Ideological Agglomeration and the Preemption of Local Control

Justin M. Ross
Associate Professor
School of Public & Environmental Affairs
Indiana University, Bloomington
justross@indiana.edu

John D. Stavick
Doctoral Candidate
School of Public & Environmental Affairs
Indiana University, Bloomington
jstavick@iu.edu

Draft 4/22/2018

Abstract

A conventional view of instances where states preempt the legal or managerial autonomy of local governments is that within-state voters attempt to extend their voting franchise into areas where they lack residency (Vigdor, 2004). This paper investigates this prospect by seeking to determine if State-on-Local Tax Expenditure Limit statutes are more binding where local voters are more liberal than the state voter population. We find some support for this hypothesis, but it is sensitive to model specification and fiscal outcome. We also find no empirical support for spatial ideological diversity as a determinant of TEL adoption.

I. INTRODUCTION

Public policies in federalist systems require a delegation of authority and responsibility to a particular level of government. Allowing more localized autonomy in policy determination allows for citizens to set different standards through their local political process or relocate to those areas with policies better suited to their preferences (Tiebout, 1956; Oates, 1999). The gains to decentralization in federalist systems is therefore primarily rooted in its ability to accommodate a diversity of preferences that correlate with geographic location. Yet, federalist systems also allow for preemptions, which occur when a higher level of government restricts autonomy in some policy space for a lower level of government. Notable examples of preemptions include state restrictions on local minimum wage laws, occupational licensing requirements, transgender public restrooms, gun control initiatives, or fracking activity. While democratically proper constitutional orders may motivate preemptions for purposes of circumventing prisoners' dilemma-style races-to-the-bottom, equity concerns, and avoiding administrative duplication, many preemptions simply seem to reflect an exercise in ideological preference of a statewide majority over rival pockets of political communities. Vigdor (2004) dubbed this expansion of voter franchise into other communities "the non-resident voter hypothesis" and provides some empirical support for it in voter pattern data in Massachusetts Proposition 2 ½. In a recent review of cases involving state preemption of local governments, Riverstone-Newell (2017) describes these instances being driven as diverging ideologies between conservative legislatures and progressive cities. Swanson and Barrilleaux (2018) similarly conclude that state court ideology is an important determinant in ruling against local laws. In summary, while normative economic theory often implies that local autonomy in public policy should be correlated with a spatial distribution of ideological preferences, the emerging positive political economy theory of local control seems to work against that direction.

The purpose of this paper is to test whether the ideological gap between cities and their encompassing statewide majority explains the public finance responses to state-on-local Tax and Expenditure Limits (TELs), a class of preemptions that bind taxing powers and revenue growth in local governments. In other words, is the degree to which TELs tend to restrict the public finances of a given local government driven by how much more liberal that political subdivision is than its encompassing state?

TELs are likely the most studied policies in the preemption literature. The previous research has not definitively determined that TELs succeed in constraining government size as measured by spending, but they do indicate substantive influences on their fiscal decisions and financial management. Some of this literature has alluded to an ideological disconnect as an explanation for the seemingly disparate impacts TELs appear to have on urban areas relative to rural. Indeed, Vigdor's (2004) non-resident voter hypothesis was born on the study of TELs as a motive for their spread, but no paper to our knowledge has established whether these policies succeed in binding the behavior of local governments according to ideological differences. It is our intention to move the idea that heterogeneous responses to TELs can be traced to sub-state divisions in political ideology to the center of the analysis because the implications are significant for broader public administration concerns in a federalist system. Increasing political polarization is a well-documented phenomenon in the literature, as is the proclivity for households to engage in political homophily in social relationships. This has extended to the community level, where localities are increasingly sorted not just according to preferences for traditional local policy, but also policy at the national level since at least the 1970s (Bishop, 2008; Sussel, 2013; Morrill & Webster, 2015; Mummolo & Nall, 2016). Consequently, interactions and contests between higher and lower levels of government will need to be increasingly understood in an ideologically federalist perspective. For example, the treatment of sanctuary cities for undocumented immigrants by the U.S. federal government should probably not be understood strictly in terms of a constitutional conflict over authority, but also as a time-varying conflict between the ideological alignment between those cities and the controlling political parties of the federal government.

To preview our research design and findings, we use voting in presidential elections as a signal of ideological differences between a given locality and the state as a whole. This does not rely on the claim that a state or locality where 60 percent voted for Nixon in 1972 is as conservative as one that voted by the same margin for McCain in 2012, but rather that a locality what went 70% for Nixon in a state that only went 55% in his favor was in some sense 15 percentage points more conservative than their encompassing state and that this can be considered comparable to similar margins in later presidential elections. This data replicates other major trends, particularly with regards to the well-documented propensity towards political homophily in migration patterns, but we also demonstrate that these trends do not appear to be good predictors of state-on-local TELs. For our main findings, in a balanced panel of 147 city governments from 1977 to 2015 we find rather modest support for the conjecture that they were more binding for local governments that were more liberal than their encompassing state. This is to say that statistical significance is sensitive to model specification and choice of controls variables. Furthermore, the effects seem to be driven by cities that are more liberal than their states but only rather modestly so.

The next section overviews relevant background on TELs for the purposes of this paper. This includes 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 the 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 taxplayer'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.[1]

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.[2] 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) restrictions on both the property tax rate and the base of assessed property values.

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

[1] We do not use the Amiel, Deller, and Stallman (2009) stringency index for this paper. First and foremost, this index would presumably be partially capturing the mechanism through which the ideological wedge between state and localities operates, and we do not want "at a given TEL stringency how binding is the TEL along ideological lines." An additional practical complication is that the index is essentially a count of the number of levers used in the TEL mechanism, but each may operate non-linearly, setting up too many potential three-way interactions for possible inference in a panel of 131 cities.

[2] 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).

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 is omitted from our study due to its use of election districts that differ from Census enumeration districts to tally votes.

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 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 and Ideology

The motivation for these limitations is 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.

While the main research question seeks to determine whether there is heterogeneity in the effect of TELs on public finances, the fact that there is potential theoretical justification for the possibility that the documented increase in political geographic sorting is actually a causal factor in the adoption of state-and-local TELs. That is, it is possible under these theories that states with more political sorting became more likely to adopt TELs. This is not necessarily an endogeneity problem for our research question, as the TEL adoption may still be considered exogenous to the individual locality. Furthermore, TEL restrictiveness might be determined by a statewide median voter and consequently have heterogeneous impacts on disproportionately liberal communities regardless of their intent. Nevertheless, we examine briefly the connection between trends in geographic sorting by political ideology among the states and the timing of their TEL adoption.

To explore the degree of geographic sorting by state over numerous decades, we follow Johnson, Manley, and Jones (2016) by using county level data on presidential votes, which is the only broad sub-state data with enough historical coverage to be inclusive of the adoption of many of the state TELs. Our measure of sorting is based on the share of the local population of eligible voters that voted for the Democratic candidate for president, with non-election years imputed from a linear interpolation of the surrounding presidential elections. We then use this county-level data to construct a state-level descriptive statistic of the degree to which political sorting is represented within each state, which we refer to as the spatial Gini coefficient.[3] The implication of this calculation is that a state where all counties voted in some equal proportion for the Democratic candidate would have a spatial Gini coefficient of zero; whereas if all counties possessed zero votes for the Democratic candidate except one (so that all the state's Democrats were located in single county) the Gini coefficient would equal one. To illustrate, **Figure 2** provides a map of Texas counties for 1972 and 2016 according to the share of the county that voted for the democratic candidates as well as their corresponding spatial Gini coefficient. Consistent with Johnson et al. (2016) for this data and most of the related literature on political sorting, our approach documents a trend of increased spatial sorting across the states that can be observed in **Figure 3.**

Figure 4 takes the cases of states which adopted a state-on-local TEL and centers the time-series of Gini coefficients around the adoption year as an event analysis. The figure shows that there is no clear trend in the spatial Gini coefficients among the states prior to adopting a TEL, but after adopting TELs there tends to be increases among the Gini coefficients. Consequently, the pattern is not consistent with an increase in political sorting causing the creation of TELs unless it was somehow anticipatory of future political sorting. Of course, the pattern is consistent with TELs causing political sorting, but it is far from clear what theoretical mechanism would cause this. It is more likely an artifact of the timing when TELs were widely adopted in the context of the larger political sorting that has occurred in the nation.

