What Property Tax Limitations Do to Local Finances: A Meta-Analysis

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ABSTRACT

Since California voters approved a state constitutional amendment to limit property taxes in 1978, most states in the United States have adopted legal limits on the annual increase of the property tax levy. Prior studies of the fiscal impact of property tax limitation on local government come to mixed conclusions. This study summarizes the literature with meta-regression analyses of the effect of property tax limitation on per capita property tax revenues, non-property-tax revenues, and total local revenues and expenditures. Aggregating estimates across studies provides better evidence that local governments are unable to circumvent limitations on property tax increases. Property tax limitations reduce property tax revenues. They may lead to compensatory increases in other taxes, but on average such increases do not fully make up for the foregone property tax revenue, and the net impact of a property tax limitation is therefore substantial fiscal constraint in the local public sector. By reducing the taxation of wealth and the spending on locally provided public services, property tax limitation may have a variety of perverse consequences for social life.
The property tax is the oldest tax in the United States, as well as being the only substantial tax on wealth, a major part of the housing expense of most American families, and the most important revenue source for local governments. It is also increasingly constrained by law. Since California’s Proposition 13 became law in 1978, 26 other states have enacted similar legislation to limit the annual increase of property tax revenues. These property tax limitations are popular—one U.S. poll from summer 2014 finds that 70% of American adults agree that “State government should limit the percentage that property taxes can be raised each year” (Princeton Survey Research Associates International 2014)—but their effects are poorly understood.

This paper reviews what is known about the effects of property tax limitation on local government finance. Many of the most vigorous debates about property tax limitation concern its impact on other aspects of social life—critics charge, for example, that a policy of property tax limitation can exacerbate housing inequality, distort land-use decisions, impair residential mobility, worsen local public services, block educational opportunity, and undermine trust in local government (see e.g. Schrag 1998)—but whatever the indirect effects of property tax limitations on such distal outcomes, they are generally assumed to exert such effects by changing how governments tax and spend. My focus here is on the first link in the causal chain. I use meta-regression analysis to summarize the literature on how property tax limitations affect what Barnes (2014) has called the “size and shape of government”: that is, the magnitude and distribution of public revenues and expenditures. In particular, this paper investigates whether property tax limitations have the intended effect of reducing property taxes; whether they induce
local governments to increase other, non-property taxes; and whether they have any net impact on the magnitude of local government budgets.

The meta-analysis shows that property tax limitations do indeed reduce property tax revenues. There is some evidence that they may also lead local governments to increase other kinds of tax revenues to compensate for the loss of property taxes. The net effect on local government budgets, however, is unambiguously negative. Property tax limitations constrain local government spending. These findings are consistent with the view that property tax limitation contributes to a climate of austerity in local government. A policy of property tax limitation may thereby have a substantial impact on the quantity and quality of local government services, and indirectly on many other aspects of social life.

HOW PROPERTY TAX LIMITATIONS WORK

The property tax limitations considered in this review are state laws that limit the annual increase in the property tax revenues that may be raised by some local governments. To understand the design of these laws, it is useful to represent the total property tax revenues $Y$ of a local jurisdiction schematically by the equation $Y = \tau \Sigma v_i$, where $\tau$ is the local property tax rate and $v_i$ is the assessed value of an individual property $i$. Early property tax limitations such as those adopted by many Southern states after Reconstruction sought to constrain the tax by setting a maximum value for $\tau$. The conventional scholarly wisdom is that these early property tax limitation laws proved to be easily circumvented. Local officials who were not free to increase $\tau$ could still achieve a desired increase of $Y$ by manipulating the values of $v_i$, for example by changing their
rules for evaluating property, or by exercising discretion in the application of those rules. The drafters of the late twentieth century and early twenty-first century property tax limitations therefore went further. The property tax levy limitations considered in this review are of two kinds. *Explicit* levy limitations impose a legal cap on the annual growth rate of $Y$, the total property tax levy. If the aggregate value of local property is increasing faster than the cap permits—for example, if the housing market is booming—then jurisdictions subject to an explicit levy limitation must compensate for the increase in $\Delta v_i$ by reducing the property tax rate $\tau$. *Implicit* levy limitations, such as California’s Proposition 13, do not constrain $Y$ directly, but instead impose both a maximum value of $\tau$ and a legal cap on the growth rate of $v_i$, the assessed value of any individual parcel. These implicit limitations work by changing the rules for valuation of property. If $\tau$ is at its legal maximum and the aggregate market value of local property is increasing faster than the cap permits, then local officials who are subject to an implicit property tax limitation must permit the assessed values of at least some individual properties to diverge from their market values. These two types of property tax limitation may have different implications for the distribution of housing wealth and the operation of housing markets. They may also have different implications for voter awareness: implicit levy limitations allow every individual real property owner to monitor a local government’s compliance with the limit simply by reading year-to-year changes in his or her own property tax bill (Seljan 2014). What explicit and implicit levy limitations have in common is simply that they dictate a maximum growth rate of property tax revenues.

