

**Do Tax and Expenditure Limitations
Disproportionately Reduce Municipal
Service Expenditures?
It Depends on Your Perspective.**

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Abstract

Two goals of tax and expenditure limitations are reducing property tax burden and limiting local government growth. A consequence of these limits is disproportionate reductions to service spending as compared to administrative spending. Another consequence of tax and expenditure limitations is abnormal spending just prior to enactment of the spending limits. Empirical tests of local government revenues and expenditures reveal these effects. However, a deeper examination of service expenditures reveals disparities in results when service expenditures are split and separately analyzed.

Keywords: municipal tax, property tax, tax and expenditure limit, government accounting, municipal spending, government spending

Introduction

When laws are enacted that limit a municipality's ability to tax and spend, it is reasonable to expect reduced municipal expenditures. However, municipalities are not required to reduce service expenditures and administrative expenditures in equal proportions. This uncertainty makes it worthwhile to review actual outcomes of tax and expenditure limits (TEs). This paper examines the relative changes in service and administrative expenditures surrounding implementations of new TEs. However, an important question remains: What makes a municipal expenditure a "service" or "productive" expenditure as opposed to an "administrative" or other type of expenditure? Is it the perception of the constituents? The perception of the municipal management or mayor? Or is there a more precise definition? That answer likely depends on the context being examined and the perspective of those who are affected. In this paper I examine the effects of tax and expenditure limitations on service expenditures using multiple classifications of service expenditures, and reveal how expenditure classifications can alter the interpretation of the results.

This paper provides insight into tax and expenditure limitations' general effects on municipal expenditures while simultaneously revealing potential issues in tax and expenditure limitation research that have made distinctions between service expenditures and other types of expenditures. Expenditure classification is subjective, and service expenditure is often operationalized using a very narrow heuristic of police and / or fire protection costs. Additional tests should be performed to account for more than one interpretation of service expenditures. Otherwise, results will lack generalizability.

Using data from the United States Census Bureau's survey of governments, I examine whether TEs affect service expenditures differently than administrative expenditures. By including multiple classifications of service expenditures, I find inconsistent results. Therefore, unless research is explicitly examining a specific expenditure or group of expenditures, the results should be interpreted conservatively.

In the remainder of this paper, I will II) describe the goals of tax and expenditure limitations, III) discuss prior related research, IV) provide initial hypotheses, V) detail the data and methodology, VI) summarize preliminary results, VII) provide results using modified service expenditure definitions, and VIII) give closing remarks.

Tax and Expenditure Limitations

In the United States, there has been a movement toward limiting local governments' ability to levy taxes and make expenditures. In fact, most local governments operate under state mandated tax and expenditure limitations. The usual goals of these laws are to reduce the tax burden of the residents and to limit the growth of local governments (Waisanen 2010). The laws do not specify the types of expenditures the municipalities must reduce, so an empirical examination of outcomes is useful.

The primary purpose of this paper is to examine whether tax and expenditure limitations affect service and administrative expenditures differently, and to utilize a broader definition of service expenditures. While a reduction in expenditure growth can be expected when a new tax and expenditure limitation is imposed, a resident could expect that a municipality would maintain its proportions of service and administrative expenditures. Research about these tax limits and service expenditures usually use narrowly specified data with significant restrictions such as examining only a specific year or only high minimum population municipalities. This paper uses panel data from a broad sample of municipalities in the United States.

The data for this paper comes from the United States Census Bureau's survey of local governments which includes revenue sources and expenditure types. I also use tax and expenditure limit data from Mullins and Wallin (2004) and Mullins and Cox (1995). The U.S. Census Bureau data, the tax and expenditure limit data, and other control data, are used in difference in differences analyses.

Tax and Expenditure Limitation Research

Tax and expenditure limitations have become widely used throughout the United States. As of 2012, the final year of this paper's dataset, there were 45 states with some form of limit in place. These state-implemented rules protect taxpayers from rising tax burdens by limiting local governments' ability to raise property taxes and by reducing the growth of local governments' expenditures (Mullins and Cox 1995). Sears and Citrin (1982) discuss the "tax revolt" that was occurring when California enacted Proposition 13 in 1978. In the few years before and after this time, there was an acceleration of states passing tax and expenditure limitations with varying levels of restrictiveness. The laws vary in the calculation of the limits and include various exceptions to those imposed limits. These exceptions allow municipal management to circumvent or reduce the impact of the tax and expenditure limits. This has motivated others to research the effects of these laws, and whether they are working as intended.

Much research regarding the effects of tax and expenditure limitations is performed at the state government level. Although my research is an examination of local governments, the results and theories of state level research can provide useful insights. New (2010) finds that the origin of the tax and expenditure limitation is key in predicting whether the rules will be successfully implemented. In his research, citizen-enacted policy is more successful than legislature-enacted policy. Kousser, McCubbins, and Moule (2008) and Seljan (2014) put forth principal-agent theory as the model for predicting the effects of tax and expenditure limit rules. Kousser et al. say "...policies passed through direct democracy can often be thwarted by the politicians charged with implementing them." Seljan says that principal-agent theory is a better predictor of tax and expenditure limit effectiveness than policy origin, as evidenced through scenarios involving varied monitoring difficulty. Another aspect of predicting tax and expenditure limit effects is provided by Kioko (2011). Kioko finds that technical elements of the limits can lead to disparity between the legislated limit and the actual revenues or expenditures.

Researchers have illuminated how tax and expenditure limits affect revenues, spending levels, and debt. Deller, Maher, Amiel, and Stallmann (2013) find that the effects on debt level depends on the type and restrictiveness of the enacted limits. States with tax and expenditure limits that restrict both revenues and expenditures have lower debt. However, having strict limits

for only revenues or only expenditures is associated with increased debt. McCubbins and Moule (2010) show that, especially in times of recession, tax and expenditure limits reduce state and local revenues. McCubbins and Moule say this effect occurs because of a reliance on “income-elastic revenue sources, such as the income tax or charges and fees.” Chapman and Gorina (2012) use simultaneous equations to show that tax and expenditure limitations restrict local revenues.