To take one further step and consider a more formal investigation than **Figure 4**, we employ a Cox (1972) survival analysis duration model. Doing so allows us to consider states that have never adopted a TEL, and allow for us to control for socioeconomic variables that might be correlated with TEL adoption that might also be correlated with political sorting, namely growth in state population and personal income. A survival analysis duration model estimates the contributions of covariates in explaining a state's likelihood of adopting a TEL given that it does not have one at the beginning of our study period. States are said to be "at risk" of adopting a TEL until it "fails" and adopts a TEL, at which point it exits the analysis. Three states that have their TELs in 1972 are excluded, and states that do not adopt a TEL at any time during the period

[3] We use Deaton's (1997) formula for calculating the Gini coefficient.

become right-censored after 2016, leaving up to 45 time periods at which states can potentially be "at risk" of adopting a TEL. Three states repealed their TELs during the study period, and do not re-enter the analysis, even though they could conceivably re-enact their TEL at a later date.

Appendix B provides a fuller treatment of the duration model results, but geographic polarization does not raise the risk of accelerating the adoption of a state-on-local TEL. Instead, population and economic growth seem to be much stronger predictors, which is more consistent with the Nechyba (1997) model of the state-as-insurance than the various spatial ideological sorting.

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 Borcherding 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:

$$(1)\ lnY_{it} = \delta V_{it} + \rho lnT_{it} + \lambda lnN_{it} + \theta lnI_{it} + \varepsilon_{it}.$$

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 (ε_{it}). We use a balanced panel of 147 city general purpose governments from 1977 to 2015. These are local governments whose public finance information is from the U.S. Census of Governments is available for most of the state-on-local TELs that have been in effect over the last several decades (see **Figure 1)**. The budget allocation outcomes we adopt include the property tax levy, total general expenditures, and total expenditures on current operations.[4] 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 1** 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 **X**, add state (θ_i) and year (τ_t) fixed effects, 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:

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

The empirical model specified in equation (2) regards α 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.[5] The main hypothesis is that the distortion is heterogeneous in the effect of TEL according to the difference between the state and local median voter. Using *DIFF* to represent this ideological difference the specification of interest becomes

$$(3)\ lnY_{it} = \alpha_1 TEL_{it} + \alpha_2 DIFF_{it} + \gamma(TEL_{it} \times DIFF_{it}) + \beta X_{it} + \theta_i + \tau_t + \varepsilon_{it}.$$

As before, **X** in equation (3) includes an independent control for the local share voting for the democratic president along with other median voter controls and the specification includes city and year fixed effects. The coefficient of interest is γ on the interaction between TEL and DIFF, with *DIFF* representing an indicator for cases where share of the local population voting for the democratic presidential candidate exceeds that of the state. The variation driving the effect of interest is to observe how the outcome changes in cities that went from becoming less liberal to more liberal than their encompassing differed between states that adopt or suspend a TEL to

[4] 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.
[5] Appendix Table C1 and C2 provides the results of estimating equations (1) and (2) for the reader's reference.

those that always or never enact a TEL. It is hypothesized that γ will be negative so as to indicate that as a locality becomes more progressive relative to the statewide population, the TEL becomes increasingly constraining on preferences for higher spending.

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

IV. RESULTS

Table 2 reports the results of estimating equation (3) for the variable of interest across the three specifications. Robust standard errors clustered by city are reported in parentheses, and the specifications alter in the inclusion of control variables described in the previous section and state-by-year fixed effects. The full results are provided in **Appendix Tables C3-C5**.

Generally, the coefficients presented in **Table 2** are consistent with the expectation that the TEL is more binding for localities that become more liberal than the rest of the state, but there is sensitivity to the choice of expenditure concept and model specification. For total expenditures, the sings range to indicate that the state becoming more conservative than the city and adopting a TEL results in a reduction in total expenditures ranging from 1.78 to 4.08 percent. However, at a five percent confidence level the only statistically significant effect size is in the specification with no controls beyond city and year fixed effects. In the case of total current expenditures, the range of point estimates is similar, but the statistical significance holds up to the inclusion of control variables. With controls, city, and year fixed effects, cities whose states become more conservative than themselves and adopt a TEL see reductions in their total current expenditures of approximately 3.4 percent. Again, the difference in these two concepts is the inclusion of capital expenditures. When turning to property tax revenues, which at the margin can determine both capital and current expenditures, the sign of the effect is sensitive to the inclusion of state-by-year fixed effects and none of the findings are statistically significant. As seen in **Appendix Table C5**, the TEL coefficient has the largest effect on property tax revenues, but the findings of **Table 2** are not clearly supportive of heterogeneity according to ideological differences between the state and city.

To further explore this heterogeneity, we add to the specification an indicator variable for cases where the city votes for the democratic candidate by at least a 10 percent margin over the statewide vote share. This indicates additional within variation for cities that went from any margin to a 10 point margin relative to the state. The effect of TELs on cities that went from below the statewide support for democrats to favoring the democratic candidate by 10 percent more than the state would be inferred from adding the two effects. These results are presented in **Table 3** for the variables-of-interest while the full results are found in **Appendix Table C6-C8**. Broadly the results are similar in terms of their sensitivity to model specification and expenditure concept as in the main results. Including controls, total expenditures decrease by 3.00 percent in TEL states among those cities that become more liberal than their encompassing state, but there is no penalty among those that move beyond the 10 percent margin. When state-by-year fixed effects are included, the effect declines for any margin but reappears in the 10 point margin as a 3.75 percent reduction in total expenditures that is statistically significant at the five percent level. Similarly for total current expenditures, the results in **Table 3** show a 3.66 percent decrease in TEL adoption for those any more liberal than the state, an effect that is statistically significant at the five percent level. Including, state-by-year fixed effects again pushes this effect to those cities with at least a 10 point vote margin in the range of 2.37 to 2.84 percent reduction, but statistical significance is only around the ten percent level. The property tax revenue results remain mixed as in the main effects.

A variety of other robustness checks were performed, but presented even less support for the hypothesis. One set employed growth regressions instead of the levels described in equation (3), but none of these findings were statistically significant on any TEL related variables. This suggests that TELs potentially affect city growth in shifting the base level, but do not alter the trajectory of government growth. In another analysis, the sample was split by quintile on the degree of partisan divide in 1972 to compare the coefficient on TEL across these subsamples. Those results demonstrated extremely similar point estimates for the TEL, but each quintile had less than 30 cities so no effect was statistically significant. Nevertheless, whether the city was most or least liberal relative to the state in 1972, the TEL did not seem to differentially impact their fiscal outcomes.

V. CONCLUSION

Federalism remains an important battleground for political contestation around the world and will likely exacerbate if people continue to sort into communities according to their preferences over global policy. Ideology almost certainly plays a role in many cases where states preempt local autonomy, including local fiscal powers. Indeed, a conventional view of state-on-local Tax Expenditure Limits is that they are a mechanism for voters to expand their franchise over communities where they do not reside. However, using a panel of 147 cities from 1977 to 2015, this paper finds only limited support for these policies as having heterogeneous impacts on public expenditures. That is, while these policies are generally binding on average, the statistical significance of how binding they are based on how much ideological distance there is between the locale and the state is rather sensitive to model specification.