Do such property tax limitations actually limit property taxes? The question might seem silly, but it is not obvious that a policy of property tax limitation should have any
effect. In the first place, the degree to which a limitation actually constrains the growth of
the property tax levy depends on quantitative policy parameters. These vary
substantially—California’s implicit property tax limitation caps annual increases at 2%,
for example, while Minnesota’s explicit limitation caps annual increases at 3.9%—and
some may be set so high that in a normal year they do not affect the behavior of local
taxing authorities at all. State legislators might adopt an effectively nonbinding property
tax limitation as a purely symbolic gesture, pandering to voters who like the idea of
property tax limitation while avoiding the difficult conflicts entailed by any serious effort
at retrenchment of local spending. In the second place, even potentially constraining
property tax limitations may be circumvented. Public officials are not always faithful
executors of policies made by others, perhaps especially not when the purpose of those
policies is to limit their power. Local officials who wish to increase revenues faster than a
property tax levy limitation permits might have a variety of options, ranging from the
creation of additional taxing jurisdictions, to the creative legal interpretation of what
counts as a property tax, to simply raising taxes in defiance of state law. The options for
circumventing a given property tax limitation will presumably depend on the particulars
of how the law is drafted, but if such options exist, we might expect that revenue-hungry
officials will be motivated to find them (Kousser, McCubbins and Moule 2008).

Even if property tax limitations do constrain property tax revenues, moreover, that
does not mean that they constrain the growth of local government revenues or
expenditures more generally. On one hand, officials who find their revenues constrained
by a property tax limitation may simply increase other taxes to compensate for the
unavailable property tax revenues. Some libertarian advocates of small government,
reasoning along these lines, have been skeptical of property tax limitation: a policy of property tax limitation may introduce market distortions without actually constraining the increase of local budgets (e.g. Buchanan 1979). On the other hand, the alternative revenue sources available to local government may not permit the same revenue growth as the property tax, inasmuch as they are more structurally constrained by tax competition. In contrast to real estate, other tax bases—such as sales or income—are more easily moved outside the city limits, and this mobility can provide a check on the growth of revenues. To the extent that a property tax limitation induces local government to shift from taxes on real estate to taxes that are levied on more mobile resources, it may constrain the growth of government indirectly.

LIMITATIONS OF PRIOR RESEARCH

The questions of whether property tax levy limitations constrain property taxes, and local taxation in general, remain unresolved for conceptual and methodological reasons. Conceptually, most studies of the impact of property tax levy limitations lump them together with one or another set of policies that limit the growth of local revenues or expenditures. This literature on the fiscal impact of “tax and expenditure limitations” (TELs) in general (see the summary in Ballal and Rubenstein 2009) provides only limited information about the effects of property tax limitation in particular. Methodologically, social scientists have relied on regression analysis of observational data to identify the effects of property tax limitation, and differences in the specification and estimation of these regression models may affect the estimates they yield. The existing studies also analyze data corresponding to different places, units of analysis, and years: they range
from cross-section time-series analysis of local government revenues aggregated to the state level, to short panels of data on specific categories of local government within a single state. Even when these studies aim to estimate the same conceptual parameter (say, the effect of property tax limitation on property tax revenues per capita), the parameter in question may be context-dependent in ways that would make a simple average misleading.

That is not to say that existing studies are uninformative. My solution to these problems is not to discard prior research but instead to summarize its findings with meta-regression analysis. This method permits us to average the findings of multiple studies, thereby increasing our sample size and reducing uncertainty, while adjusting the average for known differences in conceptualization, regression specification, and context.

