

# Did State Imposed Tax and Expenditure Limits Reduce the Fiscal Size of Local Governments? Revisiting the Evidence

Justin M. Ross\*, John D. Stavick\*\*, and Patrick Carlin\*\*\*

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## Abstract

This paper revisits the research question of whether or not state-on-local tax expenditure limits (TEs) reduced the fiscal size of local governments. The often cited research on the subject appeared in the 1990s and early 2000s with the consensus conclusion that these policies did have that consequence, however this literature had many limitations in research design common to the work of that time. We update the data, empirical strategy, and inferential techniques for American city and county governments. Namely, we explore results using more contemporary identification strategies that includes cross-state border differencing and stacked differences-in-differences. <PRELIMINARY: The simple panel with two-way fixed effects reproduce the large estimates of the previous literature. The border discontinuity design has a similar pattern of findings but indicates these policies are substantially smaller than those provided by the previous literature. Examining each state experiment against a common “never adopt” control group produces far more heterogeneous results that in aggregate suggest the policies are ineffective and even increase the fiscal size of government.>

\*Professor of Public Economics, Paul H. O’Neill School of Public & Environmental Affairs, Indiana University, Bloomington, IN; [justross@indiana.edu](mailto:justross@indiana.edu)

\*\*Doctoral Candidate of Public Affairs, Paul H. O’Neill School of Public & Environmental Affairs, Indiana University, Bloomington, IN; [jstavick@iu.edu](mailto:jstavick@iu.edu)

\*\*\*Doctoral Student of Public Affairs, Paul H. O’Neill School of Public & Environmental Affairs, Indiana University, Bloomington, IN; [patcarli@iu.edu](mailto:patcarli@iu.edu)

## I. INTRODUCTION

One of the most significant shifts in American intergovernmental relations of the 20<sup>th</sup> century was the occurrence of a wave of “tax revolts” in the 1970s and 80’s that resulted in a collection of State-on-Local Tax and Expenditure Limits (TELS). These were policies in which the state could constrain the amount of autonomy local governments could exercise in determining their public finances. The motivation for these TELS, like most public policies, were somewhat varied or unclear but certainly included concerns about individual property taxpayer burden, the fiscal size of local government, and other concerns over the trustworthiness of local government. Social scientists recognized their importance immediately: the American federalist system of disbursed political power is supported by some degree of fiscal independence, so there existed potential in these policies to substantively tilt the entire political system. In the thousands of papers written on these policies, the earliest research investigated the question of whether these policies affected the fiscal size of local government, which is the subject we seek to revisit in this paper.<sup>1</sup> To state it directly, did the wave of TELS decrease the fiscal size of local governments in the United States?

Most contemporaneous papers on the subject of TELS summarize the consensus view of the literature as being supportive of TELS having restricted the fiscal size of local government, particularly in terms of their property tax reliance. We will substantiate and clarify this claim better in section II, but suffice it to say for now that **Figure 1** illustrates the main results of the most relevant previous research informing this view. These estimates range from around -3 to -12 percent for the property tax and -2 to -10 percent for total expenditures. While these papers are all published in highly regarded journals, they otherwise differ substantially in terms of the econometrics, data, case of investigation, and methodological concerns of the authors. More striking, however, are their differences from conventional empirical microeconomics research that is published in these same journals today on questions of applied policy research. In their time, they were largely concerned with criticisms of empirical work often associated with Leamer (1983) that emphasized sensitivity analysis in model selection, rather than credibility in

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<sup>1</sup> For example, a Google Scholar search of the string “Tax and Expenditure Limitations” yields 2,250 results as of December 4, 2019.

research design that characterizes modern applied work highlighted in Angrist and Pischke (2010).

It is not, therefore, our intention to replicate the previous research nor explain their findings in the style of a meta-analysis, but rather revisit *tabula rasa* the investigation of TELs influence on local government fiscal size. To do this, after coding TELs using the Mullins and Wallin (2004) taxonomy, we rely on data from the Census of Government Finances that occurs every 5 years from 1972 to 2017 (T=10) for 22,000 general purpose local governments at the county and municipal level. Our use of local governments across the continental U.S. is different from much of the previous studies, which tended towards case studies of local governments operating under a single state. The motivation at the time was partly data limitations, but it was also motivated by policy heterogeneity, particularly in terms of the potential stringency of the TELs across states that would motivate an abundance of work that came after those cited in **Figure 1**. Of course, this is not a problem unique to TELs in applied policy research as it is seen in nearly all state level policies (e.g. occupational licensing, regulation, minimum wages, etc.). Therefore, we interpret our results as intent-to-treat estimates, provide some randomized treatment standard errors to address concerns about over-rejecting the null, and provide some state specific estimates. Lastly, we provide a state-border differencing model as a strategy for causal investigation, which most of the literature relied upon satisfactory control variables to address the concern that TELs captured an unobserved preference for smaller local government or lower tax burdens.