In addition to being the first paper to directly investigate this form of important heterogeneity, this paper provided three novel components of the analysis that will warrant further research. First, we measure ideological distance between the state and locality using county presidential vote data. Ideology is obviously a more nuanced phenomena than is expressed in voting differences, but alternative measures at congressional district level are altered by changes in gerrymandering and have little correspondence to local borders. While our measure is largely time-invariant in the geographic size and shape, county borders are imperfect proxies of city borders and presidential votes require interpolation between elections, so this variable comes with measurement error as well. Secondly, this paper represents an unusually long panel of city government public finance data. This is necessary for picking up variation in TEL adoption, but also requires we focus on some of the most aggregate measures of public finance outcomes, causing us to overlook potentially important fiscal behavior, including the off-loading of general purpose government functions onto special districts, a well-known strategy cities have used to circumvent TELs. Finally, in reviewing the previous literature, we saw discrepancies across authors in the coding of state-on-local TELs adopted in the literature and duplicate references to TELs in states that we could not verify in state statutes. We coded measure of a binding TEL according to our independent review of state statutes rather than transcribing this coding, but that different authors have reached different conclusions over the categorization suggests there is potential disagreement over the appropriate recognition of a binding TEL, albeit the particulars of a given research question matter greatly in choices.

REFERENCES

Anderson, Nathan B. (2006) Property Tax Limitations: An Interpretive Review. National Tax Journal Vol. LIX, No.3, 685-694.

Bergstrom, Theodore C, and Robert P. Goodman. 1973. “Private demand for public goods.” *American Economic Review* 63(3): 280-96.

Borcherding, Thomas E., and Robert T. Deacon. 1972. “The Demand for Services of Nonfederal Governments.” *American Economic Review* 62 (Dec.): 891-901.

Box-Steffensmeier, J., & Jones, B. (2004). *Event History Modeling*. New York: Cambridge University Press, cited Hill, E., St. Clair, T., Wial, H., Wolman, H., Atkins, P., Blumenthal, P., Friedhoff, A. (2010). “Economic Shocks and Regional Economic Resilience.” Brookings, George Washington, Urban Institute, Building Resilient Region Project. Washington, DC

Brien, Spencer T. 2018. “Compensating Changes to the Property Tax Levy? An Empirical Test of the Residual Rule.” *Public Finance Review* 46(6): 949-73.

Deaton, Angus. 1997. *Analysis of Household Surveys.* Baltimore MD: John Hopkins University Press.

Johnson, Ron, David Manley, and Kelvyn Jones. 2016. “Spatial Polarization of Presidential Voting in the United States, 1992: The “Big Sort” Revisited.” *Annals of the American Association of Geographers* 106(5): 1047-62.

Lowery, D., & Sigelman, L. 1981. Understanding the Tax Revolt: Eight Explanations. The *American Political Science Review*, 75(4), 963-974.

Mikesell, John L. 2018. *Fiscal Administration, 10th Edition*. Cengage Learning: Boston, MA.

Mullins, Daniel R. and Bruce A. Wallin. 2004. “Tax and Expenditure Limitations: Introduction and Overview.” *Public Budgeting & Finance* 24(4): 2-15.

Nechyba, Thomas J. 1997. “Local Property and State Income Taxes: The Role of Interjurisdictional Competition and Collusion.” *Journal of Political Economy* 105(2): 351-84.

Riverstone-Newell, Lori. 2017. “The Rise of State Preemption Laws in Response to Local Policy Innovation.” *Publius: The Journal of Federalism* 47(3): 403-425.

Ross, Justin M. and Sian Mughan. 2018. “The Effect of Fiscal Illusion on Public Sector Financial Management: Evidence from Local Governmen

Ross, Justin M. and Wenli Yan. 2013. “Fiscal Illusion from Property Reassessment? An Empirical Test of the Residual View.” *National Tax Journal* 66(1): 7-32.

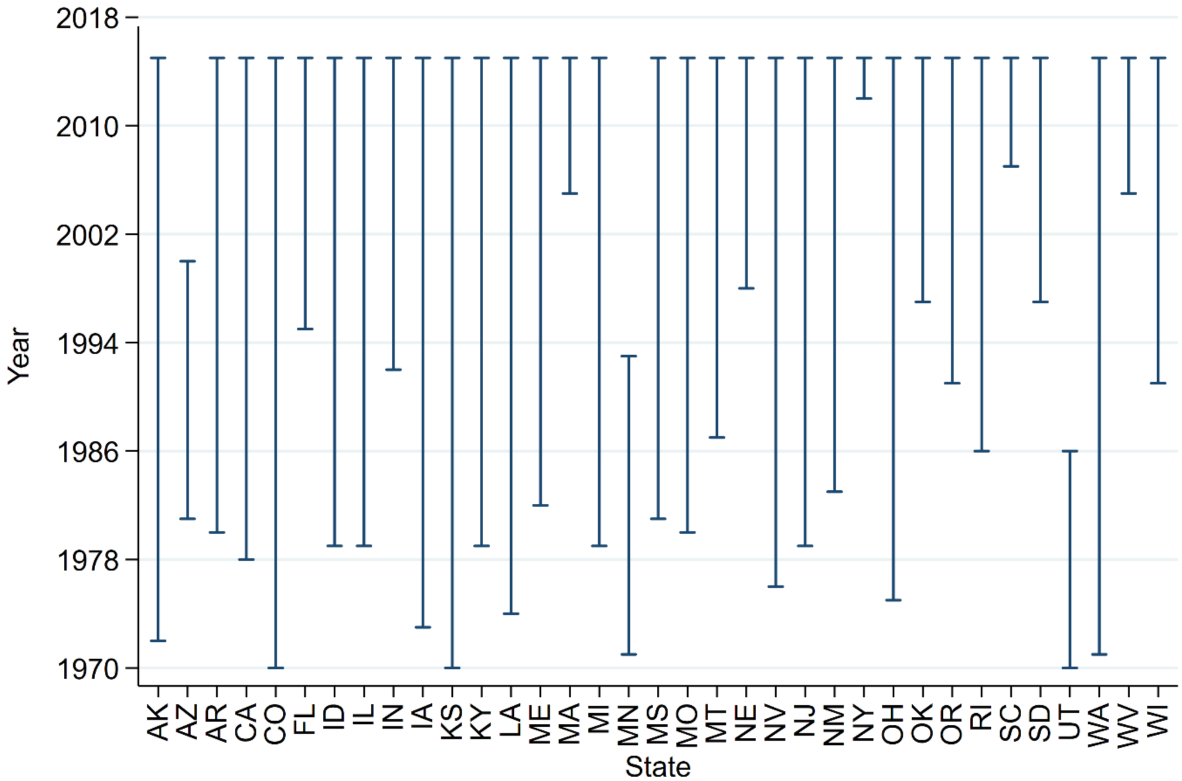
Seljan, E. (2014) “The limits of tax and expenditure limits: TEL implementation as a principal-agent problem.” Public Choice 159:485-501.

Swanson, Jeffrey and Charles Barrilleaux. 2018. “State Government Preemption of Local Government Decisions Through the State Courts.” *Urban Affairs Review* https://doi.org/10.1177/1078087418783273.

Vigdor, J. (2004) Other People’s Taxes: Nonresident Voters and Statewide Limitation of Local Government. Journal of Law and Economics, vol. XLVII. P. 453-475.

