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Authors

Seljan, Ellen Concetta
McCubbins, Colin

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**Fee for Service:
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Ellen Seljan
Lewis and Clark College
Colin McCubbins
Stanford University

Abstract

Municipalities in California are highly reliant on charges and fees to fund government services. This paper tests whether their dependence can be causally attributed to Proposition 13. We draw our conclusions by leveraging a component of the law that causes constraint from Proposition 13 to be heterogeneous among California municipalities. Specifically, because home sales lead to property reassessments, municipalities with high homeowner stability are more constrained by Proposition 13 than those with low homeowner stability. Our analysis shows that homeowner stability is associated with greater revenue per capita and increased reliance on charges and fees following the passage of Proposition 13. We do not observe the same pattern for renter stability and conclude that Proposition 13 causes revenue substitution to charges and fees.

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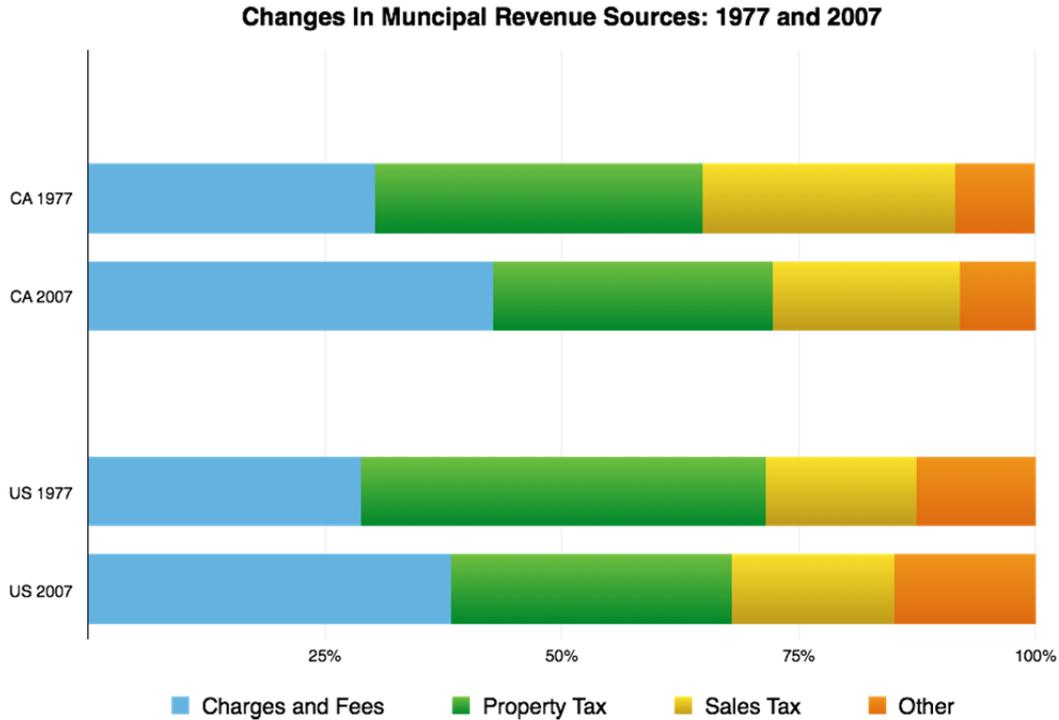
Introduction

California's landmark property tax limitation, Proposition 13, is frequently the subject of scrutiny. The law has become the canonical example of the potent influence of citizen initiatives. In addition to limiting property taxes, scholars have claimed that Proposition 13 is responsible for wide-ranging secondary consequences, including decreased residential mobility (O'Sullivan et al. 1995; Wasi and White 2005), increased housing prices (Rosen 1982), centralization of state and gubernatorial power (Kirlin 1982; Saxton et al. 2002; Citrin 2009), increased debt costs (Beebe 1979), reduced public school performance and increased private school enrollment (Catterall and Brizendine 1985; Figlio 1997; Downes and Schoeman 1998), and a less responsive criminal justice system (Walker et al. 1980), to name a few. This paper seeks to provide causal evidence for an additional secondary consequence of Proposition 13: the increase in municipal revenue derived by charges and fees.

Despite the multitude of proposed consequences of Proposition 13, it is difficult to prove causality. In the time since the initiative passed, California has shifted to a Democratic stronghold and undergone dramatic economic and demographic changes. Determining what is attributable to Proposition 13 and what was caused by these other important changes is a challenging enterprise. For this reason, the analysis of data before and after the passage of Proposition 13 is insufficient. Likewise, comparing outcomes in California to other states without property tax limitations also risks drawing unjustified conclusions. California is a unique state, and no other state or set of states can serve as a reliable "control group" for the purposes of a quasi-experiment.

The challenge of causal inference extends to the topic of this paper: the switch to alternative municipal revenues. At first glance, it seems uncontroversial to attribute this change to Proposition 13. At the time of passage, city governments relied heavily on property taxes and saw their vaults emptied. Today cities lean substantially on nontax revenue sources such as charges, fees, and assessments. In this study municipal reliance on these various revenues reaches as high as 71 percent. Figure 1 depicts the drastic changes in revenue portfolios that occurred in aggregate for California municipalities before and after Proposition 13.

Figure 1.



It is noteworthy, however, that this change is not exclusive to the state of California. Figure 1 also depicts revenue portfolios for US municipalities in aggregate. Few states have passed property tax limits of the magnitude of Proposition 13,¹ yet cities nationwide have moved toward revenues in the forms of charges and fees. This brings up the question of whether or not California's revenue shifts are attributable to Proposition 13 or simply a common trend in municipal finance. We explore this issue, augmenting temporal variation with cross-sectional variation between California municipalities differentially affected by Proposition 13.

