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

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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 τ 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 τ. 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 τ 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 $\Box v_i$ by reducing the property tax rate τ. *Implicit* levy limitations, such as California's Proposition 13, do not constrain Y directly, but instead impose both a maximum value of τ 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 τ 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.

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[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*.[2] 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]
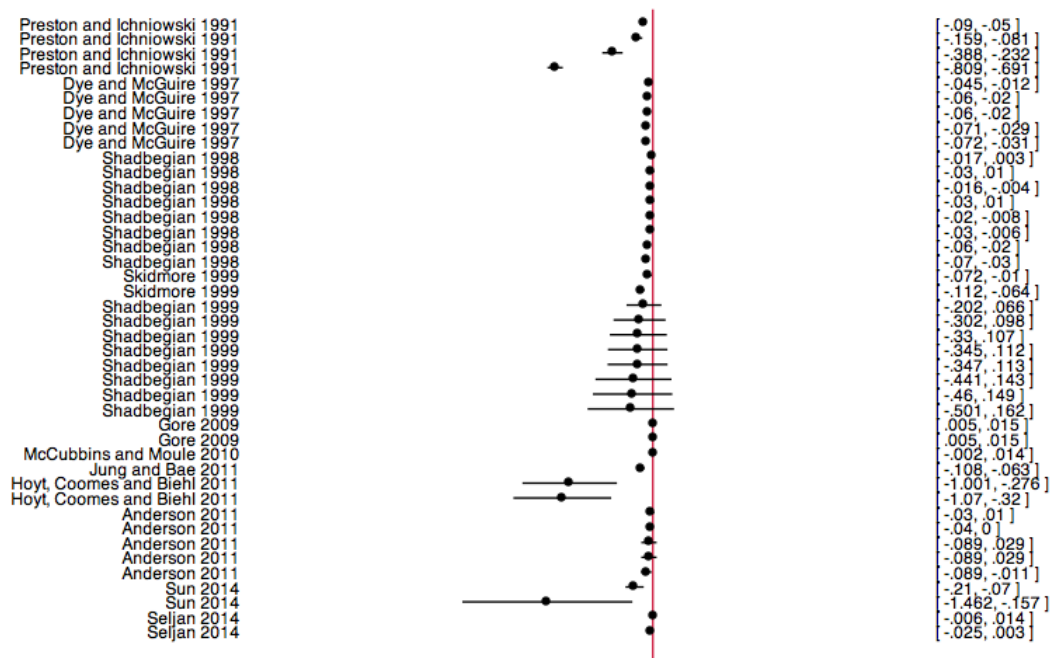
WHAT PROPERTY TAX LEVY LIMITATIONS DO TO LOCAL FINANCES

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

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[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 $y^* = \boldsymbol{\beta'x} + \delta_i u$, 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 δ_i was a randomly chosen ("wild") element of $\{-\sqrt{1.5}, -1, -\sqrt{.5}, \sqrt{.5}, 1, \sqrt{1.5}\}$. 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

Study		Estimate, CI
Preston and Ichniowski 1991		[-.09, -.05]
Preston and Ichniowski 1991		[-.159, -.081]
Preston and Ichniowski 1991		[-.388, -.232]
Preston and Ichniowski 1991		[-.809, -.691]
Dye and McGuire 1997		[-.045, -.012]
Dye and McGuire 1997		[-.06, -.02]
Dye and McGuire 1997		[-.06, -.02]
Dye and McGuire 1997		[-.071, -.029]
Dye and McGuire 1997		[-.072, -.031]
Shadbegian 1998		[-.017, .003]
Shadbegian 1998		[-.03, .01]
Shadbegian 1998		[-.016, -.004]
Shadbegian 1998		[-.03, .01]
Shadbegian 1998		[-.02, -.008]
Shadbegian 1998		[-.03, -.006]
Shadbegian 1998		[-.06, -.02]
Shadbegian 1998		[-.07, -.03]
Skidmore 1999		[-.072, -.01]
Skidmore 1999		[-.112, -.064]
Shadbegian 1999		[-.202, .066]
Shadbegian 1999		[-.302, .098]
Shadbegian 1999		[-.33, .107]
Shadbegian 1999		[-.345, .112]
Shadbegian 1999		[-.347, .113]
Shadbegian 1999		[-.441, .143]
Shadbegian 1999		[-.46, .149]
Shadbegian 1999		[-.501, .162]
Gore 2009		[.005, .015]
Gore 2009		[.005, .015]
McCubbins and Moule 2010		[-.002, .014]
Jung and Bae 2011		[-.108, -.063]
Hoyt, Coomes and Biehl 2011		[-1.001, -.276]
Hoyt, Coomes and Biehl 2011		[-1.07, -.32]
Anderson 2011		[-.03, .01]
Anderson 2011		[-.04, 0]
Anderson 2011		[-.089, .029]
Anderson 2011		[-.089, .029]
Anderson 2011		[-.089, -.011]
Sun 2014		[-.21, -.07]
Sun 2014		[-1.462, -.157]
Seljan 2014		[-.006, .014]
Seljan 2014		[-.025, .003]

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.