To preview our findings, we show that the two-way fixed effect models on the national sample produce large negative effects of the policies compared to the previous literature. Like the previous literature, the effect of TELs on property taxes is larger than the effect on total expenditures with magnitudes around -18 percent and -10 percent, respectively. Additionally, like the previous literature these results are sensitive to the inclusion of control variables. However, when we undertake a border differencing approach to identification, these estimates shrink substantially to about -3.7 percent for property taxes and -1.1 percent for total expenditures, at the very low end of the literature. Our conclusion is that the TEL policies adopted in this era did reduce the fiscal size of local government, but considerably less so than implied by the previous research.

The next section overviews relevant background on TELs for the purposes of this paper. In addition to substantiating our claims about the previous research, we also provide a discussion of the policies themselves, their passage among the states, and how they are coded for this research. Section 3 overviews the data and empirical strategy for results presented in Section 4. The paper concludes in Section 5 with a review of the findings, limitations, and areas for further research.

## **II. BACKGROUND ON TAX AND EXPENDITURE LIMITS**

### ***TELS Background***

Modern state-on-local limitations of public finances are often sourced to a series of policies originating in the 1970s. The most infamous of these is California's Proposition 13, passed in 1978 as part of a national wave of "tax revolts" that resulted in numerous similar referenda. The 1930s featured some ineffectual ancestors in the form of statutory limits on property tax rates that are widely regarded as having no real promise of serving as a binding constraint on local governments (Mikesell, 2018: 557). The modern policies are far more detail-oriented in their attention to nuances of the local budgeting systems in respective states. Consequently, public finance scholars have provided taxonomies for the degree to which these limitations are actually "binding" for the particular purposes of their research question. Seljan (2014), for example, categorizes TELs on the basis of their relationship to aggregate fiscal data versus that of an individual taxpayer's bill in order to assess how faithfully local politicians comply with TEL limits (an instance of the principal-agent problem) and where the costs imposed on taxpayers to monitor the policy makers' degree of compliance with it is sufficiently small. Amiel, Deller & Stallman (2009) construct a TEL stringency index which is a function of the process by which the TEL was enacted, what fiscal functions are restricted by the TEL, the treatment of surplus revenue collections, and the threshold necessary for voter approval of tax increases.

For the purpose of this paper, we adopt the taxonomy of Mullins and Wallin (2004) for potentially binding limitations. This taxonomy focuses on instances that restrict funds available to the local government unit, so limitations affecting individual taxpayers' bills are only binding if that limitation statutorily affects the aggregate amount of revenue available to the local

government. To explore the logic of a binding TEL in this framework, it is important to understand how the American property tax differs from most other ad valorem taxes. Instead of defining a tax rate that is applied to a flow of exchanges, local governments generally define their level of expenditure (E) after forecasting non-property tax revenue and other receipt sources (R), with the difference in the two figures resulting in the property tax levy (L) that is the amount of revenue to be raised from taxing property. Hence the local government works with the budget identity that  $E \equiv L + R$ . The property tax base is estimated through an assessment process, with the result divided into the levy to determine a property tax rate.<sup>2</sup> Consequently, at the margin, local government expenditures are determined by the property tax levy, so binding TEL restrictions are such that they restrict (1) the size or growth of total expenditures, (2) the size or growth of the property tax levy, or (3) both the property tax rate and the base of assessed property values. This does not represent the full array of policies to which scholars have applied the term “Tax Expenditure Limit” (e.g. supermajority voting requirements, public disclosure rules such as “truth in taxation” requirements, assessment only limitations, etc.), but this taxonomy is the most commonly studied form and is directly related to regulating the fiscal size of local government.

We use previous studies published under this framework from Mullins and Wallin (2004), Seljan (2013), and the Lincoln Institute for Land Policy’s Significant Features of the Property Tax as guidance on the current legal status of TELs in each state. We conducted an independent review of relevant state statutes, constitutions, legislative reports, and relevant state agency publications in conjunction with the TEL’s effective date (in states with enacted TELs) for cross-validation (documented in **Appendix A**). We assign a dummy variable a value equal to one under the Mullins and Wallin (2004) binding TEL taxonomy for states that restrict local overall tax collections, expenditures, or the combination of rates and assessment levels. Alaska and Hawaii are both omitted from the study.

