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The Unintended Consequences of Measure ULA

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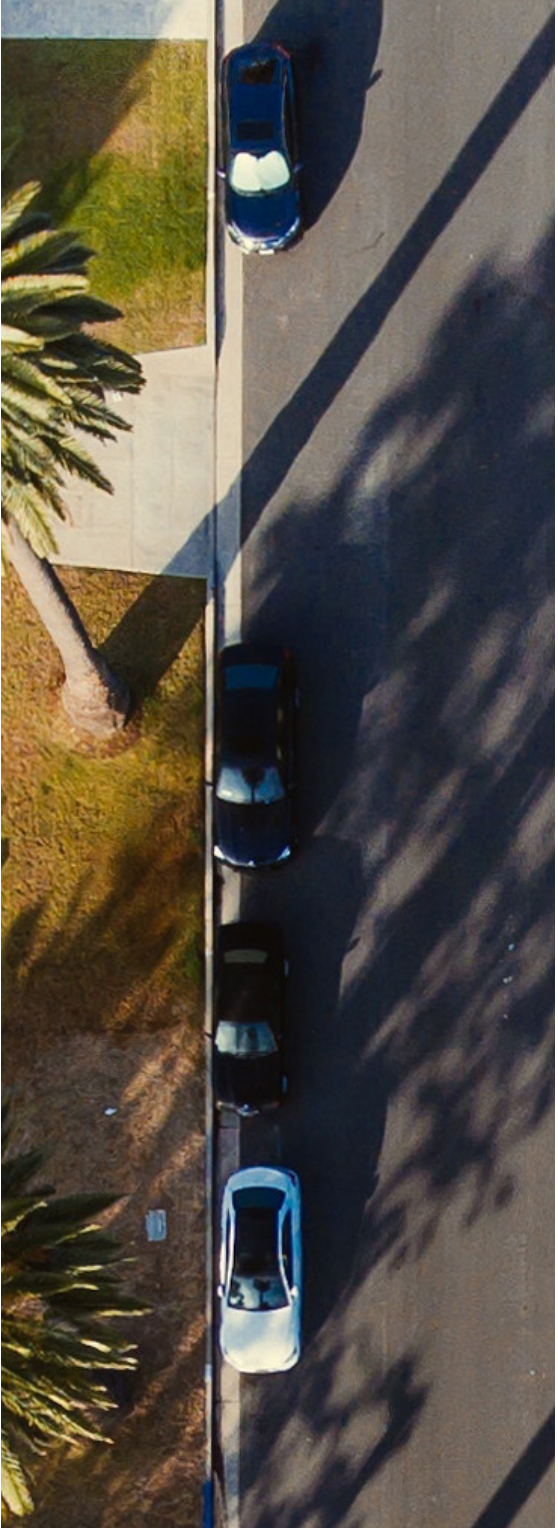
Abstract

We present evidence suggesting that Measure ULA has reduced higher-end real estate transactions in Los Angeles. Since Measure ULA was enacted, the odds of a Los Angeles property selling at a price above its tax threshold have fallen by as much as 50%. In raw terms, this sharp decline occurred across all types of properties, but our strongest evidence suggests it was particularly pronounced for non-single family transactions, which fell by 30-50%. Together the evidence suggests that Measure ULA is neither a true "Mansion Tax," nor a tax that falls solely on unearned property wealth. The tax does fall on mansions, but it also impedes the trade in commercial, industrial and multifamily property. In doing so it jeopardizes L.A.'s ability to build new housing, revitalize struggling commercial and industrial properties, and raise property tax revenue. All these processes rely on property turnover, and in particular the turnover of higher-priced, non-single family parcels. A tax that reduces this turnover will undermine property tax revenues inside and outside L.A., obstruct local and regional housing production, and slow local revitalization efforts. Thus while Measure ULA has generated visible, substantial and much-needed revenues for affordable housing in Los Angeles, it has also, less visibly, had consequences that *reduce* both housing affordability and fiscal health. These consequences, fortunately, do not need to be part of the measure. We propose some simple amendments that could make Measure ULA fairer, more efficient, and more in line with the spirit in which it was advertised.