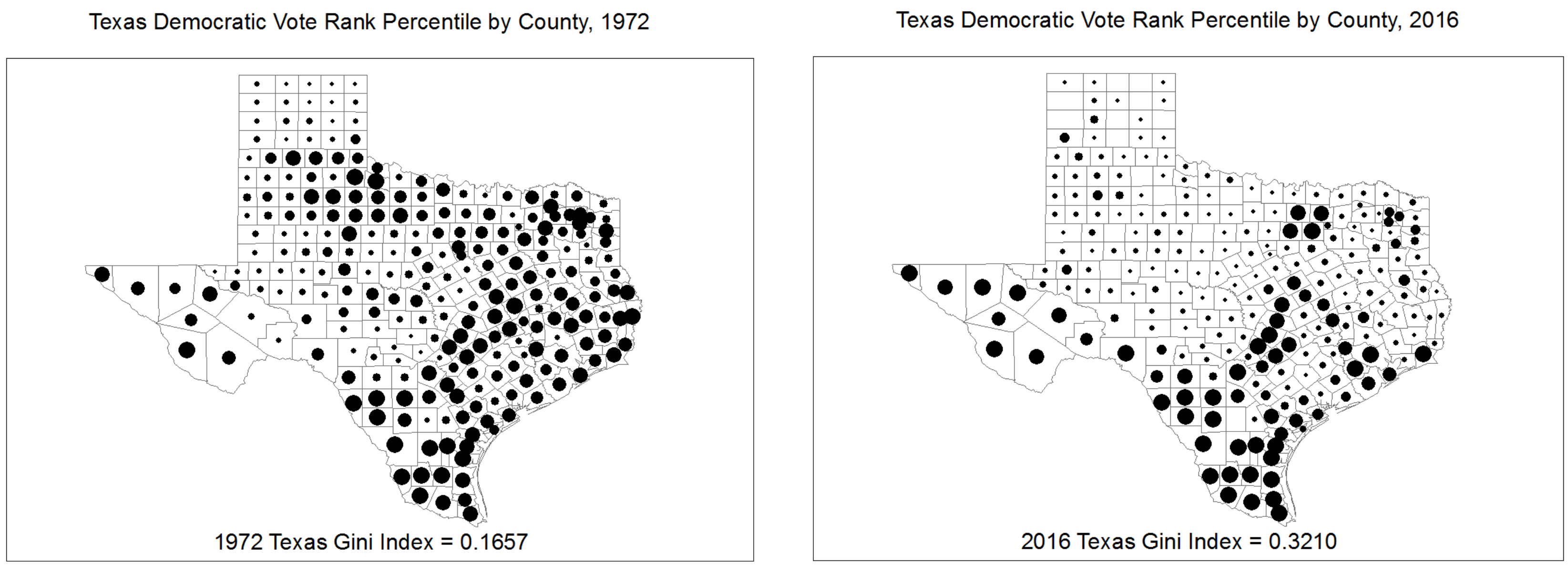
Yang, Kate (2017) “Financial management conservatism under constraints: tax and expenditure limits and local deficit financing during the Great Recession” Local Government Studies https://doi.org/10.1080/03003930.2017.1365711

Figure 1: 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.

Figure 2: County Vote Share for Democratic Presidential Candidate and Spatial Gini Coefficient, Texas 1972 vs. Texas 2016



Notes: Large size dot indicates larger share of voter support for democratic candidate for president. Authors' compilation using data from CQ Voting & Elections Collection.

Figure 3: Trend in Spatial Gini Coefficients Among the States, 1972-2016

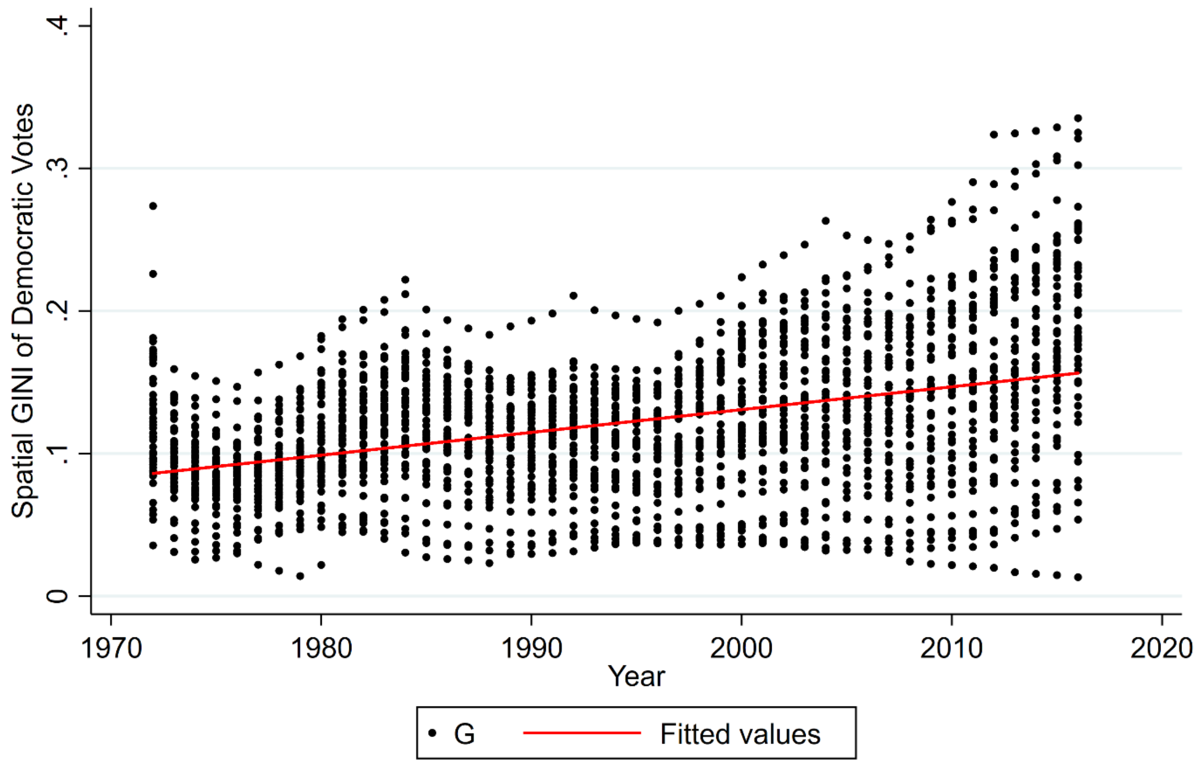


Figure 4: Gini Coefficient of Democratic Vote Share in States Pre and Post TEL Adoption

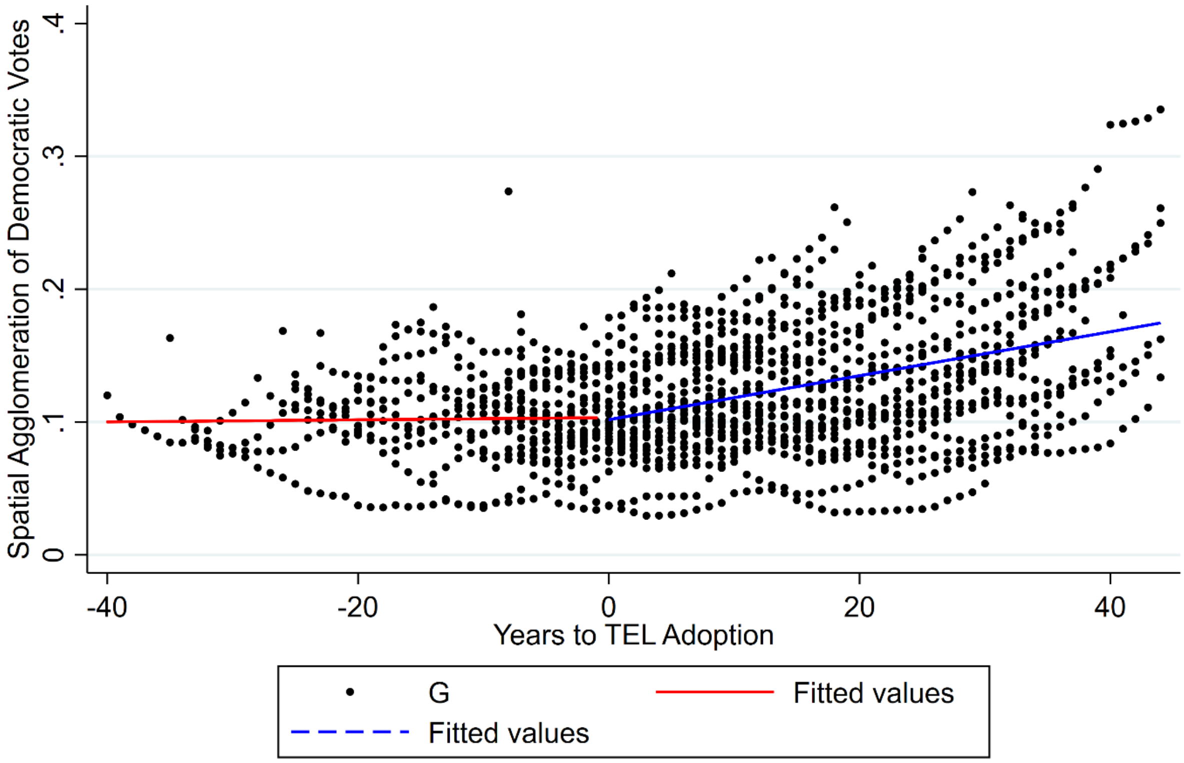


Figure 5: Histogram of difference between local and state share of voters for democratic president