**Figure 1** illustrates a state-by-state timeline that starts in 1970 through 2016 to indicate years in which binding TELs exist. **Figure 1** demonstrates that while just four states had such a TEL in 1970, this was an expansionary period during which as many as 35 states would join the scene. In the two years following the adoption of California’s Proposition 13, seven additional

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<sup>2</sup> The literature on fiscal illusion has found that the behavior of local politicians is at least consistent with voters being fooled by the workings of the property tax mechanism (see Brien, 2018; Ross and Yan, 2012; Ross and Mughan, 2018).

states adopted a TEL, including three western states (Arizona, Idaho, and New Mexico), the most rapid growth in TEL adoption over the time series. This is consistent with the policy diffusion argument made by Seljan & Weller (2011), who find that the probability of a state enacting a TEL is related to whether a neighboring state chooses to. However, TELs are not necessarily a permanent fiscal institution once enacted, as three states repealed their TELs or modified their binding nature over the study period: Arkansas (2000), Minnesota (1993), and Utah (1986). For example, Minnesota repealed its limitations on property tax collections payable beginning in 1993, and was replaced with a “Truth in Taxation” system with the goal of enhancing public participation in the property tax assessment and local budget setting process ([Minnesota House of Representatives, 2013](#)).

### ***TEL Adoption***

The pattern and motivation of TEL adoption across states is an interesting research question in its own right, but its relevance for this paper lies in the prospect for endogeneity with respect to the local government. This section briefly reviews the literature on adoption to provide two points. First, a priori there is theoretical justification for both positive and negative pre-trends in taxes and expenditures for the treatment group. To the former, voters with anti-tax and small government preferences might be systematically more likely to also vote for TELs; while to the latter, voters experiencing growing taxing bills or difficulty in constraining their local government size due to some political economy problem might result in positive pre-trends. Secondly, within state political geography might explain to some degree why voters use the state to control the actions of local governments in areas where they do not live. While this might make the individual local government’s fiscal behavior more exogenous with respect to state TEL policy, it also implies that cross-state comparisons provide a more valid control group than within state.

The motivation for TELs are subject to debate (see Anderson, 2006). One of the most popular views is that these “revolts” were linked to school finance equity reforms that substantively diminished the role of the property tax as a “benefit” tax (Fischel, 1989; 2001). Numerous scholars have posited motives for TELs that explicitly or implicitly have an ideological motivation, particularly those pertaining to anti-tax sentiments (Lowery & Siegelman, 1981). However, the evidence is mixed whether political ideology has more

explanatory power in predicting TEL adoption than growth in personal income and property tax collections (Alm & Skidmore, 1999). For instance, in constraining their own governments voters might seek to signal their preferences or provide a more credible long-term commitment to those preferences by voting for “insurance” against future property tax increases (Nechyba, 1997).

This is a more satisfying explanation in cases where voters constraining governments that directly represent them (e.g. their own city or state), but less so in understanding why they seek to constrain the governments of *other* voters. One way to make this explanation work is by introducing decisive voters who wish to constrain their local unit and do not care whether those constraints are local or statewide in origin. This would imply differences in the spatial distribution of decisive voters, where citizens with preferences for larger government are concentrated in selected areas and do not possess a broader state-wide majority. Another motivation, posited by Vigdor (2004), is that citizens seek to extend their voting franchise into non-resident areas to expand the number of prospective locations that better fit their policy preferences. This similarly requires localities that would otherwise have heterogeneous polity across the state from those preferred by the decisive voter. Other theories, such using a state-on-local TEL to institutionalize state fiscal monitoring or establishing the state as a source of external constraint, may be ideologically motivated but do not imply anything particular about the spatial distribution of voter preferences.