In addition to research on the outcomes for revenues, expenditures, and debt levels, researchers have also examined the effects of tax and expenditure limits on borrowing costs. Results from Poterba and Reuben (2001) and Johnson and Kriz (2005) show that state tax and expenditure limitations do influence borrowing costs. The specific direction of the influence depends on the nature of the limit. Limits on revenues generally increase borrowing costs, while expenditure limits are associated with reduced borrowing costs. A local level examination of borrowing costs is performed by Maher, Deller, Stallmann, and Park (2016) who find that municipalities with restrictive limits have weaker credit ratings, likely leading to higher borrowing costs.

Most local government focused tax and expenditure limitation research examines the change in property taxes (McCabe 2000; McCabe and Feiock 2005), and whether the size of municipalities has been reduced (Lowery 1983; Joyce and Mullins 1991; Shadbegian 1998). Comparatively, there have been few nationwide analyses of the limits’ effects on the proportion of services provided. Prior service expenditure research focuses on an individual state or a specific service metric, such as teacher to student ratio (Downes and Figlio 1999). Conversely, I utilize a more comprehensive definition of service expenditures for municipalities throughout the United States. A paper that addresses a similar topic is Figlio and O’Sullivan (2001).

Figlio and O’Sullivan (2001) investigate how service and administrative expenditures are affected under tax and expenditure limitations. Their findings show a reduction in the proportion of service expenditures for municipalities that possess legal provisions which allow residents to override the normal tax and spending limits. They theorize that municipal managers use threats of reductions in services as a method of spurring residents to override the limits. This is derived from the Leviathan theory by Brennan and Buchanan (1977) which posits that bureaucracies want to grow their influence through ever increasing expenditures.

Hypothesis Development

Tax and expenditure limits are designed to reduce the growth of property taxes and overall expenditures. Without the directive to maintain the same service to total expenditures ratio it is unclear how the service ratio would change following the enactment of these rules, or whether they would change at all. Figlio and O’Sullivan (2001) find that the proportion of service expenditures to administrative expenditures is reduced in their context. Their results may not directly apply in this paper because of my expanded classification of service expenditures and broader dataset. Therefore, I state the following hypothesis in the null form:

H1) Tax and expenditure limitations cause no change in the municipalities’ service expenditure ratio.

While it is difficult to predict the direction of change in service ratio after tax and expenditure limits are enacted, a logical prediction can be made for the effect on service ratio just before the limit is implemented. Using leviathan theory and agency theory (Jensen and Meckling 1976), it is natural to expect an increase in the service expenditure ratio just before the limits go into effect. Brennan and Buchanan theorize that governments are “Leviathan” and want to maximize budgets. Maximizing budgets through service expenditures builds a good reputation for elected officials, likely aiding their reelection efforts and thereby allowing them to continue reaping the benefits of controlling the government budget.

Agency issues allow local governments to increase their service expenditures just before the limit’s implementation, without fear of state government backlash (Seljan 2014). The state, as the principal, has created a law that which relies on the municipal government to implement. Various exceptions in most tax and expenditure laws allow for some flexibility to tax or spend over the general limit. Taxation over the legislated limit is usually allowed to pay off debt. This ability to raise money and spend to pay debt could embolden municipalities to borrow and spend money on service projects just before laws are implemented. The increased debt payments could be offset with additional property taxes or could be refinanced later. Although not directly related to service expenditures, McCubbins and Moule (2010) find that states show some evidence of fiscal changes just before tax and expenditure limits are enacted. Because of this, I hypothesize the following:

H2) Service Ratio will be higher just before tax and expenditure limitations are enacted in anticipation of the reduced service spending capacity after enactment.

Data and Methodology

The dataset consists of government financial data from the United States Census Bureau’s survey and census of governments. The years of data in this study are 1970 through 2012. Municipalities respond to the Census Bureau’s survey with revenue sources and expenditure details. Responses to this survey are voluntary and the sample size fluctuates from year to year. During years of the census, rather than the survey, sample sizes are larger. The U.S. Census Bureau performs the census of governments every five years for years ending in seven or two (e.g. 1977, 1982, etc.). Table 1 shows descriptive statistics for the full sample. The tax and expenditure limitation data were gathered from the Mullins and Wallin (2004) and Mullins and Cox (1995) publications, while updates through 2012 were hand collected. Table A1, in the appendix, shows tax and expenditure limit strengths for each state and year. The levels of strength are valued as zero, one, or two. A value of zero indicates no state-imposed limitation is in place. A value of one means a weaker limitation is in place. A value of two means a strong limitation is in place. Weaker limitations contain language which makes it easy to circumvent revenue or expenditure reductions. The specifics of the legislated limit differentiate a weak limitation from a strong limitation. Mullins and Cox identify the areas that tax and expenditure limitations usually target. These areas include property tax rates, property value assessments, general revenue limits, and general expenditure limits. A tax and expenditure limitation is considered weak if it only limits increases in either the property tax rate or the property value assessment. This is weak because a municipality could increase the tax burden on residents by increasing whichever value is not subject to the state-imposed limitation. A limitation is strong if it imposes a general revenue or expenditure limit, or if the law possesses both a tax rate limit and

property value assessment limit. Sample frequencies for state and region can be found in the appendix tables A2 and A3, with the tax and expenditure limit strengths in table A1.

I use difference in differences regressions to examine the effect of tax and expenditure limitations on the expenditures of municipalities. The dependent variable in these regressions is service ratio. I calculate Service Ratio as service expenditures divided by total expenditures less pension benefits paid (Service Ratio = Service Expenditures / (Total Expenditures – Pension Benefits Paid)). I identify which expenditures are categorized as services or administrative in table A4 of the appendix.

