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The General Equilibrium of Tax and Expenditure Limits

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by

Ellen Concetta Moule

Committee in charge:

Professor Gary W. Cox, Chair
Professor Mathew D. McCubbins, Co-Chair
Professor Gordon Hanson
Professor Thad Kousser
Professor Craig MacIntosh

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The Dissertation of Ellen Concetta Moule is approved, and it is acceptable in quality and form for publication on microfilm and electronically:

Co-Chair

Chair

University of California, San Diego

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TABLE OF CONTENTS

Signature Page iii

Table of Contents iv

List of Tables v

List of Figures vii

Acknowledgements ix

Vita x

Abstract xi

Introduction 1

Chapter 1: The Effectiveness of TELs 17

Chapter 2: Policy Implementation in Direct Democracy 67

Chapter 3: Secondary and Tertiary Consequences of Property Tax Limits 93

Chapter 4: The Tax Revolt in Massachusetts 118

Works Cited 169

LIST OF TABLES

Table I.1: Property Tax Limitations 4

Table I.2: Spending Limitations 5

Table I.3: Revenue Limitations 5

Table 1.1: Diagnostics for OLS Regressions on State and Local Spending 28

Table 1.2: Diagnostics for OLS Regressions on State and Local Revenue 29

Table 1.3: Diagnostics for OLS Regressions on State and Local Property Taxes .. 30

Table 1.4: Diagnostics of OLS Regression on Property Tax Growth (3 Year Intervals) 37

Table 1.5: Analysis of Trends Preceding Spending Limit Implementation 45

Table 1.6: Analysis of Trends Preceding Revenue Limit Implementation 46

Table 1.7: Analysis of Trends Preceding Property Tax Limit Implementation........ 47

Table 1.8: Correlations Between Spending Limits and Future Growth 55

Table 1.9: Correlations Between Spending Limits and Future Growth 56

Table 1.10: Analysis of Spending Limit Effectiveness 58

Table 1.11: Analysis of Revenue Limit Effectiveness 60

Table 1.12: Analysis of Property Tax Limit Effectiveness 62

Table 1.13: Analysis of Short-Term Effectiveness of Spending and Revenue Limits 64

Table 1.14: Analysis of Short-Term Effectiveness of Property Tax Limits............ 65

Table 2.1: Effect of Incompatible Preferences on TEL Effectiveness 79

Table 2.2: Tax Bill Illustration 86

Table 2.3: Effect of Ease of Monitoring on Property Tax Effectiveness 88

Table 3.1: Statewide Property Tax Limits in the US 96

Table 3.2: Summary Statistics of Data 97

Table 3.3: Effect of Property Tax Limits on Revenue Components 103

Table 3.4: An Error Corection Model of Short-Run Income Elasticity, 1963-2005 107

Table 3.5: The Effect of Recession and Tax Limits on Revenue 115

Table 4.1: Analysis of Individual Property Tax Bill Growth 150

Table 4.2: Analysis of Proposition 2 ½ Constraint of Charges and Fees 166

LIST OF FIGURES

Figure 1.1: Serial Correlation from OLS Regression on Spending in Levels in California 25

Figure 1.2: Average Property Tax Growth Before and After Property Tax Limits. 43

Figure 1.3: Average Spending Growth Before and After Spending Limits 43

Figure 1.4: Effect of Lagged Spending Growth on Current Spending Growth........ 51

Figure 2.1: Changes to Proportions of Revenue Subject to Revenue Limits 74

Figure 3.1: Reliance of Revenue Sources, Before and After Property Tax Limit Implementation.. 104

Figure 3.2: Income-Elasticity of Corporate Income Taxes 110

Figure 3.3: Income-Elasticity of Charges and Miscellaneous Revenues 110

Figure 3.4: Income-Inelasticity of Property Taxes.. 111

Figure 4.1: Actual vs. Hypothetical Property Tax Growth Under Proposition 2 ½.. 125

Figure 4.2: Components of Property Tax Limit Growth.. 126

Figure 4.3: Specific Overrides 131

Figure 4.4: General Overrides... 131

Figure 4.5: Override Passage Rates.. 132

Figure 4.6: Cumulative Cost of Overrides.. 133

Figure 4.7: Proposal and Passage of Debt Exclusions 135

Figure 4.8: Proposal and Passage of Capital Exclusions.. 136

Figure 4.9: Local Massachusetts Revenue, 1977-2007 152

Figure 4.10: Local Aid as a Percentage of State Total Own-Source Revenue 157

Figure 4.11: Growth of Charges and Fees in Massachusetts.................................. 160

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VITA

2004 Bachelor of Arts, Drew University

2010 Doctor of Philosophy, University of California, San Diego

PUBLICATIONS

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ABSTRACT OF THE DISSERTATION

The General Equilibrium of Tax and Expenditure Limits

by

Ellen Concetta Moule

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Professor Gary W. Cox, Chair
Professor Mathew D. McCubbins, Co-Chair

This dissertation analyzes the effectiveness of tax and expenditure limits. I contend that these limits are frequently implemented unfaithfully. Further, the politics of

circumvention used to evade these limits caused unintended secondary and tertiary effects. To understand the policy implementation process, I apply principal-agent theory.

The first chapter of my dissertation analyzes the effectiveness of tax and expenditure limits empirically. I pay close attention to pitfalls of time-series, cross-sectional data. Specifically, I account for violations of Guass-Markov caused by serial correlation or heteroskedasticity. I employ flexible TEL indicators to test for temporary and heterogeneous effects of the limits.

In the second chapter I leverage two conditions previously shown to produce successful delegation to agents from the principal-agent literature. I apply each condition to the case of tax and expenditure limits and test whether or not limits are more effective when these conditions are met. My results show that tax and expenditure limits are more successfully when implemented by agents that share ideological convictions for cutting the size of government. I also present suggestive evidence that making limits easier to monitor makes them more effective.

My third chapter focuses on the secondary and tertiary consequences of tax and expenditure limits. Specifically, I present evidence that property tax limits have detrimental effects on state and local revenues during recessions. Property tax limits cause states to rely on income–elastic revenue sources which cause greater revenue declines during economic downturns.

Finally, my fourth chapter is a case study of Massachusetts' 1980 Property Tax Limit, Proposition 2 ½. This initiative limits municipal property taxes to growth by 2.5% per year. In this chapter I look at the formula used to calculate the limit, highlighting how changes to this formula over time have made the limit less effective. I also analyze the extent to which revenue substitution, such as increases in state aid and charges and fees, can be attributed to the property tax limit.

Introduction

This dissertation analyzes the effectiveness of limits aimed at constraining the growth of state and local governments. The primary insight of this scholarship is that policy implementation cannot be taken for granted. This is particularly true in cases where the actors making policy are separate from the actors charged with implementing public policy, as in the case of direct democracy.

I contend that policies passed through direct democracy will not always be faithfully implemented. Further, the politics of circumvention can engender unintended secondary and tertiary effects. To understand the policy implementation process, I apply principal-agency theory. I hypothesize that state lawmakers will evade policy if their incentives are not compatible with the legislation's intent. This will particularly occur if the legislation cannot easily by monitored by voters.

What is a Tax and Expenditure Limit?

Tax and expenditure limits are constitutional or statutory laws that constrain the decisions of lawmakers as they pertaining to the size of government. In particular, the tax and expenditure limits discussed herein are all aimed at controlling the growth of state and local government finances. Instead of allowing lawmakers to tax and spend as they see fit, TELs provide ceilings that set maximum levels for revenue or expenditure growth. These limits peg the growth of government to an index (most commonly population growth, personal income growth, or inflation) or to a fixed percentage.

TELs are most commonly passed through direct democracy. From 1970 to 2006, there were 44 passed and 94 proposed TELs. Adopting these measures was particularly common in the wake of California's Proposition 13, a stringent property tax measure that limited taxes to 1% of assessed value and 2% growth per year. Indeed, there were 20 TEL proposals via direct democracy alone within two years of Proposition 13. To this day, limitations on property taxes continue to be the most popular form of TEL at the ballot box.

Defining Tax and Expenditure Limits

My research focuses on three types of TELs: expenditure limits, revenue limits, and property tax limits. The common feature of each of these limits is that they all aim to constrain the growth of the size of government. The definitions for each of these distinct types of TELs are largely self-explanatory. Expenditure limits cap state (or state and local) expenditures or appropriations (only discretionary expenditures). Revenue limits place a ceiling on revenue intake, most commonly revenues used for general operating expenses. Finally, property tax limits confine the growth of property tax revenues. Some property tax limits dictate how much revenue a taxing entity can collect from this particular tax, while others limit taxes by confining the growth of the assessed value of a homeowner's property.

There are several types of fiscal limitations not included in this definition of TELs. I consider laws that govern procedural rules for adopting tax increases to be distinct from my definition of a TEL. For example, I exclude from analysis supermajority limitations, the requirement that tax increases are supported by a supermajority in the state legislature. I also exclude voter-approval requirements, laws that require tax increases to be first approved by the voters. While each of these laws invariably constrains fiscal decision-making, they do not explicitly constrain the growth trajectory of government.

I also exclude limitations that cap expenditures or revenues to a percentage of estimated revenues. For example, Oregon's so-called "kicker law" refunds any revenues in excess of 2% of estimated revenues. While this law has repeatedly affected revenues in the state, it does not technically limit the growth of government. Lawmakers could simply make high revenue estimations (i.e. raise taxes) in order to prevent the refunds from occurring while still increasing the size of government. Similarly, I also exclude limits that cap expenditures to a fixed percentage of revenues, such as Mississippi's 1982 measure, as these laws more closely resemble balanced budget requirements, not caps on the growth of government. Finally, limits that only constrict limited tax bases, such as the sales tax or gasoline taxes, are also excluded as they are not expected to have large enough effects on total state and local fiscal outcomes.

The TELs that I have chosen to analyze in this dissertation appear in Tables I.1, I.2, and I.3. These tables list both the date of adoption as well as the date of implementation. For all analysis herein, I rely on the date of implementation as the start-date for the existence of a TEL. These tables also list important features of each limit.

Table I.1: Property Tax Limitations

State	Implemented	Type	Origin	Level	Growth
Arizona	1981-Present	Constitution	Referendum	Individual	Other
Arkansas	1982-2000	Constitution	Referendum	Aggregate	10%
Arkansas	2001-Present	Constitution	Referendum	Individual	5%
California	1979-Present	Constitution	Initiative	Individual	2%
Colorado	1993-Present	Constitution	Initiative	Aggregate	Index
Florida	1995-Present	Constitution	Initiative	Individual	3% or CPI
Idaho	1980-1992	Statute	Legislative	Aggregate	5%
Idaho	1996-Present	Statute	Legislative	Aggregate	3%
Indiana	1980-Present	Statute	Legislative	Aggregate	Other
Iowa	1979-Present	Statute	Legislative	Aggregate	6% (4% Amd.)
Kansas	1986-1998	Statute	Legislative	Aggregate	0%
Kentucky	1980-Present	Statute	Legislative	Aggregate	4%
Louisiana	1979-Present	Constitution	Initiative	Aggregate	0%
Maine	2006-Present	Statute	Legislative	Aggregate	Index
Massachusetts	1982-Present	Statute	Initiative	Aggregate	2.50%
Michigan	1979-1994	Constitution	Initiative	Aggregate	CPI
Michigan	1995-Present	Constitution	Referendum	Individual	5% or CPI
Mississippi	1995-Present	Statute	Legislative	Aggregate	10%
Missouri	1981-Present	Constitution	Initiative	Aggregate	CPI
Montana	1987-Present	Constitution	Initiative	Aggregate	1/2 CPI
Nevada	1984-Present	Statute	Legislative	Aggregate	4.5% (6% Amd)
New Mexico	1980-2000	Statute	Legislative	Aggregate	5%
New Mexico	2001-Present	Statute	Legislative	Individual	3%
Oklahoma	1997-Present	Constitution	Referendum	Individual	5%
Oregon	Start-1997	Constitution	Initiative	Aggregate	6%
Oregon	1998-Present	Constitution	Initiative	Individual	3%
South Dakota	1997-Present	Statute	Legislative	Aggregate	3% or CPI
Texas	1998-Present	Statute	Legislative	Individual	10%
Utah	Start-1986	Statute	Legislative	Aggregate	6%
Washington	1974-Present	Statute	Legislative	Aggregate	6%
Washington	2002-Present	Statute	Initiative	Aggregate	1%
West Virginia	1991-Present	Statute	Legislative	Aggregate	1%
Wisconsin	2006-Present	Statute	Legislative	Aggregate	0%

Table I.2: Spending Limitations

State	Year Adopted	Years Implemented	Type	Origin
Michigan	1978	1980-Present	Constitution	Initiative
Missouri	1980	1981-Present	Constitution	Initiative
Colorado	1992	1994-2004	Constitution	Initiative
Arizona	1978	1980-Present	Constitution	Initiative
Connecticut	1991	1993-Present	Statute	Legislative
Hawaii	1978	1980-Present	Constitution	Voter Aprrovec
Louisiana	1993	1995-Present	Constitution	Referendum
Montana	1981	1982-2004	Statute	Legislative
New Jersey	1990	1993-Present	Statute	Legislative
New Jersey	1976	1977-1982	Statute	Legislative
Oklahoma	1985	1986-Present	Constitution	Referendum
Oregon	2001	2002-Present	Statute	Legislative
South Carolina	1980	1982-Present	Constitution	Referendum
Utah	1989	1990-Present	Statute	Legislative
Washington	1993	1996-Present	Statute	Initiative
Wisconsin	2001	2004-Present	Statute	Legislative
California	1979	1981-Present	Constitution	Initiative

Note: Alaska is exluded from Analysis. This state passed a spending limit in 1982.

Table I.3: Revenue Limitations

State	Year Adopted	Years Implemented	Type	Origin
Florida	1994	1995-Present	Constitution	Referendum
Michigan	1978	1980-Present	Constitution	Initiative
Missouri	1980	1981-Present	Constitution	Initiative
Washington	1979	1981-1991	Statute	Initiative
Colorado	1992	1994-2004	Constitution	Initiative
California	1979	1981-1988	Constitution	Initiative

Identifying General Expenditure and Revenue Limits

Spending and revenue limits, broadly defined, are ubiquitous proposals in state legislatures and on initiative ballots. In some states, these sorts of limitations are proposed year after year. Sometimes these proposals are just publicity fodder, and oftentimes the limit doesn't make it past the proposal stage. More frequently than one might expect, however, TELs are passed into law only to be amended and forgotten soon-after. Even more frighteningly, it is surprisingly common for TELs to be proposed again several years after adoption without regards to the state's previous experience.

To consistently identify expenditure and revenue limits I relied on the National Council of State Legislatures (2010), Mullins and Wallin (2004), and Poterba and Rueben (1999). I compiled an exhaustive list of limits from these sources and conducted textual analysis of the laws themselves, judicial rulings, state newspaper reports, and various state government websites to analyze the degree to which the limits remain in force today. These sources were also used to identify the first fiscal year that the legislation affected state fiscal outcomes (as opposed to merely the date of adoption) as well as factual characteristics of the law.

In this dissertation, I focus only on limits that are legally enforceable, what I will refer to as “binding”. For revenue and expenditure limits, a legally enforceable limit is one that definitively constrains the actions of state legislatures.   The minimal definition of a "binding" limit is that it cannot be overridden by a majority vote in the state legislature. This determination is made through analysis of the letter of the law as well as anecdotal evidence of historical enforcement from newspapers or state records. Generally, a binding revenue or expenditure limit is Constitutional or otherwise governed by a supermajority clause.

An example of a binding, Constitutional TEL is California's Gann Amendment, the expenditure and revenue limit passed by the voters in 1979. Like all constitutional initiatives in California, the Gann Amendment can only be amended by a second initiative approved by the voters (or constitutional convention). As it happens, California voters have amended this particular limit. In 1988, Proposition 98 eliminated the component of the law that returned revenues in excess of the limit to the voters. As such, the Gann Amendment is coded as a binding revenue limit only from 1980 to 1988, and a binding expenditure limit from 1980 to the present.

Examples of binding, statutory TELs include Washington's 1993 expenditure limit and Oregon's 2001 expenditure limit. These two cases show that statutory limitations can have a variety of origins. The voters passed Washington's limit through the initiative process while Oregon's limit was passed in the state legislature. Both require a supermajority of the state legislatures to be overridden and are thus considered binding.

I turn now to a brief discussion of non-binding limitations. Previous research has mistakenly identified many non-binding TELs as binding. For example, many papers categorize North Carolina as having a binding TEL starting in 1991. My research found that this limit can and has been overridden with a majority vote of the state legislature regularly. Even more strikingly, this limit appears to have been largely forgotten by public officials and the media. In 1994 for instance, just three years after the TEL's initial adoption, the state legislature once again proposed a statutory spending limit, almost identical to the one still on the books. The very next year a similar plan was once again proposed, this time passing both houses, but not signed into law. In 1997 and 2000 statutory spending limits were floated once again, with nary a reference to the 1991 measure. Finally, in 2003, a second statutory limit was signed into law, only to be superseded by a slew of exemptions in 2006. The sheer prevalence of non-binding, statutory TELs makes identifying binding limits from non-binding ones a difficult task.

The fact that a TEL is constitutional is not a definitive characteristic of a binding TEL. For example, voters in both Tennessee and Texas passed constitutional spending limits that could none-the-less be overridden with a majority vote. These limits are also miscategorized in previous research.

There are several other ways a TEL is categorized as "non-binding" in my analysis. For example, Connecticut voters passed a 1992 referendum that required the state legislature to legally define crucial concepts of the law, such as what defines "spending". Because the legislature could never agree to these definitions, that specific initiative remains unimplemented even while a 1991 legislative limit is in force. I also consider limits that only constrain proposed, as opposed to actual, spending to be non-binding. For example, Nevada's 1979 limit only constrains the amount of spending that can be proposed by the governor. Since this recommendation may or may not resemble the final budget passed by the state legislature and signed by the governor, the limit is not considered binding.

Identifying Binding Property Tax Limits

Property tax limitations were identified in a manner similar to expenditure and revenue limits. I started my list of potentially binding property tax limits by compiling efforts of previous research.. Sources employed include ACIR 1995, Shadbegan 1998, Sexton (2003), Anderson (2006), the Lincoln Land Institute (2006), and Yuan et al. 2007. After compiling an exhaustive list from these sources, I again relied on textual analysis of the laws, judicial rulings, state newspaper reports, and various state government websites to analyze the degree to which the limits were legally binding and historically enforced. I also used these sources to code several variables pertaining to the letter of the law.

There are three types of property tax limits: revenue limits (also called "levy limits" or "rollback provisions"), assessment limits, and rate limits. A property tax revenue limit caps the cumulative level of property taxes levied by a taxing district. For example, Massachusetts's Proposition 2 ½ stipulates that municipal property tax coffers can only grow at a flat rate of 2.5% annually. Other property tax revenue limits may peg property tax growth to other indicators, but they all share the common feature of limiting the growth of property tax revenue at the level of a government or taxing district.

In contrast, assessment limits cap the growth of the assessed value of a landowner's property. This type of limitation generally applies to the level of an individual parcel, although some states cap the growth of the total, statewide assessed values of all properties. An infamous example of an assessment limit is California's Proposition 13. This limit stipulates that the assessed value of a parcel can only increase 2% annually (plus the value of major remodeling).

Finally, the third type of property tax limit is a rate limit. A rate limit caps the millage rate used to levy taxes. For example, California's Proposition 13 also included tax rate cap set at 1% of assessed value.

For the purposes of my study, I only focus on limits that necessarily restrict the growth trajectory of property taxes. Property tax revenue limits clearly meet this requirement. These limits peg growth to a fixed percentage, clearly defining how much revenues can increase for year to year.

On their own, however, neither rate limits nor assessment limits will necessarily constrain growth rates. While a rate limit will generally cause a reduction in property tax revenues the first year it is enacted, it does not control growth thereafter. This is because property taxes would still be able to grow as fast as the assessed value of a property. This growth rate could potentially be quite substantial, particularly in cases where assessed value is not the same as cash value. Similarly, a property tax rate limit, in and of itself, will not constrict the growth of property taxes. As long as lawmakers are able to set the tax rate as they please, an assessment limit by itself will not confine property tax growth from year to year. For these reasons, states must adopt both a property tax rate limit in conjunction with a property tax assessment limit to be coded as implementing property tax limits in the analysis that follows. It does not matter if the state adopts these two limits contemporaneously.

In addition to limiting the growth of property taxes, all limits included herein must be classified as binding. To identify a binding property tax limit, I verified that the limit was being enforced on all relevant localities, could not be overridden with a majority vote of the locality's governing body, and was not a "local option" (only enforced if adopted by the locality). Because property tax limits are passed by state legislatures and enforced on local governments, many statutory laws will count as binding.

To explain my coding of "binding" property tax limits, I will describe by illustrative example the limits that did not meet my standards. As previously mentioned, I exclude all TELs that can be overridden by a majority vote of a local government body. This was the case for Mississippi's 1980 limit. In this year, the Mississippi legislature adopted a statute that limited taxing entities from increasing property taxes by more than 10 percent relative to any of the preceding 3 years. Up until 1994, however, local governments could easily override the limit by a majority vote of their governing body. This changed in 1994 when the law was amended to only allow override with a popular referendum. As such, I code Mississippi as having a binding property tax limit only after this amendment.

Limits that are not self-enforcing are also considered non-binding in my analysis. For example, Texas passed a 1978 property tax limit that required all taxing entities to "rollback" the tax rate if it would increase revenues more than eight percent. This limit, however, is not automatically enforceable. In order to enforce the rollback provision, voters were required to bring forward (and pass) a referendum in opposition to the tax hike. Because the burden to take action to enforce the limit is on the people, I do not include this limit as binding. Likewise, some property tax limits are passed by state legislatures as a "local option". This means that the limit is not enforced on a municipality unless passed by the local governing body or voters.

The cases of Massachusetts and New Jersey also illustrate my coding of binding versus non-binding property tax limits. The property tax limits analyzed in this dissertation must meet a standard of constraining at least half of the property taxes collected in a given state at the time of adoption. This important because some property tax limits only affect certain types of governments. For example, Massachusetts' 1980 property tax limit only limits municipal property taxes. However, because municipal property taxes comprised more than half of all Massachusetts' property taxes at the time of adoption, I include this limit as a binding property tax limit. In contrast, New Jersey's

1980 measure only limited counties. At the time, county property taxes comprised less than a quarter of state and local property taxes. Because this limit would have a limited affect on overall property taxes, I exclude this limit from my analysis. Finally, I also exclude limits that only affect a small segment of the population, like the elderly or veterans.

Dependent Variables

My dependent variables are measures of state and local fiscal behavior. The specific fiscal outcome employed for each model follows the type of TEL. In other words, expenditures are the dependent variable for the analysis of spending limits while property tax revenues and revenues are used for the analysis for property tax limits and revenue limits. I obtained fiscal variables through annual editions of the U.S. Census Bureau's *State and Local Government Finance* publication. In particular, I have chosen to analyze state and state and local direct general expenditures, state and local general own-source revenues, and state and local property taxes. I deflated all estimates by the consumer price index and converted them to 2006 dollars.[1]

There are theoretical and empirical advantages to using state and local fiscal behavior rather than state-only fiscal behavior. First, almost half of all TELs adopted in the U.S. include provisions that limit both levels of government. Second, if the result of a TEL is to push fiscal burdens down to lower levels in order to substitute local spending or

[1] The census department did not collect local fiscal outcomes for the years 2001 and 2003. I employed multiple imputation to estimate state and local fiscal outcomes for these years. Predicting variables included multiple leads and lags, economic variables (state personal income, labor force), and state fiscal outcomes.

revenue for states dollars, this consequence is not in accordance with the spirit of tax and expenditure limits, even if it technically follows the letter of the law. I expect this sort of substitution to be typical if the TEL makes doing so possible. Third, using both state and local estimates eliminates measurement error due to accounting changes that shift finances between governments. For example, a large proportion of property taxes in Vermont are reported as state revenues, not local revenues starting in 1999. This change does not reveal substantive changes in tax burdens and would only constitute measurement error if only state or only local fiscal outcomes were analyzed.

State-by-state data for local government fiscal outcomes are unavailable for the years 2001 and 2003. Correspondence with the Census Department indicated that resource constrains guided the decision not to collect data for these two years. To achieve a balanced data set, it was therefore necessary for me use multiple imputation to estimate state and local totals for 2001 and 2001. To impute these years using a lead and lag of state and local fiscal outcomes, state fiscal outcomes, population, and personal income. These five variables have high predictive capabilities and I thus feel confident employing imputed data for these years.

Covariates

The covariates employed in this analysis are state demographic, economic, and political characteristics. Specifically, I employ three economic variables: personal income, total employment, and average home values. *Personal income* is the total personal income statewide deflated by the consumer price index. The source of this variable is the Bureau of Economic Analysis. Personal income is expected to be positively associated with state and local fiscal outcomes. *State and local total employment* is the number of full-time workers statewide. The source for this data is the Bureau of Labor Statistics. Employed is also expected to be positively associated with government spending. Finally, home values are included as a control variable for analysis of property tax limits. Home values are estimated from a combination of sale-price data and Census estimates. The source of this data is Davis and Heathcote 2007 and it is available from the Lincoln Land Institute. Unlike my other covariates, this variable is available only starting in 1975.

I employ three demographic covariates: total population, elderly population, and school-age population. All estimates are from the Census Bureau's annual population estimates. The *elderly population* is defined the number of persons 65 years or older. I expect a large elderly population to be negatively correlated with both government spending and property tax collections. S*chool-age population* is the number of persons ages five to nineteen. School age population is expected to be positively associated with both state fiscal outcomes, as is total population.

Finally, I employ political variables based on the partisan composition of state government. Unified Republican control and Unified Democratic control are dichotomous variables coded as one if the relevant political party controls a majority in both houses of the state legislature and the governor's office. Unified Republican control is expected to be negatively associated with government spending, whereas Unified Democratic control is expected to be positively associated with government spending.[2] In some models, I will also employ a dichotomous variable on the partisan composition of the Governor's Office. The source for this data is the Council of State Governments' *Book of the States.*

Outline of the Dissertation

The first chapter of my dissertation analyzes the effectiveness of tax and expenditure limits empirically. This chapter is an adaptation of a co-authored paper with Thad Kousser and Mat McCubbins. The theory about the effectiveness remains the same, but the empirics are different. My chapter pays closer attention to pitfalls of time-series, cross-sectional data. Specifically, I account for violations of Guass-Markov caused by serial correlation or heteroskedasticity. I also take greater advantage of flexible TEL indicators to test for temporary and heterogeneous effects.

In the second chapter I leverage two conditions previously shown to produce successful delegation to agents: compatible incentives between principals and agents and ease of monitoring agents. I apply each condition to the case of Tax and Expenditure Limits (TELs) and test whether or not limits are more effective when these conditions are met. My results show that tax and expenditure limits are more successfully when implemented by agents that share ideological convictions for cutting the size of government. I also present suggestive evidence that making limits easier to monitor, by tying limits to individual not aggregate tax burdens, makes them more effective.

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[2] Unless otherwise noted, Nebraska's unicameral, nonpartisan legislature is treated as a divided government.

My third chapter focuses on the secondary and tertiary consequences of tax and expenditure limits. Specifically, I present evidence that property tax limits have detrimental effects on state and local revenues during recessions. Property tax limits cause states to rely on income–elastic revenue sources, such as the income tax or charges and fees. Greater reliance on these revenue sources results in greater revenue declines during economic downturns. My results suggest that states would have fewer and more modest financial problems during economic downturns if they did not enact property tax limitations.

Finally, my fourth chapter is a case study of Massachusetts' 1980 Property Tax Limit, Proposition 2 ½. This initiative limits municipal property taxes to growth by 2.5% per year. In this chapter I look at the formula used to calculate the limit, highlighting how changes to this formula over time have made the limit less effective. I also analyze the extent to which revenue substitution, such as increases in state aid and charges and fees, can e attributed to the property tax limit.

Chapter 1
The Effectiveness of TELs

I. Introduction

Tax and expenditure limits (TELs) belong to a general class of political phenomena that attempt a tough trick: locking in the preferences of a set of political principals by constraining the future actions of potentially unknown and hostile agents. Either voters are trying to limit state lawmakers, or legislators in one era are attempting to slow the growth of government under future lawmakers. Regardless, the proponents of these limits face the common delegation problem, made especially challenging by the fact that they are trying to constrain the behavior of agents long into the future, when they may be unable to monitor their actions.

This challenge is similar to the dilemma faced by legislators attempting to control the executive branch (McKelvey and Ordeshook 1984; Shepsle and Weingast 1984; McCubbins et. al 1987, 1989; Kiewiet and McCubbins 1991; Lupia and McCubbins, 1998; Epstein and O'Halloran 1999; Huber and Shipan 2002), legislators on the floor delegating power to committees (Fenno 1973; Krehbiel 1991; Rohde 1991; Aldrich and Rohde 1998, 2000; Cox and McCubbins 1993, 2005), members of Congress trying to discipline the budgetary decisions of future Congresses (Schick 1995, 2005), and voters giving over power to elected officials (Gerber et al 2001, 2004).[1]

Tax and expenditure limits fall into this troublesome category because lawmakers charged with implementing a limit may be hostile to the goals of its backers. Why else

[1] On agency in general, see for example Ross (1973). Holstrom (1979), Grossman and Hart (1983), and Bernheim and Whinston (1986).

the need for a limit in the first place? I suspect that the lawmakers subject to a tax and expenditure limits may be canny operators with sometimes demanding constituencies who may want to see government grow at a faster rate than the limit proscribes. Lawmakers may have the ability to circumvent limits in ways that are buried deep in the details of thousand-page budget documents. Because it is difficult to monitor state fiscal actions, the initiative proponents who sponsored the TELs may be unable to follow their implementation.

My main conjecture is that principal-agent problems will prevent tax and spending limits from having their intended effect of reducing the growth of state government.[2] This prediction stands in contrast to the empirical findings of many previous works, such as Misiolek and Elder (1988), Elder (1992), Shadbegian (1998), Bails and Tieslau (2000), and New (2001, 2010). However, I suspect that much of the previous scholarship that finds TELs to be effective may be based on the flawed statistical modeling. Time-series, cross-sectional political economy data is oftentimes fraught with Guass-Markov violations, a fact particularly troublesome in the presence of dichotomous treatment variables and fixed effects. I design my empirical strategy to respond to this challenge.

My consistent finding is that TELs are almost never permanently effective. In contrast to much of the previous literature, my results do not prove sanguine for TEL backers, and support my conjecture that they rarely overcome their principal-agent problem. I begin this analysis with a brief overview of the previous literature. I then turn to analysis of the common pitfalls of political economy time-series, cross-sectional data

[2] Wildavsky (1980). Smith (1998) demonstrated that this was the intent of the TEL enactors.

and a discussion of endogeneity. Finally, I estimate the effectiveness of TELs using several empirical models to test for a uniform and temporary treatment effects.

II. Previous Research

Previous studies explore the effects of TELs by making cross-state comparisons, often supplemented by multiple observations of each state's fiscal activities over time (Abrams and Dougan 1986; Misiolek and Elder 1988; Elder 1992; Shadbegian 1996; Mullins and Joyce 1996; Shadbegian 1999; Bails and Tieslau 2000; New 2001, 2010; Mullins 2004). Similar to the first approach taken in this study, previous research typically regresses some measure of a state's fiscal behavior upon a dichotomous variable indicating the presence in each state of their institution of interest (a treatment) as well as a set of covariates or "control factors" that are meant to make the states in the cross-state comparison actually comparable. They interpret the coefficient on their treatment variable as the estimated effect of the TEL.

For example, Elder (1992) regresses taxes collected on dummy variables indicating expenditure limits and revenue limits, along with a vector of control variables for the years 1950-1985. He finds differential effects based on the type of limit enacted, noting that "States with Revenue limitation laws have experienced no change in tax growth, whereas there is strong evidence of a reduction of tax growth in states with expenditure limitations" (Elder 1992, p. 58). Bails and Tieslau (2000) use a similar panel research design for the years 1969-1994. The coefficient on their TEL indicator variable is significant and negative, indicating to them "that real per capita state and local spending in states that have a tax or spending limit in place will be more than $41 lower than in those states that do not have such limitations in place" (Bails and Tieslau 2000, p. 270). New (2001, 2010) has also found significant and negative results for a subset of cases passed by citizen initiative.