We analyze how the law has affected the relative reliance on property taxes, sales taxes, and charges and fees. We show that revenue substitution to charges and fees is attributable to Proposition 13, but not sales tax. We draw this conclusion by taking advantage of a peculiarity of the Proposition 13 law that causes a heterogeneous treatment effect among municipalities. Although Proposition 13 universally limits increases in assessed property valuation to two percent per year, this limit only applies to unsold homes. The assessed value of sold homes resets to real market value. This means that cities with frequent property sales are less constrained by Proposition 13 than communities with more stable property ownership. We use this heterogeneity to show that localities that are more constrained by Proposition 13 rely on charges and fees to a greater extent than localities less constrained by Proposition 13.

¹ Twenty states have passed property tax limits, but only four have included the potent mix of an assessment limit combined with a rate limit (Seljan 2014). Proposition 13 is arguably the most stringent and effective property tax limit in the nation.

We believe this estimation strategy to be superior to previous attempts to link Tax and Expenditure Limits (TEs) to revenue substitution for several reasons. Most previous research on this case relies exclusively on temporal or interstate cross-sectional variation, ignoring potential intrastate variation that can bolster causal inference. Additionally, unlike the variation created by policy adoption, the stability of housing markets is not endogenous to the forces that drove passage of Proposition 13, namely public mood toward taxation. We believe this analysis to contribute to the study of the effects of Proposition 13, and shed light on the wide-reaching, often hidden effects that initiatives can have as they filter through the various layers of government.

This paper proceeds as follows. We begin by discussing past literature on property tax limits and revenue substitution, highlighting how our research design departs from previous scholarship. We then present our data, methods, and results. To bolster the validity of our findings we include two secondary analyses: a test of divergent validity showing that renter stability does not effect revenue substitution, and analysis showing that highly constrained cities have actually increased total revenues since the passage of Proposition 13. We summarize all results in the conclusion.

Literature Review

In 1980 Brennan and Buchanan introduced the concept of government as a revenue-maximizing leviathan. They cite the passage of Proposition 13 as evidence for their theory of political behavior, noting that its passage “must surely raise some doubts about the extent to which normal political processes reflect the popular will” (Brennan and Buchanan 1980, 25). At the height of the tax revolt Brennan and Buchanan prescribed the continued passage of constitutional TEs to counter government’s tax power.

Numerous scholars have since questioned whether TEs can truly tame the leviathan of state government. The effectiveness of TEs has been questioned repeatedly over 30 years of scholarship (Abrams and Dougan 1986; Elder 1992; Mullins and Joyce 1996; Bails and Tieslau 2000; Kousser et al. 2008; Seljan 2014). Indeed, Brennan and Buchanan themselves recognize that TEs would accelerate a search for loopholes for evasion. They note, “If the potential taxpayer in some constitutional choice setting conceives of government in Leviathan terms, he will recognize that the imposition of maximum rates for any particular tax will result in a diversion of fiscal pressures toward those taxes that may not fall under the rate-limit constraint” (Brennan and Buchanan 1980, 197). However, while recognizing this inevitability the authors hold that constitutional limits must still reduce total revenue overall by reducing the feasible set of collection sources. Numerous authors have gone on to test this conjecture.

Analysis of the circumvention of TEs has taken on many names: the “fiscal shell game” (McCubbins and McCubbins 2010), the “politics of circumvention” (Sbagria 1996), and the “circumvention hypothesis” (Kiewiet and Szakaly 1996), to name a few. There has likewise been wide variation in the proposed conduits of circumvention. For example, several authors have attributed circumvention of property tax limits to an increase in state power or, conversely, devolution to lower levels of government (Rueben 1995; Sokolow 1998; Skidmore 1999; Sokolow 2000; Citrin 2009). Other authors have pointed to the increase in “off-budget-enterprises,” frequently taking the form of accounting tricks or innovative financial debt products (Bennett and DiLorenzo 1982; Marlow and Joulfaian 1989).

Alternatively, other scholars, particularly those focusing on the California case, look to circumvention via sales taxes (Misczynski 1986; Schwartz 1997; Lewis 2001; Chapman 1998). These scholars claim that since Proposition 13 there has been a “fiscalization of land use”

whereby cities make land-use decisions with the intention of maximizing revenues from sales taxes. For example, profitable commercial enterprises such as big-box stores and car dealerships may receive zoning preferences over other land-use designations.

The most common theory is that TELs are circumvented through increased user fees or special assessments. Sexton et al. (1999) illustrate the wide range of potential alternative, nontax revenues, including development fees, real estate transfer fees, business license fees, utility user fees, sewer charges, and park and recreation fees. Kogan and McCubbins (2008) explore the California case specifically, noting that special assessments have been used to finance local infrastructure in lieu of property taxes. These charges appear on property tax bills and are only assessed on homeowners, but are nonetheless legally distinct from the property tax since they are not based on assessed property values.

Much of the literature on circumvention uses descriptive statistics and anecdotes to illustrate the evasion of TELs. For example, Sharp and Elkins (1987) plot revenue changes over time in seven Missouri cities to show increases in fees and miscellaneous revenues following the adoption of a property tax limit. Galles and Sexton (1998) provide similar descriptive statistics for aggregate revenues in both California and Massachusetts. Hoene (2004) updates this data for California, again showing that California cities have become less reliant on property taxes and more reliant on charges and fees over time without large changes in sales taxes. Schwartz (1997) presents conflicting descriptive statistics for a smaller time frame, suggesting that sales tax reliance has indeed increased in the state. While none of this scholarship purports to be a scientific test of the hypothesis, the results are suggestive.