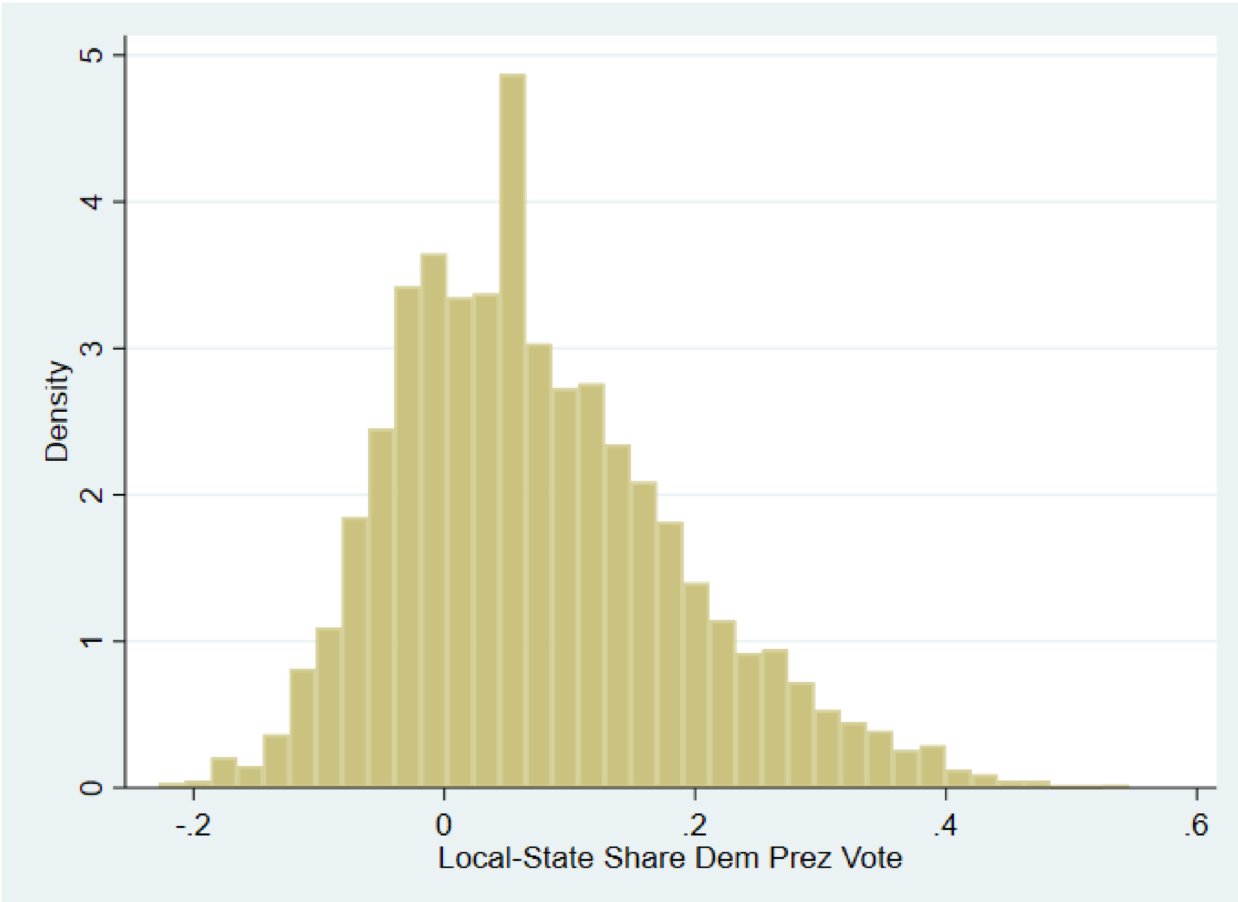


Table1: Variable Sources, Definitions, and Summary Statistics

Variable	Mean	Std. Dev	Min	Max
ln(Total Expenditures)[1]	20.05	1.31	15.75	25.84
ln(Total Current Expenditures)[1]	19.93	1.32	15.65	25.76
ln(Property Tax Revenues)[1]	18.34	1.63	0	24.49
TEL[2]	0.53	0.50	0	1
Share Dem[3]	0.49	0.12	0.10	0.87
DIFF	0.72	0.45	0	1
TEL x DIFF	0.41	0.49	0	1
DIFF + 10	0.36	0.48	0	1
(TEL x DIFF + 10)	0.22	0.41	0	1
ln(PCPI)[4]	10.74	0.55	9.21	12.63
ln(Population)[1]	13.83	1.07	10.90	16.82
ln(Intergovernmental Revenue)[1]	19.16	2.33	-18.71	25.58
Share of Employment, Agriculture[4]	0.01	0.02	0.00	0.13
Share of Employment, Manufacturing[4]	0.11	0.06	0.01	0.40

Sample size: 139 city governments observed every year from 1977 to 2015 for a total sample size of 5,681.
Notes: ln() indicates the inverse hyperbolic sine function transformation.
Variable Definitions: Total Expenditures is the city government's total governmental expenditures; Total Current Expenditures: city government's total expenditures on current operations; Property Tax Revenues: is total revenues from property taxation. TEL: A dummy variable to indicate that a binding state-on-local tax expenditure limitation is in effect; Share Dem: is the share of the eligible voters in the county that voted for the democratic candidate for president, with non-election years linearly interpolated between the surrounding two elections. DIFF: A dummy variable to indicate that Share Dem in the locality is greater than that of the state. PCPI is per capita personal income; Intergovernmental Revenue: city government revenues from state or federal sources; Share of Employment, Agriculture/Manufacturing: countywide employment in agriculture/manufacturing divided by total employment.

Sources: (1) U.S. Census Bureau Government Finance Statistics; (2) Authors compilation or calculation; (3) CQ Press Voting and Elections Collection; (4) Bureau of Economic Analysis

Table 2: Regression Estimates by Dependent Variable

	ln(Total Expenditures)			
(TEL x DIFF)	-0.0408**	-0.0178	-0.0240	-0.0217
	(0.0196)	(0.0238)	(0.0170)	(0.0229)
	ln(Total Current Expenditures)			
(TEL x DIFF)	-0.0490***	-0.00965	-0.0337**	-0.0115
	(0.0167)	(0.0193)	(0.0137)	(0.0183)
	ln(Property Tax Revenues)			
(TEL x DIFF)	-0.0284	0.0357	-0.0137	0.0275
	(0.0716)	(0.0600)	(0.0713)	(0.0599)
X Variables	No	No	Yes	Yes
State x Year Fixed Effects	No	Yes	No	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
City Fixed Effects	Yes	Yes	Yes	Yes
N	5681	5681	5681	5681
Number of Cities	147	147	147	147

Notes: All specifications include controls for TEL, DIFF, and DEM. Robust standard errors clustered by city are reported in parentheses. Statistical significance indicated with * (p<0.10), ** (p<0.05), *** (p<0.01). Full results appear in appendix Table C3-C5.