As noted, the use of OLS regression on time-series, cross-sectional data is common in the literature on tax and expenditure limits. I contend that the weakness of these analyses is that they have ignored the important pitfalls of time-series, cross-sectional data. In particular, little attention has been paid to how serial correlation of errors can bias estimated findings. Despite publishing findings with R-squareds as high as 0.99, few authors have even mentioned serial correlation as a potential source of bias. I address this issue at length in this chapter and present several diagnostic tests of various specificatiosn.

Interestingly, studies that have relied on other, often simpler, methodologies have come to opposite conclusions on the effectiveness of TELs. Howard (1989) looks at changes in the ratio of state taxes collected to personal income before and after implementation of a TEL. She finds no significant alterations in this ratio both when pooled between all TEL states and within single states in comparison to non-TEL states. Similarly, Bails (1990) takes the average percent change of revenue and expenditures for TEL states and non-TEL states. Using a difference of means test, he does not find a significant difference between the two. Stansel (1994) compares state fiscal activities before and after TEL passage, finding that some measures seem to work while others fail. Though the empirics of these efforts are relatively straightforward, they avoid some of the challenges posed by TCSC political economy data.

Regardless of the approach and techniques used, the literature is mixed on whether TELs do or do not limit revenues and expenditures. I believe that this topic is important enough to merit renewed attention with increased consideration of research design. This topic is important because the effectiveness of tax and expenditure limits – often passed via direct democracy – fits in with a larger controversy in the literature on the initiative process.

Currently, there is a significant debate between those who think that the initiative process is an effective check on the legislature and helps move policy to the median voter's preferences (Bowler & Donovan 1998; Gerber 1996, 1998, 1999; Lupia and Matsusaka 2004; Matsusaka & McCarty 2001), and those who think that initiative victories are ephemeral and that the devil is in the implementation (Gerber et al. 2001, Bali 2003, Gerber et al. 2004; Kousser and McCubbins 2005; Garrett and McCubbins 2008). Since many TELs have been initiated or endorsed by voters, my analysis here can contribute to this larger debate.

III. Estimation Issues

The workhorse method employed throughout this paper is a model of difference-in-differences. Difference-in-differences analysis is the most widely used econometric technique for observational studies of policy impacts (Wooldridge 2002). In difference-in-differences estimation, one compares the difference in outcomes before and after the policy intervention for groups that were given the policy treatment to the same difference for unaffected groups. The difference-in-differences model is identified by the inclusion of both unit and time fixed effects. Unit fixed effects control for state-specific, time-invariant unobservable characteristics, in practice by providing a unique intercept term for each state. Time effects control for unobservables affecting the dependent variable that are common to all states from year to year. This technique allows me to compare the growth patterns of states with TELs to with those without TELs both before and after adoption, conducting both a pre-test and a post-test for treatment and comparable groups.

The state and local budgetary data employed in this paper covers 49 states and 37 years. The benefit of time-series, cross-sectional data is that I am able to draw inferences from both within and between states. This rich variation has the potential to draw out the precise effect of a policy intervention. A consequence of this data structure, however, is that it is easy to produce violations the OLS Gauss-Markov assumptions. In particular, TSCS data typically suffer from serial correlation, cross-sectional correlations, and panel heteroskedasticity (Greene 2000, Beck and Katz 1995). In this section I employ diagnostic tests to evaluate the validity of the assumptions that I implicitly make about the error terms in my models. These tests will determine the validity of my empirical specifications and guide me to choose the best model possible. My close attention to empirical pitfalls is a critical omission in the previous literature testing the effects of TELs.

The diagnostic tests employed herein rely on a variety of model specifications. Specifically, I test for models of state fiscal outcomes in 1) levels 2) logged-levels 3) logged-levels with a lag dependent variable 3) logged-levels with two lag dependent variables 4) first differences 5) first-differences of logs and 6) first differences of logs with a lag dependent variable. Notably, these six specifications contain four different specifications of the dependent variable.

Dynamics

Budget making is a dynamic process. Following the advice of Beck and Katz (1995), I will address concerns regarding dynamics first before tackling other problems common in panel data, such as heteroskedasticity.

Stationarity

I start my discussion of the problems associated with dynamic processes by examining whether or not my data is stationary. A stationary variable is one whose mean and variance does not vary over time. Stationarity is a critical assumption in OLS. As first shown by Granger and Newbold (1974), OLS using non-stationary data can result in a "spurious regression", wherein the r-squareds are high and the standard errors are low, but the results are completely spurious.

While political economy data generally trends upward over time (and is thus rarely mean-reverting), transforming the data may make it stationary. A variable is trend-stationary if it can be made stationary by simply accounting for a linear time trend. A variable is difference-stationary if it is made stationary by taking the first difference. Most economic variables will, in the least, be difference-stationary (Beck and Katz 2009).

For many economic dependent variables, the answer to the question of whether or not the variable is stationary (or trend stationary) remains open. To this day, scholars continue to debate whether or not the most commonly used economic measure, Gross

Domestic Product, is a stationary variable (Nelson and Plosser 1982, Perron 1989, Cheung and Chinn 1997, Murray and Nelson 2000). Many scholars argue that most budget data is likely non-stationary (Payne 1998; Chowdhury 1998, Keele and Kelly 2006), while others contend that we simply do not have enough years of data to make a final conclusion (Beck and Katz 2009). In general, the low power of tests for non-stationary are said to lead to an under-rejection of the null hypothesis of stationarity.

I employ two tests designed for TSCS data to determine whether or not my dependent variables are stationary. One test follows the method described by Im, Pesaran, and Shin (2003) and the other from Levin, Lin, and Chu (2002). For both methods I employ two lags. Given the state of the current literature, much of which has indicated that state and local fiscal data is only difference-stationary, I was surprised to find that logged state and local budget data from 1970-2006 appear largely to be trend-stationary. Although this data does appear to be stationary, it is still important to discuss other issues related to dynamics, particularly serial correlation, the topic to which I now turn.

Serial Correlation

State and local finance data, like most political economy data, is subject to significant autocorrelation. Serial correlation is problematic because it can underestimate standard errors, resulting in type one errors (over-rejection of the null). This problem is acute for difference-in-differences estimation (Bertrand et al. 2004). An influential study by Bertrand et al. tested the affect of randomly selected placebo policy interventions on political economy data in the US states. The startling result of this work was that, as a consequence of serial correlation, 45% of the placebo interventions were found to have significant correlations with the dependent variables. These false positives are a concern to my analysis and perhaps explain why so many previous studies have found significant affects of TELs.

The problem of serial correlation is most acute when the data is untransformed. Figure 1. 1 illustrates the existence of serial correlation in a basic fixed effects regression of levels of spending on a variety of standard covariates (also in levels). The figure plots the errors over time for only the state of California. As is evident from the figure, the errors are temporally correlated, clearly violating the OLS Gauss-Markov assumptions.

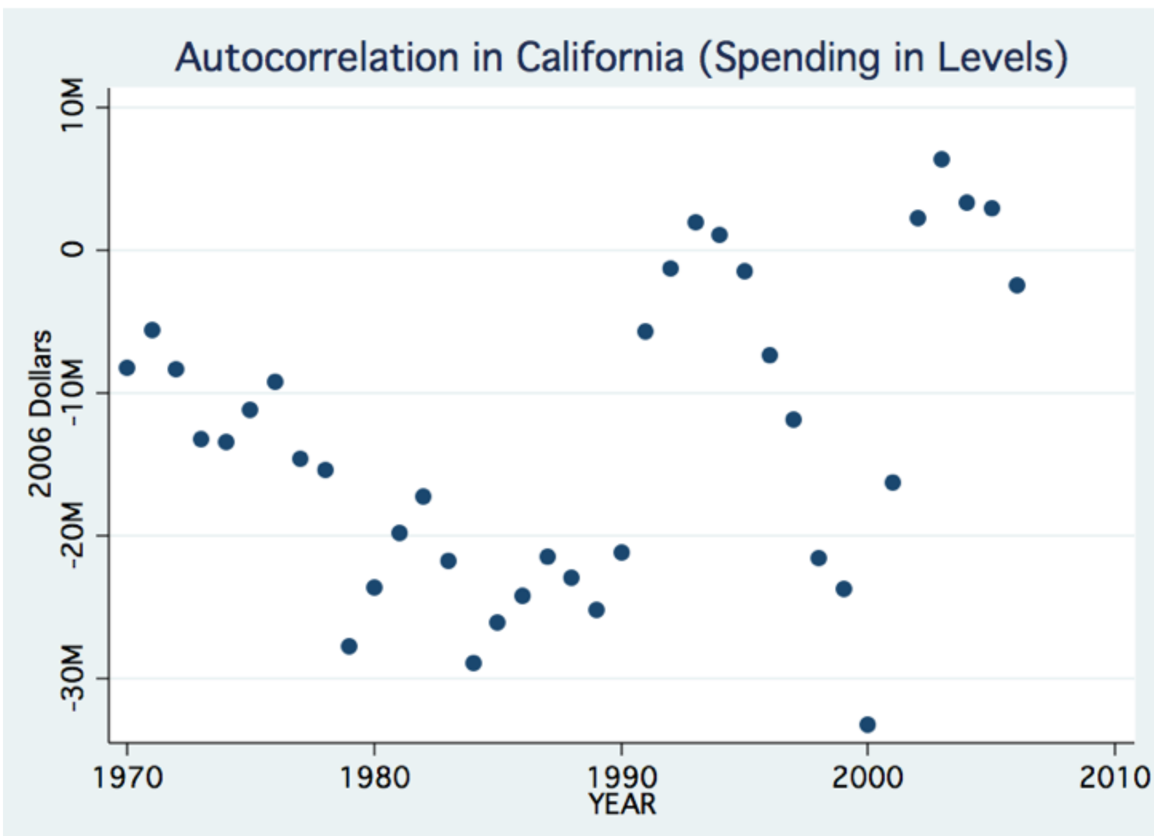

Figure 1.1: Serial Correlation from OLS Regression on Spending in Levels in California

Visual inspection of state-by-state correlograms, suggest that most state fiscal outcomes (expenditures, revenue, and property taxes) follow an AR1 process. Of my three dependent variables, general revenues were least likely to follow an AR1 process. For this dependent variable, a handful of states resembled AR2 processes, while a few had third-order serial correlations. Despite this heterogeneity, in the section that follows I attempt to diagnose serial autocorrelation using a pooled TSCS framework. The method employed herein largely follows the recommendations of Beck and Katz (1995, 1996, 2009).

I test for serial correlation using the Breusch–Godfrey Lagrange Multiplier test for serial correlation, what is otherwise simply referred to as an LM Test. In practice, this test regresses the residuals from an OLS regression on its lagged residuals as well as all original independent variables. A chi-squared test statistic is calculated from the R-squared of that regression in conjunction with the number of observations. The degrees of freedom for evaluating the test-statistic depend on the order of serial correlation. In practice a test-statistic greater than 3.81 is large enough to reject a null hypothesis of no first order serial correlation.

The results from the LM Test on the various specifications of my models appear at the bottom of Tables 1.1 through 1.3. As previously mentioned, I employ six model specifications for each dependent variable. It is important to note that the continuous-level independent variables follow the specification of the dependent variable. For example, if the dependent variable is logged, continuous variables such as personal income are also logged. Similarly, if the dependent variable is differenced, so too are all continuous-level variables. My non-continuous variables, most notably my key treatment variable, the presence of a TEL, but also the political variables measuring unified control of government, are never differenced. This allows me to test for the long-term affect of these variables, not just the immediate affect when a regime change occurs.

Table 1.1: Diagnostics for OLS Regressions on State and Local Spending

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLES	Spending	LN Spending	LN Spending	LN Spending	Δ Spending	Δ LN Spending	Δ LN Spending
Spending Limit	-530173 (289995)*	-0.018 (0.006)***	-.005 (0 .003)*	-0.004 (0.003)	38791 (115053)	-0.002 (0.003)	-0.002 (0.003)
Total Population†	-6.111 (0.612)***	1.431 (0.121)***	-0.046 (0.064)	0.017 (0.065)	9.67 (1.01)***	1.404 (0.193)***	1.349 (0.195)***
Total Employment†	0.33 (0.724)	-0.224 (0.049)***	0.089 (0.025)***	0.067 (0.026)***	1.89 (0.53)***	0.312 (0.072)***	0.311 (0.072)***
Unified Democratic Control	383831 (216925)*	0.003 (0.004)	0.002 (0.002)	0.002 (0.002)	146033 (85559)*	0.002 (0.002)	0.002 (0.002)
Unified Republican Control	-47184 (171280)	0.001 (0.003)	-0.0006 (0.0017)	-0.0003 (0.002)	9954.32 (68867)	-0.001 (0.002)	-0.0006 (0.002)
School-Age Population†	11.85 (1.062)***	-0.390 (0.056)***	-0.03 (0.029)	-0.045 (0.029)	9.57 (1.83)***	-0.104 (0.107)	-0.094 (0.107)
Elderly Population†	11.03 (1.696)***	-0.268 (0.029)***	-0.027(0.015)*	-0.024 (0.015)	22.77 (6.11)***	-0.550 (0.135)***	-0.535 (0.135)***
Personal Income†	0.261 (0.005)***	0.362 (0.036)***	0.19 (0.018)***	0.169 (0.019)***	0.03 (0.01)***	-0.016 (0.034)	-0.012 (0.034)
Lag DV			0.82 (0.011)***	0.924 (0.024)***			0.039* (0.023)
2nd Lag DV				-0.115 (0.023)***			
Constant	-2E+06 (625328)***	0.712(0.257)***	-0.279 (0.130)**	-0.2 (0.132)	279498 (179627)	0.020 (0.005)***	0.019 (0.005)***
Observations	1813	1813	1813	1764	1764	1764	1764
R-squared	0.962	0.976	0.994	0.994	0.42	0.45	0.458
Number of States	49	49	49	49	49	49	49
F (State Effects)	83.16***	122.95***	7.05***	6.52***	7.24***	0.8	0.74
F (Year Effects)	7.85***	44.92***	31.61***	28.72***	8.91***	27.72***	26.49***
LM (AR1), X2 (1)	1591.5***	1621.87***	398.39***	20.65***	221.62***	0.83	2.499
Mod. Wald (GH), X2 (49)	24735.32***	1430.19***	130000***	260000***	17787.18***	508.33***	539.85***

Standard errors in parentheses. Dependent Variable is State and Local Direct General Expenditures.

**** p<0.01, ** p<0.05, * p<0.1*

† This continous-level variable follows the specification of the DV. For instance, if the DV is first difference, this variable will also be first-differenced.

Table 1.2: Diagnostics for OLS Regressions on State and Local Revenue

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLES	Revenue	LN Revenue	LN Revenue	LN Revenue	Δ Revenue	Δ LN Revenue	Δ LN Revenue
Revenue Limit	-1.8E+06(267993)***	-0.023(0.009)**	-0.001(0.005)	-0.002(0.005)	31114 (145547)	-0.001(0.005)	-0.001(0.005)
Total Population†	-3.458(0.367)***	1.177(0.130)***	0.036(0.068)	0.049(0.069)	1.184 (0.861)	1.478(0.211)***	1.593(0.212)***
Total Employment†	-0.184(0.437)	-0.246(0.053)***	0.020(0.027)	0.019(0.028)	0.926 (0.446)**	0.126(0.078)	0.151(0.078)*
Unified Democratic Control	137722(130760)	0.001(0.005)	0.003(0.002)	0.003(0.002)	37880 (72163)	0.003(0.002)	0.003(0.002)
Unified Republican Control	19427(103283)	-0.005(0.004)	-0.004(0.002)***	-0.004(0.002)*	44960 (58126)	-0.004(0.002)*	-0.004(0.002)*
School-Age Population†	4.849(0.641)***	-0.292(0.061)***	-0.063(0.031)**	-0.069(0.031)**	5.40 (1.54)***	-0.097(0.118)	-0.092(0.117)
Elderly Population†	11.16(1.029)***	-0.227(0.032)***	-0.022(0.016)	-0.023(0.017)	2.27 (5.16)	-0.397(0.147)***	-0.453(0.147)***
Personal Income†	0.191(0.003)***	0.548(0.039)***	0.214(0.020)***	0.207(0.021)***	0.125 (0 .005)	0.112(0.037)***	0.114(0.037)***
Lag DV			0.813(0.012)***	0.839(0.024)***			-0.091(0.024)***
2nd Lag DV				-0.026(0.023)			
Constant	-1.5E+06 (377375)***	-0.766(0.275)***	-0.528(0.140)***	-0.477(0.143)***	-123698 (149664)	0.026(0.005)***	0.029(0.005)***
Observations	1813	1813	1813	1764	1764	1764	1764
R-squared	0.977	0.972	0.993	0.992	0.57	0.4	0.405
Number of States	49	49	49	49	49	49	49
F (State Effects)	120.81***	101.68***	5.79***	5.78***	1.92***	0.65	0.77
F (Year Effects)	4.56***	18.17***	21.31***	19.15***	5.27***	15.28***	15.69***
LM (AR1), X2 (1)	1 601***	1594***	327.39***	82.46***	7.16***	3.83**	0.999
Mod. Wald (GH), X2 (49)	39499***	2059***	12000***	24000***	28304***	928.70***	977.71***

Standard errors in parentheses. Dependent Variable is State and Local Own-Source General Revenues

**** $p<0.01$, ** $p<0.05$, * $p<0.1$*

† This continous-level variable follows the specification of the DV. For instance, if the DV is first difference, this variable will also be first-differenced.

Table 1.3: Diagnostics for OLS Regressions on State and Local Property Tax

	Property Taxes	LN Property Taxes	LN Property Taxes	LN Property Taxes	Δ Property Taxes	Δ LN Property Taxes	Δ LN Property Taxes
Property Limit	-811738 (99998)***	-0.117 (0.010)***	-0.0144 (0.004)***	-0.0139 (0.005)***	-49752.8 (44894.04)	0.002 (0.005)	0.002 (0.004)
Total Population†	-1.164 (0.253)***	2.607 (0.243)***	0.34 (0.112)***	0.356 (0.114)***	2.698809 (0.481)***	1.823 (0.348)***	1.992 (0.350)***
Total Employment†	-1.657 (0.300)***	-1.164 (0.098)***	-0.063 (0.046)	-0.049 (0.047)	2.607906 (0.251)***	-0.227 (0.129)*	-0.277 (0.130)**
Unified Democratic Control	-111954 (90086)	0.011 (0.009)	-0.002 (0.004)	-0.002 (0.004)	48837.16 (40557.23)	-0.003 (0.004)	-0.004 (0.004)
Unified Republican Control	123991 (71136)*	-0.005 (0.007)	-0.004 (0.003)	-0.004 (0.003)	11005.09 (32619.88)	-0.004 (0.003)	-0.004 (0.003)
School-Age Population†	3.913 (0.441)***	-0.872 (0.113)***	-0.191 (0.051)***	-0.205 (0.052)***	3.29 (0.867)***	-0.042 (0.194)	-0.048 (0.193)
Elderly Population†	7.012 (0.702)***	-0.299 (0.059)***	-0.0646 (0.026)***	-0.0727 (0.027)***	2.26 (2.889)	-0.477 (0.243)*	-0.544 (0.243)**
Personal Income†	0.037 (0.002)***	0.689 (0.073)***	0.124 (0.033)***	0.114 (0.034)***	0.015 (0.003)***	-0.136 (0.062)**	-0.146 (0.062)**
Lag DV			0.866 (0.010)***	0.833 (0.024)***			-0.0922 (0.024)***
2nd Lag DV				0.033 (0.024)			
Constant	1.20E+06 (261451)***	-4.426 (0.506)***	-1.032 (0.230)***	-0.997 (0.238)***	359289 (109575.8)	0.033 (0.009)***	0.0364 (0.009)***
Observations	1813	1813	1813	1764	1764	1764	1764
R-squared	0.68	0.86	0.97	0.97	0.18	0.27	0.27
Number of States	49	49	49	49	49	49	49
F (State Effects)	51.61***	274.43***	4.49***	4.24***	1.52**	0.83	0.98
F (Year Effects)	5.3***	15.83***	16.37***	14.6***	3.34***	10.10***	10.54***
LM (AR1), X2 (1)	1626.85***	1685.33***	283.83***	332.37***	7.16***	15.1***	17.33***
Mod. Wald (GH), X2 (49)	130000***	6889.29***	52971***	140000***	73340.77***	3176***	3093***

Standard errors in parentheses. Dependent Variable is State and Local Property Tax Collections

**** p<0.01, ** p<0.05, * p<0.1*

† This continous-level variable follows the specification of the DV. For instance, if the DV is first difference, this variable will also be first-differenced.

As evident from the LM tests, simple OLS models of levels of fiscal outcomes, both logged and untransformed, suffer from severe autocorrelation. The problem is evident not only in the enormous test-statistic of the LM Test, but also the artificially inflated R-squareds of the models. This severe level of autocorrelation suggests that the estimates derived from these models should not be trusted.

Models 3-7 in Tables 1.1 through 1.3 attempt to model the dynamic process in this data. The methods employed include adding lag dependent variables and first differencing. Additionally, I will discuss employing Newey-West standard errors. I will explain each of these methods in turn, addressing the strengths and weaknesses of each method.

The first solution, employing a lag-dependent variable, is motivated by Beck and Katz (1995, 1996, 2009). A lag dependent variable is useful in modeling dynamics because it generally eliminates serial correlation mechanically so that standard errors can then be estimated properly. Theoretically, a lag dependent variable is suitable when there is an expectation that the maximal effect of a shock is mostly immediate, but with some monotonically declining consequences thereafter. I believe this to be a reasonable characterization to how state and local governments respond to financial shocks in most circumstances. In addition, a lag dependent variable is suitable when the outcome from previous years is a substantive consideration for decision makers in subsequent years. In this case, employing a lag dependent variable makes sense if lawmakers use last year's budget as a guide for determining this year's budget, making incremental changes instead of starting from scratch year-to-year.

Despite the theoretical grounds for using a lag dependent variable, this method by itself fails to fully remove serial correlation in the error terms. A variety of research suggests that residual serial correlation is highly problematic for OLS regressions employing lag dependent variables (Greene 2000, Achen 2000, Keele and Kelly 2006). In the presence of residual correlation, a lag-dependent variable can cause biased and inconsistent results. This problem is compounded by the fact that employing a lagged dependent variable in a highly autoregressive fixed effects model produces additional bias (Baltagi 2001). As warned by Kittel and Wittner 2005, in these cases, "the substantive interpretation of the coefficients and their standard errors is, as such, meaningless" (Kittel and Wittner 2005, p. 278).

Beck and Katz 1996 suggest that residual serial correlation can be accounted for by using a second lag dependent variable. The LM Tests for Model 4 of Tables 1.1 through1.3, suggest that this method does not remove remaining serial correlation for any of the dependent variables. In addition, Kittel and Wittmer show that using additional lag dependent variables increases the bias of estimates in the presence of large autoregressive parameters (Kittel and Wittner 2005). For these reasons, this method of correcting for residual serial correlation will be abandoned.

A second method of handling dynamics is estimating Newey-West standard errors. The Newey-West technique simply adjusts the size of the standard errors without consequence to the coefficients of the estimate. This method is consistent for both undetermined autocorrelation as well as heteroscedasticity.

One consideration in the Newey-West correction is the threat of omitted variable bias. Again, the Newey-West method simply adjusts the size of the standard errors and does not directly model the dynamic process that is occurring. If the data generating process is one where the previous year's outcome substantively affects the outcome of the next year, omitting a lag dependent variable becomes a specification error that can result in omitted variable bias (Keele and Kelly 2005, Beck 2004).

To determine whether this is a relevant consideration in my model, I follow Keele and Kelly (2005) and look at the size of the coefficient of lag dependent variable, what they refer to in their notation as α. As noted by the authors, "while OLS without an LDV utilizing Newey-West standard errors may be best when α is 0, if α is non-zero, OLS without an LDV will be biased due to an omitted variable. In short, if $\alpha \neq 0$, omitting the lag of Yt is a specification error, and the bias due to this specification error will worsen as the value of α increases" (Keele and Kelly 2006, p. 10). The value of the lag dependent variables in Model 3of Tables 1.1 through 1.3 range from 0.81-0.86. These high values suggest that this solution to serial correlation may cause additional bias in my estimates.

Finally, a third option for removing serial correlation is first-differencing the data. A first-differenced model simply regresses changes of X on changes of Y. The results of my diagnostic tests suggest that first-differencing is sometimes very successful at removing serial correlation. First and foremost, the success of first-differencing depends on the specification of the dependent variable. Specifically, first-differencing does not remove serial correlation if the data is untransformed, this is true regardless of whether or not a lag dependent variable is also employed. In contrast, first-differencing is largely successful when the data is log-transformed. A first-differenced model of logged fiscal outcomes can be interpreted as a model of growth rates. I believe this to be an appropriate for my topic theoretically since I define TELs as measures aimed at limiting the growth of government.

The diagnostic tests suggested that a first-differenced, log transformed model successfully removes all residual serial correlation in the case of expenditures, and most of the residual serial correlation in the case of revenues. However, this method is unsuccessful for property taxes. This suggests that some first-differenced models will still require additional precautions against violations of Guass-Markov caused by dynamics. For example, a lag dependent variable or Newey-West standard errors could also be applied to a first-differenced model. I will return to these options at the end of this section.

I have so far reviewed three methods for modeling the dynamic process in state and local budget data: lag dependent variables, Newey-West standard errors, and first-differencing. No single method proved to be a panacea for the problems associated with serial correlation. The application of lag dependent variables failed to remove residual correlation, a consequence that can result in biased estimates. Newey-West standard errors fail to take account of how budgets are actually made, consequentially permitting a second form of bias from an omitted variable. First-differencing was successful in some, but not all, specifications.

Moving forward, my preferred option for modeling dynamics is a first-differenced model of log-transformed data. The first-differenced model has several benefits. First, by modeling changes instead of levels, this specification inherently takes into account that lawmakers use previous years' budgets as the starting point for drafting new budgets. Second, first-differences in conjunction with natural logs provide a way to measure the growth rates of fiscal outcomes before and after the adoption of a TEL. This is an appropriate choice because tax crusaders wanted to limit the growth of government. Finally, the diagnostic tests show that first-differencing completely eliminates the problem of serial correlation for at least one of my dependent variables, state and local expenditures. To tackle the residual correlation in state and local revenues and property taxes, it will be possible for me to employ either a lag dependent variable or Newey-West standard errors.

Indeed, adding a lag dependent variable to my model of state and local revenues successfully removes all serial correlation for revenues. Once the data is first differenced, the coefficient of the lag dependent variable is significantly closer to zero than that in a model of levels. Intuitively, this makes sense. The growth rate of budgets from year to year is not as dependent on each other as the levels of budgets from year to year. This implies that Newey-West standard errors would also be an adequate solution

It is noteworthy, however, that I fail to correct for the serial correlation in property taxes even after first-differencing the data and employing a lag (or even two lag) dependent variables. The LM test reveals a high level of residual correlation. Specifically, when lagged residual is used to predict residuals, the first lag has a large, negative coefficient (-0.86) and is strongly significant. These results suggest that this dependent variable comes from a different data generating process than either expenditures or revenues. The residual correlation from property taxes reveals that this dependent variable has first-order positive serial correlation, but residual negative serial correlation.

This unusual residual correlation may be caused by property tax collecting practices. Specifically, many states have laws that govern the regularity of property reassessment. For example, the state of Ohio runs on a three-year reassessment cycle. Every three years local assessors (or, more likely, contracted assessors) adjust the assessed value of the home, either from a home-inspection or by using data on local market values. The consequence of this reassessment cycle is that assessed home values lay stagnant in-between assessment cycles. If the locality tax rate is flat, perhaps limited by law or otherwise sticky, the reassessment cycle will result in big, positive changes in assessment years, followed by only negligible changes in non-assessment years. A survey of state assessment practices shows that at least 16 US states currently rely on multi-year assessment practices, suggesting that this lumpiness in the data may be serious enough to cause the residual correlation I see in my diagnostic tests.

To correct for this "lumpiness" in the data, I rely on multi-year intervals of data instead of annual data. Specifically, I choose to analyze three-year intervals. According to a survey on reassessment cycles the modal reassessment cycle is three years. Additionally, three years is a long enough time period to remove some of the cyclical patterns seen in the data, while still short enough to retain some precision on the dates for TEL implementation.

Table 1.4 presents diagnostics for models of state and local property taxes where variables are differenced in three-year intervals. Substantively, I am now analyzing the growth rates of three-year intervals. The diagnostic tests reveal that collapsing the data in this manner corrects the problem of serial correlation in the first-differenced model (lag dependent variable is unnecessary).

Table 1.4: Diagnostics of OLS Regression on Property Tax Growth (3 Year Intervals**)**

VARIABLES	Δ3 SL LN Property Taxes
Property Limit	0.002
	(0.013)
Δ3 LN Total Population	2.673***
	(0.444)
Δ3 LN Total Employment	-0.391*
	(0.205)
Unified Democratic Control	-0.008
	(0.012)
Unified Republican Control	0.002
	(0.010)
Δ3 LN School Age Population	-0.227
	(0.226)
Δ3 LN Elderly Population	-0.792***
	(0.285)
Δ3 LN Personal Income	-0.138
	(0.102)
Constant	-0.019
	(0.027)
State Effects	Included
Year Effects	Included
Observations	588
R-squared	49
Number of States	0.418
F (State Effects)	0.96
F (Year Effects)	18.54***
LM (AR1), X2 (1)	1.502
Mod. Wald (GH), X2 (49)	2949.61***

Standard errors in parentheses
**** p<0.01, ** p<0.05, * p<0.1*

This section has showed that time-series cross-sectional data is often fraught with problems of serial correlation. While previous authors have largely ignored this subject, I have provided clear diagnostic tests of when serial correlation poses problems for estimation. Thankfully for my research, the best model in terms of correcting dynamics is also my preferred theoretical model – analysis of growth rates. I will analyze one-year growth rates of expenditures and revenues, additionally employing a lag dependent variable in the case of revenues. I will analyze three-year growth rates of property taxes.

Heteroskedasticity

In addition to temporal dependence, it is also possible for cross-sectional dependence to lead to violations of Gauss-Markov. As a consequence of my usage of fixed and year affects, however, my TCSC data tests negative for cross-sectional dependence using a Pesaran's test of cross sectional independence.

A more significant consideration in my model specification is the presence of panel heteroskedasticity. Panel heteroskedasticity implies that the variance of the errors in my model vary from state to state. This violation of the Gauss-Markov assumptions is problematic because it also leads to incorrect estimates of standard errors.

I test for panel heteroskedasticity using a method suggested by Greene (2000) and Kittel and Winner (2005). Specifically, a modified Wald statistic is constructed from the variances of the residuals from fixed effects regressions, as implemented in the STATA routine xttest3. The results of this test appear at the bottom of Tables 1.1 through 1.4. For each of my models, regardless of specification, the results indicate that I must reject the null hypothesis of panel homoskedasticity.

To account for this violation of Gauss-Markov, I will follow the recommendations of Beck and Katz (1995, 1996) and employ panel corrected standard errors. Simulations in Beck and Katz (1995) indicate that PCSEs are very accurate for panels with more than 15 time periods, whether or not panel heteroskedasticity is present. As summarized by Beck (2001), "there is no cost, and some potential gain, to using PCSEs in place of the usual OLS standard errors" (Beck 2001, p. 278).

Fixed Effects

As previously mentioned, my estimation relies on a technique known as "Difference-in-Differences". This method combines state fixed effects and year fixed effects to control for two varieties of unobservables: unobservables constant across states and unobservables constant across time. While the theoretical justification for employing each type of fixed effect is sound, it is an empirical question whether or not these relevant unobservables actually exist in the data. To test whether or not state and time fixed effects are needed, I employ F-Tests where the null hypothesis is that the coefficients for each fixed affect are jointly equal to zero. I test state and year effects separately. The results of these diagnostics again appear at the bottom of Tables 1.1 through 1.4.

