increase the volatility among those governments that vote to override the requirement. Table 2.1 is also consistent with the literature demonstrating that tax and expenditure limitations encourage states and localities to rely less on property taxes, typically considered a stable source of revenue, in favor of more income-elastic sources, such as sales taxes and user charges. This section further investigates these results by applying difference-in-difference regression analysis to local governments in Colorado and a selection of control states that were not subject to tax and expenditure limitations during the period.

Without a quasi-experimental control group that can be compared with the local governments in Colorado, it is impossible to make inferences about the effect of TABOR; localities in other states may see greater swings in revenue volatility, even in the absence of TELs, as a result of national economic conditions or shifting industrial structures. A comparison group is necessary to control for changes that may have affected localities in other states and impacted the volatility of their revenues at the same time that they impacted Colorado.

Of all of Colorado's neighboring states, Wyoming is the logical choice for a comparison, as it is the only one of Colorado's neighbors that did not enact either a state or local TEL during the time period in question.[9] In fact, the only tax and expenditure limits that had ever appeared on Wyoming's books at that point were specific property tax rates enacted in 1890 and 1911 respectively (Mullins & Wallin, 2004). Moreover, as Table 2.2 demonstrates, not only did local governments in the two states rely on property taxes to the same extent, but revenues among local governments in Wyoming also exhibited roughly the same amount of volatility as did local governments in Colorado prior to the passage of TABOR in 1992.

Though further differences exist between the economies and populations of the two states, the advantage of difference-in-difference estimation is that it controls for unobserved heterogeneity between the two states that is constant over time. That is, by focusing on the change in volatility that occurred, the model sweeps away differences between the two states that might result in different levels of volatility, such as different industrial structures or revenue structures. To provide additional robustness to the results, a further set of regressions compares Colorado's revenues to a wider group of states – Wyoming, Connecticut, and New Hampshire – with the choice of the two additional states driven by the fact that neither Connecticut nor New Hampshire had ever been subject to a TEL as of 2004 (Mullins & Wallin, 2004).[10]

[9] Utah repealed a property tax revenue limit in 1986 and enacted a state expenditure limit in 1988. Arizona enacted a state revenue limit in 1992. New Mexico amended property tax rate limits in 1987. Nebraska enacted a property tax revenue limit in 1990. Kansas suspended a property tax revenue limit in 1989. Oklahoma enacted a state revenue limit in 1992. Mullins & Wallin (2004).

[10] Regressions were also run in which Vermont, another state that had not enacted a TEL, was included as part of the control group. However its inclusion left regression results unchanged and only worsened the fit of the model.

Table 2.2 Summary Statistics

Variable	Colorado	Wyoming	Connecticut	New Hampshire	All
Mean Percent of Revenue from Property Taxes, 1986	.28	.28	.45	.64	.43
Mean Volatility 1986-1992	.130	.160	.148	.196	.161
Mean Volatility 1994-2000	.127	.131	.139	.116	.127
Mean Change in Volatility	-.003	-.028	-.008	-.080	-.034
Mean Change in Population, 1986 to 2000 (Thousands)	13.1	-0.5	1.4	2.3	4.2
Mean Change in County-Level Employment, 1986 to 2000 (Thousands)	43.3	2.3	23.1	18.0	22.3
Mean Change in County-Level Wages, 1986 to 2000 (Millions of $)	2.8	0.1	7.8	1.6	3.0
Number of Observations	113	92	102	144	451

Table 2.2 presents summary statistics for the census data used in the difference-in-difference estimation. The data are taken from the Census Bureau's Annual Survey of State and Local Government Finances and Census of Governments. Data were collected for every year over the time period in question – 1986-2000. Government units that did not show observations for every year were excluded. Because only larger districts are required to provide information for the survey every year, with smaller districts being sampled at a lesser frequency, this had the effect of excluding smaller districts that did

not merit inclusion in the sample on a yearly basis,[11] introducing a potential source of sample bias. That is, the sample of local governments used for the analysis below consisted disproportionately of larger governments. However, when introduced into regression models, neither population size nor the change in population was found to have a significant effect on changes in volatility. What's more, population size was already accounted for in the volatility measure as volatility was calculated in terms of per-capita revenues. Hence, it is unlikely that size alone impacted volatility.

One advantage to the census data is that, unlike the data presented in the previous section, all types of government entities were sampled, including special districts and independent school districts. Volatility was calculated as a standard deviation in the same manner as in the sections above. Nine outliers with volatility swings of greater of greater than 100 percentage points were excluded.[12]

A standard difference-in-difference regression equation might use volatility as the dependent variable and examine the interaction term between the TEL-state and a time variable. However, since there are only two time periods in this case (pre- and post-

[11] Survey methodology has varied over time. The certainty criteria for the 2008 sample were listed as all county governments with a 2002 population of 100,000 or more, all cities with a 2002 population of 75,000 or more, and all special districts meeting the following criteria: 1. Full-time equivalent employment of 1,000 or more, 2. A function code of 42(Mortgage Credit), 92(Electric Power), 93(Gas Supply), or 94(Transit), and 3. Either $20+ million total revenue or expenditure. U.S. Census Bureau 2008 Annual Survey of State and Local Finances Methodology. Accessed at http://www.census.gov/govs/estimate August 2010.

[12] These include four from Connecticut (East Lyme, Stonington, the Westport Housing Authority, and the Meriden Transit District), three from New Hampshire (Manchester, Seabrook, and the Rochester Housing Authority), and two from Wyoming (the Campbell County Hospital District and the West Park County Hospital District).

TABOR)[13], this study examines the estimates from an equation in first differences, similar to Card (1992):

$$\Delta y_i = \beta_1 CO_i + \beta_2 B_i + \beta_3 \Delta X_i + \Delta \varepsilon_i$$

where Δ y is the change in volatility, CO is a dummy variable for local governments in Colorado, B is a dummy variable indicating whether a local government successfully de-Bruced (passed a broad spending limit waiver exempting all revenues indefinitely), X represents a vector of control variables, and $\Delta\varepsilon$ is the error term in the differenced equation. The economic control variables come from County Business Patterns. De-Brucing data come from the Colorado Municipal League and Colorado Counties Inc. Because data on de-Brucing elections were only available for counties and municipalities, and because population and employment data can only be accurately matched with general purpose governments, those models that include the full list of variables necessarily exclude school districts and special districts.

Regression results are presented in Table 2.3. The dependent variable is the same measure of volatility that has been used throughout this essay – the standard deviation of the annual percent change of real per-capita revenues. As a larger standard deviation reflects greater variability in yearly revenue fluctuations, a positive coefficient indicates that a variable has caused volatility to increase. Models 4-6 are similar to Models 1-3, except that they include data from New Hampshire and Connecticut in addition to Wyoming and Colorado. Models 1 and 4 are a simple comparison of the change in

[13] As previously noted, because fiscal year 1994 was the first full fiscal year in which the law was in effect, the analysis uses 1986-1992 as the pre-TABOR time period, and 1994-2000 as the post-TABOR time period.

Table 2.3 Difference-in-Difference Results (Revenues)

	Colorado vs. Wyoming			Colorado vs. Wyoming, Connecticut, and New Hampshire		
	All Observations	Counties and Municipalities Only		All Observations	Counties and Municipalities Only	
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable	Δ Volatility of Revenues	Δ Volatility of Revenues	Δ Volatility of Revenues	Δ Volatility of Revenues	Δ Volatility of Revenues	Δ Volatility of Revenues
TEL State (Colorado)	0.0259 (0.0194)	0.0920*** (0.0302)	0.141*** (0.0414)	0.0418** (0.0168)	0.0818*** (0.0208)	0.180*** (0.0405)
De-Bruced		0.0405 (0.0445)	0.0150 (0.0359)		0.0405 (0.0440)	-0.00295 (0.0394)
Δ Population			-7.56e-05 (0.000379)			0.000333 (0.000434)
Δ Employment			-0.000686 (0.000826)			-0.00268*** (0.000840)
Δ Wages			-0.00237 (0.00667)			0.0123*** (0.00299)
R^2	0.01	0.13	0.16	0.01	0.06	0.13
F	1.73	5.98	2.82	5.18	6.67	6.14
Prob > F	0.19	0.13	0.02	0.02	0.00	0.00
N	205	83	83	451	221	221

Robust standard errors in parentheses. * p < 0.10, ** p < 0.05, *** p < .01

volatility between Colorado and the control state(s). Models 2 and 5 incorporate the dummy variable for districts that de-Bruced, while Models 3 and 6 also include additional controls. Models 1 and 4 include all types of local governments, such as school districts and public authorities. Models 2, 3, 5, and 6 are limited to municipalities and counties.

In all six models, Colorado saw an increase in the volatility of its revenues following TABOR relative to other states. This result was statistically significant at the five percent level in five of the six models. When no other variables are taken into account, the coefficient on the dummy variable for Colorado indicates that districts that did not de-Bruce saw an increase in their volatility somewhere between 2.6 and 4.2 percentage points. As more variables are introduced into the model, this effect seems to

Table 2.4 Difference-in-Difference Results (Expenditures)

	Colorado vs. Wyoming			Colorado vs. Wyoming, Connecticut, and New Hampshire		
	All Observations	Counties and Municipalities Only		All Observations	Counties and Municipalities Only	
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable	Δ Volatility of Operating Expenditures	Δ Volatility of Operating Expenditures	Δ Volatility of Operating Expenditures	Δ Volatility of Operating Expenditures	Δ Volatility of Operating Expenditures	Δ Volatility of Operating Expenditures
TEL State (Colorado)	0.0453* (0.0254)	0.141*** (0.0461)	0.139** (0.0562)	0.0306* (0.0173)	0.0744*** (0.0212)	0.106*** (0.0375)
De-Bruced		0.0163 (0.0390)	0.0180 (0.0389)		0.0163 (0.0386)	0.0118 (0.0393)
Δ Population			-0.000126 (0.000349)			0.000123 (0.000381)
ΔEmployment			-0.000495 (0.000824)			-0.000857 (0.000636)
Δ Wages			0.00945 (0.00923)			0.00862*** (0.00269)
R^2	0.02	0.13	0.13	0.01	0.04	0.07
F	3.37	6.09	2.39	2.66	4.74	3.49
Prob > F	0.07	0.00	0.05	0.10	0.01	0.00
N	204	83	83	451	225	225

Robust standard errors in parentheses. * p < 0.10, ** p < 0.05, *** p < .01

increase. Model 6 suggests that districts in Colorado that did not de-Bruce saw an increase in their volatility of 18 percentage points. Interestingly, Table 2.3 does not indicate that there was a statistically significant difference between districts that de-Bruced and those that did not. This differs somewhat from the findings in Table 2.2, which suggested that de-Bruced districts were driving the increase in volatility.

Table 2.4 presents results for changes in the volatility of operating expenditures. Although the focus in this essay has been on revenue volatility, volatile revenues are only harmful insofar as they disrupt planning and service provision. Measuring expenditure volatility gives insight into the extent to which service provision was disturbed. The results for operating expenditures are broadly similar to those for revenues. The coefficient on the dummy variable for Colorado is positive in each model, and it is statistically significant at the 5 percent level in four of the six. The expenditure results

are consistent with those for the revenue data in that they also do not indicate that de-Brucing districts exhibited different patterns in their volatility than districts that did not de-Bruce. The positive coefficients on the de-Brucing dummy variable, though not statistically significant, suggest that volatility increases may have been even larger in those districts.

Previous work has documented the effect that TABOR has had on the size of Colorado's public sector; despite strong economic growth in the 1990s, the state's ranking of combined state and local taxes as a percentage of income dropped from 22nd in 1979 to 43rd in 2003 (The Bell Policy Center, 2003). The results in this section suggest that the law may have had additional effects besides shrinking the size of the public sector. Instead of making revenues more predictable, as might have been expected, it appears that TABOR in fact may have made budgeting more difficult for local governments. Districts in the state – whether they overrode the amendment's provisions or not – experienced greater revenue and spending swings in the wake of the amendment than comparable states did, potentially complicating infrastructure projects and casting uncertainty over the government's capacity to honor its commitments.

This finding is consistent with recent work that focuses on how tax and expenditure limits interact with states' revenue mixes, particularly during economic downturns. In arguing that property tax limits have caused states to rely on more income-elastic sources, such as user charges and fees, McCubbins and Moule (2010) stress the effect that this can have during recessions when revenues decline precipitously in response to deteriorating economic conditions. Although their essay specifically documents the effect of TELs during recession years, they suggest that the harmful

effects of property tax limits may even extend to periods of strong growth, writing: "States are building a revenue system that puts them on a roller coaster with huge swings between the apex of the coaster's climb and the nadir of its fall" (McCubbins & Moule, 2010, p. 619).

Although in theory the general revenue limits imposed on local governments by TABOR should prevent sharp revenue increases during times of economic growth – which Colorado enjoyed throughout the 1990s – the vast number of local governments that overrode the law's mandates demonstrates that in fact this may not have been the case. In response to TABOR, localities did shift toward more income-elastic revenue sources. This, combined with the de-stabilizing effects of over-riding the law in some districts, likely caused revenue swings to increase. This may have been particularly problematic for Colorado, as the Colorado Legislature was limited in its ability to form a rainy day fund that might have enabled it to assist local governments during times of need because excess revenues had to be returned to taxpayers (The Bell Policy Center, 2003).

2.6 Conclusion

This essay has attempted to fill a gap in the literature on tax and expenditure limitations by examining the impact of TELs on the revenue volatility of local governments. Much of the literature on TELs is focused on absolute levels of spending and on state-level effects, while there has been little to no focus on budgeting and financial planning at the local level. It has argued that stable revenues play a crucial role in the ability of local governments to plan and allocate resources and has analyzed

whether, at least in the case of Colorado, a link exists between tax and expenditure limitations and both revenue and expenditure volatility.

This is not to suggest that the volatility of taxing and spending is the most important metric in assessing the impact of tax and expenditure limitations. The overall size of government is clearly relevant, as are other metrics such as surveys of voter satisfaction and confidence in government. Rather, volatility is a key input that has often been ignored by scholars studying the effects of budget policy. Without taking into account financial risk, it is difficult to have productive conversations about the appropriate size of government or the level of revenues necessary to sustain the desired level of services. More recent research highlights the importance of accurate revenue estimation, which allows for more informed spending decisions and long-term planning (The Pew Center on the States and the Nelson A. Rockefeller Institute of Government, 2011).

Results show that the tax limitation enacted in Colorado in 1992, commonly known as TABOR, appeared to destabilize both revenues and expenditures relative to local governments in non-TEL states over the same time period. This estimation controlled for changes in key demographic and economic variables as well as for de-Brucing elections in which some districts succeeded in over-riding the law's mandates. Regression estimates suggest that the standard deviation of the annual percent change in revenues in Colorado grew between 3 and 18 percentage points, while the standard deviation of the change in operating expenditures grew between 3 and 14 percentage points.

While data from the Colorado Division of Local Government suggest that increases in volatility may have been the result of such de-Brucing efforts – the effect of instituting revenue limitations and then removing them within a short period of time – results from difference-in-difference estimation indicate that in fact even governments that were subject to TABOR over the entire time period of study also saw their revenue stream de-stabilized. The most likely explanation for this increase is that as local governments shifted away from property taxes in response to the provision of TABOR that specifically limited property tax revenues, they shifted into more income-elastic revenue sources, leaving them more vulnerable to shifts in economic conditions.

What does this say about TELs, and about TABOR in particular? It adds to the wide body of literature that already exists chronicling TABOR's effect on public spending in the state, particularly in the area of education. However, it refocuses attention on the practical matter of budgeting and financial planning. If voters hope that fiscal limits will force governments to be more efficient with their resources, these results suggest that at least in one respect the opposite is happening: TELs may make it more difficult for lawmakers to budget and plan efficiently.

Any study that attempts to isolate the effects of a particular policy change faces numerous challenges, and this is no less true when studying Colorado, which passed a series of fiscal-tightening measures even before TABOR. Prominent among these amendments was the Gallagher amendment, passed in 1982, ten years before TABOR, which capped the proportion of property taxes that could come from residential sources. As residential property values rose dramatically in the late 1990s, particularly in the Denver area, the Gallagher amendment, along with TABOR, prevented much of this

added value from being taxed (Martell & Teske, 2007). Although the Gallagher amendment was in place for some time before TABOR, it is possible that some of the effects picked up in the prior sections can be attributed to the confluence of the two constitutional amendments, particularly if a rise in property values brought the Gallagher amendment into play in a way that it had not been previously.

Another challenge of studying local government revenues is obtaining a comprehensive sample. This essay utilizes several data sources, including the Colorado Division of Local Governments and the Census Bureau's Annual Survey of State and Local Government Finances, in order to provide a more accurate picture of revenue volatility in Colorado before and after the enactment of TABOR. However, neither source is fully comprehensive; the Colorado data set lacks information about component units and special districts, while demographic variables and data on de-Brucing elections are available only for general purpose governments. There are also significant data limitations in terms of the number of covariates available to explain variation in revenue volatility at the local level. Further research on the effect of TELs on local government finances would benefit from a wider sample and an expanded control group.

Chapter 3 The Impact of Budget Stabilization Funds on State Pension Contributions

3.1 Introduction

Ever since the late 1990's, when the Government Accounting Standard Board (GASB) substantially increased the amount of reporting that was required for public employee pensions and other post-employment benefits (OPEB), the financial condition of state and local governments has come under increasing scrutiny. This scrutiny was further heightened by the most recent economic downturn and by the publication of numerous reports that highlighted the funding status of public pension plans, with some lawmakers even calling for states to be allowed to declare bankruptcy in order to avoid their unmanageable pension obligations.

While numerous factors have contributed to the states' current financial condition, the primary reason that pension obligations have drawn concern is that most states offer their employees (or have offered in the past) defined benefit plans. A defined benefit plan promises the employee a pre-determined benefit level, in contrast to a defined-contribution plan, in which the employer contributes a set amount to a pension fund on behalf of the employee, with benefits to be determined based on the investment performance of the fund. As of 2005, more than 90 percent of state and local employees participated in defined benefit plans (A. H. Munnell, Haverstick, & Soto, 2007).