### ***Effects of State TELs on Local Finances***

Consistent with the Mullins and Wallin (2004) taxonomy, the earliest literature investigating the effects of TELs on the finances of local governments overwhelmingly examined the effects on measures of property taxes and total expenditures. An excellent comprehensive overview of the literature on these and many outcomes is provided in Stallmann et al. (2017). Figure 1 already previewed this literature by plotting comparable point estimates, but we caution the reader that this is a subset of papers in which it was possible to adopt, or calculate from the study’s given descriptive statistics, comparable elasticities as the treatment effects. More studies exist where this was not possible, or there were modeling choices in the measurement or design of the TEL that made it less theoretically comparable, and most of these papers had more nuanced interests being explored than just the broad effect of the TEL. The overall view from Figure 1 does not

depart substantively from the likely consensus view of scholars, which we agree with Stallmann et al. (2017, p. 209) in characterizing as (citations withheld for brevity):

*Many local-level TELs studies have been case studies or cross-sectional and panel analyses that have relied on dummy variables for TEL measurement. The local-level (municipal, school district, and county) research on TELs has consistently demonstrated that TELs reduce reliance on property taxes. A recent analysis of the effects of a uniform TEL in Denmark reveals findings that are consistent with U.S.-based research: TELs cause revenue-shifting strategies away from those taxes that are constrained to greater reliance on intergovernmental aid and, thus, have little effect on expenditures.*

Appendix Table B provides the summary of the papers themselves that our review turned up as the most frequently cited support for this consensus view. Figlio and O’Sullivan (2001) and Chaman and Gorina (2012) are the only studies which use local governments as the unit of observation and rely on cross-state variation, and only Figlio and O’Sullivan (2001) employ a panel dataset. The other studies rely on state aggregates of local governments, or represent single state case studies with pre-post analysis.

### III. EMPIRICAL STRATEGY

We then seek to determine if state-on-local TEL statutes are more binding as a function of the ideological wedge between the local voters and the state median voter. Let local government  $i$  from state  $s$  in year  $t$  allocate budget  $Y$  that is the result of a local decisive voter demand function for public goods in the tradition of Borcharding and Deacon (1972) and Bergstrom and Goodman (1973). This model assumes utility to the voter is derived from both private and public good consumption with some degree of non-rivalry in the public good. Deriving the expenditure function from a constant elasticity demand function in log-log form results in the following empirical demand function for government services:

$$(1) \ln Y_{it} = \delta V_{it} + \rho \ln T_{it} + \lambda \ln N_{it} + \theta \ln I_{it} + \varepsilon_{it}.$$

In the above, intergovernmental transfers from non-local sources ( $T$ ), local population ( $N$ ), per capita income personal ( $I$ ), and other demand shifting voter attributes ( $V$ ) result in the budget allocation along some optimization error ( $\varepsilon_{it}$ ). The budget allocation outcomes we adopt include



the property tax levy, total general expenditures, and total expenditures on current operations.<sup>3</sup> The voter attributes included in  $V$  will be the share of the local population that voted for the democratic candidate in the presidential election, as described in section II, as well as the share of the employment in manufacturing and the share in farm. **Table Z** provides summary statistics, variable definitions, and data sources.

In principle, equation (1) provides the presumed empirical demand function of the local decisive voter that would determine local public expenditures in the absence of any external constraints. State-on-local TELs are presumed to have some potential effect on altering those public financing outcomes away from those preferred by the local decisive voter. We sweep all the predictors of equation (1) into vector  $\mathbf{X}$ , add state ( $\theta_i$ ) and year or state-year fixed effects ( $\tau_t$ ), and update the equation to include an indicator for a state-on-local TEL that is in effect but is exogenous to the decisive voter in the locality:

$$(2) \ln Y_{it} = \alpha TEL_{it} + \beta X_{it} + \theta_i + \tau_t + \varepsilon_{it}.$$

The two-way fixed effect (TWFE) empirical model specified in equation (2) regards  $\alpha$  as a shift parameter that distorts outcomes on average by comparing variation within cities whose states adopted or suspended a state-on-local TEL during the study period to those that either always or never adopted a TEL.<sup>4</sup>

Equation (2) will be estimated and reported in the next section as a baseline estimate, but the primary concern is in obtaining a well-identified estimate of  $\alpha$ . If TEL adoption is endogenous to local preferences for budget size in some manner that is not captured by covariates and the included fixed effects. If state policies on restricting local government fiscal size are correlated with local preferences over own-government fiscal size in a manner that is time-varying, then the estimation of equation (2) will yield an  $\alpha$  that is more negative than the true effect of the policy.

### *Inference using Stacked Differences-in-Differences*

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<sup>3</sup> Total general and total current expenditures differ primarily by expenditures on capital projects, which is a very noisy fiscal outcome for many local governments. As described in Section 2, the property tax levy supports the marginal dollar of local expenditure, so all three of these outcomes are different measures of spending concepts.

<sup>4</sup> Appendix Table C1 and C2 provides the results of estimating equations (1) and (2) for the reader's reference.

