Fiscal Externalities of Transaction Taxes: Evidence from the Los Angeles Mansion Tax*

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Abstract

We estimate the fiscal externalities of a property transfer tax, the Los Angeles "Mansion Tax", on the revenues from property taxes when assessed values are closely tied to transactions. In California, as in over half of U.S. states, growth in tax assessments between transactions lags market values, so any reduction in transaction frequency reduces the growth of property tax revenue. Assuming that Measure ULA did not directly change transaction rates outside of Los Angeles, we estimate a sizable fiscal externality: in our benchmark calibration, the resulting property-tax revenue loss offsets about four-fifths of the revenue generated by the transfer tax, and under alternative parameter values the offset can range from 30 percent to over 100 percent. The net revenue loss is larger for high-value and commercial properties.

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1 Introduction

Tax systems often consist of multiple tax instruments over interdependent tax bases. The behavioral response to one tax can impact other tax bases even within the same jurisdiction. The presence of such fiscal externalities implies that the efficiency costs of a tax can be larger or smaller, depending on the sign and magnitude of the externality (Saez et al., 2012). In this paper, we present novel estimates of a previously unexamined, sizable fiscal externality between standard annual property taxes and transfer taxes on property sales. We examine how the introduction of Los Angeles' Measure ULA "Mansion Tax", a transfer tax on real property transactions above $5 million, interacts with the LA property tax base. Because California reassesses properties at sale, a transfer tax that discourages sales also delays reassessments that would otherwise raise the property tax base. We find that much of the revenue raised by the transfer tax is offset by its adverse impact on the property tax base.

By several measures, property tax is the least popular tax instrument. Revenue from the tax has declined as a share of all taxes raised and as a share of U.S. GDP as it has become subject to popularly imposed limitations (Cabral and Hoxby, 2012). California's Proposition 13, which limits tax reassessment growth between sales to 2 percent is a leading example of such limitations and has been replicated in over 19 other U.S. states (Higginbottom, 2010; Paquin, 2015; Horton et al., 2024).[1] All but five of these states reset assessments at the time of sale (Walczak, 2018; Haveman and Sexton, 2008).[2] At the same time, there is a competing desire for highly progressive taxes on the property tax base through alternative instruments such as a real estate transfer tax. More than 30 states impose a property transfer

[1] Fewer than half of U.S. states require assessments to occur annually, and nineteen states impose some form of statewide local assessment limit (Higginbottom, 2010; Paquin, 2015; Horton et al., 2024)

[2] These types of property tax systems are also known as *acquisition-value* property taxes since the assessed value is correlated with the sale price rather than the market value and they can theoretically depress transaction frequencies though to a lesser extent than a transaction tax. Conceptually, property transfer taxes can be viewed as an extreme variant of acquisition-value taxation: while the effective tax rate under acquisition-value systems decreases over time as property values appreciate without reassessment, transfer taxes apply exclusively at the moment of purchase, imposing a high instantaneous effective rate and zero thereafter. O'Sullivan et al. (1995) make this point and suggest that the transfer tax rate would have to be more than 10 times higher to raise the same revenue because of its chilling effect on transactions, which in turn raises the excess burden of a transaction tax.

tax. Globally, high-value transfer taxes have been introduced in some of the world's largest markets, including London, Toronto, and Tokyo (Chi et al., 2021). Our paper shows that combining these objectives of restraining traditional property tax while imposing a transfer tax on high-value property compounds the inefficiency of both taxes.

The interaction between Measure ULA and California's Proposition 13 provides an ideal context in which to demonstrate the link between assessment-limited property taxes and transfer taxes. Because California real estate values have grown significantly faster than Proposition 13's two percent statutory limit on assessment growth, the growth of the aggregate property tax base is highly sensitive to the frequency of transactions. The properties potentially subject to Measure ULA are also high-value properties, and therefore account for a disproportionate share of the reassessment-limited property-tax base. Our contribution is to show how a transfer-tax-induced change in transaction frequency maps into future property-tax revenue when reassessments are tied to sales. The conceptual point does not depend on a particular estimate of the transaction response: in an acquisition-value property-tax system, any wedge that reduces turnover also slows reassessment and therefore adds a fiscal externality to the usual welfare cost of the transfer tax. Measure ULA provides a natural setting to estimate the relevant transaction response and quantify the magnitude of this additional term. We estimate the causal impact of Measure ULA on reported transaction frequency, the reported property tax base, and revenue collection using detailed property records and variation created by the introduction of Measure ULA. Interpreting the empirical comparison as causal requires that Measure ULA did not directly change transaction rates outside Los Angeles, an assumption we discuss below. Under that assumption, our estimates imply that Measure ULA dampened the rate of transactions by about 50 percent, similar to effects in many other contexts where real estate transfer taxes have been imposed and evaluated (Best and Kleven, 2018; Kopczuk and Munroe, 2015; Slemrod et al., 2017; Dachis et al., 2012; Han et al., 2022).[3] As a result, our benchmark calibration implies that about

[3] A large literature analyzes transaction, stamp, and mansion taxes. Using notches in the UK stamp duty, Best and Kleven (2018) show substantial reductions in mobility and welfare. Kopczuk and Munroe (2015)

80 percent of Measure ULA revenue is offset by lower future property-tax collections; under lower baseline turnover or stronger behavioral responses, the offset exceeds 100 percent.

It is intuitive that a mansion tax would lead to less frequent property tax reassessments. However, quantifying the resulting revenue loss is not as straightforward as measuring the short-term change in property tax revenue and comparing it to the revenue raised by the mansion tax. First, a transaction generates mansion tax revenue in the year of sale, but a transaction delay decreases property tax revenue in every year of the delay—meaning that its full revenue effect cannot be captured in a single year. Comparing the two revenue streams requires a present value calculation that discounts both the gained and lost revenue to the present. Second, after a delay each deterred sale may eventually occur. When that happens, the change in reassessed value will be higher than it would have been because the market value will have appreciated over a longer time. The mansion tax therefore lowers reassessment frequency but also increases the change in property tax revenue conditional on reassessment, a channel that may be quantitatively important. For these reasons, we calculate the fiscal externality in present value terms using a formal model that captures the mechanical link between change in probability of a sale and change in reassessment value at time of sale.

To estimate the model, we need to know how the mansion tax reduced transaction frequency. Our main empirical specification is a Poisson difference-in-differences estimate of the change in the property transaction rate that resulted from the mansion tax. The specification compares parcels in the City of Los Angeles to parcels outside of the city but inside of Los Angeles County. We separately estimate the effect of Measure ULA on transaction frequencies after its passage in November 2022 and after its implementation in April 2023, in order to quantify anticipation effects of the law. We also show heterogeneity in responsiveness by property type and explicitly estimate time shifting in property transactions, since

document price bunching below New York's mansion-tax threshold, while Dachis et al. (2012) find similar distortions in Toronto. Evidence from Switzerland (Brülhart and Parchet, 2014) and Australia (Davidoff and Leigh, 2013) confirms that transfer taxes depress volumes and impede allocative efficiency.

we provide evidence that some transactions were moved forward into the period between announcement and implementation. Then we calibrate the model using the difference-in-differences estimate combined with other parameters that are either measured in the data or calibrated. The framework provides analytical formulas for the net revenue change, given the estimates. We perform sensitivity analyses varying the primitives to gauge how robust our estimates are to alternative growth, discount, and behavioral scenarios.

Our empirical design also requires that Measure ULA did not directly change transaction rates outside Los Angeles. We discuss this assumption and provide evidence on it below. The evidence does not point to a large spillover: sellers cannot relocate a Los Angeles parcel, placebo estimates show no offsetting rise in transactions outside the city, and buyer and seller identity checks do not show a shift toward non-Los Angeles markets. At the same time, these tests are necessarily imperfect. Buyers could substitute toward non-Los Angeles properties, and reduced supply in Los Angeles could induce some non-Los Angeles owners to sell. If these channels increased transaction rates outside Los Angeles, our estimates would overstate the decline in Los Angeles, and the implied fiscal offset should be interpreted as an upper bound with respect to that channel.

There are three main lessons from the calibration exercise. First, our benchmark estimates show that the net revenue raised by Measure ULA is substantially smaller if we include its offsetting effect of property tax revenue. Our preferred calibrations show a loss of most or all of the revenue from Measure ULA due to lower transaction frequency. The larger effects come from calibrations with a low turnover rate absent the tax or from calibrations with parameters that make its effects more severe. The second lesson is that the future path of house prices and discount rates could have large effects on realized revenue loss. Lower future house price growth would reduce the gap between assessed and market values, decreasing the loss in tax revenue. Third, we learn that the results are highly sensitive to the baseline rate of property transactions. A lower transaction rate absent the tax results in both a lower revenue from the mansion tax, and also larger reassessment lags, which are scaled up even more when the tax is in place.

Our paper adds to the empirical literature estimating sizeable externalities of taxation on related tax bases. In some cases, the externality offsets the direct behavioral effects on the narrow tax base as in the case of responses to the relative tax rates on personal and corporate income (Slemrod and Gordon, 2002; Slemrod, 1998; Sivadasan and Slemrod, 2008; Smith et al., 2022). In others, the externality exacerbates the direct revenue effects by shrinking a related tax base as in the case of out-migration of taxpayers (Agrawal et al., 2025). The relationship between property taxes and transfer taxes that we study is an example of the latter, all the more remarkable because some of the "externality" occurs *within* jurisdiction. Cross-jurisdictional externalities, where the tax rates set by one jurisdiction affects tax revenues in another, create welfare costs that are not internalized by the rate-setting jurisdiction (Marion and Muehlegger, 2014; Kleven, 2025; Bovenberg and de Mooij, 1994). We provide what, to our knowledge, the first empirical quantification of fiscal externality of transaction taxes to acquisition-value property tax systems.

Evidence on Los Angeles's Measure ULA is beginning to accumulate. Ling et al. (2022) provide a pre-election UCLA Lewis Center white paper arguing that the measure could raise substantial revenue for affordable housing with limited broader effects. Two subsequent UCLA Lewis Center reports take a more cautionary view. Phillips and Ward (2025) show that Measure ULA sharply reduced transactions for multifamily-zoned parcels and discuss the implications for housing production. Using a broader set of high-value transactions and a longer post-policy sample, Manville and Smith (2025) document persistent declines in turnover and explicitly note that lower transaction frequency can reduce property-tax revenues. Their longer post-period evidence is consistent with our interpretation that the post-ULA decline is not simply a short-lived retiming effect.

Our main contribution is to provide a way to quantify the fiscal externality of real estate transaction taxes when reassessments are tied to sales, and to show that this externality can be sizable. As with other existing estimates of the elasticity of real estate transactions with respect to a transaction tax, our elasticity estimates are necessarily short-run but fall in the middle of the range from previously published research Duranton et al. (2024). While we believe our elasticity estimate is accurate, one may plausibly expect that the elasticity may decrease or increase over time. Our model provides a way to plug in a preferred elasticity to estimate the implied fiscal externality. For any nonzero elasticity, the fiscal externality lowers the optimal transaction tax rate.

We provide details of Measure ULA and California Proposition 13 in Section 2. The present-value formulas we use to calculate revenue impacts and the implied excess burden are given in Section 3. We describe our calibration and data sources in Section 4. Section 5 presents the empirical strategy, causal estimates of the reduction in transaction frequency, and implied revenue impacts. Section 6 concludes.

2 Measure ULA and Property Tax in California

Measure ULA—the "Mansion Tax"—was passed by referendum in November 2022 and took effect on April 1, 2023 in the City of Los Angeles. It established a transfer tax on property sales $5 million and above.[4] The new tax rate was 4 percent on transactions between $5 million and $10 million, and 5.5 percent on transactions above $10 million, with thresholds regularly adjusted for inflation (see Table 1). Although nominally a mansion tax, Measure ULA applies to virtually all categories of real property—single-family residential, multifamily, commercial, industrial, and vacant land—and is remitted by the seller. Transfers to qualified nonprofits, affordable-housing developers and government entities are exempt.

Measure ULA operates in a statewide legal environment shaped by Proposition 13, which caps the annual property tax rate at 1 percent and limits the annual growth rate in assessed taxable value to the minimum of 2 percent in the absence of a sale.[5] Upon sale, properties

[4]The Measure ULA transfer tax is imposed in addition to the pre-existing real estate transfer tax, which has a much lower rate of 0.56 percent.

[5]Substantial improvements, such as new construction on a property, increase the taxable value of the improvement component of the property only. Proposition 13 still caps the assessment growth on the

are reassessed to market value and annual property tax payments on the property increase permanently. The consequence for local public finance is that growth in the property-tax base depends disproportionately on transactions that trigger reassessment.[6]

City budget documents expected Measure ULA to raise approximately \$672 million in its first fiscal year for subsidized housing and tenant assistance (City of Los Angeles, 2023). Actual receipts, shown in Figure 1, have been about half as large, likely due to the behavioral response to the tax and the increase in interest rates that occurred after the tax was introduced. Revenue raised through Measure ULA goes directly to the city government while property tax revenue is collected by the county and apportioned to local governments following legislated allocation rules. Because property tax revenue is allocated countywide, part of the property-tax revenue loss we estimate is borne outside the City of Los Angeles. Jurisdictions elsewhere in Los Angeles County are therefore unambiguously harmed by this inter-jurisdictional externality. Unlike Measure ULA revenue, the one-percent property tax supports multiple local public entities, including the county, school districts, and special districts. The fiscal externality is therefore not confined to the City's budget.

3 Net Fiscal Effect of Mansion Tax in Present Value Terms

The short-run effect of Measure ULA on property tax revenues in the years immediately after the introduction of the tax will be substantially smaller than the total long-run effect. The reason is that Measure ULA's annual impact on property tax revenues increases over time as the number of discouraged sales rises. For example, a decrease in annual transaction probabilities from 5% to 3% will reduce the likelihood that a given property transacts by

taxable value of the land at 2 percent.

[6]Our fiscal externality estimates will exclude property taxes not limited by Proposition 13, such as voter-approved debt and direct assessments (non-assessed value based). These are small and their exclusion makes the fiscal externality estimates conservative.

2 percentage points in the first year, by 3.8 percentage points over the first two years, and by 5.5 percentage points over the first three years.[7] Therefore, the expected difference in revenues rises over time. Rather than taking the naive approach of measuring only the short-run property tax effect, we calculate the total revenue effect implied by the mansion tax's causal effect on transaction frequency: a parameter we can estimate reliably in the short run. Together with other macro parameters that we calibrate using available data, we estimate both the revenue raised by the mansion tax and the lost property tax revenue in present value terms.