The practice of offering defined benefit plans has led to large unfunded liabilities in the public sector, especially during economic downturns. Recent estimates of states' unfunded pension liabilities have ranged from as low as $500 billion to as high as over $3 trillion (Lav & McNichol, 2011; Pew Center on the States, 2010). Much of the disagreement over this figure stems from the fact that states are allowed substantial

leeway in the discount rate that they use to value future benefit obligations, which also determine the amount of money that states are supposed to contribute on an annual basis to their pension funds. A state's overall funding level (or funded status) is calculated by dividing the total assets that are set aside to pay for pension costs divided by a fund's actuarially accrued pension obligation.

The assumptions that states have used in calculating their funding level, in particular their assumptions about future investment returns, have been the focus of much of the recent research on public pensions. Novy-Marx and Rauh (2009) argue that unfunded pension liabilities should be valued using a riskless rate of return to reflect the fact that states are legally obligated to make due on their pension promises. Lav and McNichol (2011) suggest that this approach may go a bit too far, pointing out that median investment returns over the last 25 years would suggest a discount rate even higher than the 8 percent discount rate most states already use.

However, despite the surge of interest in pension liabilities and the ability of states to make due on their obligations, there has been little focus on the role that fiscal institutions may play in pension funding, despite the importance that has been attached to fiscal institutions in the larger context of public financial management.[14] This essay focuses on one institution in particular. Budget stabilization funds, sometimes referred to as "rainy day" funds, facilitate savings during years of flush revenues so that states will have resources to draw upon during economic downturns. Studies have found that states often reduce or skip altogether required deposits to their pension funds in order to meet balanced budget requirements (National Conference of State Legislatures, 2010; Peng, 2004; U.S. General Accounting Office, 1993).

[14] See, for example, Poterba (1996).

Insofar as budget stabilization funds and pension funds can be used similarly as countercyclical tools, the question arises: does the design of budget stabilization funds impact pension funding? Do budget stabilization funds serve as a source of reserves that state legislatures may draw upon to maintain pension contributions during times of fiscal stress? Or conversely, do states face trade-offs when depositing to one fund or the other?

This essay attempts to explain variation in the percent of the annual required contribution (ARC) that states made to their pension funds over the period 1997-2008. It employs the Blundell-Bond System GMM estimator to account for the potential endogeneity of the different types of deposit and withdrawal rules that drive the management of budget stabilization funds. Results support the growing body of literature demonstrating the significant role that fiscal institutions play in public financial management.

3.2 Determinants of State Pension Funding

The measure that is most commonly used to assess the soundness of a state's pension funds is the funding level – the ratio of assets to liabilities. The funding level is determined by: 1) the amount that employees contribute, 2) the amount that the employer (usually the state) contributes, 3) the investment performance of the fund, and 4) past liabilities that have accrued for which the state has not set aside money.

The funding level also depends on the accounting assumptions that are used to discount the amount of future liabilities. These accounting assumptions, some of which are based on government accounting standards, have been the basis for much of the work on pension funds. In a widely cited paper, Novy-Marx and Rauh (2009) show that the

actuarial method recommended by the Governmental Accounting Standards Board – using the historical return on a fund's assets – causes states to inaccurately discount their liabilities, resulting in liabilities that are lower than they would be under market-based discount rates. Their analysis suggests that the liabilities listed on state balance sheets may not be reliable estimates of the amount taxpayers will be required to fund in the future.

Because accounting rules play a part in determining the funding levels of pension funds, they also play a role in the amount that states contribute to their funds on an annual basis. The annual required contribution (ARC, sometimes referred to as the actuarially required contribution) is the amount that actuaries have determined a state must contribute on an annual basis if it is to cover the benefits earned by current employees as well as unfunded liabilities that have accrued. The term is somewhat of a misnomer as states are rarely *required* to contribute the full amount of their ARC, though they must disclose any shortfall in their contributions. Eaton and Nofsinger (2004) examine numerous actuarial assumptions, including the salary growth assumption, amortization period, and expected rate of return, and conclude that states facing financial constraints are more likely to have optimistic accounting assumptions as well as to underfund their plans. Their result is consistent with that of Chaney, Copley, and Stone (2002), who find that states under fiscal stress that also face strict balanced budget requirements are more likely both to underfund their pension funds and select discount rates that obscure the underfunding.

The focus on discount rates and actuarial assumptions has arisen because of the concern that state pension funds, regardless of how their liabilities are measured, may be

severely unfunded. In one of the first comprehensive reports on state retirement systems nationwide, the Pew Center on the States found that, as of fiscal year 2008, state pension plans had roughly $2.3 trillion in assets and $2.8 trillion in liabilities, reflecting a gap of approximately half a trillion dollars (Pew Center on the States, 2010). A report by the Government Accountability Office concluded, as the Pew report did, that health benefits are even more underfunded than pensions, with liabilities between $600 billion and $1.6 trillion in present value terms (U.S. Government Accountability Office, 2008). The Center for Retirement Research at Boston College has released a series of reports detailing the funding levels of the largest state and local plans, projecting that the aggregate funding level of the 126 plans in their sample will decline from 84 percent in 2008 to 72 percent in 2013 (A. Munnell, Aubry, & Quinby, 2010). For the most part these accounts report on the liabilities as measured by the states themselves and thus may understate the strain that pensions will place on state budgets in the future.

Other reports suggest that the alarm over public pension funding may be overblown. Lav and McNichol (2011) point out that 60 percent of public pension revenues come from investment earnings and that before the two most recent recessions, state and local pension obligations were fully funded on average. Giertz and Papke (2007) also suggest that swings in stock market movements are largely responsible for the changes in pension assets, noting that investment returns were 3.3 times larger than employer contributions and 5.8 times larger than employee contributions between 1993 and 2004. Moreover, the GAO cites various experts that consider a funded ratio of 80 percent to be sufficient for public plans, as a well-funded plan may become a target for

legislators with other priorities or unions arguing for higher benefits (U.S. Government Accountability Office, 2008).

Because of the large impact that investment returns have on funding levels, the impact of fiscal and political institutions is more likely to be visible in the amount that states contribute to their funds on an annual basis. Nevertheless, few studies have examined the link between fiscal institutions and the contributions that states make to their pension funds. Part of the reason for this omission may be that many states follow statutes specifying exact contribution rates for each of their retirement funds. In some states, statutes require that the state contribute exactly the actuarially determined amount. In others, contribution rates are established as a percentage of payroll and then periodically reviewed to ensure that the payments are sufficient to pay off unfunded liabilities. However, these statutory requirements are often overlooked or ignored in times of fiscal stress (National Conference of State Legislatures, 2010; Peng, 2004; U.S. General Accounting Office, 1993).

Consider the case of California, which in 1995 enacted a statute requiring the state to make the actuarially determined contribution to its Public Employees Retirements System (CALPERS) and then failed to contribute the full ARC every year between 2002 and 2008. Some cross-sectional studies cite statutorily determined contribution rates as the main reason that some states do not meet their full ARC contributions in certain years (A. Munnell, Haverstick, Aubry, & Golub-Sass, 2008). However, as states often adjust their statutory rates from year to year in order to maintain an amortization schedule, this does not explain why states would fail to meet the required payments. In some states, such as Wyoming, actuaries establish contribution rates that are then subject to approval

by the legislature. Hence, understanding why some states fail to pay their annual required contribution in full is not as straightforward as understanding the statutes that are in place, as these statutes are only as strong as the legislature's ability and desire to comply with them.

Even when states face constitutional provisions regarding their pension contributions, the law is not always a clear guide. For instance, Louisiana's constitution requires that the state "provide an amount necessary to fund the employer portion of the normal cost" as well as "provide for the amortization of the unfunded accrued liability existing as of June 30, 1998".[15] However, this provision does not mean that the state will be required to pay its ARC each year, as the unfunded accrued liability may have changed over time due to changes in assumptions about member characteristics or benefits that were retroactively granted. Only Arizona faces a constitutional requirement that appears to be binding. [16] Indeed, the state paid its annual requirement contribution in full every year between 1997 and 2008. Because of the strictness of their constitutional provisions, both Louisiana and Arizona are excluded from the analysis in the following sections.

This essay focuses on the impact of budget stabilization funds on pension contributions, and in doing so, attempts to shed further light on the role that fiscal institutions play in public financial management. It does not attempt to explain the variation in funding levels, as the funding levels of most plans are determined in large part by investment earnings, nor does it examine OPEB contributions. While few states

[15] La. Const. Art. X, § 29.
[16] Even the language in Arizona's constitution appears to leave room for payments that might not meet the full ARC: "Public retirement systems shall be funded with contributions and investment earnings using actuarial methods and assumptions that are consistent with generally accepted actuarial standards." A.R.S. Const. Art. XXIX, § 1.

have set aside adequate funding for their OPEB liabilities, health benefit promises do not face the same constitutional protections as pensions and as a result states have some flexibility to change the provisions of these plans if necessary. Moreover, not only is there is a great deal of variability to health benefit promises, but with the passage of the Affordable Care Act, there is a great deal of uncertainty over health benefits going forward (Lav & McNichol, 2011).

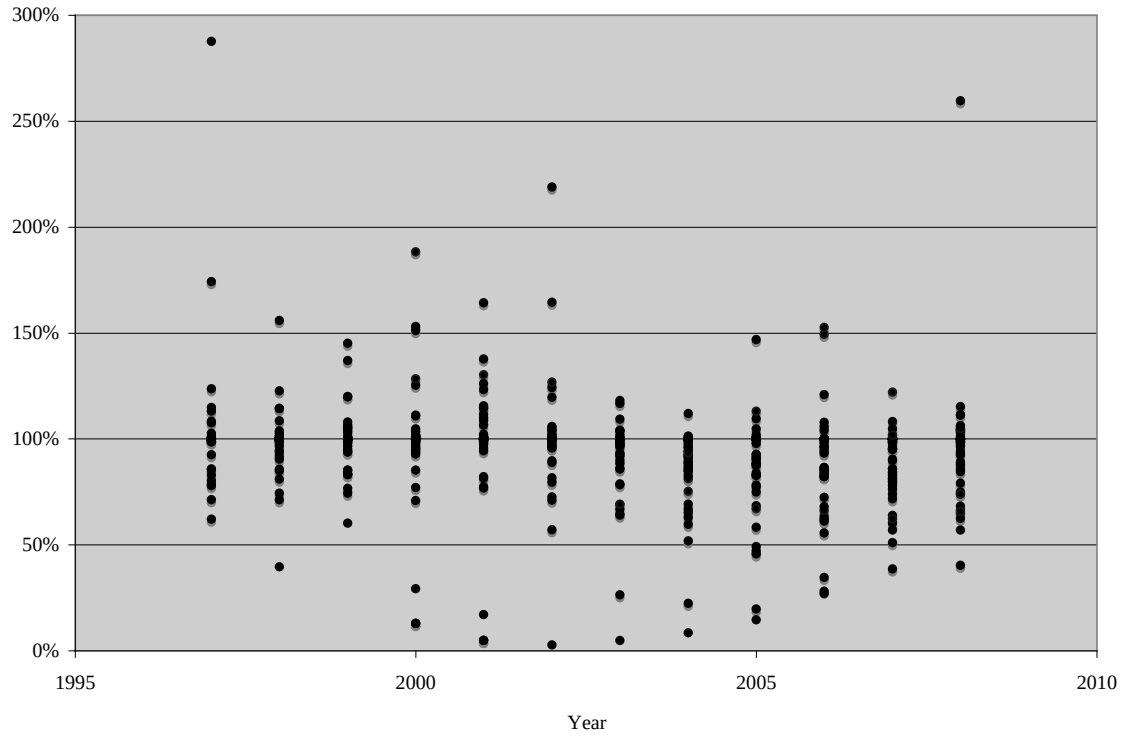
A common way of measuring state contributions is by comparing them to the actuarially required contribution (ARC), the amount that actuaries have determined a state must contribute on an annual basis if it is to cover the benefits earned by current employees as well as unfunded liabilities that have accrued. States are rarely *required* to pay the full amount of the ARC, though as noted previously a few states do face legal stipulations regarding their payment of the ARC. Nonetheless, the ARC is a good benchmark to compare contributions against because it reflects the amount that the state should contribute based on a number of factors, including the demographic make-up of the workforce.

Figure 3.1 displays the percent of their annual required contribution that states contributed over the period 1997-2008.[17] Most states contributed close to the full amount of the ARC on average. However, as the chart makes clear, there is substantial variation, and at times states contributed amounts that were far in excess of their required payment. In 1997, New Jersey issued pension obligations bonds, enabling the state to contribute 288 percent of its ARC.[18] The following year, with its funding level over 100 percent,

[17] Two outliers that fell outside the bounds of the chart are not shown.
[18] Illinois issued $10 billion worth of pension obligations bonds in 2003 and subsequently contributed 111 percent of its ARC in 2004. Connecticut issued $2.6 billion in pension obligation bonds in 2008 and contributed 260 percent of its ARC. Other states also issued pension obligation bonds during the time

the state contributed only 40 percent, and in 2002 it contributed less than 3 percent. In 2007, West Virginia used money from a tobacco settlement to pay down the unfunded liability in its teachers' retirement system.

Figure 3.1 Percent ARC Contributed



This essay focuses on states' contributions as a percentage of the ARC in order to gain a better understanding of the trade-offs that are made at the margins of the budget process and the manner in which different counter-cyclical policies may interact with each other. Budget processes in some states may favor current period spending at the expense of setting aside adequate funds to meet future commitments.

period but did not include these amounts as part of their contributions toward the ARC. For consistency's sake, these three observations are excluded from the subsequent analysis.

3.3 Budget Stabilization Funds

In modeling the effect of fiscal institutions on pension contributions, this essay focuses on one institution in particular: budget stabilization funds (BSFs). States adopt budget stabilization funds in order to ease constraints imposed on them by balanced budget requirements; the savings that such funds promote enable states to have reserves to draw upon during times of fiscal stress.

To the extent that a state has few reserves to draw upon during a recession, state legislators may be forced to reduce pension contributions in order to balance the state's operating budget. In this respect, both BSFs and the pension contributions that a state makes (or does not make) are part of the state's countercyclical fiscal policy. Although a state's reserves are fungible, and thus the link between BSFs and pension contributions is indirect, a few states make the connection explicit, calling for excess revenues to be deposited into a state's budget stabilization fund until the fund reaches its cap, at which point some of the remainder is deposited into the state's retirement funds. For instance, the statute that governs Connecticut's budget stabilization fund states:

> "When the amount in said fund equals ten per cent of the net General Fund appropriations for the fiscal year in progress, no further transfers shall be made by the Treasurer to said fund and the amount of such surplus in excess of that transferred to said fund shall be deemed to be appropriated to the State Employees Retirement Fund."[19]

Similar language can be found in Maine's statutes:

> "At the close of every month during which the stabilization fund is at the 12% limitation described in subsection 1, the State Controller shall transfer from the General Fund to the Retirement Allowance Fund established in section 17251 an amount equal to the investment earnings that otherwise would have been credited to the stabilization fund."[20]

[19] Conn. Gen. Stat. § 4-30a
[20] Me. Rev. Stat. Ann. Tit. 5 § 1532

Despite these connections, researchers have yet to probe the link between BSFs and pension funds. The existing research on budget stabilization funds focuses on the extent to which the funds produce savings and impact borrowing costs. Knight and Levinson (1999) find that states with BSFs have higher reserves than states without them. Wagner (2003) finds that the reserves that are maintained in BSFs are to some extent substitutable with reserves in the general fund; every $1 dollar deposited in a budget stabilization fund results in roughly 51 to 56 cents in savings. A follow-up study reveals that most states realize a modest decrease in borrowing costs following the adoption of a BSF, but that different types of deposit and withdrawal rules affect borrowing costs differently (Wagner, 2004). Just as Knight and Levinson (1999) do, Wagner characterizes BSFs according to the rules that govern deposits and withdrawals as well as the size of the fund. Strict deposit rules require a state to deposit money according to a mathematical formula or in the case that revenue growth is positive; that is, a strict deposit rule may require a state to deposit money in its BSF even if it is facing a budget deficit. A strict withdrawal rule means that a state can only withdraw money with the approval of a supermajority of the legislature or following a mathematical formula; it may at times be unable to withdraw money even if it is facing a budget deficit. Grizzle (2010) finds that weak deposit rules are associated with lower credit ratings while weak withdrawal rules are associated with higher credit ratings.

More recent work builds on the classification schemes that are used to categorize BSFs. McNichol and Boadi (2011) discuss rules that require states to replenish their funds over a specified period of time after a withdrawal. Such “replenishment rules” are in a sense equivalent to a strict deposit rule as they may force states to make deposits to

their BSF even during economic downturns.[21] New York is an example of a state that has had a difficult time making optimal use of its BSF because of a replenishment rule that requires it to replace funds that have been withdrawn within three years of the withdrawal. As a result, the state has been wary of making use of the fund in times of fiscal stress.