As the results show, the state and year fixed effects are necessary in my models of state and local fiscal outcomes. The only exception to this conclusion is for the models employing first-differences. First-differencing the data has similar affect to state fixed affects of demeaning the data. Indeed, a first difference model and fixed effects model are equivalent in a two-period dataset. The results of this test suggest that state fixed effects could be omitted in first-differenced models, though I prefer to include them.

It is also important to discuss the trade-off between fixed and random effects. The random effects estimator is an efficient estimator that produces random intercepts for unobservable state characteristics. Unfortunately, the random effects estimator can produce biased coefficients if there is any correlation between the unobservable state-specific characteristics being picked up by the random effects and the regressors (including the TEL). This assumption has little face-validity in this case. Hausman tests between fixed and random affects for my diagnostic models (excluding first-differenced models) suggest that there are systematic differences between the coefficients produced by these two models. Following the advice of Greene (2000), this leads me to prefer the fixed effect model. With my empirical strategy set, I turn now to a discussion of the problems associated with omitted variables.

IV. Endogeneity

There is little doubt that the adoption of a TEL is endogenous to unobservable factors. The adoption of public policy is never a random process. It is a reasonable to assume that voters or legislatures have reasons for adopting limits when and where they are adopted. In this section, I take a closer look at how TELs are endogenous and the extent to which endogeneity could bias my conclusions

The research design employed in this paper, differences-in-differences, accounts for some of the most basic concerns about endogeneity. For instance, Shadbegian (1998) noted that “if voters in states with bigger governments are more likely to vote for a TEL and government spending patterns persist over time, then I would expect to find a positive relationship between a TEL and government size, even though a causal relationship does not exist" (Shadbegian 1998 , p. 125-126). This concern is most problematic for cross-sectional research designs. More specifically, endogeneity biases cross-sectional results by comparing two groups, TEL states and non-TEL states, that are simply systematically different in unobservable ways beyond adoption of a TEL. By adding a time dimension and employing state fixed-effects, I can hold constant these sorts of unobservable state-to-state differences and instead focus my estimation on the before-and-after effect of a limit within a state.

While fixed effects can control for some unobservables characteristics of states, this modeling technique is not a panacea for endogeneity. The important assumption underlying fixed-effects is that the unobserved variable is constant over time. If an unobserved, omitted variable varies over time, omitted variable bias can persist. Specifically, omitted variable bias occurs when variables, not included in an estimation, are determinants of the dependent variable and correlated with one ore more of the included independent variables (Greene 2000). The best solution for omitted variable bias, of course, is to identify the omitted variables and include them in the model. While data limitations hinder implementation of this ideal solution, it is still possible to identify the missing variables and sign the bias caused by omission.

Omitted variable bias is a particular concern for my analysis if the omitted variable is correlated with the timing of TEL adoption. I will discuss two time-variant omitted variables possibly correlated with the adoption of a TEL: changes in public mood and the existence of impending or recent “high growth regimes”.

Public Mood

One possible omitted variable correlated with the adoption of TELs is the concept of public mood. As noted by Reuben (1997), "The passage of limits could reflect changes in voter preferences about the size of government, and that these changes could lead to changes in state revenue or expenditure levels, but limits may not play a causal role"(Rueben 1997, p. 8). Because I have no variable to control for public mood concerning the size of government, its omission could lead to bias. Shadbegian (1998), for example, spells out how this might overestimate the effectiveness of TELs. He notes that "If voters signal the desire for a smaller government by voting for a TEL, then I would expect to find a negative relationship between a TEL and government size even though a causal relationship does not exist (i.e, the TEL is not acting as a binding constraint)" (Shadbegian 1998 , p. 125).

I have some evidence that this omitted variable bias hinders my estimation. Figures 1.2 and 1.3 present scatter plots of average spending growth and property tax growth for the 5 years before and after states implement TELs. These plot of the leads and lags of the dependent variable have an important implications. The plots suggest that spending and property taxes slow *prior* to the implementation of TELs. This finding may speak to the anticipation of constituent demands for lower spending by state legislators, and thus suggests that TELs are endogenous to the state's spending "mood."

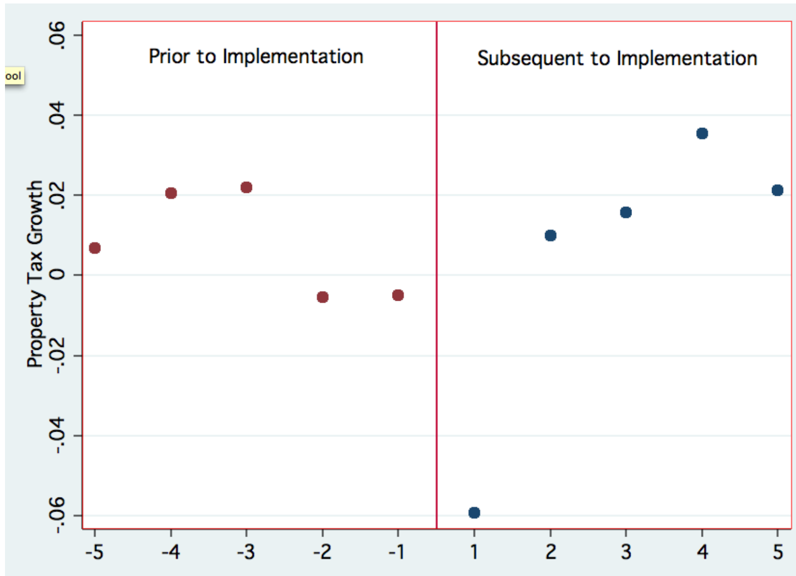

Figure 1.2: Average Property Tax Growth Before and After Property Tax Limits

Figure 1.3: Average Spending Growth Before and After Spending Limits

This illustrative evidence is bolstered with regression analysis. Models 1 of Tables 1.5 through 1.7 present the affects of leads of TEL indicators on government expenditures, revenues, and property taxes. Each TEL indicator variable is transformed into a set of dummy variables, each variable representing a unique time period prior to TEL implementation. For example “Limit T-1” represents the affect of a limit one year before it is implemented. “Limit T-(1-3)” represents the time period 1-3 years before implementation for property taxes. I use these variables, along with a standard set of covariates, to analyze the affect of a limit before implementation.

Table 1.5: Analysis of Trends Preceding Spending Limit Implementation

VARIABLES	Δ LN SL Expenditures	Δ LN Personal Income	Δ LN Total Population
Spending Limit T-1	-0.014*	-0.002	0
	(0.008)	(0.005)	(0.001)
Spending Limit T-2	0.005	-0.012**	0.002
	(0.007)	(0.005)	(0.001)
Spending Limit T-3	0.01	-0.001	0
	(0.007)	(0.005)	(0.001)
Spending Limit T-4	0.017**	0.002	-0.002
	(0.008)	(0.005)	(0.001)
Spending Limit T-5	0.011	0.009*	0.001
	(0.007)	(0.005)	(0.001)
Spending Limit T-6	0.016**	0.002	0.002*
	(0.008)	(0.005)	(0.001)
Spending Limit T-7	-0.012	-0.006	0.004***
	(0.008)	(0.005)	(0.001)
Spending Limit T-8	0.001	-0.002	0.002*
	(0.008)	(0.005)	(0.001)
Spending Limit T-9	-0.004	0.001	0.001
	(0.008)	(0.005)	(0.001)
Spending Limit T-10	-0.005	-0.002	0.002
	(0.009)	(0.006)	(0.002)
Δ LN Total Population	1.430***	1.297***	
	(0.251)	(0.234)	
Δ LN Total Employment	0.294***	0.220**	0.300***
	(0.087)	(0.087)	(0.018)
Unified Democratic Control	0.002	0	0
	(0.002)	(0.002)	(0.000)
Unified Republican Control	-0.001	0	0
	(0.002)	(0.002)	(0.000)
Δ LN School Age Population	-0.107	-0.490***	
	(0.142)	(0.134)	
Δ LN Elderly Population	-0.545***	-0.192*	
	(0.162)	(0.111)	
Δ LN Personal Income	-0.021		0.039***
	(0.043)		(0.010)
Constant	0.013**	0.039***	-0.003***
	(0.006)	(0.007)	(0.001)
State Effects	Included	Included	Included
Year Effects	Included	Included	Included
Observations	1764	1764	1764
R-squared	0.488	0.596	0.834
Number of States	49	49	49

Panel corected standard errors in parentheses
**** p<0.01, ** p<0.05, * p<0.1*

Table 1.6: Analysis of Trends Preceding Revenue Limit Implementation

	(1)	(2)	(3)
VARIABLES	Δ LN SL Revenue	Δ LN SL Personal Income	Δ LN SL Total Population
Revenue Limit T-1	-0.027***	0.003	-0.002
	(0.010)	(0.005)	(0.002)
Revenue Limit T-2	0.007	0.001	-0.001
	(0.010)	(0.005)	(0.002)
Revenue Limit T-3	0.006	0.006	-0.002
	(0.010)	(0.005)	(0.002)
Revenue Limit T-4	-0.012	0.002	-0.004*
	(0.010)	(0.005)	(0.002)
Revenue Limit T-5	0.005	0.006	-0.003*
	(0.010)	(0.005)	(0.002)
Revenue Limit T-6	0.01	0.005	0.000
	(0.010)	(0.005)	(0.002)
Revenue Limit T-7	0.006	-0.008	-0.002
	(0.010)	(0.005)	(0.002)
Revenue Limit T-8	0.021**	-0.004	-0.003*
	(0.010)	(0.005)	(0.002)
Revenue Limit T-9	0.015	-0.011**	-0.003
	(0.011)	(0.005)	(0.002)
Revenue Limit T-10	-0.007	-0.013	-0.002
	(0.015)	(0.009)	(0.004)
Δ LN Total Population	1.466***	1.283***	
	(0.308)	(0.236)	
Δ LN Total Employment	0.141	0.228***	0.299***
	(0.104)	(0.088)	(0.018)
Unified Democratic Control	0.003	0	0
	(0.002)	(0.002)	(0.000)
Unified Republican Control	-0.004*	0	0
	(0.002)	(0.002)	(0.000)
Δ LN School Age Population	-0.085	-0.492***	
	(0.166)	(0.135)	
Δ LN Elderly Population	-0.382**	-0.196*	
	(0.172)	(0.112)	
Δ LN Personal Income	0.114**		0.038***
	(0.050)		(0.010)
Constant	0.020***	0.039***	-0.003***
	(0.006)	(0.007)	(0.001)
State Effects	Included	Included	Included
Year Effects	Included	Included	Included
Observations	1764	1764	1764
R-squared	0.431	0.595	0.832
Number of States	49	49	49

Panel Corrected standard errors in parentheses
**** p<0.01, ** p<0.05, * p<0.1*

Table 1.7: Analysis of Trends Preceding Property Tax Limit Implementation

VARIABLES	(1) Δ3 LN SL Property Taxes	(2) Δ3 LN SL Total Population	(3) Δ3 LN SL Personal Income	(4) Δ3 LN SL Home Values
Property Limit T-(1-3)	-0.023	0	-0.001	0.016
	(0.015)	(0.002)	(0.004)	(0.016)
Property Limit T-(4-6)	0.01	0.001	0	0.022
	(0.015)	(0.002)	(0.004)	(0.017)
Property Limit T-(7-9)	-0.014	-0.001	0	-0.035**
	(0.017)	(0.002)	(0.005)	(0.018)
Property Limit T-(10-12)	0.025	0.001	0.004	-0.013
	(0.018)	(0.002)	(0.006)	(0.018)
Δ3 LN Total Population	2.671***		0.778**	0.972
	(0.606)		(0.369)	(0.629)
Δ3 LN Total Employment	-0.354	0.378***	0.764***	0.969***
	(0.255)	(0.038)	(0.150)	(0.325)
Unified Democratic Control	-0.008	0	-0.006	0.012
	(0.010)	(0.001)	(0.005)	(0.016)
Unified Republican Control	0.002	-0.001	0	-0.001
	(0.008)	(0.001)	(0.006)	(0.011)
Δ3 LN School Age Population	-0.243		-0.348*	-0.355
	(0.288)		(0.199)	(0.300)
Δ3 LN Elderly Population	-0.783**		-0.061	-1.179**
	(0.367)		(0.124)	(0.520)
Δ3 LN Personal Income	-0.143	0.033		0.278
	(0.112)	(0.025)		(0.183)
Constant	0.044*	-0.015***	0.006	0.170***
	(0.026)	(0.004)	(0.026)	(0.040)
State Effects	Included	Included	Included	Included
Year Effects	Included	Included	Included	Included
Observations	588	588	588	490
R-squared	0.48	0.886	0.767	0.51
Number of States	49	49	49	49

Panel Corrected standard errors in parentheses
**** p<0.01, ** p<0.05, * p<0.1*

The results of these regressions suggest that TELs have an affect *before* they are actually officially implemented. This result is significant for expenditure and revenue limits, but insignificant for property taxes. The latter result is perhaps indicative of the loss of precision caused by moving to three-year time intervals.

Because it cannot be the law itself causing these declines in growth prior to implementation, I have to assume that there is a variable affecting the dependent variable that I am not capturing in this estimation. As previously discuss, a likely candidate is "public mood". Voting on a TEL, or threatening to propose a TEL, is a clear sign that public mood disfavors the growth of government. Lawmakers react by lowering taxes or spending, even if they are not legally obliged to do so. While it is problematic that I am omitting an important variable from my estimation, this form of omitted variable should bias against finding that TELs fail to limit the growth of government.

Growth Regimes

In this section I discuss endogeneity associated with what I will refer to as a "growth regimes." A growth regime is associated with legislators with high taxing and spending preferences or is the consequence of high population or personal income growth. If the existence of a growth regime is correlated with the adoption of a TEL, this could potentially be the source of omitted variable bias. While I discuss the existence of a growth regime as ultimately an omitted variable problem, I assume that growth regimes can be identified by measurable variables - namely changes in expenditures, revenues, property taxes, home prices, personal income, or population. In this section I probe the existence and discuss the consequences of growth regimes before and after TEL adoption.

Preceding Growth Regimes

One hypothesis is that growth regimes systematically occur prior to the adoption of a TEL. Indeed, this hypothesis has high face validity and has been discussed in previous literature on the causes of the tax revolt. It is reasonable to believe that voters would go to the polls to limit taxing and spending only after witnessing large increases in tax burden or state expenditures. Numerous scholars have attributed the passage TELs to the significant growth in tax burden (Oakland 1979, Citrin and Levy 1981).

To test this hypothesis, I again look for significant correlations between growth indicators and the future adoption of TELs. I test whether the years prior to implementation are correlated with increased growth in taxing, spending, personal income, home prices, and population. Many states adopt multiple property tax limits during my time series. I code my variables to take account of these multiple adoptions.

The results for these tests appear in Tables 1.5 through 1.7. Overall, there are few socioeconomic trends that predict the adoption of property tax limits. The exception to this finding is a decrease in home values 7-9 years prior to adopting a property tax limit. Since I only have home value data starting in 1975, this finding is only applicability to a subset of cases, notably not the large number of property tax limits passed in the late seventies and early eighties. Since this is the only significant prediction for property tax limits, I will not dwell on its importance.

More significantly increases in state and local spending growth often precede the adoption of a spending limit. While only the forth and sixth year prior to adoption reach traditional levels of significant, the second through sixth years are jointly significant using a two-tailed F-test (p=0.04). Additionally, spending limits are also preceded by statistically significant increases in population growth prior to implementation (6-8 years for population). This evidence leads me to conclude that growth regimes indeed precede the adoption of expenditure limits.

How do previous growth regimes bias the estimation of the effect of a limit? To answer this question, I must first diagnose whether previous growth regimes are positively or negatively correlated with future growth. This is an empirical question. If previous growth regimes are positively correlated with future growth regimes, my results will be biased upwards - perhaps leading to a null results when in fact TELs decrease the size of government. In contrast, if previous growth regimes are negatively correlated with future growth regimes, my results will be biased downwards - perhaps leading to negative results when the TEL in fact has no affect on the size of government in and of itself.

To estimate the effect of lagged growth regimes on future outcomes, I estimate a set of bivariate OLS regressions. For example, the first set of regressions I will conduct analyze the affect of past spending growth on future spending growth. Here, the dependent variable in each regression is spending growth. The independent variables are various lags of spending growth. I will plot the coefficients of each of these regressions in order to determine the temporal affect of growth regimes. Although I present the results for all states combined, this result holds for TEL states and non-TEL states alike.

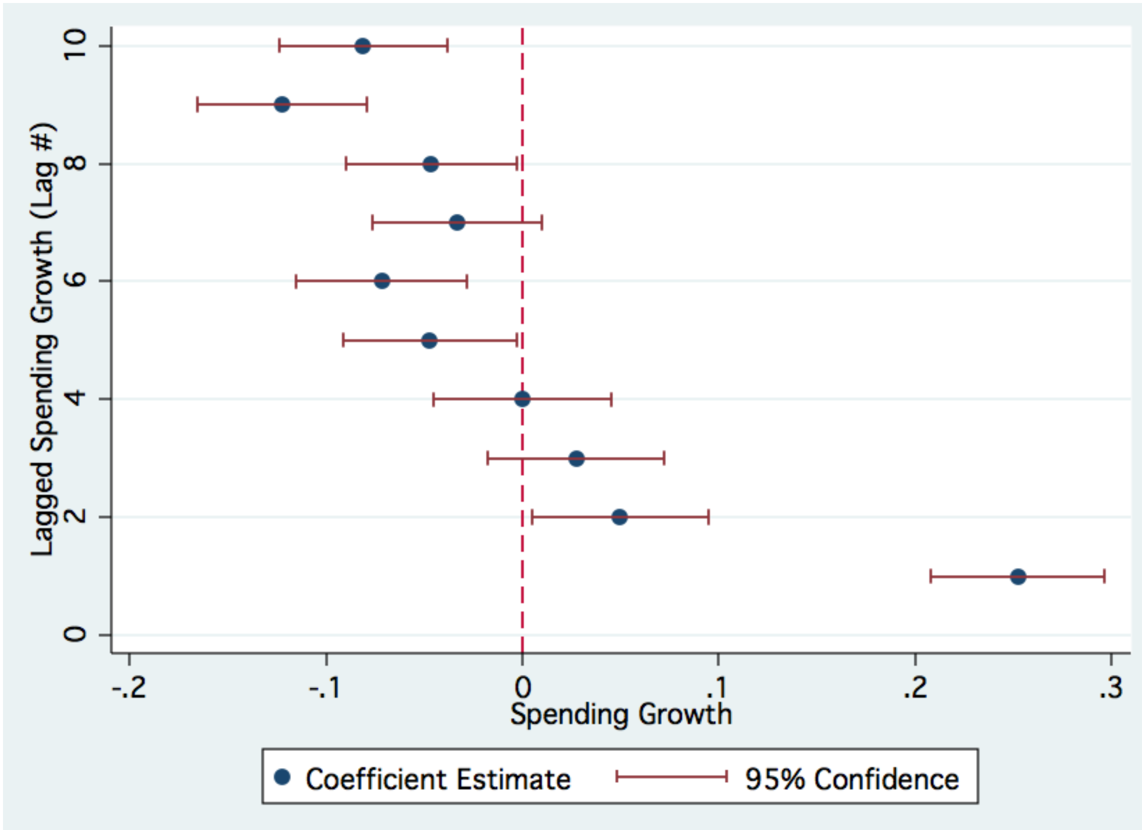

Figure 1.4: Effect of Lagged Spending Growth on Current Spending Growth

Figure 1.4 plots the coefficient estimates (and 95% confidence intervals) of the affects of lagged spending growth on spending growth. Each point on the graph represents the coefficient of a bivariate regression. As is clear from the, the spending growth in T1 is positively and significantly correlated with spending in T-1 and T-2. In contrast, spending growth in T1 is negatively and significantly associated with spending growth in T-5 and T-6. In other words, this figure shows, not surprisingly, that state budgets are subjects to business cycles and perhaps bubbles. Although I only present the results for the first 10 lags, it is noteworthy that a negative (and generally significant) coefficient is sustained until the 13th lag. Afterwards the 13th lag, the affect of lagged spending growth is zero).

What this suggests is that we should expect to see periods of high growth followed periods of low growth after two years. This reality has a significant implication for signing the direction of bias for spending limits. Since spending limits are systematically preceded by periods of high government spending, we should expect - regardless of the adoption of a spending limit - for this period to be followed by low growth periods of spending. As such, if the existence of a bubble or otherwise high growth regime is correlated with the adoption of a spending limit, we should be biased in the direction of finding that spending limits significantly reduce expenditures, even if this result in reality is simply the artifact of a bursting bubble.

I also found that periods of high population growth precipitated the adoption of spending limits. Lags of population growth are positively correlated with spending growth regardless of the number of lags employed. Although the size of the effect is incredibly small (the first lag has the largest effect, and the size its coefficient is still only 6.10e-08), this positive correlation does theoretically pose a problem for my estimation. Since this could possible bias me in favor of finding null results, I will correct for this possible bias by including lagged population variables in my final estimates of the affect of spending limits on spending growth.

Impending Growth Regimes

Another, perhaps even more problematic, example of omitted variable bias is if

TEL adoption is correlated with voter predictions about *impending* growth regimes. Predictions of future government growth has been suggested as a variable that could spur passage of a TEL. Voters foresee future increases and try to curtail them by adopting a TEL. This omitted variable could lead to the null results predicted in this dissertation because, despite an accurate anticipation of skyrocketing growth in government, instead taxing and spending stayed on course with previous growth rates.

There are several reasons to doubt the validity of this suggested omitted variable. First, no one is omniscient on the future prospects of government actions. This is particularly true for voters, whose limited political knowledge has been documented time and again (see for example, Campbell et al. 1960, Zaller 1992, Kinder 2006). Second, although it is possible to use demographic indicators to predict future size of government, such as the rate of school-age children or elderly population, these variables are already controls in my estimation. If these variables, or even per-capita income, were to rise and spending did not follow in tandem in concurrence with the passage of a TEL, then this under-prediction of spending would cause the TEL variable to be significant.

Nonetheless, to test the hypothesis that TELs are correlated impending growth regimes, I use my set of TEL indicators and standard covariates to see how well they predict future growth indicators. Specifically, I test whether the adoption of TELs is correlated with future growth rates of population, home prices, and personal income. To do so I again transform my basic TEL indicators into sets of dummy variables, each representing a unique year subsequent to TEL implementation. Models 1-3 of Table 1.8

estimate the affect of spending limits on personal income, population, and home prices. Table 1.9 estimates the affect of property tax limits. Results for revenue limits (excluded) likewise have null results.

As is clear from the estimates in Tables 1.8 and 1.9, TELs do not foreshadow future growth regimes. With the exception of a few isolated coefficients, such as spending limits predicting home values seven years in the future, all results are null. F-test of the combined significance of coefficients, such as the effects of property tax limits on home values 1-4 years in the future prove also to be insignificant. Given these results, there is little concern that that adoption of TELs is correlated with foresight of impending growth regimes. Subsequent economic and demographic changes are simply not correlated with the adoption of a TEL. As such, I reject this hypothesis as a possible source of endogeneity.

Table 1.8: Correlations Between Spending Limits and Future Growth

	(1)	(2)	(3)
VARIABLES	Δ LN Personal Income	Δ LN Population	Δ LN Home Values
Spending Limit Year 1	-0.002	-0.001	-0.008
	(0.005)	(0.001)	(0.012)
Spending Limit Year 2	-0.005	-0.001	-0.017
	(0.005)	(0.001)	(0.012)
Spending Limit Year 3	-0.002	0	-0.013
	(0.005)	(0.001)	(0.012)
Spending Limit Year 4	0.006	0	-0.008
	(0.005)	(0.001)	(0.012)
Spending Limit Year 5	0.002	0	-0.007
	(0.005)	(0.001)	(0.012)
Spending Limit Year 6	0.003	0.001	-0.016
	(0.005)	(0.002)	(0.013)
Spending Limit Year 7	0.004	0.001	-0.022*
	(0.006)	(0.002)	(0.013)
Spending Limit Year 8	0.004	0.001	-0.019
	(0.006)	(0.002)	(0.013)
Spending Limit Year 9	-0.004	-0.001	-0.005
	(0.006)	(0.002)	(0.013)
Spending Limit Year 10	-0.005	0	0.013
	(0.006)	(0.002)	(0.013)
Δ LN Total Population	1.267***		1.105***
	(0.235)		(0.335)
Δ LN Total Employment	0.230***	0.300***	0.646***
	(0.087)	(0.018)	(0.126)
Unified Democratic Control	0	0	-0.001
	(0.002)	(0.000)	(0.004)
Unified Republican Control	0	0	-0.002
	(0.002)	(0.000)	(0.002)
Δ LN School Age Population	-0.481***		-0.339*
	(0.135)		(0.186)
Δ LN Elderly Population	-0.197*		-0.728***
	(0.111)		(0.255)
Δ LN Personal Income		0.038***	0.300***
		(0.010)	(0.059)
Constant	0.039***	-0.003***	0.027***
	(0.007)	(0.001)	(0.007)
State Effects	Included	Included	Included
Year Effects	Included	Included	Included
Observations	1764	1764	1519
R-squared	0.595	0.832	0.456
Number of statenum	49	49	49

Panel Corrected Standard errors in parentheses
**** p<0.01, ** p<0.05, * p<0.1*

Table 1.9: Correlations Between Property Tax Limits and Future Growth

VARIABLES	Δ LN Personal Income	Δ LN Total Population	Δ LN Home Values
Property Limit Year 1	-0.002	0	0.008
	(0.003)	(0.001)	(0.008)
Property Limit Year 2	-0.004	0	0.002
	(0.004)	(0.001)	(0.008)
Property Limit Year 3	0.001	-0.001	0.003
	(0.004)	(0.001)	(0.008)
Property Limit Year 4	-0.004	0	0.005
	(0.003)	(0.001)	(0.008)
Property Limit Year 5	0.003	0	0
	(0.003)	(0.001)	(0.008)
Property Limit Year 6	-0.004	0.001	-0.002
	(0.003)	(0.001)	(0.008)
Property Limit Year 7	0.002	0.002**	-0.001
	(0.004)	(0.001)	(0.008)
Property Limit Year 8	-0.004	0.001	0.006
	(0.004)	(0.001)	(0.008)
Property Limit Year 9	-0.004	0	0.003
	(0.004)	(0.001)	(0.008)
Property Limit Year 10	-0.005	0.001	-0.001
	(0.004)	(0.001)	(0.008)
Δ LN Total Population	1.287***		1.094***
	(0.235)		(0.339)
Δ LN Total Employment	0.223**	0.300***	0.651***
	(0.087)	(0.018)	(0.129)
Unified Democratic Control	0	0	0
	(0.002)	(0.000)	(0.004)
Unified Republican Control	0	0	-0.002
	(0.002)	(0.000)	(0.002)
Δ LN School Age Population	-0.494***		-0.326*
	(0.134)		(0.192)
Δ LN Elderly Population	-0.183		-0.781***
	(0.112)		(0.256)
Δ LN Personal Income		0.038***	0.300***
		(0.010)	(0.059)
Constant	0.040***	-0.003***	0.026***
	(0.007)	(0.001)	(0.007)
State Effects	Included	Included	Included
Year Effects	Included	Included	Included
Observations	1764	1764	1519
R-squared	0.596	0.832	0.452
Number of States	49	49	49

Panel Corrected Standard errors in parentheses
**** p<0.01, ** p<0.05, * p<0.1*

V. Differences-in-Differences

As previously noted, I employ a difference-in-differences model to estimate the effect of TELs. My diagnostic tests have led me to prefer a model specification of first-differenced logged fiscal outcomes. Specifically, I estimate the following equation:

$$\Delta(\ln)FISCAL_{it} =$$

$$\alpha + \beta_1 TEL_{it} + \beta_2 \Delta(\ln)ECON_{it} + \beta_3 \Delta(\ln)POP_{it} + \beta_4 POL_{it} + \lambda_i + \kappa_t + u_{it}$$

The economic covariates include personal income and total employment. For some models of property tax limits I also include a covariate on home values, though this shortens the length of my time series (1975-2006). Population variables include total population, elderly population, and school-age population.

Table 1.10: Analysis of Spending Limit Effectiveness

	(1)	(2)
VARIABLES	Δ LN State and Local Expenditures	Δ LN State Expenditures
Spending Limitation	-0.002	0.001
	(0.003)	(0.005)
Δ LN Total Population	1.404***	0.970**
	(0.258)	(0.388)
Δ LN Total Employment	0.312***	0.362***
	(0.089)	(0.132)
Unified Democratic Control	0.002	0.004
	(0.002)	(0.004)
Unified Republican Control	-0.001	-0.002
	(0.002)	(0.003)
Δ LN School Age Population	-0.104	-0.187
	(0.147)	(0.225)
Δ LN Elderly Population	-0.550***	-0.500**
	(0.163)	(0.253)
Δ LN Personal Income	-0.016	0.038
	(0.044)	(0.062)
Constant	0.026***	0.036***
	(0.005)	(0.007)
State Fixed Effects	Included	Included
Year Fixed Effects	Included	Included
Observations	1764	1764
R-squared	0.482	0.343
Number of statenum	49	49

Panel Corrected Standard Errors in Parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

The results of the difference estimation support my initial hypothesis. Table 1.10 presents the results for spending limits, Table 1.11 for revenue limits, and Table 1.12 for property tax limits. The dependent variable follows the fiscal outcome constrained by the limit. I will start my discussion with my analysis of the common treatment affect of expenditure limits

Model 1 of table 10 tests for the affect of spending limits on state and local revenue, while Model 2 looks at state-only revenue. Regardless of the dependent variable employed, the affect of spending limits on spending growth is statistically indistinguishable from zero. This is true regardless of whether lags of total population if employed (as previously mentioned in the section on endogeneity). In contrast, spending growth is positively significantly predicted by growth in the total population and total employment. It is negatively and significantly predicted by growth of the elderly population. While the political variables achieve their predicted sign (increased growth under Democratic control, decreased growth under Republican control), neither variable achieves statistical significance.

The results presented in Table 1.11 are very similar. I present two models of state and local revenues, one with and one without a lag dependent variable to correct for residual autocorrelation. The results for either model do not vary substantially.

Most importantly, I find that revenue limits fail to reduce the growth of state and local revenues. Interestingly, in this model personal income growth is a more substantial predictor of fiscal outcomes than is employment growth. Growth in total population and the elderly population have the same statistically significant affect on revenues as they

Table 1.11: Analysis of Revenue Limit Effectiveness

	(1)	(2)
VARIABLES	Δ LN State and Local Revenues	Δ LN State and Local Revenues
Lag Dependent Variable		-0.091**
		(0.044)
Revenue Limitation	-0.001	-0.001
	(0.004)	(0.004)
Δ LN Total Population	1.478***	1.593***
	(0.308)	(0.314)
Δ LN Total Employment	0.126	0.151
	(0.104)	(0.105)
Unified Democratic Control	0.003	0.003
	(0.002)	(0.002)
Unified Republican Control	-0.004*	-0.004*
	(0.002)	(0.002)
Δ LN School Age Population	-0.097	-0.092
	(0.167)	(0.171)
Δ LN Elderly Population	-0.397**	-0.453***
	(0.171)	(0.170)
Δ LN Personal Income	0.112**	0.114**
	(0.050)	(0.051)
Constant	0.027***	0.029***
	(0.006)	(0.006)
State Fixed Effects	Included	Included
Year Fixed Effects	Included	Included
Observations	1764	1764
R-squared	0.427	0.432
Number of statenum	49	49

Standard errors in parentheses
**** p<0.01, ** p<0.05, * p<0.1*

do on expenditures. Interestingly enough, the coefficient estimating the affect of unified republican control is significant in this model, suggesting that revenue growth decreases during conservative regimes (but not necessarily expenditures).

Finally, the results for the effectiveness of property tax limits tell largely the same story. Although states often adopt multiple property tax limits during this time series, in this analysis I do not discriminate between changes to the letter of the law. Instead, I test only for a common effect of any property tax limit. Model 1 presents results for the full period of my time-series, while Model 2 is limited to the years 1975-2006 in order to include a covariate on housing prices.