Consider a property whose market value M_t in year t evolves deterministically at rate $g > 0$ from an initial value M_0:

$$M_t = M_0 e^{gt}, \qquad t \geq 0. \tag{1}$$

Between sales, the assessed taxable value of the property only grows at the Proposition 13-capped assessment inflation rate $g_s < g$ and is given by:[8]

$$a_{th} = M_{t-h} e^{g_s h} \tag{2}$$

where h is the number of years since the property was last sold. Sales arrive according to a Poisson process with constant sale hazard p, so the holding period—the number of years since the property's last sale—has distribution:

$$h \sim \text{Exp}(p), \qquad f_H(h) = pe^{-ph}, h \geq 0. \tag{3}$$

Present value of property tax base. Let $k \equiv g - g_s > 0$ denote the wedge between market appreciation and statutory assessment growth. Then the expected assessed value of

[7] The change in the probability that a property will not have sold for t years when probability of a transaction falls from p to p' is $(1 - p')^t - (1 - p)^t$.

[8] We calibrate g_s to be the average change in assessments including changes in improvements.

properties at time t is

$$\mathbb{E}_t[a] = \int_0^\infty a_{th} f_H(h)\, dh = M_t \int_0^\infty e^{-kh} p e^{-ph}\, dh = M_t \frac{p}{p+k} = M_t \Phi(p). \tag{4}$$

Discounting future revenue at rate $r > g$, the present value of total annual property tax assessments is therefore

$$A \equiv \int_0^\infty \mathbb{E}_s[a] e^{-rs} ds = \frac{M_0}{r-g} \Phi(p). \tag{5}$$

where $\Phi(p) \equiv \frac{p}{p+k}$ is the present-value wedge between assessed and market values.

Present value of transaction tax base. The transaction tax base at time t is simply the market value of all properties that sell in year t: pM_t. The present value of the transaction tax base is therefore:

$$T(p) \equiv \int_0^\infty p M_s e^{-rs} ds = p \frac{M_0}{r-g} \tag{6}$$

Comparing equations (6) and (5), we see that $A = \frac{T}{p+k}$ showing the direct link between the property tax base and the transaction tax base.

Net fiscal change. The property tax base depends on the distribution of time-since-last-sale among all properties. When the mansion tax is introduced, the rate of transactions changes from p to p', which also changes the distribution of time-since-sale but only gradually. Ignoring transitional dynamics for the time being and assuming that property tax rolls adjust instantly to the new hazard rate, the net change in revenue under a transaction tax rate of τ and property tax rate θ can be given by:

$$\Delta R = \tau T(p') - \theta \Delta A \tag{7}$$

where

$$\Delta A = \frac{M_0}{r-g}\big[\Phi(p) - \Phi(p')\big]. \tag{8}$$

Including the transitional dynamics, whereby the distribution of holding periods for properties slowly adjusts to the distribution under p', the full change in present value of property tax assessments is given as:

$$\Delta A^{full} = \frac{M_0}{r-g}\Phi(p) - M_0\left[\frac{\Phi(p')}{r-g} + \frac{\Phi(p) - \Phi(p')}{r-g+p'+k}\right] \tag{9}$$

Using these formulas, we can calculate the fiscal externality in terms of the property tax revenue loss due to the introduction of the transfer tax. We express these as a share of revenue raised by the transfer tax.

More generally, we can consider what the estimated elasticity of transactions with respect to the transfer tax implies for the marginal excess burden of a transfer tax, including its effects on property tax revenue, and the implied revenue-maximizing transfer tax rate. The formulas for the marginal excess burden and revenue maximizing rates are as follows. See Appendix D for details of derivation.

Defining ϵ^T and ϵ^A as the elasticities of the two tax bases with respect to the net of transfer tax rate $(1-\tau)$ and denoting the property tax rate as θ, the marginal excess burden, defined as the change in revenue coming from the behavioral response for each additional dollar of revenue raised is given by:

$$-\frac{\mathrm{d}B}{\mathrm{d}R} = \frac{\tau\epsilon^T + \theta\epsilon^A(p+k)^{-1}}{(1-\tau(1+\epsilon^T)) - \theta\epsilon^A(p+k)^{-1}}. \tag{10}$$

And, the revenue-maximizing rate is as follows:

$$\tau^* = \frac{1}{1+\epsilon^T} - \frac{\theta\epsilon^A}{(p+k)(1+\epsilon^T)}. \tag{11}$$

From equation (6) we see that $\epsilon^T = \epsilon^p$ since M_0, r and g are unaffected by the transaction tax, where ϵ^p is the elasticity of transactions with respect to the transfer tax rate. From equation (5), substituting in $A = \frac{T}{(p+k)}$, we see that $\epsilon^A = \epsilon^T - \Phi(p)\epsilon^p$. We estimate ϵ^p using a difference-in-differences empirical strategy exploiting the introduction of the LA mansion tax.

The model can accommodate other channels that depress $A(p')$. First, if the sellers bear some of the burden of the transfer tax or if prices "bunch" around the \$5mn threshold, transaction prices may fall. Under full price pass-through, we would replace $\Phi(p')$ with $(1-\tau)\Phi(p')$ in Equation (9), lowering the long-run tax base by 4%. The lower tax-exclusive sale price will lower the property tax base going forward by the degree of price adjustment. This capitalization channel reinforces the transaction-frequency channel in our benchmark calculation, but we do not include it in the benchmark because post-ULA high-value transactions are too sparse to estimate the price response precisely. Second, the transfer tax can deter capital improvements by effectively raising the user cost of owning property, thereby lowering property values and the tax base even further.

4 Mapping Theory to Data

We can now use equations (7), (8) and (9) to calculate the decrease in property tax revenue and the revenue generated by the LA mansion tax.

To do so, we calibrate or estimate the necessary parameters using data assembled from LA county assessor records, deeds data from CoreLogic, and commercial property transactions from CoStar (CoreLogic, 2025; Los Angeles County, 2024; CoStar, 2025). Our primary estimation sample consists of a monthly parcel-level panel from January 2020 through December 2023. We use CoStar as a complementary commercial-property sample: it extends the series of high-value commercial transactions through December 2024 and provides buyer and seller identities used in the spillover checks. LA county assessor data for fiscal years 2020 to 2023 on all land parcels within the county provides assessed land and improvement values, base-year assessment dates, and land-use codes for each parcel. We identify arm's-length sales, classify parcels as residential, multifamily, commercial or other based on use codes, and flag parcels inside LA city. We complement this parcel-level data with exact dates of property transfer using CoreLogic deeds data. We treat a deed as an arm's-length sale when it is recorded as a grant, warranty, or trustee deed and is not identified as a quit-claim or related-party conveyance. Matching deeds to assessor parcels by parcel number and month yields a unique link for almost 88% of assessor-identified transfers between 2020 and 2023. Unmatched assessor-identified transfers are not counted as dated sale events; in the monthly panel, parcel-months without a matched dated sale are coded as zero sales. We extend our sample post-2023 through December 2024 for large commercial transactions using CoStar data, which provides high-quality coverage for commercial property transactions above $5 million.

After excluding Santa Monica and Culver City, which have separate transaction taxes, the analysis encompasses about 2.35 million taxable parcels, of which about 788,000 lie within the City of Los Angeles. From January 2020 to December 2023 we observe 309,242 matched arm's-length sales, 2.49% of which exceed $5 million. See Appendix B.4 for transaction counts pre- and post-tax. Table 2 includes the mean of the variables used in the main analysis. Finally, we validate our constructed count of taxable transfers and benchmark our revenue calculations using monthly Measure ULA revenue reports published by the Los Angeles Office of Finance. We match 89% of Measure ULA remittances to properties (see Appendix Figure A1).

Using this data, we calibrate our key parameters as follows:

1. k (wedge between market and assessed growth): We estimate a regression with data on property sales: $\log(1 - \Delta a_i/M_i) = -kh_i + u_i$, where h_i is the number of years since the last sale for parcel i and Δa_i is the reassessment jump net of drift at rate g_s.

2. g_s (between-transactions growth in assessed values): For parcels that did not transact,

we calculate the mean logarithmic growth of assessed values.[9]

3. p (sale hazard pre-Measure ULA): We impute the market value of each property by multiplying its assessed value by $\exp(kh_i)$. The probability of sale in each period, p_t, is calculated as the number of properties sold with value above \$5 million divided by the total stock of properties with imputed values above the same amount. We set p as the average of p_t over 2022 assessment rolls.[10]

4. p' (post-Measure ULA sale hazard): We model the policy as having a proportional effect on the sale probability. We set $p' = p \exp(\hat{\beta})$ where $\hat{\beta}$ comes from our Poisson difference-in-differences estimate.

5. A_{pre} (Size of the existing assessed base): We calculate the sum of the 2022 assessed values for parcels whose imputed market value exceeds \$5 million.

6. $r - g$ (Discount-growth spread): Our benchmark sets $r - g = 4.75\%$, motivated by the benchmark multifamily cap rate reported in CBRE Research (2026); Appendix Figure B3 varies this spread directly.

5 Estimating Measure ULA-induced change in transaction frequency

5.1 Empirical Strategy

Measure ULA applies to transactions within LA city to property transactions above \$5 million, creating a notch in tax liability at \$5m. These transactions form our "treated"

[9] Proposition 13 caps g_s at two percent per year; the data confirm this median rate, while the mean is 2.98% percent because of improvement-induced reassessments.

[10] We think that the 2022 transaction probability is a reasonable measure of the long-run tax-free transaction frequency that excludes other direct effects of 2022 interest rate increases. For example, this abstracts away from mortgage rate lock that temporarily lowered property frequencies starting in 2023 (Liebersohn and Rothstein, 2025).

group whose frequency is lowered by the tax. As one would expect, the notch creates bunching of sale prices at that threshold (see Figure A2) to avoid the mansion tax but unlike most evasion or avoidance responses that affect both property and mansion tax bases, this response reduces the property tax base by less than the absence of a transaction.[11] For the purpose of estimating the fiscal externality, therefore, we ensure that they are included in our count of post-ULA transactions to avoid overestimating the effect on property tax revenue. We measure transaction volumes including observations precisely at the cutoff by defining treatment as Price $\geq$ 4.9 million; later we will show that the results are robust to changing the cutoff. A property that would have sold just above $5 million but instead sells just below the threshold still transacts and still triggers reassessment. Relative to a sale that disappears altogether, bunching changes the future property-tax base much less. By counting these near-threshold transactions in the treated group, we treat them as transactions that still trigger a reassessment, our estimates remain conservative.

Measure ULA leaves two types of transactions in our data where the tax does not apply: transactions below the five million dollar range within the City of Los Angeles and high-value transactions outside of LA City but within LA County.[12] We exploit these excluded groups in a difference-in-differences framework estimated via Poisson regression to estimate the Measure ULA-induced decrease in likelihood of transactions. Panels c and d of Figure 2 show the time-series of the number of transactions in these different treatment and control categories since 2020, and panels a and b show the corresponding value of net reassessed tax values. Property transactions below $5m within LA city are on different seasonal and time trends as compared to transactions above $5m within the city, so these comparisons will require seasonal adjustment. High-value transactions outside Los Angeles city, which were not subject to Measure ULA, display similar seasonality and trends (red series in panel A). The main concern with the latter group, which we adopt as our main control, is that the

[11] While these would be straightforward to quantify, bunching analysis has been the focus of previous work by (e.g., Best and Kleven, 2018); we focus on the effect on property tax revenues rather than behavioral bunching responses.

[12] Excluding Santa Monica and Culver City as we note in Section 4.

sample of high-value properties outside of LA is not that large, which creates noise in the estimates. We will re-create the same treatment and control groups using data on high-value commercial properties from CoStar, which has a longer sample available.

An immediate concern with using LA county outside of the city as a control group is that the city tax could directly cause transactions to be shifted to these control regions. We test for these spatial spillovers below and in Appendix B.5, and find little evidence that they occur.

Another consideration is the extent to which the mansion tax can be avoided or evaded, and how such responses affect our estimates of the fiscal externality. Many types of evasion/avoidance have isomorphic effects on the fiscal externality as real responses. For example, if the true value of a transaction is underreported, the impact on both property tax revenue and mansion tax revenue is the same as if the property were transacted for a lower value in reality. The fiscal externality and total revenue effect that we estimate are a result of both real and evasion responses that nevertheless have the same impact on revenue and do not need to be distinguished. Where the distinction between real and evasion/ avoidance response matters for our estimates is in cases where only one tax base is affected, which by construction creates no externality. The main such concerns are splitting sales into multiple, smaller transactions or reporting lower transaction value to stay below the $5 million mansion tax threshold. We discuss several possible avoidance/ evasion strategies, whether they would result in different effects than real responses in greater detail, and provide evidence on relevant evasion responses in Appendix E. While taxpayers are likely evading and avoiding these tax obligations, what we will show is that evasion of the type that would affect the accuracy or interpretation of our estimates was not prevalent.

We work at the parcel-month level and let Sale_{izt} be an indicator that equals one if parcel i located in zip code z transfers in month t. The Poisson pseudo–maximum-likelihood specification is

$$\mathbb{E}[\text{Sale}_{izt} \mid X_{izt}] = \exp\Big(\alpha + \beta\,\text{Treat}_i \times \text{Post}_t + \gamma\,\text{Treat}_i \times \text{Anticip}_t + \lambda_z + \delta_t\Big) \tag{12}$$

where Treat is an LA-city indicator, λ_z are time-invariant zip-code fixed effects and δ_t are calendar-month fixed effects. All specifications include month-of-year dummies interacted with the Treat fixed effect to purge seasonality. The sample is limited to \$4.9mn+ sales. Post_t turns on in April 2023, while Anticip_t is one from October 2022 through March 2023 to capture accelerated closings. The coefficient of interest is β; under parallel trends and the no-spillover assumption, $\exp(\beta)$ is the post-policy sale hazard for treated parcels relative to the pretax baseline. We weight by each parcel's lagged assessed value and standard errors are clustered at the zip-code level. In the event-study, the single post indicator is replaced by event-time dummies omitting 2020 monthly indicators as the reference period.

The validity of this interpretation requires two assumptions. First, treated and control parcels would have followed parallel trends in the absence of Measure ULA. Second, Measure ULA did not directly affect transaction outcomes for control-group parcels through spillovers; this is the no-spillover component of the stable unit treatment value assumption (SUTVA) in our setting. We validate parallel trends using the event-study in Figure 3 and placebo estimates for parcels unaffected by the tax. We address potential SUTVA violations using the spatial-spillover and buyer- and seller-switching exercises reported below and in Appendix B.5.

5.2 Results

Table 3 presents our estimates of the change in transaction frequency. Under the assumptions above, the benchmark estimate in column 1 is -0.683, implying a 49% drop in the sale hazard for \$5 million+ parcels after April 2023.[13] Equal weighting (column 3) produces -0.297, a

[13] $\exp(-0.683) - 1 = -0.495$.

26% decline, suggesting larger properties are more sensitive to the tax.