Mullins and Joyce (1996), Shadbegian (1999), Johnston et al. (2000), and Kousser et al. (2008) use more advanced methodologies to test the hypothesis that TELs are circumvented using charges and fees. Both Johnston et al. (2000) and Mullins and Joyce (1996) use panel data for the 50 states to show significant correlations between the adoption of TELs and increased usage of charges and fees. Using a series of state-by-state time-series regressions, Kousser et al. (2008) show that 15 of 23 states had significant increases in the growth of revenue from charges and fees following adoption of a TEL.

Causal inference from these studies is limited. All of these studies rely on either temporal comparison within states and/or cross-sectional comparisons between states. Both of these strategies rely on the tenuous assumption that unobservable factors, such as public mood, are not simultaneously driving TEL adoption and relevant dependent variables. Shadbegian (1999) addresses this endogeneity problem using an instrumental variables approach, though the exogeneity of his instrument is questionable.²

While the TEL substitution effect is well documented in anecdotes and descriptive statistics, causal empirical evidence is limited. Our paper seeks to build on this literature with a more robust research design. Specifically, previous scholarship has failed to leverage the heterogeneous effects of TELs on localities. Instead of simply looking at the effects of the initiative in localities before and after adoption, we also make inferences from differences in TEL constraint that exist between localities.

² Shadbegian's (1999) instrument for TELs is a state's propensity to pass initiatives. While this instrument does a good job of predicting passage of TELs, its exogeneity to public mood is questionable, given previous scholarship showing that voters turn to the ballot box when disgruntled with the status quo (Bowler et al. 1998).

Research Design and Data

We tested for revenue substitution using data from 107 California municipalities for the years 1970, 1980, 1990, and 2000.³ Municipalities more constrained by Proposition 13 were expected to exhibit more extensive revenue substitution using charges and fees. We measured heterogeneity in constraint from Proposition 13 by leveraging a peculiar component of the law that calls for the reassessment of sold homes.

Proposition 13 combines two different limitations: a rate limit, and an assessment limit. The rate limit dictates that the tax rate is at most one percent of assessed property values. The assessment limit dictates that property assessments are limited to an annual increase of two percent. However, an important element of this component is that property assessments reset to full market value upon a change in property ownership. By extension, changes in home ownership result in a significant increase in the allowable amount of property tax that can be collected. This legal stipulation creates variation among localities: cities with stable homeownership will be more constrained by Proposition 13 than cities with more homeowner volatility.

We see significant benefits to leveraging this heterogeneity for the purpose of causal inference. Utilizing cross-sectional municipal variation in addition to temporal variation mitigates for the issue of omitted variable bias. For one, it guards against the risk of history threats wherein alternative events omitted from an analysis are responsible for changes in the dependent variable. If data were analyzed only before and after the adoption of Proposition 13, it could be the case that an alternative policy or social change caused the shift toward alternative revenues rather than the limit itself.

Additionally, of significant concern to causal inference is the fact that the act of adopting Proposition 13 cannot be disentangled from negative public mood on taxation. This endogeneity problem makes it difficult to determine whether the shift to alternative nontax revenues was due to the policy or public mood. By analyzing heterogeneity in constraint, not simply policy adoption, we avoid this pitfall.

If Proposition 13 causes revenue substitution, then stable homeownership within municipalities will cause increased reliance on alternative revenues, in particular charges and fees. Alternatively, high homeowner mobility will decrease constraint from Proposition 13 and allow for continued reliance on property taxes. Observing the effect of homeownership stability both before and after passage of Proposition 13 is key to our research design. It is the differential effect of stability on revenue portfolios before and after adoption that is attributable to Proposition 13.

We conducted our test of revenue substitution using a multiple-equation model. We regressed a vector of dependent variables that measured reliance on various revenue sources on an interaction effect between homeowner stability and Proposition 13, including pertinent covariates. Revenue portfolio choices take the form of three dependent variables: sales taxes, charges and fees, and property taxes. Because we were interested in the mix of revenues collected by a municipality, we divided each of these revenue sources by a municipality's total own-source general revenues. This produced the proportion of revenues that come from each of these three sources.⁴

³ The termination of the long-form questionnaire in the 2010 Census necessitates that the time series ends in 2000 since sufficiently detailed city-level homeowner stability data was no longer available.

⁴ Multiple dependent variables with opposing directional hypotheses augment the internal validity of our research design. Using the language from Trochim (2001), this constitutes a "Pattern Matching Nonequivalent Dependent Variables" design.

Since our dependent variables take the form of proportions, we expect significant error correlations in the regressions. If unaccounted for, this could substantially affect the estimation of our coefficients and standard errors. To correct for this, we relied on Zellner's seemingly unrelated regression (Zellner 1962). This empirical strategy estimates separate linear regressions for each dependent variable while capturing shared error disturbances. This method has been analogously applied in analysis of seat-share proportions in multiparty elections (Tomz et al. 2002; Jackson 2002).

The key independent variable is homeowner stability. The decennial census provides a good measure for municipal-level changes to homeownership. The Census Bureau collects information on the date that a homeowner moves into his or her housing unit. From this information, we calculated the percentage of homeowners in a municipality who have owned their housing unit for more than five years.⁵ We refer to this variable as homeowner stability. Cities with high homeowner stability are the most constrained by Proposition 13 since total assessed values will be smaller than in cities with low homeowner stability.

Homeowner stability varies significantly over our time series and across municipalities. At the low end of the spectrum, only 4.5 percent of homeowners in the city of Cerritos, a suburban Los Angeles city incorporated in 1956, had owned their own homes for more than five years in 1970. In contrast, in the city of Compton in 1980, over 76 percent of homeowners had a five-year tenure. On average across California municipalities over our time series, 60 percent of homeowners maintained ownership of their homes for five years or more. In general, higher ownership stability is correlated with higher housing prices and personal incomes. We controlled for these socioeconomic differences in our model, but also relied on municipal fixed effects to account for cross-sectional differences between cities. Additionally, because the relationship between homeowner stability and municipal revenue choices was observed before the implementation of Proposition 13, we can account for this baseline relationship in our model.