META-ANALYTIC METHODS

Meta-regression analysis is a formal method for summarizing the results of quantitative research. When confronted with multiple studies of the same quantitative relationship, one often wants to know an average result, and when the studies come to different conclusions, one may also wish to know whether the differences depend systematically on characteristics of the study. This is a problem in estimating a conditional mean; and in principle, regression analysis by the method of least squares calculates the best linear unbiased estimate of a conditional mean. Thus meta-regression analysis: a regression analysis that takes as its data the coefficients estimated in previous regression analyses.
The process of summarizing the literature on property tax limitation with a meta-regression analysis proceeded in three stages: the construction of a sample of estimates, the coding of study characteristics, and the statistical estimation of a conditional mean.

The construction of the sample. I selected every estimate I could identify that met three criteria. First, it was published since 1978, the year of California’s Proposition 13, in a peer-reviewed journal. Some recent methodological texts on meta-analysis counsel the inclusion of unpublished studies in order to counteract the assumed bias of journal editors in favor of publishing papers that show large and statistically significant results (see e.g. Ringuist 2013, Poot 2014). The available methods for identifying, locating, and retrieving the relevant unpublished studies, however, have not been shown to address the most important sources of selection bias, and they may introduce potential selection biases of their own that are at least as grave as those they are designed to correct.¹ The virtue of selecting only peer-reviewed studies is that it provides an impersonal and replicable criterion for identifying available results of adequate quality.

¹ The literature on meta-analysis discusses the problem of publication bias as the “file drawer problem” (Rosenthal 1979; Simonsohn et al. 2014): journal editors may regard statistically insignificant results as uninteresting, and such results may therefore end up gathering dust in a file drawer, even if they are correct. The potential for such bias is real, but the mechanisms producing it are many and subtle, and the image of dozens of relevant papers sitting in file drawers for every published result is misleading. The realities of contemporary statistical practice in the social sciences are such that a statistically insignificant or equivocal result may not even be saved on a hard drive, or recognized by the investigator as a result worth writing up—much less written up in a paper that will be saved for decades in a file drawer, only to be remembered and made available, on request, to a stranger who may be suspected of having an axe to grind. An additional complication is that many rejected or unsubmitted papers, and perhaps most, may have been rejected or remained unsubmitted for the good reason that their methods were unsound. Even if an unbiased index to the proverbial file drawer studies existed, so that the studies could be found, they must then be vetted for relevance and quality. There is no reason to think that the judgments of meta-analysts about these matters are more unbiased than the judgments of journal referees.
Second, studies to be included must have *reported at least one regression estimate of the partial coefficient of property tax levy limitation* in a regression of state or local revenues or expenditures. Different studies employed different operational definitions of property tax limitation. Because the focus of this review is on modern levy limitations, I excluded studies of laws that merely capped the property tax rate, without also limiting the annual growth of property tax revenues; but I included studies of implicit levy limitations that capped the property tax levy by capping the property tax rate and the annual increase of assessed property values. I included studies that grouped subtypes of modern property tax levy limitation together, as well as studies that treated them separately. I also included studies that subsumed property tax limitations into a more general category of policy such as “potentially binding tax and expenditure limitations” (Joyce and Mullins 1991) and reported estimates of the average impact of policies belonging to the more general category.

Third, I included only estimated coefficients of property tax limitation from regression models that operationalized the presence of property tax limitation as a *dichotomous variable*. The estimates summarized here therefore refer to the average difference in the measured outcome between units with and without property tax limitations. This regression specification disregards potentially relevant differences in the stringency and coverage of property tax limitations, but it is the only specification common to multiple studies. A study using this method will yield an estimate of what the average property tax levy limitation has done, which is likely to be a conservative estimate of what the most stringent property tax limitations have done, to say nothing of what property tax limitations *can* do. To the extent that this dummy variable specification
averages over differences among heterogeneous policies, we may expect that coefficients will be biased towards zero.