Figure 3 charts the monthly coefficients where both the parallel pre-trends and anticipation effect in the lead up to implementation are apparent. From April 2023, the weighted series shows a large decline in transaction volumes throughout the observed post-period, although later-month estimates are attenuated and the unweighted estimates are noisier. We therefore view the figure as supporting, rather than proving, a persistent effect; the more formal timing-shift exercises below ask how much of the initial decline can be explained by retiming. The parallel pre-trends assumption is further supported by placebo tests in columns 4 and 5 of Table 3 —above $4.9 million-valued versus below $4.9 million-valued parcels outside the city, and below $4.9 million-valued parcels inside versus outside—show post coefficients near zero. These placebos are also important for helping assess inter-jurisdictional spillovers, which we discuss further below.

Timing-shift diagnostics Our results show a large increase in transactions between the announcement of the mansion tax and its implementation, indicating that there were likely transactions moved forward in time to avoid the tax. If there is substantial time shifting from the months immediately after implementation, the short-run effects of the tax on transaction frequencies might be different than the long-run effects, leading us to overestimate the effects using the available data.

The most transparent way of dealing with this concern is simply to drop the first four post-implementation months. If the initial decline were mostly transactions pulled forward from April through July 2023, removing those months should make the estimated post effect smaller in magnitude. It does not. Column 2 of Table 3 instead yields -0.826, a 56 percent decline in the sale hazard, compared with -0.683 in the full sample. Dropping the first four post-implementation months therefore does not reveal reversion toward zero; the estimated post effect is, if anything, larger in magnitude. This finding implies that the time-shifted sales are spread out over many years and there is little short-run reversion. This exercise cannot rule out further attenuation outside the sample period, so we treat the calibration as conditional on the post-period evidence available.

The CoStar sample lets us ask a slightly different question because it extends through 2024. In the parcel panel, we drop April–July 2023 to check whether the estimate is driven by the first few months after implementation. In the CoStar sample, we can instead drop the rest of 2023 and ask whether the decline remains in 2024.

We compare several ways of asking whether the large initial post-implementation decline reflects temporary retiming or a persistent fall in transaction frequency. Different methods yield similar results. Our second approach allows the "excess" transactions pre-implementation to be shifted forward from any date after implementation. It assumes that the probability that a transaction is retimed is decreasing exponentially over time. We estimate the exponential function from the post-ULA event-study dynamics. Spreading the excess transactions over time using the estimated functional form tells us how many of them would have occurred during the observed post-event window, and therefore how much of the initial decline should be interpreted as temporary time shifting rather than the long-run change in sale hazard. Formally:

Let $k = 0$ denote April 2023 and index subsequent months by $k = 1, 2, \ldots$. We model the change in the monthly sale hazard as

$$\Delta p_k = \underbrace{\Delta_\infty}_{\substack{\text{long-run decline} \\ \text{(object of interest)}}} + \underbrace{\Gamma e^{-\delta k}}_{\substack{\text{exponential adjustment} \\ \text{calibrated to excess mass}}} .$$

where Δ_∞ is the long-run decrease in the rate of transactions, δ determines the rate of convergence to the long-run, and Γ determines the number of "missing" transactions after tax enactment. The five-month anticipation window generates cumulative "excess mass" in the anticipation region $\sum_{k=-5}^{-1} \widehat{\Delta p_k}$. Setting the excess mass equal to the number of missing transactions leaves two free parameters, (Δ_∞, δ), which we estimate by nonlinear least squares on the post-April coefficients. In the main sample, the observed post effect is -0.495

in proportional units and the estimated long-run effect is $\widehat{\Delta_\infty} = -0.482$, with a 95% confidence interval of $[-0.817, -0.234]$. The estimated reversion parameter is essentially zero, although it is imprecisely estimated. The point estimates therefore imply little evidence of economically meaningful reversion over the horizon we observe. Because the reversion parameter is imprecisely estimated, however, we do not use this nonlinear decomposition as the benchmark. Put differently, if some transactions were shifted forward into the anticipation window, they appear to have been shifted over many periods, with a small change each period, so our short-run estimates are close to the long run effect in the observed data.

A third piece of evidence that the initial decline reflects the long-run decline in transactions comes from the longer panel on commercial transactions from CoStar data. Estimating our main specification (12) on this sample yields a coefficient of -1.87 in the weighted specification and -0.55 in the unweighted specification. Repeating the nonlinear least squares time-shifting exercise on the CoStar panel yields an implied long-run effect of -0.434 in proportional terms, or -0.569 log points when mapped to the log specification, with a bootstrapped standard error of 0.116, and again estimates essentially no reversion. Thus the longer CoStar panel leads to the same substantive conclusion as the parcel-level sample: there is little sign that the initial post-ULA decline is followed by a rebound toward the pre-policy transaction rate.

A final piece of evidence comes from previous research. Manville and Smith (2025) use a longer panel of assessment data and likewise find a persistent decline in turnover.

A related caveat is that some sellers may have perceived Measure ULA as temporary. If sellers expected the tax to be repealed or modified, they may have delayed transactions until a lower-tax future date. This would make the observed post-implementation decline larger than the steady-state response to a fully permanent tax. We cannot separately estimate this channel, but the formulas below allow smaller permanent transaction responses to be substituted directly into the calibration.

Taken together, these exercises point to the same conclusion. Different ways of handling time shifting imply very similar long-run effects, and all suggest that any retimed transactions were drawn from much farther in the future than the first few post-implementation months. These exercises do not prove that the effect is permanent; rather, they show that the available post-period evidence does not support a complete rebound over the observed horizon. This interpretation is also consistent with the longer post-period evidence from CoStar and with other recent work studying Measure ULA over a longer horizon. For the benchmark calibration below, we therefore use the raw weighted DD estimate from Table 3, Panel A, column 1. It yields an estimate similar to the nonlinear time-shifting adjustments, while avoiding reliance on an imprecisely estimated nonlinear reversion parameter.

Property-Type Heterogeneity Using LA-county assessed use codes, which describe each parcel's primary assessed use, we examine variation in transaction tax elasticity by property type.[14] Table B2 presents the results. Commercial properties are the most responsive and show the largest drop in transactions (Column 3). Single and multifamily housing show smaller drops in transaction rates of about -.293 (Column 1 and 4). We use these property-type specific estimates in our sensitivity analysis of calibrated revenue estimates in Section 5.3.

Spillovers Our main estimate of transaction decline relies on the assumption that Measure ULA did not directly affect the rate of transactions among high-value properties outside of LA city. This is the no-spillover component of the stable unit treatment value assumption (SUTVA) in our setting. Several observations limit this concern. First, the nature of real property makes geographic substitution difficult for sellers: a seller wishing to dispose of a specific building in Los Angeles cannot relocate it to a neighboring jurisdiction. The main concern is therefore that buyers could substitute toward properties outside Los Angeles, or

[14] The property-type splits are constructed from the Los Angeles County Assessor Secured Roll land-use (use) codes, which describe each parcel's primary assessed use rather than municipal zoning designations. We therefore interpret the "multifamily" column as capturing parcels assessed as multi-unit residential at the time of sale (e.g., existing apartment buildings), regardless of the set of uses permitted by the underlying zoning.

that reduced supply in Los Angeles could induce some owners outside Los Angeles to sell. The placebo estimates in Table 3 provide evidence against large transaction-count spillovers. We find no relative change in transaction rates between high- and low-value properties outside LA city, nor do we find any change among low-value properties inside versus outside LA city (columns 4 and 5).

We also take a more direct approach to test for buyer and seller substitution away from LA city. We use buyer and seller information from CoStar data to follow firms that were active in the commercial LA high-value market before Measure ULA. In Appendix Figure B2, we plot the share of transactions involving such buyers or sellers inside versus outside of LA city and find no persistent increase outside LA after Measure ULA. These findings indicate that even among this sophisticated group of market participants, there is no relative shift to markets outside the city. For the full sample where we do not observe buyer or seller identities, we track whether limited-liability entities, who are more likely to be repeat-buyers and developers sensitive to Measure ULA and more easily able to switch locations, become more active outside LA city. Appendix Figure B1 again suggests that this was not the case. The share of purchases by LLCs outside LA city looks very similar before and after Measure ULA.

If Measure ULA increased transactions in the control group, our difference-in-differences estimate would overstate the decline in treated LA transactions. As an upper bound, we can consider a one-for-one increase in control group transactions given the decrease in the treatment group; if this were the case, the true elasticity would be half of what we estimate. The placebo results could still be consistent with an increase in demand for properties outside LA city that is reflected only in prices. Such a price-only spillover would not affect the internal validity of our transaction-rate estimate, and would affect property-tax revenue outside Los Angeles rather than the Los Angeles revenue channel we study.

5.3 Calibration and present-value estimates

Table 4 summarizes the parameters estimated or backed out of the administrative data as described in Section 4. We estimate that assessed values increase between sales by roughly 2.9 percent, slightly higher than the statutory maximum due to other reassessment triggers like improvements ($g_s = \ln 1.0298 \approx 0.0293$). Nevertheless, assessed values trail market values of properties by roughly 2 percentage points per year ($k = 0.0214$), and market value grew at a rate of $g = 0.0508$ on average. For \$5 million–plus parcels the baseline probability of sale in 2022 was $p = 0.0485$, and applying the raw weighted DD estimate from Table 3, Panel A, column 1 under the no-spillover assumption implies a post-ULA sale probability of $p' = 0.0245$. Since the present-value formulas depend on the spread $r - g$ rather than on r and g separately, we calibrate this spread directly using a 4.75 percent benchmark multifamily cap rate (CBRE Research, 2026). We use these values as our benchmark calibration and then show how alternative, realistic values affect the results. The calibration is intended to make the mapping from transaction responses to fiscal effects transparent: alternative estimates of the transaction response can be substituted directly into the formulas above, and Appendix Table A1 reports uncertainty around the benchmark estimate.

Table 5 presents the implied revenue effects on both standard property-tax revenue and Measure ULA, conditional on the no-spillover assumption, calculated using equations (6), (8), and (9) multiplied by the respective tax rates. We first consider the benchmark scenario: the lost property-tax present value is \$5.6 bn while Measure ULA generates \$6.9 bn, so the property-tax loss equals 80 percent of Measure ULA revenue and the net present-value effect is \$1.4 bn. Transaction rates were especially high in 2022, so using 2021 to estimate the baseline hazard produces a larger loss and a slightly negative net revenue effect. Using the 2021 sales probability yields a loss equal to 110.7 percent of Measure ULA revenue. To give a sense of the range, setting $p = 1.5\times$ baseline yields an offset of 52.7 percent, while $0.5\times$ baseline yields 141.6 percent. Appendix Table A1 reports bootstrap confidence intervals for the benchmark row.

We can reasonably expect interest rates and market growth rates to vary over time. Higher interest rates weigh short-term revenues more relative to long-term. We consider a larger $r - g$ spread of 5.75%, which weights short-term revenues more and reduces the fiscal externality. A higher baseline p also lowers it. Thus a given proportional drop in sales matters most when turnover is already low—important if high interest rates continue to suppress transactions through lock-in effects (Liebersohn and Rothstein, 2025).

Halving the sales elasticity reduces the property-tax loss to \$2.9 bn and raises ULA PV to \$9.7 bn, so 30.2 percent of new revenue is offset. A 1.5-times response lowers ULA PV to \$4.9 bn and increases the loss to \$7.8 bn, producing a \$2.9 bn net deficit. The sensitivity rows show how the fiscal externality changes under smaller or larger transaction responses. Even when the sales elasticity is halved, the reassessment channel remains quantitatively important.[15]

Allowing the transaction frequency to vary across property types leaves the benchmark offset nearly unchanged: splitting the tax base by property type (single-family, multifamily, commercial, other) raises the loss to \$6.2 bn while ULA PV is \$7.7 bn, leaving a net gain of \$1.5 bn and an offset of 80.6 percent. Dividing parcels into deciles of years since last sale—capturing the larger assessment gaps for long-held properties—yields a \$3.0 bn loss and \$5.1 bn in ULA PV, an offset ratio of 59.6 percent.

The calibration can also help us understand the dynamics of the fiscal externality. The change in present-value of property tax is dominated by the long-run in each scenario (column 2) while the short-run adjustment period has only a small offsetting effect (column 3). Also relevant is the fact that the gains from each taxed transaction occur immediately, whereas the losses from deferred transactions accumulate in each year without a sale. Therefore, on net, the long-run impact on total revenue is worse. Considering only annual revenue effects in the immediate future would be akin to assuming a very high government discount factor.

[15] A fifty percent reduction in estimate is helpful as a lower-bound given complete spatial spillovers: If every reduced transaction in Los Angeles resulted in a new transaction outside of Los Angeles, the difference-in-differences estimator would yield a coefficient roughly double the true effect, given that both markets are of a similar size at baseline (Butts, 2021).

A higher discount rate places greater relative weight on short-run mansion tax gains and smaller relative weight on long-term losses, leading to a lower fiscal externality. Appendix Figure B3 traces this relationship under benchmark parameters: at $r - g = 6\%$ the offset is 70.9 percent, while at $r - g = 12\%$ it falls to 45.2 percent.

To calculate the marginal excess burden, we convert the difference-in-differences estimate into a constant elasticity with respect to the net-of-tax rate $(1 - \tau)$. Along with the benchmark calibrations, equation (10) implies that net marginal revenue is negative at $\tau = 4\%$ (the denominator is below zero), so the long-run marginal excess burden at the statutory rate is not well-defined. The corresponding revenue-maximizing transfer-tax rate is approximately 1.5%, well below the statutory 4%. Existing estimates of transfer-tax deadweight loss capture the costs of distorting transactions, prices, mobility, or tenure choice (Dachis et al., 2012; Best and Kleven, 2018; Besley et al., 2014; Han et al., 2022). The reassessment channel adds a separate fiscal cost. Whatever one takes to be the direct excess burden of the transfer tax, an acquisition-value property-tax system adds the further loss of property-tax revenue from delayed reassessment. This additional term is the object of our calibration.

6 Discussion and Conclusion

Interpreting the transaction estimates under the no-spillover assumption discussed above, we show that the revenue raised by the new Los Angeles Mansion Tax (Measure ULA) will be substantially offset by its dampening effect on property-tax revenues in the long run. Measure ULA reduces the frequency of transactions by about 50%, which lengthens the holding period of properties and therefore the length of time when growth in taxable value lags market-value growth. The benchmark calibration implies that the resulting property-tax loss is about 80 percent of the revenue raised by the mansion tax. Reasonable alternative parameter values yield offset ratios ranging from about 30 to 159 percent, depending on assumptions about the transaction probability absent the tax, the discount-growth spread, and the size of the behavioral response. If Measure ULA increased transactions outside Los Angeles, the estimated transaction decline in Los Angeles and the resulting offset ratios should be interpreted as upper bounds with respect to that channel.