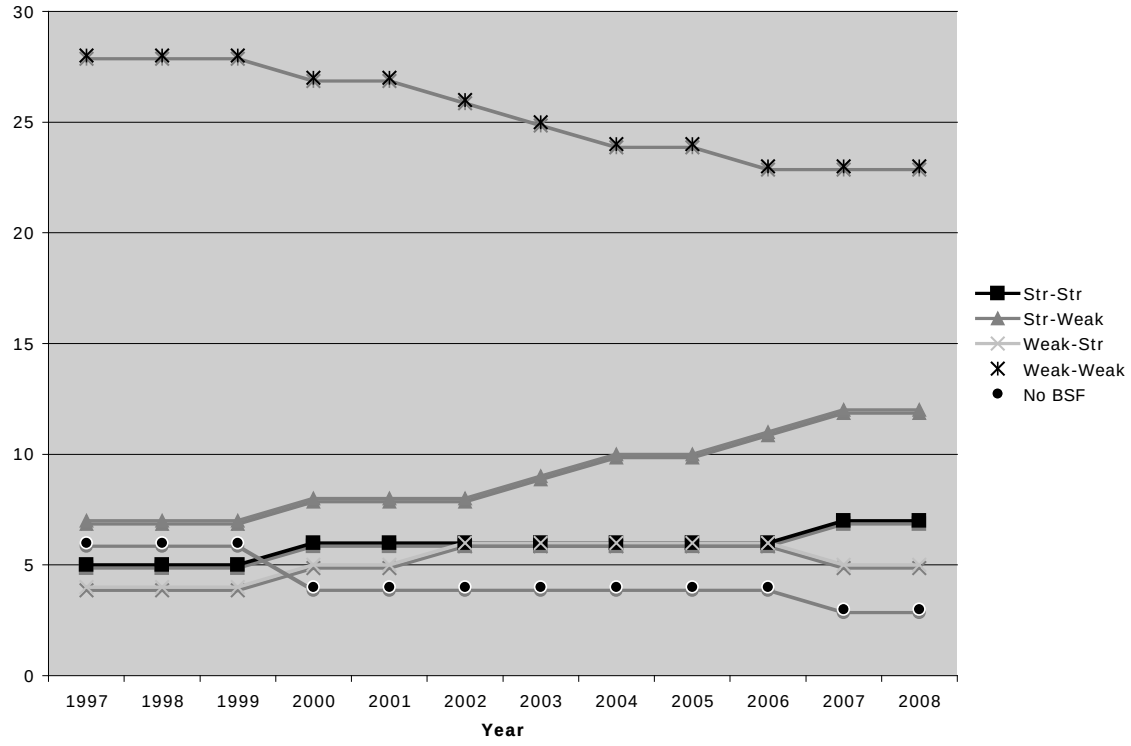
It should be noted that some states have more than one stabilization fund, and in fact there is some disagreement over whether certain funds qualify as BSFs or not. This disagreement often occurs in the case of funds that are intended for specific purposes, such as for capital projects only. In its list of BSFs, the National Conference of State Legislatures (2008a) includes some funds that are expressly intended for education spending, although Wagner (2003) does not. This essay follows the more inclusive definition used by the NCSL and thus does include some education funds in its data as such funds can rival other BSFs in size.

One question that arises in states with multiple funds is how to characterize their deposit and withdrawal rules in the case that different funds follow different rules. If a state has more than one BSF, and one fund applies more strict requirements than the other, the following rules are applied: the state is categorized as having the more lenient withdrawal rule since the state has access to reserves from at least one fund. On the other hand, the more strict deposit rule would apply since the state would be forced to set aside money even on certain occasions when it is unfavorable to do so.

[21] For the sake of the data analysis in the following section, states with replenishment rules requiring them to build up funds within fewer than five years following a withdrawal are treated as having a strict deposit rule, regardless of what the deposit provisions are, as replenishment rules of this sort are equivalent to a strict deposit rule. The five year cut-off is used because, as McNichol and Boadi (2011) point out, replenishment that occurs more than five years after a withdrawal is more likely to occur after a fiscal crisis has passed.

Figure 3.2 delineates the number of states that fall into each BSF category. (Appendix I provides a complete listing of state deposit and withdrawal rules as compiled by the author.)[22] The majority of states (41) remained in the same category between 1997 and 2008, while nine states switched categories at least once. Only three states – Arkansas, Kansas, and Montana – did not have a budget stabilization fund at any point. As the chart makes clear, the majority of state budget stabilization funds have both weak deposit rules and weak withdrawal rules, giving the legislature considerable discretion in making both deposits and withdrawals. On average, approximately 20 states had either a strict deposit rule or a strict withdrawal rule in place.

Figure 3.2 Number of States in Each BSF Category

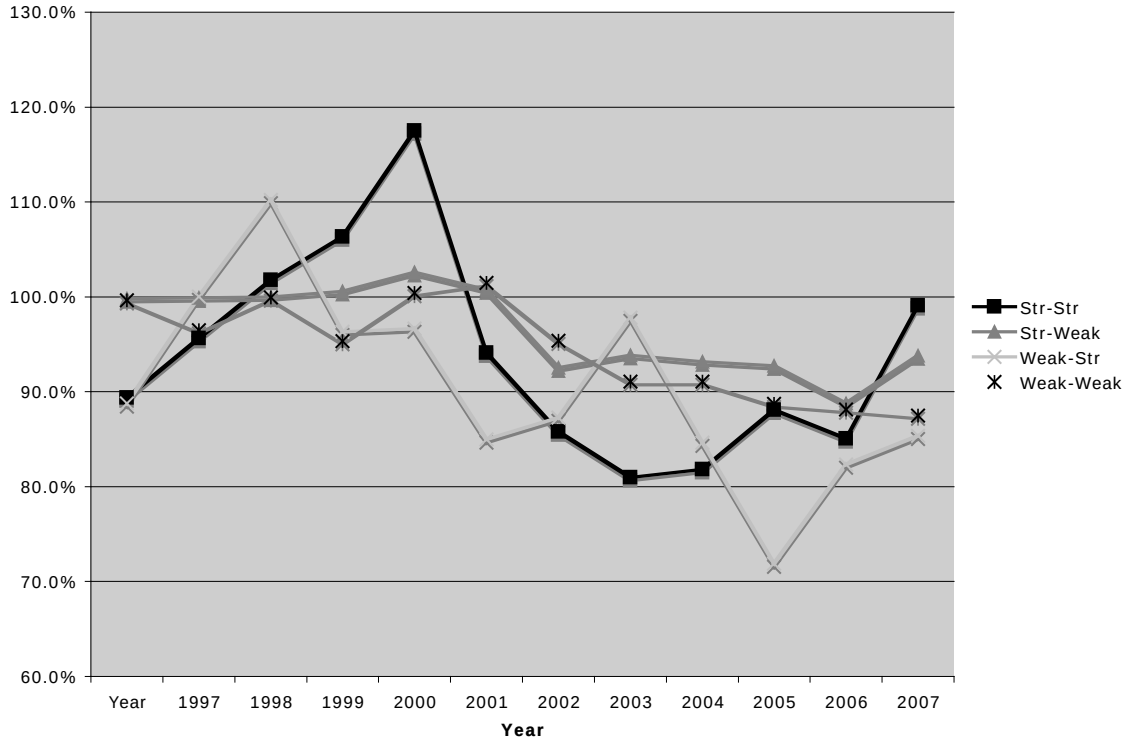


[22] Detailed descriptions of state budget stabilization funds are also available from McNichol and Boadi (2011), National Conference of State Legislatures (2008a), and National Association of State Budget Officers (2008).

Figure 3.3 displays the average percent of the ARC that states with different types of BSFs contributed. For the sake of clarity, states that changed BSF categories during the time period in question are excluded, as are extreme outliers that may skew the mean (in this case, state-years in which a state contributed less than 25% or more than 200% of the ARC). One noteworthy feature of the graph is that states with strict deposit rules and weak withdrawal rules consistently contributed more than states with weak deposit rules and strict withdrawal rules. One possible explanation for this is that states with strict deposit rules but weak withdrawal rules are more likely to have funds to draw upon in the case of a true fiscal emergency and thus are more able to meet their pension contributions. On the other hand, states with weak deposit rules and strict withdrawal rules may have fewer funds to draw upon in times of fiscal stress. This result is consistent with Knight and Levinson's (1999) finding that states with strict deposit rules have larger BSF balances than those that deposit only through appropriation.[23] However, as is discussed further below, unobservable preferences that affect pension funding may also have caused states to adopt the types of BSFs that they did.

[23] Wagner's (2003) findings regarding strict deposit rules are more inconclusive.

Figure 3.3 Mean Percent ARC Contributed by BSF Category



Note: Includes only states that did not change categories between 1997 and 2008. Excludes outliers, defined as state-years in which a state contributed less than 25% or more than 200% of the ARC.

3.4 Data and Methods

3.4.1 Empirical Model

Data on state pension contributions come from the Pew Center on the States (2010). Pew collected data on state-administered defined benefit plans directly from the states' Comprehensive Annual Financial Reports (CAFRs), pension plan annual reports,

and actuarial valuations. The advantage of the Pew data set is that it aggregates all of a state's contributions to its various plans, as in most cases states cover various categories of workers under different plans and thus contribute to a number of different funds. Thus, the Pew data enable meaningful comparisons across states and a comprehensive analysis of state budgeting institutions, unlike prior work that uses individual funds as the level of observation.

However, one drawback to aggregating state-administered plans is that variation exists in the extent to which state-administered plans include local workers. A common example of a state-run plan that includes local workers is a teachers' retirement system. Though practices differ across states, state-administered systems generally include general state government employees and teachers, while locally-administered systems generally include public safety workers and general municipal employees. Since local governments contribute a portion of the annual pension costs for those plans that include local workers, the burden on the state government to meet pension contributions may be lower as a proportion of the total in states that cover a large number of local government employees. Hence, the percentage of local government employees covered under state-administered plans is included as a control variable (see below).

The empirical model may be expressed as

$$S_{it} = \alpha + \beta X_{it} + \gamma B_{it} + \rho S_{it-1} + \lambda_t + \upsilon_{it} \qquad (1)$$

where S_{it} denotes state i's pension contribution as a percent of its ARC in year t. X_{it} denotes state-specific economic and political control variables. B_{it} is a vector of dummy variables representing fiscal institutions, including strict deposit rules and strict

withdrawal rules.[24,25] λ_t denotes time fixed effects, and υ_{it} is the error term. A lagged dependent variable is included in order to account for autocorrelation.[26]

State fixed effects are not included in the model as the two types of fiscal institutions included – balanced budget requirements and budget stabilization funds – show little variation over time, and as a result are likely to be correlated with the fixed effects. In order to account for state-specific factors that may influence the likelihood of a state to meet the expected level of contributions to its pension funds, a variety of economic and political variables are included in the model.

Table 3.1 provides a list of variables and summary statistics. In addition to a number of economic characteristics, the size of the ARC as a percentage of state expenditures is included as a control. As Chaney et al. (2002) and Eaton and Nofsinger (2004) have pointed out, states may manipulate their actuarial assumptions to reduce the size of their annual payments. As the focus of this essay is on budget stabilization funds, controlling for the size of the ARC is a way of more closely focusing on the role of fiscal institutions rather than the choice of discount rates and actuarial assumptions.

In addition to dummy variables for strict deposit rules and strict withdrawal rules, the model also includes a dummy variable that is equal to one in those cases where the state has capped its budget stabilization fund (or funds, in that case that there is more than one) at five percent or less of appropriations or revenues. States that do not place strict

[24] The decision to focus on strict rules is driven by the fact that almost all states have by now adopted BSFs. Earlier studies that examined the impact of both weak and strict rules had a larger group of states with no BSFs to serve as a comparison group.

[25] A variable representing the interaction between a strict deposit rule and a strict withdrawal rule was originally included in the model. However, the interaction term never attained statistical significance in any specification and was dropped so that the coefficients on the two variables of interest could be more easily interpreted.

[26] Once outliers are removed, a Wooldridge test produces a test statistic of 30, rejecting the null hypothesis that there is no first-order autocorrelation.

constraints on the size of their funds presumably are able to build up larger reserves and subsequently have greater resources to draw upon.

The other fiscal institution included in the model is a dummy variable indicating that a state faces a strict balanced budget requirement. Although all states except for Vermont face some form of balanced budget requirement, some states must only balance the budget that is passed by the legislature, while other states are prohibited from carrying over a deficit from one year to the next. This more strict form of balanced budget requirement exacerbates the trade-offs that states must make during periods of fiscal stress. Chaney et al. (2002) show that fiscally stressed states meet balanced budget requirements through reduced funding of pensions.

Two characteristics of the public sector are included – the number of public employees and the percentage of unionized public employees – in order to account for the relative size and political influence of public employees as an interest group. Similarly, dummy variables reflecting whether or not the government is Democrat or Republican-controlled (indicating control of the governorship as well as both houses of the legislature) capture differences across the two parties in spending priorities.

Finally, two state-specific characteristics of the retirement systems are also included. The aggregate funding level, lagged by one year, accounts for the fact that states with low funding levels may feel more pressure to make their annual payments. The percentage of members who are local government employees accounts for the fact that, as noted above, local governments also make contributions to state-administered plans on behalf of their employees.

3.4.2 Endogeneity

None of the factors mentioned above accounts for the fact that an omitted variable – an appetite for fiscal discipline or fiscal restraint – may be correlated with both fiscal institutions and pension contributions. As Poterba (1996) has pointed out, there are typically two methods used to account for the potential endogeneity of fiscal institutions: including controls for voter preferences or drawing on the history of fiscal institutions to more directly model their causal effect. This essay utilizes two approaches: all models include a control for voter ideology (drawn from Berry, Ringquist, Fording, & Hanson (1998)[27]), and the Blundell-Bond system GMM estimator is employed to correct for possible endogeneity arising from the omitted variable and the use of a lagged dependent variable.

A common approach to dealing with endogeneity in a panel data set is to use instrumental variables. Wagner (2004) and Grizzle (2010) both use instrumental variables to account for potential endogeneity when modeling the effect of budget stabilization fund rules. However, the difficulty of finding valid instruments limits the use of this approach.[28] Additionally, instrumental variables do not address the potential bias introduced by the lagged dependent variable.

[27] The updated series is available at http://www.uky.edu/~rford/stateideology.html. Accessed June, 2011.
[28] A previous version of this essay used an instrumental variable strategy that drew upon the work of Wagner (2004). However, first stage results revealed that the instruments were only weakly correlated with budget stabilization fund rules.

Table 3.1 List of Variables

Variable	Source	Mean	Min	Max
Dependent Variable				
Percent of actuarially required contribution that states contributed in a given year (1997-2008)	Pew Center on the States (2010)	0.96	.03	4.69
Fiscal and Economic Characteristics				
Personal income per capita (Thousands of 2005 dollars)	Bureau of Economic Analysis (BEA)	33.64	23.41	53.09
Real debt outstanding per capita (Thousands of 2005 dollars)	Census	2.82	0.55	10.11
Unemployment Rate	Bureau of Labor Statistics	4.7	2.3	8.3
Fiscal Institutions				
Strict balanced budget requirement (no deficit carryover)	National Conference of State Legislatures (NCSL)	0.73	0	1
Budget stabilization fund: strict deposit rule	NASBO (1999, 2002, 2008) , NCSL (2008), State Statutes and Constitutions	0.30	0	1
Budget stabilization fund: strict withdrawal rule	NASBO (1999, 2002, 2008) , NCSL (2008), State Statutes and Constitutions	0.22	0	1
Budget stabilization fund: capped at 5% or less of appropriations/revenues	NASBO (1999, 2002, 2008) , NCSL (2008), State Statutes and Constitutions	0.43	0	1
Public Sector Characteristics				
Number of public employees per 100 population	Census / Unionstats.com	7.34	4.94	13.33
Percentage of unionized public employees	Unionstats.com	33	5.2	72
Political Factors				
Democratic-controlled (both legislature and governorship in hands of Democrats)	The Book of the States	0.18	0	1
Republic–controlled (both legislature and governorship in hands of Republicans)	The Book of the States	0.24	0	1
State Citizen Ideology	(Berry et al., 1998), revised 1960-2008 series	51.29	8.45	95.97
Funding Characteristics				
Actuarially Required Contribution as Percent of State Expenditures	Pew Center on the States (2010)	3.17	0.04	11.64
Lagged Funding Level - Assets divided by state's actuarially accrued pension obligation	Pew Center on the States (2010)	0.89	0.39	1.28
Percent of contributions from local governments	Census	0.45	0	0.93

An alternative approach, first proposed by Anderson and Hsaio (1982), and further developed by Arellano and Bond (1991), Arellano and Bover (1995) and Blundell and Bond (1998), is to derive instruments that are internal to the equation and then estimate the model using a generalized method of moments (GMM) estimator.

The Arellano-Bond/Arellano-Bover/Blundell-Bond estimator is well-suited for the econometric problems at issue in this essay, as it was designed for 1) "small T, large N" panel estimation involving 2) a lagged dependent variable, 3) endogenous regressors, 4) unobserved time-invariant characteristics, and 5) heteroskedasticity and autocorrelation within individuals, all problems that are relevant to the case at hand.[29] The original Arellano-Bond estimator works by using lagged values of the endogenous regressors as instruments and differencing the equation to eliminate the unobserved state-specific effect (referred to as "difference" GMM). In case the lagged regressors are poor instruments for the differenced regressors, system GMM augments this approach by using a system of equations that includes both the original model and the differenced model. System GMM also allows for the inclusion of time-invariant regressors, which would disappear in difference GMM.

For the purpose of system-GMM estimation, the following variables in equation 1 are treated as endogenous: the lagged dependent variable, a strict balanced budget requirement, a strict deposit rule, a strict withdrawal rule, a cap of five percent on the BSF, the ARC as a percent of a state's budget, and the lagged funding level. All are likely to be correlated with time-invariant state characteristics and thus may bias OLS estimates. In order to limit the number of instruments to be less than the number of

[29] For an extended discussion of difference and system GMM, see Roodman (2009).

groups, only the second lag of the endogenous variables are used as instruments.[30] A small-sample correction is also applied.

3.5 Results

Table 3.2 presents the results of ordinary least squares regressions. Regressions 1-2 present results for the full sample of states, while Regressions 3-4 exclude observations in which the percent of the ARC contributed is less than 25% or more than 200%. Regression 2 and 4 include year dummies to account for unobserved time-specific characteristics that are not captured by the model. The decision to exclude outliers in Regressions 3-4 is motivated by the concern that outlying observations with unusually large errors can have a strong influence on OLS estimates. Furthermore, many of the outliers result from unusual events that are not representative of states' typical budget processes, such as West Virginia's decision in 2007 to use money from a tobacco settlement to pay down its unfunded liability. Standard errors are robust and clustered by state.

The large number of outlying observations observed in Figure 3.1 suggests that, in addition to a specification that excludes outliers, it may be useful as a further robustness check to treat the dependent variable as dichotomous; that is, states either contribute at least the full amount of the ARC or they do not. Table 3.3 presents the results of probit regressions. The dependent variable in Regressions 1-2 is equal to one in the case that a

[30] This approach is recommended by Roodman (2009), who points out that instrument overproliferation can overfit endogenous variables. In its output table, Stata warns when the number of instruments is large relative to the number of observations. What's more, using deeper lags reduces sample size. Limiting the number of instruments to the second lag of the endogenous variables produces an instrument count of 45, slightly less than the number of groups.

state contributes an amount equal to or greater than the ARC. The dependent variable in Regression 3-4 is equal to one in the case that a state contributes an amount equal to or greater than 90% of the ARC. Coefficients represent marginal effects.