Different states have enacted tax and expenditure limitations in different years. This scattering of enactment years does not naturally lend itself to a difference in differences methodology. To perform the difference in differences tests, I create a control and a test group based on a few criteria. My dataset aligns test observations on the event year, as opposed to the actual year. Each year a strong tax and expenditure limit is enacted by a state, I create a test and control group based around that year. The included observations for this subset of data have eight consecutive years of municipal data: four years before the limit and four years subsequent to, and inclusive of, the limit enactment year. All states enacting a strong limitation that year are the test group. Municipalities with eight years of data that did not change limitations during that same eight-year period are the control group. This process is repeated for all event years and merged into the final dataset on which the difference in differences regressions are run. Errors are clustered by municipality to account for repeated observations. Year, region, county, and size fixed effects are included in the regressions. The variable of interest is the interaction between the “Test” group variable and the “Post” tax and expenditure limitation variable.

The same dataset is utilized for difference in differences testing of the expenditure limit anticipation hypothesis. To examine individual temporal effects, the generic “Post” variable is not utilized in the regression. It is replaced by seven relative time variables (relative to the year of the limit implementation). The event year is excluded so results can be interpreted in reference to the event year. The variables of interest are the interactions between the time variables and the “Test” variable. The interaction between the year just prior to the event year and the test group indicates whether service expenditures change in anticipation of a tax and expenditure limit. This regression also uses the control variables of year, region, county, and size.

Results and Analysis

The primary question examined is whether tax and expenditure limitations lead to municipalities reducing the proportion of service expenditures. The initial set of tests described here use the broad definition of service expenditures, rather than the traditional usage of only police and fire expenditures. Figure 1 shows the service ratio separately for the test and control groups. This high-level view shows a decline in the service ratio following strong limits being placed into effect. Since both the control and test groups show a similar trend, it is possible that time is the major factor causing the decline. I use the difference in differences approach to address whether implementation of the law is a significant factor in the decline of service ratio, while controlling for the effect of time. The descriptive statistics of the two separate groups in the difference in differences dataset are found in tables 2a and 2b. Table 2b, the Test Group data, is pre-event data. This is done to provide a fair identification of the structural differences

between the groups before the tax and expenditure limits confound the data. The differences between the groups in service ratio and other variables are accounted for in the difference in differences methodology. Results of the regression are shown in table 3. Results show that the test group has a systematically higher service ratio as shown by the positive and significant coefficient on the “Test Group” variable. This is consistent with the figures shown in the descriptive statistics. The first hypothesis is answered by the “Test Group x Post Event” interaction. It shows a negative coefficient with a two-tailed p-value of .085. This may indicate a reduction in the proportion of service expenditures when tax and expenditure limitation laws are implemented. However, given the .085 p-value, it is possible to view these results as less than definitive proof of tax and expenditure limitations’ effects on expenditure types.

A separate difference in differences regression examines whether municipalities anticipate the enactment of a tax and expenditure limitation (hypothesis 2). Examining figure 1 once again, there is a spike in the service ratio the year just before a strong limit is implemented for the test group. This contrasts with the figure 1 line of the control group which stays relatively consistent even just before the limit is in place. If municipalities increase their proportion of service expenditures just before the limit goes into effect, the interaction between the “Test Group” variable and the “1 Year Prior” variable should be positive and significant. Viewing table 4, we see that is indeed the case. In fact, the coefficient for the “Test Group x 1 Year Prior” interaction is more positive and significant than any other interaction variable in the timeline except the year after. The spike in the year after could be explained in a “channel stuffing” analogy. If the municipality accelerated projects from the year that the limit was enacted into the year just before the limit was enacted, then service expenditures in the enactment year will be reduced. Once the flow of service expenditures normalizes in the following year, there is an increase in relative service ratio despite the limit generally causing a service ratio decrease post tax and expenditure limit implementation. These results suggest that there is a tax and expenditure limit anticipation effect leading to inflated service ratios in the year before a strong limit goes into effect.

Disaggregation of Service Ratio

To analyze the causes of service ratio changes detailed in the results, I split service costs into two categories. In one category are the costs related to fire and police expenditures, which prior research such as Figlio and O’Sullivan (2001) have often used as the only service costs. In the second category are all other service expenditures which I have defined through an examination of the U.S. Census Bureau’s survey of local governments. Each category is valued as its own ratio:

$$(Fire + Police Expenditures) / (Total Expenditures - Pension payments) = Fire and Police Ratio$$

$$All Other Service Expenditures / (Total Expenditures - Pension Payments) = Other Service Ratio$$

Separately analyzing these ratios helps reveal the levers municipal managers used to affect change under tax and expenditure restrictions. Identifying the causes also highlights differing characteristics of certain types of expenditures. For example, some administrative costs may be “sticky” in comparison to service costs. This could be caused by union contracts and a

significant fixed amount of necessary administration. These characteristics also seem applicable for the fire and police service costs. There is a need for these services, and a significant portion of government employees (40 percent of local government employees) are unionized per the Bureau of Labor Statistics' 2016 data news release. Additionally, police and fire expenditures are often highly visible to residents and may therefore lead to greater consequences for municipal leadership if those expenditures are reduced.

With results for hypothesis 1 showing a reduction in service ratio after tax and expenditure limits are in place, and since reducing fire and police expenditures are costly to municipal leadership due to their visibility, I hypothesize that the major cause of service ratio reduction will be "other" services. I also hypothesize there will be little to no reduction in the fire and police expenditure ratio. Thus, I define the following hypotheses:

H3) Tax and Expenditure limitations cause no reduction in fire and police service ratio.

H4) Tax and Expenditure limitations cause a reduction in the ratio for other services (Services exclusive of fire and police expenditures).

Disaggregation of Service Ratio Results

Tables 5 and 6 show the results of the difference in differences regression tests of H3 and H4. Table 5 supports hypothesis three by showing that the fire and police expenditure ratio is not reduced by the implementation of tax and expenditure limitations. In fact, contrary to results shown by Figlio and O'Sullivan (2001), an increase is shown in the proportion of fire and police protection expenditures. The coefficient of interest, "Test Group x Post Event," is positive with a p-value of .001.