One of the possible sources of bias in estimating the TWFE model is that the post period of early adopters become controls for the late adopters. Furthermore, as already stated multiple times, the pre-trends going into TEL adoption could be positive or negative, so heterogeneity in pre-trends is by itself an interesting phenomena to explore. Therefore, we will further explore TEL adoption by considering each state as a unique experiment tested against a control group of local governments from the 14 states with no TEL policy to collect a group of 24 difference-in-difference regressions, as well as a pooled group of all experiments.

A key advantage of this approach is that early adopter's post periods will not be employed as a control for the later treated groups in their pre-period (Goodman-Bacon, 2021). A drawback of the stacked differences in the context of these TEL policies is that some of the earliest and latest adopters with a shorter pre-period cannot be readily adapted to a common stack length. Furthermore, some states repeal the binding TEL (see Figure 2) and are dropped from our analysis.

#### *Identification Using State Border Differencing*

Another identification strategy we consider is a cross-state border differencing of the model, a strategy used in other public finance literature that includes Thompson and Rohlin (2012), Ross and Mikesell (2017), <several more>.

To illustrate more clearly what the cross-state contiguous border differencing plausibly accomplishes, we can augment equation (2) with a unit-specific and time-varying factor ( $\vartheta_{it}$ ) that is correlated with  $TEL$  and the error term:

$$(3) \ln Y_{it} = \alpha TEL_{it} + \beta X_{it} + \vartheta_{it} + \theta_i + \tau_t + \varepsilon_{it}.$$

We can similarly identify an identical data generating process for some local government  $j$  that differs from  $i$ . If we difference the processes described in equation (3) for  $i$  from that of  $j$ , after some rearrangement it yields:

$$(4) \ln Y_{it} - \ln Y_{jt} = \alpha(TEL_{it} - TEL_{jt}) + \beta(X_{it} - X_{jt}) + (\vartheta_{it} - \vartheta_{jt}) + (\theta_i - \theta_j) + (\varepsilon_{it} - \varepsilon_{jt}).$$

If units  $i$  and  $j$  are matched such that the unobserved factors are equal ( $\vartheta_{it} = \vartheta_{jt}$ ), then these terms drop out and an unbiased estimate of  $\alpha$ . The implicit assumption of state border studies is

that these terms are equal, or at least highly correlated, for the pairs of cross-state contiguous observations so as to dampen the bias; If the terms were uncorrelated and therefore not differencing out, or if there were no bias to begin with when estimating equation (3), then equation (4) would replicate the results of (3). Using subscript  $p$  to denote such border pairs (BP) the equation (4) is rewritten as

$$(5) \ln Y_{pt} = \alpha TEL_{pt} + \beta X_{pt} + \theta_p + \varepsilon_{pt}.$$

The next section presents the main findings of estimating equation (3).