The search for estimates that met these criteria proceeded in stages. First, I consulted published literature reviews including Anderson (2006), Haveman and Sexton (2008), Martin (2008: Ch. 7), and Ballal and Rubenstein (2009). Second, I searched the Social Science Citation Index, Google Scholar, and JSTOR with the keywords “property tax” or the combination of the keywords “tax” and “limitation.” I inspected all titles and abstracts for relevance, and I read a subset of 80 studies that appeared likely to meet the criteria for inclusion. Third, I identified relevant sources cited in the studies I had selected for inclusion. I also conducted a reverse bibliographic search for recent studies that cited the studies I had already selected for inclusion. Fourth, I consulted the tables of contents of relevant journals that I had identified in the early stages of my search, including the U.S. Tax Journal, Regional Science and Urban Economics, Public Finance Review, Education Finance and Policy, and Public Budgeting and Finance. The studies that were selected for inclusion are described in Table 1.

Coding procedures. Once I selected studies, I recorded the estimated impact of property tax limitation. I transformed each estimate into a semi-elasticity of dollars per capita, or the estimated percentage difference in dollars per capita between a unit with property tax limitation and a unit without property tax limitation. In most cases this required no transformation whatsoever, as most of the regression specifications in the literature are designed to estimate semi-elasticities of per capita fiscal quantities. The

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2 While this systematic search strategy gives me some confidence that my sample is complete, in the sense that it includes every estimate meeting my search criteria that was published in a peer-reviewed journal from 1978 to the present, I would welcome references to any additional studies that meet these criteria if I have missed them.
Appendix to this paper lists the different functional forms of the regression estimates included in the database, and the algebraic transformations (if any) that were required to render them into comparable semi-elasticities.

I also coded each estimate for characteristics of the underlying regression model. First, I coded a dichotomous variable equal to one if the model lumped together property tax limitation with other limitations on local government revenues or expenditures. Second, I coded a dichotomous variable equal to one if the regression equation omitted to control for the presence of other local TELs. A model that does not control for the presence of alternative forms of local TEL may underestimate or overestimate the impact of property tax levy limitation. Third, I coded a dichotomous variable equal to one if the underlying study sample was limited to a single state. Fourth, I also coded for whether the underlying regression equation included an adjustment for unmeasured time-invariant differences between jurisdictions with and without property tax limitation. A study might adjust for such time-invariant characteristics by including jurisdiction-specific fixed effects, or by first-differencing all variables. A study that fails to adjust in either of these ways is likely to confound the impact of a property tax limitation with the impact of unmeasured characteristics of the states that subsequently adopt a property tax limitation. All of these dichotomous variables are coded so that the reference category is the preferred specification. Thanks to this coding scheme, the intercept in the meta-regression analysis that follows may be interpreted directly as the effect we would expect to estimate in a model with the best specification: namely, one that that treats property tax limitation separately from other TELs, controls for the presence of other local TELs, samples multiple states, and controls for time-invariant characteristics of the jurisdiction.
I report separate meta-analytic results for each of three dependent variables. The first dependent variable is the estimated impact of property tax limitation on property tax revenues per capita. The second dependent variable is the estimated impact of property tax limitation on other (non-property-tax) revenues per capita. This general heading encompasses all estimates of the impact of property tax limitation on revenues from any non-property-tax revenue source, where the sources in question range in specificity from such specific categories as school lunch fees per capita to such general categories as total per capita local own-source revenues net of property taxes. The third dependent variable is the estimated impact of property tax limitation on any aggregate measure of local government budgets, including measures of local expenditures and various total measures of local revenues (e.g., total revenues, total own-source revenues, or total tax revenues).

At the local level, aggregate revenues and aggregate expenditures are roughly equivalent measures of the overall magnitude of a public budget, because bond markets and, in many cases, state and local law effectively prohibit local governments from operating at a deficit for any length of time. Table 2 reports the number of estimates and the means of these variables, alongside the mean of the dependent variable, for each of three dependent variables in this study.