My estimation suggests that property tax limits are ineffective at slowing property tax growth in three-year intervals. In this model the only statistically significant predictors of property tax growth are total population growth (positive influence) and elderly population growth (negative influence). No economic variables affect property tax burdens, including the variable on housing prices. Partisan control of the state legislature and governor's office also do not affect property tax growth, a not surprising null result since most property taxes are collected at the local level.

The primary conclusion of this section is that tax and expenditure limits are unsuccessful at limiting the growth of government. In the next section I break down this common treatment affect to examine whether or not some limitations are more effective than others.

Table 1.12: Analysis of Property Tax Limit Effectiveness

VARIABLES	(1) Δ3 LN State and Local Property Taxes	(2) Δ3 LN State and Local Property Taxes
Property Limitation	0.001	0.001
	(0.014)	(0.017)
Δ3 LN Total Population	2.674***	2.905***
	(0.606)	(0.627)
Δ3 LN Total Employment	-0.391	0.403
	(0.254)	(0.313)
Unified Democratic Control	-0.008	0.001
	(0.010)	(0.011)
Unified Republican Control	0.002	0.003
	(0.008)	(0.008)
Δ3 LN School Age Population	-0.227	0.317
	(0.278)	(0.240)
Δ3 LN Elderly Population	-0.792**	0.879*
	(0.366)	(0.469)
Δ3 LN Personal Income	-0.138	0.025
	(0.111)	(0.180)
Δ3 LN Home Values		0.029
		(0.044)
Constant	0.022	0.167
	(0.041)	(0.031)
State Effects	Included	Included
Year Effects	Included	Included
Observations	588	490
Number of statenum	49	49
R-squared	0.47	0.45

Standard errors in parentheses
**** p<0.01, ** p<0.05, * p<0.1*

VI. Temporary Effects

In this final section I test whether or not there are temporary affects of tax and expenditure limits. Using the same methodology previously enjoyed, I divide my treatment variable into sets of dummy variables, each representing a unique year of TEL implementation. For revenue and expenditure limits, for example, the variable identified as “Limit Year 1” represents the first year of implementation. For revenue and expenditure limits I test for temporary effects of up to 10 years. For property tax limits I do a similar analysis except I use indicators for three-year intervals.

The results of this exercise are in Tables 1.13 and 1.14. The first table shows the temporary affects of revenue and expenditure limits. For both of these limits, there is only one year in which we see a significant decline in revenues or expenditures. For revenue limits, this decline occurs in the first year of implementation. The implementation of a revenue limit is associated with approximately a 3.1% decline in state and local own-source revenues. For expenditure limits, state and local direct general expenditures decline by 1.6% in the second year of implementation.

The results for property tax limits depart significantly in this model specification. This model clearly shows that property tax limits are effective for a longer period of time. In the first three years of implementation, property tax limits decrease state and local property taxes by 9.2%, all else equal. The effect continues to be significant in years four through six and seven through nine. During these intervals, an implemented property tax limit is associated with a decline in the growth of property taxes by 3%. The affect of property tax limits, however, is not permanent. After 10 years in place, property tax limits have no affect on state and local property taxes. This suggests that while property tax limits are initially successfully, they are eventually evaded.

The fact that property tax limits are more successful at reducing the size of government than revenue or spending limits raises an interesting puzzle. One hypothesis, following from agency theory, is that property tax limits are easier for citizens to monitor than are revenue or expenditure limits. While it is difficult for citizens to calculate the

Table 1.13: Analysis of Short-Term Effectiveness of Spending and Revenue Limits

	(1)	(2)
VARIABLES	Δ LN SLExpenditures	Δ LN SL Revenues
Limit Year 1	-0.008	-0.031***
	(0.008)	(0.010)
Limit Year 2	-0.016**	-0.002
	(0.008)	(0.010)
Limit Year 3	-0.005	0.003
	(0.008)	(0.010)
Limit Year 4	0.002	0.006
	(0.008)	(0.010)
Limit Year 5	-0.009	0.000
	(0.008)	(0.010)
Limit Year 6	0.000	0.005
	(0.008)	(0.010)
Limit Year 7	0.000	0.005
	(0.009)	(0.010)
Limit Year 8	-0.005	-0.002
	(0.009)	(0.010)
Limit Year 9	0.006	0.013
	(0.009)	(0.011)
Limit Year 10	0.001	-0.005
	(0.009)	(0.011)
Δ LN Total Population	1.401***	1.486***
	(0.258)	(0.307)
Δ LN Total Employment	0.311***	0.123
	(0.089)	(0.103)
Unified Democratic Control	0.002	0.004
	(0.002)	(0.002)
Unified Republican Control	-0.001	-0.004*
	(0.002)	(0.002)
Δ LN School Age Population	-0.101	-0.103
	(0.147)	(0.166)
Δ LN Elderly Population	-0.531***	-0.399**
	(0.161)	(0.171)
Δ LN Personal Income	-0.018	0.110**
	(0.044)	(0.050)
Constant	0.026***	0.020***
	(0.005)	(0.006)
State Effects	Included	Included
Year Effects	Included	Included
Observations	1764	1764
Number of States	49	49
R-squared	0.484	0.43

Standard errors in parentheses. Type of Limit (revenue or expenditure) follows DV
*** p<0.01, ** p<0.05, * p<0.1

Table 1.14: Analysis of Short-Term Effectiveness of Property Tax Limits

VARIABLES	Δ3 LN Property Taxes
Property Limit Yrs 1-3	-0.092***
	(0.022)
Property Limit Yrs 4-6	-0.033**
	(0.017)
Property Limit Yrs 7-9	-0.036**
	(0.017)
Property Limit Yrs 10-12	-0.001
	(0.018)
Property Limit Yrs 13-15	0.008
	(0.019)
Property Limit Yrs 16-18	0.002
	(0.025)
Δ3 LN Total Population	2.730***
	(0.604)
Δ3 LN Total Employment	-0.405
	(0.262)
Unified Democratic Control	-0.008
	(0.010)
Unified Republican Control	0.002
	(0.007)
Δ3 LN School Age Population	-0.322
	(0.281)
Δ3 LN Elderly Population	-0.836**
	(0.370)
Δ3 LN Personal Income	-0.13
	(0.113)
Constant	0.013
	(0.051)
State Effects	Included
Year Effects	Included
Observations	588
Number of statenum	49
R-squared	0.495

Panel Corrected Standard errors in parentheses
**** p<0.01, ** p<0.05, * p<0.1*

total amount that they contribute to government revenue per year or gauge how much the government is spending, property tax burdens are – literally – delivered to your doorstep. The opacity of this tax might contribute to the lasting effectiveness of a property tax limit.

VII. Conclusion

Based upon the logic of principal-agent relationships, I doubted that those who enact tax and spending limits would be able to constrain the future actions of lawmakers possessed of different goals and direct control of state purse strings. The data largely confirmed these doubts. Records of spending and revenue patterns show that TELs have in almost every instance failed to constrain the size of government in American states. Property tax limits only have a temporary affect at reducing tax burdens.

I believe that my findings have significant implications for a wider literature. As noted, many TELs are passed through the initiative process. The inability of voters (principals) to constrain their legislators and governors (agents) in this instance casts doubt upon the overall effectiveness of the initiative process. As noted in Gerber et al. 2001, “initiatives do not implement or enforce themselves” (Gerber et al. 2001 p. 109). I agree with this assertion and argue that officials in the vast majority of states have been able to circumvent the TELs that were intended to limit them.

Chapter 2
Policy Implementation in Direct Democracy

I. Introduction

Policy implementation in direct democracy invokes a difficult political problem: delegation to potentially hostile agents. When voters pass laws at the ballot box, they may believe it is a foregone conclusion that the law will be faithfully implemented. This paper questions this conclusion, and tests several hypotheses about the conditions under which successful implementation of initiatives will occur.

In this paper I argue that the challenge of policy implementation in direct democracy is subject to a principal-agent problem. The voters, as principals, pass laws at the ballot box that they expect to be faithfully implemented. Despite their power to make these decisions, however, they have little control over the agents, in this case lawmakers, charged with implementation. This is similar to the dilemma faced by legislators attempting to control the executive branch (Mitnick 1980; Moe 1984; McKelvey and Ordeshook 1984; Shepsle and Weingast 1984; McCubbins et. al 1987, 1989; Kiewiet and McCubbins 1991; Lupia and McCubbins, 1998; Epstein and O'Halloran 1999; Huber and Shipan 2002), legislators on the floor delegating power to committees (Fenno 1973; Krehbiel 1991; Rohde 1991; Aldrich and Rohde 1998, 2000; Cox and McCubbins 1993, 2005), and when members of Congress try to discipline the budgetary decisions of future Congresses (Schick 1995).

I use the theoretical framework forged by this existing research to test the applicability of the principal-agent model for policy implementation in direct democracy.

Specifically, I leverage two conditions derived by previous research believed to produce successful delegation to agents: compatible incentives between principals and agents and ease of monitoring agents. In this chapter I apply each condition to the case of Tax and Expenditure Limits (TELs), testing whether or not limits are more effective when these conditions are met.

II. Competing Models of Policy Implementation

This paper tests the applicability of a principal-agent model to policy implementation in direct democracy. While a literature on the implementation of initiatives and referenda is just burgeoning, it is helpful to look back on the larger scholarship on policy implementation in general. Rational-choice models have been a mainstay of this literature since its inception, though there has been little empirical research testing the constructs of these models.

Goggin et al. (1990) identified three generations of policy implementation scholarship. The first generation, often construed as a pessimistic take on the policy implementation process, used case studies to show that implementation failure can and does occur (Derthick 1972, Pressman and Wildavsky 1973, Bardach 1977). Before these studies, it was simply assumed that policy implementors would act according to the intentions of the decision makers (Hill and Hope 2002)

The second of generation of policy implementation scholarship molded distinct theories of public policy and public administration into the case study research. The two schools of thought during this period were the "top-down" and "bottom up" approaches to policy implementation. The latter took a hierarchical approach to policy implementation, focusing on the ability of decision makers to achieve their policy objectives (Van Meter and Vaan Horn 1975; Sabatier and Mazmanian 1979, 1980). The former argued that bureaucrats are the primary actors in the implementation process, and as such implementation research should focus on the networks and incentives of these actors. (Lipsky 1971, 1980; Elmore 1980; Hjern and Porter 981; Hjern 1982).

The third generation of scholarship sought to combine the theoretical efforts of the second generation while aiming to "be more scientific" than previous approaches (Goggin et al. 1990, p. 18). Goggin and his colleagues called on the field to specify clear hypotheses and find empirical operationalizations to test them. Despite the strong motivation for this third generation, several authors have suggested that in practice there has been little research that has met these objectives (de Leon 1999 p.318; O'Toole 2000, p 268). Further, to date most empirical studies remain almost exclusively cross-sectional. My work aims to contribute to this third generation research with an application to direct democracy using time-series, cross-sectional data. Without empirically testing a theory, it is difficult to weigh the accuracy of this model for policy implementation in direct democracy, versus other models that suggest that the larger the degree of democracy in the adoption process, the more faithful implementation (DeLeon and DeLeon 2002).

Tax and Expenditure Limits (TELs) provide an excellent test-case for models of policy implementation. TELs are proscriptions to curb the growth of government that peg taxing or spending to an explicit rule. These laws are frequently adopted through the initiative or referendum process and today TELs exist in over half the American states. Given TELs explicitly aim to limit the growth of government, it is possible to test their effectiveness through analysis of fiscal outcomes. My analysis will compare growth rates of fiscal outcomes before adoption to growth rates after adoption and to the growth rates of a set of control cases. Since lawmakers set budgets every year (or every other year in case of states with biennial budgets), there exists a large number of observations for which to observe faithful or unfaithful implementation. Variation between legal content of TELs and characteristics of the implementing agents will provide leverage for my test of the applicability of a principal-agent model.

TELs do face a particularly tough implementation problem. First and foremost, lawmakers charged with implementing a TEL often have preferences that conflict with the limit's goals. If a conflict of interest did not exist, there would be no need for a limit in the first place. Second, because it is difficult to monitor state fiscal actions, both voters as well as the initiative proponents who sponsored the TELs may be unable to observe the rule changes or circumvention techniques that affect implementation. Third, it may also be the case that after the enactment of a TEL the principals themselves may change, either by population migration or changes in preferences over time. This means that principals themselves may push the lawmakers to find ways around the proscribed limitation. Either way, these potential difficulties suggest that implementation problems are likely to occur.

The application of the principal-agent framework to the case of tax and expenditure limits leads to several testable hypotheses. First and foremost, the potential for agency loss suggests that most TELs, most of the time, will not succeed at limiting the size of state and local governments. This hypothesis was tested in the first chapter of this dissertation. Second, the principal-agent model also helps identify the conditions under which TELs should be most effective. Specifically, if the principal-agent framework is a good model of policy implementation, then TELs should be most effective when agent preferences do not conflict with the mandate of the limit. Additionally, this model also predicts that TELs should be most effective when faithful implementation is easy to monitor. This paper will analyze the effectiveness of TELs through each of these model predictions.

The goal of this paper is to provide an empirical test of the principal-agent model for policy implementation using the case of tax and expenditure limits. First I review previous research on the inefficacy of TELs and provides insight on how these limits are evaded using the analogy of a complete contract. What follows explores whether competing preferences between principals and agents is the source of policy implementation failures in spending limits. I then consider whether property tax limits that are easier to monitor are more successful at cutting property tax growth. Finally, I offer some concluding remarks.

III. TEL Evasion

As previously mentioned, the primary prediction of the principal-agent model is that tax and expenditure limits will be ineffective at cutting the size of government. The empirical results presented in chapter one of this dissertation largely confirms this prediction. Before testing secondary predictions of the principal-agent model, it is helpful to explore how these limits are evaded. I will do so by discussing TELs as necessarily incomplete contracts, a fact that gives legislatures the discretion to evade the spirit of the law.

An incomplete contract is a prerequisite for a principal agent problem. If a complete contract could be drafted, one that specifies rules of action for every possible state of the world, than no principal-agent problem would every arise. A complete contract would simply nullify any and all discretion of the agent. For practical purposes, drafting a complete contract would impose incredible costs and is ultimately impossible because of uncertainty. That said, the policy implementation literature is aware of the importance of contract design, sometimes referring to this construct as “scenario writing” (Bardach 1977).

Because no complete contract can ever exist, I can not test whether TELs with complete contracts are more effective than TELs with incomplete contracts. Instead, I focus my analysis on how incomplete contracts lend themselves to TEL evasion. My analysis in this section will focus on revenue limitations. This choice is grounded on the small number of limits that have passed in the U.S., at least relative to spending and property limits.

TELs are often evaded is by changes to legal definitions. Specifically, in this section I will look at how lawmakers have changed the definition of "revenue" over time to decrease the proportion of revenue that is subject to a revenue limitation. Determining what revenue is subject to limitation in each state is relatively straightforward. Revenue limits must be published from year-to-year and I was able to locate these official reports for each state that has passed a revenue limit.

To calculate the proportion of revenue subject to limitation, I rely on the Census Department's estimates of state general own-source revenues as a denominator. The Census Department aggregates all general operating expenses, regardless of funding source, to make the estimate as comparable as possible between states. For example, all revenue derived from transportation is included in this category, regardless of whether or not transportation funding is splintered from the state's official "general fund" for accounting purposes. The only revenues systematically excluded from this estimate are federal funds, utility revenue, and insurance trust revenue - all of which are excluded from constraint of revenue limits anyway. Using the Census variable as the denominator and the state reported estimates of revenue subject to limitation as a numerator, I am able to construct a variable of the percentage of state revenues for general operating expenses that are subject to constraint by a revenue limit.

Figure 2.1 reports the percentage of state revenues subject to revenue limits over time. I report annual estimates for each year a revenue limitation is in force, with the exception of the state of Washington which budgets biannually. The red line represents the linear trend over time. As is clear from the figure, there is downward trend in the proportion of revenues subject to limitation for all states. A conclusion from this pattern is that all states change their definitions of what is subject to the limit over time. I will explore this more with short case studies of Florida, Colorado, and Missouri.

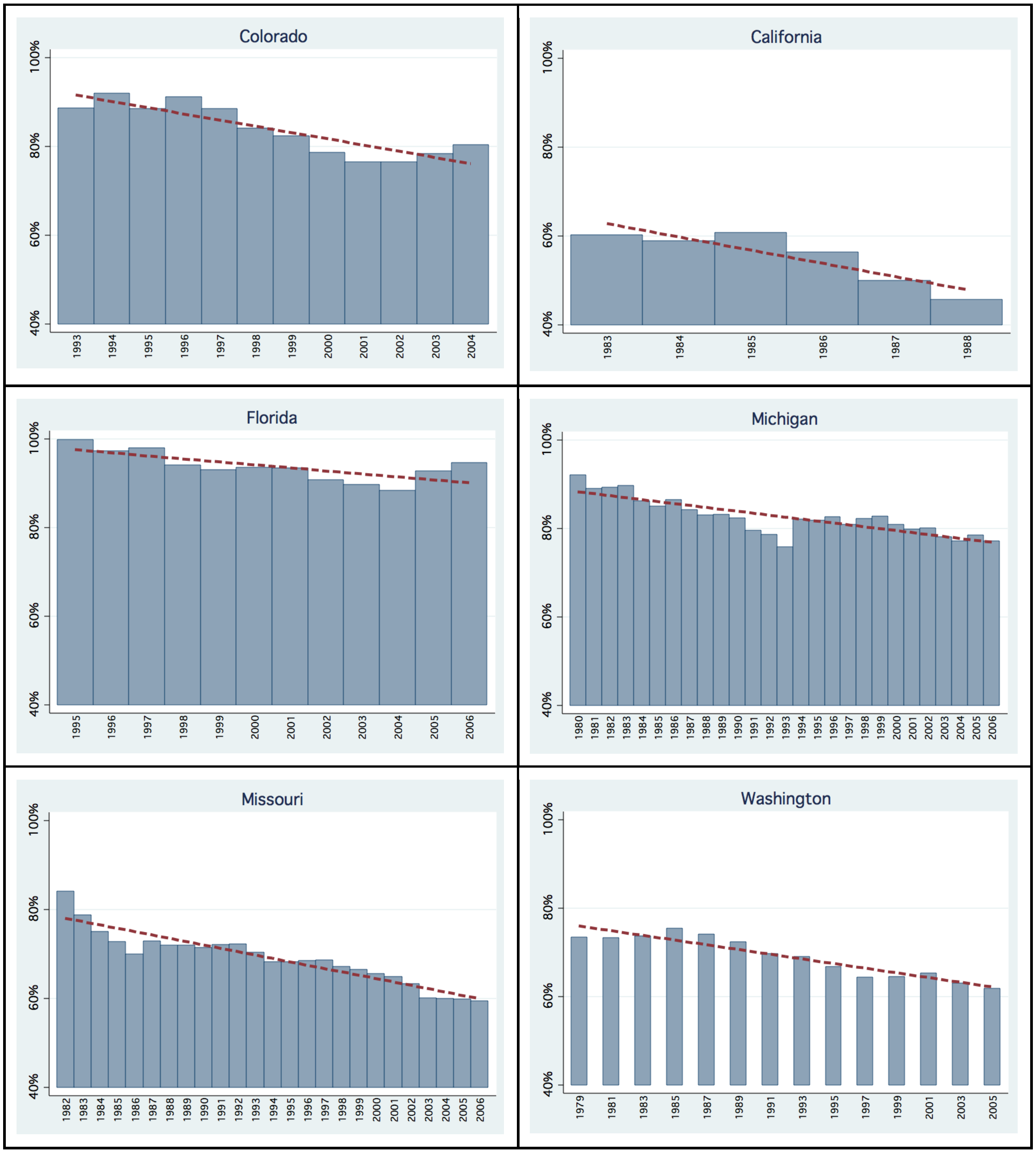

Figure 2.1: Changes to Proportions of Revenue Subject to Revenue Limits

Changes to the definition of what constitutes what is and what is not subject to limitation can occur in many ways. In Florida, for example, there was a exclusion of higher education revenue starting in the 2002-2003 fiscal year. At that time, fees for tuition or auxiliary services (e.g., book stores, student unions, health services) charged by Florida universities in the State University System were devolved from state accounts to the university's own local accounting system. As such, they were no longer considered state revenue and would not fall under the auspices of the revenue limit.

A very similar exemption occurred in Colorado. In 1998, the Colorado legislature passed a law that allow institutions of higher education to classify themselves as "enterprises" upon votes of their governing boards. Enterprises, government businesses, are fully exempt from Colorado's revenue limit. These accounting changes in both Florida and Colorado have two important consequences: less revenue was subject to the revenue limit and universities could freely raise tuition.

Finally, proponents of Missouri's Hancock Amendment contend that the spirit of their law has also been whittled by exemptions to the limit. The largest of these exemptions unknowingly occurred at the hands of the voters themselves. Courts in Missouri ruled in favor of the exclusion revenues that have resulted in tax increases with voter approval, even if those increases are only statutory whereas the limit is constitutional. Specifically, all proceeds from the sales tax bump in 1982 and motor vehicle fuel tax in 1987 are not subject to the revenue limit for as long as those taxes are in place. Critics have challenged that voters were unaware of the consequences of their actions and that this has led to billions of dollars of collected, uncapped revenues (Stansel 1994). Today Missouri is substantially under its revenue limit.

The politics of circumvention is possible because of incomplete contracts. Even when initiative writers attempt to seal off loopholes, it is impossible to predict every possible method of circumvention. An analysis of the definitions of revenue used in revenue limitations reveals no pattern between the letter of the law and degree of TEL evasion. Regardless of how long the definitions are, how many inclusions or exclusions they identify, or references they contain, I find no correlation between decreases in the percentage of revenues subject to the limit and definition descriptions. Revenues subject to limitation simply decline in all states, regardless of of the specificity of the definition of revenue in the contract.

Surprisingly, finding is actually consistent with scholarship on agency theory, as much of the literature focuses on designing contracts that expressively affect the incentives of the actors, not attempt to curtail agent discretion by the letter of the law (Kiewiet and McCubbins 1991). I turn now to the first empirical test of the principal-agent model, whether faithful implementation depends on principal-agent preference compatibility.

IV. Principal-Agent Preference Compatibility

A fundamental assertion of the principal-agent model is that agency loss is a function of competing preferences between principals and agents (Fama 1980). If principals and agents agree on the goals of delegation, than there is no principal-agent problem. In economics, competing preferences between principals and agents are solved by giving the agent a stake, in the form of an economic incentive, in an outcome that aligns with the preferences of the principal (Shavell 1979). In politics, applying this type of incentive is difficult. Instead, one way to assure compatible preferences is for the principal to select a compatible agent by "type" (Calvert, McCubbins, Weingast 1989; Fearon 1999). If voters select the correct "type" of agents on Election Day, this will increase the likelihood of faithful implementation of passed initiatives.

The implication for the case of TELs is that limits should be more faithfully implemented when the ideological convictions of the agents are compatible with the dictates of the TEL. Indeed, consensus among actors has been an important variable in models of policy implementation for many years (Sabatier and Mazmanian 1980 ; Van Horn and Van Meter 1985). I test the hypothesis that faithful policy implementation hinges on the compatible preferences between principals and agents by interacting a TEL indicator variable with variables indicating partisan control of government. Given that the principals who enact TELs are in favor of a small size of government, I predict that TELs will be effective in eras of unified Republican control. In contrast, TELs should not be faithfully implemented during eras of unified Democratic control.

This analysis makes the explicit assumption that the Democratic Party's preferred size of state government is in greater conflict with the mandate of a TEL. Alternatively, Republicans in control of state government should have a significantly smaller preference misalignment. I base this assumption on the previous research that found that Republican governments have smaller optimal taxing levels than do Democrats (Alt and Lowery 1994; 2000), at least in non-Southern US states. To make this assumption as innocuous as possible, I limit my analysis to non-Southern states. I also exclude Nebraska as it has a non-partisan, unicameral legislature.

I estimate the following difference-in-differences equation using panel-corrected standard errors:

$$\Delta(\ln)FISCAL_{it} =$$

$$\alpha + \beta_1 TEL_{it} + \beta_2 \Delta(\ln)ECON_{it} + \beta_3 \Delta(\ln)POP_{it} + \beta_4 POL_{it} + \beta_5 POL_{it} * TEL_{it} + \lambda_i + \kappa_t + u_{it}$$

The fiscal outcome of interest is state and local direct general expenditures and the TEL indicator represents the presence of a spending limit. The economic and population covariates are identical to those used in the first chapter of this dissertation. Also as before, I use a set of dummy variables to capture the effect of partisan control of state government. The first dummy variable is coded as one (zero otherwise) if there is unified Democratic control. Likewise, the second dummy variable is coded as one (zero otherwise) if there is unified Republican control. All other compositions of state government are captured in the omitted category, which can be interpreted as the effect of divided government. I interact each of these dummy variables with the variable indicating the presence of a spending limit.

The results in Model 1 of Table 2.1 show that Democratic control of state government has a significant and positive effect on state and local expenditures relative to divided government in the presence of a TEL. Specifically, the existence of a TEL during an era of unified Democratic control increases spending growth rates 1.3 percent relative to spending growth rates in a state regime without a TEL and divided government. By contrast, the interaction between unified Republican control and a TEL is negative, but does not reach any conventional level of significance. These results suggest that tax and expenditure limits are most prominently evaded during eras of liberal government control. This follows the prediction of the principal-agent model as this regime would have the greatest conflict of interest with the proscription of the law.

Table 2.1: Effect of Incompatible Preferences on TEL Effectiveness

Dependent Variable: Δ State and Local Direct General Expenditures (Real)		
	Model 1	Model 2
Spending Limit	-0.004	-0.011***
	(0.004)	(0.004)
Limit X Unified Republican Control	-0.006	
	(0.005)	
Limit X Unified Democratic Control	0.0137*	
	(0.007)	
Unified Democratic Control	0.0008	
	(0.003)	
Unified Republican Control	0.0003	
	(0.002)	
Limit X Democratic Governor		0.0144***
		(0.005)
Democratic Governor		-0.002
		(0.002)
Δ Total Population	1.845***	1.836***
	(0.287)	(0.275)
Δ School-Age Population	-0.214	-0.196
	(0.164)	(0.152)
Δ Elderly Population	-0.696***	-0.733***
	(0.189)	(0.190)
Δ Total Employment	0.239**	0.221**
	(0.0999)	(0.096)
Δ Personal Income	-0.106**	-0.092*
	(0.049)	(0.048)
State Effects	Included	Included
Year Effects	Included	Included
Constant	0.021***	0.026***
	(0.006)	-0.001
Observations	1152	1186
R-squared	0.512	0.506
Number of statenum	32	33

Notes: Panel-Corrected Standard errors in parentheses. * denotes $p<0.10$, ** $p<0.5$, *** $p<0.01$. Southern states excluded in both models. Nebraska excluded in Model 1. Years covered 1970-2006.

Surprisingly, I find no effects of partisan control of state government in the absence of a TEL. This requires some explanation. The first thing to note is that the difference-in-differences estimator employed in this paper leverages both across-time and across-state variation. These two types of variation provide two alternative explanations for the null results of partisan effects in the absence of a TEL. First, this result could be a consequence of party polarization being greater in TEL states than non-TEL states. If voters systematically adopted TELs in states with relatively greater party polarization, then it would follow that we see greater partisan effects in TEL than in non-TEL states.

The second explanation recognizes that a difference-in-difference estimator takes advantage of estimation across time. Previous research has recognized a growing degree of elite polarization (Jacobson 2007; McCarty, Poole, and Rosenthal 2007). This trend towards polarization may very well have caused state-level party effects on fiscal outcomes to become more prominent over time. Given that TELs are more likely to be in place at the end of the time series than at the beginning, and that temporal pattern correlates with the rise of party polarization, it is not surprising that we find stronger party effects when states adopt TELs rather than prior to adoption.

One of the weaknesses of this estimation strategy is the limited number of observations of unified Republican and Democratic control. Of the 1152 state-year observations in this analysis, only 68 state-years have unified Republican control and 34 state-years have unified Democratic control in the presence of a spending limit (in ten unique states). To achieve greater variation in this key independent variable, I also examine the effect of the partisanship of the governor's office on TEL implementation. This approach is merited given the strength of many governors in state budget processes.

In many states, the governor is the first-mover in the budget process, laying out the initial proposal to be evaluated by state legislatures (Kousser and Phillips 2009). Additionally, numerous state governors have ex-post control through the use of line-item vetoes. Though there is wide variation between states, the presence of strong governors in some states supports this estimation technique.

Model 2 of Table 2.1 presents results for the effect of the governor's office on TEL implementation. I employ a variable coded as one if a state has a Democratic state governor, zero otherwise. The omitted group is states led by governors of all other parties, most notably Republicans but also Reform party members. An interaction of this variable with the TEL indicator reveals even stronger results for the predictions made by the principal-agent model. These results suggest that a spending limit under the implementation of a Democratic governor once again increases state and local spending growth rates, this time by 1.4 percent. It is also the case, however, that TEL implementation is successful under Republican and Reform Party governors, decreasing state and local growth rates by 1.2 percent. Once again, there are no significant party effects on state and local spending growth rates in the absence of a TEL.

The previous results strongly support the conjecture that the preferences of the political agents affect the degree that an initiative is implemented faithfully. I turn now to a second prediction of the principal agent model, that costly oversight prohibits faithful implementation.

V. Importance of Monitoring

Information asymmetry is a central component of the principal-agent problem. Any or all use of ex-post corrections is contingent on the principal having information about the agent's actions. This is particularly crucial in the case where voters are the principals and elected officials are the agents. If the voters have information of whether or not their elected officials are evading the tax and expenditure limit, they could potentially punish their agents come Election Day (Ferejohn 1986). In this manner, information is a prerequisite for employing "the big club behind the door" and modifying agent behavior through the law of anticipated reactions (Weingast 1984).

A foundational conclusion of the principal-agent literature is that monitoring involves costs. In economic realm, the firm bears the costs associated with monitoring its agents (Alchian and Demsetz 1972). Similarly, Congress is also willing to bear the costs associated with monitoring, whether it is through active, oversight committees (Weingast and Moran 1983) or through the less-costly method of constituent fire-alarms (McCubbins and Shwartz 1984). When voters are the principals, however, there may not necessarily exist an organized interest that will readily bear the costs of monitoring the implementation of initiatives.

This section argues that the effectiveness of TELs hinges on the ability of principals to monitor their agents. The empirical analysis employed herein takes advantage of the fact that some limits are less costly to monitor than others. Specifically, I argue that limits tied to individual pocket books will be less costly to monitor, and therefore more effective, than limits tied to aggregate coffers. The logic supporting this prediction is that if a taxpayer knows how a TEL should affect his or her tax-growth from year to year, that taxpayer would easily be able to pull the fire-alarm on a violation of the limit on his own. This form of monitoring is not costly and requires no collective action. For example, a wronged homeowner could bring a lawsuit against an assessor unlawfully increasing assessed values.

Variation among property tax limits provides a test for the monitoring hypothesis. Property tax limits fall in two general categories: revenue limits and assessment limits. Revenue limits dictate the amount of revenue from property taxes that a taxing entity (municipality, county, special district) can collect from year to year. For example, Massachusetts's property tax limit stipulates that aggregate municipal property tax collections can only rise 2.5 percent per year. Revenue limits are always aggregate-level limits because they are calculated at the level of a government or a number of overlapping governments.

Assessment limits are another way to limit property tax growth. Assessment limits place restrictions on the growth of assessed valuations of individual properties. Most famously, California's Proposition 13 limits the assessed value of a property to 2 percent growth per-year. When assessment limits are combined with a tax-rate limit, such as California's maximum tax-rate of 1 percent of assessed value, an individual's property tax burden becomes predictable from year-to-year. I will refer to this category of property tax limitations as individual-level limits.

My argument is that the latter form of TEL, an individual assessment limit combined with a tax-rate limit, is easier to monitor than an aggregate revenue limit. If a voter has some ability to predict his or her individual tax bill from one year to the next under strict implementation of the limit, then that voter has the ability to pull a fire alarm if taxes exceed their expectation. This calculation does not require collective action, nor does it require costly analysis of government tax records.

I test the hypothesis that ease of monitoring affects TEL effectiveness in two ways. First, I compare individual property tax bills in the state of California (individual property tax limit adopted in 1978), to those in the state of Massachusetts (aggregate-level property tax limit adopted in 1980). I show that even though the limits in the state appear to be similar - a 2.5 percent limit in Massachusetts versus a 2 percent limit in California – California's limit has been far more effective at reducing individual property tax burdens, at least for individuals who have not sold their homes.