We predicted that the correlation between homeowner stability and revenue portfolios would change after the passage of Proposition 13. Specifically, we predicted that high homeowner stability (implying greater constraint by Proposition 13) would become correlated with higher reliance on charges and fees following adoption. To establish this relationship, we interacted homeowner stability with a dummy variable indicating the implementation of Proposition 13 (taking the value of one in 1980, 1990, and 2000).⁶ This allowed us to interpret the differential effect of homeowner stability on revenue portfolios before and after Proposition 13, providing insight specifically into the effect of Proposition 13's assessment limit.

This empirical strategy also allowed us to extrapolate the effect of Proposition 13's rate limit. The constitutive terms of a multiplicative interaction effect are interpreted as the isolated effect of each variable when the other interacting variable is zero. When homeowner stability takes the value of zero, it assumes the precise counterfactual of Proposition 13 with only a rate limit in place. This is because assessed value would continuously reset to real market value in a universally unstable housing market.

⁵ The choice of a five-year time frame was arbitrary. The results in this paper hold when using a 10-year measure of stability.

⁶ Although Proposition 13 was only recently enacted in 1980, we still expected homeowner mobility to be consequential in this year. According to the original legislation, properties were reset to their 1975 values. As such, even if a property changed ownership only shortly after the adoption Proposition 13, the resulting reassessment to full and fair cash value would be significant.

To hold constant observable differences between municipalities, a variety of covariates were employed. Specifically, measures of personal income, home values, population, and homeowner-ship rates were used. Descriptive statistics for all employed variables appear in Table 1. We expected personal income to be positively correlated with sales taxes and charges and fees, but negatively correlated with reliance on property taxes since the latter is an income-inelastic revenue source. In contrast, we expected to see property tax reliance positively correlated with home values and home-ownership rates. No predictions were made about the relationship between population and revenue portfolio choices, but this variable was included because small cities and large cities undoubtedly have different revenue strategies. To hold constant unobservable differences between cities, city fixed effects were employed.⁷

Results

The results of the multiple equation regression analysis appear in Table 2. Looking first at the covariates, it is evident that city median income and population are correlated with revenue portfolio choices. Specifically, increases in median income are associated with greater reliance on charges and fees and less reliance on sales taxes. Increases in city population are also associated with reliance on charges and fees, seemingly at the expense of property taxes.

Proper interpretation of the interaction effect is crucial to our primary results. The Proposition 13 indicator should be interpreted as the effect of Proposition 13 when homeowner stability is set equal to zero. In practical terms, zero stability implies that property values would continuously reset to real market values, negating the effect of the assessment limit in its entirety. As such, it is possible to interpret this coefficient as the unitary effect of Proposition 13's rate limit.

The interaction term between homeowner stability and Proposition 13 can be used to gain insight into the effect of the assessment limit. On its own, the homeownership stability coefficient describes the relationship between stability and revenue portfolios prior to the passage of Proposition 13 (in 1970). The interaction term can be interpreted as how Proposition 13 affected this relationship. To ease interpretation, we calculated the linear predictions of stability before and after Proposition 13 while holding all other variables at their means. Differences can be attributed to constraint from Proposition 13's assessment limit. We expected the strongest effect when stability is high since higher stability augments the effect of the assessment limit. We begin the detailed interpretation of our results by looking at the effect on property tax reliance.

Since Proposition 13 directly limits property taxes, a reduction in property tax reliance should undoubtedly be observed. However, the coefficient on the Proposition 13 indicator variable is positive, though insignificant. Although this might appear counterintuitive, it is important to remember that this coefficient is only the estimated effect of Proposition 13's rate limit. The insignificance of this coefficient suggests that on its own the rate limit has not significantly reduced reliance on property taxes. This is less surprising when one takes into account the tremendous growth in property values over our time series. The growth of property values has easily exceeded the growth of revenue needs, negating the relevancy of a one percent rate cap. Indeed, numerous states adopted similar property tax rate limits in the nineteenth century, none of which are relevant today.

As the model shows, the true effect of Proposition 13 on property taxes comes from its assessment limit. This is evidenced by the differential effect of homeowner stability before and

⁷ The results presented in this paper hold without the use of city fixed effects.

Table 1.

Abrev.	Description	Source	Mean	S.D.	Min.	Max.
PROPERTY	Property Taxes as a % of General-Own Source Revenues	Historical Finance Database (Indfin)	22.19	9.87	0	59.99
CHARGES	Charges, Fees, and Miscellaneous Revenues as a % of General-Own Source Revenues	Historical Finance Database (Indfin)	38.47	12.04	13.01	79.17
SALES	Sales Taxes as a % of General-Own Source Revenues	Historical Finance Database (Indfin)	31.64	10.85	6.92	70.03
REVENUE	General Own-Source Revenues Per capita (Real, 2000 Dollars)	Historical Finance Database (Indfin)	713.7	487.4	18.0	4,127.3
INC	Median Family Income (Real, 2000 dollars, Thousands)	Decennial Census	53.43	13.93	26.26	117.57
HOME	Median Value of Owner-Occupied Housing (Real, 2000 dollars, Thousands)	Decennial Census	204.93	117.98	59.71	993.6
POP	Population (Hundreds of Thousands)	Decennial Census	1.54	3.51	0.16	36.95
OWN	% of Occupied Housing Owned	Decennial Census	55.24	11.78	20.94	86.47
PROP13	Proposition 13 Indicator	Authors' Coding	-	-	0	1
STABLEOWN	% of Homeowners with Tenure Greater than 5 yrs.	Decennial Census	59.72	10.41	4.5	76.23
STABLERENT	% of Renters with Tenure Greater than 5 yrs.	Decennial Census	17.29	7.44	1.19	48.87

after Proposition 13. Without Proposition 13, stability is strongly associated with property tax reliance. Specifically, a one percent increase in homeowner stability leads to a 0.43 percent increase in property tax reliance. After Proposition 13, the effect of homeowner stability is substantially attenuated. With the limit in place, a one percent increase in homeowner mobility leads to only a 0.22 percent increase in reliance on property taxes. This change is attributable to Proposition 13's assessment limit.