Several results are consistent across the two tables. Among the economic variables, personal income per capita and the unemployment rate have negative coefficients that are statistically significant. An increase of one percentage point in the unemployment rate appears to decrease the percent of the ARC that states contribute by approximately two to three percentage points.[31] The coefficient on personal income per capita, although statistically significant across the two models, is extremely small. The negative sign may indicate that states with a higher tax base are able to defer current payments because of their greater capacity to increase future taxes.[32] As expected, the ARC as a percentage of state expenditures also is also negatively associated with pension contributions (though it is not statistically significant in Table 3.2); states for which the annual required contribution represents a larger burden are less likely to contribute the full amount. A one percentage point increase in the relative burden that the pension payments represent decreases the percent of the ARC that the state will contribute by two percentage points.[33]

Among the variables of interest – the three BSF variables – the signs on the coefficients are consistent across all specifications – positive for the strict deposit rule,

[31] Alternatively, the marginal effects displayed in Regression 2 in Table 3 can be interpreted as follows: a one percentage point increase in the unemployment rate, from half a unit below the mean to half a unit above, decreases the probability that a state will contribute the full ARC by eight percentage points, holding all other variables fixed at their mean.

[32] Other revenue measures, such as general revenue per capita, were initially included in the model but were excluded because of the high correlation with personal income.

[33] Alternatively, according to Table 3 a one percentage point increase, from half a unit below the mean to half a unit above, decreases the probability that a state will contribute the full ARC by four to six percentage points, holding all other variables fixed at their mean.

negative for the strict withdrawal rule, and negative for the cap. As expected, states that limit the size of their budget stabilization funds are less likely to contribute the full amount of their ARC as the amount of funds available during times of fiscal stress may be insufficient. The coefficient on the five percent cap is statistically significant at the five percent level in Regressions 1-2 in Table 3.2.

Both the dummy variable for a strict deposit rule and the dummy variable for a strict withdrawal rule are statistically significant at the five percent level in Regressions 1-2 in Table 3.2. Although both variables do not attain statistical significance at the five percent level in Regressions 3-4, their coefficients retain their signs and are only slightly decreased in magnitude. Table 3.3 finds the strict deposit rule statistically significant at the five percent level in Regressions 1-2; Regressions 3-4 find that a strict withdrawal rule is statistically significant at the five percent level. According to Table 3.2, a state that adopts a strict deposit rule will see a four to six percentage point increase in the percent of its ARC that it contributes on an annual basis. On the other hand, a strict withdrawal rule is associated with a four to seven percentage point decrease in the percent of the ARC contributed. According to Table 3.3, a state that adopts a strict deposit rule will increase its probability of contributing the full ARC by 15 percentage points, given that all covariates are at their mean. A state that adopts a strict withdrawal rule decreases its probability of contributing at least 90 percent of the ARC by 12-14 percentage points. These results are consistent with the descriptive results in Figure 3.3. States with a strict deposit rule may be more prone to save and thus have consistent access to funds when making pension contributions, even during years of fiscal hardship.

On the other hand, states with strict withdrawal rules may find it difficult to withdraw money, even when the need is justified.

The only other variables in Table 3.3 that attain statistical significance at the five percent level are the amount of real debt per capita, the number of public employees per 100 members of the population, and the percentage of unionized public employees. The amount of debt per capita is positively associated with pension contributions, suggesting that some states prefer to incur hard debt instead of shortchanging their pension funds. The number of public employees is negatively associated with the likelihood of a state making at least 90 percent of its required pension contribution. The coefficient on the percentage of unionized public employees, although extremely small, is positive in all specifications, reflecting the influence of public employees as an interest group.

Table 3.4 presents the results of Blundell-Bond system GMM estimation. As with Table 3.2, Table 3.4 treats the dependent variable as continuous. The results presented in Table 3.4 are largely similar to those presented in Tables 3.2 and 3.3. The unemployment rate and the ARC as a percent of state expenditures are statistically significant and negatively associated with pension contributions, with coefficients that are of approximately equal value to what they were in Table 3.2. The coefficients for personal income per capita and real debt per capita, while not statistically significant in every specification, also do not appear dissimilar from the earlier results.

The coefficients of all three BSF variables retain their signs and are now statistically significant in almost every regression, with magnitudes that in some cases are considerably larger than those in Table 3.2. Imposing a cap of five percent on a budget stabilization fund appears to decrease the percent of the ARC that states contribute by 11-

12 percentage points, according to Regressions 3-4 in Table 3.4. While the coefficients on the strict deposit and withdrawal rules in some cases appear larger in Tables 3.4 than they do in Table 3.2, the signs remain consistent: strict deposit rules appear to increase pension contributions, while strict withdrawal rules appear to decrease pension contributions.

Tables 3.4 also present the results of Hansen overidentification tests. The null hypothesis in the Hansen test is that the instruments employed in the model are exogenous. Thus, a high p-value is desirable, as a rejection of the null hypothesis suggests that the instruments are not valid. The overidentification test is unable able to reject the null of instrument exogeneity at the five percent level in any of the regressions, suggesting that the system GMM instruments are uncorrelated with the error term, conditional on the other covariates.[34]

These results suggest that budget stabilization funds with strict deposit rules assist states in meeting their required pension contributions. By building up their reserves, state legislatures may have funds to draw upon that enable them to maintain contributions even in the face of budget deficits. On the other hand, strict withdrawal rules appear to prevent states from accessing those same funds, thus lowering contribution rates.

3.6 Conclusion

In the past, researchers and policy experts have often recommended budget stabilization funds as a tool that states can utilize to smooth out their revenue flows and

[34] Roodman (2009) points out that the Hansen statistic should not be relied upon too faithfully when it is applied after GMM.

improve their financial management. For instance, McNichol & Boadi (2011) suggest that states build up their BSF balances and revise strict replenishment rules that force states to make deposits before they are ready. Wagner (2004) shows that states that adopt budget stabilization funds witness a reduction in borrowing costs.

However, little is known about how budget stabilization funds interact with other countercyclical tools, and in particular what effect they have on pension funds, states' largest source of investment earnings and long-term liabilities. While budget stabilization funds with strict deposit rules may aid states in keeping up with their pension contributions, strict withdrawal rules appear to hinder them. In this respect, this essay is consistent with other work that demonstrates that the design of fiscal institutions is integral when considering their impact.

OLS results give an indication of the magnitude of these effects. Strict deposit rules are associated with a four to six percentage point increase in the percent of the ARC that states contribute. Strict withdrawal rules are associated with a four to seven percentage point decrease in the percent of the ARC that states contribute. Probit results are also successful at uncovering a statistically significant relationship. Strict deposit rules increase the probability that states will contribute the full amount of their ARC by 15 percentage points, given that all covariates are at their mean. Strict withdrawal rules decrease the probability that states with contribute at least 90 percent of their ARC by 13-14 percentage points, given that all covariates are at their mean.

The positive coefficient on strict deposit rules and the negative coefficient for strict withdrawal rules appear consistent across OLS, probit, and system GMM results. The coefficient on the variable for voter ideology is close to zero. The primary finding

from GMM estimation is that, if anything, OLS results may understate the impact of budget stabilization funds on pension contributions.

The conclusion regarding withdrawal rules is particularly significant as previous work on budget stabilization funds has tended to emphasize the benefits of strict withdrawal rules. Knight and Levinson (1999) show that states with stricter withdrawal provisions tend to save more than states that allow access to balances through appropriation. Wagner (2004) finds that states with strict deposit and withdrawal rules see the greatest reduction in borrowing costs. Rose (2008) finds that requiring gubernatorial approval for the withdrawal of budget stabilization funds may prevent political manipulation of the balances. The emphasis on building up BSF balances and ensuring that they are safe from political misuse has distracted researchers from the fact that strict withdrawal rules may prevent lawmakers from accessing these same funds when the withdrawal is justified, as may be the case with pension contributions, which can be invested in the stock market and earn a return.

In addition to considering one unexpected impact of budget stabilization funds, this essay also raises the question, what is the politically optimal funding plan for pension funds? That is, why would states choose to deliberately underfund their pensions, apart from cost constraints? The GAO and others have suggested that states may choose to target a funding level of 80 percent for their pension funds, given the political dangers of maintaining a high funding level (namely, legislators exploiting excess funds and public-sector unions using the high funding level to argue for higher benefits). This suggests that legislators may behave rationally at times in deciding to not contribute the full ARC

even when funds are available. Future research may assist in developing a better understanding of how this decision-making process occurs.

Table 3.2 OLS Results

VARIABLES	Full Sample		Excluding Outliers	
	(1)	(2)	(3)	(4)
Personal income per capita	-0.0123**	-0.0113*	-0.0108***	-0.00900**
	(0.00510)	(0.00571)	(0.00364)	(0.00429)
Real debt per capita	0.0110	0.00886	0.0125*	0.0103
	(0.00688)	(0.00737)	(0.00737)	(0.00759)
Unemployment rate	-0.0279***	-0.0187*	-0.0244***	-0.0167*
	(0.00867)	(0.0102)	(0.00746)	(0.00958)
ARC as percent of state expenditures	-0.0191	-0.0216	-0.0158	-0.0179
	(0.0141)	(0.0157)	(0.0109)	(0.0116)
Strict balanced budget requirement	0.00399	0.00681	0.0421	0.0510
	(0.0421)	(0.0405)	(0.0352)	(0.0338)
BSF: strict deposit rule	0.0636**	0.0593**	0.0442	0.0412
	(0.0282)	(0.0288)	(0.0286)	(0.0294)
BSF: strict withdrawal rule	-0.0727***	-0.0692***	-0.0482*	-0.0450*
	(0.0254)	(0.0245)	(0.0256)	(0.0256)
BSF: capped at 5% or less of appropriations/revenues	-0.0595**	-0.0608**	-0.0273	-0.0311
	(0.0277)	(0.0285)	(0.0244)	(0.0246)
Number of public employees per 100 population	-0.00288	-0.00143	-0.0112	-0.00975
	(0.00929)	(0.00987)	(0.00917)	(0.00898)
Percentage of unionized public employees	-4.87e-05	-0.000129	0.000783	0.000456
	(0.000889)	(0.000875)	(0.000853)	(0.000895)
Democratic-controlled government	-0.0179	-0.0257	-0.0114	-0.0133
	(0.0304)	(0.0299)	(0.0244)	(0.0238)
Republican-controlled government	-0.0149	-0.0123	-0.0349	-0.0290
	(0.0415)	(0.0441)	(0.0273)	(0.0292)
Citizen ideology	0.000291	0.000256	0.000230	0.000619
	(0.00112)	(0.00115)	(0.000990)	(0.00107)
Lagged funding level	0.0637	-0.00111	0.124	0.0569
	(0.129)	(0.126)	(0.102)	(0.108)
Percentage of contributions from local governments	0.0322	0.0261	0.0509	0.0499
	(0.0513)	(0.0506)	(0.0523)	(0.0522)
Lagged dependent variable	0.378***	0.376***	0.236***	0.230***
	(0.0661)	(0.0644)	(0.0738)	(0.0725)
Year dummies	No	Yes	No	Yes
Number of observations	513	513	501	501
R-squared	0.271	0.288	0.324	0.344

*** p<0.01, ** p<0.05, * p<0.1 Standard errors are robust and clustered by state. Outliers are defined as state-years in which a state contributed less than 25% of the ARC or more than 200%.

Table 3.3 Probit Results

VARIABLES	P(% Contributed ≥ 1.0)		P(% Contributed 0.9)	
	(1)	(2)	(3)	(4)
Personal income per capita	-0.0318***	-0.0323***	-0.0215***	-0.0202**
	(0.00855)	(0.00979)	(0.00695)	(0.00810)
Real debt per capita	0.0373*	0.0362*	0.0428***	0.0424***
	(0.0207)	(0.0220)	(0.0158)	(0.0163)
Unemployment rate	-0.0912***	-0.0791**	-0.0610***	-0.0449*
	(0.0247)	(0.0310)	(0.0171)	(0.0244)
ARC as percent of state expenditures	-0.0437*	-0.0591**	-0.0142	-0.0224
	(0.0227)	(0.0276)	(0.0188)	(0.0221)
Strict balanced budget requirement	0.138*	0.144*	0.0459	0.0509
	(0.0753)	(0.0823)	(0.0753)	(0.0715)
BSF: strict deposit rule	0.151**	0.145**	0.0656	0.0553
	(0.0651)	(0.0678)	(0.0534)	(0.0522)
BSF: strict withdrawal rule	-0.132*	-0.132	-0.126**	-0.143**
	(0.0802)	(0.0818)	(0.0592)	(0.0594)
BSF: capped at 5% or less of appropriations/revenues	-0.0515	-0.0558	-0.0125	-0.0339
	(0.0614)	(0.0627)	(0.0448)	(0.0442)
Number of public employees per 100 population	-0.0248	-0.0281	-0.0351**	-0.0376**
	(0.0208)	(0.0217)	(0.0143)	(0.0147)
Percentage of unionized public employees	0.00477**	0.00499**	0.000292	-0.000573
	(0.00234)	(0.00250)	(0.00209)	(0.00215)
Democratic-controlled government	0.0698	0.0720	-0.0301	-0.0673
	(0.0831)	(0.0902)	(0.0607)	(0.0605)
Republican-controlled government	0.0113	0.0175	-0.0352	-0.0394
	(0.0777)	(0.0768)	(0.0643)	(0.0667)
Citizen ideology	0.000212	-0.000978	0.000918	0.00164
	(0.00264)	(0.00289)	(0.00237)	(0.00248)
Lagged funding level	0.264	0.168	0.297	0.193
	(0.265)	(0.292)	(0.208)	(0.233)
Percentage of contributions from local governments	0.0309	0.0128	0.0499	0.0342
	(0.110)	(0.114)	(0.0949)	(0.0945)
Lagged dependent variable	0.644***	0.679***	0.727***	0.749***
	(0.0473)	(0.0448)	(0.0455)	(0.0485)
Year dummies	No	Yes	No	Yes
Number of observations	513	513	501	501

*** p<0.01, ** p<0.05, * p<0.1 Standard errors are robust and clustered by state. Coefficients represent marginal effects.

Table 3.4 Blundell-Bond System GMM Results

VARIABLES	Full Sample		Excluding Outliers	
	(1)	(2)	(3)	(4)
Personal income per capita	-0.0135	-0.0114*	-0.00991***	-0.00674
	(0.0137)	(0.00590)	(0.00348)	(0.00449)
Real debt per capita	0.0302**	0.00280	0.00868	1.78e-05
	(0.0150)	(0.0121)	(0.00533)	(0.00794)
Unemployment rate	-0.0281**	-0.0291**	-0.0209***	-0.0136
	(0.0137)	(0.0120)	(0.00673)	(0.0110)
ARC as percent of state expenditures#	-0.0352	-0.0570**	-0.0628***	-0.0657***
	(0.0285)	(0.0237)	(0.0121)	(0.0172)
Strict balanced budget requirement#	0.0825	0.154**	0.0925**	0.148***
	(0.122)	(0.0732)	(0.0409)	(0.0422)
BSF: strict deposit rule#	0.149	0.216***	0.0748**	0.117**
	(0.118)	(0.0572)	(0.0344)	(0.0497)
BSF: strict withdrawal rule#	-0.209**	-0.154**	-0.112***	-0.143***
	(0.0959)	(0.0630)	(0.0376)	(0.0473)
BSF: capped at 5% or less of appropriations/revenues#	-0.174**	-0.167***	-0.116***	-0.114**
	(0.0712)	(0.0523)	(0.0349)	(0.0476)
Number of public employees per 100 population	-0.00881	-0.00509	-0.0118	-0.00434
	(0.0181)	(0.00868)	(0.00896)	(0.00980)
Percentage of unionized public employees	-0.000768	0.00114	-0.000484	0.000585
	(0.00182)	(0.00115)	(0.000783)	(0.00102)
Democratic-controlled government	-0.0206	-0.0249	-0.00958	-0.0273
	(0.0379)	(0.0201)	(0.0133)	(0.0203)
Republican-controlled government	-0.0181	-0.0198	-0.0185	-0.0127
	(0.0545)	(0.0259)	(0.0215)	(0.0198)
Citizen ideology	0.00129	0.00127	0.00139**	0.00143
	(0.00155)	(0.00129)	(0.000608)	(0.000919)
Lagged funding level#	0.482*	-0.0395	0.197**	0.100
	(0.248)	(0.273)	(0.0888)	(0.197)
Percentage of contributions from local governments	-0.0195	-0.0199	0.00966	-0.0133
	(0.0654)	(0.0383)	(0.0310)	(0.0421)
Lagged dependent variable#	0.118	0.145***	0.0973***	0.129***
	(0.226)	(0.0349)	(0.0282)	(0.0367)
Year dummies	No	Yes	No	Yes
Number of observations	513	513	501	501
Hansen Test of Overidentifying Restrictions (H_0: instrument exogeneity)	25.55	22.11	30.27	22.70
P-value of Hansen Test	0.598	0.776	0.302	0.701

*** p<0.01, ** p<0.05, * p<0.1 Standard errors are robust to panel-specific autocorrelation and heteroskedasticity. Outliers are defined as state-years in which a state contributed less than 25% of the ARC or more than 200%. # indicates an endogenous variable.