*Statistical methods.* The standard regression assumption that errors are independent and identically distributed is inappropriate in the context of a meta-regression of published research results. Because the data are regression estimates with unequal variances, they are not identically distributed. I therefore fit the meta-regression equations by generalized least squares, with each observation weighted in proportion to the inverse of the standard error of the semi-elasticity (see Koetse, Florax and de Groot...
2010; Poot 2013). Because multiple estimates from the same published study are unlikely to be independent of each other, I treat each published study as a cluster of observations, and compute the standard errors of the meta-regression from a cluster robust variance estimator that permits an arbitrary correlation structure among observations from the same cluster (see Ringquist 2013). Finally, I compute statistical significance tests for coefficients using the “wild cluster bootstrap” recommended by Cameron and Miller (2015) for clustered data with fewer than ten clusters.³

WHAT PROPERTY TAX LEVY LIMITATIONS DO TO LOCAL FINANCES

Do property tax levy limitations actually limit the property tax levy? To answer this question I summarize 41 estimates of the impact of property tax limitation on per capita property tax revenues from 11 studies. Figure 1 is a plot of the published estimates, expressed as semi-elasticities, in order of publication. Each point is an estimated effect and each horizontal line is a 95% confidence interval. The preliminary answer suggested by the figure is that property tax limitations do have the intended effect: they reduce property tax revenues. The precise magnitude of the effect is unclear. The unweighted

³ The p-value associated with a focal variable represents the proportion of times that a t-statistic computed from the coefficient and standard error of that variable exceeded the analogous t-statistic computed after a regression over one of 999 pseudo-samples generated from a simulation that assumed the null hypothesis to be true. In each pseudo-sample, the dependent variable y was replaced by a predicted value $y^* = \beta'x + \delta_i u$, where $u$ was the residual from a regression of y on all x’s except the focal variable; i indexes the cluster (in this case, the published study); and $\delta_i$ was a randomly chosen (“wild”) element of {-1.5, -1, -0.5, 0.5, 1, 1.5}. In Monte Carlo studies of clustered data with few clusters, this “wild cluster bootstrap” has been shown to produce more accurate p-values than the cluster robust variance estimator, which only approaches satisfactory levels of unbiasedness and efficiency as the number of clusters exceeds about fifty. See Cameron and Miller (2015).
average semi-elasticity is -0.13; the weighted average semi-elasticity, with weights proportional to the inverse of the standard error, is -.11. Many of these studies lump property tax levy limitations together with other TELs, and it is not clear from the average what portion of the average effect is attributable to property tax limitation as such.

Figure 1.
Estimates of the proportional impact of adopting a property tax limitation on property tax revenues per capita

Sources: see text

To adjust for differences in conceptualization and measurement of the impact of property tax limitation, I therefore regressed the semi-elasticity of property tax revenues
per capita on a vector of dummy variables describing study characteristics. The first column of Table 3 reports the results. The intercept, -0.11, describes the proportional impact of property tax limitation on property tax revenues per capita that we would expect to measure in a study with the best specification (a study of property tax limitations only, controlling for the presence of other local TELs and for time-invariant characteristics of the jurisdiction, in a sample covering multiple states). The bootstrapped p-value of .098 means that an effect of this magnitude would be expected by chance in fewer than 10% of cases if the true effect were zero. The meta-analysis also reveals how measurement choices of prior studies may affect their estimates. Studies of single state, for example, are expected to find even stronger effects of property tax limitation on property tax revenues. It may be that single-state case studies are likely to be conducted in states that have especially stringent property tax limitations, while multi-state studies average across property tax limitations of variable stringency in ways that attenuate estimates of the effects of the strictest property tax limitations. The meta-regression results also show that regression analyses that fail to control for the confounding presence of other forms of TEL may substantially underestimate the impact of property tax limitation.