Table 2.

DV: Percentage of Own-Source Revenue From Property Taxes

INC	-0.04	(0.07)	
		(0.007)	
HOME	-0.003)	
POP	-1.90	(0.57)	***
OWN	-0.04	(0.12)	
PROP13	1.43	(4.11)	
STABLEOWN	0.43	(0.06)	***
PROP13 X STABLE-			
OWN	-0.21	(0.07)	***
Constant	22.87	(6.47)	***

DV: Percentage of Own-Source Revenue From Charges and Fees

INC	0.31	(0.09)	***
		(0.009)	
HOME	-0.005)	
POP	2.07	(0.73)	***
OWN	-0.07	(0.15)	
PROP13	-10.49	(5.28)	**
STABLEOWN	-0.42	(0.08)	***
PROP13 X STABLE-			
OWN	0.34	(0.09)	***
Constant	35.24	(8.33)	***

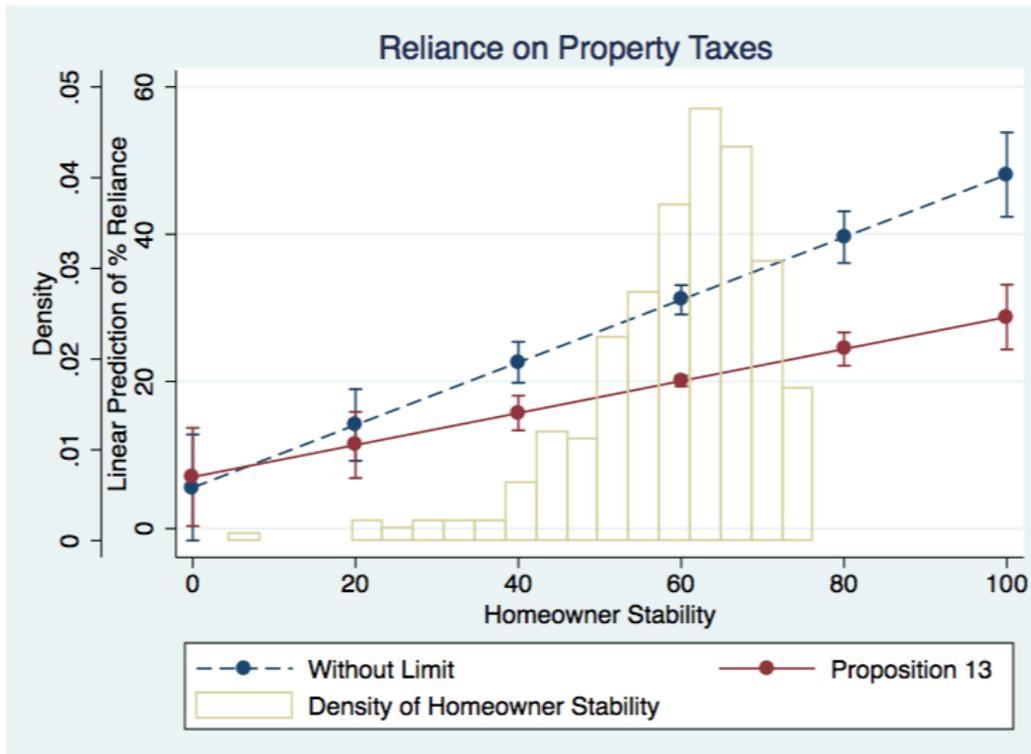
DV: Percentage of Own-Source Revenue From Sales Taxes

INC	-0.29	(0.08)	***
HOME	0.004	(0.01)	
POP	0.14	(0.65)	
OWN	-0.03	(0.13)	
PROP13	7.33	(4.68)	
STABLEOWN	0.19	(0.07)	***
PROP13 X STABLE-			
OWN	-0.14	(0.08)	*
Constant	30.69	(7.37)	***

Notes: City fixed effects included;
 standard errors in parentheses; number of observations is 365
 R-squareds range from 0.70 to 0.73;
 * p <= .1; **p <= 0.05; *** p <= 0.01.

Figure 2 illustrates the marginal effects of homeowner stability on property tax reliance with and without Proposition 13. The vertical caps represent 95 percent confidence intervals for the predictions. As one would expect, the effect of Proposition 13 on property tax reliance is strongest when homeowner stability is high. Whereas property tax reliance is indistinguishable with or

Figure 2.



without Proposition 13 with zero stability, property tax reliance changes by a full 20 percent margin with 100 percent homeowner stability, from 48 percent to 28 percent of general revenues.

We now turn to the interpretation of the effect of Proposition 13 on reliance on charges and fees. The results from Table 1 corroborate our predictions. The dichotomous variable indicating the implementation of Proposition 13 is once again insignificant, suggesting that the rate limit component of Proposition 13 is not the factor that has driven increased reliance on charges and fees. In contrast, the interaction term is positive and statistically significant. The linear predictions for this model are displayed in Figure 3. While the marginal effect of homeowner stability is negative both before and after Proposition 13, the slope of the relationship is more gradual following the implementation of Proposition 13. The maximal effect of the assessment limit, given a hypothetically 100 percent stable housing market, is a 23 percent increase in reliance on charges and fees (from 14 percent to 37 percent).