Figure 2.
Estimates of the proportional impact of adopting a property tax limitation
on various categories of non-property-tax revenues per capita

Sources: see text

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 -.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
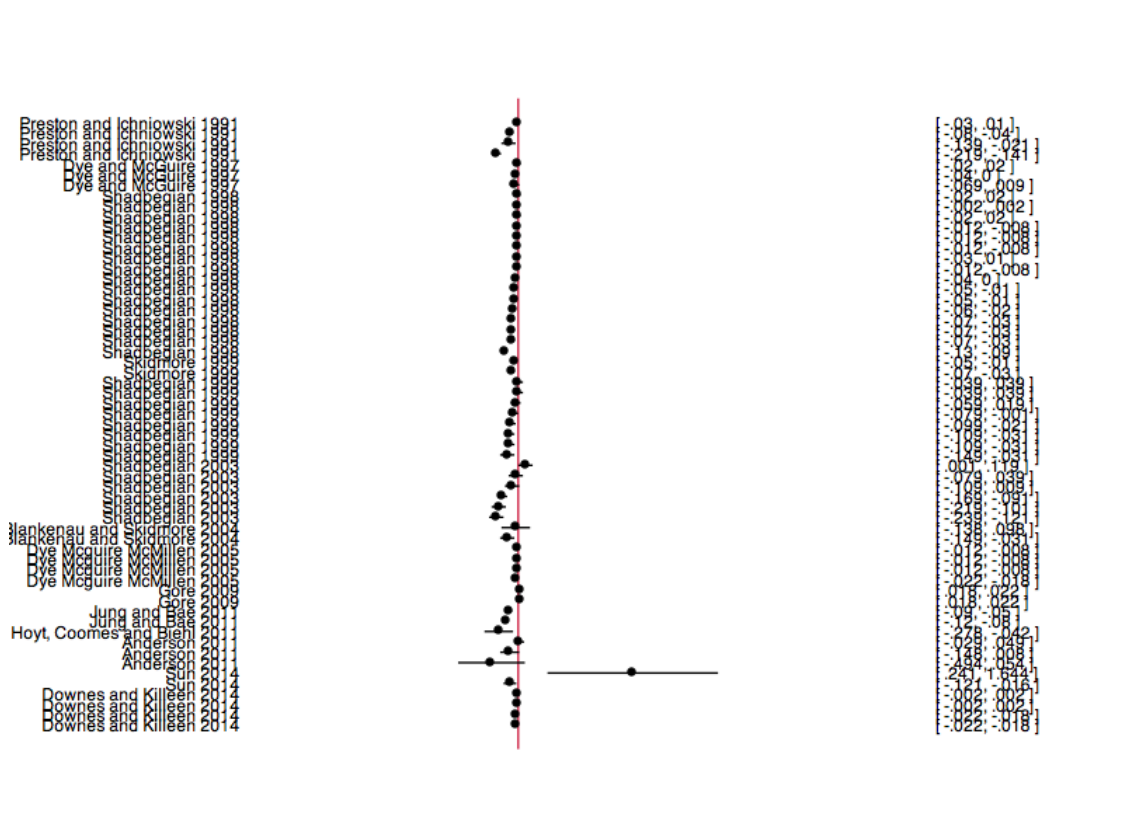
Sources: see text

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.

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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 β represents

the regression coefficient of T, γ represents the regression coefficient of X, φ represents

the regression coefficient of P, and ε represents the error term. Horizontal bars over letters represent mean values. All quantities are annual measures, unless prefixed by the Greek letter Δ, which signifies year-on-year change.