Second, I employ a difference-in-differences model on panel data to test the effectiveness of individual-level property tax limits versus aggregate-level property tax limits. The results suggest that individual-level limits have been more successful over the last 30 years at reducing property tax revenue growth rates.

Illustrative Case-Study Analysis

Analysis of sample property tax bills in Massachusetts and California illustrates how individual-level property tax limits have resulted in decreased tax burdens. The individual-level assessment limit in California (in conjunction with a strict tax rate limit) mandates that a homeowners' property taxes will only increase two percent per year. In contrast, property taxes in the state of Massachusetts mandates that municipal revenues from property taxes will only grow 2.5% per year.

Table 2.2 presents actual property tax bills for homeowners in the state of California and Massachusetts from 1982-2000. Taxpayer A is from San Diego, California and Taxpayer B is from Marblehead, Massachusetts. Neither home-owner sold their property during this time period. These two particular properties were chosen exclusively for convenience, with the caveat that I tried to match the level of property taxes paid in 1982. Since rules governing assessed value vary significantly between states, I have no way of knowing whether the values of these two homes are similar. Again, because these properties are were not randomly selected, the analysis here should only be taken for illustrative purposes, not causal analysis.

As is clear from Table 2.2, property taxes for the homeowner in Massachusetts far outgrew the property taxes for the homeowner in California. This large of divergence would not be expected if the only difference between the laws was that one state limited property taxes to two percent growth while the other limited taxes to 2.5 percent growth. The property taxes of the California homeowner only grew by more than 2% on three occasions. In contrast, the property taxes of the Massachusetts homeowner grew by more than 2.5% percent for a total of ten years in this series. The Massachusetts homeowner also experienced more volatility in his taxes, despite limited volatility in the assessed value of his home (with the exception of the move to full and fair cash value in 1985).

Table 2.2: Tax Bill Illustration

	Taxpayer A - California		**Taxpayer B - Massachusetts**	
Year	*Property Taxes*	*Percent Change*	*Prperty Taxes*	*Percent Change*
1982	$1,834.60	-	1,889.40	-
1983	$1,748.84	-4.67%	$1,931.70	2.19%
1984	$1,766.33	1.00%	$1,974.00	2.14%
1985	$1,795.74	1.66%	$3,535.20	44.16%
1986	$1,770.57	-1.40%	$3,535.20	0.00%
1987	$1,915.03	8.16%	$3,667.32	3.60%
1988	$1,896.21	-0.98%	$4,109.94	10.77%
1989	$1,933.35	1.96%	$2,644.38	-55.42%
1990	$1,828.38	-5.43%	$2,903.58	8.93%
1991	$1,838.52	0.55%	$3,128.35	7.18%
1992	$1,742.08	-5.25%	$3,214.17	2.67%
1993	$1,927.72	10.66%	$3,416.19	5.91%
1994	$1,891.06	-1.90%	$3,789.76	9.86%
1995	$1,917.05	1.37%	$3,956.72	4.22%
1996	$1,920.43	0.18%	$4,010.90	1.35%
1997	$1,982.23	3.22%	$4,177.74	3.99%
1998	$1,977.55	-0.24%	$4,212.00	0.81%
1999	$2,010.02	1.64%	$4,534.44	7.11%
2000	$2,007.90	-0.11%	$4,962.87	8.63%

The take-away message from this illustration is that aggregate-level property limits are less effective than individual-level property limits, at least for homeowners who have owned their home since the passage of these limits. I will explore this question again more methodically in the proceeding panel data analysis by looking at aggregate property tax revenue outcomes.

Panel Data Analysis

In the following analysis I once again employ the difference-in-differences estimator to test for significant changes in fiscal outcome growth rates. Specifically, I test whether individual-level property tax limits are more effective than aggregate-level property tax limits at reducing the growth of state and local property tax revenues.

I code individual-level property tax limits as one, zero otherwise, if a state has implemented an assessment limit that dictates the growth of assessed valuation for homestead parcels in conjunction with a tax-rate limit. The assessment limit must be at the level of an individual parcel, not statewide, aggregate valuation. If a state has both a revenue limit and individual-level limit in place, I code them in the group with a high degree monitoring, namely individual-level limits. There are nine states that fall into this category for at least one year of the time series.

I code aggregate-level property tax limits as one, zero otherwise, if the state has implemented a revenue limit that dictates the growth of property tax revenues. This group also includes states that limit growth of statewide assessed values in aggregate, such as Iowa. There are 23 states that fall into this category for at least one year of the time series.

The same difference-in-differences estimation is employed in this estimate as the initial analysis on property tax limits in Chapter 1, with the sole exception that I will estimate separate coefficients for aggregate-level and individual-level limits. Again, the analysis is estimated using panel-corrected standard errors with the following equation:

$$\Delta(\ln)FISCAL_{it} =$$

$$\alpha + \beta_1 TEL_{it} + \beta_2 \Delta(\ln)ECON_{it} + \beta_3 \Delta(\ln)POP_{it} + \beta_4 POL_{it} + \lambda_i + \kappa_t + u_{it}$$

The results of this estimation appear in Table 2.3. As is evident from the Table, neither individual-level property tax limits nor aggregate property tax limits reach conventional levels of significance. That said, the coefficient on individual-limits is much negative while the the coefficient on aggregate limits is positive. Additionally, the p-value for individual-limits not large (p=0.25).

Table 2.3: Effect of Ease of Monitoring on TEL Effectiveness

DV: 3Δ State and Local Property Taxes (Real)	
Aggregate Property Limit	0.009
	(0.015)
Individual Property Limit	-0.028
	(0.024)
3Δ Total Population	2.658***
	(0.606)
3Δ Total Employment	-0.394
	(0.251)
Unified Democratic Control	-0.008
	(0.010)
Unified Republican Control	0.003
	(0.008)
3Δ School-Age Population	-0.224
	(0.279)
3Δ Elderly Population	-0.866**
	(0.369)
3Δ Personal Income	-0.133
	(0.110)
Year Effects	Included
State Effects	Included
Constant	0.0284
	(0.0416)
Observations	588
Number of statenum	49
R-squared	0.477

Notes: Panel-Corrected Standard errors in parentheses. * denotes p<0.10, ** p<0.5, *** p<0.01. Years covered 1970-2006.

The insignificance of the individual-level property tax limit variable is explained in part by the heterogeneity of limitations across states. Whereas California's strict limit only allows two percent growth per year, Oklahoma's assessment limit allows for five percent increases in valuation per-year. Currently, lawsuits are being sought against Oklahoma assessors, some of whom have publicly commented that they feel obliged to raise assessments five percent per year regardless of how much a house has gone up or down in value (presumably to achieve equity across homes purchased at different times). If the three cases with high assessment limits are excluded from the analysis (Arkansas, Oklahoma, and Texas), the coefficient on individual tax limits decreases to -0.061 (a 6.1% drop in growth rate) and the p-value drops to 0.79, nearing significance. The results are, in the least, suggestive that at least a subset of individual-level property tax limits are effective at reducing the growth of property taxes in the long-term.

Monitoring Failures

The results presented in this analysis beg the question of how public officials are able to raise taxes and still escape detection by taxpayers. There are a variety of ways that local officials could change rules governing the calculation of aggregate property revenue limits in order to collect more taxes, many of which would likely go unnoticed by tax-payers. The following anecdote shows how rule changes can affect property tax bills in ways that are difficult, if not impossible, to monitor.

A common stipulation in an aggregate revenue limitation is a "new growth provision". A new growth provision states that the revenue limit increases annually in accordance with additional valuation of new construction and other allowable growth in the tax base that is not the result of property revaluation. In laymen's terms, this means that new construction will allow the limit to be set higher than it would in the absence of construction. Theoretically, new growth should not affect pre-existing taxpayers as the existence of new growth should always imply the existence of new taxpayers.

One possible consequence of new growth provisions, however, is that taxpayers fail to recognize is that revenue limit increases due to new growth are actually being passed onto pre-existing taxpayers over time. It is important to note that a revenue limit can only increase, not decrease, over time. This ratchet effect has consequences both in cases of foreclosure and when a piece of land goes from one class of property to another. For example, if a plot of land held by a non-profit organization is sold to a for-profit organization, the revenue limit would increase in accordance. In contrast, if the reverse situation occurs and the land becomes tax-exempt, the limit would not decrease. Instead, the revenue limit would remain the same and surrounding homeowners would be subject to increased tax burden to make up the difference. A corollary to this situation is a company that develops in a municipality, but goes out of business or leaves town; the tax limit does not fall with these changing circumstances, thereby allowing to municipalities to raise property taxes without constraint.

It is very difficult to monitor precisely how and when new-growth provisions cause tax-burdens to rise over time. Taxpayers will always enter and leave the system, and small rule can greatly affect who owes taxes. As a result, aggregate-level limits become very difficult to monitor. In the least, they require collective and costly action to monitor.

Conclusion

Based upon the logic of principal-agent relationships, I doubted that those who enact tax and spending limits would be able to constrain the future actions of lawmakers possessed of different goals and direct control of state purse strings. In my initial analysis in Chapter 1 of this dissertation I found that most TELs, most of the time, were ineffective at stopping the growth of state and local government. Using predictions derived from the classic principal-agent model, however, I found that TELs can be effective under certain circumstances. Specifically, spending limits were effective at cutting the growth of state and local spending under the direction of Republican governors. When those charged with implementing initiatives are favorable to the initiative's goals, faithful implementation will ensue.

Additionally, TEL effectiveness also hinges on ease of monitoring. I found that the TELs that are the least costly to monitor, those constructed at an individual-level, are in at least some subset of cases, effective at reducing property tax growth. In contrast, aggregate-level property tax revenue limits were never effective. The conclusion that can be drawn from these results is that TELs may be effective if the limit can be constructed so that oversight is not costly.

These findings constitute an empirical test of the validity of the principal-agent model for policy implementation in direct democracy. The proscriptions suggested herein, as well as additional solutions to the principal-agent problem discussed in previous literatures, can be broadly applied for all types of initiatives. For example, one solution to the principal-agent model I have not yet discussed is the importance of administration procedures (McCubbins et al 1987, 1989). Unlike congressional bills, which often set up implementation committees or set up strict reporting requirements, initiatives are rarely passed with accompanying enabling legislation. Initiative proponents should look towards the tactics taken by legislatures who are aware that implementing agents may be hostile to heir policy preferences.

Chapter 3

Secondary and Tertiary Consequences of Property Tax Limits

I. Introduction

In this chapter I analyze the effect of property tax limitations on state and local revenue during economic recessions. I argue that the changes to revenue policy precipitated by property tax limits cause short-term instability during fiscal crises. My work continues a string of research that argues that fiscal limitations often have unintended secondary and tertiary consequences. Instead of cutting the size of government, scholars have shown that public officials almost always find ways to circumvent the spirit of most taxing, spending, and deficit limitations (Kiewiet and Szakaty, 1996; Gerber et al., 2001; Kousser, McCubbins, and Moule, 2008). For example, if a limit only restricts property taxes, a locality might switch to revenues derived from charges and fees or sales taxes. Likewise, if a revenue limit only restricts state revenues, a hike in property tax collections at the local level might ensue. These evasion techniques, while increasing government size, allow public officials to abide by the letter of the law. This chapter analyzes the tertiary consequences of these actions.

Despite these known evasiation tactics, the complaints of politicians regarding the bite of voter proscribed revenue limitations are especially shrill during recessions. I consider the hypothesis that tax caps lead to greater short-term declines in revenue during recessions than would otherwise occur in the absence of these caps. I posit that property tax limits and the politics of circumvention that they engender have a tertiary effect, aggravating the effects of public economic crises. Specifically, I argue that tax revolt legislation has led state and local governments to rely on sources of revenue that are increasingly elastic with respect to changes in personal income. These new revenue sources are less stable during recessions than the previous mainstay of state and local government revenue, the property tax. As a result, state and local revenues are more pro-cyclical, they grow quickly during economic booms and crash during recessions.

This chapter tests the hypothesis that property tax limits aggravate revenue declines during fiscal crises by analyzing time-series, cross-sectional data for the U.S. states. During the time frame analyzed in this chapter, all 50 states experienced multiple economic declines. For the purposes of my test it is especially useful that the states do not suffer downturns at the same time and are not subject to property tax limits at the same time. This wide array of variation allows us to estimate the interaction effect between property tax limits and recessions. My results support the hypothesis that property tax limits aggravate revenue declines in state and local governments during recessions. This suggests that states would have fewer and more modest financial problems during economic downturns if they did not enact tax limitations.

This chapter proceeds as follows. In the next section I describe the data used in this chapter. In Section III I review the previous literature with respect to the consequences of property tax limits on government revenue streams. In Section IV I show that property tax limits lead to increases in income taxes and the assessments of charges and fees. In Section V I tie these consequences with what is known about revenue stability during fiscal downturns from the public finance literature. In particular, I show that state income taxes, charges and fees have a relatively high income-elasticity. In Section VI I present a model of the effect of property tax limits, recessions, and their interaction. In so doing, I conclude that property tax limits aggravate revenue declines during recessions. In Section VII I summarize my conclusions.

II. Data

I start by describing the data used in all statistical analyses in this chapter. My key independent (i.e., treatment) variable is an indicator for the presence of a property tax limit (see Table 3.1). The following rules were used to determine the existence of a property tax limit. First, the limit must restrict property taxes for all geographic areas of the state (no local options). Second, if the limit does not restrict all taxing entities (e.g., it only limits municipalities but not special districts), then constrained taxing entities must collect a majority of the state's property taxes. Third, the limit must be either a revenue limit (pegging increases in total property tax revenues to an explicit rule) or it must stipulate both a tax rate limit *and* cap the growth of assessed property values simultaneously. This last rule means that states that have assessment limits but not tax rate limits are excluded from consideration. The tax limit indicator is coded as one in a fiscal year if a state has an active property tax limit and zero otherwise. Note that several states have repealed their tax limits over my time series.

Table 3.1: Statewide Property Tax Limits in the US

State	Years Implemented
Arizona	1981–Present
Arkansas	1982–Present
California	1979–Present
Colorado	1993–Present
Florida	1995–Present
Idaho	1980–1992, 1996–Present
Indiana	1980–Present
Kansas	1986–1998
Kentucky	1980–Present
Maine	2006–Present
Massachusetts	1982–Present
Michigan	1979–Present
Missouri	1981–Present
Montana	1987–Present
Nevada	1984–Present
New Mexico	1980–Present
Oklahoma	1997–Present
Oregon	Start–Present
South Dakota	1997–Present
Utah	Start–1986
Washington	1974–Present
West Virginia	1991–Present
Iowa	1979–Present
Louisiana	1979–Present
Wisconsin	2006–Present
Mississippi	1995–Present

I collected a variety of covariates standard in the state and local finance literature to control for other factors that affect revenue collections. Specifically, I control for three measures of population fluctuation: total population, elderly population (as defined by the number of individuals over the age of 65), and school-age population (this group was approximated using the age category 5–19). Each of these variables was collected from annual estimates of statewide residents by the Census Bureau. I also control for state political characteristics using a set of dummy variables that indicate the existence of unified Republican government, unified Democratic government, or divided government (Alt and Lowry, 1994, 2000). For modeling purposes, divided government is omitted and used as the reference group for the two other dummy variables. Finally, I control for state personal income and statewide total employment to hold constant changes in the economy. Both of these variables were collected from the Bureau of Economic Analysis. Descriptive statistics for these variables and others appear in Table 3.2.

Table 3.2: Summary Statistics of Data

Variable	Mean	Std. Dev.	Min	Max
Percent Income Taxes	16.28	8.20	0	32.88
Percent Sales Taxes	25.43	8.22	5.16	45.71
Percent Charges and Fees	30.91	5.41	16.71	53.90
Percent Property Taxes	20.93	7.61	5.70	50.47
School Age Population (millions)	1.12	1.18	0.11	7.65
Elderly Population (millions)	0.63	0.66	0.04	3.56
Total Population (millions)	5.12	5.45	0.45	33.50
Total Employment (millions)	2.72	2.88	0.26	18.50
Personal Income (millions)	151.3	177.3	11.1	1216.2
Unified Republican Control	0.30	0.46	0	1
Unified Democratic Control	0.16	0.36	0	1
General Revenue Growth (Δ LN)	0.03	0.04	–0.16	0.22
Recession Indicator	0.36	0.48	0	1

Note: This data represents information from 49 states, Alaska excluded, from 1980–2000

Another variable used in some of my analysis is a measure of *state recessions*. Our data here comes from work by Owyang, Piger, and Wall (2005). These authors produce data that measures the number of quarters per calendar year that each of the fifty states should be classified as being in recession between fiscal years 1980–2001. I use this data to create an annual indicator of *state recessions*. Specifically, I classify a state as being in recession if at least three quarters of its fiscal year have a recession probability greater than 0.5.[1]

Owyang, Piger, and Wall (2005) estimate state recession probabilities quarterly for each of the 50 states from 1980–2001 using the Markov-switching model developed by Hamilton (1989). Hamilton's method estimates endogenously the timing of shifts from expansion to contraction of the economy. This model estimates when the mean growth rate switches between high and low growth regimes.[2] This estimation procedure produces recession probabilities, ranging from zero to one that represent the probability that a state is in a recession in a given quarter. In this analysis I rely on a simple cut-off method to identify whether or not a quarter can be classified as in a recession. If the recession probability is greater than 0.5 during any given quarter, a state is coded as being in recession for that quarter. This cut-off rule is non-controversial as Owyang, Piger, and

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[1] The cut-off of at least three quarters in recession was chosen through a non-parametric estimate of the effect of each additional quarter of recession on general revenues. The results of this estimation showed that a state must have three or four quarters of recession to see a statistically significant decline in general revenues. The size of the coefficients for three and four quarters were statistically indistinguishable, suggesting that a dummy variable specification of this variable is superior to a count variable that assumes a linear relationship between revenue outcomes and the numbers of quarters in recession.

[2] The underlying data used to calculate recession probabilities is a state-level coincident index by Crone (2002). Crone's widely used index follows the methodology developed by Stock and Watson (1989) for the national economy. Crone uses three monthly and one quarterly economic indicator to estimate the underlying state of the economy. These indicators are nonagricultural payroll employment, unemployment rate, average hours worked in manufacturing, and real wage and salary disbursements. This data is preferable to other economic indicators because it displays substantial business cycle variability (unlike personal income) and is available on a quarterly basis (unlike gross state product) for each state.

Wall (2005) report that recession probabilities are regularly either close to zero or close to one.

Finally, the dependent variables in all analysis are measures of state and local fiscal behavior. I rely on data from the Commerce department's publication of Annual State and Local Government finances. The variables included in my analysis are state and local *general own-sources revenues*, *income taxes, sales taxes, property taxes,* and *charges and fees*. The exact specification of each of these variables will be detailed in the discussions of research design that precede all statistical analyses.

III. Consequences of Property Tax Limits on Government Revenues

In this section I review previous findings on the secondary consequences of property tax limits. Before the tax revolt even ended, newspaper columnists and policy experts immediately identified ways in which property tax limitations would change government fiscal structure. Since then, empirical tests have confirmed many of these speculations (Danziger and Ring, 1982; Joyce and Mullins, 1991; Mullins and Joyce, 1996; Kousser, McCubbins, and Moule, 2008). Specifically, property tax limits increase a state's reliance on charges and fees, sales taxes, income taxes, and the use of off-budget activities (Bennet and DiLorenzo, 1982; Schwartz, 1997; Thompson and Green, 2004).

A significant conclusion of the previous literature is that property tax limits lead to increases in income and sales taxes. Specifically, Thompson and Green (2004) show that Oregon's property tax limit prompted the state to rely more heavily on income taxes. Skidmore (1999), using data from all 50 states, show that local government restrictions lead to growth in state aid to local governments. This is clearly the case in Massachusetts. Increases in state aid occurred immediately after the adoption of their property tax limit, Proposition 2 ½. Though this increase was initially sustained by a strong economy, the so-called "Massachusetts Miracle", the state was later forced to raise the flat rate personal income tax to sustain high levels of state aid.

By contrast, increases in the use of the sales taxes were evident in California. Several scholars have argued that localities have been turned into “sales-tax farms”, affecting redevelopment, zoning, and eminent domain, favoring car dealerships and significant shopping malls over mom–and–pop businesses. This activity even garnered a name, the “fiscalization of land use.” (Schwartz, 1997; Lewis, 2001)

There is also strong evidence that property tax limits increase assessments of charges and fees. Charges and fees are assessed in a variety of forms: increases in college tuition, business licenses and fees, charges for school lunches, park fees, impact fees, or costs associated with public parking. Many property tax bills today are now loaded with "special assessments" in lieu of ad valorem property taxes (Kogan and McCubbins, 2009). Sometimes, the assessment of charges in fees instead of property taxes is a simple case of substitution: water bills that were once subsidized by local government property taxes and now paid for in full directly by the user in the form of standby charges or sewerage fees (Moule, 2010).

Alternatively, charges and fees can also be a consequence of changes to the structure of government. Previous research suggests that property tax limits splinter government revenue sources. Instead of classic budgetary procedures where the whole of government spending is allocated from general revenue sources, property tax limits led to the creation of special funds and devolve finances to newly formed special districts or enterprises. Bennet and DiLorenzo's (1982) early work on this subject posited that property tax limits led to a "massive amount of off-budget spending and borrowing". In particular, Bennet and DiLorenzo were concerned with the proliferation of "off-budget enterprises," the political entities referred to as authorities, districts, commissions, or agencies. Most recently, Bowler and Donovan (2004) found that property tax limits were the cause of special district formation, at least in states that heavily used the initiative process. Special districts and the like, given their purpose of service delivery, are likely to rely on user–fees instead of traditional taxes.

To bolster and systemize this evidence, I conduct my own test of whether or not property tax limits increase reliance on sales taxes, income taxes, and charges and fees. I rely on a differences-in-differences model (Wooldridge, 2006) to estimate the effect of property tax limits on the relative usage of each revenue stream. This model allows us to hold constant unobserved, time-invariant state-level characteristics that predict state and local revenues. Additionally, this model controls for variation of the dependent variable related only to the passage of time that is constant across all states.

My dependent variables are constructed as the specific revenue stream (charges and fees, sales taxes, income taxes, and property taxes) as a proportion of general own-source revenues. Because the errors across these equations are likely to be correlated, I employ a seemingly unrelated regression model (Zellner, 1962). This model shows statistically significant negative correlation in the error terms between each revenue source, as one would expect when these taxes are substitutes for each other.

Measuring these variables as a proportion of general revenues relieves some of the pernicious autocorrelation that often concerns analysis of fiscal outcomes in differences-in-differences analysis (Bertrand et al. 2004). As an added precaution, however, I present results for a limited subset of data. Specifically, the analysis that follows only includes data for every fifth year starting in 1977. This method is preferable to first differencing, another effective way of removing serial correlation, in this instance because I are able to retain my dependent variable in levels, as opposed to changes. My hypothesis predicts that property tax limits will affect the level of reliance on each revenue stream. Using every fifth year of data only slightly attenuates the significance of my findings.

I regress my dependent variables on an indicator for property tax limits as well as an array of covariates and state and year fixed effects. My model is estimated by (1):

(1) $$Y_{it} = \beta_o + \beta_1 \mathrm{T}_{it} + \beta_2 \theta_{it} + \kappa_t + a_i + u_{it}$$

Where:

y = fiscal outcome as a proprtion of general, own–source revenues
T = indicator a property tax limit
θ = Covariates
κ = Year fixed effects
a = State fixed effects

Table 3.3: Effect of Property Tax limits on Revenue Components

	Charges and Fees	Income Taxes	Property Taxes	Sales Taxes
Property Tax Limit	0.82 (0.44)*	0.91 (0.43)**	-1.67 (0.44)***	-0.09 (0.49)
School Age Population	0.43 (1.96)	-0.94 (1.88)	-1.57 (1.93)	0.38 (2.16)
Elderly Population	3.44 (3.31)	-4.64 (3.17)	7.32 (3.26)**	-5.31 (3.64)
Total Population	-0.51 (1.11)	-0.41 (1.06)	2.45 (1.09)**	-0.17 (1.22)
Employment	-1.32 (1.35)	1.70 (1.29)	-4.93 (1.33)***	3.79 (1.48)**
Personal Income	0.02 (0.01)**	-0.003 (0.01)	0.002 (0.01)	-0.03 (0.01)***
Unified Democratic Control	0.64 (0.40)	-0.71 (0.39)*	-0.07 (0.40)	-0.37 (0.44)
Unified Republic Control	-0.19 (0.30)	-0.03 (0.28)	-0.31 (0.29)	0.68 (0.33)**
Constant	41.34 (1.21)***	16.14 (1.16)***	4.16 (1.22)***	34.98 (1.41)***
Observations	294	294	294	294
R-squared	0.904	0.951	0.946	0.938

Note: Estimated using seemingly unrelated regression. * signifies that the coefficient is significant at the 0.1 confidence level; ** at the 0.05 level; and *** at the 0.01 level. The dependent variables are state and local revenue components as a percentage of state and local general own–source revenue. Continous covariates are in millions. Years estimated are 1977, 1982, 1987, 1992, 1997, and 2002. Alaska is excluded.

The results of my estimation are displayed in Table 3.3. Each continuous covariate (total *employment, personal income*, and all population variables) is transformed to represent the effect of a million–unit change. As is clear from the table, however, the covariates generally do a poor job predicting reliance on each type of revenue stream, with the noticeable exception of the property tax.

As predicted the indicator for the presence of a property tax limit has noticeable effects on revenue choices. As intended, the adoption of a property tax limit decreases reliance on property taxes as a proportion of general revenues. Specifically, the adoption of a property tax limit is associated with approximately a 1.66 percentage point reduction of property taxes relative to general own-source revenues. In contrast, the adoption of a property tax limit is associated with increased reliance on both income taxes as well as the assessment of charges and fees (while the latter is not significant at conventional levels of confidence). Cumulatively, there is perfect substitution between declines in property taxes and increases in income taxes and charges and fees, as the latter increase 1.73 percentage points cumulatively. Finally, contrary to the previous literature, I find no statistically significant relationship between property tax limits and the sales tax in this model.

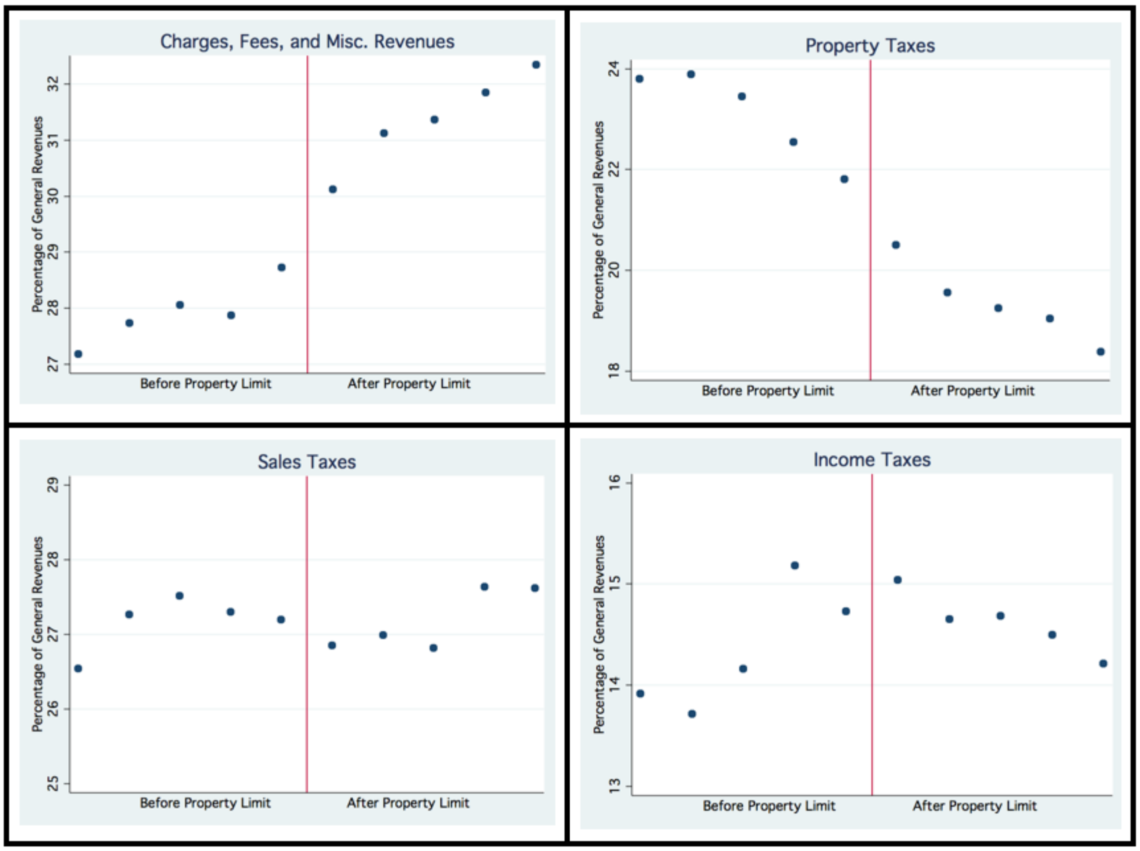

Figure 3.1: Reliance on Revenue Sources, Before and After Property Tax Limit Implementation

Figure 3.1 supports the statistical results. This figure presents four graphs, each showing average reliance on each revenue source five years before and five years after states implement property tax limits. States where data is not available for this full timespan are excluded (Maine, Wisconsin, Idaho, Oregon, Utah), as are states that never adopt limits. The vertical line in each figure represents the implementation of the limit. These figures show descriptively that property tax limits are associated with a decreased reliance on property taxes and increased reliance on charges and fees and to a lesser extent on income taxes. Again, there is no clear relationship with property tax limits and reliance on sales taxes.

Interestingly, the affect of property tax limits appears to occur shortly before the official implementation of the limit. This may mean that lawmakers change revenue policy in anticipation of TEL implementation, perhaps at the time when the limit is adopted. Another possibility is that the early changes in revenue policy are reflections of other events that are correlated with the adoption of property tax limits. This possibility led Kousser, McCubbins, and Moule (2008) to conclude that TELs, by themselves, are not responsible for the declines in total state own-source revenue. For example, state legislatures commonly adopt property tax cuts or change assessment practices in an attempt preempt the passage of limits at the ballot box. Regardless of the exact timing, it is clear that property tax limits are significantly associated with changes to revenue policy. In the next section, I more thoroughly explain the implication of this consequence during fiscal downturns, turning financial molehills into mountains.

IV. Estimating Short–Run Revenue Stability

I argue in this chapter that shifts in revenue streams associated with property tax limitations have had deleterious affects on state financial health during recessions. My conclusion is drawn from a literature in public finance that tells us that many of the new revenue sources that states rely on to replace lost property tax revenue are income-elastic. Research in public finance shows that income-elastic revenues lead to larger revenue growth in the long-run but are less stable in the short-run during a fiscal crisis.

The most comprehensive examination of short-term revenue instability during fiscal crises is by Holcombe and Sobel (1997). The authors present an error-correction model of tax elasticity. Elasticity refers to the responsiveness of revenues to changes in personal income. They find that corporate income taxes, personal income taxes, and non-food retail sales taxes are income-elastic whereas taxes on fuel usage and liquor sales are income-inelastic. Although they do not formally test the elasticity of property taxes (which is generally a local, not state revenue source), they characterize this revenue as "relatively stable over the business cycle." (Holcombe and Sobel, 1997, p. 186). Looking at state-level data, Bruce, Fox, and Tuttle (2006) found that short-run income elasticity was greater for income taxes than for sales taxes.

An omission in the literature is the absence of analysis on the elasticity of charges and fees. Charges and fees are now the largest single revenue source for state and local governments in many states (McCubbins and Moule, 2009). In this chapter I replicate the aforementioned results on the income-elasticity of tax revenue sources, and present new results on the elasticity of charges and fees and property taxes.