#### **IV. RESULTS [INCOMPLETE AND PRELIMINARY]**

Tables 1 and 2: Two way fixed effect Results

Tables 3 and 4: Border Pair Differencing Results

#### **V. CONCLUSION**

TBD

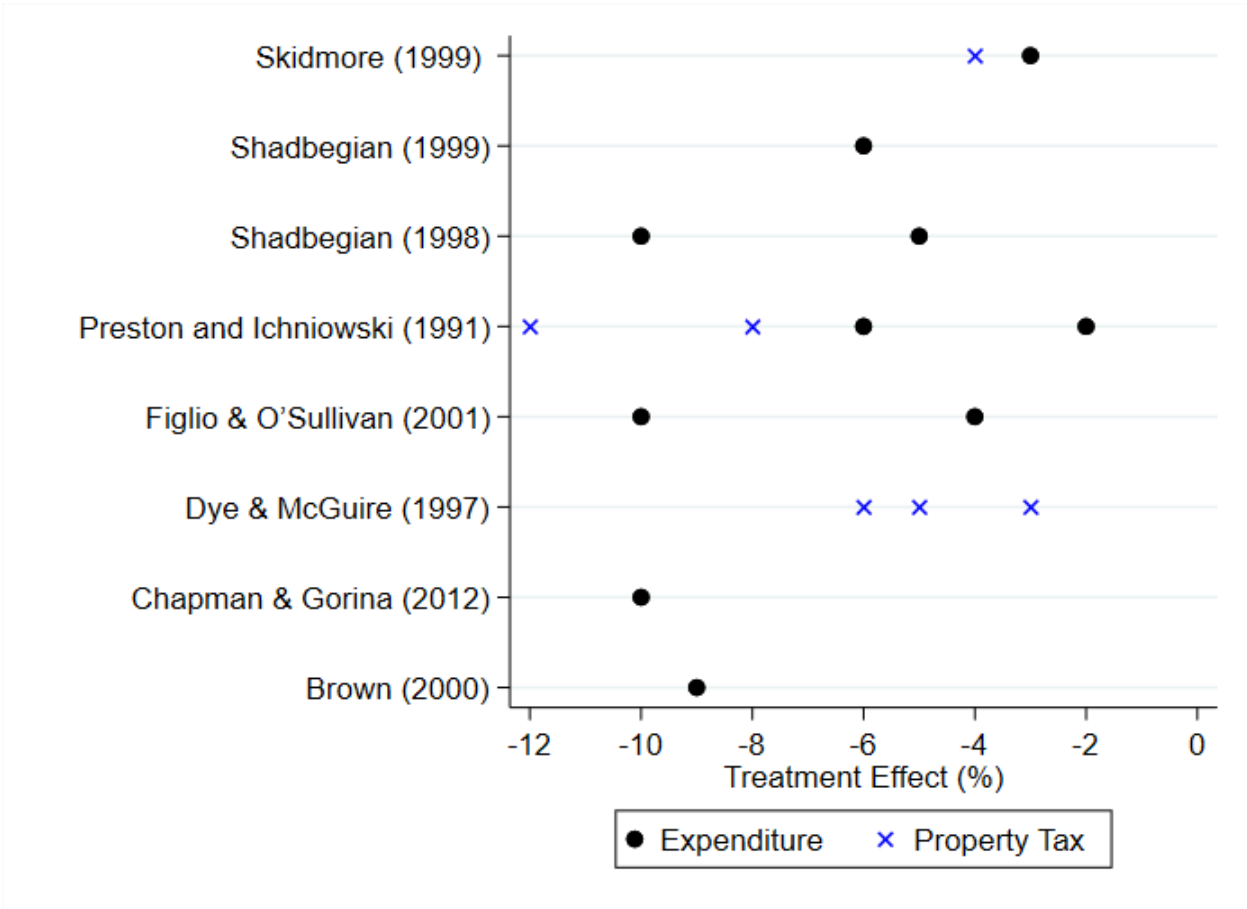
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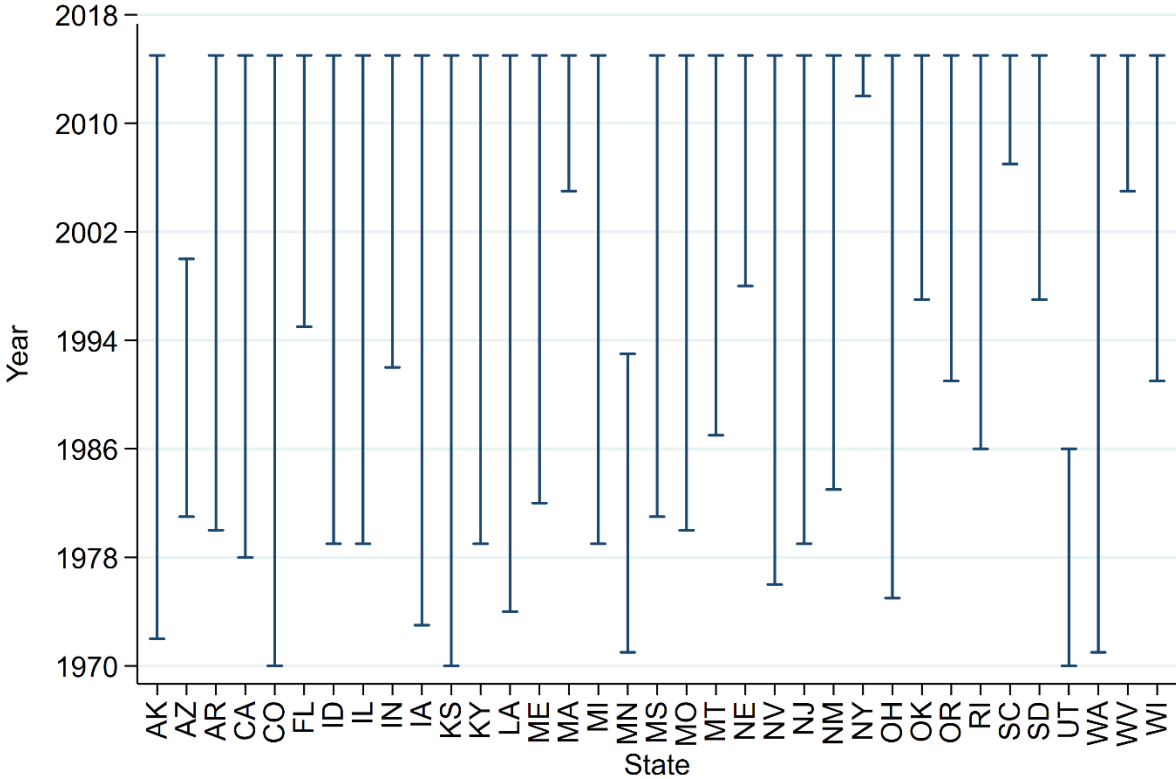
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**Figure 1: Previous Estimates of State-on-Local Tax and Expenditure Limits on Local Government Fiscal Outcomes**