Table 1. Selected characteristics of studies in the sample

Publication	Unit of analysis	Sample(s)	Sample years	Dependent variables
Preston and Ichniowski 1991	Municipality	U.S.	1976 - 1986	Total rev., property taxes
Dye and McGuire 1997	Various	Various	1987 - 1993	Total exp., instructional exp., operating exp.
Figlio 1997	School	U.S.	1987 - 1993	Administrative exp.
Shadbegian 1998	State	U.S., excl. Alaska	1972 - 1992	Local own-source general rev., property taxes
Skidmore 1999	State	U.S., excl. Alaska	1976 - 1990	Property taxes, non-property taxes, federal and state aid, local charges and misc. rev., state misc. rev.
Shadbegian 1999	County	All counties	1976 - 1990	Own-source general rev., property taxes, non-property taxes, non-tax rev.
Skidmore 1999	State	U.S., excl. Alaska	1962 - 1987	Local own source rev.
Figlio and O'Sullivan 2001	Municipality	Cities with pop. >2000	1975 - 1986	Administrative exp., police and fire exp.
Shadbegian 2003	State	Contiguous states	1966 - 1992	Local direct own-source education exp.
Blankenau and Skidmore 2004	State	Contiguous states	1971 - 1993	Local direct own-source education exp., state aid to school districts
Dye Mcguire McMillen 2005	School district	Illinois school districts	1988 - 2001	Operating exp., instructional exp.
Gore 2009	Municipality	U.S.	1997 - 2003	Total taxes, property taxes
McCubbins and Moule 2010	State	U.S., excl. Alaska	1980 - 2000	State and local property taxes
Anderson 2011	City	Minnesota	1977 - 2002	Total exp.
Hoyt, Coomes and Biehl 2011	State	States	1977 - 2002	Property taxes, non-property-taxes, education exp.
Jung and Bae 2011	County	U.S.	1980 - 2003	Own source rev., total taxes, user charges

Anderson 2011	City	Minnesota	2000 - 2001	Capital exp., current exp., property taxes, intergovernmental aid
Seljan 2014	State	U.S., excl. Alaska	1970 - 2006	State and local property taxes
Sun 2014	Municipality	Cities with pop.>25,000	1970 - 2006	Property taxes, sales taxes, income taxes, other taxes, misc. general rev., user charges
Downes and Killeen 2014	School district	U.S.	1972 - 2008	Fees, transportation fees, rev. from school lunch, total own-source non-tax rev., rev. from local sources

Table 2. Sample means of selected study characteristics, by dependent variable

	Dependent variable is impact of property tax limitation on...					
	...property tax revenue		... non-property-tax revenues		...total revenues or expenditures	
	unweighted	weighted	unweighted	weighted	unweighted	weighted
Semi-elasticity	-0.13	-.011	0.19	.035	-0.027	-0.0065
Independent variable *includes* other local TELs? (1=yes)	0.54	.37	0.85	.83	0.69	0.73
Model *fails* to control for other local TELs? (1=yes)	0.76	.80	0.73	.89	0.71	1.00
Sample is from a single state? (1=yes)	0.24	.97	0	1	0.17	0.26
Model is *unadjusted* for time-invariant characteristics of state or jurisdiction? (1=yes)	0.46	.10	0.14	.03	0.39	0.40
N estimates	41		59		59	
N studies	11		6		14	

Table 3. Results of meta-regression analysis

	Property tax		Other revenues		Total revenues or expenditures	
	Effect	p-value	Effect	p-value	Effect	p-value
Intercept	-.114	.098	0.006	.55	-.053	.046
Estimated effect *includes* other local TELs? (1=yes)	-.006	.466	-.021	.42	-.017	.434
Model *fails* to control for other local TELs? (1=yes)	.108	.168	.052	.326	.063	.026
Sample is from a single state? (1=yes)	-.034	.098	...		-.027	.300
Model is *unadjusted* for time-invariant characteristics of state or jurisdiction? (1=yes)	.009	.122	-.002	.89	-.0085	.390
N estimates	41		59		59	
M studies	11		6		14	

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

Functional form	Derived semi-elasticity	Sources reporting models with this functional form
$ln(Y/P) = \beta T + \gamma X + \varepsilon$	β	Anderson 2011, Downes and Killeen 2014, Shadbegian 1998
$(Y/P) = \beta T + \gamma X + \varepsilon$	$\beta/\bar{Y}$	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
$ln(Y) = \beta T + \varphi ln(P) + \gamma X + \varepsilon$	β	McCubbins and Moule 2010, Seljan 2014
$\Delta Y/Y = \beta T + \varphi \Delta P/P + \gamma X + \varepsilon$	β	Dye and McGuire 1997; Dye, McGuire and McMillen 2005
$\Delta(Y/P) = \beta T + \gamma X + \varepsilon$	β	Dye and McGuire 1997; Gore 2009; Preston and Ichniowski 1991; Shadbegian 1998