I rely on the method described by Holcombe and Sobel (1997) to estimate the short–run elasticity of state and local revenue sources. For this analysis I use data from the Department of Commerce on aggregate state and local revenue components from 1963–2005. This dataset includes a breakdown of state and local revenues into sales taxes, personal income taxes, corporate income taxes, property taxes, and other revenue sources, nationwide. This last category includes motor vehicle license taxes, other taxes, charges and fees, and miscellaneous revenues. All variables are transformed to constant dollars using the consumer price index.

Optimally, to estimate income elasticity it is best to have data on tax bases, not tax revenues. As explained by Holcombe and Sobel, elasticity estimates will be biased if policy decisions to raise or lower taxes are correlated with economic changes. Though this is a consideration in my analysis, Holcombe and Sobel's own estimates show that there is a strong correlation between estimates derived from tax bases and tax revenues. Further, it would be difficult, if not impossible, to estimate the "tax base" from which charges and fees are drawn. As such I follow previous analysis, including Box, Fox, and Tuttle (2006), and estimate the income elasticity of actual revenues.

Holcombe and Sobel (1997) develop an error correction model to estimate short–run income elasticity described by (2):

$$\text{(2)} \qquad \Delta \ln(R_t) = \alpha + \beta_1 \Delta \ln(I_t) + \beta_2 (\mathrm{E}_{t-1}) + \varepsilon$$

Where R_t is the time-series of a revenue component, I_t is the time-series of state *personal income*, and E_t is a variable used for *error-correction*. As described by Holcombe and Sobel, error correction is necessary in the estimation of short-run elasticity because "Two non-stationary variables that have a long-run relationship with one another will tend to move back together whenever they get too far apart (a regression to their mean relationship). Thus one may observe one variable moving down in the same period another is moving up simply because the variables deviated from the levels implied by their long-run relationship" (Holcombe and Sobel, 1997, p 83). Here, the error correction variable is the lagged residual derived from an estimate of long-run elasticity (as discussed by Sobel and Holcombe, 1997).

Table 3.4 presents the short-run income-elasticity estimates of the major components of state and local revenue. These coefficients represent the percentage change in the revenue component associated with a one percent change in state personal income. The results largely confirm the analysis by Holcombe and Sobel. Corporate income tax revenue has the highest-elasticity, varying by 2.83 percentage points for every one percent change in total state personal income. This result is graphed in Figure 3.2. Changes in corporate income tax revenues follow roughly, and magnify nearly three-fold, changes in personal income. The results for personal income taxes are very similar, with an income-elasticity of 2.17.

Table 3.4: An Error Corection Model of Short–Run Income Elasticity, 1963–2005

Revenue Component	Coefficient
Corporate Income Tax	2.83 (0.52)***
Personal Income Tax	2.17 (0.31)***
Charges and Fees	1.06 (0.15)***
Sales Tax	0.93 (0.13)***
Property Tax	0.12 (0.16)

Note: Estimates are from a regression of changes of logged personal income on changes of logged revenue sources. Error correction from long–run elasticity estimates are employed. Analysis uses Commerce Department data of state and local government revenues (constant dollars) and BEA annual estimates of national personal income (constant dollars) from 1963–2005. R–squareds range from 0.07 (property taxes) to 0.59 (pesonal income taxes). * signifies that the coefficient is significant at the 0.1 confidence level; ** at the 0.05 level; and *** at the 0.01 level.

Table 3.4 also shows that receipts from "other" sources, largely charges and fees, have elasticity greater than one. Again, this level of elasticity means that this revenue source will fluctuate more than the general economy. The income-elasticity of charges and fees is not surprising given what I know about consumer behavior during recessions. As the most recent Census data shows, recessions stop consumers from getting married, moving, immigrating, and a variety of other behaviors associated with government fees for services. Revenues from impact fees, charges paid by real-estate developers for development projects, certainly slow or can even cease during downturns. If citizens are not paying as much charges and fees during recessions, revenues will go down even when costs for the government are fixed. The elasticity of charges and fees is graphed against income in Figure 3.3. Again, as supported by the regression data, this revenue source matches and magnifies changes in the economy.

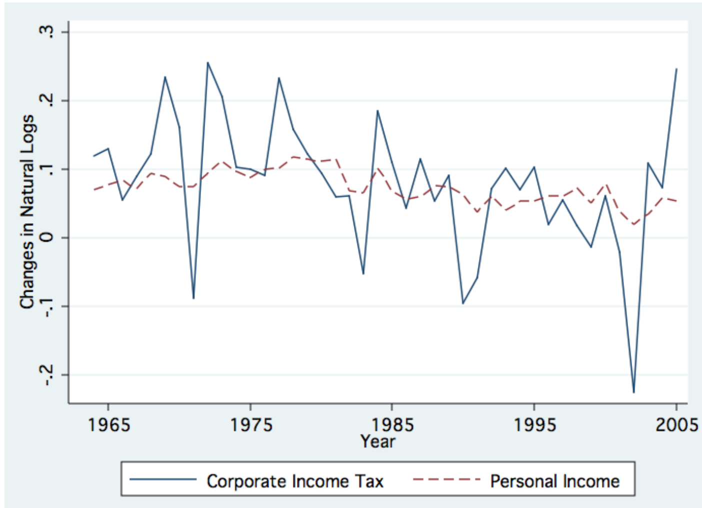

Figure 3.2: Income–Elasticity of Corporate Income Taxes

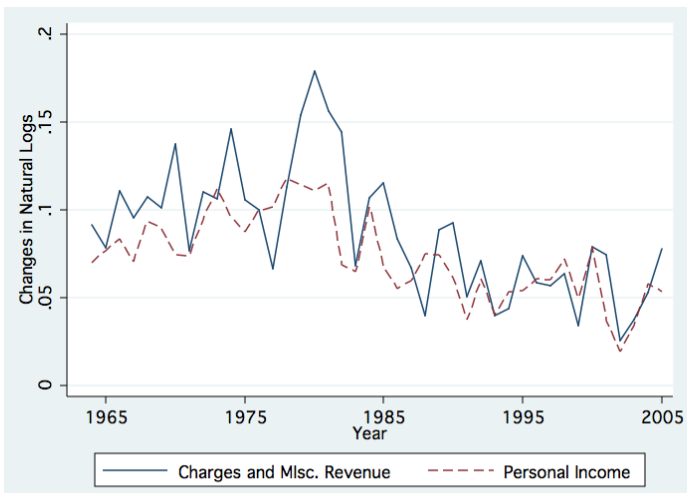

Figure 3.3: Income–Elasticity of Charges and Miscelaneous Revenues

I also confirmed through this analysis that property tax revenues are highly income-inelastic. Of the five revenue sources analyzed herein, property taxes are the only source of revenue that is not significantly predicted by changes in personal income. Figure 3.4 plots the change in log state and local property tax revenue with the change in log personal income. As evident from the figure, property tax revenues often appear almost counter-cyclical.

Figure 3.4: Income–Inelasticity of Property Taxes

The important lesson from this analysis is that some revenues will be more stable than others during times of fiscal crisis. Although the most recent fiscal crisis was precipitated by falling home prices, historically property values are stable during downturns. Moving away from property taxes to more elastic forms of revenue, such as charges and fees or income taxes, could make states more susceptible to cyclical volatility. This danger was recognized by Holcombe and Sobel who noted that, “If the trend away from local reliance on property taxes continues, however, local governments may not be as insulated from recessionary fiscal crisis in the future.” (Holcombe and Sobel, 1997, p. 51).

The consequence of increased elasticity after adoption of a property tax limit is particularly clear in the case of Oregon. Although Oregon officially had a binding property tax limit in place at the beginning of the time series (it passed a levy-limit of 106 percent growth starting in 1916), Oregon passed additional limitations in 1990, 1996, and 1997 (the 1996 limit was never implemented). The change in revenue policy before and after 1990 has particularly noticeable implications for elasticity. Prior to 1990, Oregon had a levy-based system of calculating property taxes. Local governments passed a budget and deducted the amount of state aid from the total. The remaining revenue requirement would determine that year's property tax rate. The anti-cyclical behavior of this system, as noted by Thompson and Greene (2004), is that the level of tax burden from year to year was highly dependent on state aid. As noted by Thompson and Greene (2004), "fluctuations in state school aid were not random; the state legislature tended to increase funding during economic upswings and cut it during recessions, thereby exacerbating the local property tax's bite" (Thompson and Greene, 2004, p. 75). The adoption of the 1990 property tax limit eliminated the property tax as a revenue safety net during recessions. Oregon's revenues have become far more income-elastic following the adoption of this limit (Thompson and Greene, 2004).

V. Property tax limits and Recessions

I turn now to my central analysis, the effect of property tax limits during recessions. In this section I test whether property tax limits aggravate revenue declines during fiscal downturns. I rely on indicators from Owyang, Piger, and Wall (2005) for statewide recessions. The Owyang, Piger, and Wall (2005) data is a significant improvement to previous research that simply relied on national-level recession data. As shown by Owyang, Piger, and Wall (2005), there is tremendous variation between states regarding business cycles. This finding is not surprising given the diverse economies of the fifty states. Using this data I are able to take advantage in the rich variation in state business cycles to produce more accurate estimates of their effects.

I estimate the effect of property tax limits, recessions, and their interactions on state and local general, own-source revenue using differences-in-differences. As previously mentioned, this model holds constant trends common to states over time as well as unobserved, time-invariant state-level characteristics. I do, of course, sweep many of the requirements for the Stable Unit Treatment Value Assumption (SUTVA), such as unconfoundedness, under the rug, however. I estimate the following equation:

$$\Delta y_{jt} = \beta_0 + \beta_1 \mathrm{T}_{it} + \beta_2 \Psi_{it} + \beta_3 \mathrm{T} * \Psi_{it} + + \beta_4 \Delta\theta_{it} + \beta_5 \gamma_{it} + a_i + \kappa_t + u_{it} \tag{3}$$

Where:

y = fiscal outcome
T = indicator a tax limit
ψ = indicator of a recession
θ= Population and Economic Covariates
Y= Political Covariates
κ = Year fixed effects
λ = State fixed effects

All continuous variables are log-transformed and first-differenced. This specification is common with econometric data, particularly in the study of short-term effects of fiscal crisis. First-differencing is particularly helpful in eliminating autocorrelation. However, because the Breuch-Pagan Test for residual autocorelation was affirmative, I also employ a lag dependent variable, as suggested by Beck and Katz (2009). Removing serial correlation is important to my analysis because, as noted by Bertrand, Duflo, and

Mullainathan (2003), serial correlation often causes one to underestimate standard errors in differences-in-differences estimation leading to mistaken rejection of the null hypothesis. Indeed, from a series of simulations, those authors found effects "significant at the 5 percent level for up to 45 percent of the placebo interventions." (Bertrand, Duflo, and Mullainathan, 2004, p. 1).

The coefficients of the model are interpreted as effects on state and local general, own-source revenue growth rates. My results are presented in Table 3.5. I present two models of my results, the second excluding the continuous economic variables as they are highly correlated with the recession indicators. Here I report the results for the first model. I find that both *elderly* and *school-age population* variables are insignificant, but that that a one percentage point change in total population leads to a 2.1 percentage change in the growth of general, own-source revenues. Unified Republican and Democratic control of state government has the expected, although only weakly-significant effects on revenue growth, increasing growth during unified Democratic control and decreasing growth during unified Republican control. The inclusion of economic variables in model 2 has negligible effects on these findings.

Table 3.5: The Effect of Recession and Tax Limits on Revenue

	Δ LN General Revenue	
	Model 1	Model 2
Lag DV	-0.146 (0.064)**	-0.151 (0.064)**
Δ LN School-Age Population	-0.071 (0.207)	-0.023 (0.204)
Δ LN Elderly Population	-0.411 (0.308)	-0.404 (0.305)
Δ LN Total Population	2.119 (0.345)***	1.971 (0.449)***
Δ LN Total Employment		-0.036 (0.156)
Δ LN Personal Income		0.149 (0.094)
Unified Democratic Control	0.006 (0.004)*	0.006 (0.004)*
Unified Republican Control	-0.005 (0.003)*	-0.005 (0.002)**
Recession X Limit	-0.003 (0.002)**	-0.003 (0.001)**
Property Tax Limit	0.006 (0.004)	0.006 (0.004)
Fixed Effects	included	included
Year Effects	included	included
Recession	-0.012 (0.004)***	-0.010 (0.004)**
Constant	0.0375 (0.010)***	0.032 (0.010)***
Number of Obs.	1029	1029
R-Squared	0.396	0.401

Note: Panel Corrected Standard Errors are in parentheses. There are 49 states included; Alaska is excluded. Covers the years 1980–2000. * signifies that the coefficient is significant at the 0.1 confidence level; ** at the 0.05 level; and *** at the 0.01 level.

The most important independent variables in this model are the effects of tax limits, recessions, and their interaction of the two on revenue growth rates. The results suggest that *property-tax limits*, in absence of a recession, have no effect on general revenue growth. This finding replicates the findings in previous research (Kousser, McCubbins, and Moule, 2008). However, new to this chapter is the finding that property tax limits do in fact have significant effects on revenue during recessions.[3] The interaction variable

[3] This result is also consistent with the possibility that Property tax limits themselves are binding only during recessions. This alternative hypothesis has some anecdotal evidence to support it, and is a reasonable possibility given the fact that many property tax limits are tied directly to an index of economic indicators, such as growth in personal income or the inflation rate (Poterba and Reuben, 1996; National Council of State Legislatures, 2009). We do not reject the possibility that this is an additional mechanism by which property tax limits reduce revenues during recessions.

suggests that in the presence of a recession, a tax limit would decrease state and local own-source general revenue by an additional 0.3 percent. This result is significant at the 5 percent level. This decline should be interpreted cumulatively with the overall affect of recessions, which adds an additional 1.2 percent decline in revenue. States that enact property tax limitations fare much worse than states without limits during recessions. Given likely heterogeneity of the effectiveness of property tax limits, these results are likely underestimate the effect of these limits.

VI. Conclusion

I have demonstrated that property tax limits have negative effects on state and local revenues during fiscal crises. Property limits cause states to rely on income-elastic revenue sources, such as the income tax or charges and fees. The consequence of this substitution is apparent when you look at how these revenues are differentially affected by the economy. For many years, property taxes were a highly inelastic form of revenue, a source of stability in the face of personal income declines. Greater reliance on an income-elastic revenue source will result in greater revenue declines during economic downturns. This was shown in the negative and significant interaction effect between the recession indicator and property tax limits.

My results suggest that states, in response to tax limits, are builidng a revenue system that puts them on a budgetary roller-coaster with huge swings between the apex of the coaster's climb and the nadir of its fall. As it seems unlikely that politicians will choose to limit spending during the good times, and so far attempts to adopt strict Rainy Day Funds have been limitted.

Scholars of the origin of California's Proposition 13 have identified the highly progressive, and thus income-elastic, state income taxes as an immediate cause of the property tax limit. The economic boom of the late 1970s prompted high taxes and large surpluses. Paradoxically, as this chapter has shown, the passage of the property tax limit only aggravates the problem of elastic revenues. It is ironic that heavier reliance in income-elastic revenues will, in the long-term, have the opposite effect of the tax reformer's intentions. In the long term, income-elastic revenue sources grow at rates higher than the economy itself. This means that it is plausible that tax reforms have actually set the course for the higher growth of government.

Chapter 4
The Tax Revolt in Massachusetts

I. Introduction

Since 1920, voters in Massachusetts have used the initiative process to mold public policies to popular will. They defined what constituted intoxicating liquors during the years of prohibition (neither cider nor beer), decided when sporting events could take place (Sunday was fine), and even repealed prohibition in one fell swoop[1]. In 1980, Massachusetts' citizens took aim at another consequential public policy, taxes. Their central argument was that property taxes were too high and growing too fast. Their goal was to take away tax rate discretion from public officials, and instead peg all property tax growth to a fixed rate. Their magic number was two and half: the property tax rate could be no larger than 2.5 percent of assessed valuation, and tax growth could not occur at a rate larger than 2.5 percent annually.

Like many other states' tax revolts in the 1980s, Proposition 2 ½ fell in the shadow of California's Proposition 13. A mere two days after Proposition 13 passed in June of 1978, the concept of a 2 ½ percentile limit on property taxes was floated by a Boston newspaper columnist[2]. One day after that, four Republican legislators introduced a bill that incorporated this concept (Susskind 1983, Chapter 1).

Momentum for a tax revolt, however, was not found in the legislature of Massachusetts. At the time the bill was introduced, the legislature only had one month left in its session and the bill was defeated before session's close. With that defeat,

[1] http://www.iandrinstitute.org/Massachusetts.htm
[2] Kenney, Charles. "How the Tax Revolt Came to Massachusetts\ A Citizen's Group Took the Issue to the People" Boston Globe-May 11, 1981.

several legislators turned towards the taxpayer's organization Citizens for Limited Taxation (CLT) to harness popular momentum for the tax revolt.

In August of 1978, CLT took the lead in filing an initiative to limit property taxes. Though the Attorney General rejected their first initiative attempt on technical grounds, they were able to pass a non-binding initiative calling for the legislature to pass legislation to cut property taxes. This non-binding initiative passed by large margins in the general election, fueling CLT to continue fanning the flames of a Massachusetts tax revolt. A revised version of the property tax initiative was approved by the Attorney General in the summer of 1979, and signature gathering occurred through the next year to officially put Proposition 2 ½ on the ballot in November of 1980.[3]

As noted, the original proposition limits property taxes in two parts, commonly referred to as the "levy ceiling" and "levy limit". The levy ceiling dictates that municipalities cannot impose a property tax rate higher than 2.5% of assessed values. The levy limit states that the maximum allowable amount of property taxes collected by a municipality cannot grow faster than 2.5% annually. Today, the levy limit is almost always below the levy ceiling, and it is the lower of the two limits by which the municipality must abide. [4]

Per Massachusetts' law, direct democracy initiatives have the full force of law but do not amend the constitution. In other words, they are only statutory provisions. State

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[3] Kenney, Charles. "How the Tax Revolt Came to Massachusetts \ A Citizens' Group Took the Issue to the People". Boston Globe-May 11, 1981

[4] Proposition 2 ½ did much more than limit property taxes. Provisions were also written to limit vehicle excise taxes ($25 per thousand dollars of valuation), prohibit unfunded mandates, allow renters to deduct one-half of rent from state income taxes, end binding arbitration in public labor disputes, and end autonomous budget control in local school boards. The latter change was significant, as many people considered school budgets to be inflated s a result of declining enrollment. Here, however, we will exclusively focus on the initiative's effect on property taxes.

legislators have full discretion to amend the statute upon majority vote. However, since legislators are not the agents charged with levying municipal property taxes, this law is still considered binding in my analysis.

After the passage of Proposition 2 ½ there was constant political discussion of whether or not the state legislature would respect Proposition 2 ½. This chapter revisits this discussion, analyzing the degree to with the proposition remained binding over time. At the time of the initiative's passage, the common perception was that the force of the initiative rested not in the power of the law, but in the mandate of the people. If the margin of victory constituted a large-enough mandate, Proposition 2 ½ would work, otherwise it would not. As reported by the Boston Globe, one anonymous legislator said that "If it passes by 51-49 percent, we have a little flexibility, but if it passes by 55-45, the law will be cast in concrete."[5] Similarly, Gregory Hyatt of CLT noted that, given a high margin of victory, "There'll be a lot of caterwauling by voters if the Legislature tries to frustrate their intent."[6] The final vote of Proposition 2 ½ was 59-41, suggesting a strong mandate.

This chapter takes a different perspective on what makes an initiative binding. Instead of popular mandate, my theory puts emphasis on agency theory. Specifically, using the case of Massachusetts I will discuss the importance of monitoring and complete contracts. These two mechanisms must be in place in order for an initiative to be respected over time (Kiewiet and McCubbins 1991; Gerber et al 2000).

In the case of Massachusetts, I will argue that the effectiveness of Proposition 2

[5] Robinson, Walter V. "What if Prop 2 ½ Passes?" Boston Globe-November 2, 1980
[6] Robinson, Walter V. "Prop 2 ½" Boston Globe-November 5, 1980

½ has decreased over time because the letter of the law did not effectively incorporate these mechanisms. First, the state legislature was able to significantly amend the limit subsequent to its passage with limited media attention or voter redress. Second, Proposition 2 ½ failed to provide for sufficient monitoring by tying levy limits to aggregated figures at the level of the municipality. Had the limit been tied to individual tax bills, taxpayers could have more easily blown the whistle on rapidly increasing taxes. Third, Proposition 2 ½, perhaps purposely, is not a complete contract since it fails to take into account revenue substitution. As I will discuss in detail, increases in state aid and charges and fees meant that the Proposition did not cut the size of government, but only changed its revenue sources.

This chapter is comprised of three sections. The first section looks at how the letter of the law has changed over time. I describe in detail the components of the levy limit and how legislative amendments have loosened the degree to which Proposition 2 ½ constrains municipalities. Many of these changes have occurred without significant public attention, highlighting failures in monitoring. The next section more closely analyzes the ease of monitoring compliance to Proposition 2 ½. I show that limits calculated at the level of an individual taxpayer are easier to monitor than the formula employed by Proposition 2 ½. In the last section, I look at how the spirit of Proposition 2 ½ was evaded by substitution. The limitation increased the usage of other revenue sources, namely state aid and charges and fees. These sections reflect how Proposition 2 ½ was an incomplete contract for the purposes of cutting taxes across the board.

The conclusions of this chapter is that Proposition 2 ½ has changed Massachusetts' municipal finance, altering its structure entirely while not necessarily effecting the level of burden placed upon the people. This departs from previous research that found that Proposition 2 ½ significantly reduced revenues (Bradbury et al 1997; Galles and Sexton 1998)

II. The Letter of the Law

This section explores the letter of the law of Proposition 2 ½. I begin by describing the anticipated consequences of property tax cuts, contrasting initial fears with eventual realizations. I then show that municipal property taxes have grown greater than 2.5% per year. I describe amendments to the law that have occurred since the limit's passage, explaining how each amendment contributes to growing tax property burden in the state of Massachusetts.

The projections of revenue cuts in the first year of Proposition 2 ½ were dire. The Department of Revenue predicted that Massachusetts' cities and towns would lose $557 million in revenue from the Proposition's first year cuts from the combined force of limited property taxes and slashed excise taxes. Towns and cities would have to cut property taxes by an average of 41.6%, with cuts closer to 75% for the cities of Boston and Chelsea.[7] At the time, Governor King accused municipal officials of "saber rattling" by announcing cuts for public effect. This was particularly true for the Mayor of Boston who announced mass firings in the police and fire departments, even though many of the officers and firemen were rehired prior to their official layoffs.

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[7]Robinson, Walter V. "The Impact of Proposition 2 ½" Boston Globe-October 10, 1980

While some of the statements made by public officials may have been political fear mongering, it is also true that the estimates by the Department of Revenue were highly inaccurate. The inaccuracy was in part the result of rapid reevaluations that would occur in preparation for implementation Proposition 2 ½. Cutler et al. 1999 show that Proposition 2 ½ caused an initial reduction in property taxes in 42% of Massachusetts municipalities, with the average city forced to cut taxes by 16%.

The Massachusetts Institute of Technology project studying the effect of Proposition 2 ½, aptly named “IMPACT: 2 ½”, provides substantial insight on the immediate consequences, or lack thereof, of the proposition. Through case studies and analysis of aggregate data, that project’s conclusions were that “In the end, local officials did not have to make the deep cuts that had been predicted” (Susskind and Horan 1983, p 266). They identify the major elements of preventing major cuts were revaluation, state aid increases, hiring freezes, and rising usage of charges and fees. In addition, the state was able to make significant cuts in education without affecting service as a result of declining student enrollments. As noted by Oliff and Lav (2008), “Between school years 1980 and 1989 the number of K-12 students in Massachusetts fell by 21 percent, reducing school costs. And because the enrollment decline was a continuation of an earlier trend, schools were likely better positioned to consolidate services when Proposition 2 ½ took effect than they otherwise would have been.”

Reevaluation was an important way for municipalities to prevent drastic, initial cuts in property taxes (Davies 1985, Susskind and Horan 1983). Although all municipalities had been mandated to assess at full and fair value in 1974 by a ruling of the Massachusetts Supreme Court, only 98 communities had done so by 1981 (Bradbury et al 1983). The reluctance to comply was due to the fact that state aid formulas benefited municipalities with lower assessed values. However, since under Proposition 2 ½ municipalities could only collect 2.5% of assessed values, municipalities now rushed to assess and full and fair values in fiscal years 1982 and 1983. For some communities, such as Burlington, reevaluation allowed the municipality to actually increase property taxes in 1982 instead of making the expected cuts (Susskind and Horan 1983).

While declines may not have been as severe as expected, there is no question that aggregate property taxes decreased in the wake of Proposition 2 ½. Whereas almost $3.4 billion was collected in property taxes in 1981, only 2.9 billion was collected in 1982, the first full year of implementation.[8] This is only year in this time series where property taxes declined in nominal dollars. This suggests that immediate tax cuts enacted through direct democracy can be effective.

Despite the effective decrease in property taxes in the first year of Proposition 2 ½, the question remains as to whether can voters control the growth of government long into the future. The case of Massachusetts provides little evidence for this conjecture. Municipal property taxes have consistently increased in the state of Massachusetts, notably beyond a rate of 2.5% annually. Figure 4.1 displays aggregate state-wide municipal property taxes from 1981 to 2006. Again, the first year of Proposition 2 ½ implementation is 1982.

[8] Data (nominal) from U.S. Census Bureau, Annual Survey of State and Local Government Finances, Government Finances, Volume 4, and Census of Governments (1977-2007).

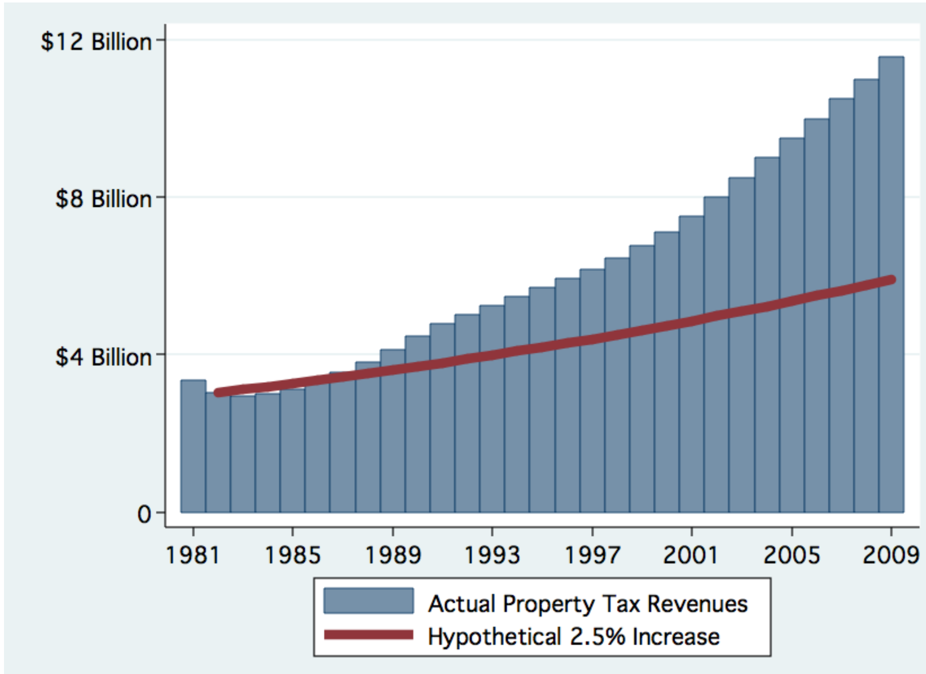

Figure 4.1: Actual vs. Hypothetical Property Tax Growth Under Proposition 2 ½

Property tax growth at a rate higher than 2.5% raises a red flag. The initial limit passed by the voters provided little flexibility to realize growth higher than 2.5%. When the limit passed, the only exception to this benchmark was if the legislature proposed an override, which then would need to be approved by two-thirds of municipal voters – a very high bar to pass. To understand how taxes have grown higher than 2.5% legitimately under Proposition 2 ½, one must look closely at changes to the letter of the law. This section will argue that the provisions that govern the levy limit have weakened over time, steadily decreasing the extent to which the Proposition 2 ½ reigns in property taxes.

The maximum levy limit currently has four components.[9] Of these, the writers of the proposition only envisioned the first two methods. First, and most intuitively, the maximum levy can only grow by 2.5% each year. Second, the voters of a community can pass overrides to permanently raise the maximum levy by a set amount. Third, voters can also pass capital or debt exclusions, which allow communities to raise additional property taxes for debt service costs or capital expenditures for a set amount of time. In some situations, city councils or boards of selectmen can also pass special exclusions. Finally, new growth in the form of new construction or renovations cumulatively and permanently adds to the maximum levy.

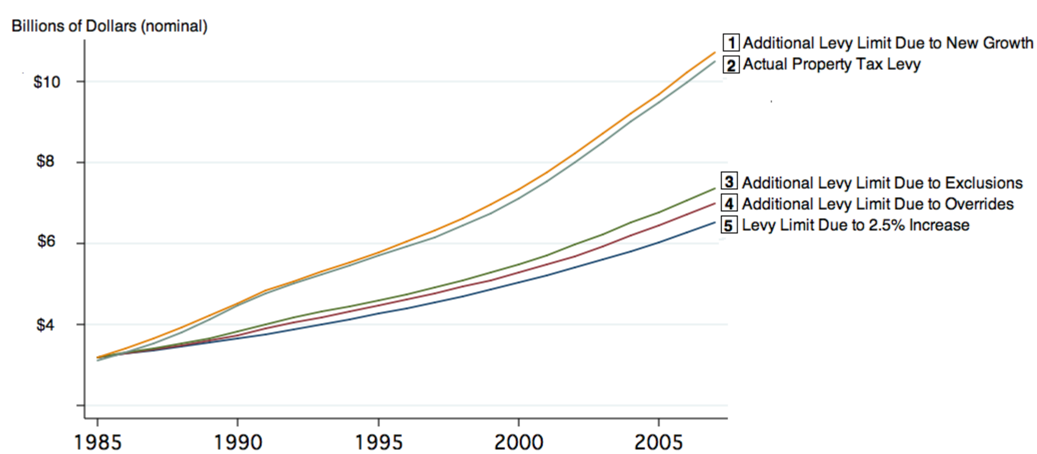

Figure 4.2: Components of Property Tax Limit Growth

[9] As previously noted, Proposition 2 ½ says that municipal property taxes must fall below both a levy limit and a levy ceiling. The levy limit dictates the annual growth of property taxes, while the levy ceiling says that property taxes cannot be higher than 2.5% of assessed values. In this discussion, I will ignore the levy ceiling since rising property values make it consistently higher than the levy limit.

In the following section, I will look at the effects of overrides, exclusions, and new growth in detail. I will review how the legislature has changed the rules governing these allowances since Proposition 2 ½ passed at the ballot box. I will also discuss the ease of monitoring each allowance. Before moving on to these descriptions, however, it is helpful to understand the degree to which each of these components has affected the size of the levy limit historically.

Figure 4.2 breaks down the maximum allowable levy limit into its separate components to gauge each component's significance. This figure is helpful in understanding how it is that property taxes have grown at a rate higher than 2.5% in the aggregate. The top line (line 5) presents the maximum levy limit aggregated to the state level, in other words the sum of all municipal levy limits. The line that falls directly below it (line 4) represents the amount of property taxes collected statewide. The actual tax levy runs fairly close to the maximum levy throughout the time series. The distance between the two is closest in 1992, a year where total taxes collected were only $23 million shy of the maximum allowable. The distance between the tax levy and maximum levy limit is greatest in the last year of data available, 2008. In this year, taxes collected were $220 million (nominal dollars) less that the allowable limit statewide.

The lowest line on Figure 4.2 (line 1) represents what taxes would have been given a 2.5% increase annually, starting in 1985[10]. In 2008, this amount is equal to almost half of what is actually collected in property taxes today. In addition to the base 2.5% increase, Proposition 2 ½ allows additional levies for new growth, voter overrides, and debt or capital exclusions.

[10] This is the first date when data is available at the municipal level.