Finally, no meaningful effect of Proposition 13 on city reliance on sales taxes was identified. The coefficient for the interaction effect is indistinguishable from zero, as are the constitutive variables. Figure 4 displays the linear predictions for this model. As is clear in the figure, homeowner stability maintains a positive marginal effect both before and after implementation of Proposition 13. The linear predictions of sales tax reliance with and without Proposition 13 are statistically indistinguishable even at the maximal level of homeowner stability. The null hypothesis that Proposition 13 had no effect on reliance on sales taxes cannot be rejected.

Figure 3.

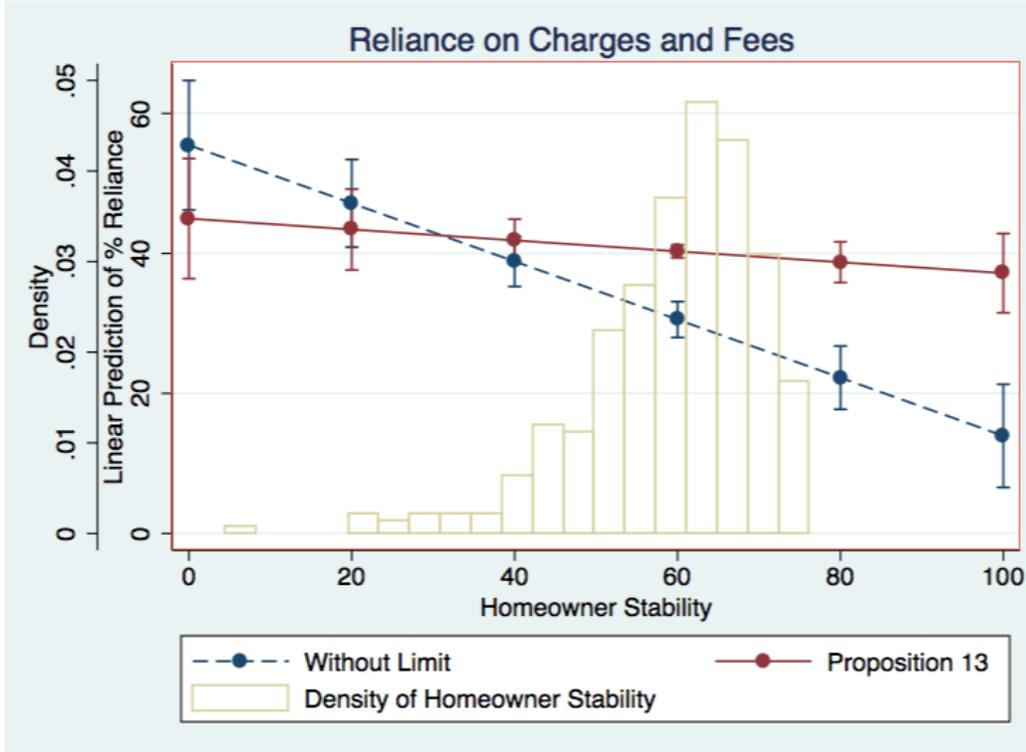
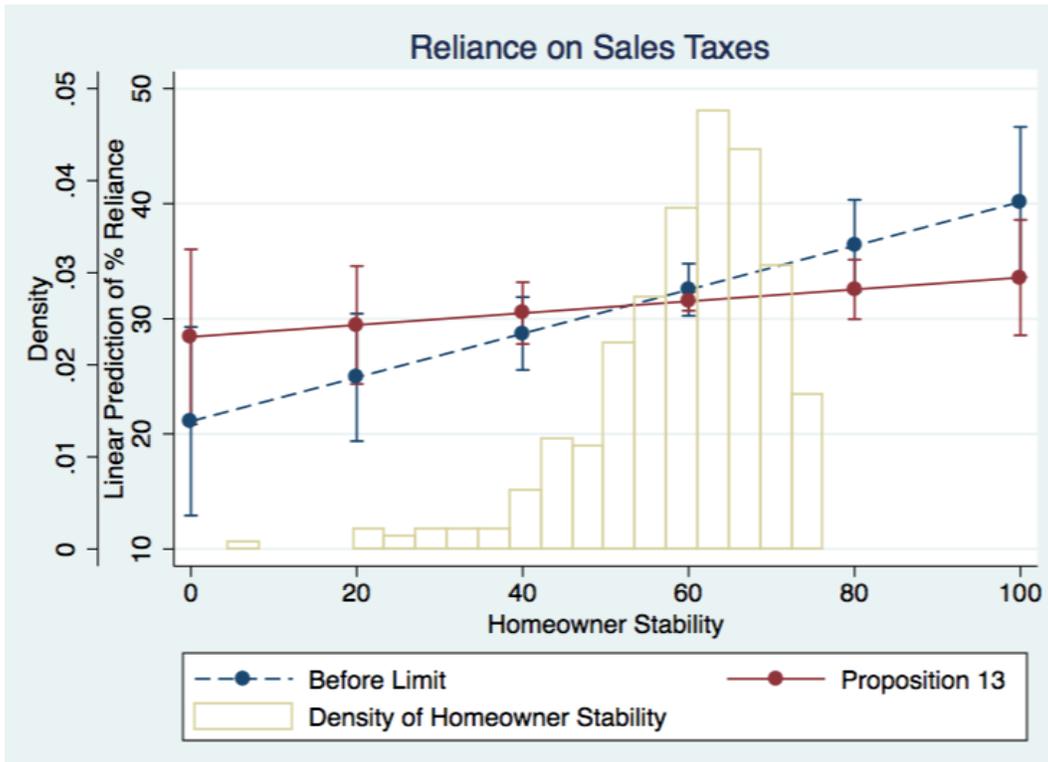


Figure 4.