Appendix

Table 3.5 State BSF Categories, 1997-2008

Weak Deposit Rules and Weak Withdrawal Rules (23)	*Strict Deposit Rules and Strict Withdrawal Rules (5)*	*Strict Deposit Rules and Weak Withdrawal Rules (7)*	*Weak Deposit Rules and Strict Withdrawal Rules (3)*	*Switched from One Category to Another (9)*	*No BSF (3)*
Alabama	Arizona	Florida	Delaware		
Alaska	Indiana	Idaho	Louisiana	Hawaii (None to W/S)	Arkansas
Colorado	Michigan	Maryland	Pennsylvania	Illinois (None to S/W)	Kansas
Connecticut	Texas	New York		Oregon (None to S/S)	Montana
Georgia	Virginia	Rhode Island		California (W/W to S/W)	
Iowa		South Carolina		Massachusetts (W/W to S/W)	
Kentucky		Tennessee		West Virginia (W/W to S/W)	
Maine				South Dakota (W/W to W/S)	
Minnesota				Missouri (W/W to S/S)	
Mississippi				Washington (W/S to S/W)	
Nebraska					
Nevada					
New Hampshire					
New Jersey					
New Mexico					
North Carolina					
North Dakota					
Ohio					
Oklahoma					
Utah					
Vermont					
Wisconsin					
Wyoming					

Chapter 4 Procedural Debt Restrictions and California's Proposition 39

4.1 Introduction

While many restrictions on taxes and expenditures were established during the Tax Revolt of the 1970s, limitations on bonded indebtedness have been a part of state constitutions for far longer, appearing as early as the 1840s (Wallis & Rodriguez Tejedo, 2010). As of 1996, there were only five states without constitutional debt limitations (Kiewiet & Szakaly, 1996). Moreover, there is considerable variety in the type and stringency of debt limitations that exist, with some pertaining only to full faith and credit debt and others applicable to both guaranteed and non-guaranteed debt.

As with other fiscal constraints, the most commonly asked question regarding debt limitations is, do they actually succeed? That is, do they reduce the amount of debt that a state or local government issues? Researchers have employed a variety of methods to answer this question, typically concluding that while debt limits are not generally successful, the large number of different limitations defies easy categorization and that limits may be effective in certain contexts (Bahl & Duncombe, 1993; Ellis & Schansberg, 1999; Kiewiet & Szakaly, 1996).

Complicating these efforts is the lack of a clear terminology around debt issuance, as constraints may come in a variety of forms. Tejedo (2007) distinguishes between debt limits and debt restrictions, referring to debt limits as restrictions on the absolute amount of debt allowed or on the amount determined by a relative measure (eg. percent of assessed value). Procedural debt restrictions are restrictions on the process that is used to

authorize the debt and can come in the form of a requirement for voter approval or a legislative majority. In many cases, debt limits are coupled with procedural restrictions.

While most of the research on debt constraints focuses on debt limits, there is less known about the impact of procedural debt restrictions. Most of the work on procedural requirements focuses on understanding the level of voters' preferences for certain public goods. For instance, researchers have looked to referenda outcomes as estimates for the demand for public capital, particularly in the case of education spending (Balsdon, Brunner, & Rueben, 2003; DeBartolo & Fortune, 1982; Zimmer, Buddin, Jones, & Liu, 2011). However, it is perhaps surprising that more work has not been done to examine the effect that procedural debt restrictions have the amount of debt that is issued by local governments, as the level of borrowing, not bond election results, is what ultimately matters to districts' fiscal condition.

This essay exploits two developments in the municipal bond market to develop further insight into procedural debt restrictions. In 2000, California passed Proposition 39, a constitutional amendment that modified the normal referendum requirement of two-thirds voter approval for bond issues; so long as they meet certain requirements, school districts require only 55 percent of the local vote in order to issue a general obligation bond. Less than a decade later, in 2009, Build America Bonds were introduced as part of the stimulus program, enabling state and local borrowers to issue debt at lower yields (U.S. Treasury Department, 2011).

This essay examines a panel of municipal debt issuance in California to investigate whether the lower supermajority requirement that school districts faced as a result of Proposition 39 caused them to 1) issue more new debt, and 2) respond more

aggressively to the Build America Bond program. Although one might assume that a lower vote requirement would necessarily impart greater flexibility to local governments, enabling them to issue debt at higher rates and react more swiftly to changing credit conditions, the literature on fiscal institutions suggests that institutions affecting spending and debt are often irrelevant to fiscal outcomes, particularly if they are not well-targeted. Nonetheless, what makes Proposition 39 particularly apt for study is that it was a constitutional amendment that restricted the debt process rather than a limit on the total amount of debt. If procedural debt restrictions are in fact effective at reining in new borrowing, this would suggest an alternative and perhaps preferable approach to limiting debt than the more common limitation on the absolute amount of debt outstanding.

The data come from the California Debt and Investment Advisory Commission (CDIAC) and consist of all public debt issued in the state since 1984. Results suggest that Proposition 39 had no impact on either the amount of new debt that school districts issued or on the elasticity of school debt with respect to changing credit conditions.

4.2 Determinants of State and Local Government Debt Levels

Research on state and local government debt has uncovered a variety of factors that determine the level of government indebtedness. Primary among them are social and demographic factors that reflect the need to borrow as well as financial characteristics that reflect the ability of governments to repay the debt. Clingermayer and Wood (1995) found that, in addition to economic conditions, political factors such as electoral cycles and partisan competition also affect the level of state debt. Ellis and Schansberg (1999) concluded that age group effects and income growth are particularly important to state

debt financing decisions, with an increase in the elderly population associated with a decrease in debt financing and an increase in the youth population associated with an increase in debt financing. Wang, Hou, and Duncombe (2007) found that the decision to fund capital projects through pay-as-you-go financing, rather than through borrowing, is determined by a state's income level, as well as economic conditions, budgetary institutions, and political factors.

While the above-mentioned articles focus on the overall determinants of debt financing, much of the debt literature tends to focus on one particular facet of the debt process. For instance, research on local government debt issuance has often used referenda outcomes to estimate the demand for public capital. DeBartolo and Fortune (1982) estimated that the tax-price elasticity of demand for local government services is in the range of 0.10-0.15. They also found that renters and more-educated voters are more likely to support an expansion of services than home-owners and less-educated voters. Many models that incorporate data on bond referenda focus on school spending, as education spending is quite often the main reason for local government borrowing. Balsdon, Brunner, and Rueben (2003) found that communities with lower taxes and higher incomes are more likely to hold bond referenda. Zimmer, Buddin, Jones, and Liu (2011) contended that the type of project matters; funding for the maintenance of existing facilities has a higher approval rate than funding for new construction. Similarly, Rueben and Cerdan (2003) found that fiscal measures involving transportation and emergency services are more likely to pass than measures relating to parks and recreation. They also found that ballot measures proposed by special districts passed at a higher rate than city or county measures.

Other work has examined specifically the ability of debt limitations to effectively constrain debt, though most of this work focuses on debt limits (as opposed to procedural restrictions) at the state level. In order to properly answer this question, researchers first had to decide on the debt ratio that was most appropriate for them to measure. Aronson and Hilley (1986) wrote that limitations on state debt financing may simply shift the debt from full faith and credit debt to non-guaranteed debt. Using cross-sectional data on state and local debt and controlling for ideological and economic factors, David Nice confirmed these early hypotheses, finding that debt limits are associated with lower levels of full faith and credit debt, but that debt limits and balanced budget requirements are unrelated to total debt levels (Nice, 1991).

Bahl and Duncombe (1993) examined state and local debt for the 1988-1990 period. Unlike some of the prior research, they found that total debt is lower in states with statutory or constitutional debt limits. However, one distinguishing feature of their study is that they categorized debt limits according to their type and level of stringency. As expected, the more stringent limitations are more effective at constraining the amount of debt. Although focused only on constitutional debt limits, Kiewiet and Szakaly (1996) reached similar conclusions as Bahl and Duncombe, using panel data on total debt outstanding to find that some debt limits do indeed limit the issuance of debt. This contradicted many earlier studies that concluded that constitutional limitations do not restrict long-term bonded indebtedness. Ellis and Schansberg followed up on the work of Kiewiet and Szakaly by using another large data set, with 50 states and as many as 25 years of observations (Ellis & Schansberg, 1999). Their findings agreed with those of Kiewiet and Szakaly; the strictest debt limitations are effective at limiting debt.

As debt limits have become more numerous and in some cases more restrictive, states and localities have devised various means of circumventing those limits. Perhaps the most common method is by creating public authorities that can serve as a financial intermediary to issue revenue bonds. Using cross sectional data from the 80s, Bunch (1991) looked at the effect of constitutional debt limits on the number of state authorities as well as the scope of the functions addressed by them. Her results support the claim that governments use public authorities to circumvent limits on debt issuance. Debt limits have also given rise to a range of innovative debt instruments that officials have used to avoid restrictions. As Hildreth & Zorn (2005, p. 133) wrote, "Over the past 25 years, issuers have shown a stead preference for customizing their solution to a specific financing need rather than relying solely on a long-term, fixed-coupon general obligation (GO) bond. Creative ways to avoid debt limits and procedural hurdles have propelled the growth of lease financing, including Certificates of Participation (COPs), and the increased use of tax increment financing (TIF), bond banks, and revolving loan funds."

While there has been considerable work on debt limits, fewer researchers have examined procedural debt restrictions at the local level. Farnham (1985) disputed findings that state-imposed debt limits do not depress local debt levels, however he found that bond referenda requirements do not affect the amount of local debt. Harris and Munley (2011) also found that referenda requirements for bond issues do not have an effect, though they did find that referenda requirements for the approval of annual school district budgets raise borrowing costs.

In addition to affecting overall debt levels, institutional constraints may affect the ability of local governments to respond to changing credit conditions. One of the first to

write on the topic was Netzer (1960, p 228), who contended that "the major difficulty [with the borrowing process] is the slowing of the state-local sector's reaction time; in numerous instances the time lag between official recognition of the need for borrowing and the actual sale of securities has been as much as five years" (228). Peterson (1971) provided descriptive results from survey data to demonstrate that state and local governments do respond to adverse credit conditions by delaying or decreasing their borrowing and pointed out that many governments are limited in their ability to use short-term financing for capital expenditures. Hildreth (1993) also pointed out that the institutional hurdles governments face mean that they encounter a lag time between the decision to borrow and the actual date of borrowing, which can put them at a disadvantage in responding to market conditions.

Tanzer (1964) examined state and local debt separately and concluded that local government debt is less interest elastic than state government debt. Unlike research on debt limits that looks at the level of debt outstanding, he focused on the amount of new debt, regressing the amount of new local government debt (and, separately, state government debt) in year t on an index of interest rate changes in the same year. In support of his conclusion, he offered several possible explanations: 1) local governments lack the services of skilled market participants; 2) local governments have a smaller proportion of liquid assets to expenditures and therefore often can't afford to postpone debt issues; 3) new debt issues are relatively infrequent for smaller local government entities and, as they cover unusual expenditures, can't be postponed as easily; 4) a smaller proportion of local government debt issues are in the form of revenue bonds, which at least in theory, should be more interest elastic.

The next section examines the recent history of public finance in California and presents descriptive findings regarding the extent to which procedural debt restrictions impact the level of municipal borrowing. Section 4.4 examines the relative responsiveness of school districts to changes in credit conditions.

4.3 Municipal Debt in California

Various features of California's debt landscape make the state an attractive one to study in the context of procedural debt restrictions. According to Article 16, Section 18 of the California state constitution, "No county, city, town, township, board of education, or school district, shall incur any indebtedness … without the assent of two-thirds of the voters of the public entity voting at an election to be held for that purpose." However, in 2000, voters in the state approved Proposition 39, which amended the supermajority requirement for school bond issues from 2/3 of voters to 55 percent. The difference in procedural requirements between school districts and other types of local government issuers raises the following questions, which this essay will attempt to answer: did Proposition 39 cause school districts to issue more debt relative to other local governments than they used to? Are school districts able to respond more effectively to changes in credit conditions as a result of the less restrictive hurdle that they must overcome?

An understanding of California's current debt policies requires revisiting its recent history of public finance, which began with Proposition 13 in 1978. An instrument of property tax relief, Proposition 13 limited the property tax rate to 1 percent and capped increases in assessed values. By limiting property tax rates to 1 percent, the amendment

had the effect of preventing any new issuance of general obligation bonds. Local agencies that issue general obligation bonds do so by pledging to levy property taxes as necessary to support the debt service. With a cap on the amount that they could raise through property taxes, local governments in California were essentially denied the ability to issue general obligation bonds.

In 1982, the state legislature enacted the Communities Facility District Act (commonly referred to as Mello-Roos), which enabled local governments to establish "Community Facilities Districts" (CFDs) which could issue bonds for the purpose of funding public improvements. By allowing local agencies to levy special taxes in support of these bonds (provided that a two-thirds vote was obtained), the act was essentially a means of circumventing Proposition 13. The restriction on borrowing was further lifted in 1986 with the approval of Proposition 46, which allowed the issuance of general obligation bonds only for the acquisition or improvement of real property, provided that the necessary two-thirds voter approval was secured.

As a result of the limitation on property tax revenue imposed by Proposition 13, local governments had initially turned to revenue bonds, as fees and charges were exempted from the two-thirds voter requirement. Proposition 218, passed in 1996, imposed additional procedural limitations on these other revenue sources, ensuring that all taxes, and not just property taxes, were subject to voter approval.

Between 1986 and 2000, voters in local K-12 jurisdictions had $17 billion worth of new bond issues. However, over the same time period, approximately $13 billion worth of bonds received more than 55 percent of the vote but less than the necessary 2/3 and were therefore defeated (California Legislative Analyst's Office, 2000). In response

to the difficulties that schools were having issuing debt, supporters succeeded in putting Proposition 39 on the ballot, which passed in the 2000 general election, lowering the voting requirements for the passage of local school bond referenda from two-thirds to 55 percent. Once again, a corollary of the amendment was that governments could raise property taxes above the 1 percent ceiling in order to repay the bonds. Several reports in the years since have documented the increase in school borrowing that followed the passage of Proposition 39 (Brunner, 2007; Public Policy Institute of California, 2005); over the next four years, school districts passed more than 250 bonds worth $20 billion, approximately half of which would not have met the 2/3 vote requirement (Public Policy Institute of California, 2005).[35] The inference that Proposition 39 increased school borrowing was supported by descriptive statistics on the passage rate of local general obligation bonds measures. However, no study to date has examined the actual debt levels of schools and other municipal borrowers or considered other economic factors that may have come into play.

A more exhaustive review of the legal restrictions on debt issuance in California is beyond the scope of this essay.[36] However, it is important to note that not all bond issues are subject to voter approval. For instance, cities and counties can issue public enterprise revenue bonds to finance public facilities that will be able to generate revenue independently. Revenue bonds issued for airports, water supply, garbage collections, and

[35] Not all school districts are willing to adhere to the restrictions that are necessary for the lower voting threshold to apply. Among other requirements that come with the 55 percent threshold include the appointment of a citizens oversight committee and a more limited selection of election dates. In 2005, a year in which there were only 35 bond measures proposed, 74 percent of school bond elections faced the lower threshold. In 2006, a year with a much greater number of viable election dates, there were 109 measures and the percentage facing the lower threshold rose to 96 percent (Cellini, Ferreira, & Rothstein, 2010).

[36] For a more complete review, see California's Debt Issuance Primer at www.treasurer.ca.gov/cdiac.

other revenue-generating facilities are not subject to voter approval as they are not paid for from taxes or the general fund. Instead, they are supported through a special fund consisting of service charges and fees collected by the facility being financed.

Similarly, local governments avoided many of the restrictions imposed on them by Proposition 13 by issuing debt in the form of lease financing agreements such as certificates of participation. Although essentially equivalent to other forms of debt financing, certificates of participation are not legally considered debt due to the terms of the agreement, and as a result are not subject to debt restrictions of any kind.

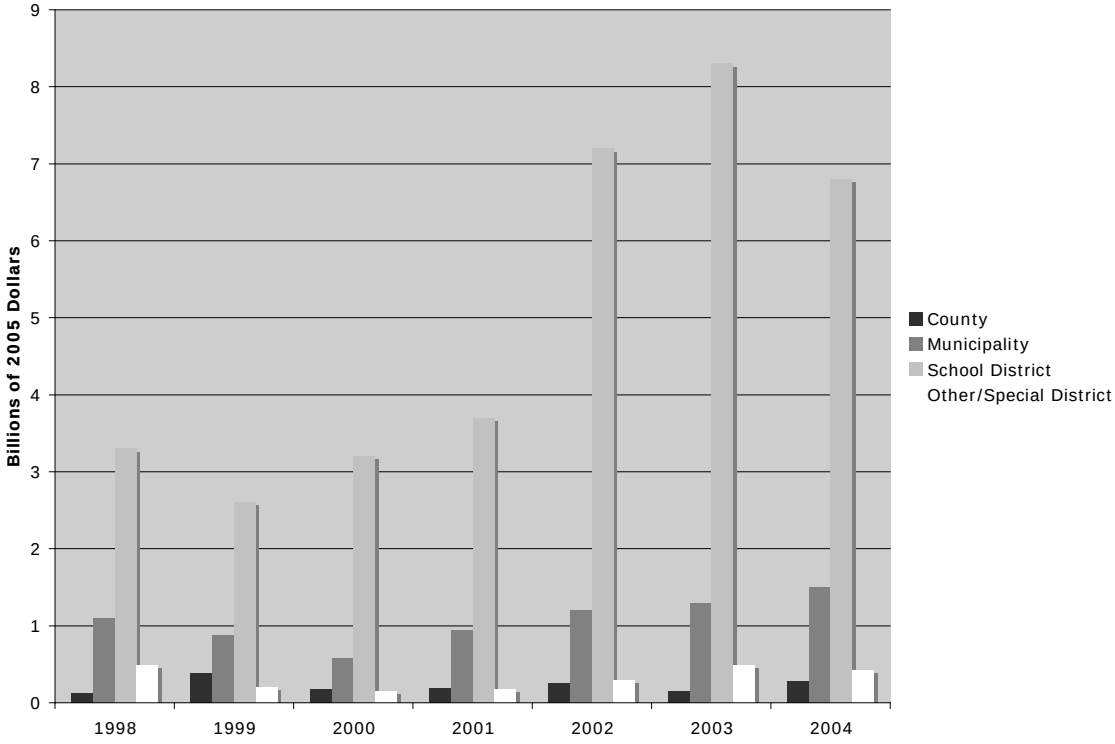
This essay focuses on general obligation bonds (including limited tax general obligation bonds[37]) because of their requirement for supermajority approval. General obligation bonds are the preferred form of financing for municipal governments and carry better terms than revenue bonds since they are backed by the full taxing power of the district rather than a particular revenue stream.