Table 3: Regression Results by Dependent Variable

	ln(Total Expenditures)			
(TEL x DIFF)	-0.0363*	-0.0142	-0.0300*	-0.0182
	(0.0202)	(0.0237)	(0.0175)	(0.0229)
(TEL x DIFF + 10)	-0.00777	-0.0475**	0.0138	-0.0375**
	(0.0162)	(0.0190)	(0.0140)	(0.0178)
	ln(Total Current Expenditures)			
(TEL x DIFF)	-0.0429**	-0.00790	-0.0366**	-0.00957
	(0.0172)	(0.0193)	(0.0144)	(0.0183)
(TEL x DIFF + 10)	-0.0106	-0.0284*	0.00738	-0.0237
	(0.0140)	(0.0171)	(0.0116)	(0.0156)
	ln(Property Tax Revenues)			
(TEL x DIFF)	-0.0230	0.0406	-0.0151	0.0309
	(0.0765)	(0.0610)	(0.0761)	(0.0610)
(TEL x DIFF +10)	0.0121	-0.0623	0.0224	-0.0430
	(0.0335)	(0.0433)	(0.0313)	(0.0412)
X Variables	No	No	Yes	Yes
State x Year Fixed Effects	No	Yes	No	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
City Fixed Effects	Yes	Yes	Yes	Yes
N	5681	5681	5681	5681
Number of Cities	147	147	147	147

Notes: All specifications include controls for TEL, DIFF, and DEM. Robust standard errors clustered by city are reported in parentheses. Statistical significance indicated with * ($p<0.10$), ** ($p<0.05$), *** ($p<0.01$). Full results appear in appendix Table C6-C8.

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	Arizona Legislature Historical Property Tax Changes
Arkansas	1981-2000	Amendment 79 Assessor Guide
California	>=1978	California Legislative Analyst
Colorado	>=1913	CO Department of Local Affairs, Colorado Legislature
Connecticut	0	Connecticut General Assembly
Delaware	0	
Florida	>=1995	Florida Senate, Florida Department of Revenue, Fla. Stat. 193.1554, 1555
Georgia	0	
Hawaii	0	
Idaho	>=1979	Idaho State Tax Commission, Source 2
Illinois	>=1995	Illinois Department of Revenue (note that TEL does not have statewide applicability)
Indiana	>=1973	Indiana University Public Policy Institute
Iowa	>=1979	Iowa Department of Revenue, Iowa Legislature
Kansas	>=1970	Wichita State University, Kansas Department of Revenue, Kansas Legislature
Kentucky	>=1979	KRS § 160.470
Louisiana	>=1974	
Maine	>=2005	Maine Legislature
Maryland	0	
Massachusetts	>=1982	Massachusetts Department of Revenue
Michigan	>=1979	Headlee Amendment Article IX §25-33 Mich. Constitution
Minnesota	1971-1993	Minnesota House of Representatives
Mississippi	>=1980	Miss Code § 27-39-320
Missouri	>=1981	Article X, § 16-24 MO Constitution
Montana	>=1987	Initiative I-105, Montana Legislature
Nebraska	>=1998	Neb. Rev. Stat. §77-3442, Nebraska Legislative Fiscal Office
Nevada	>=1983	SB27 (1983), Nevada Tax Commission, NRS 361.471 thru 361.4735
New Hampshire	0	
New Jersey	>=1976	N.J.S.A. 40A:4-45.44, NJ Division of Local Government Services
New Mexico	>=1979	NMSA §7-37-7.1, New Mexico Department of Finance and Administration
New York	>=2012	New York Department of Taxation and Finance
North Carolina	0	NC Legislature
North Dakota	0	North Dakota Legislature
Ohio	>=1975	Sec. 2, Art. XII, Ohio Const.

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

Appendix B: Hazard Model of TEL Adoption

As described in section 2, we use a Cox (1972) nonparametric survival time estimation model, which does not require us to specify a distribution for the baseline hazard.[6] The generic form of the Cox hazard function used in this model is:

$$h(t_j) = h_0(t)g(\mathbf{x}_j)$$

where $h_0(t)$ is the baseline distribution left unspecified and $g(\mathbf{x}_j)$ is the vector of covariates. The model estimates hazard ratios for the individual covariates (see Box-Steffensmeier and Jones, 2004). A hazard ratio of 1.0 suggests that a one-unit increase in a variable does not change the risk of experiencing the event in question, conditional that it hasn't already occurred. A hazard ratio of 2.0 suggests that a one-unit increase in a variable doubles the risk of experiencing the event, conditional on that it hasn't already occurred. If the ratio is less than one, then an increase in the independent variable reduces the probability that a state adopts a TEL. The reduction in probability is 1 – hazard ratio = reduction in probability of adoption a TEL in year t.

In all three specifications, the annual change in the Gini Index is not statistically different from 1, which means it has no significant influence on the "risk" of state i adopting a TEL in year t given it had not previously done so.

TABLE B1: Hazard Ratios for Three Cox Models

	(1) analysis time when record ends	(2) analysis time when record ends	(3) analysis time when record ends
Growth in Gini Index	0.995	0.952	0.972
	(-0.00)	(-0.03)	(-0.01)
Mean Share of Dem Vote		2.648	0.918
		(0.45)	(-0.03)
Western state=1		2.254	3.540***
		(1.62)	(2.77)

[6] We believe the Cox (1972) nonparametric approach is the most appropriate technique to address our research question given an absence of theory on the distribution of TEL adoption and consistent with the argument of Box-Steffensmeier & Jones (2004) regarding the Cox nonparametric approach having more flexible assumptions about the data generating process.

ln(population)			27.21** (2.22)
ln(real personal income)			0.0512** (-2.09)
Observations	1093	1093	1093

The results of Model 3 above yield the most significant results: ln(population) ($p = 0.027$) and real personal income growth ($p = 0.036$). The dummy variable "West" equals unity for states along the West Coast and mountain states such as Idaho, Colorado, Arizona, Utah, Arizona, and New Mexico. The motivation for this variable is from Sokolow (2000), who observed, ". . . state-imposed property tax limitations are more widespread and more severe here than in other regions of the United States." The "West" variable is significant at the 0.6% level, and in the third model, it suggests that western states are about 3.5 times "greater risk" of adopting a TEL.. The remaining variables, annual growth in the Gini Coefficient and the mean Democratic vote, are not statistically significant, and may suffer from multicollinearity as well. Their hazard ratios are not statistically different from 1.

Some of the hazard ratios do not yield intuitive interpretations. For example, it is hard to fathom that a 1% increase in population results in 27 times greater risk of adopting a TEL. However, it may be possible that states with TELS happen to have grown the fastest, such as California, Arizona, and Colorado, and may be putting upward pressure on that hazard ratio. Likewise, the hazard ratio for the log of personal income does not seem intuitive: Its interpretation is a 1% increase in real personal income implies about a 95% reduction in probability of adopting a TEL.

The standard errors are robust to 44 clusters at the state level.

Tests of the proportional hazard assumption are run after model estimation and are used to determine how well-specified the models are. We use Stata's link test in which the following model is estimated:

$$LRH = \beta_1(\mathbf{x}\hat{\beta}_x) + \beta_2(\mathbf{x}\hat{\beta}_x)^2$$

Under the assumption that $\mathbf{x}\hat{\beta}_x$ is properly specified in the Cox models above, we test that $\beta_1 = 1$ and $\beta_2 = 0$. The results for the three models are below:

Table B2: Test of Proportional Hazards Assumption

Model	β_1 Coefficient	β_2 Coefficient	Result
1	-332.1148 ($p = 0.688$)	-1090773 ($p = 0.305$)	Fails test of PH assumption
2	2.776 ($p = 0.46$)	-1.159 ($p = -0.30$)	Fails test of PH assumption
3	$1.025, p = 0.006$	$.0208, p = 0.719$	Best-fit model

The results of the proportional hazards test for Model 3 show that β_1 coefficient is close to 1, statistically different from zero, and β_2 is not statistically different from zero, which indicates it is the best-specified model.