It is useful to visualize these results together for a city with average (60 percent) homeowner stability. Figure 5 displays predicted revenue portfolios. If all other variables were set to mean values, we would expect a city with average homeowner stability to rely on property taxes for 30 percent of own-source general revenues in the absence of Proposition 13. With the limit in place, the average city would expect property taxes to be 20 percent of own-source general revenues. Given that the effect of the rate limit was insignificant, this suggests that for the average municipality Proposition 13's assessment limitation is responsible for a 10 percent reduction in the reliance on property taxes. For charges and fees, the average municipality without Proposition 13 would be 31 percent reliant on charges and fees. With the limit in place, that city would be expected to be 40 percent reliant on charges and fees. The results for sales taxes are indistinguishable, with an average city relying on sales taxes for 32 percent of own-source general revenues with or without Proposition 13.

Divergent Validity Test

To further bolster our claims of a causal relationship between Proposition 13 and revenue substitution, we employed an exercise of divergent validity. Divergent validity, also known as discriminant validity, tests whether concepts or measurements that are supposed to be unrelated are, in fact, unrelated. Here we argue that any relationship between *renter* stability and revenue portfolios should be unchanged by Proposition 13. This additional test further guards against the possibility that unobservable factors, not Proposition 13 constraint, explain our findings.

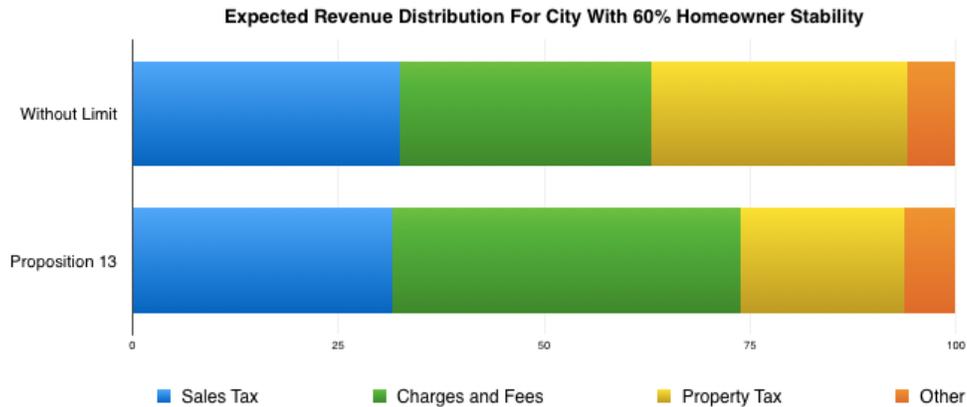
For example, a skeptic could claim that unobservable factors such as voter preferences are driving our findings. Specifically, this would be a problem for our analysis if homeowner stability became correlated with voter preferences for nontax revenues after the passage of Proposition 13. For example, this might occur through the conduits of economic or population growth or decline, variables that could simultaneously affect both stability and preferences. Although control variables for each of these constructs are included in the model, it is unreasonable to assume they are perfectly accounted for by our measures.

In order to safeguard against potential omitted variable bias, a test of divergent validity was conducted and the analysis extended to include renter stability. Renter stability should be positively correlated with any of the aforementioned omitted variables, but does not influence property taxation constraint due to Proposition 13. This new variable was constructed identically to homeownership stability, measuring the percentage of renters that lived in their housing unit for five years or more. As in our first analysis, this variable was interacted with the Proposition 13 indicator. Since assessed values of properties are not affected by renter stability, we did not expect this variable to affect revenue portfolios.

The results of this exercise bolster our conclusion that Proposition 13 constraint due to homeowner stability explains municipal increases in charges and fee reliance. As shown in Table 3, Proposition 13 did not affect the relationship between renter stability and reliance on charges and fees and sales taxes. There is a small differential effect for property tax reliance ($p = 0.094$), but this is only an artifact of the strong correlation between homeowner stability and renter stability. The effect goes away entirely when controlling for homeowner stability.⁸ In contrast, the effect of homeowner stability on revenue portfolios changed after adoption of Proposition 13.

⁸ Estimation not shown; available upon request.

Figure 5.



This exercise in divergent validity reduced the likelihood that omitted variables such as economic growth or migration drive our findings.

Revenue Totals

Our analysis shows that Proposition 13 substantially affected revenue portfolios. Cities more highly constrained by the limit were more likely to increase their proportion of revenues derived from charges and fees. However, this analysis leaves open the possibility that highly constrained cities decreased total revenues, and that the increase in charges and fees as a proportion of revenue was nothing more than a product of a decreasing denominator. A secondary analysis, using revenue per capita as the dependent variable, showed that this was not the case.

The determinants of real revenue per capita are analyzed in Table 4. Following the previous model specification, we interacted homeowner stability with a Proposition 13 indicator and included pertinent covariates and city fixed effects. This analysis supports the argument that Proposition 13 led to revenue substitution. Indeed, cities strongly constrained by Proposition 13 actually increased total revenue relative to less-constrained cities.

This finding is apparent in Figure 6, which displays the linear predictions of the model. At low levels of stability, revenue per capita is statistically indistinguishable with or without Proposition 13. In contrast, at higher levels when constraint from the limit should be at its maximal level, revenue per capita actually increases with the limit in place. This suggests that switching to alternative revenue sources permitted higher total revenue collections, in stark contrast to the claims of Brennan and Buchanan (1980) that TELs would impede a leviathan government even if revenue loopholes were discovered. The maximal effect of this increase (at 100 percent housing stability) is \$456. At the average level of stability, 60 percent, Proposition 13 led to an increase of \$143 per capita, all else equal.