Figure 4.1 illustrates the amount of general obligation debt that was issued by local governments in California between 1998 and 2004.[38] The figure clearly shows that school borrowing did increase after 2000, echoing the results of Brunner (2007) and the Public Policy Institute of California (2005). However, it appears that borrowing on the part of municipalities increased over the same time period, calling into question whether Proposition 39 was in fact responsible for the increase in borrowing or whether other economic factors were involved.

[37] Limited tax general obligation bonds are equivalent to general obligation bonds except that there is a limit placed on the amount of tax increases that will be levied in the case that other revenues prove insufficient to service the bond. In this essay statistics on general obligation bonds include data on limited tax general obligation bonds.

[38] In 1998, California enacted the School Facility Program (SFP), through which the state provides financial support for new school construction. Since SFP changed somewhat the conditions under which schools would borrow for capital needs, the analysis on the following pages does not consider years prior to 1998.

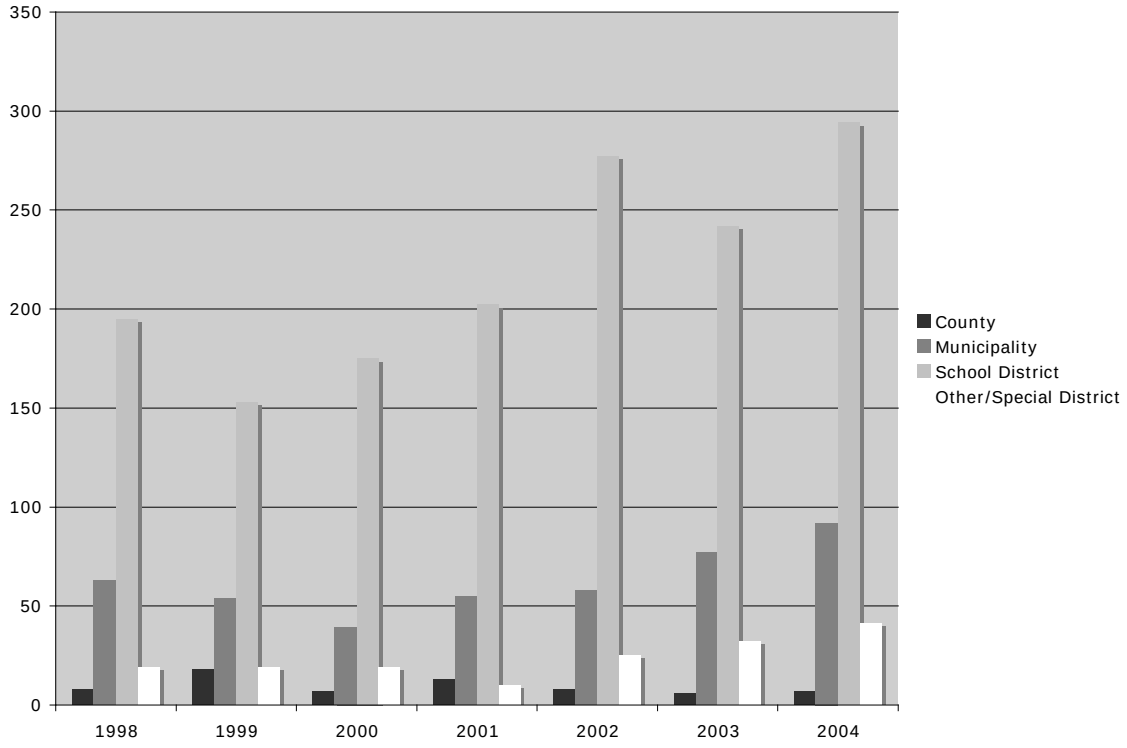
Figure 4.1 General Obligation Debt Issued by Local Governments in California, 1998-2004



Source: CDIAC and author's calculations

Figure 4.2 shows the number of bonds that were issued. It too shows an increase in borrowing, not just for school districts, but for municipalities and special districts as well. Given the trends illustrated by Figure 4.1 and 4.2, and the fact that all municipal debt appears to have increased between 2001-2004, it is not at all clear that the rise in school borrowing during this time period can be attributed to Proposition 39 alone. Section 4.5 explores this question further through regression analysis.

Figure 4.2 Number of General Obligation Bonds Issued by Local Governments in California, 1998-2004



Source: CDIAC and author's calculations

4.4 Build America Bonds

That procedural debt restrictions may have an impact on overall debt levels seems relatively straightforward. However, equally important to the financial condition of local governments are the terms under which the borrowing occurs. This raises a second question of interest: do governments with less restrictive procedural hurdles adapt to changing market conditions more quickly? That is, do procedural constraints limit the timing and flexibility of borrowing in addition to the overall amount of debt? In 2009, as part of the stimulus program, the Obama administration introduced the Build America Bonds Program in order to stimulate municipal borrowing during the economic

downturn. This essay exploits the fact that the program appeared suddenly and was only available to issuers for a limited period of time. In doing so, it casts light on whether localities that face more lenient procedural debt restrictions are more likely to take advantage of favorable short-term opportunities such as the one that Build America bonds represented. This section provides an overview of Build America Bonds and the effect they had on the municipal bond market.

Numerous features of the municipal bond market distinguish it from the corporate bond market. Primary among them is the fact that the federal government has traditionally subsidized state and local borrowing by exempting municipal bond yields from the federal income tax. However, the exemption is generally considered to be inefficient, as the revenue cost exceeds the subsidy provided to state and local governments by a substantial amount.

The reason for the inefficiency is as follows. Purchasers of tax-exempt bonds only invest in the bonds if the after-tax yield they can obtain on the bonds is comparable to the yields of taxable bonds. The interest rates set by the issuer then ultimately depend on the income tax rate of the marginal (market-clearing) bond buyer. Because of the progressive rate structure of the U.S. federal income tax, the market-clearing investor receives less savings from the exemption than does the investor paying the highest marginal tax rates; in other words, investors in the highest tax bracket receive interest rates that are greater than what they would have needed to receive in order to purchase the bonds. This differential means that the savings that accrue to state and local governments are less then the amount of revenue that is foregone by the federal government. One estimate suggests that about 80 percent of the tax expenditure from

tax-exempt bonds results in savings for states and localities, while the other 20 percent is essentially a transfer to high-income investors (Congressional Budget Office and Joint Committee on Taxation, 2009).

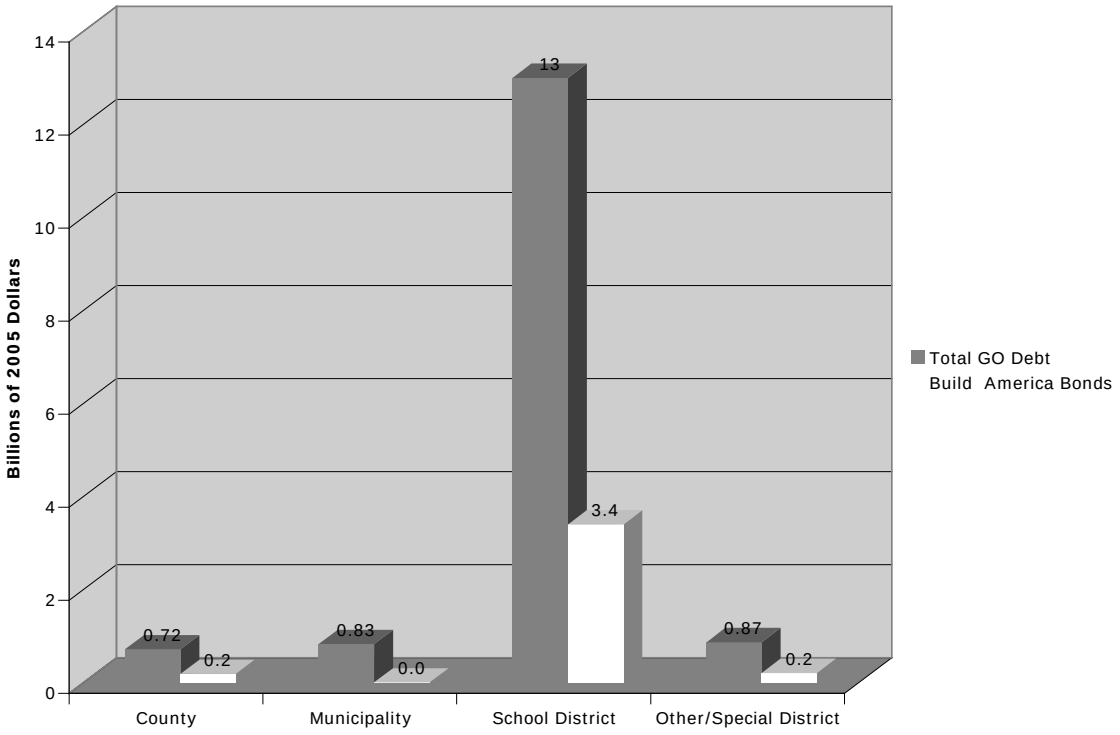
There exist two alternative ways for the federal government to subsidize state and local borrowing: tax-credit bonds, and the taxable bond option. The purchaser of a tax-credit bond receives a credit against their federal tax liability instead of – or in addition to – the cash interest that they usually receive on bonds. The taxable bond option has the federal government subsidize state and local borrowing through the appropriations process in the form of direct subsidies rather than through the tax system. The American Recovery and Reinvestment Act of 2009 (ARRA) allowed state and local governments to replace the interest exclusion with a subsidy equal to 35 percent of each interest payment made under bonds issued in 2009 and 2010. These bonds, called Build America Bonds (BABs), expired at the end of 2010.

There were two kinds of Build America Bonds: Tax Credit BABs and Direct Payment BABs. The difference stems from the fact that the issuer had the option to receive a direct payment from the federal government or give a tax credit to the investor equal to 35 percent of the interest payments on the bond. Direct payment BABs were modeled after the taxable bond option, whereas tax credit BABs were modeled after tax credit bonds. Direct Payment BABs were much more common than Tax Credit BABs as only investors with U.S. taxable income were able to take advantage of the credit, which is not the case for pension funds and many foreign investors (Ang, Bhansali, & Xing, 2010).

In either case, Build America Bonds were a good option for issuers and investors as long as the market-clearing interest rate was lower than the subsidy rate on Build America Bonds of 35%. Indeed, a report by the Treasury Department in May of 2011 estimated savings on the yield for a 30 year bond at 84 basis points (U.S. Treasury Department, 2011). What's more, there is evidence that a great number of states and municipalities rushed to take advantage of the program once it appeared that the subsidies would not be continued (Lutz, 2010).

Hence, the Build America Bonds program can be looked upon as a favorable borrowing opportunity for state and local governments. Figure 4.3 illustrates the total amount of general obligation debt that was issued in California between April 2009 and December 2010, the time period during which the Build America Bonds Program was in effect. Alongside these figures is the amount of Build America Bonds that were issued. School districts issued the largest amount of debt at $13 billion, while counties and municipalities issued relatively paltry amounts in comparison at $720 million and $830 million respectively. For school districts alone, Build America Bonds represented 26 percent of the total debt. For all other types of governments, Build America Bonds represented 17 percent of the total debt.

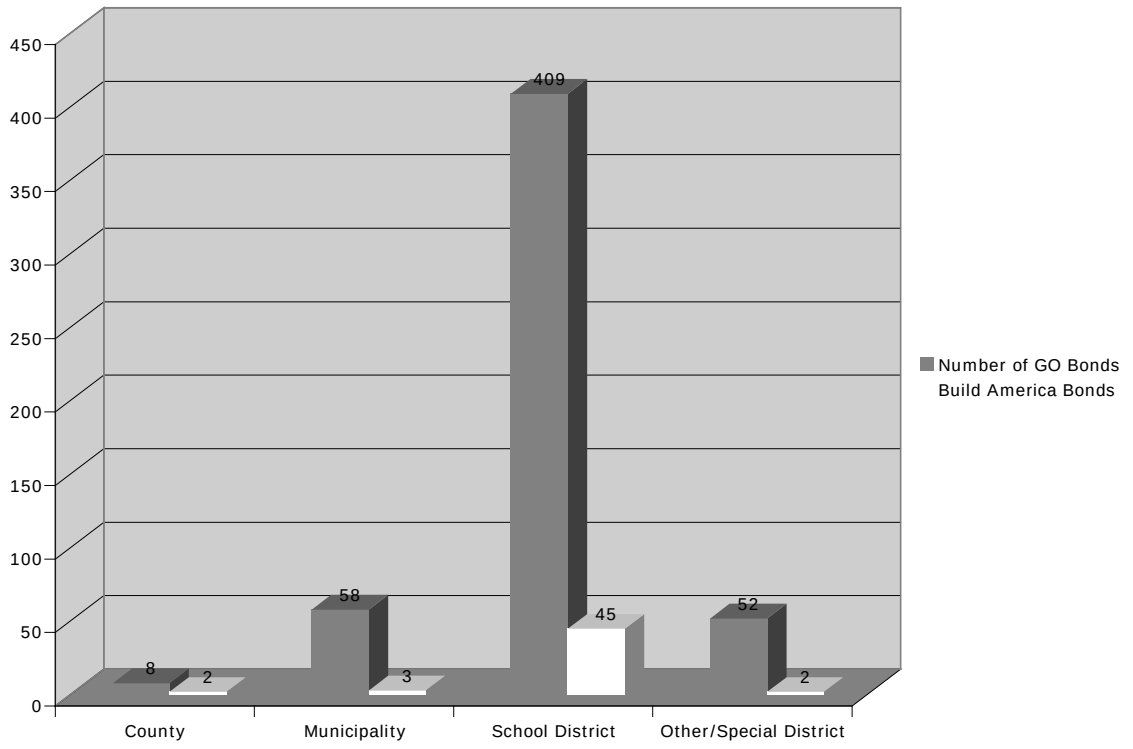
Figure 4.3 Total Amount of General Obligation Debt Issued by Local Governments in California, April 2009 – December 2010



Source: CDIAC and author's calculations.

Figure 4.4 illustrates the total number of bonds issued. These results largely mirror those of Figure 4.3. School districts issued 409 general obligation bonds, compared with 58 for municipalities, 52 for counties, and 8 for counties. Of the 52 Build America Bonds that were issued as general obligation debt, school districts issued 45.

Figure 4.4 Total Number of General Obligation Bonds Issued by Local Governments in California, April 2009 – December 2010



Source: CDIAC and author's calculations.

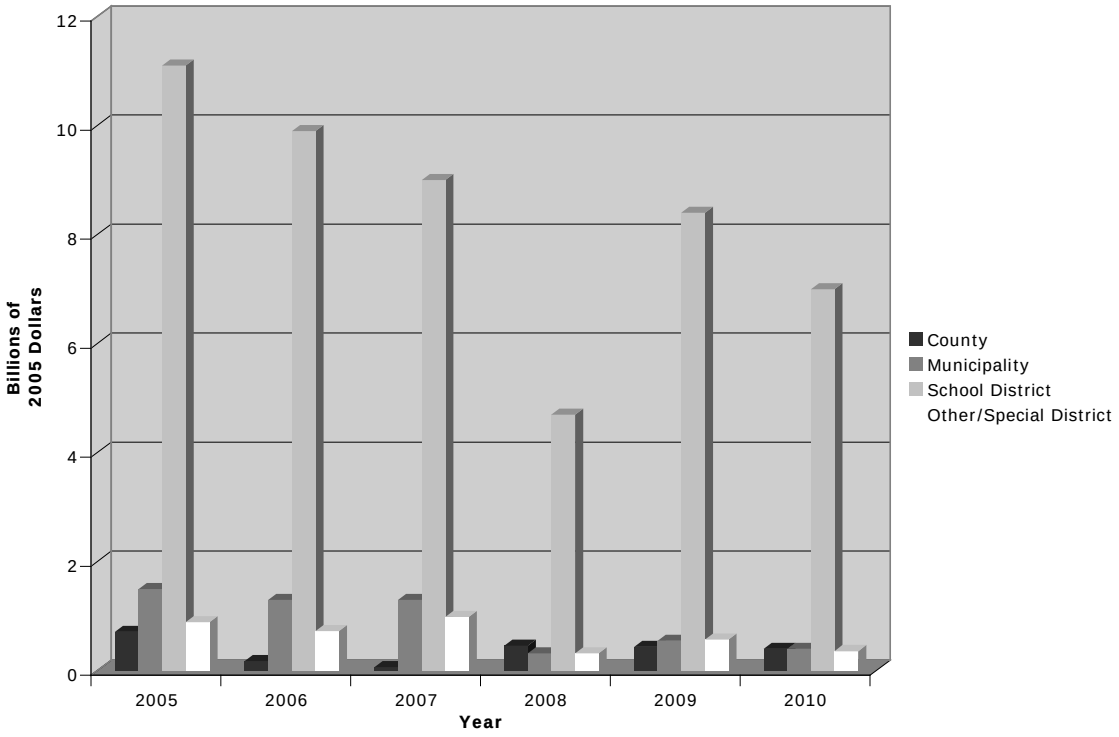
Figure 4.5 illustrates the total amount of general obligation debt issued by local governments between 2005 and 2010.[39] Once again, the majority of the debt issued was by school districts, suggesting that the disparity in debt issuance demonstrated in Figures 4.3 and 4.4 may simply be a function of different capital financing needs and may not necessarily indicate any particular advantage gained by school districts due to their lower procedural hurdles. Between 2005 and 2007, school borrowing showed a steady decrease before a considerable drop-off in activity in 2008. This decrease may reflect the beginning of the national recession and the resultant financial uncertainty faced by state

[39] Because many of the demographic variables used in the analysis in the following section are only available on an annual basis, the rest of this essay will treat 2009 is the starting point for the Build America Bonds program, even though governments only began to issue Build America Bonds in April of that year.

and local governments nationwide. However, debt issued by county governments does not show the same pattern.