**Figure 2: State Timelines for Binding State-on-Local Tax Expenditure Limitation Policies in Effect, 1970-2016**



Source: Based on authors' coding of state laws using the Mullins and Wallin (2004) taxonomy for binding TELs.





Table 1: Two Way Fixed Effects for Property Taxes

	(1)	(2)	(3)	(4)	(5)	(6)
	<b>Ln(Property Tax Revenue)</b>					
TEL	-0.187*** (0.00606)	-0.187 (0.121)	-0.161*** (0.0513)	-0.163*** (0.00577)	-0.163 (0.113)	-0.0425 (0.0677)
SE Cluster	ID	State	ID	ID	State	ID
X Variables	No	No	No	Yes	Yes	Yes
(State x Year) FE	No	No	Yes	No	No	Yes
N	193162	193162	193162	193162	193162	193162
K1	22652	22652	22652	22652	22652	22652

Standard errors in parentheses

\* p<0.10, \*\* p<0.05, \*\*\* p<0.01

Table 2: Two Way Fixed Effects for Total General Expenditures

	(1)	(2)	(3)	(4)	(5)
	IHS(Total Expenditures)	IHS(Total Expenditures)	IHS(Total Expenditures)	IHS(Total Expenditures)	IHS(Total Expenditures)
State-on-Local TEL	-0.0752*** (0.00504)	-0.0752* (0.0435)	-0.102* (0.0554)	-0.0204*** (0.00439)	-0.0204 (0.0243)
SE Cluster	ID	State	ID	ID	State
X Variables	No	No	No	Yes	Yes
(State x Year) FE	Yes	Yes	Yes	Yes	Yes
N	193162	193162	193162	193162	193162
K1	22652	22652	22652	22652	22652

Standard errors in parentheses

\* p<0.10, \*\*

p<0.05, \*\*\* p<0.01

Table 3: Border Pair Difference Estimates for Property Tax Revenues

	<b>Ln(Property Tax Revenue)</b>			
TEL	-0.0449***	-0.0449	-0.0372***	-0.0372
	(0.00245)	(0.0703)	(0.00243)	(0.0732)
SE Cluster	ID	State	ID	State
X Variables	No	No	Yes	Yes

Table 4: Border Pair Difference Estimates for Total General Expenditures

	<b>Ln(Total Expenditures)</b>			
TEL	-0.0185***	-0.0185	-0.0111***	-0.0111
	(0.00203)	(0.0256)	(0.00193)	(0.0286)
SE Cluster	ID	State	ID	State
X Variables	No	No	Yes	Yes