Table 3.**DV: Percentage of Own-Source Revenue From Property Taxes**

INC	0.04	(0.08)	
		(0.007)	
HOME	-0.006)	
POP	-1.38	(0.60)	**
OWN	-0.12	(0.13)	
PROP13	-7.78	(2.17)	***
STABLERENT	0.40	(0.18)	**
PROP13 X STABLERENT	-0.27	(0.16)	*
Constant	43.14	(6.14)	***

DV: Percentage of Own-Source Revenue From Charges and Fees

INC	0.17	(0.10)	*
		(0.009)	
HOME	0.000)	
POP	1.48	(0.75)	**
OWN	-0.04	(0.16)	
PROP13	4.41	(2.72)	
STABLERENT	0.03	(0.22)	
PROP13 X STABLERENT	0.18	(0.20)	
Constant	16.10	(7.71)	**

DV: Percentage of Own-Source Revenue From Sales Taxes

INC	-0.20	(0.08)	**
		(0.008)	
HOME	0.002)	
POP	0.25	(0.65)	
OWN	-0.03	(0.14)	
PROP13	2.71	(2.35)	
STABLERENT	-0.05	(0.19)	
PROP13 X STABLERENT	-0.14	(0.17)	
Constant	38.49	(6.65)	***

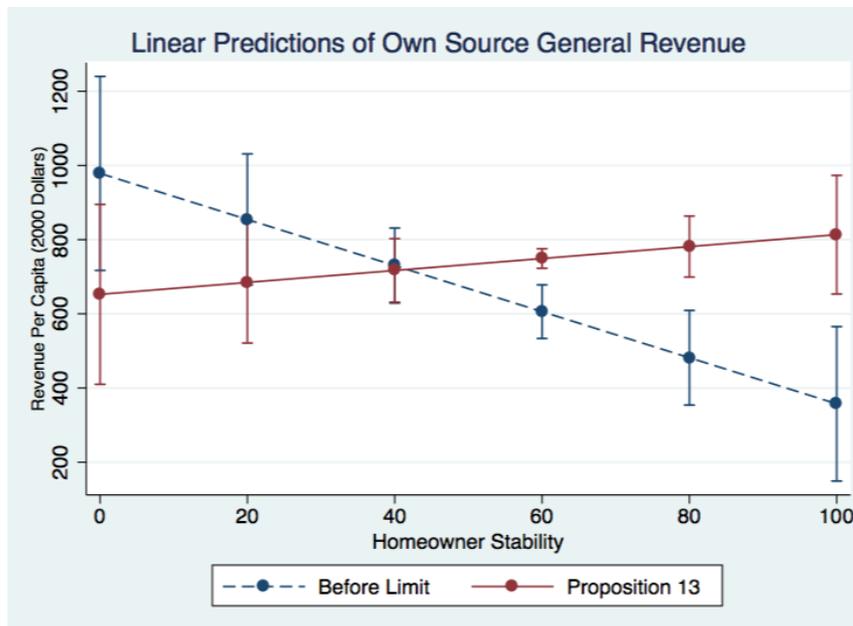
Note: city fixed effects included;
 standard errors in parentheses;
 number of observations is 365; R-squareds range from 0.70 to 0.72;
 * $p < .1$; ** $p < 0.05$; *** $p < 0.01$.

Table 4.

DV: Real Own-Source General Revenue Per-Capita			
INC	19.66	(2.56)	***
HOME	1.18	(0.25)	***
POP	53.81	(20.68)	***
OWN	6.67	(4.25)	
PROP13	-325.97	(149.24)	**
STABLEOWN	-6.21	(2.28)	***
PROP13 X STABLEOWN	7.82	(2.47)	***
Constant	-835.95	(235.27)	***

Note: city fixed effects included;
 standard errors in parentheses;
 * $p < .1$; ** $p < 0.05$; *** $p < 0.01$.

Figure 6.



Conclusion

Our results provide strong evidence for a causal link between the adoption of Proposition 13 and revenue substitution with charges and fees. We take advantage of heterogeneous degrees of constraint in California municipalities and show that there was stronger revenue substitution for municipalities with high constraint due to homeowner stability. Because the relationship between homeowner stability and revenue portfolios changed after the adoption of Proposition 13, the dif-

ference can be attributed to the law, not simply because municipalities with stagnant homeownership differ from those with greater homeowner mobility.

Our findings also suggest that the bulk of the substitution effect caused by Proposition 13 is attributable to the law's assessment limit, not its rate limit. We found that the assessment limitation component of Proposition 13 was responsible for a 10 percentage point reduction in reliance on property taxes and a nine percentage point increase in reliance on charges and fees, both statistically significant differences. No effects of the assessment limit were evident for municipal reliance on sales taxes.

We tested the validity of these findings by showing that Proposition 13 did not change the relationship between renter stability and revenue portfolios. This provides a test of divergent validity because renter stability does not affect Proposition 13's constraint, and as such should not be associated with revenue substitution.

Finally, we also confirmed that highly constrained cities were not simply decreasing total revenues. To the contrary, cities with high homeowner stability were more likely to increase total own-source general revenues after the passage of Proposition 13. This supports the claim that charges and fees and miscellaneous assessments can be a fruitful revenue source, and counters Brennan and Buchanan's (1980) original hypothesis that constitutional tax limitations must necessarily reduce overall revenues.

Unquestionably, Proposition 13 has had a wide array of secondary consequences. Although many may seem plainly attributable to the law, their magnitude and importance nonetheless deserve rigorous causal testing. The research design deployed in this paper advances the literature, and could be used to analyze a variety of other secondary consequences of Proposition 13 as well.

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