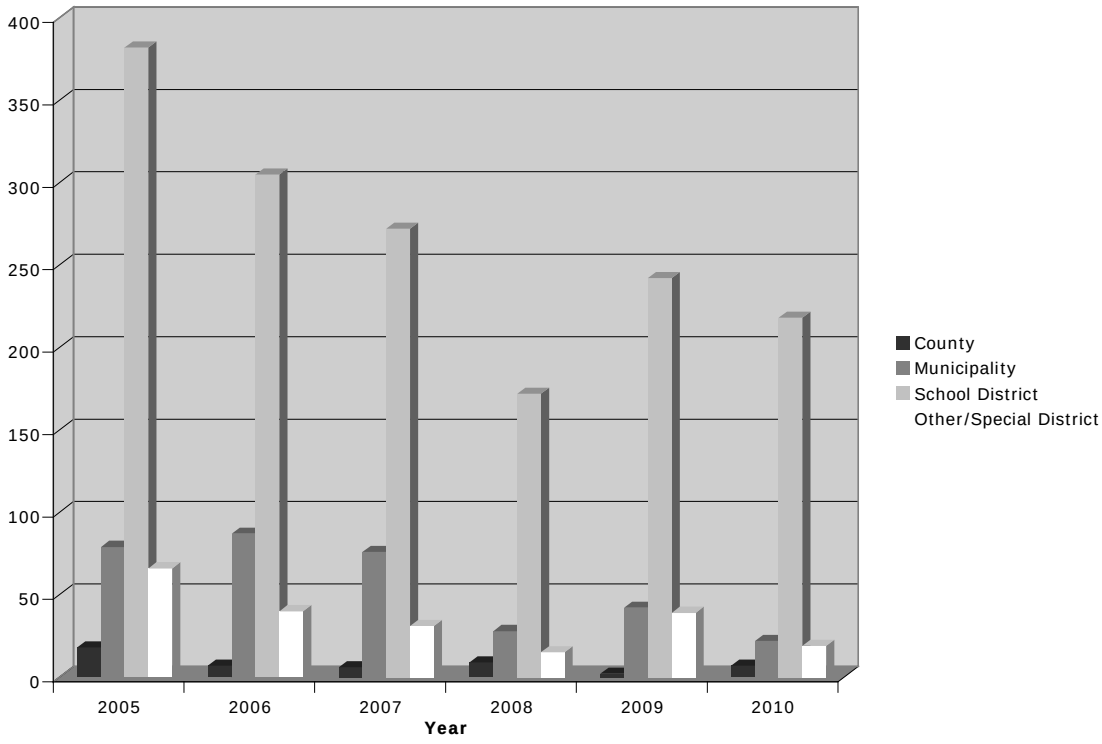
Figure 4.6 illustrates the total number of general obligation bonds issued by local governments and generally mirrors the results of Figure 4.5. The number of bonds issued by school districts appears to decline steadily between 2005 and 2008. Municipalities and special districts show similar patterns.

Figure 4.5 Amount of General Obligation Debt Issued by Local Governments in California, 2005 - 2010



Source: CDIAC and author's calculations.

Figure 4.6 Number of General Obligation Bonds Issued by Local Governments in California, 2005-2010



Source: CDIAC and author's calculations.

4.5 Data & Analysis

The descriptive findings presented in section 4.3 and 4.4 are intended to examine the effect of procedural debt restrictions on the ability of local governments to 1) issue new debt, and 2) respond to changing credit conditions. While it appears that school districts issued more debt following the passage of Proposition 39 in 2000 (Figures 4.1 and 4.2), other local governments also increased their debt issuance between 2001 and 2004, thus casting some doubt on whether this increase can in fact be attributed to the lower procedural requirement. Similarly, while school districts took advantage of the Build America Bonds program to a greater degree than other local governments did, as

evidenced by Figures 4.3 and 4.4, Figures 4.5 and 4.6 indicate that this finding is not necessarily inconsistent with prior patterns of debt issuance; it is not obvious that school districts increased their borrowing in response to the Build America Bonds program by more so than other local governments.

A more thorough analysis of different government's propensities for issuing debt requires a regression model that can account for economic and demographic factors that influence rates of debt issuance. In order to account for unobserved factors that may influence a locality's decision to issue debt, this essay employs difference-in-difference estimation in order to compare the level of debt issued by local governments in California 1) before and after Proposition 39 in 2000, and 2) before and after the Build America Bonds program was instituted in 2009.

Based on the literature reviewed in the prior section, the following factors may impact a government's propensity to issue long-term debt: demographics, such as the size and age breakdown of the population, the existing price of public services, and the financial condition of the government.

The following variables are employed to capture local demographics: the proportions of the population under 18 and over 65, the unemployment rate, median household earnings, educational characteristics, the proportion of households below poverty level, and the percent of the housing stock that is owner-occupied. The existing price level of public services is represented by the median tax burden (measured by real estate taxes).[40] The financial condition of the government is represented by the amount

[40] Census data are available for counties and municipalities. For issuers that are special governments or school districts, demographic data from the corresponding county or municipality were used. Total population was excluded as a regressor because of the possibility that certain special districts or school

of debt outstanding and the credit rating of the government in question.[41] An additional variable, the number of bonds issued since 1984, gives an indication of different localities' experience with financial markets.[42] Governments that frequently issue debt may have more sophisticated knowledge of financial markets (Bland, 1985) and as a result be more likely to take advantage of a program such as Build America Bonds. A dummy variable is used to indicate whether a district is a school district or not; this dummy encompasses community college districts and county offices of education in addition to regular K-12 school districts, as the Prop 39 decision applied to them as well.

Summary statistics are presented in Table 4.1. Because demographic data from the American Community Survey were unavailable for local governments prior to 2005, the analysis of Proposition 39 (presented on the following pages in Tables 4.2 and 4.3) is based on time-invariant Census data from the year 2000. The analysis of debt during the period of the Build America Bonds program (Tables 4.4 and 4.5) incorporates time-varying demographic data for all years of the analysis (2005-2010).

The data on debt issuance come from the California Debt and Investment Advisory Commission (CDIAC) and consist of all public debt issued in the state since 1984. Data on Build America Bonds were cross-checked with data from the Treasury Department. CDIAC's database is used not only to identify the public debt that was issued in California between April of 2009 and December of 2010, but also to identify all

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districts do not serve the entire county. When introduced into the regressions presented in Section 4.6, population was not statistically significant.

[41] Credit ratings were only available for approximately half of the issuers in the sample. As a result, credit rating was excluded from the regressions presented in the next section. However, separate regressions were performed only including those issuers for whom credit ratings were available. The coefficients on the variables of interest were not substantially different from those presented in Tables 4.2 and 4.3.

[42] In the absence of population or revenue data, the amount of debt outstanding and the number of bonds issued since 1984 also serve to control for the size of the government in question.

Table 4.1 Summary Statistics, 2000, 2005-2010

Variable	Source	Mean	S.D.	Range
Dependent Variables				
Issued General Obligation Bond	CDIAC	0.07	0.26	0-1
Issued Any Long-Term Debt	CDIAC	0.17	0.37	0-1
General Obligation Debt Issued as Share of Total Debt Outstanding	CDIAC/Author's calculation	0.02	0.10	0-1
Long-Term Debt Issued as Share of Total Debt Outstanding	CDIAC/Author's calculation	0.05	0.18	0-1
Population demographics				
Percent of the population under 18	Census	26	3.7	12 – 37
Percent of the population 65 or older	Census	11	2.3	3.0 – 27
Unemployment rate	Census	6.9	2.8	1.7 – 19
Median household income (Thousands of 2005 dollars)	Census	53	19	0.7 – 143
Percent of population (aged 25+) with a BA or higher	Census	27	11	3.5 – 70
Percent of families below poverty level	Census	10	4.5	0.8 – 26
Percent of the housing stock that is owner-occupied	Census	54	6.2	24 – 85
Existing price/level of public services				
Tax burden (Median real estate taxes) (Thousands of 2005 dollars)	Census	2.5	0.9	0.9 – 6.9
Financial condition				
Credit Rating (Ordinal ranking with AAA = 10)	California Debt and Investment Advisory Commission (CDIAC)	7.2	3.6	1 – 10
Debt Outstanding[43] (Millions of 2005 Dollars)	CDIAC/Author's Calculations	137	615	0 – 17,471
Experience with financial markets				
Number of bonds issued since 1984	CDIAC/Author's Calculations	7.0	13.3	1 – 313
Type of government				
Indicator variable for school district	CDIAC	0.25	0.43	0 – 1

[43] Total debt outstanding is estimated based on prior years' debt issuance. It does not take into account debt that was retired early.

other government entities that had previously issued debt but did not do so during this period; that is, the sample consists of all governments that have issued debt in California since 1984.[44,45]

In order to accurately assess the amount of debt issuance, this essay models two different forms of the dependent variable: 1) a dummy variable indicating whether a local government issued a general obligation bond in a given year, and 2) the share of the total debt outstanding that a particular year's worth of new general obligation debt represents. Unlike much of the prior work on debt limits, which focuses on the total level of debt outstanding, the focus here is on new debt so as to better assess the immediate impact of changes to the debt process.

In order to control for time-invariant factors that may influence the level of debt that school districts issue, this essay uses the difference-in-difference estimator. The empirical model may be expressed as:

$$Y_{it} = \beta_0 + \beta_1 S_i + \beta_2 T_t + \beta_3 (S_i * T_t) + \beta_4 X_{it} + \upsilon_{it} \qquad (1)$$

where Y_{it} represents the debt issued by district i during year t, X represents a vector of financial and demographic characteristics, S is a dummy variable indicating whether or not district i is a school district, T represents the time period in which a change is expected, and $(S_i * T_t)$ is the interaction term equal to one for school districts during the time period of interest. The time period of interest in the first analysis is 2001-2004, the

[44] The only issuers that were excluded were special districts that covered multiple counties and could not be easily matched with a corresponding municipality or county for the purpose of assigning demographic characteristics.

[45] This raises the concern that, by excluding certain local governments that never issued debt, the sample may produce biased results. However, the long time frame (1984-present) over which the data were collected mitigates this concern, as does the fact that certain small districts may simply rely on one particular agency (eg. the county or a financing authority) to issue debt for public purposes.

years immediately following Proposition 39. In the second analysis, the years of interest are 2009 and 2010, the years in which the Build America Bonds Program was in effect.

While the first set of regressions in this case are a classic example of difference-in-difference analysis – the analysis incorporates treatment (school districts) and control groups (all other local governments), which it observes before and after the change in the law – the second set of regressions does not precisely fit this model. This is because the "treatment" in this case – the enactment of Build America Bonds – is available to all local governments, and there is no separate control group. Nevertheless, the difference-in-difference estimator can be adapted here because it measures the differential response to the changing credit conditions; that is, it measures whether school districts reacted differently to the new set of conditions relative to other types of local governments.

4.6 Results

Tables 4.2 and 4.3 present the results of a difference-in-difference analysis of the effect of Proposition 39 on debt levels. Tables 4.4 and 4.5 present the results of a similar analysis that focuses instead on debt levels after the Build America Bond program was introduced.

Table 4.2 presents the results from a probit model. The dependent variable takes on the value of one in a year in which a district issued debt and zero otherwise. Model 1 incorporates a dummy variable ("Post- Prop 39 Time Period") that takes the value of one in the years 2001-2004 and zero during 1998-2000. The coefficient of interest is the coefficient on the interaction between the post-prop 39 time period and the variable indicating whether or not the district in question is a school district ("Post-Prop 39 Time

Period * School district"). This coefficient expresses whether or not school districts saw a change in their level of debt issuance following the passage of Proposition 39 relative to other local governments. Including a separate variable for school districts accounts for the fact that school districts may simply have different capital financing needs than other types of local governments. Hence, the panel nature of the data helps to account for time-invariant factors that might contribute to the likelihood of school districts issuing debt that are otherwise unobserved.

Model 2 introduced a fully set of year dummies to account for unobserved factors specific to individual years. These "time fixed effects" capture yearly fluctuations in economic conditions that may have a significant impact on debt issuance in ways that are not otherwise captured by the control variables (in particular, the unemployment rate).

Model 3 incorporates a linear time trend. This variable is introduced in order to account for the possible non-stationarity of the data.[46] If the likelihood of debt issuance is non-stationary, this threatens the consistency and asymptotic normality of the estimator. Indeed, the upward sloping trend of debt issuance that Figure 4.1 depicts between 1999-2004 indicates that economic pressures may have encouraged localities to issue more debt over time. (Similarly, a downward sloping trend is visible in Figure 4.5 for the years 2005-2010). These economic pressures may be unobserved in the data. If this is in fact the case, and the unobserved factors are linear, then a time trend can correct for the non-stationarity.

Models 4 – 6 are identical to Models 1 – 3 except that they consider all forms of long-term debt and not just general obligation debt. The reason for including data on all

[46] A Fisher unit root test produces a p-value of 0.0751 for the 2005-2010 data; it rejects the null hypothesis that a unit root exists at the 10 percent level, but not at the five percent level. The ambiguity of this result suggests that a time trend should be included in at least one specification.

forms of debt as a point of comparison is that in theory school districts should be no different from other types of governments in terms of the trends in their total debt issuance; the advantage afforded to them by Proposition 39 is only relevant to general obligation debt.

In Models 1- 3 in Table 4.2, the coefficient on the dummy variable for school districts is positive in all three models. School districts in California appear to be more likely to issue general obligation bonds than other types of local governments. This is likely due to the high capital financing needs of schools, as education spending is the most common form of capital spending for state and local governments. According to Models 1-3, school districts are approximately 18 percentage points more likely to issue a general obligation bond in a given year than other types of local governments, given that all other covariates are at their mean values.[47] This result was statistically significant at the 1 percent level.

The coefficients on the variables of interest – the interaction between "Post-Prop 39 Time Period" and "School district" – are also positive. However, they are not statistically significant. Hence, there is no evidence that school districts were more likely to issue debt as a result of Proposition 39; other districts, not subject to the Prop 39 decision, also saw an increase in borrowing.

The results from Models 4-6 also show positive and statistically significant coefficients on the school district dummy, demonstrating that school districts are again more likely to issue debt, regardless of whether it is full faith and credit debt or other types of bonds. However, in Models 4-6 the coefficient on the interaction term is

[47] The coefficients in Table 4.2 are marginal effects and are thus interpreted as the effect that a one unit change in the independent variable has on the probability of a local government issuing a general obligation bond, given that all other covariates are at their mean.

statistically significant, albeit at the ten percent level. A p-value of less than 0.10 does not meet the standard threshold of statistical significance in most social science research, and thus this result should be interpreted with caution. Moreover, the size of the coefficient indicates only a modest effect: school districts saw an increase in the probability of issuing long-term debt that was two percentage points greater than what other districts saw following Proposition 39.

One thing that the results in Table 4.2 do not make clear is whether the probability of issuing a general obligation bond increased or declined during the 1998-2004 time period. According to Model 1, local governments were more likely to issue debt between 2001 and 2004 than they were between 1998 and 2000; the coefficient on "Post-Prop 39 Time Period" is positive. On the other hand, when time dummies are introduced in Model 2, this result reverses itself. With the assumption of an unobserved linear trend in the debt data, the coefficient on "BAB Time Period" becomes positive once again in Model 3. A similar pattern appears in Models 4-6. Ultimately this essay's findings do not rest on an assumption of stationarity; as noted above, the interaction terms that are of interest are relatively unchanged whether a time trend is introduced or not. Nonetheless, this result highlights the importance of unit root tests in panel estimation.

Several other results bear mentioning as well. The coefficient on the percent of the population that is under 18, although small in magnitude, is positive and statistically significant at the one percent level in the models pertaining to general obligation debt. The coefficients on median real estate taxes and the median household income are positive and statistically significant at the five percent level (the latter only in Models 4 and 6). In Models 4-6, coefficients on the percent of families below poverty and the

percent of housing that is owner occupied are negative and statistically significant at the five percent level. These results are all consistent with prior work showing that wealthy communities with a large number of school age children will be more likely to authorize government debt. One would expect that communities with a large number of children would be more likely to pass bond measures as voters' children would directly benefit from new facilities. Rueben and Cerdan (2003) found that California school districts that proposed and passed bond measures had greater average household income and higher assessed property values than districts that proposed and failed. The finding with regard to the percent of housing that is owner-occupied is consistent with DeBartolo and Fortune's (1982) finding that renters are more willing to finance capital expenditures that will benefit the community. This may be explained by the fact that authorized school bonds are typically paid off through an increment to the local property tax rate (usually 0.25 percentage points) and thus would have no immediate effect on renters. Also attaining statistical significance at the one percent level is the coefficient on the amount of debt outstanding and the number of prior debt issues. Districts with more debt outstanding are less likely to issue debt, while districts with a higher frequency of bond offerings are more likely.

Table 4.3 presents the results from a tobit model. The tobit estimator is used because the dependent variable in this case – debt issued as a share of the total debt outstanding – varies in the 0-1 interval, and there are many left-censored observations at 0. Models 1-6 parallel those from Table 4.2, with Models 1-3 pertaining only to general obligation debt and Models 4-6 pertaining to all long-term debt. Estimated tobit coefficients can be interpreted similarly to OLS coefficients, except that they represent

the effect on the uncensored latent variable (unconstrained by the 0-1 interval) rather than the observed outcome.

Once again the coefficients on the variable of interest – the interaction term between school district and time – are not statistically significant. As with Table 4.2, school districts appear to issue more debt than other types of local governments. However, there is no evidence that school districts responded to the Proposition 39 result by issuing more debt *relative to other districts* than they had previously. As with Table 4.2, the coefficients on the percentage of the population under 18, the number of debt issues, and median real estate taxes are positive and statistically significant at the one percent level in at least three of the models, while the coefficient on the percent of housing that is owner-occupied is negative and statistically significant.

Tables 4.4 and 4.5 show the results of analyzing municipal debt in California over the time period 2005-2010. The time period of interest in this case is 2009-2010, when governments could issue Build America Bonds. As with Tables 4.2 and 4.3, the results fail to indicate that school districts changed their rates of debt issuance relative to other types of local governments in response to changing credit conditions. Once again the coefficient on the dummy variable for school districts was positive and statistically significant; according to Models 1-3 in Table 4.4, school districts were more than 20 percentage points more likely to issue a general obligation bond than other types of local governments. However, none of the models found that the coefficient on the interaction term – "BAB Time Period * School District" – was statistically significant.