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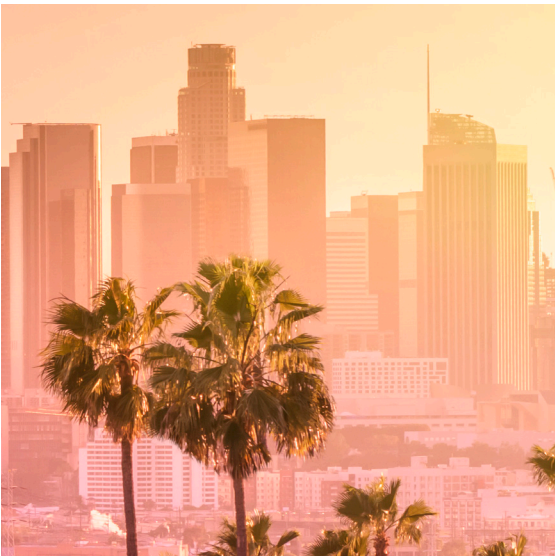

The Unintended Consequences of Measure ULA

Highlights

We analyzed 2020-2024 Los Angeles County real estate transaction data through two primary statistical frames, each of which produced directionally consistent findings:

- One statistical analysis focuses on quantities, and suggests that Measure ULA led to a 30-50% decrease in the number of commercial, industrial and multifamily transactions
- A second analysis focuses on prices, and suggests that Measure ULA resulted in a 50% reduction in the odds that a transaction occurring will be above ULA's price threshold. This change in odds translates to a 49-55% total reduction in such transactions in the measure's first 20 months.
- Reductions of that size imply both a loss in new housing and commercial/industrial development, as well as a loss in property tax revenue. Our estimate is that Measure ULA resulted in an initial loss of property tax revenue of approximately $25 million annually. If the measure continues without reform, these revenue losses will compound.

I. Introduction

This report describes some unintended consequences associated with Measure ULA ("United to house Los Angeles"). Measure ULA, which was approved by Los Angeles voters in 2022, and went into effect in April 2023, is a real estate transfer tax designed to help address L.A.'s housing crisis.

The measure initially applied to real estate transactions over $5 million. It imposed a 4% tax on transactions priced between $5 million and $10 million, and a 5.5% tax on transactions over $10 million. A provision in ULA calls for its tax thresholds to be adjusted for inflation every year, so in July 2024 its tax rates began applying to transactions over $5.15 million and $10.3 million, respectively. Prior to Measure ULA Los Angeles, like many cities, did have a real estate transfer tax, but it was very low: its rate was 0.45%, on transactions above $100,000. For high-end sales, then, Measure ULA represented an almost ninefold increase in the tax rate. ULA's goal is to help alleviate housing burdens, and the revenue it raises is dedicated to projects like homeless housing, affordable housing, and tenant assistance.

ULA's appeal is easy to understand. L.A.'s housing crisis is severe, and money to help low-income tenants is essential. The city's ability to raise that money, however, is highly constrained by Proposition 13 and related state laws. These laws limit the growth of existing taxes, and only allow new taxes if voters directly approve them. That constraint makes it very difficult for cities to raise new money via the property tax (the most common fiscal instrument for local governments), because property taxes are notoriously unpopular.

In these political circumstances, transfer taxes like Measure ULA can have legitimate advantages. A property tax is a tax on wealth, and a common complaint about wealth taxes is that wealth and income aren't the same. If a property's value rises while its owner's income doesn't, the tax can increase beyond the ability to pay, creating a burden some property owners will struggle with. A transfer tax, in principle, can be designed to minimize that problem. Because they are only levied when property is sold, transfer taxes are triggered when wealth becomes income. So long as the sale is profitable, taxpayers can use that income — the proceeds of the sale — to pay the bill, and thus offset the tax's burden.[1] Measure ULA, moreover, offers the typical voter even more protection, by only taxing large transactions. Many voters pay property taxes, but most people will never sell a $5 million or $10 million property, meaning the typical voter will never directly pay a Measure ULA levy. This fact, combined with proponents' framing of Measure ULA as a tax only on the luxurious lives of the very rich (a "Mansion Tax") may help explain why almost 58% of voters supported the measure.

1 Property taxes are also unpopular because they are wealth taxes, and a hallmark of wealth taxes is that the government sets both the rate and the base. The taxing authority decides how much the property is worth, and how much to tax it. This makes a property tax different from most other taxes where the government sets the rate but the market sets the base (e.g., the government can decide your income tax rate, but it can't decide your income). Having the government play a determining role in both the rate and the base can make property taxes seem rigged. A transfer tax, by letting the market set the base, avoids this problem as well.