**Do property tax limitations increase reliance on other revenue sources per capita?**

The forest plot in Figure 2 summarizes 59 estimates from six studies. The weighted average semi-elasticity of 0.035 suggests a positive impact, but the forest plot shows that this quantity averages a decidedly mixed pattern of findings, including several negative semi-elasticities, two large positive outliers, and most estimates quite close to zero. The variability in findings may be dependent on the particular non-property-tax revenue
source in question or on other study characteristics. The meta-regression results summarized in the second column of Table 3 clarify that the expected effect of a property tax limitation on non-property-tax revenues is indistinguishable from zero. The intercept of .006 implies that the best estimate (from a multi-state study that distinguishes property tax limitations from other TELs, controls for the confounding influence of other TELs, and controls for time-invariant characteristics of the jurisdiction) is trivially different from zero, and the bootstrapped p-value of .55 indicates that an estimate at least this large might be expected by chance in a majority of cases even if the true effect were zero. It may be that property tax limitations truly have no effect on other tax and non-tax revenue sources, or it may be that the average effect is attenuated by the heterogeneity of estimates summarized in this meta-regression, which concern a variety of non-property-tax dependent variables. All of the estimates are from multi-state studies, but the variation in findings also might reflect other sampling differences in the time periods or jurisdictions studied, and, by extension, real heterogeneity in local governments’ revenue strategies. Local officials have many options for adapting to a property tax limitation; one city might raise sales taxes, while another might cut expenditures, and a third might rely on a mix of increased fees and hotel taxes, or what have you.
If property tax limitations decrease property tax revenues, and do not cause an increase in revenues from alternative sources, then it is to be expected that their net impact on aggregate local government revenues and aggregate local expenditures will be negative. That is indeed what meta-analysis shows. Figure 3 summarizes 59 estimates of the impact of tax limitation on various measures of aggregate local budgets from 14 studies. The estimates included here include estimated impacts on measures of aggregate local government revenues per capita (from multiple sources that include the property tax), and measures of local government expenditures per capita. The forest plot in Figure
3 shows that most estimates of the impact of property tax levy limits on these per capita budget quantities are negative. The unweighted mean semi-elasticity is \(-0.027\), but this average is influenced by a substantial outlier, and the weighted semi-elasticity is only \(-0.0065\). Property tax limitations reduce local government spending, the proportional impact on spending is not as great as the proportional impact on property tax revenues. The finding of a moderate negative impact is what we should expect: all else being equal, an 11% reduction in property tax revenues will imply a less-than-11% reduction in the local budget, unless a local government relies exclusively on property tax revenues.

Figure 3.
Estimates of the proportional impact of adopting a property tax limitation on measures of aggregate revenues or aggregate spending per capita

Sources: see text
The meta-regression results in the last column of Table 3 confirm that property tax limitation has a negative impact on total local spending per capita. The intercept, which represents the expected impact of property tax limitation on aggregate local budget estimated in a model with the preferred specification, is -0.053, with a bootstrapped p-value of .046. We may infer with a relatively high degree of certainty that budgets per capita after adoption of a property tax limitation are, on average, about 5% lower than they would be otherwise. The coefficient of +.063 associated with models that omit controls for other local TELs implies that the impact of property tax limitation can be entirely obscured by the confounding presence of other policies that limit local government revenue or expenditure growth.

THE SOCIAL IMPACT OF PROPERTY TAX LIMITATION

The findings summarized in this paper imply that a state policy of property tax limitation is a major constraint on the growth of local government finance. By limiting the ability of local officials to tax the one resource that cannot be moved across county lines, property tax limitation constrains local revenues in general. Because local governments cannot long spend more than they take in, property tax limitation may contribute to an overall climate of austerity in the provision of local public services.

Another implication is that studies of property tax limitation should take care to distinguish among types of tax and expenditure limitation policy. The heterogeneous findings of the literature, particularly with respect to the effects of TELs on aggregate fiscal quantities other than property tax revenues, result at least in part from
heterogeneous measurement decisions, and at least in part from real heterogeneity in the effects of the various policies at issue. The umbrella category itself may have limited analytical utility. To generalize about the effects of “TELs” is to overgeneralize. A limitation on the growth of the property tax levy can have the effect of limiting the growth of local spending, but it does not necessarily have the same effect as an explicit limitation on local spending, or as an easily evaded limitation on the property tax rate, or as a non-binding “truth-in-taxation” policy that merely requires voter notification of revenue increases.

The findings summarized here also suggest that some of the critics’ claims about the broader social impacts of property tax limitation may be plausible hypotheses worth testing. By reducing the taxation of real estate, a policy of property tax limitation may have a substantial impact on the inequality of housing wealth, and thereby on wealth inequality in general. By reducing local spending, property tax limitation may reduce spending on services from public education to public health that are particularly important to the household budgets of low-income Americans. Property tax limitation may thereby affect other social outcomes with socio-economic gradients, from subjective outcomes such as well-being, satisfaction with public services, and trust in government, to intersubjectively verifiable outcomes such as educational attainment, morbidity, and even mortality (see Newman and O’Brien 2010). In California, where property tax limitation has been especially contentious, critics of the policy are sometimes accused of blaming everything on Proposition 13. It is true that not everything can be blamed on property tax limitation. But it is also true that property tax limitation may have a wide range of unintended and harmful consequences.
REFERENCES