Figure B1: Meier-Kaplan Survival Probability

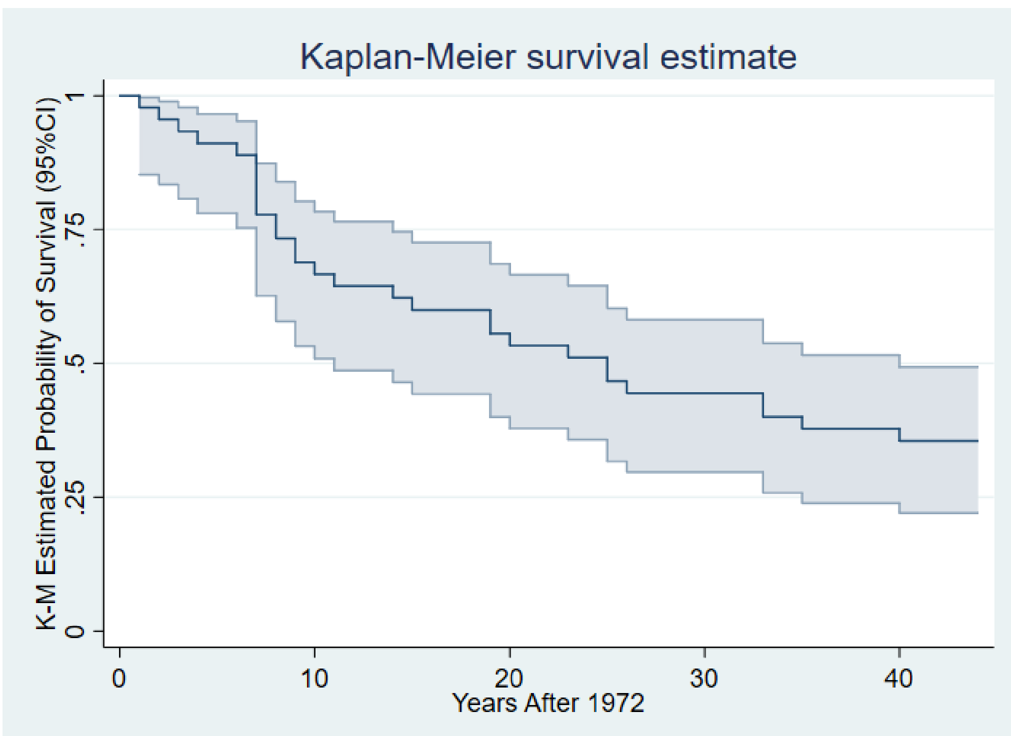


Figure B1 shows the Kaplan-Meier (1958) estimate of probability that state i survives past period t conditional on having survived (not adopted a TEL) through period t, where t is measured in years after 1972. The most severe drop in the probability of survival is evident in the late 1970s, especially in the year after California adopted Proposition 13. Between 1978 and 1979, the probability of a state not adopting a TEL in the given year fell from 88.9% to 77.8%, which coincides with the peak of the late 1970s tax revolts. It is also worth noting that the standard error of the survivor estimates increases through time. Below, Table X provides some estimates of the survival function during the tax revolt era:

Table B3: Numerical estimates of Kaplan-Meier Survival Function

Time	Beg. Total	Fail	Survivor Function	Std. Error
1973	45	1	0.978	0.022
1974	44	1	0.956	0.031
1975	43	1	0.933	0.037
1977	42	1	0.911	0.042
1978	41	1	0.889	0.047
1979	**40**	**5**	**0.778**	**0.062**
1980	35	2	0.733	0.066
1981	33	2	0.689	0.069

Figure X below provides further visualization of the rate at which TELs diffused across states, as well as the three states that repealed their TELs and are therefore censored from our analysis. In 1970, only three states bound their cities with a TEL that fit our definition of binding.

Appendix C: Supplemental Regressions

Table C1: Regression Results for Equation (1)

	ln(Property Tax Revenues)	**ln(Total Expenditures)**	**ln(Current Expenditures)**
Share Dem Prez Vote	3.973***	2.704***	2.471***
	(0.188)	(0.130)	(0.142)
ln(Per Capita Personal Income)	1.648***	0.957***	0.756***
	(0.0869)	(0.0742)	(0.0770)
ln(Population)	0.307***	0.441***	0.440***
	(0.0313)	(0.0240)	(0.0262)
ln(Non-Own Source Revenues)	0.123***	0.152***	0.180***
	(0.0277)	(0.0310)	(0.0357)
Farm Share Emp. Share	-3.143***	-2.876***	-4.007***
	(0.747)	(0.658)	(0.610)
Manufacturing Share Emp. Share	0.589**	-1.805***	-1.821***
	(0.276)	(0.180)	(0.184)

Notes: Sample size of 5,727 in all specifications. Robust standard errors clustered reported in parentheses. Statistical significance indicated with * (p<0.10), ** (p<0.05), *** (p<0.01).

Table C2: Regression Results for Equation (2)

	ln(Property Tax Revenues)	ln(Total Expenditures)	ln(Current Expenditures)
State-on-Local TEL	-0.378***	0.00405	-0.0889***
	(0.0281)	(0.0203)	(0.0203)
Share Dem Prez Vote	4.101***	2.703***	2.501***
	(0.186)	(0.132)	(0.143)
ln(Per Capita Personal Income)	1.495***	0.959***	0.720***
	(0.0834)	(0.0727)	(0.0753)
ln(Population)	0.351***	0.441***	0.450***
	(0.0303)	(0.0248)	(0.0270)
ln(Non-Own Source Revenues)	0.117***	0.152***	0.178***
	(0.0268)	(0.0312)	(0.0356)
Farm Share Emp. Share	-1.046	-2.899***	-3.513***
	(0.743)	(0.661)	(0.606)
Manufacturing Share Emp. Share	0.863***	-1.808***	-1.756***
	(0.271)	(0.177)	(0.180)

Notes: Sample size of 5,727 in all specifications. Robust standard errors clustered reported in parentheses. Statistical significance indicated with * ($p<0.10$), ** ($p<0.05$), *** ($p<0.01$).

Table C3: Full Results for Total Expenditure Results in Table 2

	ln(Total Expenditures)			
(TEL x DIFF)	-0.0408**	-0.0178	-0.0240	-0.0217
	(0.0196)	(0.0238)	(0.0170)	(0.0229)
TEL	0.0172		-0.00519	
	(0.0214)		(0.0191)	
DIFF	0.0130	-0.00501	-0.0447***	-0.0468**
	(0.0134)	(0.0195)	(0.0116)	(0.0183)
Share Dem Prez Vote	0.160*	-0.456***	0.288***	-0.176
	(0.0848)	(0.110)	(0.0789)	(0.111)
ln(Per Capita Personal Income)			0.0586	0.0372
			(0.0368)	(0.0505)
ln(Population)			0.825***	0.679***
			(0.0263)	(0.0417)
ln(Non-Own Source Revenues)			0.00886***	0.00754***
			(0.00211)	(0.00207)
Farm Share Emp. Share			1.923***	3.192***
			(0.566)	(0.661)
Manufacturing Share Emp. Share			0.653***	0.676***
			(0.126)	(0.179)
State x Year Fixed Effects	No	Yes	No	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
City Fixed Effects	Yes	Yes	Yes	Yes
N	5681	5681	5681	5681
Number of Cities	147	147	147	147

Notes: All specifications include controls for TEL, DIFF, and DEM. Robust standard errors clustered by city are reported in parentheses. Statistical significance indicated with * (p<0.10), ** (p<0.05), *** (p<0.01).