Two channels are absent from the benchmark model that could potentially make our estimates conservative. First, we do not model capitalization of the transfer tax into transaction prices beyond some descriptive evidence on price pass-through shown in Appendix Section B.1 and Figure A4. In the transfer-tax literature, capitalization is typically measured through pass-through of the tax into observed sale prices; many studies find substantial (and sometimes close to full) pass-through to sellers.[16] The tax reduces transactions so much that estimating these price changes with precision is infeasible. As it lowers sales prices, the tax further reduces revenue raised and also reduces the future property tax base, resulting in even less net revenue than we estimate. A second channel is that it may cause reductions in investment, with similar effects.

The fiscal externality arises because of the *interaction* of the transfer tax and Proposition 13. In 2020, a California-wide ballot measure proposed to remove Prop 13 restriction on commercial properties above $3 million in value; the measure failed but a majority of Los Angeles county voted in its favor, suggesting a desire to tax high-value property. The restrictiveness of Proposition 13 forces local governments to seek alternative, less efficient means of raising revenue than an annual property tax. Measure ULA allows policy-makers to act on the apparent desire to raise the effective tax on high-value commercial properties, but at the cost of a substantial fiscal externality. Easing Proposition 13 limits could lower the marginal cost of public funds by allowing for more flexible tax instruments. Note that in the Californian context, the externality is also inter-jurisdictional as mansion tax revenue accrues to the city government while the property tax losses are borne by LA county as a whole. This interaction also has a political-economy dimension: Measure ULA produces visible earmarked revenue for housing programs, while the foregone property-tax revenue is

[16] See, for example, Dachis et al. (2012), Besley et al. (2014), Best and Kleven (2018), and Kopczuk and Munroe (2015)

less visible, spread across countywide beneficiaries, and realized gradually.

The findings have broad relevance for policy makers wishing to raise revenue from taxing property. First, stricter assessment limits between sales amplify the effects of the transaction tax, so the fiscal externality of a transaction tax will be larger in states such as Florida and Michigan where assessments are strictly capped like in California. Second, transaction volumes on commercial and high-value properties are more responsive to the tax; exempting commercial properties would reduce the amount of fiscal externality. Third, extent of externality depends on the economic environment. If transaction frequencies are already low — as they have been since 2022 — the effects will be larger.

Prior work on transaction taxes has largely focused on how these taxes affect transaction volume, prices, welfare, and the revenue raised by the transaction tax itself. Our analysis highlights an additional fiscal externality: when another tax base is reassessed at sale, any tax or wedge that reduces turnover can lower future revenue from that tax base. This logic is not specific to Measure ULA. It applies more broadly to tax systems in which multiple tax instruments interact through a common behavioral margin.

During the preparation of this work the authors used ChatGPT 5.5 in order to copy-edit the writing and check code for errors. After using this tool/service, the authors reviewed and edited the content as needed and take full responsibility for the content of the published article.

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7 Figures

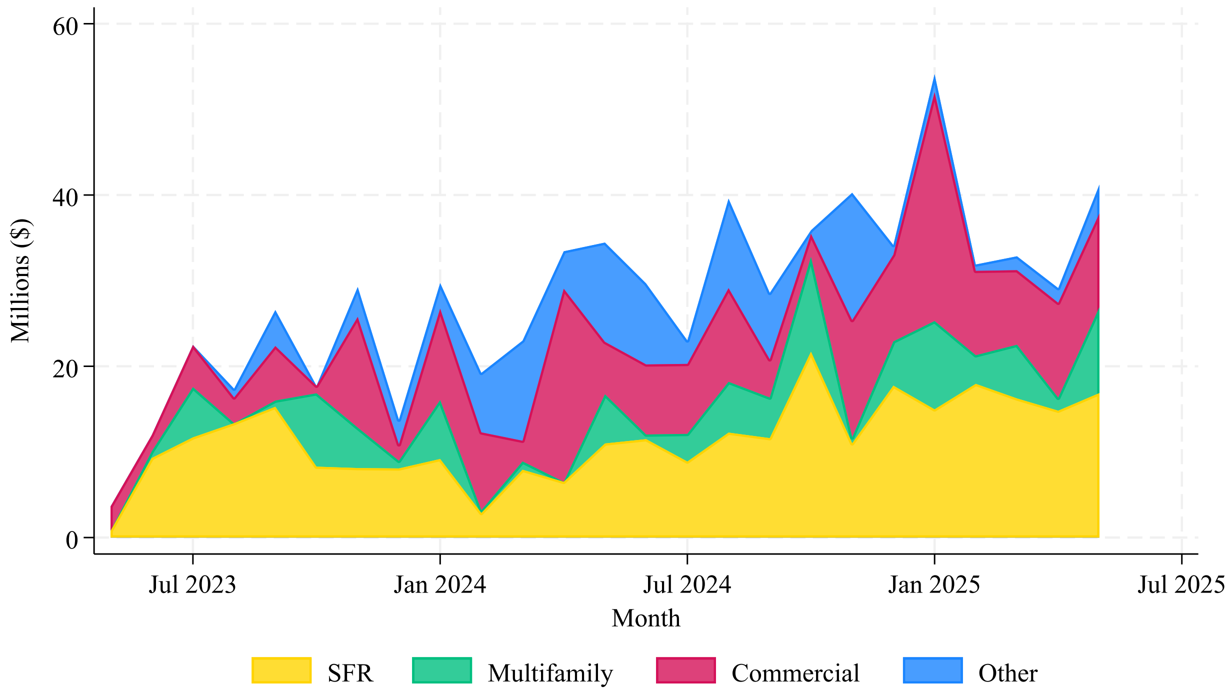
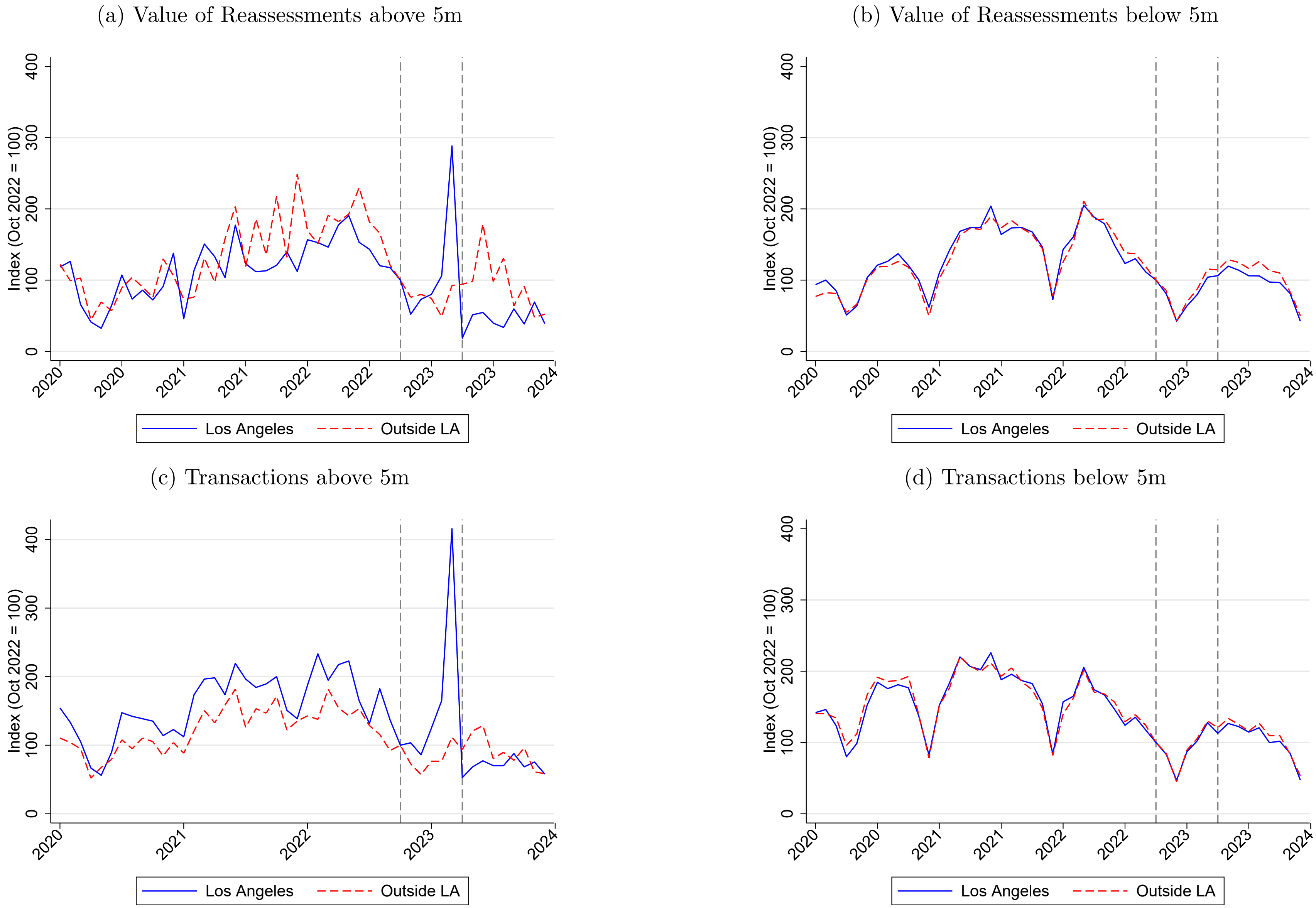


Figure 1: Reported Revenue Raised from Measure ULA

Notes: Monthly transaction tax revenue collected under Measure ULA by broad property type after the passage of the measure reported by The Los Angeles Office of Finance.

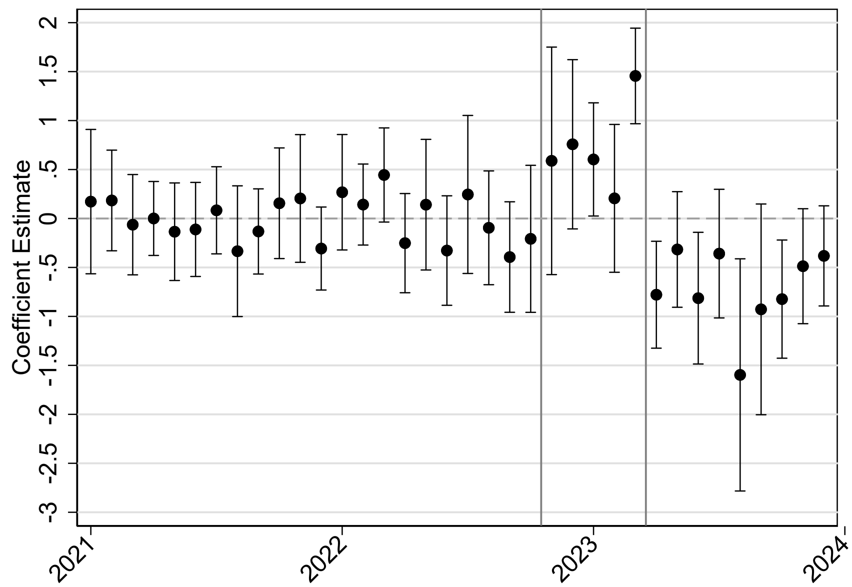
Figure 2: Monthly Value of Property Tax Reassessments and Transaction Counts



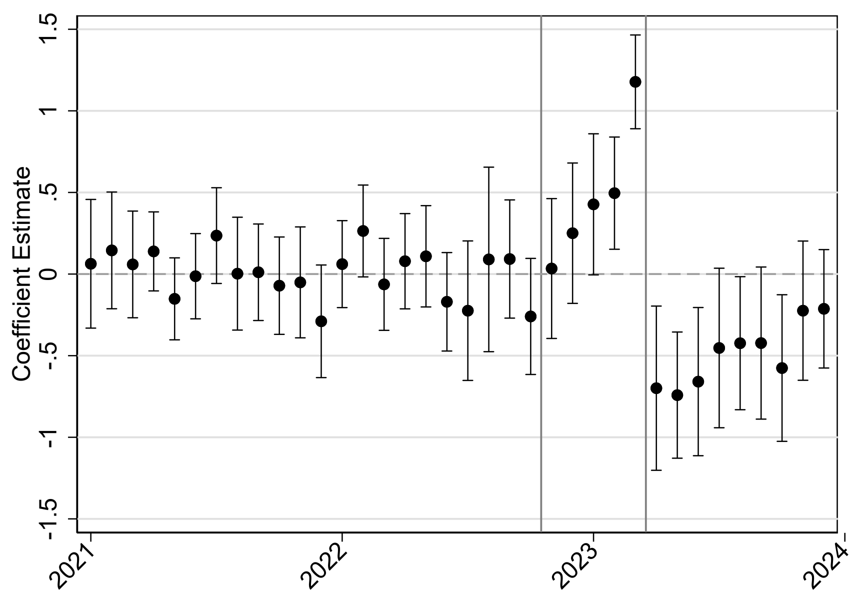
Notes: Panels (a)–(b) plot reassessment values and panels (c)–(d) plot transaction-count indexes for sales above and below \$5 million. Blue solid line is LA city; red dashed line is LA County outside the city. Each series is normalized to 100 in October 2022. The vertical lines mark Measure ULA passage in November 2022 and implementation in April 2023.

Figure 3: Impact of Measure ULA on Monthly Transaction Probability

(a) Weighted by lagged assessed value



(b) Unweighted



Notes: This figure plots event-study coefficients estimated by replacing the indicator Post_t in Specification (12) with dummies for each month omitting months between January-December 2020 as the baseline period. We include zip-code fixed effects, calendar-month fixed effects, and standard errors clustered at the zip level. The sample is January 2020–December 2023 and includes properties with sale value $\geq$ \$4.9 million. Treat_i is an indicator for whether the parcel is inside LA city. Anticip_t is an indicator for the calendar months October 2022 - March 2023 between shortly before Measure ULA passage and its implementation. The first vertical gray line denotes Nov 2022 when Measure ULA passed, the second gray line denotes Measure ULA implementation in April 2023. Panel (a) are our main estimates where we weight observations by lagged assessed value. Panel (b) is unweighted.

8 Tables

Table 1: Measure ULA (Mansion Tax) Rates and Other Real Estate Transfer Tax in LA City by Property Value over Time

Year	**Value of Property**	**ULA Rate (%)**	**Tax Rate (%)**
Apr 2023-Jun 2024	$\$100; \leq \$5,000,000$	0	0.45
	$> \$5,000,000; < \$10,000,000$	4	4.45
	$\geq \$10,000,000$	5.5	5.95
Jul 2024-Jun 2025	$\$100; \leq \$5,150,000$	0	0.45
	$> \$5,150,000; < \$10,300,000$	4	4.45
	$\geq \$10,300,000$	5.5	5.95
Jul 2025-Jun 2026	$\$100; \leq \$5,300,000$	0	0.45
	$> \$5,300,000; < \$10,600,000$	4	4.45
	$\geq \$10,600,000$	5.5	5.95

Notes: The ballot measure was voted on November 8, 2022 and implemented on April 1, 2023. Each July, the thresholds are inflation-adjusted. The City of Los Angeles base rate is $2.25 for every $500 or fractional part thereof. This does not apply to the Measure ULA rate calculations, which are percentage-based.