The advantages that can accompany transfer taxes, however, do hinge on those taxes being well-designed. A poorly designed tax can create incentives for avoidance and strategic behavior, and lead to unintended consequences that render the tax less effective. So how well does ULA work? In April 2024, after the measure had been in place for a year, proponents declared it a success. ULA, they observed, was already raising far more revenue than any other housing program in Los Angeles, and had withstood a number of legal challenges from the real estate industry. "ULA has transformed our city," one city councilmember said. "We want a rising tide to raise all boats. If folks are going to have multimillion-dollar homes, they ought to also contribute to making sure that everyone has a home."[2] Similarly, a group of academics concluded a report on ULA by saying, "Los Angeles voters made the right decision when they approved Measure ULA in November 2022. Despite efforts by the real estate industry to undermine its implementation, Measure ULA is already a success."[3]

We are less sanguine. While ULA has real and visible benefits, there are signs that it also carries considerable, albeit less visible, costs. One sign of those costs is the revenue ULA *hasn't* raised. The measure's supporters are correct that ULA has raised more money than any other local housing program, but this is a depressingly low bar to clear. It's also a *different* bar than the one laid out when voters were asked to support the measure. When proponents first placed the "Mansion Tax" on the ballot, they did so estimating that it would raise between $600 million and $1.1 billion annually.[4] It has not come close to hitting that mark. According to the city's ULA Dashboard, from April 2023 to December 2024 the measure actually raised $480 million, or roughly $288 million per year — well below what was anticipated.[5]

This shortfall suggests two potential problems:

Measure ULA appears to be reducing higher-value real estate sales in Los Angeles, particularly sales of properties other than detached single-family homes. A reduction in such sales is troubling, because these sales directly and indirectly drive construction of much-needed new housing, and help create new commercial and manufacturing opportunities for workers. These sales are also disproportionately important to the city's overall fiscal health, because they largely determine the growth in property tax revenues. Since ULA began, the number of higher-value sales in the City of Los Angeles has plummeted, and this trend has been driven by a decline in non-single family transactions (i.e., multifamily, commercial, industrial). Tellingly, such sales have *not* fallen by nearly as much in the rest of L.A. County, and neither have sales of under $5 million in the city of L.A. (which are exempt from Measure ULA). These differential trends within the county strongly suggest that Measure ULA is responsible for the downturn in large transactions within Los Angeles.

2 https://www.youtube.com/watch?v=_zqXDCs21ZM

3 Peter Dreier, Joan Ling, Scott Cummings, Regina Freer, Manuel Pastor, Ananya Roy, Chris Tilly. "Measuring LA's Mansion Tax: An Evaluation of Measure ULA's First Year". April 2024. https://www.oxy.edu/about-oxy/community-engagement/uepi/publications/ula-report

4 Peter Dreier, Joan Ling, Shane Phillips, Scott Cummings, Manuel Pastor, Seva Rodnyansky, and Jackson Loop. 2022. An Analysis of Measure ULA. September. https://www.lewis.ucla.edu/research/an-analysis-of-measure-ula-a-ballot-measure-to-reform-real-estate-transfer-taxes-in-the-city-of-los-angeles/

5 https://housing.lacity.gov/ula-dashboard

Sales of all types of properties fell in Los Angeles after ULA was enacted. The ULA-induced portion of the downturn, however, appears to be driven primarily by a decline in properties other than single-family homes. Our estimates suggest that Measure ULA reduced the total number of non-single family transactions in L.A. ZIP codes by 7-15% per month, relative to other places in the county. While a decline of 7-15% may not sound large, these are monthly losses, and monthly losses compound: the 7-15% monthly decline represents, over almost two years, a 30-50% reduction in the volume of non-single family transactions. A decrease of this magnitude is consequential, because commercial, industrial and multifamily parcels account for a disproportionate share of higher-end transactions. (They are seven times more likely than single-family homes to sell for over $5 million). Even modest percentage reductions in their turnover, as a result, can translate into substantial reduction in higher-end sales overall. Such a reduction is in fact what we see. When we analyze prices before and after ULA, our estimates suggest that the tax is responsible for a roughly 50% decline in sales above the $5 million tax threshold.