As shown in the figure, overrides and exclusions account for only a small portion of levy increases. The gap between line 1 and line 2 represents the amount of additional property tax levies allowable as a result of voter overrides. Overrides permanently add to the maximum levy limit, so the effect of an override is cumulative over the time series. In 2008, overrides accounted for almost $523 million worth of additional collected property taxes, or 4.8% of all property taxes collected.

The space between line 2 and line 3 represents the amount of additional property taxes collected as a result of exclusions. Unlike overrides, exclusions are not permanent additions to the levy limit, and as such do not cumulatively affect the levy. That said, they still have a noteworthy effect on the maximum levy limit annually, accounting for $355 million extra tax dollars in 2008 alone, or 3.2% of the maximum levy limit.

Finally, the space between lines 3 and 5 depicts the amount of additional tax dollars that can be collected as a result of new growth allowances. As evident in the figure, new growth has the most significant effect on how much property taxes are collected in Massachusetts today. I turn now to a more in-depth description of each of these components.

Overrides

Since 1982, there have been 4,350 proposed voter overrides of Proposition 2 ½, of which 1,750 passed. In many ways, Proposition 2 ½ overrides are, by definition, effectively monitored from the outset since they require voter approval. The bigger monitoring problem, however, is one that arises over time. Overrides are permanent additions to levy limits, so voters are not only approving increases in their taxes that year, but also for all future years. Very few underrides are ever proposed (16 total), suggesting that overrides are rarely ever undone.

This section looks at trends in the passage of Proposition 2 ½ overrides. Overrides have become easier to pass in localities for two reasons. First, the legislation governing override proposal and passage has changed substantially from the original voter-approved initiative. These changes have made overrides both more likely to be proposed and more likely to pass. Second, municipalities have made overrides more politically palatably by changing how they are framed to the public. Commonly, overrides are now proposed for smaller sums of money and are promised to specific purposes. These two changes have arguably led to more override passages than originally intended by Proposition 2 ½ drafters.[11] The ease of passing overrides contributes to rising property taxes, particularly since overrides accumulate over time.

The original legislation contained in Proposition 2 ½ gave the power to propose local overrides exclusively to the state legislature. Overrides were only allowed in November general elections, and had to pass with two-thirds super-majority approval by voters. The intent, as recently described by CLT, was a "safety net" for municipalities in case of emergency.[12][13] Early amendments to Proposition 2 ½ (1983) changed these rules

[11] Barbara Anderson reflected on the increases passage of overrides in a recent newspaper column. She noted that "To CLT's surprise and dismay, some local voters began passing overrides, not just for emergencies, but for operating expenses, including pay raises and public employee benefit levels." Anderson, Barbara. "Look at the bright side: Property taxes still going up, but could be worse" The Salem News. Thursday, December 18, 2008

[12] Anderson, Barbara. "Override Mania: You pay more so city, town employees can get more" The Salem News. Thursday, May 10, 2007

[13] Overrides could be considered a safety-net in that trends in override proposal have closely follow economic swings. Overrides increase in hard economic times and decrease during economic booms[13]. Specifically, overrides were most common during the downturn from 1989-1992, and began increasing once again in the year 2000. Overrides were particularly encouraged in 1991, when Governor Weld publicly promoted the concept of a "Super Tuesday" as a panacea for fiscally strapped municipalities.

to allow Selectmen or City councils to place an override on a local ballot during any election with a two-thirds council vote. Even more significantly, these amendments also allowed a community to pass an override with a simple majority vote, as long as the override did not increase the levy by a rate greater than 5%. In 1987, this law was amended again so that all overrides, regardless of size, only need simple majority approval. These changes have significantly effected the number of municipalities able to pass overrides. Less than 7% of all overrides proposed have ever passed with a margin greater than two thirds.

Initially, proponents and opponents of Proposition 2 ½ alike lauded these changes. Notably, Governor King remarked upon the initial passage of these amendments that "It was the people, in the voting booths, who overwhelmingly approved of 2 ½. And it ought to be the people who decide if that vote should be amended in any way. This law ensures that."[14] Even Barbara Anderson of Citizens for Limited Taxation, the group that led the fight to pass the law, has publicly supported overrides. She has been quoted as saying, "It has finally occurred to everyone that an override is not a way around 2 ½ but part of 2 ½. The whole purpose of 2 ½ was to establish the ascendancy of voters over government".[15] Others have had divergent opinions on how overrides affect the intent of Proposition 2 ½. Economist Edward Moscovitch, director of the Massachusetts

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"Super Tuesday" was initiated by legislation calling for a special election on September 24, 1991. During this special election, towns could pass temporary overrides that would raise taxes for a single year instead of permanently raising the levy limit. This would allow communities to recoup lost state aid using temporary property taxes. This was maligned as a public relations stunt by critics, and ultimately called a " super bust" and a "super fraud" due to the lack of community participation. In sum, only one out of Massachusetts 351 communities proposed an override on Super Tuesday, although a record number of regular overrides (597) ended up being proposed in that year.

[14] Collins, Laurence. "King Signs Bill to Ease Impact of Prop. 2 ½" Boston Globe-January 6, 1982

[15] Sleeper, Peter B. "Prop. 2 ½ Override Efforts Increase." Boston Globe - Monday, April 25, 1988 A1

Municipal Association, called overrides the "the time bombs we planted in the measure."[16]

1989 marked another important change in Massachusetts overrides. Prior to that year, Department of Revenue records show that overrides funded the "general operating budget"[17]. Starting in 1989, general overrides gave way to special-purpose overrides, which are exclusively targeted to fund a specific purpose such as schools or fire departments. There were 74 such overrides in 1989 and 133 in 1990. As noted by the Boston Globe in 1990, "'Cafeteria-style' government is on the rise in Massachusetts, as more taxpayers believe that they need pay only for what they order. Yes for plowing, no for schools. Hold the bridge repairs."[18] Figures 3 and 4 chart the trends or passage and proposal over special purpose and general fund overrides.

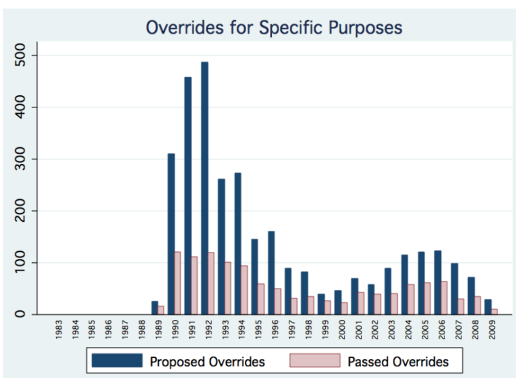

Figure 4.3: Specific Overrides

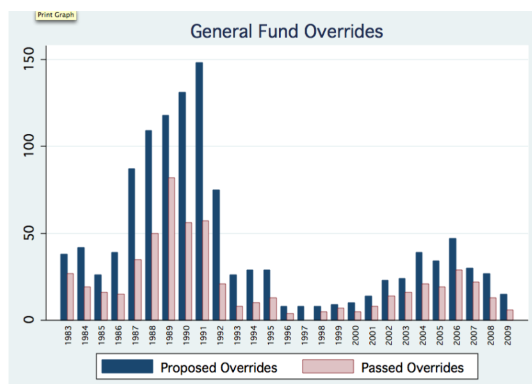

Figure 4.4: General Overrides

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[16] Sleeper, Peter B. "Prop. 2 ½ Override Efforts Increase." Boston Globe - Monday, April 25, 1988 A1.
[17] Department of Revenue, Division of Local Services. Municipal Databank. http://www.mass.gov/?pageID=dortopic&L=3&L0=Home&L1=Local+Officials&L2=Municipal+Data+and+Financial+Management&sid=Ador
[18] Powers, John. "Let them Eat Cake "Cafeteria-Style" Government is on the Rise in Massachusetts" Boston Globe - Sunday, April 1, 1990 page 16

The motivation for the rise in special-purpose overrides, as stated by municipal officials, was to be more politically palatable to voters.[19] Municipalities that had previously proposed but failed to pass general overrides turned to special purpose overrides in the hopes that at least some of them would pass.[20] The conventional wisdom continues to be that special overrides pass at greater rates than general-purpose override, though the aggregate data does not support this conjecture. As shown in Figure 4.5, special-purpose overrides generally pass at rates lower than general overrides. 1172 of 3157 special-purpose overrides have passed (37%) while 578 out of 1193 general overrides have passed (48%). These statistics, of course, are subject to selection bias in that municipal governments only propose general overrides when they are confident that they are going to pass.

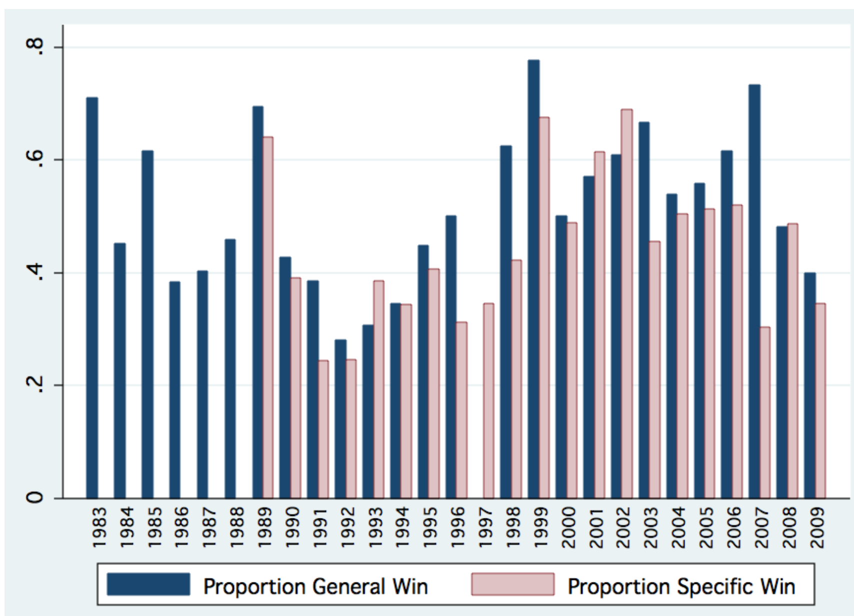

Figure 4.5: Override Passage Rates

[19] O'Brien, Karen. "Hampden voters May Face 3 Override Options on Ballot." Union News. February 1, 1989.
[20] O'Brien, Karen. "Hampden voters May Face 3 Override Options on Ballot." Union News. February 1, 1989.

Figure 4.6 shows the cumulative effect of overrides over time. Over $1.1 billion in property taxes has been collected as a result of voter overrides since 1982. As can be seen from the figure, special-purpose overrides account for almost as much money as general purpose overrides over the time series. This is significant in light of the fact that they carry on average half the price tag (the average override amount of passing general overrides is $1,078,687 compared to $428,649 for passed special purpose overrides).

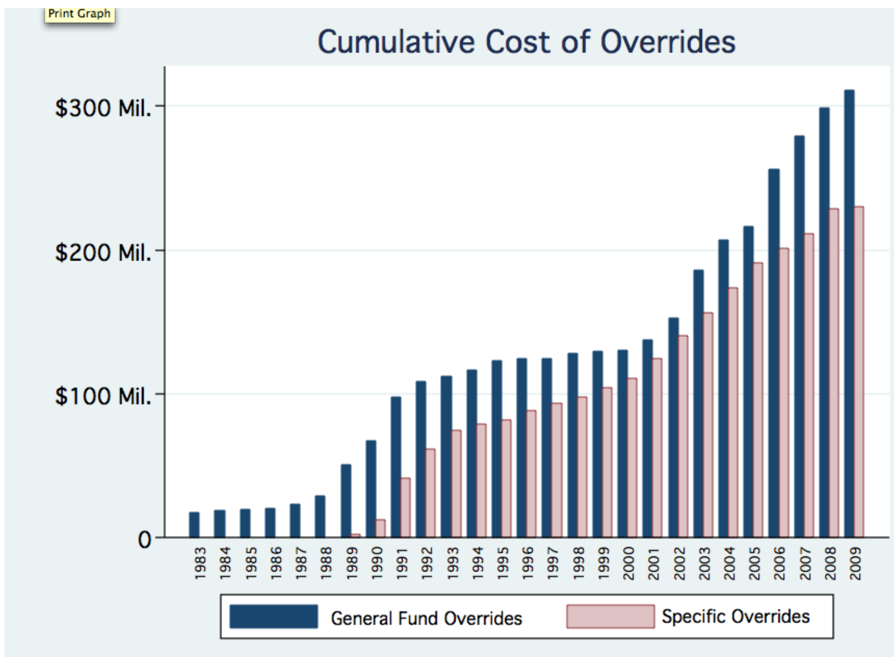

Figure 4.6: Cumulative Cost of Overrides

There is some indication that municipalities will go to the voters repeatedly, often in consecutive years, to get required funds. As summarized by the Boston Globe in 2000, "Officials first propose a big construction plan. Voters reject it in a Proposition 2 ½ override election. Officials trim costs, scale back the project, and put it to the voters again. This time it passes."[21] The data supports this commentary. Of the 597 observations where at least one override was proposed but no overrides passed, 244 of those municipalities attempted another override the very next year. Of those attempts, 127 were successful.

Despite the ease of passing voter overrides, overrides have only had a marginal effect on rising property taxes in the state of Massachusetts. Over the time series, the average effect of overrides per parcel of property is $43 per parcel for special-purpose overrides and $116 for general overrides. In 2007, the median community in Massachusetts had passed a total of two overrides in its history. As will be evident in analysis to follow, overrides only account for a small amount of growth in the property tax burden of a single-family home.

Exclusions

Levy exclusions play a similar role to overrides in rising property taxes. The concepts of levy exclusions were not initially included in Proposition 2 ½. This is significant example of the legislative changes to Proposition 2 ½ that have occurred since its initial passage. Debt exclusions were written into law in 1983, capital exclusions in 1988, and special exclusions in 1993. Debt and capital exclusions, much like overrides, are voter-approved measures that allow for increases in the levy limit. The differences between these types of exclusions and override are three fold. Capital and debt exclusions 1) are limited to costs associated with debt or capital improvements, and 2) only raise property taxes for a set duration of time (generally the life of the loan or the

[21] "Propose, Pare, then Resubmit"; Boston Globe January 30, 2000. Robert Preer

years in which the capital project is conducted), and 3) are not taken into account in the baseline calculation for future levy limits. Debt and capital exclusions are in some ways easier to monitor than overrides since they do not permanently add to the levy limit. Once the exclusion retires, properties taxes will decrease.

Debt exclusions are a particularly popular fiscal innovation. To date, an additional $3.49 billion worth of property taxes has been collected as a result of debt exclusions, including $332 million in 2007 alone. As can be shown in Figure 4.7, the usage of debt exclusions increased during the economic slump of the early nineties, with over 800 being proposed in 1991 alone. To date there have been 8,168 proposed debt exclusions with 5,193 of those achieving majority support.

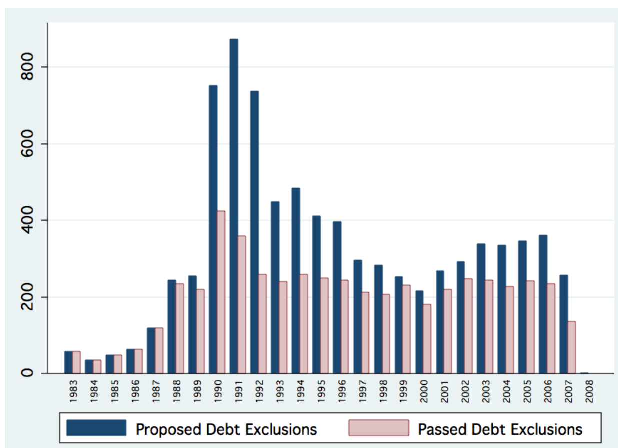

Figure 4.7: Proposal and Passage of Debt Exclusions

Capital exclusions have been used less frequently, but have still had a significant cumulative effect over time. To date there have been 1,356 proposed capital exclusions with 795 of those passing. This has led to an extra $66 million property taxes being collected (approximately 3.47 million in 2007). Proposed and passed capital exclusions are represented annually in Figure 4.8.

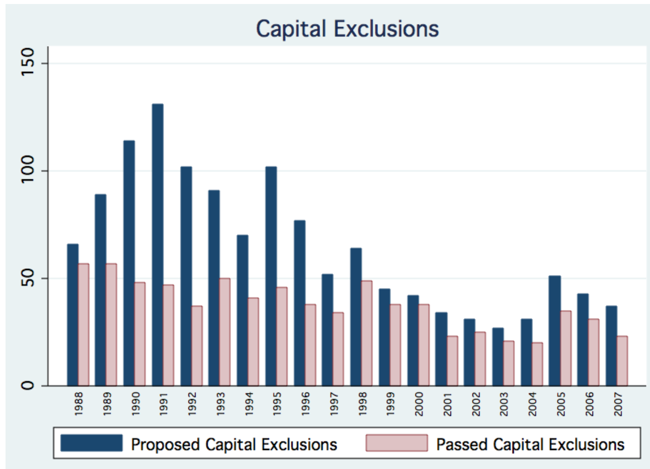

Figure 4.8: Proposal and Passage of Capital Exclusions

The third type of exclusion, special exclusions, is extremely difficult for citizens to monitor. Special exclusions differ even more substantially from overrides, as municipal governments can approve them without affirmation by the voters. Special exclusions are limited to specific costs (debt service), related to the provision of water and sewer services. This type of exclusion was authorized in 1993 legislation as a response to growing complaints about high water and sewer fees (a topic to be discussed further in this chapter). Special exclusions allow a municipality to raise the additional taxes outside its levy limit under Proposition 2½ in exchanges for reducing water and sewer charges in tandem.[22] For example, in 1994 the town of Needham passed a special exclusion, which raised property tax bills on average by $155 per household, while

[22] Property Tax Bureau Informational Guideline Release No. 93-207 October 1993 http://www.mass.gov/Ador/docs/dls/publ/igr/1993/93-207.PDF

lowering water fees by the identical amount.[23] The connection between water fees and property taxes will be addressed in greater detail in the final section of this chapter.

The amount of property tax collected from special exclusions is not readily available from the Department of Revenue. The absence of statistics on this variable is surprising given that detailed information on debt and capital exclusions, as well as total exclusions, are recorded. Simple calculations can estimate special exclusions by taking total exclusions and subtracting from that amount known debt and capital exclusions. This method has high face validity for estimating special exclusions in 2007. In that year, 36 municipalities used special exclusions to raise their levy limit. In sum, special exclusions accounted for over $14.3 million additional property tax dollars in that year. While this figure is low compared to that of the other type of exclusions available, it is still notable given that it represents property tax dollars raised without voter approval.

A closer look at the municipalities using special exclusions suggests, not surprisingly, that these communities were on average more affected by Proposition 2 ½ limits. In 2007, those communities with special exclusions levied property taxes much closer to their maximum levy limit than communities without special exclusions. Specifically, communities passing special overrides had an average excess capacity of $119,223 compared to $631,708 in all other communities. This suggests that municipalities turn to special exclusions not only to counter raising water prices, but also because they are constrained by Proposition 2 ½.

The total amount of property tax dollars collected by exclusions since 1982 is startling. Exceeding 3 billion dollars, this amount has had a cumulatively larger effect

[23] Pappano, Laura. "Drop in Water Bill, Rise in Property Tax." Boston Globe-July 31, 1994 page 3

than overrides, despite the fact that exclusions were never originally envisioned by the drafters of Proposition 2 ½. In 2007, the median number of debt exclusions passed by a municipality since 1982 was eight. On average, the net cost of combined exclusions per parcel per year was $113.89 ($210 per parcel in 2007 alone). Exclusions, much like special-purpose overrides, have heightened "cafeteria-style" government where voters can choose exactly what purposes they want to fund. While this activity is well monitored and promotes the ascendancy of citizens in government, it is nonetheless noteworthy that the ultimate effect is a substantial increase in property taxes beyond what was allowed by the original version of Proposition 2 ½.

As will be discussed further in this section, however, these "voter approved" increases still do not represent the biggest cause of property tax growth in Massachusetts. Much of the increases are a result of less-monitored changes in the levy limit, such as the substantial effect of new growth.

New Growth

Allowances for new growth have the largest effect on the maximum levy limit. Like exclusions, new growth was not considered in the original version of Proposition 2 ½ approved by the voters in 1980, but instead was proposed as a legislative amendment. Before the proposition took effect in 1982, several advocacy groups and public officials, both opponents and proponents of Proposition 2 ½, proposed that new growth add to the levy limit. Their simple argument was that the Massachusetts economy depended on it. For example, the Tax Foundation believed that “the legislature must change the present no-growth provisions in the law. Under the present law municipalities cannot afford to service new development because revenue from that construction can only be used to lower the tax rate." Similar statements were echoed by the Massachusetts Municipal Association, which said that the current state of the law would "strangle the state's economy."[24]

The new growth provision of Proposition 2 ½ was ushered into law simultaneously with amendments concerning the ease of passing overrides, and allowance of debt exclusions. Of these many changes, overrides received the most media attention. In fact, there was no mention of the new growth allowance in Massachusetts' largest newspaper, the Boston Globe, on its report of the new legislation.[25] This is ironic, since new growth has increased the levy limit more than any other component.

The new growth provision of Proposition 2 ½ states that the levy limit increases annually in accordance with additional valuation of new construction and other allowable growth in the tax base that is not the result of property revaluation. More specifically, the amount added to the levy limit is calculated by taking the assessed value of new growth multiplied by the previous year's tax rate for the appropriate property class. New growth includes any new residential or commercial development, condominium conversions, substantial improvements to existing properties, or any parcel of real or personal property that is subject to taxation for the first time.

Over the years, towns have relied on new growth to balance their budgets, particularly in times when inflation was greater than 2.5%. As noted by Anne Carney, past president of the Massachusetts Association of Assessors, new growth "gave you that

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[24] Anderson, Barbara. "Public Figures have their Say on 2 ½." Boston Globe-May 11, 1981
[25] Collins, Laurence. "King Signs Bill to Ease Impact of Prop. 2 ½." Boston Globe - Wednesday, January 6, 1982.

extra measure, so you could give a 5 or a 6 percent pay increase when you were only getting a 2.5 percent increase in tax revenue under Proposition 2 ½."[26] The new growth provision of Proposition 2 ½ was called a "safety valve" because it offset the ever-increasing costs faced by municipalities. [27]

Reliance on new-growth revenues can lead to financial difficulties during slow economic times. When new growth stops, the safety valve is gone. There is some indication, however, that municipalities can "find" new taxable property during tough times so that they can continue to raise property taxes. This is true in three respects: municipalities can 1) look for new construction not counted to their limits historically, 2) redefine what constitutes new growth, and 3) do audits of personal property, which is often vastly undercounted in the state of Massachusetts. Each of these tactics is discussed in turn.

Over the years, the rules concerning new growth have been expanded without much public notice. The first substantial change came in 1987, as municipalities started to feel what would be the beginning of a dip in the Massachusetts economy. In that year, legislation passed to allow communities to submit retroactive growth that occurred from 1983 to 1986. This allowed communities that had not carefully counted new growth in the 1980s, when development was strong, to expand their limit retroactively. Similar legislation passed again in 1989 so that communities could capture growth for the years 1987-1989. Then, in 1991 when a state-wide budget crunch was in full effect, Massachusetts' towns and cities were given legislative authorization to count new growth

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[26] McGrory, Brian. "Revenue Source Starts to Dry Up Towns Bemoan Lack of New Construction Funds. Boston Globe. March 10, 1991. page 1

[27] Nealon, Patricia. "Building Slowdown Jams Municipal Safety Valves." Boston Globe - Sunday, February 3, 1991. *page 1*

that occurred for an 18th month period, January 1 to June 30th of the following year, instead of the normal 12 month cycle. This meant that any new growth up until the day before the next fiscal year would count to the next fiscal year's levy limit. 85 communities chose to enact this local option in 1991, 15 in 1992, and 11 in 1993. Since then, the largest number of cities enacting this option was 11 in 2004.[28] This remains an option for municipalities today who are in need of increasing their levy limit.

Another major change to the new growth provision occurred in 1992. In this year, Massachusetts amended the general law that defined what constituted "new growth". This change allowed new growth to include "all increases in assessed valuation of a parcel or article of personal property over its prior year's valuation, except those attributable to a revaluation or value adjustments in the years between certification".[29] Previously, requirements on what was considered new growth were much stricter. For example, renovations had to be "substantial" in order to count as new growth, with substantial being defined as a 50% increase in valuation from the prior year. With this change, any renovation, no matter how minor could be counted towards expanding the levy limit. There is some anecdotal evidence that municipalities have in practice interpreted this definition of renovation liberally.

Finally, municipalities have frequently turned to the personal property taxes as another source of new growth. Massachusetts' property tax applies not only to real estate, but to personal property as well. While tax rates vary from town to town, all Massachusetts localities can assess personal property taxes for all non-real estate,

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28 Browne, Marilyne. 2008. "New Growth: History and Numbers" in City and Town April 2008/ page 1. available at www.mass.gov/dls

29 Browne, Marilyne. 2008. "New Growth: History and Numbers" in City and Town April 2008/ page 1. available at www.mass.gov/dls

tangible assets. Items in your primary residence are excluded from this tax, but not second homes or most businesses. For example, a restaurant would pay annual personal property taxes on its furniture, counters, baking equipment, kitchen appliances, cleaning supplies and any inventory on hand.

Anecdotal evidence suggests that substantial audits of personal property have taken place when new growth from real estate has declined. For example, this phenomenon occurred in the city of Worcester in 1989. As previously noted, 1989 was a difficult year economically and one in which new construction had slowed substantially. To keep the city solvent, the city conducted a mail survey of 7,000 businesses to assess personal property. The survey asked business to list all their tangible assets, including machinery, shelving, furniture, computers, and inventory. The survey included 2,000 businesses that had never paid property taxes before. Estimates at the time suggested that the survey led to $600,000 in additional personal property taxes collected, out of a total $3.3 million collected. [30] Similarly, the city of Marlboro completed its reassessment of personal property in 1994. They paid a private firm $30,000 to complete a physical inspection of personal property, a task that had not been done since 1982. Their efforts nearly doubled the number of businesses assessed personal property taxes, and increased personal property revenue by $800,000 in that year.[31] These additions were counted as new growth and therefore raised the levy limit.

In addition to looking for new property to tax using surveys or physical inspections, municipality have also benefited by redefining the rules of the personal

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[30] Bliss, Robert R. "More Property Found to Tax." Worcester Telegram & Gazette (MA)-April 25, 1989. Page: A1

[31] Thompson, Elaine. "Audit Yields $800,000." Worcester Telegram & Gazette (MA)-May 11, 1994. Telegram & Gazette. Page: B1.

property tax. Ad hoc rule change has helped fill municipality coffers on multiple occasions. In 2004, for example, the Massachusetts Department of Revenue increased taxing capacity by allowing towns and cities to tax limited liability telecommunication companies for telecommunications equipment such as cables, dishes, and switches. This had a significant effect on municipal finances. For example, the city of Wesboro estimated that this would raise the taxes paid by Verizon wireless from $673 in 2003 to over $910,000 in 2004.[32] In 2008, towns and cities once again changed the rules and gained the ability to tax wires and poles from electric and telecommunications companies.

As is clear from this section, changes to the statutes governing Proposition 2 ½ have dramatically affected the growth of property taxes. In the following section, I will discuss in greater detail how the new growth provision makes compliance to the limitation difficult to monitor.

III. Monitoring the Limit

The dearth of media coverage of the major amendments to Proposition 2 ½ highlight how little monitoring of the limit occurs after the passage. This next section further highlights how the construction of the levy limit further makes it difficult for taxpayers to monitor compliance with the limit's constraint. In particular, I focus on how increases to the levy limit caused by new growth are passed on to pre-existing homeowners.

[32] Keenan, Kevin. "Tax bill windfall on telecom firms aids some towns - Verizon owes Westboro $910,000." Worcester Telegram & Gazette (MA)-November 10, 2003. Page A1.

A reasonable justification for the new growth provision is that new growth provides for broadening of the tax base. In theory, new property owners enter as new-taxpayers, increasing municipality coffers while not effecting the tax bills of pre-existing property owners. Analysis of single-family tax bills, however, puts this into question. As I will show, individual property tax bills have been growing at rates higher than that justified by a 2.5% increase and accounting for overrides and exclusions. The only other component that adds to the limit is new growth, suggesting that somehow new growth is passed over to pre-existing taxpayers over time. The problem with the new growth provision is that it is quite difficult to monitor who is paying for the new growth allowances. The crux of the problem, as will be discussed further in this section, is that levy limits that are constructed through aggregation are difficult to monitor.

How is it that new growth is passed onto preexisting taxpayers? One conjecture points to the fact that the levy can only be increased, not decreased over time. This singular directionality has consequences both in cases of foreclosure and when a piece of land goes from one class of property to another. For example, if a plot of land that was formally held by a non-profit organization is sold to for-profit organization, the assessed value of this land multiplied by the tax rate is added to the levy limit. In contrast, if the reverse situations were to occur and the land became tax-exempt, the limit would not decrease.

This can have profound effect on tax rates for average households. For example, in the case of Westboro, a pharmaceutical manufacturing site owned by (Astra AB of London) changed tax classes after a corporate merger with Zeneca Group PLC of Sweden. The new company, AstraZeneca, was reclassified as a limited partnership corporation instead of a manufacturing company. As a result, local property taxes that were previously exempt for manufacturing companies on machinery, equipment, and inventory, suddenly became taxable. In 1999 the site paid $2,661 in personal property taxes to the town of Wesboro, compared to $1 million in 2000, despite having roughly the same operation. [33]

A change in this direction both increases the tax limit and the number of entities paying taxes towards this limit. The effect on taxpayers would either be negligible or negative. But imagine if the situation had been reversed, and this company had moved from tax to tax-exempt. In this situation, the maximum tax levy would not decrease and it is possible that taxpayers could be stuck with the burden. This happened in the town of Warren in 2002. In that year, William E. Wright Co. changed from a limited partnership to a domestic corporation, thereby becoming exempt from the personal property tax. This left the city with a shortfall of $360,000. Since the maximum levy did not fall in stride with the change in tax-exempt status, city officials raised property taxes on their residents. This case gained media attention when a family wrote a formal complaint to their Board of Selectmen that their property taxes jumped 55% in three years. [34] A corollary to this situation is a company that develops in a municipality, but goes out of business or leaves town; The tax levy would not fall with these changing circumstances, thereby allowing to municipalities to raise property taxes without constraint.

Analysis of actual property tax bills in Massachusetts illustrates how calculating the levy limit at the level of the municipality leads to increased property taxes. For this

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[33] Keenan, Kevin. "Tax bill windfall on telecom firms aids some towns - Verizon owes Westboro $910,000." Worcester Telegram & Gazette (MA)-November 10, 2003. A1

[34] Ellery, J.P. "Couple protest tax hike - Warren pair says their bill up 55 percent." Worcester Telegram & Gazette (MA)-February 12, 2004. B1

analysis, I will rely on historical records of property tax bill of actual homeowners collected by the Center for Limited Taxation's (CLT). This is a convenience sample since each of the nine taxpayers are tax-activists associated with the CLT. If any bias exists in this sample, one would expect it would be in the favor finding Proposition 2 ½ to be effective since tax-activists might self-select to live in towns that keep taxes low. Additionally, these activists have greater incentives and capabilities to monitor their local officials than the average homeowner. The average annual rate of change for these nine tax payers between 1982 and 2005 was 4.1%. As I will discuss shortly, the discrepancy between this level of growth and the proscribed 2.5% limit is not a result of voter approved tax increases.

I argue that increased tax burden is a direct result of the monitoring difficulties that arise when a levy limit is calculated through aggregation. Using the data from nine taxpayers provided by CLT, I present a hypothetical analysis that compares actual growth in tax bills to what would have been billed had Proposition 2 ½'s limit applied to the level of the individual. In other words, this analysis calculates what would have happened if Proposition 2 ½ stipulated that an individual's tax bill could only grow at a rate of 2.5%, as opposed to municipal coffers in aggregate. In this analysis, tax bills increase by both the 2.5% annually as well as additional increases that result from overrides and exclusions passed by the voters. Again, this is a sample of convenience in towns that are closely monitored by tax activists, which should bias my results towards finding that Proposition 2 ½ is effective at constraining property taxes.