Table 2: Characteristics of Taxable Property Sales in LA County

Variable	City of LA			Outside City of LA		
	Pre	Anticipation	Post	Pre	Anticipation	Post
Assessed value at sale ($ mn)	1.71	1.91	1.46	1.26	1.19	1.20
Current-to-prior assessed val ratio	3.41	3.27	3.55	3.68	3.64	3.81
Assessed val above $4.9 mn = 1	0.04	0.06	0.02	0.02	0.02	0.02
Single-family property = 1	0.81	0.79	0.82	0.85	0.84	0.85
Multifamily property = 1	0.13	0.13	0.13	0.06	0.06	0.06
Commercial property = 1	0.03	0.04	0.03	0.04	0.04	0.04
Other property type = 1	0.03	0.03	0.03	0.05	0.06	0.06
# obs	23,031	9,245	15,121	40,497	16,137	28,440

Notes: This table reports average characteristics of taxable property sales in the City of Los Angeles and in the rest of Los Angeles County before the policy period (January 2022 to September 2022), during the anticipation period (October 2022 to March 2023), and after implementation (April 2023 to December 2023). Each observation is a sold parcel matched from deed records to the Los Angeles County assessor roll. "Assessed value at sale ($ mn)" is the assessed value of the property at the time of sale, measured as land value plus improvement value and reported in millions of dollars. "Current-to-prior assessed val ratio" is the ratio of the current assessed value to the parcel's prior assessed value in the assessor roll. "Assessed val above $4.9 mn = 1" is an indicator for whether the property's assessed value exceeds $4.9 million, the threshold used to define the treated segment of the market. The next four rows report indicators for the parcel's property type: single-family, multifamily, commercial, or other. Average assessed values, reassessment ratios, and property-type shares are broadly similar inside and outside the City of Los Angeles.

Table 3: Difference-in-differences estimates of Measure ULA's effect on transactions

	(1)	(2)	(3)	(4)	(5)
Panel A: Main specification and placebos					
Post × Treat	-0.683^{***}	-0.826^{***}	-0.297^{**}	0.005	-0.109
	(0.169)	(0.235)	(0.118)	(0.143)	(0.080)
Anticipation × Treat	0.560^{***}	0.547^{***}	0.441^{***}	0.122	-0.019
	(0.193)	(0.194)	(0.080)	(0.131)	(0.080)
Specification	Baseline (weighted)	Drop Apr–Jul 23	Unweighted	Placebo (\$4.9mn+ vs $\leq$ \$4.9mn, outside LA)	Placebo ($\leq$ \$4.9mn, LA vs outside LA)
# obs	382,045	350,169	353,557	425,456	487,797
Panel B: Robustness and CoStar evidence					
Post × Treat	-0.651^{***}	-0.647^{***}	-0.550^{***}	-1.865^{***}	-0.335^{***}
	(0.160)	(0.169)	(0.098)	(0.411)	(0.130)
Anticipation × Treat	0.534^{***}	0.534^{***}	0.343^{***}	-0.278	0.333^{***}
	(0.186)	(0.194)	(0.113)	(0.392)	(0.109)
Specification	Lower cutoff (\$4.5 M)	Sample $\geq$ 2021	CoStar unweighted	CoStar weighted	CoStar unweighted excl. May–Dec 23
# obs	437,596	286,624	271,044	268,104	290,236

Notes: Each column reports a separate estimation of specification (12), with the variation in each column described in the row titled "Specification." The unit of observation is a parcel-month. The dependent variable in every regression is an indicator for whether parcel i transacts in month t. The coefficient on Post × Treat is the main effect of Measure ULA on transaction probability in LA city relative to outside LA after Measure ULA is implemented. The coefficient on Anticip × Treat estimates the relative increase in transaction rates during the anticipation window, October 2022 through March 2023. All specifications include zip code and month fixed effects, and month-of-year × treatment fixed effects. Except where noted, specifications are weighted by lagged assessed value. Our benchmark estimate is in column (1) of Panel A. In column (2) we drop the four months immediately following implementation (April–July 2023) to account for potential re-timing of transactions. Column (3) removes weighting by value. Columns (4) and (5) are placebo difference-in-differences estimates comparing large- versus small-value properties outside LA and small-value properties inside versus outside LA. In Panel B, column (1) lowers the treatment cutoff to \$4.5 million. Column (2) drops 2020 from the sample. Columns (3) and (4) use the CoStar sample described in Section 4, with column (3) unweighted and column (4) weighted by lagged assessed value as in the main specification. Column (5) uses the unweighted CoStar sample and excludes May–December 2023, so the post-period effect is estimated from 2024. Property-type heterogeneity estimates are reported separately in Appendix Table B2. Standard errors clustered by zip code are in parentheses. Stars denote significance at the 10% (*), 5% (**), and 1% (***) levels.

Table 4: Calibration targets and parameter values

Symbol	Definition	Calibrated value
k	Gap between market value and assessed value growth, $k = g - g_s$	0.0214
g	Realized market-value growth of high-value stock	0.0508
g_s	Assessed-value growth between sales ($\ln 1.0298$)	0.0293
p	Pre-ULA annual sale hazard for \$5 M+ parcels in 2022	0.0485
p'	Post-ULA annual hazard ($p \exp \beta$)	0.0245
p^{2021}	Pre-ULA annual sale hazard for \$5 M+ parcels in 2021	0.0337
p'^{2021}	Post-ULA annual hazard ($p^{2021} \exp \beta$)	0.0170
θ	Property tax rate	1.0%
$r - g$	Benchmark discount-growth spread	4.75%
A_{pre}	2022 assessed base of LA \$5 M+ parcels	\$233.1 bn

Notes: This table reports the benchmark calibration targets used in the present-value calculations. The wedge k is estimated from the relationship between reassessment jumps and time since last sale. The assessed-value growth rate between sales, g_s, is the mean log growth in assessed values for parcels that do not transact, and market-value growth is set to $g = g_s + k$. The pre-ULA sale hazard p is measured as the 2022 transaction rate among City of Los Angeles parcels with imputed market value above \$5 million, where market value is imputed by scaling assessed value by $\exp(kh)$. The post-ULA hazard p' is constructed as $p \exp(\hat{\beta})$, where $\hat{\beta}$ is the raw weighted DD estimate from Table 3, Panel A, column 1. The alternative 2021 hazard values p_{2021} and p'_{2021} are defined analogously using 2021 sales rates. A^{pre} is the total 2022 assessed value of City of Los Angeles parcels with imputed market value above \$5 million. The property tax rate θ is fixed at California's 1 percent statutory rate, and benchmark discounting is summarized by the spread $r - g$, set to 4.75 percent based on the Green Street Advisors cap-rate benchmark, as reported by CBRE research. See Section 4 for additional details.

Table 5: Revenue Effects in Present Value terms (billions of 2025 $)

Scenario	Lost PV	Long-run	Transitional	ULA PV	Net effect	% loss
Benchmark	5.6	11.3	-5.8	6.9	1.4	80.4
ULA PV $312 mn yr	5.6	11.3	-5.8	6.6	1.0	84.7
Alternative baseline p (2021)	6.0	13.5	-7.5	5.5	-0.6	110.7
1.5× baseline p	4.9	8.9	-4.0	9.3	4.4	52.7
Half baseline p	6.4	15.4	-9.0	4.5	-1.9	141.6
Higher $r-g$ by 1 pp ($r-g$=5.75%)	4.2	9.4	-5.2	5.7	1.6	72.6
Softer response ($\beta/2=-0.34$)	2.9	5.4	-2.5	9.7	6.8	30.2
Stronger response ($1.5\beta=-1.02$)	7.8	17.3	-9.5	4.9	-2.9	158.5
Type heterogeneity	6.2	15.8	-9.6	7.7	1.5	80.6
Lock-in heterogeneity	3.0	6.9	-3.9	5.1	2.1	59.6

Notes: Table shows the present value (PV) of the components of the net revenue change from Measure ULA: ULA transfer-tax revenue and lost property-tax revenue, with the latter broken into long-run and transitional components. The first row presents estimates under the benchmark calibrated parameter values in Table 4. Each row below varies parameter values to consider alternative scenarios. "Alternative baseline p (2021)" uses the baseline sale hazard estimated in 2021. "Type heterogeneity" allows p and β to vary by property type. "Lock-in heterogeneity" allows p and β to vary by deciles of years since last sale. "ULA PV $312 mn yr" uses transfer-tax revenue implied by annualizing actual collections observed from April 2023 through December 2024. The remaining rows vary p, β, or the discount-growth spread $r-g$ as labeled. "Long-run" loss is the permanent drop in the steady-state property-tax flow, while "Transitional" loss captures the one-time shortfall during the housing stock's convergence to the new steady state. "Net effect" equals ULA PV minus total property-tax loss; "% loss" is the ratio of total property-tax loss to ULA revenue. All PVs are expressed in billions of 2025 dollars. Discounting is summarized by the scenario-specific spread $r-g$.

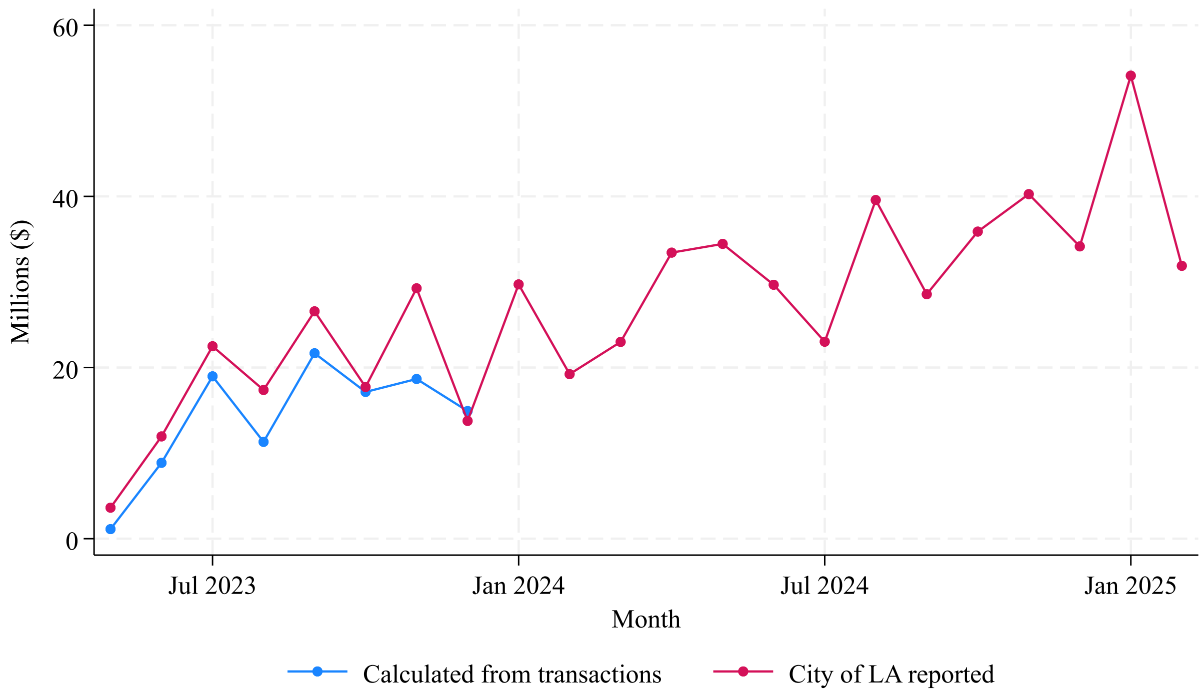
A Supplementary Exhibits

Table A1: Benchmark Present-Value Revenue Effects

Scenario	Lost PV	Long-run	Transitional	ULA PV	Net effect	% loss
Benchmark (raw DD effect)	5.6	11.3	-5.8	6.9	1.4	80.4
95% CI (bootstrap)	[3.0, 8.5]	[5.6, 17.7]	[-9.5, -2.6]	[4.2, 12.7]	[-2.7, 6.9]	[31.9, 167.8]
80% CI (bootstrap)	[3.8, 7.2]	[7.4, 15.5]	[-8.5, -3.5]	[4.5, 9.4]	[-1.7, 4.9]	[48.7, 134.3]

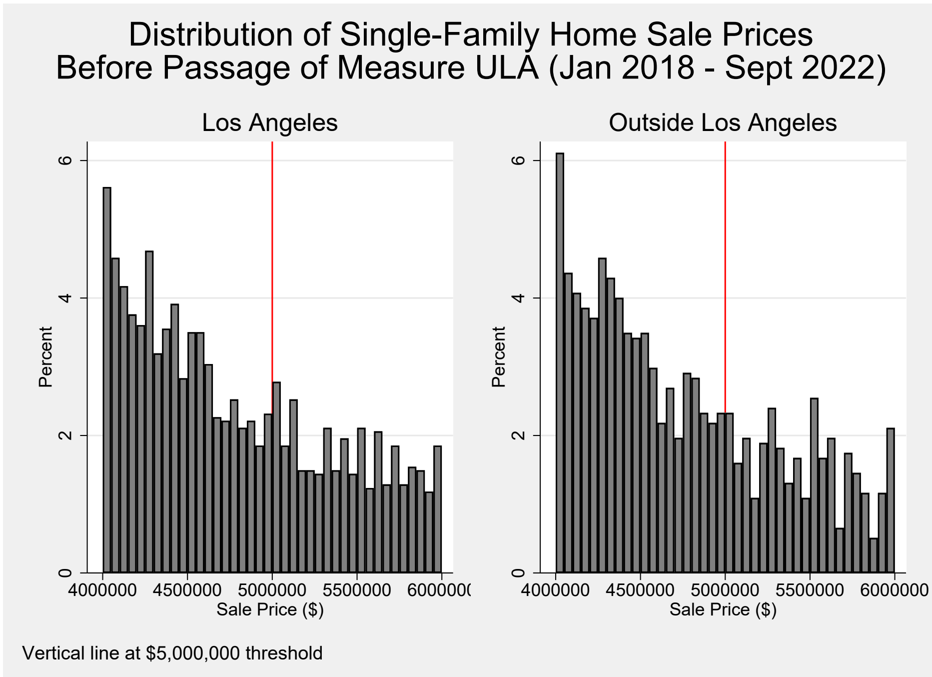
Notes: Point estimates use the benchmark calibration formulas from Table 5 with the raw weighted DD estimate from Table 3, Panel A, column 1, and benchmark discount-growth spread $r - g = 4.75\%$, set from the Green Street Advisors cap-rate benchmark. Confidence intervals come from a zip-level weighted bootstrap of the full benchmark procedure with 100 successful draws. In each draw, we re-estimate the weighted DD coefficient and the benchmark calibration primitives k, A_{pre}, and p_{pre}, while holding the benchmark spread fixed. Bootstrap SE for the headline post effect is $\beta_{\text{benchmark}} = -0.68$ (SE 0.16). PV entries are billions of 2025 dollars.