APPENDIX

All of the studies summarized in this meta-analysis report regressions of fiscal quantities on a dummy variable representing the presence of a property tax limitation (or a more encompassing category of TEL). The regression specifications vary, and in order to summarize them meaningfully it is necessary to transform regression coefficients into a common measure of impact. I chose the *semi-elasticity of dollars per capita*, or the proportional change in per capita fiscal quantities associated with the presence of a property tax limitation. Most regression specifications included one coefficient that could be interpreted directly as a semi-elasticity of dollars per capita, but in some cases the transformation of the coefficient into a semi-elasticity required additional information about the mean of the dependent variable. The appendix Table A lists the studies summarized here by generic regression specification, and details the algebraic form of the semi-elasticity derived from each generic specification. The notation is as follows: $Y$ is the dollar-denominated fiscal variable (usually representing a measure of aggregate revenues from a particular source, but in some cases representing aggregate expenditures or aggregate revenues from multiple sources, as in the meta-analysis summarized in Figure 3). $T$ is a dummy variable for the presence of a property tax limit. $P$ is the measure of population that provides the denominator of per-capita calculations (usually $P$ represents the total residents of a given jurisdiction, but some measures of school revenues or expenditures are reported in the literature per pupil rather than per adult resident). $X$ is a vector of covariates including a constant. The Greek letter $\beta$ represents the regression coefficient of $T$, $\gamma$ represents the regression coefficient of $X$, $\phi$ represents
the regression coefficient of $P$, and $\varepsilon$ represents the error term. Horizontal bars over letters represent mean values. All quantities are annual measures, unless prefixed by the Greek letter $\Delta$, which signifies year-on-year change.
Table 1. Selected characteristics of studies in the sample

<table>
<thead>
<tr>
<th>Publication</th>
<th>Unit of analysis</th>
<th>Sample(s)</th>
<th>Sample years</th>
<th>Dependent variables</th>
</tr>
</thead>
<tbody>
<tr>
<td>Shadbegian 1999</td>
<td>County</td>
<td>All counties</td>
<td>1976 - 1990</td>
<td>Own-source general rev., property taxes, non-property taxes, non-tax rev.</td>
</tr>
<tr>
<td>Anderson 2011</td>
<td>City</td>
<td>Minnesota</td>
<td>1977 - 2002</td>
<td>Total exp.</td>
</tr>
<tr>
<td>Jung and Bae 2011</td>
<td>County</td>
<td>U.S.</td>
<td>1980 - 2003</td>
<td>Own source rev., total taxes, user charges</td>
</tr>
<tr>
<td>Authors</td>
<td>Level</td>
<td>Location</td>
<td>Period</td>
<td>Revenue Sources</td>
</tr>
<tr>
<td>------------------</td>
<td>-----------</td>
<td>----------------</td>
<td>------------</td>
<td>---------------------------------------------------------------------------------</td>
</tr>
<tr>
<td>Anderson 2011</td>
<td>City</td>
<td>Minnesota</td>
<td>2000-2001</td>
<td>Capital exp., current exp., property taxes, intergovernmental aid</td>
</tr>
<tr>
<td>Sun 2014</td>
<td>Municipality</td>
<td>Cities with pop. &gt; 25,000</td>
<td>1970-2006</td>
<td>Property taxes, sales taxes, income taxes, other taxes, misc. general rev., user charges</td>
</tr>
<tr>
<td>Downes and Killeen 2014</td>
<td>School district</td>
<td>U.S.</td>
<td>1972-2008</td>
<td>Fees, transportation fees, rev. from school lunch, total own-source non-tax rev., rev. from local sources</td>
</tr>
</tbody>
</table>
Table 2. Sample means of selected study characteristics, by dependent variable