The hypothetical estimates of interest are the average annual rates of property tax growth that would have occurred under Proposition 2 ½ if the limit were tied to increases in individual property tax bills. Each year for each municipality, a percentage change in taxes is calculated using three components: 1) a 2.5% increase relative the previous year's limit minus exclusions, 2) the percent increase allowed by successful overrides, and 3) the percent increase allowed by successful exclusions. The latter two are calculated using actual data from municipalities. The assumption used in these calculations is that if a municipality passes an override that would raise municipal taxes by 5%, then we assume that this passage would likewise raise individual property tax bills by 5%. Additionally, it is also assumed that the municipality taxes to the absolute maximum level each year. This second assumption will overestimate tax bills in this hypothetical analysis.

The first component, the increase resulting from the allowable 2.5% growth, is calculated as follows:

% change due to 2.5% increase in time t = $\frac{(LL_{t-1} - E_{t-1}) * 0.025}{LL_{t-1}}$

where t is a fiscal year, LL is the Maximum Levy Limit (including exclusions) and E is the amount of exclusions approved for that year. As stipulated by Proposition 2 ½, exclusions are deducted prior to calculating allowable 2.5% growth. Mathematically, this means that if a municipality holds constant the amount of exclusions allowed over time, taxes as a whole will grow at a rate slightly lower than 2.5%

The second component, the percent increase allowed by successful overrides, is calculated as followed:

% change due to overrides in time t = $\frac{O_t}{LL_{t-1}}$

where t is a fiscal year, O is the amount of overrides approved for that year, and LL is the maximum levy limit (including exclusions). For example, in 2006 the town of Marblehead passed a \$2.7 million dollar override. The previous year's maximum levy limit was \$39.9 million, so this override could lead to no more than a 6.8% increase in property taxes from the previous year (2.7/39.9). Since overrides add permanently to the levy limit, this increase would enter into the baseline used for future calculations.

The percentage change allowed by successful exclusions is calculated slightly differently since they are not permanent additions to tax bills. For example, an exclusion that exists in T1 but expires by T2, will cause a decrease in property taxes in T2. To accurately account for this for this impermanence, I use changes in exclusions to calculate percentage changes from year to year. Specifically:

$$\text{\% change due to exclusions in time t} = \frac{E_t - E_{t-1}}{LL_{t-1}}$$

again where t is a fiscal year, LL is the Maximum Levy Limit (including exclusions) and E is the amount of exclusions approved for that year. This formula takes into account that, all else constant, exclusions that are smaller in t than in t-1 will lead to a decrease in property taxes relative to the previous year.

The sum of these three components represents the total percent change in property taxes from one year to the next. This value, for each municipality for each year, is calculated as follows:

Percent Change in property taxes time t = $\frac{(LL_{t-1} - E_{t-1}) * 0.025 + O_t + E_t - E_{t-1}}{LL_{t-1}}$

O = overrides E = exclusions LL = Maximum Levy Limit

These calculations reveal that property taxes would be substantially different in the state of Massachusetts had the levy limit been calculated at individual level. Had each of the towns in this analysis taxed to the absolute maximum each year using a limit calculated at the level of the individual, seven of our nine taxpayers would be paying substantially lower property taxes today. This is a remarkable conclusion given that municipalities rarely tax to the absolute maximum of their limit.

Table 4.1 presents actual and hypothetical property tax growth for the nine taxpayers. For example, taxpayer A's actual average property tax rate of growth was 5.6%. Had Proposition 2 ½ been calculated at the level of the individual, that taxpayer would have only have seen an average growth rate of 3.1%, a difference of 2.4%. To put this difference into context, this would have meant that taxpayer A would have paid $1,781 in taxes in 2005 instead of the $3,109 that he or she actually paid.

The two taxpayers whose actual property tax bills are lower than what would have been calculated using an individual limit lived in the towns of Malden and Saugus. This limited tax growth is not surprising since these two towns have made low taxes a priority. Neither town has ever passed a voter override and only Saugus has ever passed debt exclusions (two total). The conclusion for these two anomalies is not that they would have been taxed more with a limit calculated at the individual level, but that those towns would have chosen to keep taxes low regardless of how the limit was calculated.

Table 4.1: Analysis of Individual Property Tax Bill Growth

Taxpayer	Municipality	Actual Percent Change 1982-2005 (municipal level)	Hypothetical Percent Change since 1982 (individual level)	Difference
A	Marblehead	5.6%	3.1%	2.4%
B	Marblehead	5.9%	3.1%	2.7%
C	Marblehead	4.3%	3.1%	1.2%
D	Malden	1.8%	2.5%	-0.7%
E	Scituate	3.5%	3.0%	0.5%
F	Rockport	6.4%	3.7%	2.7%
G	Saugus	2.1%	2.8%	-0.7%
H	Billerica	3.7%	2.5%	1.1%
I	E. Bridgewater	3.3%	2.8%	0.5%

The lesson from this exercise is that calculating the levy limit in aggregate leads to higher taxes than would occur than if the limit was calculated at the level of the individual. The explanation for this difference is that the aggregated limit is harder to monitor. There is no way to ensure that new property owners are actually paying for limit increases that result from new growth. As the limit grows in aggregate over time, the tax burden rises for everyone.

This section has analyzed how failures in monitoring have lead to increased property taxes since the passage of Proposition 2 1/2. The final section at how the need for revenue substitutions led to increased reliance on state aid and charges and fees.

IV. Revenue Substitution

Revenue substitution is the consequence of failing to make Proposition 2 ½ a complete contract. In particular, the writers did not write provisions to fully anticipate increases in state aid and charges and fees. Although the proponents of Proposition 2 ½ have subsequently proposed and passed additional initiatives to correct this mistake, these additional statutes are also largely ignored by state legislators.

State Aid Under Proposition 2 ½

Proposition 2 ½ has had a pronounced affect on Massachusetts' fiscal structure. In other states, where TELs were aimed at the state budget, the effect of the TEL was to balloon local government (Gold and Ritchie 1990; Rafool 1996; New 2001). The limit in Massachusetts, aimed specifically at tightening local coffers, had the opposite effect and led to increased usage of state funds. After Proposition 2 ½, cities and towns began to rely on the state's generosity in the form of aid to localities to increasing extents. This change in fiscal structure occurred because Proposition 2 ½ was an incomplete contract. While the spirit of the law was seemingly aimed at reducing size of government, the letter of the law did not limit the extent to which state revenues could supplement fiscal losses.

In this section I discuss how state aid has compensated for diminished property tax revenues since the passage of Proposition 2 ½, particularly in the 1980s.

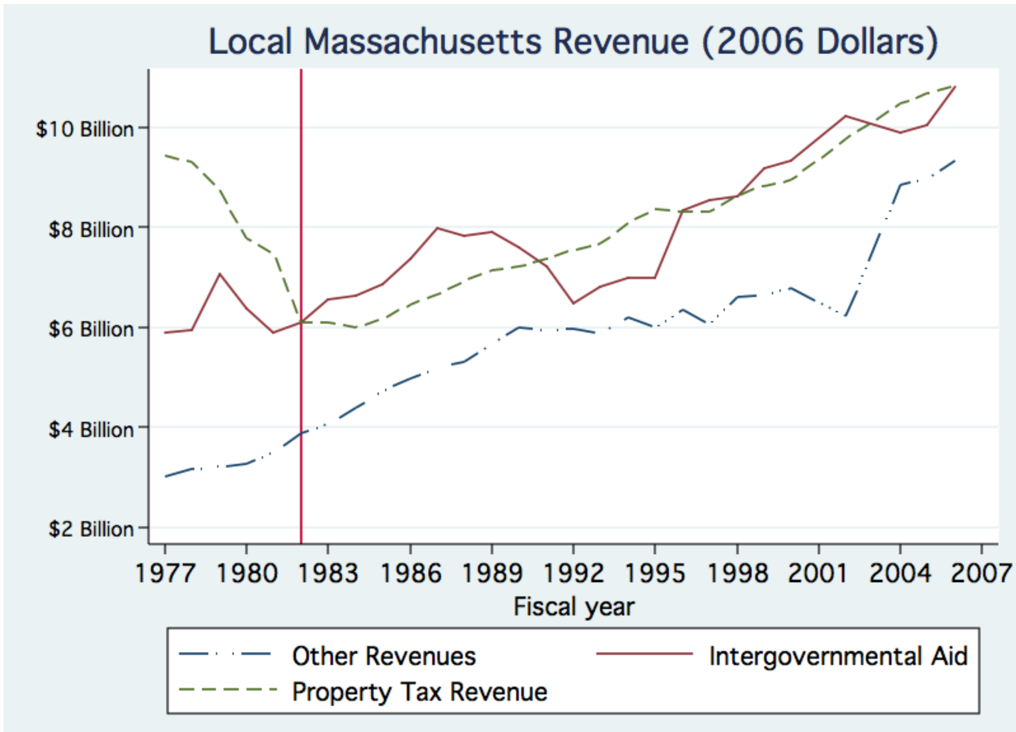

Figure 4.9: Local Massachusetts Revenue, 1977-2007

Figure 4.9 shows changes in local revenue sources before and after Proposition 2 ½. This figure, starting in the year 1977, graphs three components of local revenue, aggregated across the state[35]. This includes not only municipal revenues, but also that of counties and special districts. Not surprisingly, the most important revenue source at the start of the time series is property tax. By 1989, however, significant increases in both state aid other local government revenue sources, put each of these components on par

[35] Data from U.S. Census Bureau, Annual Survey of State and Local Government Finances, Government Finances, Volume 4, and Census of Governments (1977-2007).

with property taxes in terms of relative significance. State aid has off and on since 1982 been the single largest source of local revenues.

Aid rose most notably between the years between 1981 and 1988. These were crucial years for local government finance, since this was before many of the legal changes that made Proposition 2 ½'s levy limit more flexible. Increases in state aid during these years allowed municipalities to avoid drastic cuts associated with disappearing revenues. Additionally, this sort of substitution was not as necessary after the numerous amendments to the law loosened the limit's constraint on property taxes.

It is noteworthy that the significant increase in state aid was both expected and, perhaps, intended. In this sense, it is possible that drafters of Proposition 2 ½ intended to make the proposition an incomplete contract so that other revenue sources would supplant property taxes. Prior to the passage of Proposition 2 ½, numerous supporters of the measure, including CLT's Barbara Anderson, went on record recommending substantial increases in state aid to make up for local lost revenues.[36] Her group followed through on this recommendation, lobbying for $300 million in additional local aid for 1982. CLT's legal counsel at the time, Gregory Hyatt, even said that "if it's necessary to raise state taxes to make up that lost property tax revenue, that can be done too, although we believe that's not necessary because there's enough waste in state spending to provide the funds."[37] Such anecdotal evidence suggests that the extension of state aid was an intended consequence of the property tax limit.

Initial increases in state aid were made possible in the 1980s by a good economy.

[36] "A Vote for Proposition 2 ½" Boston Globe. August 3, 1980

[37] Robinson, Walter V. "Prop. 2 ½ and Poorer Cities." Boston Globe. October 17, 1980.

At that time Massachusetts benefited from its technology companies receiving lucrative contracts from the U.S. Department of Defense. The economy was so strong that this “Massachusetts Miracle” was the basis of Governor Dukakis’ run for the Presidency in 1988. A significant increase in Massachusetts’ gross state product made it possible for the state government to increase state aid without increasing the income tax rate, at least until 1989. In that year, the legislature approved an 18-month increase of income taxes from a flat rate of 5% to 5.75%. While this increased was supposed to be temporary, it remained law until the voters passed an initiative in 2000 that called for a rollback of income taxes to the “traditional” rate of 5%. The legislature stymied this initiative as well, freezing the tax rate at 5.3% and permitting further reductions only if certain revenue growth requirements are met.

There is significant evidence from the Massachusetts ballot box that voters have tried to close this incomplete contract by passing a TEL directed at the state level. Wallin 1999 tracks the numerous ballot measured that have, unsuccessfully, sought to either limit state revenues as a whole or limit income tax specifically. For example, voters passed a revenue limit in 1986, only to have it redacted by the state legislature years later.

The changing fiscal balance in Massachusetts has had significant consequences at the local level as well. Municipalities are dependent on the state to fairly distribute aid and to continue to deliver it year after year. This has brought heightened attention to how aid is distributed. Specifically, two issues of controversy are 1) What formula is used to distribute aid across municipalities? and 2) Is the money guaranteed? Issues of distribution and dependency underscore the difficulties that municipalities face when relying on the state for revenues.

The first controversy has implications for local control over the size of government. According to a classic public choice model, in a given local area, residents are able to "vote with their feet" and sort themselves into communities with like-minded preferences over government policy (Tiebout 1956). When revenues arise at the municipal level, citizens are able to determine their preferred level of tax contributions by either voting for elected representatives, voting for an override, or moving out of a municipality. When revenues come from the state instead of the municipality, however, this latter option is taken away. The best that the state can do is to try to distribute municipal aid in a way that is both equitable while still satisfying the diversity in preferences over the size of government. The latter is difficult, if not impossible, since the state cannot tax municipalities differentially.

The first year under Proposition 2 ½ provides a good example of the difficulties of equitable distribution of state aid. In that year, the Massachusetts Department of Revenue used a pre-existing formula for state aid, one developed to distribute state lottery windfalls. This formula took into account a locality's population size and the value of property statewide[38]. The problem was that population and property values alone did not predict which communities would be most hurt by Proposition 2 ½. Since the so-called lottery formula was blind to the specific effects of Proposition 2 ½ in communities, those communities with big losses but little aid increases quickly branded the resulting

[38] Black, Chris and Charles Kenney. "Local Aid Formula Not Linked to 2 ½." Boston Globe. July 17, 1981

distribution as inequitable. For instance, as reported by the Boston Globe in nominal dollars, the town of Amherst received 340% of what it lost under the first year of Proposition 2 ½ while Newton only received 15% of its losses. [39] Further, reports of the town of Harvard's one million dollar surplus raised numerous questions, even leading the town's selectmen to claim that they didn't want the increased aid and branding their receipt of 10 times what they lost by Proposition 2 ½ as simply unfair.[40] Not surprisingly, state legislators changed the formula one-year later to take into account how much a community is able to raise taxes given Proposition 2 ½'s constraints ("excess capacity"). [41]

The second controversy involving state aid involves local fiscal autonomy. Increases in state aid made municipalities dependent on revenues outside their control, particularly in the late eighties and early nineties. As John Bullard, mayor of New Bedford, pronounced, "We are now essentially a creature of the state."[42] Each year, towns and cities had to wait for the distribution of so-called cherry sheets, the rose-colored paperwork that outlines projected state aid, prior to drafting their annual budgets. Even today, looming threats about cuts to local aid make big headlines.

Local aid is particularly susceptible to economic swings. During the 1980s, state aid was a reliable source of funding. With sufficient funds coming in through state income taxes, Dukakis made a campaign promise that 40% of the growth revenues would be reserved for aid to cities and towns. Dukakis' campaign promise, however, became

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[39] Black, Chris and Charles Kenney. "New Local Aid Formula is Branded as Unfair." Boston Globe. July 18, 1981
[40] Sejgal, Ritu "Wanted, $1-Million Problem." Boston Globe-August 11, 1981
[41] Zitner, Aaron. "Local Aid Formula Creates Confusion." Boston Globe-July 14, 1991
[42] Biddle, Frederic M. "Filling the Gap Created by Proposition 2 ½." Boston Globe-February 24, 1989

difficult to keep as the state economy tightened. Increases in 1989 barely kept up with inflation and the first cuts to aid came in 1990. Initially, Dukakis had planned on increasing FY 1990 fiscal by $120 million but miscalculations about revenues in conjunction with high spending and no new taxes eventually led to a veto of $100 million dollars in state aid, and freezing of an addition $110 million dollars.[43] These cuts were both surprising and difficult for localities.

The change in spending priorities in the early 1990s is evident in Figure 4.11. This figure depicts the proportion of total, own-source revenues that the state appropriated for local aid. In 1989, right before the economic downturn, the state provided almost 34% of its revenues to localities. In comparison, in 1992, local aid was less than 24% percent of total revenues. This drop is evidence that during tough times, retaining local aid was not a top priority for the state.

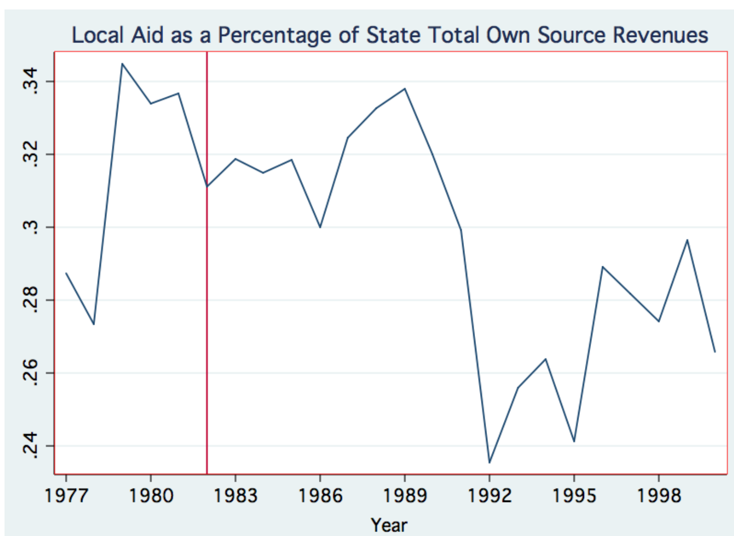

Figure 4.10 Local Aid as a Percentage of State Total Own Source Revenues

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[43] Mohl, Bruce. "Localities Receive Cuts in State Aid of 2% to 77%." Boston Globe-August 4, 1989.

In sum, Massachusetts localities saw a 5% decrease in the amount of local aid in FY 1990, followed by 6% and 12% cuts in 1991 and 1992 respectively. The declines led to increasing distrust that the state would deliver local government spending a priority. The sentiment is best described by a statement made by Representative John H. Flood of Canton, chairman of the House Taxation Committee and then Democratic candidate for governor. He said, "You cannot rely upon the political good will of any governor. Our moods change as do our ambitions.. . . There are lots of times when state government wants to keep its own candy to itself."[44]

The controversy of guaranteed state aid founds its way to the ballot box in November of 1990. On the ballot that year was Question 5, an initiative requiring the state to give 40% of its revenue from income, sales and corporate taxes as direct aid to cities and towns. This initiative, much like Dukakis' 1982 campaign promise was meant to guarantee that localities would not see the first cuts as the economy continued to slide downward.

The story of Question 5, however, reveals some of the difficulties of legislating through the ballot box. Unbeknownst to probably most of the 1.2 million voters who cast a ballot for Question 5, this initiative would never be binding. The text of the initiative read,

> "This proposed law would regulate the distribution to cities and towns of the local aid fund, which consists of at least 40 percent of the revenue generated by the state income, sales and corporate taxes as well as the balance of the state lottery fund. Subject to appropriation by the legislature, the state treasurer will distribute the local aid fund to cities and towns on a quarterly basis, and each town and city would receive at

[44] Black, Chris. "If you live by Local Aid, you Die by Local Aid" Boston Globe-June 3, 1990.

> least the same amount of local aid it received in the previous fiscal year unless the total local aid fund decreases."

It is the words "subject to appropriation" that caused the bill to be nonbinding. This phrase was included because Article 48 of the Massachusetts constitution requires that "No measure that makes a specific appropriation of money from the treasury of the commonwealth shall be proposed by an initiative petition." However, the ultimate effect is that levels of local aid were the sole discretion of the state legislature and governor, just as they were prior to the passage of the initiative. This led to significant public outrage in FY 1992 and the 40% requirement was not met and local aid was once again slashed. This ignored initiative continues to be a political albatross today every time the Massachusetts economy dips.

Charges and Fees Under Proposition 2 ½

The first year of Proposition 2 ½ saw new assessments of charges and fees to supplant lost property taxes (Davies 1985, Susskind 1983). This compensation provides further evidence that Proposition 2 ½ was subject to significant revenue substitution. Services that were once paid for with property tax dollars were restructured to introduce service fees. Prior to Proposition 2 ½, like many other Northeastern states, Massachusetts had relatively few fees for government services (Susskind 1983). Much like the increase in state aid, it is also evident that an increase in fees was expected. In an interview with CLT's Barbara Anderson, Anderson highlighted that fees were "underutilized" in Massachusetts and would be implemented for a variety of nonessential services.[45]

Proposition 2 ½, foreseeing the shift towards this form of government revenue collection, did include a clause that fee collection could not outstrip cost for services. This limit, however, proved insignificant since costs for government services could almost always be shown to be much higher than fee collection.

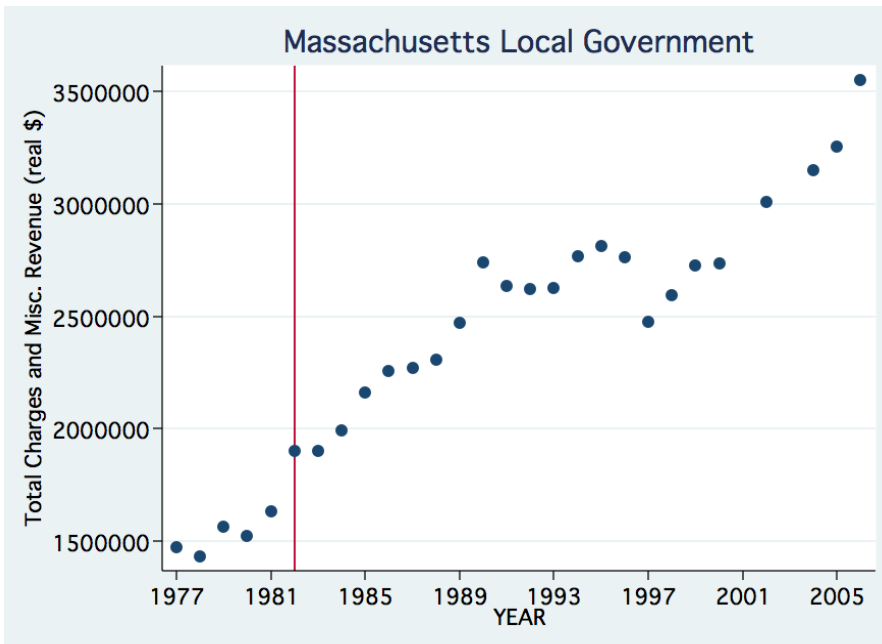

Figure 4.11: Growth of Charges and Fees in Massachusetts

Indeed, the charges and fees assessed in the state of Massachusetts have skyrocketed. In nominal dollars, local government fees have increased from $77 per capita statewide in 1977, to $158 per capita in 1982, and are currently at a whopping $551 in 2006, excluding fees for public utilities. Figure 4.11 charts this rise in charges and miscellaneous revenues in real dollars.

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[45] "A Vote for Proposition 2 ½" Boston Globe - August 3, 1980

Fees were implemented for a variety of government services, school athletics being among the most notable initially. Joining the movement of what was called, tongue-in-cheek, “pay-for-play”, many Massachusetts communities began charging students for participation in sports. Fees were also introduced for adult education, summer school, school lunches, and driver’s education. Outside of the realm of education, Massachusetts’s residents began being charged for trash collection, library cards, recreational facilities, and other services previously funded with property tax dollars.[46]

Among the most significant fee introductions were those associated with water and sewage provision. During the 1980s, Massachusetts went through a significant organizational restructuring of how water and sewage services were provided to localities. Prior to 1984, a large number of localities received water from the Metropolitan District Commission. However, this commission, which fell under the legislature’s domain, allowed the water and sewage systems to deteriorate significantly and polluted Boston harbor. After a threatened takeover by a federal court, the Massachusetts legislature transferred authority of water and sewage provision to a new public authority, the Massachusetts Water Resources Authority (Dolin 1992). This agency fell outside the annual state budget process and was immediately given the authority to sell $600 million in bonds. [47]

Within the first year of operation, MWRA implemented a 28% retroactive increase and water and sewage rates, further estimating that rates would increase between

[46] Collins, Laurence. “Assessing Year’s Worth of Prop. 2 ½.” Boston Globe-December 27, 1981
[47] Blake, Andrew. “Panel OK’s Harbor Bill” Boston Globe-December 18, 1984.

15-20% for the following six years. These estimates were not far off. Between 1986 and 1991, the annual rate of change for MWRA water bills was 20.5%. Through 1994, the average annual rate of change decreased slightly to 17.5%. These changes, based on increases of a typical household using 90,000 gallons of water annually meant that a family paying $161 in 1986 was paying $590 annually in 1994.[48]

Following the creation of MWRA, many towns began changing the way residents paid for water and sewerage. MWRA is only a wholesaler for public works, billing municipalities for the sum of water and sewerage usage and allowing the municipality full discretion of how it passes along the costs to users. Previously, many towns used property taxes to subsidize water and sewerage costs instead of passing the full cost of the service to the user. When local aid started restricting in the late eighties and early nineties, it sparked a movement for localities to stop the subsidies and tap further into direct fees for usage.

This trend was facilitated with the passage of statute G.L. c.44, § 53F½, in 1986. This legislation set up special accounts - specific to the provision of a function such as electricity, water, or sewerage – called enterprise funds. Enterprise funds separately accounted for indirect costs such as capital improvements for fixed assets, not just operating costs. This allowed municipalities to pass on the full cost of service provision directly to the user. Identifying the total cost of service was important in part because Proposition 2 ½ limited fees to this maximum.

By 2006, 20 years after the enabling legislation passed, Massachusetts' 351 municipalities had a total of 486 enterprise funds, including 150 enterprise funds for the

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[48] Allen, Scott. "MWRA predicts 5.4% rate hike; foes unimpressed" Boston Globe February 12 1994 A1.

provision of water. Of the $833 million dollars of revenue raised for water services by Massachusetts localities in 2006, more than half ($499 million) was generated through the use of enterprise funds. The widespread usage of enterprise funds to collect sewage fees is very similar, with 145 enterprise funds in existence and totally $425 million in collected revenues.

In addition to enterprise funds, many cities and towns set up special districts to manage their water. Special districts fall outside Proposition 2 ½ limits and are able to levy property taxes directly to residents that live within their boundaries. As a result, many residents of Massachusetts see a "water tax" on the very bill provided by their municipality to assess property tax.

This explosion of fees over time in and of itself cannot be attributed to Proposition 2 ½. Indeed, most states, Northeastern states included, have seen similar increases. Instead, to show causality, I look to the variation in the level of property tax constraint caused by Proposition 2 ½ that exists between and within Massachusetts' municipalities over time.

Not all municipalities are constrained by Proposition 2 ½. Many have passed overrides and exclusions that have raised the levy limit high enough so that it not longer constrains taxing decisions. Other municipalities may have created enterprise funds or special districts to raise revenues outside municipal budgets. Still others may simply have low-tax preferences and fall below the limit without need for evasion tactics. This sort of variation exists not only between municipalities, but also within municipalities over time. I will leverage this variation to show a statistically significant relationship between constraint and the assessment of charges and fees.

I employ a difference-in-differences model to test the affect of Proposition 2 ½ on municipal assessments of charges and fees. The unit of analysis is a municipal-year. A municipality fixed effect is used to capture heterogeneity that occurs across municipalities. This modeling specification controls for all time-invariant unobservable characteristics of a municipality. For example, a fixed effect would control for an unobservable demand for services. Since the time series used in this analysis is relatively short, the assumption that assumption that unobservables are time-invariant is reasonable.

Year fixed effects are included to capture unobservable time-variant factors that affect all Massachusetts simultaneously. For example, year effects should capture change in the dependent variable related to business cycle fluctuations or variations in state aid.

The dependent variable used in this analysis is the first-difference of logged municipal charges and fees from 2001-2007. This limited duration is direct function of data availability. Although the Massachusetts Department of Revenue collects the amount of general fund municipal charges and fees from 1985 to the present, general fund data by itself is subject to a substantial amount of measurement error. Plots of this time series showed dramatic swings that, when cross-checked with data from individual municipality annual reports, were evidently the result of accounting changes, not substantive variation over time. This measurement error is significantly reduced when general fund charges and fees are aggregated with special fund charges and fees (moneys reserved from enterprise fund activities, reserved funds, and other miscellaneous accounts reserved for specific purposes). This data is available from 2000-2007 from the

Department of Revenue. The 2000 data, however, is excluded because of an accounting consolidation that occurred in that year (Department of Revenue)[49]

This analysis looks at how property tax constraint and changes in excess capacity cause municipalities to collect more charges and fees. I use a measure of constraint previously employed by Bradbury et al 1997. Constraint is defined as being within 0.1% of the maximum levy limit of Proposition 2 ½. This variable is coded as one is the municipality is constrained, zero otherwise. I predict a positive and significant coefficient for this variable, as constraint should increase reliance on charges and fees.

To control for other factors affecting the usage of charges and fees, I include a set of pertinent covariates. I control for population, total employment, and the partisan composition of registered voters. With the exception of the partisan composition of voters, each of these variables is available annually at the municipal level from the Department of Revenue. The partisan composition of voters is available only in even number years. I impute the odd-numbered years using a simple prediction based on the variables lead and lag as well as changes in personal income.

[49] I do not include all special fund revenues in my dependent variable. Categories of special funds were chosen to most accurately represent those funds that come from charges and fees, not grants, gifts, or other sources of revenues not directly charged to residents of Massachusetts. The choice of which special fund revenues to include was conducted using consolation with the Department of Revenue. Included variables are revenues from: general fund charges and fees, general fund licensing fees, enterprise funds, ambulances, parking meters, and athletics

Table 4.2: Analysis of Proposition 2 ½ Constraint of Charges and Fees

Dependent Variable: Δ LN Municipal Charges and Fees	
Δ LN Total Population	-0.145
	(0.699)
Δ CPI	-1.92E-05
	(0.005)
Δ Percent Democrat	-0.179
	(1.238)
Δ LN Employment	-0.195
	(0.137)
Constrained by 2 ½	0.063**
	(0.025)
Constant	0.072**
	(0.032)
Municipality Effects	Included
Year Effects	Included
Observations	1755
R-squared	0.009
Number of Municipalities	351

The results reported in Table 4.2 suggest that constraint has a positive and significant effect on growth of charges and fees. Specifically, constraint by Proposition 2 ½ affects the growth rate of charges and fees by 6.3 percent. This result is statistically significant at the five percent level. This finding is notable given that little else predicts changes in the growth of municipal charges and fees.

The conclusion to be drawn from this exercise is that municipalities use charges and fees as a substitute revenue source when they are constrained by Proposition 2 ½. This systematic relationship suggests that the overall growth in charges and fees in

Massachusetts can be specifically attributed to Proposition 2 ½, not simply a trend shared by other states across the county.

V: Conclusion

This chapter has analyzed the effects of Proposition 2 ½ in the state of Massachusetts. The conclusion of this chapter is that the effectiveness of Proposition 2 ½ has decreased over time as a result of agency loss. Proposition 2 ½ was not faithfully executed because the initiative was not a complete contract and did not sufficiently provide for easy citizen monitoring. As an incomplete contract, Proposition 2 ½ allowed municipal property tax growth to be supplanted by special district property taxes, state aid, and charges and fees. Evidence for this conclusion includes the creation of special districts to levy their own taxes and the dramatic increase of state aid in the 1980s. Additionally, I have shown that municipalities systematically increase charges in times when they are constrained by Proposition 2 ½'s maximum levy limit.

Proposition 2 ½ was poorly monitored in two ways. First, over time legislators whittled away at constraining requirements of the proposition by amending the laws relating to overrides, exclusions, and new growth. Second, by using an aggregated limit instead of one calculated at the level of the individual, taxpayers could not adequately monitor how new growth allowances were increasing their own personal tax burdens. Evidence for this latter failure was shown with an exercise that compared actual property tax growth to growth that would have occurred given an limit based on individual property tax growth for nine actual tax payers.

The policy lesson of this case study of Massachusetts is that careful thought must be paid to how initiatives will be implemented upon drafting an initiative. In order to ensure faithful implementation, principals (voters) delegating implementation of an initiative to agents (municipal officials) must take into account that they most likely do not have aligned preferences. As such, they must take into account the methods of minimizing agency loss, such as monitoring, contract design, screening and selection of agents, or institutional checks (Kiewiet and McCubbins. 1991).

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