Figure A1: Revenue raised from Measure ULA

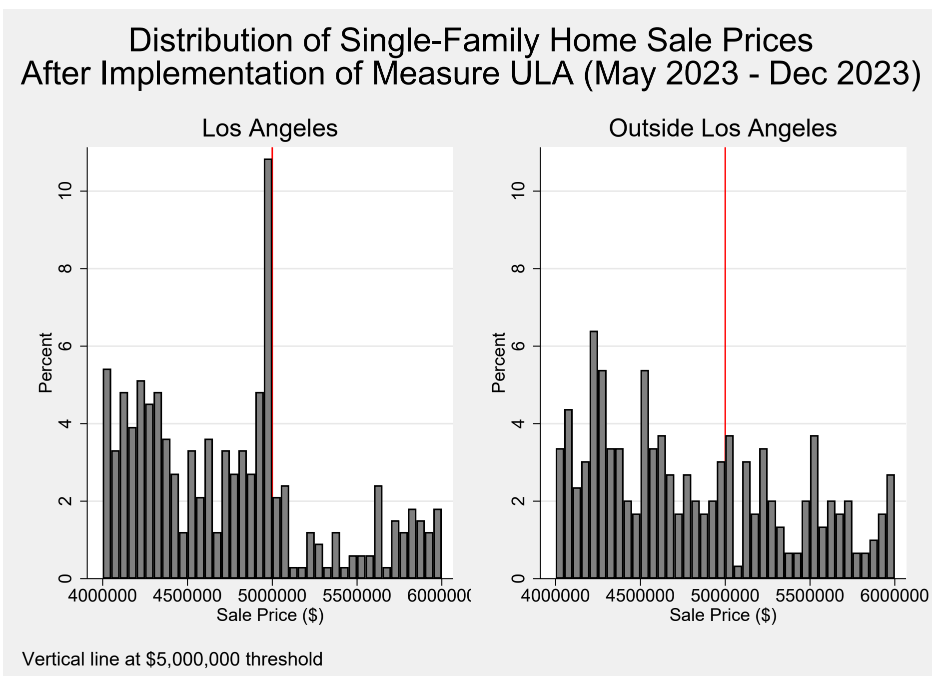


Notes: Figure shows monthly transaction tax revenue collected by Measure ULA as reported by The Los Angeles Office of Finance (in red) compared to revenue imputed by transactions observed in CoreLogic Deeds data that we determine to be arms-length (in blue). The imputed series closely tracks actual revenue reports suggesting that we are accurately defining and capturing all measure-ULA triggering sales in our data.

Figure A2: Distribution of single-family home sale prices within and outside the City of LA



(a) Before the passage of Measure ULA

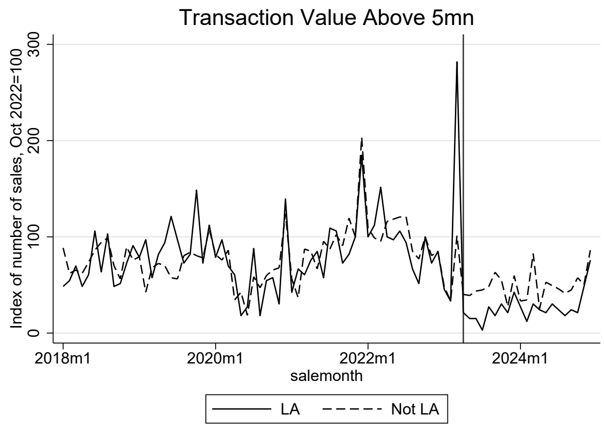


(b) After the implementation of Measure ULA

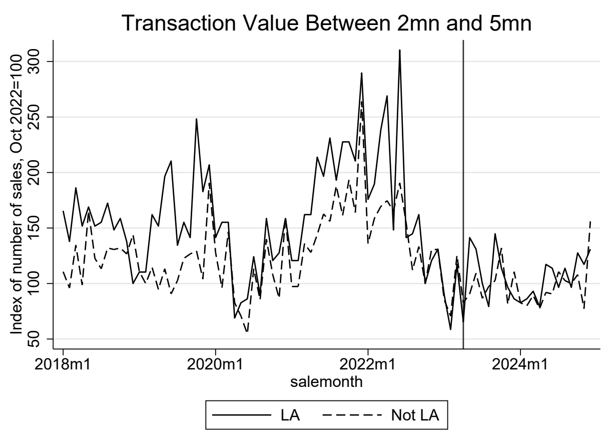
Notes: Plot of the percent of single-family home sale prices in each \$50,000 price bin between \$4,000,000 and \$6,000,000 within and outside the City of LA. Panel (a) shows distribution of home sale prices inside and outside LA before the passage of Measure ULA (January 2018 to September 2022). Panel (b) shows the distribution of home sale prices inside and outside LA after the implementation of Measure ULA (May 2023 to December 2023). The left-hand side graph in Panel b shows a clear bunching of sale prices just below \$5 million after Measure ULA is passed.

Figure A3: Monthly Transaction Frequency among Commercial Property in CoStar data

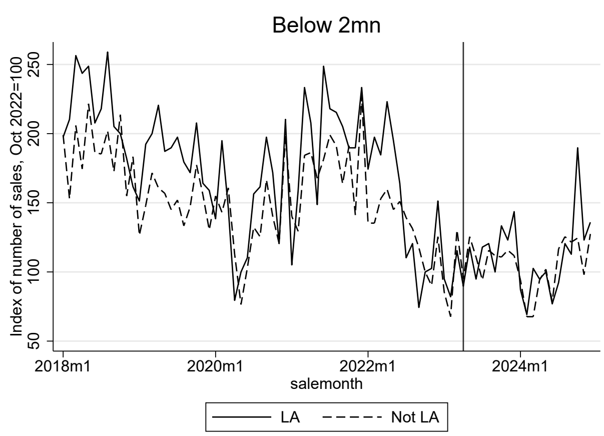
(a) Above 5 million



(b) Between 2 million and 5 million

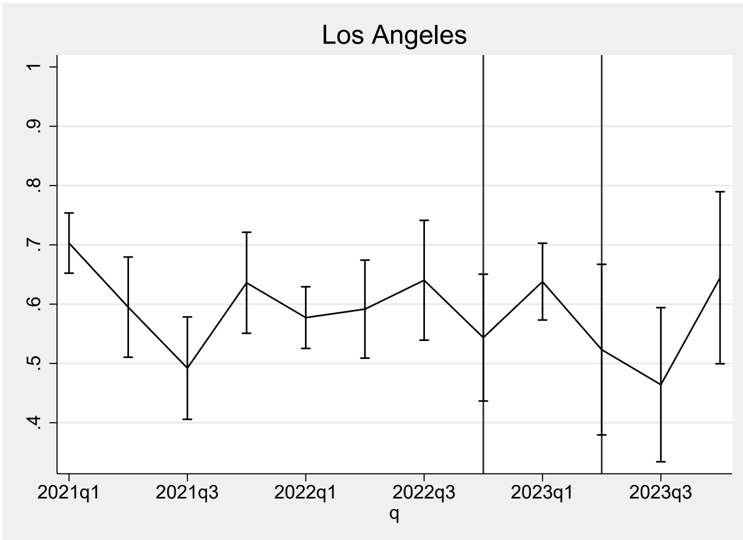


(c) Below 2 million

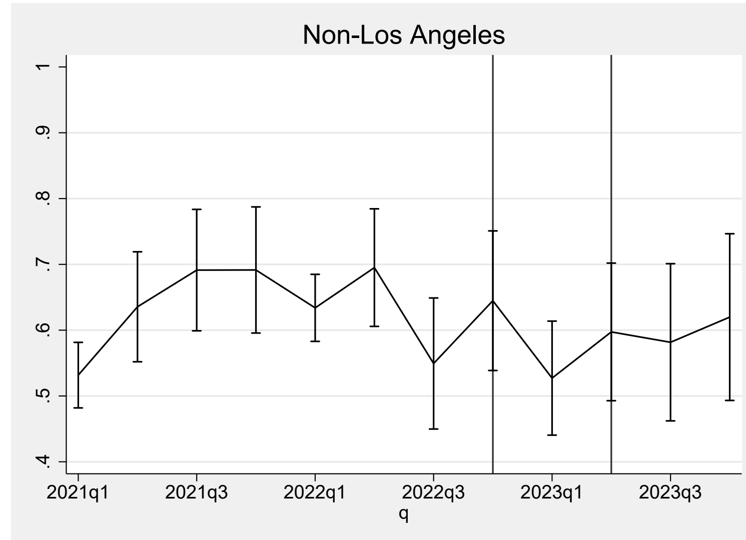


Notes: Using data from CoStar, Panel (a) shows the total transaction volume for sales above $5 million comparing parcels within the City of Los Angeles with otherwise similar parcels elsewhere in Los Angeles County. Panels (b) and (c) repeat the exercise for sales between 2 and 5 million and below two million, respectively. Each series has been normalized to 100 in October 2022, the month immediately preceding the ballot results for Measure ULA.

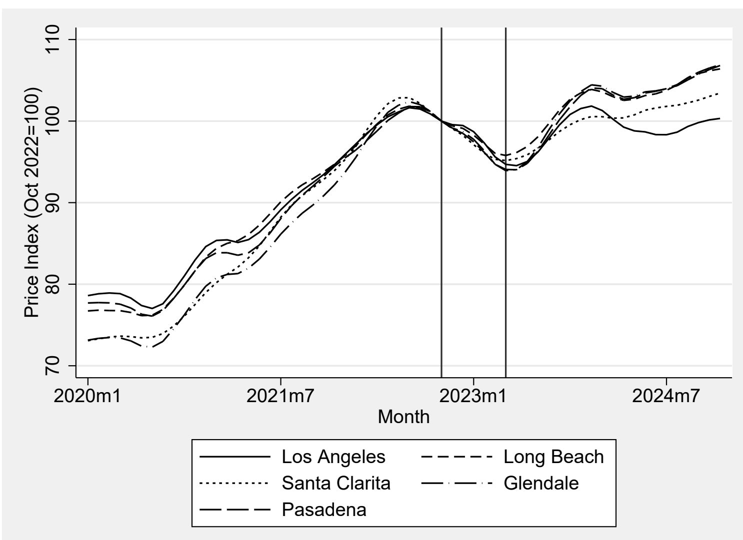
Figure A4: House Price Indexes



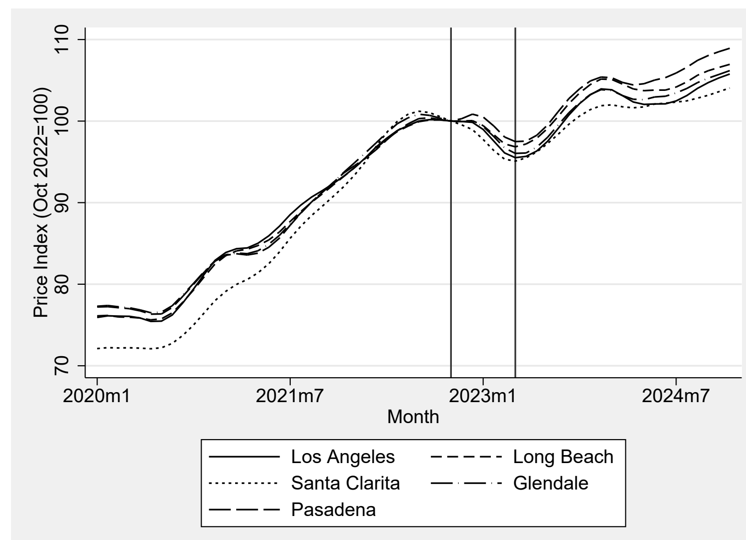
(a) Custom Repeat-Sales Index, Price Above $5mn, Los Angeles



(b) Custom Repeat-Sales Index, Price Above $5mn, Outside Los Angeles



(c) Zillow House Prices, Top Tier



(d) Zillow House Prices, Bottom Tier

Notes: Panels (a) and (b) show the results of a custom repeat-sales index created following the Case-Shiller methodology for transactions above $5mn sale price, shown separately for properties inside and outside the City of Los Angeles. Panels (c) and (d) show Zillow house-price indexes (normalized to 100 in October 2022) for the largest cities in Los Angeles County. Zillow "Top Tier" is the 65th–95th percentile band and "Bottom Tier" is the 5th–35th percentile band within each city. Source: Zillow.

B Descriptive results

This section documents further facts that support our estimates and empirical strategy.

B.1 Price dynamics

The price impact of the transaction tax shows the incidence of the tax. Since this has been the focus of previous work on housing transaction taxes (e.g., Best and Kleven, 2018), we do not make this the focus of this paper. Aside from its theoretical interest, one reason to study incidence is that a price impact of the mansion tax reduces both the revenues from the tax itself, and the future property tax base of the homes. Both these effects mean that the revenue from the tax is lower than it otherwise would be. The overall findings are suggestive of a price effect: We find noisy evidence of price pass-through in results from a custom house price index we construct from assessor rolls. We also find evidence of price pass-through using Zillow HPI data, but this data is subject to concerns about coverage and representativeness which we discuss below.

The first two panels in Figure A4 show estimates from our custom repeat-sale index. There is some evidence of a dip in prices in Los Angeles right after the passage of the tax, but we find no such evidence outside of Los Angeles. However, the large standard errors rule out a significant effect. To make this figure, we create a custom repeat-sales index use the Case-Shiller methodology to study the price impacts. The Case-Shiller methodology involves creating transaction pairs for properties sold more than once, and running a fixed-effects regression of price changes on month fixed effects. The fixed effects take a value of -1 in the month the home is purchased and +1 in the month it is resold. We infer the purchase year using the last assessed value in the assessor rolls. We infer the purchase price by rolling back the assessed value using annual assessment limits published since 1978, and limit the data to properties with no reassessments in the value of improvements.

We also study the Zillow Home Value Index (ZHVI), which combines a repeat-sales estimator with a hedonic regression to infer current market values for residential properties Zillow Research (2023). We download monthly ZHVI series for the five largest incorporated cities Zillow reports in Los Angeles County—Los Angeles, Long Beach, Santa Clarita, Glendale, and Pasadena—and use the two bands Zillow reports consistently across places: Top Tier (65th–95th percentile) and Bottom Tier (5th–35th percentile) of each city's valuation distribution. This dataset has two limitations. First, the price bands are fixed by Zillow and cannot be tailored to the $5 million Measure ULA threshold; many homes in the Top Tier are below the tax cutoff. Second, ZHVI excludes commercial real estate. From 2020 through 2024, Bottom Tier indexes evolve similarly inside and outside Los Angeles before and after

April 2023. The Top Tier series diverge after implementation: neighboring jurisdictions continue their prior upward trend, while the Los Angeles Top Tier index declines by roughly two percent immediately after April 2023 and remains lower relative to the comparison group. This pattern is suggestive of substantial seller-side pass-through, but it is descriptive and not definitive, especially because Zillow does not provide standard errors.