Table 6 shows results that support hypothesis four. It shows that tax and expenditure limits cause a reduction in the proportion of expenditures on other services. The variable of interest, the interaction between Test Group and Post Event, has a coefficient of -.0062 with a p-value of .001.

The results of this disaggregation of service expenditures demonstrates how different expenditures are affected by tax and expenditure limitation laws. The results contrast with the previous "fire and police" service definition research, although I acknowledge the differences this paper possesses in sample and parameters.

Conclusion

This paper shows that although the intent of tax and expenditure limitations is to reduce tax burden and limit the growth of local governments, there is a potential asymmetry that arises from these laws. Municipalities generally spend a smaller proportion of their expenditures on services after tax and expenditure limitations are implemented. However, these results may be driven by the functional definition of service expenditures. What qualifies as a service expenditure should be scrutinized, and this paper attempts to expand upon what has been accepted as the norm.

In addition, this paper gives evidence that suggests municipalities are anticipating the implementation of expenditure limits and responding by increasing their service expenditures

just before the law goes into effect. This acceleration of spending makes it worthwhile to consider whether these tax and expenditure limits are inadvertently causing long-term negative financial consequences for municipalities.

Finally, this paper shows that reductions in the service ratio are driven by reductions to the non-protection service costs (service costs excluding fire and police spending). This stands in contrast to prior research showing reduced service ratio, where service expenditures were defined as spending for fire and police services.

Future research can determine whether tax and expenditure limits increase the “price” of services. Even if property tax growth is reduced, if services are disproportionately reduced then residents could pay more per service expenditure dollar than before the limits went into place. Also worth investigating is whether accelerated spending (just before limits are implemented) on service projects is funded by extra debt or property tax revenues. This brings into question whether tax and expenditure limits spur municipalities to increase their spending and consequently weaken their capital structure.

Another path of research involves investigating the tax and expenditure laws which are imposed by the state, and their implementation which is performed by the municipality, as a principal-agent problem with potentially limited monitoring. If monitoring does influence the municipalities’ decision-making (Seljan 2014), can high quality audits serve as the monitor? Do state audits provide better monitoring? The results described in this paper warrant further investigation into these and other issues.

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Tables

Table 1 – Descriptive Statistics for Full Sample

Variable	Full Sample							
	n	Mean	S.D.	Min	0.25	Median	0.75	Max
Total Expenditures	665,185	19,133.11	4.90E+05	-663.9	84.00	433.78	3,565.39	8.70E+07
Service Expenditures / Total Expenditures	655,031	0.80	0.16	-0.50	0.74	0.83	0.91	1.19
Service Expenditure Growth	342,196	0.20	6.40	-28.20	-0.11	0.01	0.17	2,576.81
Expenditures Per Capita	664,889	0.91	28.03	-0.41	0.15	0.39	0.90	19,272.46
Expenditure Growth	344,043	0.40	20.18	-1.87	-0.10	0.01	0.16	9,184.79
Property Tax Per Capita	664,889	0.19	2.49	-0.54	0.03	0.08	0.18	1,150.00
Property Tax / Total Revenues	659,571	0.31	0.26	-0.79	0.10	0.24	0.50	4.45
Property Tax	665,185	3,619.62	7.58E+04	-	18.75	90.83	625.84	1.40E+07

Descriptive statistics for full sample data. Data obtained from US Census Bureau Survey of Governments and Census

Table 2a – Control Group Descriptive Statistics

Variable	Control Group							
	n	Mean	S.D.	Min	0.25	Median	0.75	Max
Total Expenditures	184,680	49,711.52	7.00E+05	-663.9	1,463.17	6,943.78	24,013.04	7.80E+07
Service Expenditures / Total Expenditures	184,426	0.81	0.14	0	0.76	0.84	0.9	1.19
Service Expenditure Growth	177,086	0.18	9.48	-2.34	-0.09	0.01	0.15	2,576.81
Expenditures Per Capita	184,618	1.48	18.48	-0.41	0.44	0.83	1.49	3,112.39
Expenditure Growth	177,363	0.22	10.22	-1.87	-0.08	0.01	0.15	2,577.75
Property Tax Per Capita	184,618	0.27	1.32	0	0.06	0.13	0.26	185.09
Property Tax / Total Revenues	184,433	0.25	0.21	0	0.08	0.19	0.37	1.00
Property Tax	184,680	9,056.95	1.20E+05	0	207.43	1,030.64	4,354.93	1.10E+07

Descriptive statistics for control group data used in difference in differences regression.

Table 2b – Test Group (Pre-Event)

Variable	Test Group							
	n	Mean	S.D.	Min	0.25	Median	0.75	Max
Total Expenditures	7,540	64,695.25	9.20E+05	0	2,741.75	11,184.41	31,588.25	4.30E+07
Service Expenditures / Total Expenditures	7,535	0.82	0.11	0	0.78	0.85	0.89	1
Service Expenditure Growth	7,378	0.13	1.97	-1	-0.07	0.02	0.15	134.41
Expenditures Per Capita	7,540	1.15	2.88	0	0.49	0.81	1.45	136.35
Expenditure Growth	7,380	0.11	1.73	-0.99	-0.08	0.02	0.14	129.51
Property Tax Per Capita	7,540	0.33	0.54	0	0.09	0.15	0.29	12.83
Property Tax / Total Revenues	7,535	0.29	0.21	0	0.11	0.23	0.45	0.97
Property Tax	7,540	14,254.06	1.60E+05	0	481.80	1,968.57	8,085.22	6.80E+06

Descriptive statistics for test group data used in difference in differences regressions. Data is pre enactment of tax and expenditure limitations to better compare the descriptive data of the test group and the control group.