Table C4: Full Results for Current Expenditure Results in Table 2

	ln(Current Expenditures)			
(TEL x DIFF)	-0.0490***	-0.00965	-0.0337**	-0.0115
	(0.0167)	(0.0193)	(0.0137)	(0.0183)
TEL	0.0182		-0.00405	
	(0.0178)		(0.0150)	
DIFF	0.0432***	-0.0215	-0.00630	-0.0560***
	(0.0110)	(0.0153)	(0.00917)	(0.0138)
Share Dem Prez Vote	-0.0712	-0.487***	0.0699	-0.257***
	(0.0688)	(0.0969)	(0.0621)	(0.0962)
ln(Per Capita Personal Income)			0.0861***	0.168***
			(0.0333)	(0.0475)
ln(Population)			0.672***	0.568***
			(0.0224)	(0.0372)
ln(Non-Own Source Revenues)			0.00913***	0.00819***
			(0.00198)	(0.00208)
Farm Share Emp. Share			1.327***	1.899***
			(0.459)	(0.546)
Manufacturing Share Emp. Share			0.891***	0.227
			(0.104)	(0.149)
State x Year Fixed Effects	No	Yes	No	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
City Fixed Effects	Yes	Yes	Yes	Yes
N	5681	5681	5681	5681
Number of Cities	147	147	147	147

Notes: All specifications include controls for TEL, DIFF, and DEM. Robust standard errors clustered by city are reported in parentheses. Statistical significance indicated with * (p<0.10), ** (p<0.05), *** (p<0.01).

Table C5: Full Results for Property Tax Revenue Results in Table 2

	ln(Property Tax Revenues)			
(TEL x DIFF)	-0.0284	0.0357	-0.0137	0.0275
	(0.0716)	(0.0600)	(0.0713)	(0.0599)
TEL	-0.0376		-0.0748	
	(0.0586)		(0.0586)	
DIFF	0.0304	-0.00588	-0.0131	-0.0471**
	(0.0241)	(0.0264)	(0.0255)	(0.0238)
Share Dem Prez Vote	0.232	-0.146	0.676***	0.256
	(0.243)	(0.167)	(0.241)	(0.186)
ln(Per Capita Personal Income)			0.0977	0.357***
			(0.0870)	(0.0932)
ln(Population)			0.554***	0.683***
			(0.155)	(0.112)
ln(Non-Own Source Revenues)			0.00115	0.00526**
			(0.00403)	(0.00215)
Farm Share Emp. Share			-4.589***	1.463*
			(1.391)	(0.825)
Manufacturing Share Emp. Share			2.407***	2.104***
			(0.357)	(0.469)
State x Year Fixed Effects	No	Yes	No	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
City Fixed Effects	Yes	Yes	Yes	Yes
N	5681	5681	5681	5681
Number of Cities	147	147	147	147

Notes: All specifications include controls for TEL, DIFF, and DEM. Robust standard errors clustered by city are reported in parentheses. Statistical significance indicated with * (p<0.10), ** (p<0.05), *** (p<0.01).

Table C6: Full Results for Total Expenditure Results in Table 3

	ln(Total Expenditures)			
(TEL x DIFF)	-0.0363*	-0.0142	-0.0300*	-0.0182
	(0.0202)	(0.0237)	(0.0175)	(0.0229)
(TEL x DIFF + 10)	-0.00777	-0.0475**	0.0138	-0.0375**
	(0.0162)	(0.0190)	(0.0140)	(0.0178)
TEL	0.0179		-0.00692	
	(0.0216)		(0.0192)	
DIFF	0.00815	-0.00403	-0.0401***	-0.0479***
	(0.0137)	(0.0195)	(0.0119)	(0.0184)
DIFF + 10	0.0309**	0.0465***	-0.0287**	0.0238*
	(0.0130)	(0.0159)	(0.0114)	(0.0144)
Share Dem Prez Vote	0.124	-0.512***	0.309***	-0.178
	(0.0868)	(0.123)	(0.0802)	(0.122)
ln(Per Capita Personal Income)			0.0590	0.0441
			(0.0368)	(0.0508)
ln(Population)			0.833***	0.678***
			(0.0268)	(0.0418)
ln(Non-Own Source Revenues)			0.00878***	0.00749***
			(0.00211)	(0.00206)
Farm Share Emp. Share			2.109***	3.223***
			(0.575)	(0.662)
Manufacturing Share Emp. Share			0.675***	0.654***
			(0.126)	(0.180)
State x Year Fixed Effects	No	Yes	No	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
City Fixed Effects	Yes	Yes	Yes	Yes
N	5681	5681	5681	5681
Number of Cities	147	147	147	147

Notes: All specifications include controls for TEL, DIFF, and DEM. Robust standard errors clustered by city are reported in parentheses. Statistical significance indicated with * (p<0.10), ** (p<0.05), *** (p<0.01).

Table C7: Full Results for Current Expenditure Results in Table 3

	ln(Current Expenditures)			
(TEL x DIFF)	-0.0429**	-0.00790	-0.0366**	-0.00957
	(0.0172)	(0.0193)	(0.0144)	(0.0183)
(TEL x DIFF + 10)	-0.0106	-0.0284*	0.00738	-0.0237
	(0.0140)	(0.0171)	(0.0116)	(0.0156)
TEL	0.0191		-0.00483	
	(0.0181)		(0.0151)	
DIFF	0.0366***	-0.0196	-0.00429	-0.0558***
	(0.0113)	(0.0152)	(0.00958)	(0.0138)
DIFF + 10	0.0413***	0.0363***	-0.00909	0.0201*
	(0.0110)	(0.0138)	(0.00925)	(0.0120)
Share Dem Prez Vote	-0.120*	-0.553***	0.0729	-0.278***
	(0.0699)	(0.109)	(0.0631)	(0.107)
ln(Per Capita Personal Income)			0.0866***	0.173***
			(0.0334)	(0.0477)
ln(Population)			0.674***	0.566***
			(0.0228)	(0.0373)
ln(Non-Own Source Revenues)			0.00911***	0.00816***
			(0.00198)	(0.00208)
Farm Share Emp. Share			1.375***	1.909***
			(0.466)	(0.545)
Manufacturing Share Emp. Share			0.895***	0.216
			(0.104)	(0.150)
State x Year Fixed Effects	No	Yes	No	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
City Fixed Effects	Yes	Yes	Yes	Yes
N	5681	5681	5681	5681
Number of Cities	147	147	147	147

Notes: All specifications include controls for TEL, DIFF, and DEM. Robust standard errors clustered by city are reported in parentheses. Statistical significance indicated with * (p<0.10), ** (p<0.05), *** (p<0.01).

Table C8: Full Results for Property Tax Revenues Results in Table 3

	ln(Property Tax Revenues)			
(TEL x DIFF)	-0.0230	0.0406	-0.0151	0.0309
	(0.0765)	(0.0610)	(0.0761)	(0.0610)
(TEL x DIFF + 10)	0.0121	-0.0623	0.0224	-0.0430
	(0.0335)	(0.0433)	(0.0313)	(0.0412)
TEL	-0.0388		-0.0743	
	(0.0577)		(0.0577)	
DIFF	0.0163	-0.00484	-0.0164	-0.0465*
	(0.0242)	(0.0277)	(0.0249)	(0.0255)
DIFF + 10	0.138***	0.0595*	0.102***	0.0381
	(0.0254)	(0.0328)	(0.0320)	(0.0318)
Share Dem Prez Vote	0.00817	-0.215	0.512*	0.212
	(0.271)	(0.263)	(0.264)	(0.282)
ln(Per Capita Personal Income)			0.106	0.367***
			(0.0883)	(0.0937)
ln(Population)			0.521***	0.679***
			(0.162)	(0.112)
ln(Non-Own Source Revenues)			0.00173	0.00521**
			(0.00399)	(0.00218)
Farm Share Emp. Share			-5.517***	1.478*
			(1.577)	(0.795)
Manufacturing Share Emp. Share			2.276***	2.084***
			(0.333)	(0.475)
State x Year Fixed Effects	No	Yes	No	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
City Fixed Effects	Yes	Yes	Yes	Yes
N	5681	5681	5681	5681
Number of Cities	147	147	147	147

Notes: All specifications include controls for TEL, DIFF, and DEM. Robust standard errors clustered by city are reported in parentheses. Statistical significance indicated with * (p<0.10), ** (p<0.05), *** (p<0.01).