B.2 Extended commercial series

Using transaction data from CoStar shown in Figure A3, we extend the volume analysis to 2024 and partition sales into three bands: below \$2 million, \$2-\$5 million, and above \$5 million. The anticipatory March 2023 spike and the subsequent collapse in high-value sales are even more pronounced than in the assessor data, and the post-tax decline persists throughout 2024. The sharper response among high-value commercial parcels underscores both their greater flexibility in timing transactions and the larger fiscal externalities generated when they defer sales.

B.3 Statutory schedule

Table 1 summarises the inflation-indexed tax brackets. Beginning on April 1st, 2023, a 4 percent rate applies to consideration above \$5,000,000 and below \$10,000,000; a 5.5 percent rate applies thereafter. Thresholds are indexed each July. Our empirical work therefore uses the exact statutory cut-offs except when constructing volume series, where we adopt a \$4.9 million upper bound to capture bunching immediately below the notch.

B.4 Composition of taxable transactions and property heterogeneity

Table B1 tabulates annual counts of taxable transfers by property type and tax bracket. Prior to the levy's introduction commercial and multifamily parcels dominate the \$5-\$10 million range, whereas single-family homes are prevalent below the threshold. Above \$10 million commercial assets again predominate and volumes are sparse. The table confirms that the so-called "mansion" tax is in practice a broad-based levy on high-value commercial real estate, a fact we discuss in our analysis of heterogeneous effects across property types in Table B2.

Table B1: Number of Property Transactions Under Applicable Measure ULA Tax Categories and Property Type

ULA Tax Rate	Commercial			Multifamily			Single-family		
	Pre	Anticipation	Post	Pre	Anticipation	Post	Pre	Anticipation	Post
0	676	272	431	3,052	874	1,869	19,437	5,769	12,167
4	88	46	17	141	60	30	403	180	146
5.5	48	28	9	73	48	18	134	61	48
# obs	812	346	457	3,266	982	1,917	19,974	6,010	12,361

Notes: This table shows the counts of property transactions by applicable ULA transaction tax rate and property type before the passage of the policy (January 2022 - September 2022), anticipation before the implementation (October 2022 - March 2023) and post implementation (April 2023 - December 2023). Transactions are identified from CoreLogic Deeds data as described in Section 4.

Table B2: Heterogeneity in Measure ULA transaction effects by property type

	(1)	(2)	(3)	(4)	(5)
Property type	Single-family	All CRE	Commercial	Multifamily	Other
Post × Treat	-0.293^{*}	-1.002^{***}	-1.501^{***}	-0.240	-0.666^{*}
	(0.152)	(0.246)	(0.444)	(0.324)	(0.368)
Anticipation × Treat	0.094	0.800^{***}	0.992^{***}	0.646	0.589^{**}
	(0.170)	(0.238)	(0.310)	(0.493)	(0.284)
# obs	177,380	204,665	71,132	66,294	67,239

Notes: Each column reports the baseline weighted PPML difference-in-differences specification estimated separately for the indicated property type, restricting the sample to parcel-months from January 2020 through December 2023 with transaction value at least $4.9 million. The treated group is parcels inside the City of Los Angeles and the control group is parcels elsewhere in Los Angeles County. All CRE pools multifamily, commercial, and other non-single-family parcels. All specifications include month fixed effects and month-of-year × treatment fixed effects. Standard errors clustered by zip code are in parentheses. Stars denote significance at the 10% (*), 5% (**), and 1% (***) levels.

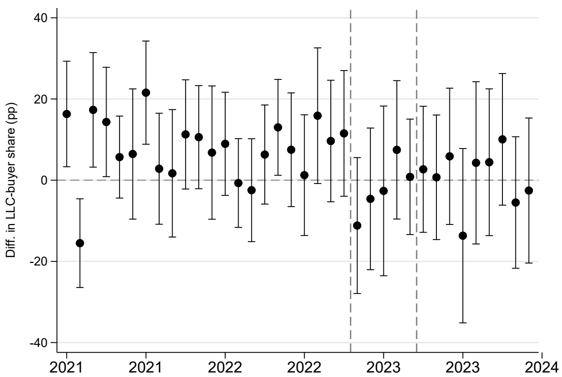
B.5 Spatial spillovers and buyer switching

Beyond the placebo estimates discussed in the main text, we present two additional tests against quantitatively important spillovers. These tests are useful, but they are less precise than the main transaction-frequency estimates.

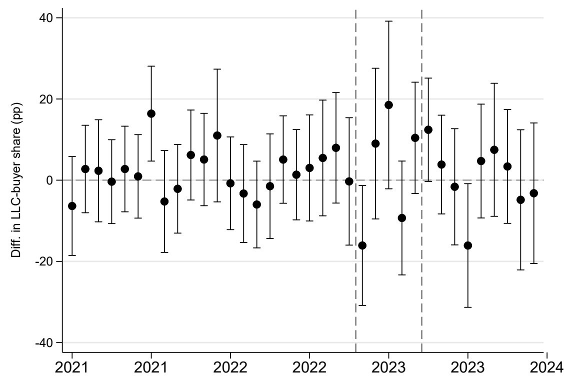
Corporate purchasers, particularly LLCs, are among the most mobile buyers and they are likely more sophisticated than occasional buyers. Making the plausible assumption that these LLCs should be relatively more sensitive and able to shift activity outside LA or to smaller properties, comparing their behavior relative to all others in our placebo markets provides a natural test for spatial reallocation. Figure B1 shows difference-in-differences event-study estimates for our placebo comparison groups: large versus smaller properties outside LA, and smaller properties inside versus outside LA. The outcome is an indicator for whether the buyer is recorded as an LLC. If LLCs are shifting activity to any of these markets where the mansion tax does not apply, we should see a persistent relative increase in the LLC share after Measure ULA. The figure shows no persistent differential change in LLC-buyer share after Measure ULA in either large non-LA properties relative to smaller properties or below-$5m properties inside LA relative to outside LA city.

An even more direct test of spillovers uses CoStar buyer and seller identities to track firms that were active in the pre-ULA City of Los Angeles $5M+ market. We define these as firms that appeared as buyers or sellers in a CoStar transaction above $5 million in Los Angeles before Measure ULA. Figure B2 reports analogous difference-in-differences event studies for the share of transactions involving those firms. If there are substantial spillovers, we would expect a positive effect after Measure ULA was implemented. Again, there is no persistent post-ULA increase for either buyers or sellers. These findings suggest little reallocation into the control groups.

Figure B1: Difference-in-differences event studies of LLC-buyer share in control markets



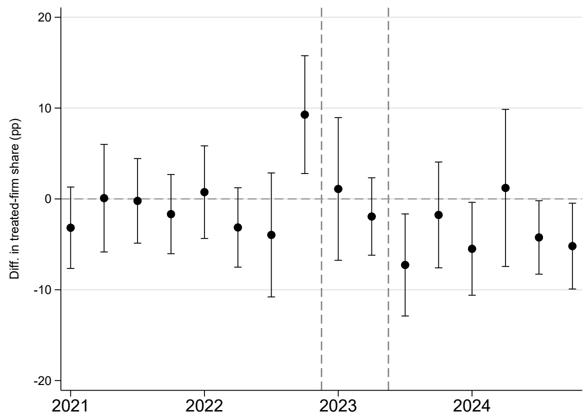
(a) Non-LA, large vs. smaller



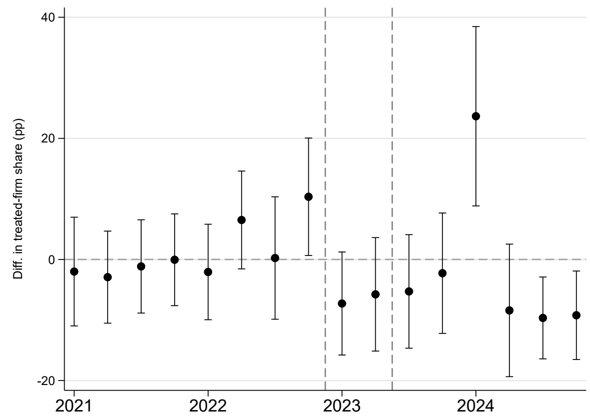
(b) Below \$5M, LA vs. non-LA

Notes: Each panel plots coefficients with 95% confidence intervals from a linear event-study difference-in-differences regression, reported in percentage points. The unit of observation is a transaction. The dependent variable is an indicator for whether the buyer is recorded as an LLC. Panel (a) uses the sample of non-LA transactions with assessed value at least \$3 million and estimates month effects interacted with an indicator for transactions above \$4.9 million, controlling for month and size fixed effects. Panel (b) uses the sample of below-threshold transactions with assessed value at least \$3 million and estimates month effects interacted with an indicator for being in the City of Los Angeles, controlling for month and LA fixed effects. Standard errors are clustered by zip code. Dashed lines separate the pre-period, the October 2022–March 2023 anticipation window, and the post-April 2023 period used in the main DD.

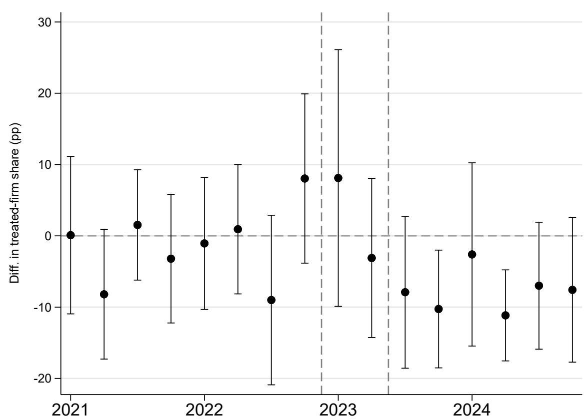
Figure B2: Difference-in-differences event studies of treated-firm share in control markets



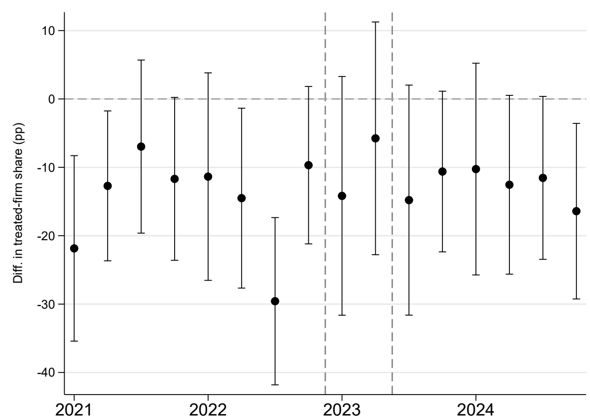
(a) Sellers: non-LA, large vs. smaller



(b) Buyers: non-LA, large vs. smaller



(c) Sellers: below \$5M, LA vs. non-LA



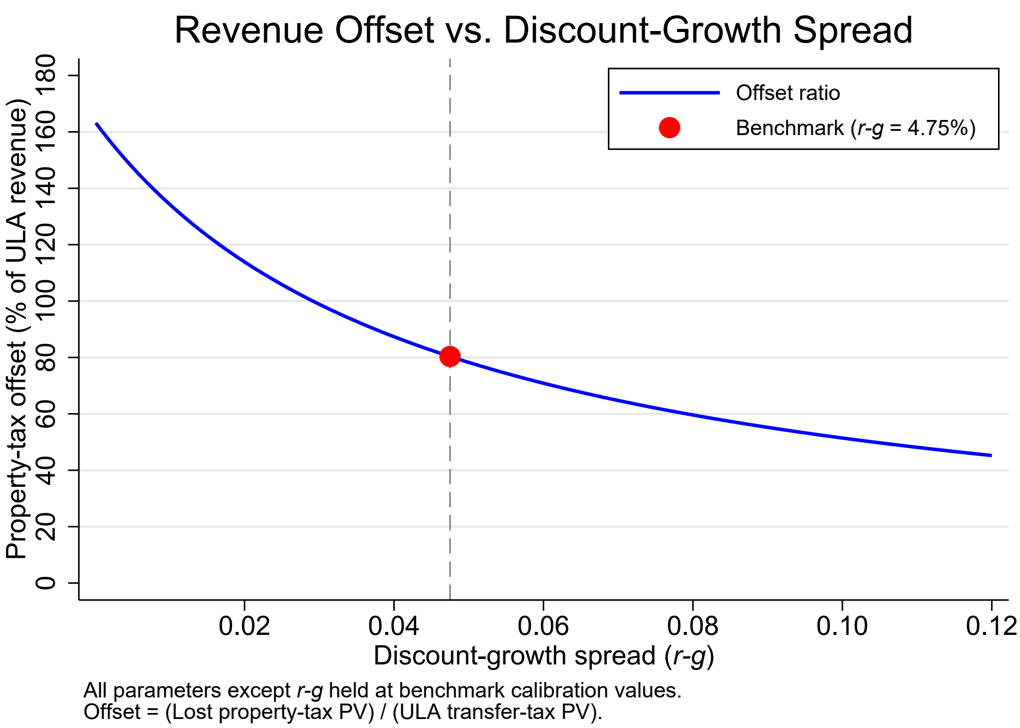
(d) Buyers: below \$5M, LA vs. non-LA

Notes: Each panel plots coefficients with 95% confidence intervals from a linear event-study difference-in-differences regression, reported in percentage points. The unit of observation is a transaction. The dependent variable is an indicator for whether the buyer or seller is a "treated" firm, defined as a firm active in the pre-ULA Los Angeles \$5M+ market. Panels (a) and (b) use the sample of non-LA transactions with sale price at least \$3 million and estimate quarter effects interacted with an indicator for transactions above \$5 million, controlling for quarter and size fixed effects. Panels (c) and (d) use the sample of below-\$5 million transactions with sale price at least \$3 million and estimate quarter effects interacted with an indicator for being in the City of Los Angeles, controlling for quarter and LA fixed effects. Standard errors are clustered by zip code. Because these figures are quarterly, the dashed lines bracket the quarters containing the November 2022 passage and April 2023 implementation of Measure ULA.

B.6 Evaluating Net Revenue Effects Under Different Discount Factors

The mansion tax shifts revenue from future periods forward because mansion tax revenue is collected at the time of transaction while property tax revenue loss accumulates over time. Net revenue effects will therefore vary depending on the assumed discount factor as Figure B3.