Table 3 – Difference in Differences Regression - Service Ratio

Service Ratio			
	Coefficient	t	p
Test Group x Post Event	-0.0029	-1.72	0.085
Test Group	0.0088	4.13	0.000
Post Event	0.0082	14.46	0.000
Observations	383,941		
R-squared	0.2984		
Additional Controls include: Year, Census Region, County, Indicator of Greater than median expenditures.			
Errors are clustered by municipality			

Difference in differences regression. Dependent variable is service ratio (Service Expenditures / (Total Expenditures – Pension Payouts)). Variable of interest is “Test Group x Post Event” which shows reduced service ratio for municipalities operating under tax and expenditure limitations.

Table 4 – Difference in Differences Regression – Relative to Enactment Year

Service Ratio			
	Coefficient	t	p
Test Group x 4 Years Prior	0.0048	1.82	0.068
4 Years Prior	-0.0082	-11.72	0.000
Test Group x 3 Years Prior	0.0039	1.49	0.136
3 Years Prior	-0.0068	-11.90	0.000
Test Group x 2 Years Prior	-0.0006	-0.28	0.779
2 Years Prior	-0.0049	-11.56	0.000
Test Group x 1 Year Prior	0.0049	2.44	0.015
1 Years Prior	-0.0032	-11.29	0.000
Test Group x 1 Year After	0.0052	2.74	0.006
1 Year After	0.0031	11.33	0.000
Test Group x 2 Years After	0.0002	0.0022	0.927
2 Years After	0.0038	9.27	0.000
Test Group x 3 Years After	-0.0036	0.0024	0.140
3 Years After	0.0039	7.21	0.000
Test Group	0.0053	2.12	0.034
Observations	383,941		
R-squared	0.2985		
Additional Controls include: Year, Census Region, County, Indicator of Greater than median expenditures.			
Errors are clustered by municipality			

Difference in differences regression. Dependent variable is service ratio (Service Expenditures / (Total Expenditures – Pension Payouts)). Variable of interest is “Test Group x 1 Year Prior” which shows a significant increase in service ratio for municipalities just before tax and expenditure limitations are implemented.

Table 5 – Difference in Differences Regression – Fire & Police

Fire and Police Service Ratio			
	Coefficient	t	p
Test Group x Post Event	0.0033	3.24	0.001
Test Group	-0.0023	-1.64	0.102
Post Event	0.0025	6.06	0.000
Observations	383,941		
R-squared	0.4031		
Additional Controls include: Year, Census Region, County, Indicator of Greater than median expenditures.			
Errors are clustered by municipality			

Difference in differences regression. Dependent variable is Fire and Police service ratio (Fire + Police Expenditures) / (Total Expenditures – Pension Payouts). Variable of interest is “Test Group x Post Event” which shows no indication of a significant reduction in the Fire + Police Service ratio for municipalities operating under tax and expenditure limitations (as shown by Figlio and O’Sullivan 2001), but instead shows a significant increase.

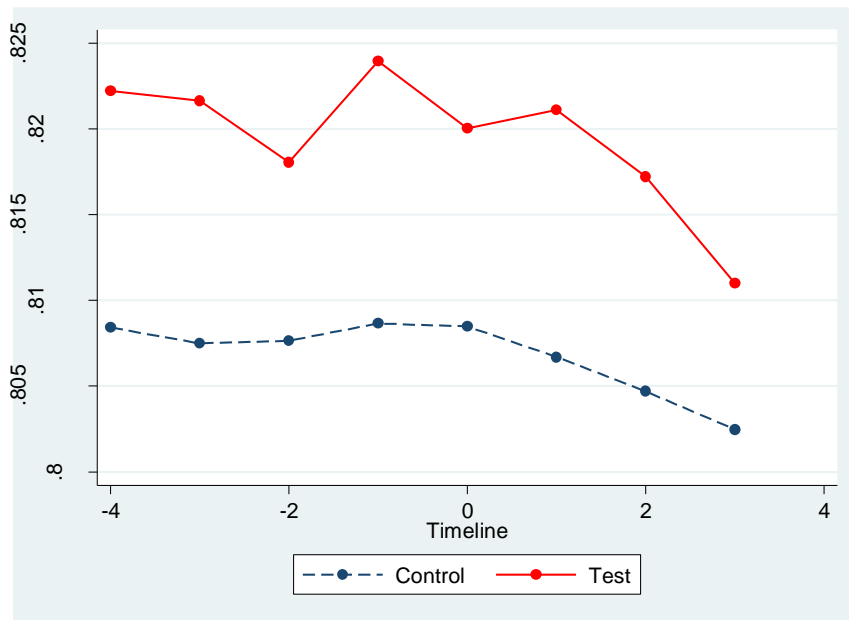
Table 6 – Difference in Differences Regression – Other Services

Other Services Ratio			
	Coefficient	t	p
Test Group x Post Event	-0.0062	-3.21	0.001
Test Group	0.0111	4.79	0.000
Post Event	0.0057	8.67	0.000
Observations	383,941		
R-squared	0.3829		
Additional Controls include: Year, Census Region, County, Indicator of Greater than median expenditures.			
Errors are clustered by municipality			

Difference in differences regression. Dependent variable is Other Services ratio (Service Expenditures – (Fire + Police expenditures)) / (Total Expenditures – Pension Payouts). Variable of interest is “Test Group x Post Event” which shows a significant decrease in the Other Services Ratio for municipalities operating under tax and expenditure limitations.