This reduction in sales is disquieting, and not only because the lost sales represent lost revenue for Measure ULA. The high-value transactions that ULA targets are only a small share of total sales in the city, but they account for a disproportionate share of growth in the city's property tax base, and a disproportionate share of the sales that lead to new housing starts and new local jobs. When these transactions are made more difficult, housing and revenue growth is impeded.

The revenue effects of ULA occur because property in California, as result of Proposition 13, can only be reassessed when it is sold or substantially improved, meaning that anything that reduces property turnover also reduces local revenue. The housing effects of ULA arise because developers who build multifamily housing often start by buying an expensive site, and — more importantly — finish by selling the improved property to long-term owners when the project is complete. The ULA tax thus looms large over their development decisions: the tax liability they face when the building is complete changes the terms on which they can get financing, and makes development less feasible on many parcels. Efforts to create new movie studios or manufacturing and distribution facilities, or to reposition moribund office buildings, face similar hurdles, because these efforts also often involve buying and selling high-priced properties. If Measure ULA deters these transactions (as the evidence suggests it does) the costs for the city will be substantial. And because the city holds a disproportionate share of the region's land where commercial, industrial and multifamily development is legal and feasible, lost development opportunities in the city can have regional repercussions. Some lost development in the City of Los Angeles might be pushed across the city's borders, but some simply won't occur.[6]

6 Although over 65% of Los Angeles's land is zoned for single family homes only, most of the county's other municipalities restrict non-single family development by as much or more. L.A. is also far and away the geographically largest jurisdiction in the county, and the relatively few localities that zone for more commercial, multifamily or industrial (such as Huntington Park or Bell) are very small. The City of L.A. therefore holds most of the county's land suitable for non-single family development, and developments displaced from the city risk not finding suitable sites elsewhere. For more, see Stephen Menendian, Samir Gambhir and Chih-Wie Hsu, 2022, *Single Family Zoning in Greater Los Angeles*, UC Berkeley Othering and Belonging Institute.

The fact that Measure ULA is falling heavily on sales that *aren't* single-family homes, moreover, suggests the second problem:

It is inaccurate to think of Measure ULA solely as a "mansion tax" or a tax on unearned property wealth. If designed well, real estate transfer taxes can capture some of the windfall gains associated with owning property during booming markets. Someone who bought a house in Los Angeles 10 or 20 years ago, even if they made few improvements to it, could sell it today for a substantial profit. That profit is arguably unearned, and it can be fair if the government captures some of that value through taxation, and spends the revenue on programs that help people not fortunate enough to own property when prices are rising.[7]

Measure ULA, however, does *not* exclusively target such windfall gains. ULA is triggered only by high prices, and high prices do not necessarily signify large profits, unearned gains, or the presence of "mansions." ULA can and does apply to commercial and industrial land, to properties that have been extensively improved (like new apartment buildings) and to properties that are bankrupt.

Nor does ULA just wrongly tax some proceeds that aren't windfalls. It also fails to tax many proceeds that *are*. A person who buys a house for $500,000 and sells it for close to $1 million after several years, and who is able to do so simply because the neighborhood became more desirable even as fewer homes were built, would pay no ULA tax on their windfall profit. Someone who buys a parcel and builds an apartment building, in contrast, is taxed heavily when they sell it, even if the project loses money. Thus the person who only profits from housing scarcity is rewarded, while the person who acts to combat that scarcity is penalized, even if their action yields no profit at all. And while ULA contains an exemption for builders of deed-restricted affordable housing, the exemption is oddly constructed: it is restricted to nonprofit builders of such housing, and thus excludes the class of affordable developers who contribute *most* to the city's deed-restricted housing stock (for-profit LIHTC developers). It also grants nonprofit developers an exemption even when they buy parcels that aren't used for new housing.

In what follows we expand on the points above. In Section II we review some basic principles of taxation. The point we emphasize is that all taxes must balance the goal of more revenue against the unintended consequence of altered behavior. As tax rates get higher, people have more incentive to avoid the tax, and that tax avoidance can have costs not just in lost revenue, but also because the avoidance behavior itself is costly for society.