4.7 Conclusion

This essay uses panel data on municipal debt issuance to examine whether procedural debt restrictions impact local governments' level of new debt and ability to take advantage of favorable credit conditions. It exploits the difference in procedural debt requirements that Proposition 39 brought about in California in order to determine whether school districts changed their patterns of general obligation debt issuance relative to other governments following the passage of Prop 39 in 2000 and then again in 2009 when lower borrowing costs were available in the form of Build America Bonds.

Results from probit and tobit estimations suggest that school districts in California 1) issued no more debt relative to other districts after Proposition 39 than they had before, and 2) are no more interest elastic in their debt issuance than other types of local governments. Although school districts issued the majority of general obligation debt following 2000 as well as the majority of Build America Bonds in the state, these outcomes were not inconsistent with prior trends in debt issuance; they did not see a statistically significant increase in debt issuance in either 2001-2004 or in 2009- 2010 relative to other governments, as might have been expected. These findings were consistent across models that measured the change in likelihood of issuing a bond as well as models that looked at the proportional change in the amount of debt issued.

These results build on the prior work suggesting that referenda requirements do not affect the amount of debt that local governments issue. Farnham (1985) found that there are no differences in the debt levels of communities with simple majority referenda and those without debt referenda. Harris and Munley (2011) found that the existence of

referenda approval requirements is actually positively associated with the probability of a district issuing a bond, a result that they struggle to explain.

The relative unimportance of procedural requirements may be explained in part by the tendency of local governments to keep bringing bond measures to the ballot box until they are successful. If a bond election is close to the 2/3 majority needed, then further elections may prove fruitful. Dunne, Reed, and Wilbanks (1997) showed that the decision to schedule a referendum vote is related to the likelihood of the vote's success. A lower procedural requirement might hasten the process, but ultimately would not result in any more debt. Hence, one limitation to this study's findings may be the relatively small difference in procedural requirements that it exploits (2/3 of voters versus 55 percent of voters). Researchers studying the interest elasticity of municipal borrowing may be more likely to uncover statistically significant differences between governments if they compare districts that face voting requirements (supermajority or majority) with districts that do not.

Of course, the fact that districts can propose multiple bond measures until they are successful would not explain the results pertaining to Build America Bonds, as the BAB program was only available for a short period of time, and thus school districts might have been expected to take better advantage. However, due to the relative infrequency of local government debt and the amount of time between election dates, it may be the case that many districts needed to have taken steps toward a bond measure prior to April of 2009 in order for them to take advantage of the program.

This points to another possible limitation to the study's findings. In examining only the amount of new debt that was issued, the model proposed above does not account

for referenda results. It is a model that explains debt issuance, not election results. This was in part a deliberate attempt to shift the focus of research on procedural restrictions away from election outcomes and toward debt levels, which in many ways is a more important measure of fiscal health. And while one obviously cannot occur without the other (at least in the case of general obligation bonds), there are distinctions between how one might attempt to empirically model the outcomes. For instance, as noted above, legislators may be more likely to schedule a vote if a prior referendum failed narrowly. What's more, more bond elections are held during state election years, complicating the time-series analysis of referenda outcomes (see footnote 35). Once a referendum is passed, districts have some discretion as to when they may ultimately issue a bond, and in many cases may wait years to do so.

Nonetheless, as the two topics are clearly related, the results presented here would be informed by data on voting outcomes. Were referenda elections more successful in the wake of the stimulus than before? That is, were citizens aware of the favorable terms available through the Build America Bonds program and was this reflected at the voting booth? The study of bond elections may yield further insight into rational voting behavior and its impact on municipal borrowing.

Table 4.2 Probit Model – Difference-in-Difference Analysis of Proposition 39's Effect on Debt Issuance (1998-2004)

Variables	(1) Issued General Obligation Bond	(2) Issued General Obligation Bond	(3) Issued General Obligation Bond	(4) Issued Any Long-Term Debt	(5) Issued Any Long-Term Debt	(6) Issued Any Long-Term Debt
Percent of population under 18 (2000)	0.00241***	0.00236***	0.00244***	0.00239	0.00235	0.00238
	(0.000881)	(0.000879)	(0.000884)	(0.00184)	(0.00184)	(0.00184)
Percent of population 65 and older (2000)	-0.000493	-0.000460	-0.000478	-0.00237	-0.00246	-0.00237
	(0.000877)	(0.000874)	(0.000878)	(0.00176)	(0.00176)	(0.00176)
Percent of the population with a BA or higher (2000)	0.000539*	0.000452	0.000549*	0.000145	6.57e-05	0.000144
	(0.000303)	(0.000304)	(0.000303)	(0.000600)	(0.000605)	(0.000600)
Unemployment rate (2000)	-0.000857	-0.00122	-0.000813	0.00395*	0.00296	0.00394*
	(0.000915)	(0.000928)	(0.000916)	(0.00204)	(0.00207)	(0.00204)
Median household income (2000)	-4.48e-05	-2.28e-06	-3.95e-05	0.000519***	0.000382*	0.000518***
	(6.29e-05)	(7.15e-05)	(6.27e-05)	(0.000185)	(0.000202)	(0.000185)
Median real estate taxes (2000)	0.00538**	0.00541**	0.00529**	0.0165***	0.0168***	0.0165***
	(0.00256)	(0.00255)	(0.00256)	(0.00529)	(0.00527)	(0.00529)
Percent of families below poverty (2000)	-0.00132*	-0.00118	-0.00133*	-0.00403**	-0.00405**	-0.00402**
	(0.000729)	(0.000727)	(0.000730)	(0.00162)	(0.00162)	(0.00162)
Percent of housing that is owner-occupied (2000)	-0.000381	-0.000407	-0.000392	-0.00137**	-0.00135**	-0.00137**
	(0.000281)	(0.000278)	(0.000281)	(0.000607)	(0.000607)	(0.000607)
Debt Outstanding	-1.60e-05***	-1.62e-05***	-1.62e-05***	5.03e-06	4.56e-06	5.08e-06
	(5.98e-06)	(6.03e-06)	(6.03e-06)	(1.91e-05)	(1.90e-05)	(1.91e-05)
Number of debt issues since 1984	0.00219***	0.00218***	0.00219***	0.0124***	0.0124***	0.0124***
	(0.000210)	(0.000210)	(0.000210)	(0.000939)	(0.000938)	(0.000939)
School district dummy	0.175***	0.174***	0.175***	0.109***	0.110***	0.110***
	(0.00995)	(0.00995)	(0.00996)	(0.0105)	(0.0105)	(0.0105)
Post-Prop 39 Time Period	0.00722**	0.00604	0.000196	0.0199***	-0.00813	0.0213**
(Indicator variable equal to 1 in 2001 - 2003)	(0.00341)	(0.00466)	(0.00472)	(0.00624)	(0.0102)	(0.0103)
Post-Prop 39 Time Period * School district	0.00709	0.00739	0.00705	0.0209*	0.0206*	0.0209*
	(0.00506)	(0.00506)	(0.00506)	(0.0122)	(0.0122)	(0.0122)
Year Dummies	No	Yes	No	No	Yes	No
Time Trend	No	No	Yes	No	No	Yes
Observations	22,849	22,849	22,849	22,849	22,849	22,849

Coefficients represent marginal effects. Standard errors in parentheses are robust and clustered by issuer. *** p<0.01, ** p<0.05, * p<0.1

Table 4.3 Tobit Model – Difference-in-Difference Analysis of Proposition 39's Effect on Debt Issuance (1998-2004)

Variables	(1) General Obligation Debt Issued as Share of Debt Outstanding	(2) General Obligation Debt Issued as Share of Debt Outstanding	(3) General Obligation Debt Issued as Share of Debt Outstanding	(4) Long-Term Debt Issued as Share of Debt Outstanding	(5) Long-Term Debt Issued as Share of Debt Outstanding	(6) Long-Term Debt Issued as Share of Debt Outstanding
Percent of population under 18 (2000)	0.0251***	0.0245***	0.0254***	0.0104*	0.0101*	0.0103*
	(0.00918)	(0.00922)	(0.00921)	(0.00532)	(0.00534)	(0.00533)
Percent of population 65 and older (2000)	-0.00343	-0.00323	-0.00333	-0.00653	-0.00674	-0.00654
	(0.00904)	(0.00907)	(0.00905)	(0.00504)	(0.00506)	(0.00504)
Percent of the population with a BA or higher (2000)	0.00757**	0.00658**	0.00766**	0.000590	0.000259	0.000575
	(0.00307)	(0.00310)	(0.00308)	(0.00170)	(0.00171)	(0.00170)
Unemployment rate (2000)	-0.00937	-0.0135	-0.00906	0.00664	0.00349	0.00658
	(0.00956)	(0.00977)	(0.00957)	(0.00584)	(0.00593)	(0.00585)
Median household income (2000)	-0.000639	-0.000166	-0.000605	0.00123***	0.000888*	0.00122***
	(0.000665)	(0.000777)	(0.000664)	(0.000460)	(0.000527)	(0.000460)
Median real estate taxes (2000)	0.0340	0.0351	0.0334	0.0528***	0.0539***	0.0529***
	(0.0259)	(0.0260)	(0.0260)	(0.0141)	(0.0140)	(0.0141)
Percent of families below poverty (2000)	-0.0126*	-0.0112	-0.0128*	-0.0119**	-0.0117**	-0.0118**
	(0.00764)	(0.00767)	(0.00765)	(0.00465)	(0.00466)	(0.00465)
Percent of housing that is owner-occupied (2000)	-0.00371	-0.00400	-0.00379	-0.00482***	-0.00476***	-0.00481***
	(0.00298)	(0.00298)	(0.00298)	(0.00179)	(0.00179)	(0.00179)
Debt Outstanding	-0.000243***	-0.000245***	-0.000244***	-9.03e-05**	-9.06e-05**	-9.01e-05**
	(8.69e-05)	(8.80e-05)	(8.74e-05)	(3.86e-05)	(3.87e-05)	(3.86e-05)
Number of debt issues since 1984	0.0188***	0.0188***	0.0189***	0.0150***	0.0150***	0.0150***
	(0.00208)	(0.00209)	(0.00208)	(0.00173)	(0.00173)	(0.00173)
School district dummy	1.091***	1.088***	1.091***	0.353***	0.354***	0.353***
	(0.0541)	(0.0541)	(0.0541)	(0.0280)	(0.0281)	(0.0280)
Post- Prop 39 Time Period	0.0711*	0.0498	0.0189	0.0461**	-0.0321	0.0582*
(Indicator variable equal to 1 in 2001-2003)	(0.0388)	(0.0539)	(0.0539)	(0.0191)	(0.0317)	(0.0310)
Post- Prop 39 Time Period * School district	0.0270	0.0292	0.0268	0.00736	0.00710	0.00731
	(0.0510)	(0.0511)	(0.0511)	(0.0358)	(0.0357)	(0.0358)
Year Dummies	No	Yes	No	No	Yes	No
Time Trend	No	No	Yes	No	No	Yes
Observations	22,849	22,849	22,849	22,849	22,849	22,849

Standard errors in parentheses are robust and clustered by issuer. *** p<0.01, ** p<0.05, * p<0.10.

Table 4.4 Probit Model – Differential Effect of Build America Bonds on Debt Issuance (2005-2010)

Variables	(1) Issued General Obligation Bond	(2) Issued General Obligation Bond	(3) Issued General Obligation Bond	(4) Issued Any Long-Term Debt	(5) Issued Any Long-Term Debt	(6) Issued Any Long-Term Debt
Percent of population under 18	0.000893	-7.41e-05	-2.22e-05	0.00109	-0.00144	-0.00129
	(0.000863)	(0.000879)	(0.000866)	(0.00188)	(0.00195)	(0.00191)
Percent of population 65 and older	-0.00167	-0.00173	-0.00176	-0.00257	-0.00242	-0.00282
	(0.00120)	(0.00118)	(0.00119)	(0.00208)	(0.00206)	(0.00207)
Percent of the population with a BA or higher	0.000233	-0.000362	-0.000396	0.000484	-0.000876	-0.00118
	(0.000368)	(0.000399)	(0.000390)	(0.000778)	(0.000836)	(0.000818)
Unemployment rate	0.00148***	8.74e-05	-0.00106	0.00112	0.00202	-0.00559***
	(0.000555)	(0.000889)	(0.000662)	(0.00132)	(0.00215)	(0.00160)
Median household income	-1.96e-06	-4.13e-05	-2.77e-05	0.000114	-5.39e-05	5.05e-05
	(8.48e-05)	(9.83e-05)	(8.14e-05)	(0.000199)	(0.000231)	(0.000195)
Median real estate taxes	0.00556	0.0108***	0.00992***	0.0103	0.0273***	0.0218**
	(0.00364)	(0.00379)	(0.00370)	(0.00846)	(0.00885)	(0.00863)
Percent of families below poverty	-0.000726	-0.000314	-0.000219	0.00102	0.00172	0.00228
	(0.000635)	(0.000643)	(0.000635)	(0.00144)	(0.00148)	(0.00146)
Percent of housing that is owner-occupied	-0.000336	-0.000387	-0.000379	0.000387	0.000185	0.000234
	(0.000264)	(0.000255)	(0.000254)	(0.000635)	(0.000630)	(0.000624)
Debt outstanding	-1.06e-05**	-1.07e-05**	-1.07e-05**	3.33e-05*	3.36e-05*	3.36e-05*
	(5.15e-06)	(5.14e-06)	(5.14e-06)	(1.76e-05)	(1.77e-05)	(1.77e-05)
Number of debt issues since 1984	0.00191***	0.00188***	0.00189***	0.00792***	0.00790***	0.00791***
	(0.000206)	(0.000206)	(0.000206)	(0.000630)	(0.000627)	(0.000627)
School district dummy	0.214***	0.213***	0.213***	0.158***	0.158***	0.159***
	(0.00918)	(0.00924)	(0.00923)	(0.00957)	(0.00962)	(0.00961)
BAB Time Period (Indicator variable equal to 1 in 2009 and 2010)	-0.0160***	-0.0261***	0.0244***	-0.0415***	-0.0961***	0.0623***
	(0.00405)	(0.00517)	(0.00847)	(0.00811)	(0.0123)	(0.0159)
BAB Time Period * School district	-0.000738	-0.00141	-0.00107	-0.0139	-0.0139	-0.0138
	(0.00533)	(0.00515)	(0.00520)	(0.0121)	(0.0121)	(0.0121)
Year Dummies	No	Yes	No	No	Yes	No
Time Trend	No	No	Yes	No	No	Yes
Observations	18,359	18,359	18,359	18,359	18,359	18,359

Coefficients represent marginal effects. Standard errors in parentheses are robust and clustered by issuer. *** p<0.01, ** p<0.05, * p<0.1

Table 4.5 Tobit Model – Differential Effect of Build America Bonds on Debt Issuance (2005-2010)

Variables	(1) General Obligation Debt Issued as Share of Debt Outstanding	(2) General Obligation Debt Issued as Share of Debt Outstanding	(3) General Obligation Debt Issued as Share of Debt Outstanding	(4) Long-Term Debt Issued as Share of Debt Outstanding	(5) Long-Term Debt Issued as Share of Debt Outstanding	(6) Long-Term Debt Issued as Share of Debt Outstanding
Percent of population under 18	0.00547	-0.00127	-0.000949	0.00340	-0.00239	-0.00228
	(0.00570)	(0.00594)	(0.00581)	(0.00476)	(0.00489)	(0.00482)
Percent of population 65 and older	-0.00942	-0.0101	-0.0103	-0.00745	-0.00678	-0.00802
	(0.00737)	(0.00739)	(0.00741)	(0.00522)	(0.00514)	(0.00521)
Percent of the population with a BA or higher	0.00275	-0.00149	-0.00169	0.00269	-0.000358	-0.00130
	(0.00243)	(0.00270)	(0.00263)	(0.00184)	(0.00198)	(0.00194)
Unemployment rate	0.0126***	0.00212	-0.00508	0.00354	0.00830	-0.0126***
	(0.00402)	(0.00649)	(0.00489)	(0.00356)	(0.00568)	(0.00432)
Median household income	9.74e-05	8.72e-06	-0.000115	-9.82e-05	-0.000506	-0.000271
	(0.000529)	(0.000632)	(0.000511)	(0.000455)	(0.000534)	(0.000446)
Median real estate taxes	0.0114	0.0467*	0.0428*	0.00987	0.0511**	0.0371*
	(0.0248)	(0.0263)	(0.0256)	(0.0207)	(0.0216)	(0.0211)
Percent of families below poverty	-0.00614	-0.00307	-0.00274	0.00172	0.00313	0.00465
	(0.00434)	(0.00448)	(0.00442)	(0.00377)	(0.00387)	(0.00384)
Percent of housing that is owner-occupied	-0.00208	-0.00257	-0.00244	0.00183	0.00128	0.00143
	(0.00173)	(0.00170)	(0.00168)	(0.00159)	(0.00157)	(0.00156)
Debt outstanding	-7.46e-05*	-7.68e-05*	-7.70e-05*	8.81e-06	8.79e-06	8.23e-06
	(4.10e-05)	(4.17e-05)	(4.18e-05)	(1.39e-05)	(1.38e-05)	(1.40e-05)
Number of debt issues since 1984	0.0102***	0.0102***	0.0102***	0.00910***	0.00901***	0.00907***
	(0.00138)	(0.00140)	(0.00140)	(0.00134)	(0.00135)	(0.00135)
School district dummy	0.739***	0.737***	0.737***	0.309***	0.306***	0.307***
	(0.0323)	(0.0324)	(0.0324)	(0.0193)	(0.0193)	(0.0193)
BAB Time Period (Indicator variable equal to 1 in 2009 and 2010)	-0.132***	-0.204***	0.142***	-0.114***	-0.271***	0.132***
	(0.0335)	(0.0464)	(0.0478)	(0.0230)	(0.0378)	(0.0380)
BAB Time Period * School district	-0.0123	-0.0167	-0.0149	-0.0471	-0.0452	-0.0455
	(0.0386)	(0.0384)	(0.0384)	(0.0323)	(0.0321)	(0.0322)
Year Dummies	No	Yes	No	No	Yes	No
Time Trend	No	No	Yes	No	No	Yes
Observations	18,359	18,359	18,359	18,359	18,359	18,359

Standard errors in parentheses are robust and clustered by issuer. *** p<0.01, ** p<0.05, * p<0.10.

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