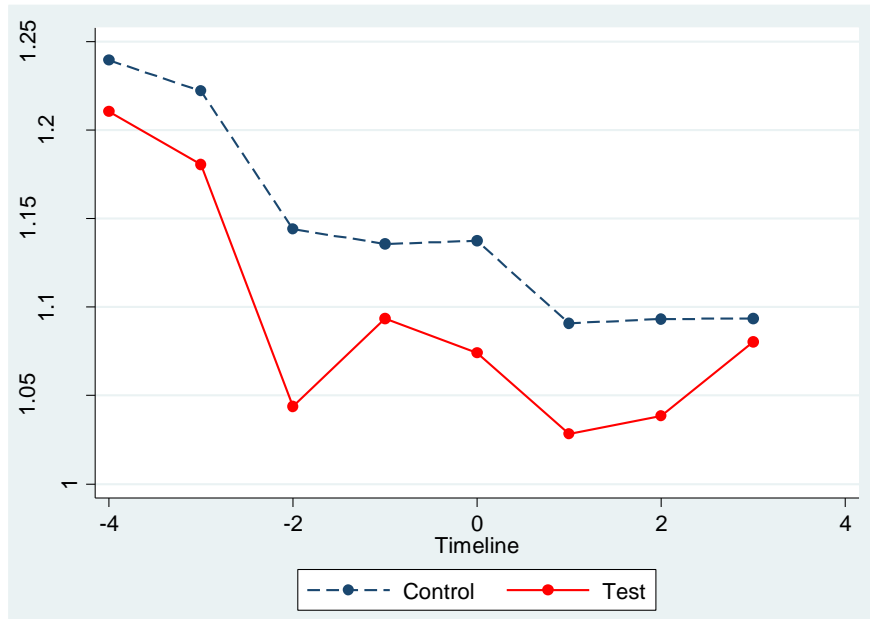
Figures

Figure 1 – Mean Service Ratio Relative to Limitation Enactment Year



Service Ratio trend (using the difference in differences regression dataset) over time. All time periods are in reference to the tax and expenditure limitation enactment year.

Figure 2 – Service Expenditure Growth Relative to Limitation Enactment Year



Trend of Service growth (Service Expenditures/Service Expenditures_{t-1}, using the difference in differences dataset). Time periods are in reference to the limitation enactment year.

Appendix

Table A1

State	1970	1971	1972	1973	1974	1975	1976	1977	1978	1979	1980	1981
Alabama	1	1	1	1	1	1	1	1	1	1	1	1
Alaska	0	0	2	2	2	2	2	2	2	2	2	2
Arizona	2	2	2	2	2	2	2	2	2	2	2	2
Arkansas	1	1	1	1	1	1	1	1	1	1	1	2
California	0	0	0	0	0	0	0	0	2	2	2	2
Colorado	2	2	2	2	2	2	2	2	2	2	2	2
Connecticut	0	0	0	0	0	0	0	0	0	0	0	0
Delaware	0	0	0	0	0	0	0	0	0	0	0	0
Florida	1	1	1	1	2	2	2	2	2	2	2	2
Georgia	0	0	0	0	0	0	0	0	0	0	0	0
Hawaii	0	0	0	0	0	0	0	0	0	0	0	0
Idaho	1	1	1	1	1	1	1	1	1	2	2	2
Illinois	1	1	1	1	1	1	1	1	1	1	1	1
Indiana	0	0	0	2	2	2	2	2	2	2	2	2
Iowa	0	0	1	1	1	1	1	1	2	2	2	2
Kansas	2	2	2	2	2	2	2	2	2	2	2	2
Kentucky	1	1	1	1	1	1	1	1	1	2	2	2
Louisiana	0	0	0	0	1	1	1	1	2	2	2	2
Maine	0	0	0	0	0	0	0	0	0	0	0	0
Maryland	1	1	1	1	1	1	1	1	1	1	1	1
Massachusetts	0	0	0	0	0	0	0	0	0	0	2	2
Michigan	1	1	1	1	1	1	1	1	2	2	2	2
Minnesota	0	2	2	2	2	2	2	2	2	2	2	2
Mississippi	0	0	0	0	0	0	0	0	0	0	2	2
Missouri	1	1	1	1	1	1	1	1	1	1	2	2
Montana	1	1	1	1	1	1	1	1	1	1	1	1
Nebraska	1	1	1	1	1	1	1	1	1	1	1	1
Nevada	1	1	1	1	1	1	1	1	1	1	1	1
New Hampshire	0	0	0	0	0	0	0	0	0	0	0	0
New Jersey	0	0	0	0	0	0	2	2	2	2	2	2
New Mexico	1	1	1	1	1	1	1	1	1	2	2	2
New York	1	1	1	1	1	1	1	1	1	1	1	1
North Carolina	0	0	0	1	1	1	1	1	1	1	1	1
North Dakota	1	1	1	1	1	1	1	1	1	1	1	2
Ohio	1	1	1	1	1	1	2	2	2	2	2	2
Oklahoma	1	1	1	1	1	1	1	1	1	1	1	1
Oregon	2	2	2	2	2	2	2	2	2	2	2	2
Pennsylvania	1	1	1	1	1	1	1	1	1	1	1	1
Rhode Island	0	0	0	0	0	0	0	0	0	1	1	1
South Carolina	0	0	0	0	0	1	1	1	1	1	1	1
South Dakota	1	1	1	1	1	1	1	1	1	1	1	1
Tennessee	0	0	0	0	0	0	0	0	0	1	1	1
Texas	1	1	1	1	1	1	1	1	1	1	1	1
Utah	2	2	2	2	2	2	2	2	2	2	2	2
Vermont	0	0	0	0	0	0	0	0	0	0	0	0
Virginia	0	0	0	0	0	0	1	1	1	1	1	1
Washington	1	2	2	2	2	2	2	2	2	2	2	2
West Virginia	1	1	1	1	1	1	1	1	1	1	1	1
Wisconsin	2	2	2	2	2	2	2	2	2	2	2	2
Wyoming	1	1	1	1	1	1	1	1	1	1	1	1