## Appendix A: TEL Coding Decisions and Sources by State

State	Years TEL effective	Sources
Alabama	0	Amendment 373 (1978)
Alaska	N/A	Alaska is omitted from our study.
Arizona	>=1980	<a href="#">Arizona Legislature Historical Property Tax Changes</a>
Arkansas	1981-2000	<a href="#">Amendment 79 Assessor Guide</a>
California	>=1978	<a href="#">California Legislative Analyst</a>
Colorado	>=1913	<a href="#">CO Department of Local Affairs</a> , <a href="#">Colorado Legislature</a>
Connecticut	0	<a href="#">Connecticut General Assembly</a>
Delaware	0	
Florida	>=1995	<a href="#">Florida Senate</a> , <a href="#">Florida Department of Revenue</a> , Fla. Stat. 193.1554, 1555
Georgia	0	
Hawaii	0	
Idaho	>=1979	<a href="#">Idaho State Tax Commission</a> , <a href="#">Source 2</a>
Illinois	0	<a href="#">Illinois Department of Revenue</a> : State statute provides the statutory form for selected local governments to self-impose a TEL.
Indiana	>=1973	<a href="#">Indiana University Public Policy Institute</a>
Iowa	>=1979	<a href="#">Iowa Department of Revenue</a> , <a href="#">Iowa Legislature</a>
Kansas	>=1970	<a href="#">Wichita State University</a> , <a href="#">Kansas Department of Revenue</a> , <a href="#">Kansas Legislature</a>
Kentucky	>=1979	<a href="#">KRS § 160.470</a>
Louisiana	>=1974	
Maine	>=2005	<a href="#">Maine Legislature</a>
Maryland	0	
Massachusetts	>=1982	<a href="#">Massachusetts Department of Revenue</a>
Michigan	>=1979	<a href="#">Headlee Amendment Article IX §25-33 Mich. Constitution</a>
Minnesota	1971-1993	<a href="#">Minnesota House of Representatives</a>
Mississippi	>=1980	Miss Code § 27-39-320
Missouri	>=1981	Article X, § 16-24 MO Constitution
Montana	>=1987	<a href="#">Initiative I-105</a> , <a href="#">Montana Legislature</a>
Nebraska	>=1998	Neb. Rev. Stat. §77-3442, <a href="#">Nebraska Legislative Fiscal Office</a>
Nevada	>=1983	<a href="#">SB27 (1983)</a> , <a href="#">Nevada Tax Commission</a> , <a href="#">NRS 361.471 thru 361.4735</a>
New Hampshire	0	
New Jersey	>=1976	N.J.S.A. 40A:4-45.44, <a href="#">NJ Division of Local Government Services</a>
New Mexico	>=1979	NMSA §7-37-7.1, <a href="#">New Mexico Department of Finance and Administration</a>
New York	>=2012	<a href="#">New York Department of Taxation and Finance</a>
North Carolina	0	<a href="#">NC Legislature</a>
North Dakota	0	<a href="#">North Dakota Legislature</a>
Ohio	>=1975	<a href="#">Sec. 2, Art. XII, Ohio Const.</a>

Oklahoma	>=1997	<a href="#">State Question 676 (1996)</a>
Oregon	>=1991	<a href="#">Sec. 11b, Art. XI, Oregon Constitution; Sec. 310.140, ORS</a>
Pennsylvania	0	<a href="#">Pennsylvania Legislature</a>
Rhode Island	>=1986	<a href="#">Rhode Island Legislature</a>
South Carolina	>=2007	<a href="#">South Carolina Department of Revenue, SC Code §12-37-3140</a>
South Dakota	>=1997	10-13-35 SDCL
Tennessee	0	
Texas	0	
Utah	1969-1986	<a href="#">Utah Legislature</a>
Vermont	0	
Virginia	0	<a href="#">Sec. 58.1-3321 VA Code</a>
Washington	>=1972	<a href="#">Washington Legislature</a>
West Virginia	>=1991	<a href="#">WV Code §11-8-6b</a>
Wisconsin	>=2005	<a href="#">Wisconsin Department of Revenue</a>
Wyoming	0	

### Appendix B: Studies Cited for Figure 1 Estimates

<b>Study</b>	<b>Sample</b>	<b>Level of Observation</b>	<b>Dependent Variable</b>	<b>Comparison Groups</b>	<b>Method</b>
Dye & McGuire (1997)	Illinois, 1987-1993	Local governments	Per Capita Property Taxes and Expenditures	Local governments in counties with/without TEL	Comparison of mean growth rate for treatment and control during pre and post period.
Skidmore (1999)	National, 1976-1990	State aggregates of local government finances	Per Capita Property Taxes and Expenditures	States with/without TELs	Panel estimated with two way fixed effects.
Shadbegian (1998)	National, 1972-1992	State aggregates of local government finances	log of property taxes per capita	States with/without TELs	Panel estimated with two way fixed effects.
Figlio & O'Sullivan (2001)	Cities, 1975-86	U.S. Cities with more than 25,000 residents	per capita program expenditures	Cities in states with/without TEL override	Panel estimated with two way fixed effects.
Chapman & Gorina (2012)	378 municipalities in 44 states, 2002	Municipalities with more than 50,000 residents	Per capita revenues and expenditures	Municipalities in states with/without TEL	Three-stage least squares with county fixed effects.
Brown (2000)	Colorado, 1975-1996	Municipal governments	Per capita revenues and expenditures	None, uses time trend interaction with policy amendments.	Unit fixed effects with time trend and lagged dependent variable.
Preston and Ichniowski (1991)	National, 1976-1986	State aggregates of local government finances	Change in per capita revenues; change in logged revenues	States with/without TELs	Ordinary least squares