<table>
<thead>
<tr>
<th>Dependent variable is impact of property tax limitation on...</th>
<th>...property tax revenue</th>
<th>... non-property-tax revenues</th>
<th>...total revenues or expenditures</th>
</tr>
</thead>
<tbody>
<tr>
<td>Semi-elasticity</td>
<td>unweighted</td>
<td>weighted</td>
<td>unweighted</td>
</tr>
<tr>
<td></td>
<td>-0.13</td>
<td>-0.011</td>
<td>0.19</td>
</tr>
<tr>
<td>Independent variable includes other local TELs? (1=yes)</td>
<td>0.54</td>
<td>.37</td>
<td>0.85</td>
</tr>
<tr>
<td>Model fails to control for other local TELs? (1=yes)</td>
<td>0.76</td>
<td>.80</td>
<td>0.73</td>
</tr>
<tr>
<td>Sample is from a single state? (1=yes)</td>
<td>0.24</td>
<td>.97</td>
<td>0</td>
</tr>
<tr>
<td>Model is unadjusted for time-invariant characteristics of state or jurisdiction? (1=yes)</td>
<td>0.46</td>
<td>.10</td>
<td>0.14</td>
</tr>
<tr>
<td>N estimates</td>
<td>41</td>
<td>59</td>
<td></td>
</tr>
<tr>
<td>N studies</td>
<td>11</td>
<td>6</td>
<td>14</td>
</tr>
</tbody>
</table>
Table 3. Results of meta-regression analysis

<table>
<thead>
<tr>
<th></th>
<th>Property tax</th>
<th>Other revenues</th>
<th>Total revenues or expenditures</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Effect</td>
<td>p-value</td>
<td>Effect</td>
</tr>
<tr>
<td>Intercept</td>
<td>-.114</td>
<td>.098</td>
<td>0.006</td>
</tr>
<tr>
<td>Estimated effect</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td><em>includes</em> other local TELs? (1=yes)</td>
<td>-.006</td>
<td>.466</td>
<td>-.021</td>
</tr>
<tr>
<td>Model <em>fails</em> to control for other local TELs? (1=yes)</td>
<td>.108</td>
<td>.168</td>
<td>.052</td>
</tr>
<tr>
<td>Sample is from a single state? (1=yes)</td>
<td>-.034</td>
<td>.098</td>
<td>...</td>
</tr>
<tr>
<td>Model is <em>unadjusted</em> for time-invariant characteristics of state or jurisdiction? (1=yes)</td>
<td>.009</td>
<td>.122</td>
<td>-.002</td>
</tr>
<tr>
<td>N estimates</td>
<td>41</td>
<td></td>
<td>59</td>
</tr>
<tr>
<td>M studies</td>
<td>11</td>
<td></td>
<td>6</td>
</tr>
</tbody>
</table>

Note: P-values are computed from a “wild cluster bootstrap” (see Cameron and Miller 2015).
Table A-1. Functional forms of regression equations and derived semi-elasticities of dollars per capita with respect to property tax limitation

<table>
<thead>
<tr>
<th>Functional form</th>
<th>Derived semi-elasticity</th>
<th>Sources reporting models with this functional form</th>
</tr>
</thead>
<tbody>
<tr>
<td>$\ln(Y/P) = \beta T + \gamma X + \varepsilon$</td>
<td>$\beta$</td>
<td>Anderson 2011, Downes and Killeen 2014, Shadbegian 1998</td>
</tr>
<tr>
<td>$(Y/P) = \beta T + \gamma X + \varepsilon$</td>
<td>$\beta / \bar{Y}$</td>
<td>Blankenau and Skidmore 2004; Figlio and O’Sullivan 2001; Hoyt, Coomes and Biehl 2011; Jung and Bae 2011; Shadbegian 1999; Shadbegian 2003; Skidmore 1999; Sun 2014</td>
</tr>
<tr>
<td>$\ln(Y) = \beta T + \phi \ln(P) + \gamma X + \varepsilon$</td>
<td>$\beta$</td>
<td>McCubbins and Moule 2010, Seljan 2014</td>
</tr>
<tr>
<td>$\Delta Y / Y = \beta T + \phi \Delta P / P + \gamma X + \varepsilon$</td>
<td>$\beta$</td>
<td>Dye and McGuire 1997; Dye, McGuire and McMillen 2005</td>
</tr>
<tr>
<td>$\Delta (Y/P) = \beta T + \gamma X + \varepsilon$</td>
<td>$\beta$</td>
<td>Dye and McGuire 1997; Gore 2009; Preston and Ichniowski 1991; Shadbegian 1998</td>
</tr>
</tbody>
</table>