Table A1 continued

State	1982	1983	1984	1985	1986	1987	1988	1989	1990	1991	1992
Alabama	1	1	1	1	1	1	1	1	1	1	1
Alaska	2	2	2	2	2	2	2	2	2	2	2
Arizona	2	2	2	2	2	2	2	2	2	2	2
Arkansas	2	2	2	2	2	2	2	2	2	2	2
California	2	2	2	2	2	2	2	2	2	2	2
Colorado	2	2	2	2	2	2	2	2	2	2	2
Connecticut	0	0	0	0	0	0	0	0	0	0	0
Delaware	0	0	0	0	0	0	0	0	0	0	0
Florida	2	2	2	2	2	2	2	2	2	2	2
Georgia	0	0	0	0	0	0	0	0	0	1	1
Hawaii	0	0	0	0	0	0	0	0	0	0	0
Idaho	2	2	2	2	2	2	2	2	2	2	1
Illinois	1	1	1	1	1	1	1	1	1	2	2
Indiana	2	2	2	2	2	2	2	2	2	2	2
Iowa	2	2	2	2	2	2	2	2	2	2	2
Kansas	2	2	2	2	2	2	2	2	2	2	2
Kentucky	2	2	2	2	2	2	2	2	2	2	2
Louisiana	2	2	2	2	2	2	2	2	2	2	2
Maine	0	0	0	0	0	0	0	0	0	0	0
Maryland	1	1	1	1	1	1	1	1	1	1	1
Massachusetts	2	2	2	2	2	2	2	2	2	2	2
Michigan	2	2	2	2	2	2	2	2	2	2	2
Minnesota	2	2	2	2	2	2	2	2	2	2	2
Mississippi	2	2	2	2	2	2	2	2	2	2	2
Missouri	2	2	2	2	2	2	2	2	2	2	2
Montana	1	1	1	1	1	2	2	2	2	2	2
Nebraska	1	1	1	1	1	1	1	1	2	2	2
Nevada	1	2	2	2	2	2	2	2	2	2	2
New Hampshire	0	0	0	0	0	0	0	0	0	0	0
New Jersey	2	2	2	2	2	2	2	2	2	2	2
New Mexico	2	2	2	2	2	2	2	2	2	2	2
New York	1	1	1	1	2	2	2	2	2	2	2
North Carolina	1	1	1	1	1	1	1	1	1	1	1
North Dakota	2	2	2	2	2	2	2	2	2	2	2
Ohio	2	2	2	2	2	2	2	2	2	2	2
Oklahoma	1	1	1	1	1	1	1	1	1	1	1
Oregon	2	2	2	2	2	2	2	2	2	2	2
Pennsylvania	1	1	1	1	1	1	1	1	1	1	1
Rhode Island	1	1	1	2	2	2	2	2	2	2	2
South Carolina	1	1	1	1	1	1	1	1	1	1	1
South Dakota	1	1	1	1	1	1	1	1	1	1	1
Tennessee	1	1	1	1	1	1	1	1	1	1	1
Texas	2	2	2	2	2	2	2	2	2	2	2
Utah	2	2	2	2	2	1	1	1	1	1	1
Vermont	0	0	0	0	0	0	0	0	0	0	0
Virginia	1	1	1	1	1	1	1	1	1	1	1
Washington	2	2	2	2	2	2	2	2	2	2	2
West Virginia	1	1	1	1	1	1	1	1	2	2	2
Wisconsin	2	2	2	2	2	2	2	2	2	2	2
Wyoming	1	1	1	1	1	1	1	1	1	1	1

Table A1 continued

State	1993	1994	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004
Alabama	1	1	1	1	1	1	1	1	1	1	1	1
Alaska	2	2	2	2	2	2	2	2	2	2	2	2
Arizona	2	2	2	2	2	2	2	2	2	2	2	2
Arkansas	2	2	2	2	2	2	2	2	2	2	2	2
California	2	2	2	2	2	2	2	2	2	2	2	2
Colorado	2	2	2	2	2	2	2	2	2	2	2	2
Connecticut	0	0	0	0	0	0	0	0	0	0	0	0
Delaware	0	0	0	0	0	0	0	0	0	0	0	0
Florida	2	2	2	2	2	2	2	2	2	2	2	2
Georgia	1	1	1	1	1	1	1	1	1	1	1	1
Hawaii	0	0	0	0	0	0	0	0	0	0	0	0
Idaho	1	1	2	2	2	2	2	2	2	2	2	2
Illinois	2	2	2	2	2	2	2	2	2	2	2	2
Indiana	2	2	2	2	2	2	2	2	2	2	2	2
Iowa	2	2	2	2	2	2	2	2	2	2	2	2
Kansas	2	2	2	2	2	2	2	1	1	1	1	1
Kentucky	2	2	2	2	2	2	2	2	2	2	2	2
Louisiana	2	2	2	2	2	2	2	2	2	2	2	2
Maine	0	0	0	0	0	0	0	0	0	0	0	0
Maryland	1	1	1	1	1	1	1	1	1	1	1	1
Massachusetts	2	2	2	2	2	2	2	2	2	2	2	2
Michigan	2	2	2	2	2	2	2	2	2	2	2	2
Minnesota	2	2	2	2	2	2	2	2	2	2	2	2
Mississippi	2	2	2	2	2	2	2	2	2	2	2	2
Missouri	2	2	2	2	2	2	2	2	2	2	2	2
Montana	2	2	2	2	2	2	2	2	2	2	2	2
Nebraska	2	2	2	2	2	2	2	2	2	2	2	2
Nevada	2	2	2	2	2	2	2	2	2	2	2	2
New Hampshire	0	0	0	0	0	0	0	0	0	0	0	0
New Jersey	2	2	2	2	2	2	2	2	2	2	2	2
New Mexico	2	2	2	2	2	2	2	2	2	2	2	2
New York	2	2	2	2	2	2	2	2	2	2	2	2
North Carolina	1	1	1	1	1	1	1	1	1	1	1	1
North Dakota	2	2	2	2	2	2	2	2	2	2	2	2
Ohio	2	2	2	2	2	2	2	2	2	2	2	2
Oklahoma	1	1	1	2	2	2	2	2	2	2	2	2
Oregon	2	2	2	2	2	2	2	2	2	2	2	2
Pennsylvania	1	1	1	1	1	1	1	1	1	1	1	1
Rhode Island	2	2	2	2	2	2	2	2	2	2	2	2
South Carolina	1	1	1	1	1	1	1	1	1	1	1	1
South Dakota	1	1	1	1	1	1	1	1	1	1	1	1
Tennessee	1	1	1	1	1	1	1	1	1	1	1	1
Texas	2	2	2	2	2	2	2	2	2	2	2	2
Utah	1	1	1	1	1	1	1	1	1	1	1	1
Vermont	0	0	0	0	0	0	0	0	0	0	0	0
Virginia	1	1	1	1	1	1	1	1	1	1	1	1
Washington	2	2	2	2	2	2	2	2	2	2	2	2
West Virginia	2	2	2	2	2	2	2	2	2	2	2	2
Wisconsin	2	2	2	2	2	2	2	2	2	2	2	2
Wyoming	1	1	1	1	1	1	1	1	1	1	1	1