Figure B3: Revenue Offset Ratio as a Function of the Discount-Growth Spread



Notes: The curve plots the ratio of lost property-tax present value to Measure ULA transfer-tax present value as the discount-growth spread $r - g$ varies, holding all other parameters at their benchmark values (Table 4). The red dot marks the benchmark specification ($r - g = 4.75\%$). Because property-tax losses from forgone reassessments accrue gradually over future decades while transfer-tax receipts arrive at the time of sale, a higher spread places less weight on those long-run losses and therefore reduces the offset ratio. The relationship is hyperbolic in $r - g$.

C Data Appendix

This paper combines parcel–level administrative data from the Los Angeles County Assessor with proprietary deed and transaction records to quantify how the City of Los Angeles's "Measure ULA" transfer tax affects property-tax reassessments and, through them, local public-finance outcomes. We organize the discussion around four sources: (i) the annual Secured Assessment Roll, (ii) deed records from CoreLogic, (iii) transactions drawn from CoStar, and (iv) publicly available monthly Measure ULA revenue reports. Throughout, we exploit the sharp boundary between the City of Los Angeles, where the tax applies, and the remainder of Los Angeles County, where it does not.[17]

Los Angeles County Assessor Secured Roll The Assessor's Secured Roll lists every taxable parcel in Los Angeles County.[18] Proposition 13 sets each parcel's *base-year value* at acquisition and caps nominal growth in assessed value at two percent per year until a "change in ownership" occurs.[19] The roll therefore reports, for every parcel p and fiscal year t, the assessed land value $A^L_{p,t}$, the assessed improvement value $A^I_{p,t}$, and the corresponding base-year assessment dates (τ^L_p, τ^I_p). A jump in $A^L_{p,t}$ coincident with $\tau^L_p = t$ identifies an arm's-length sale under Proposition 13. We keep all parcels whose tax-status code indicates liability for the one-percent ad valorem tax and discard parcels that are constitutionally or statutorily exempt.

The analysis window begins in January 2020 and ends in December 2023, the last month for which complete deed matching is possible before the 2024 roll is finalized. We classify each parcel into single-family residential, multi-family residential, office, retail, or other commercial using the assessor's use codes. Parcels are then assigned to treatment (City = 1) or control (City = 0) based on municipal boundaries.

CoreLogic Deed Records and the Construction of Sales We observe precise transaction dates from CoreLogic's Deed & Mortgage module, which aggregates county recorder documents nationwide. A transaction is classified as an arm's-length sale when the deed type is recorded as a grant deed, warranty deed, trustee's deed, or similar, and the document type excludes quit-claims, inter-corporate transfers, and other non-market conveyances. Matching on the assessor's parcel number and transaction month yields a one-to-one link for 88

[17]Santa Monica and Culver City adopted separate transfer taxes during the sample period. Because their timing and rate schedules differ from Measure ULA, all parcels in those cities are excluded from every sample.

[18]As of the 2024 roll the county reports 2.1 trillion dollars in net assessed value across roughly 2.6 million parcels; see Los Angeles County Assessor (2024).

[19]See California Revenue and Taxation Code §60-69.5. New construction can also trigger partial reassessment of the improvement component; land values, however, are updated only upon transfer.

percent of assessor-identified sales between 2020 and 2023. Unmatched assessor-identified transfers are not counted as dated sale events; in the monthly panel construction, non-sale parcel-months are coded as zero sales. This treatment is conservative for the reassessment margin. When multiple arm's-length deeds for the same parcel appear within a calendar year, we retain the earliest sale to align with the assessor's base-year coding convention. The sale price recorded in the deed file is not used; instead, the post-sale assessed value from the roll serves as the sale value, ensuring consistency with the tax base.

CoStar Transactions CoStar supplies property-level transaction data that are especially rich for large commercial assets. Coverage is near-complete for sales above five million dollars, the relevant threshold for Measure ULA, and extends through December 2024. While single-family sales are largely absent, the data complement the assessor roll by allowing us to track high-value commercial transactions after December 2023 and to benchmark the roll-based estimates.

Measure ULA Revenue Records The Los Angeles Office of Finance posts monthly reports of Measure ULA remittances that include the month of collection, the amount remitted, and the ZIP code of the property.

D Derivation Details

D.1 Marginal excess burden of a property transfer tax when there is an existing acquisition-value property tax

We model a single parcel whose market value follows the deterministic path[20]

$$M_t = M_0 e^{gt}, \qquad g > 0,\ t \geq 0.$$

Between sales, the statutory property tax assessment drifts at the capped rate $g_s < g$, so the gap $k \equiv g - g_s > 0$ measures how far assessments lag market values. Sales arrive as a Poisson process with constant hazard p.

The property tax base, i.e. the tax-assessed value a_t, depends on the duration the property has been held since last sale, h at time t:

$$a_t \mid_{H=h} = M_0 e^{g(t-h)} e^{g_s h} = M_0 e^{gt} e^{-kh}$$

The distribution of holding periods follows an exponential law, $H \sim \mathrm{Exp}(p)$. Conditioning on a realised holding period $H = h$, the assessed value at time t is

$$a_t(h) = M_0\, e^{gt}\, e^{-kh}$$

Taking expectations over H therefore gives

$$\mathbb{E}[a_t] \;=\; M_0\, e^{gt}\, \mathbb{E}\big[e^{-kH}\big] \;=\; M_0\, e^{gt} \int_0^\infty e^{-kh}\, p\, e^{-ph}\, dh \;=\; M_0\, e^{gt}\, \frac{p}{p+k}. \tag{13}$$

Define

$$\Phi(p) \;\equiv\; \frac{p}{p+k}.$$

With this notation, (13) becomes

$$\mathbb{E}[a_t] \;=\; M_0\, e^{gt}\, \Phi(p),$$

so $\Phi(p)$ is the present-value wedge between assessed and market values.

The present value of the property tax base discounting cash flows at $r > g$ is therefore

[20] Allowing M_t to follow a geometric Brownian motion would leave all expressions unchanged because only the expected growth rate matters for the present-value integrals.

given by:

$$A_t(p) = \frac{M_0}{r-g}\Phi(p)$$

The present value of the property transfer tax base is closely related, and given by:

$$T_t(p) = \frac{M_0}{r-g}p$$

Note that $A = T/(p+k)$.

Now consider a reform where the transfer tax rate τ is changed unexpectedly at $t = 0$, shifting the hazard to $p'(\tau)$; the ad valorem property tax rate θ is held fixed. In the long-run, the tax bases shift to $T(p')$ and $A(p')$. In the near-term, there are potentially important transitional dynamics to consider as the distribution of holding period of properties, and therefore the distribution of assessed values, shifts towards to the new sale hazard. We will first derive the marginal excess burden of the transfer tax and the revenue-maximizing rate assuming instant adjustment of property tax adjustment and then consider how the transition dynamics affect these quantities.

Total revenue from the property tax and transfer tax is given by $R = \tau T + \theta A$. A marginal change $\mathrm{d}\tau$ induces a change in revenue that is separable into a mechanical, $\mathrm{d}M$, and behavioral, $\mathrm{d}B$, component:

$$\mathrm{d}R = \underbrace{\mathrm{d}\tau T}_{\mathrm{d}M} + \underbrace{\tau \mathrm{d}T + \theta \mathrm{d}A}_{\mathrm{d}B}. \tag{14}$$

Defining ϵ^T and ϵ^A as the elasticities of the two tax bases with respect to the net of transfer tax rate $(1-\tau)$, we can write $\mathrm{d}T = -T\epsilon^T \frac{\mathrm{d}\tau}{1-\tau}$ and $\mathrm{d}A = -A\epsilon^A \frac{\mathrm{d}\tau}{1-\tau}$. Substituting into equation (14), we get:

$$\mathrm{d}R = \mathrm{d}\tau[(1 - \frac{\tau}{1-\tau}\epsilon^T)T - \frac{\theta}{1-\tau}\epsilon^A A]$$

The marginal excess burden, defined as the change in revenue coming from the behavioral response for each additional dollar of revenue raised is given by:

$$\frac{-\mathrm{d}B}{\mathrm{d}R} = \frac{\tau\epsilon^T T + \theta\epsilon^A A}{(1-\tau(1+\epsilon^T))T - \theta\epsilon^A A}$$

Setting $\mathrm{d}R = 0$, we can solve for the revenue-maximizing rate of τ^*:

$$\begin{aligned}(1-\tau(1+\epsilon^T))T - \theta\epsilon^A A = 0 \\ \tau^* &= \frac{1}{1+\epsilon^T} - \theta\frac{A}{T}\frac{\epsilon^A}{1+\epsilon^T} \\ &= \frac{1}{1+\epsilon^T} - \frac{\theta\epsilon^A}{(p+k)(1+\epsilon^T)}\end{aligned}$$

Note that $\epsilon^A = \epsilon^T - \Phi(p)\epsilon^p$ and $\epsilon^T = \epsilon^p$.

Including transitional adjustment Consider expected assessment at $t + \Delta t$:

$$\mathbb{E}[a_{t+\Delta t}] = (1 - p'\Delta t)[\mathbb{E}[a_t](1 + g_s\Delta t)] + p'\Delta t M_0 e^{gt}$$

Taking $\Delta t \to 0$ yields the linear ODE $\frac{d}{dt}\mathbb{E}[a_t] = (g_s - p')\mathbb{E}[a_t] + p'M_0e^{gt}$, whose solution using the pre-tax change initial condition $\mathbb{E}[a_0] = M_0\Phi(p)$ is:

$$\mathbb{E}[a_t] = M_0 e^{gt}[\Phi(p') + (\Phi(p) - \Phi(p'))e^{-(p'+k)t}]$$

Therefore, the full present value of property tax assessments including the transition adjustment is:

$$A_t^{full} = M_0\left[\frac{\Phi(p')}{r-g} + \frac{\Phi(p) - \Phi(p')}{r - g + p' + k}\right]$$

The first term is the new steady state and the second is the *transition* term, which is strictly positive when $p' < p$ and shrinks as $\Phi(p') \to \Phi(p)$.

E Evasion and Avoidance Responses

Tax evasion and avoidance are prevalent in tax systems. It is natural to expect that they would be a part of taxpayers' response to the mansion and property taxes. One key feature of the Measure ULA setting is that the same reporting instrument (record of a transaction at the county recorder office) triggers both the mansion tax and property tax reassessment. Classic evasion strategies such as underreporting the true value of the transaction have the same implications for the fiscal externality on property tax revenue as a real change in the value or probability of a transaction. For example, if buyers and sellers collude to report $10 million transaction as an $8 million transaction, with the remainder of the payment made in cash, the fiscal externality is equivalent to a scenario where the true price of a potential $10 million property was reduced to $8 million. Many tax avoidance strategies for Measure ULA have similar effects on the property tax base and are reflected in our measure of the drop in taxable transactions. For example, using ground leases instead of outright sale to avoid the mansion tax results in a drop in the mansion tax base but also avoids triggering a reassessment just as if the transaction had never occurred. The drop in transaction hazard that we estimate is the decrease in *reported*, taxable transactions, inclusive of such evasion methods and the fiscal externality includes revenue lost to evasion.

More concerning are evasion or avoidance schemes that would not appear in the mansion tax base but do appear in the property tax base, which would mean our current estimates overstate the fiscal externality. We discuss the most likely such scenarios and how we address them:

1. **Pricing transactions just below mansion tax threshold** The most widely discussed avoidance strategy is to price properties just below the $5mn threshold, which would completely sidestep the mansion tax but only reduce the property tax reassessment by the amount of the price decrease.[21] By including all property sales $4.9mn and above in the treatment group, the transaction decline we estimate is unchanged by this type of avoidance. The price effects of this avoidance scheme are also left out of our estimates, but if included would lower net revenue further relative to our estimates since it would lower both mansion and property tax revenue.

2. **Split Sales** Another possibility is that properties are split into multiple, smaller sales, well below the $5 million threshold. These sales would not count as transactions in the "treated" category because they would be priced below $4.9 million. However, they

[21] Several news articles discussing avoidance of the mansion tax bring up this pricing scheme as the main new form of avoidance. See for example https://robbreport.com/lifestyle/news/los-angeles-mansion-tax-1234864295/, https://www.latimes.com/california/story/2022-12-15/l-a-s-new-mansion-tax-scheme

would result in a relative increase in transactions in the placebo group of between \$2 and \$4.9 million within LA city, which we do not observe. Furthermore, most such transactions (aside from those discussed in point 4 below) would generate a deed that we observe in the CoreLogic data, where they would be flagged as partial-interest/split sales. We track these sales and find they are rare, and do not increase after Measure ULA: pre-ULA, 990 of 200,407 transactions are split (0.49%); post-ULA, 176 of 56,204 are split (0.31%). In 2024, only 24 of 7,353 transactions are split (0.33%), including 5 of 1,329 in LA (0.38%).[22] This channel of avoidance is unlikely to influence our estimates to a measurable extent.

3. **Reporting transaction party as exempt from the mansion tax but not the property tax** In the full sample we do not know the identity of the transacting parties and so cannot exclude potentially exempt sales. To the extent that buyers try to evade the mansion tax by posing as an exempt entity, our estimates would over-state mansion tax revenue and therefore understate the fiscal externality. However, these exempt sales are likely only a small portion of the total sales value. Figure A1 shows that our estimates of Measure ULA revenue, which includes revenue from potentially exempt transactions closely tracks official revenue, suggesting that exempt sales are likely small.

   In the CoStar sample we can identify and remove exempt sales and confirm that they are uncommon. From this data we see that 43 sales of value greater than \$4.9 million were transacted by exempt parties before Measure ULA (i.e. about 1.59 per month). That number stays about the same post-Measure ULA at 1.62 per month. As a share of total transactions, they were about 4.88% of all transactions in the pre-period and then increased to 15.74 percent after the policy due to falling non-exempt sales. This channel of avoidance is quantitatively small and unlikely to influence our estimates.

4. **Split transfers with no corresponding deed** A remaining concern is that there may be partial-interest or split transfers where no corresponding deed is generated (as identified in point 2 above). These types of transfers can only really occur if the property is owned by an LLC and the sale is a partial interest of the company and not the land itself. We can directly measure the quantity of such sales by identifying parcels where the land base year was reset in the LA county assessor data (indicating a sale and reassessment) and whose reassessed value is above \$5 million, but where there is no corresponding deed in the CoreLogic data. Performing this exercise, we find very

[22] Note that 990 splits implies that at most 495 properties could have been split since if a property was split into 2 transactions, they would be counted as 2 split transactions.

few such transactions in practice. There were a total of 15 such transactions in LA between 2021 and 2024, and 11 outside LA over the same period. We conclude that this channel of avoidance is highly unlikely to affect our estimates.

Note that there is a large incentive to evade/avoid property tax even before the advent of Measure ULA, which would certainly also affect revenue earned through the mansion tax. We do not attempt to measure evasion separately. Our fiscal externality estimates are for the most part inclusive of any evasion response. Any responses that affect the mansion tax base and property tax base differently, and therefore affect the validity of our estimates, do not seem to be implemented in practice.