Table A1 Continued

State	2005	2006	2007	2008	2009	2010	2011	2012
Alabama	1	1	1	1	1	1	1	1
Alaska	2	2	2	2	2	2	2	2
Arizona	2	2	2	2	2	2	2	2
Arkansas	2	2	2	2	2	2	2	2
California	2	2	2	2	2	2	2	2
Colorado	2	0	0	0	0	0	0	2
Connecticut	0	0	0	0	0	0	0	0
Delaware	0	0	0	0	0	0	0	0
Florida	2	2	2	2	2	2	2	2
Georgia	1	1	1	1	1	1	1	1
Hawaii	0	0	0	0	0	0	0	0
Idaho	2	2	2	2	2	2	2	2
Illinois	2	2	2	2	2	2	2	2
Indiana	2	2	2	2	2	2	2	2
Iowa	2	2	2	2	2	2	2	2
Kansas	1	1	1	1	1	1	1	1
Kentucky	2	2	2	2	2	2	2	2
Louisiana	2	2	2	2	2	2	2	2
Maine	2	2	2	2	2	2	2	2
Maryland	1	1	1	1	1	1	1	1
Massachusetts	2	2	2	2	2	2	2	2
Michigan	2	2	2	2	2	2	2	2
Minnesota	2	2	2	2	2	2	2	2
Mississippi	2	2	2	2	2	2	2	2
Missouri	2	2	2	2	2	2	2	2
Montana	2	2	2	2	2	2	2	2
Nebraska	2	2	2	2	2	2	2	2
Nevada	2	2	2	2	2	2	2	2
New Hampshire	0	0	0	0	0	0	0	0
New Jersey	2	2	2	2	2	2	2	2
New Mexico	2	2	2	2	2	2	2	2
New York	2	2	2	2	2	2	2	2
North Carolina	1	1	1	1	1	1	1	1
North Dakota	2	2	2	2	2	2	2	2
Ohio	2	2	2	2	2	2	2	2
Oklahoma	2	2	2	2	2	2	2	2
Oregon	2	2	2	2	2	2	2	2
Pennsylvania	1	1	1	1	1	1	1	1
Rhode Island	2	2	2	2	2	2	2	2
South Carolina	1	1	1	1	1	1	1	1
South Dakota	1	1	1	1	1	1	1	1
Tennessee	1	1	1	1	1	1	1	1
Texas	2	2	2	2	2	2	2	2
Utah	1	1	1	1	1	1	1	1
Vermont	0	0	0	0	0	0	0	0
Virginia	1	1	1	1	1	1	1	1
Washington	2	2	2	2	2	2	2	2
West Virginia	2	2	2	2	2	2	2	2
Wisconsin	2	2	2	2	2	2	2	2
Wyoming	1	1	1	1	1	1	1	1

Table A2

State	Freq.
Alabama	8,433
Alaska	2,821
Arizona	1,931
Arkansas	8,792
California	12,070
Colorado	5,137
Connecticut	5,854
Delaware	1,343
Florida	8,632
Georgia	11,506
Hawaii	43
Idaho	3,975
Illinois	45,844
Indiana	32,107
Iowa	16,252
Kansas	31,824
Kentucky	8,739
Louisiana	5,627
Maine	10,726
Maryland	2,956
Massachusetts	10,912
Michigan	30,591
Minnesota	43,038
Mississippi	5,847
Missouri	20,331
Montana	2,772
Nebraska	15,797
Nevada	610
New Hampshire	5,787
New Jersey	13,856
New Mexico	2,127
New York	30,598
North Carolina	10,276
North Dakota	27,168
Ohio	39,115
Oklahoma	10,000
Oregon	4,825
Pennsylvania	46,482
Rhode Island	1,567
South Carolina	5,646
South Dakota	20,662
Tennessee	6,403
Texas	19,541
Utah	4,402
Vermont	7,236
Virginia	4,575
Washington	5,530
West Virginia	4,908
Wisconsin	37,600
Wyoming	2,371

Full sample data frequency.

Table A3

Region	Freq.
Midwest	360,329
Northeast	133,018
South	123,224
West	48,614

Full sample data frequency by official Census Region.

Table A4

Service (Productive) Expenditures	Administrative Expenditures
Road Maintenance	Total Interest on Debt
Fire	Long Term Debt Retired
Parks	Financial Administration (accountants, etc.)
Waste Management	Judicial and Legal (juries, prosecutors, attorneys..)
Water Supply	Central Staff Services (city council)
Hospitals	General Public Buildings (government offices, not police)
Health	Correctional Institutions
Police	
Libraries	
Public Welfare (support to needy)	
Inspections for protection of public	
Housing and Community Development	
Natural Resources	
Airports	
Electrical Grid	
Parking	
Gas Supply	
Transit System	
Ports	
Education	

List of U.S. Census Bureau expenditure classifications as detailed in the survey to municipal governments. I determine classification of costs as service or administrative subjectively to capture the likelihood that residents find more than fire and police expenditures to be beneficial.