In Section III we examine transfer taxes in general, and ULA specifically, in light of this point. The academic literature on real estate transfer taxes makes clear that they tend to depress property sales. Measure ULA differs from many other transfer taxes by having higher rates (its lowest rate is 4%) and a smaller base (it taxes nothing beneath $5 million). The smaller base reduces the likelihood of ULA broadly depressing transactions; a tax that doesn't apply to most transactions will not directly affect them. The higher rate, however, makes ULA *more* likely to reduce transactions within the small segment of transactions it targets. Owners of these properties are more likely to hold them off the market. As we discuss, this reduction in sales is likely to

7 This is the primary fairness justification for high transfer taxes. Lower transfer taxes, like L.A.'s 0.45% base rate, have a more straightforward economic logic: it costs the government money to record and enforce deeds and other property contracts, so people who buy and sell property, and benefit from that government activity, should help pay for it.

undermine both property tax revenue and housing development. We emphasize that ULA's structure makes it particularly harmful for development: even when a high-end property does sell, ULA makes it less likely that the buyer will use the site to build housing. As a result of ULA, the winning bidder for a developable site becomes less likely to be someone who wants to build on it, rather than merely operate what's already there.

In Section IV we test these ideas by analyzing four years of property transaction data from Los Angeles County. We first show, consistent with the reasoning above, that ULA does not have a statistically detectable impact on the *total* volume of real estate transactions in the City of LA. This finding *doesn't* mean, necessarily, that the measure has no impact on total volume. It could also be that because the vast majority of transactions don't have to pay the tax, an analysis that examines all transactions may have difficulty detecting the tax's effect.

Our two subsequent analyses bear this latter interpretation out. When we restrict our analysis to the class of properties most likely to be affected by Measure ULA (the non-single family properties), we see the reduction in transaction volumes we mentioned above. Specifically, when examining transactions at the ZIP code level, we see that when ULA begins non-single family transactions fall 7-15% more per month in Los Angeles than they do elsewhere in the county.

We then shift from analyzing quantities to analyzing prices, and ask if high-end sales of any kind became less prevalent in the City of Los Angeles after ULA was enacted. Here the evidence becomes stark and unambiguous. Our regressions suggest that both the odds and probability of an L.A. real estate transaction being above the ULA price threshold plunged, by 50-55%, after Measure ULA was enacted. Among other costs, this volume of lost transactions implies a nontrivial loss of property tax revenue in the City of LA, on the order of $25 million per year in the first years.

As we will discuss, our evidence isn't definitive. Social science evidence rarely is. Real estate data can be noisy and incomplete, and (again) because ULA targets only a small share of transactions, its impact can be hard to detect statistically. At the same time, the decline in higher-priced transactions in Los Angeles is unmistakable, and most non-ULA explanations for that downturn are either implausible, or explicitly ruled out by the evidence we have. A skeptic might argue, for example, that high interest rates caused L.A.'s downturn in sales. But interest rates affect all of Los Angeles County equally, so they cannot explain why $5 million sales fell so much more in the City of Los Angeles than elsewhere in the county. Similarly, one might argue that the downturn is an artifact of one-time efforts to avoid Measure ULA when it was first enacted — that people rushed to sell before the tax came into effect. That certainly happened, and explains some of the city's sales decline. But the effects associated with Measure ULA persist in our analysis even when we control for this strategic behavior. Our results persist, in fact, with a broad array of different controls, and using statistical approaches explicitly designed to uncover causal relationships.

Our results are also not surprising. Both economic theory and previous research strongly suggest that transfer taxes depress the volume of real estate activity in the places they are administered. Our findings related to Measure ULA are consistent with that body of work. The preponderance of evidence right now suggests that Measure ULA is inhibiting large real estate sales, particularly non-single family sales, and thus very likely costing Los Angeles, and arguably the region overall, housing, jobs and property tax revenue.

ULA's problems, fortunately, are fixable. There is no need to jettison Measure ULA. At the end of the report, in Section V, we suggest a range of reforms that would mitigate these unintended consequences while preserving, and arguably even enhancing, ULA's ability to raise revenue for affordable housing. The most ambitious reform we propose would apply ULA only to properties that have not been reassessed in 20 years; this step would ensure that ULA accomplishes its stated goal taxing people who benefit inordinately from the real estate market, and prevent it from inadvertently taxing people who are building or retrofitting property. Other, simpler reforms could also be pursued, either together or on their own. ULA could exempt all properties that are not single-family homes; doing so would make Measure ULA the "Mansion Tax" that was advertised to voters. Another idea is to exempt any sale that leads to significant new construction. A third idea involves making ULA's rates marginal, to reduce some of the large behavior changes that depress transactions. A final option, if there is a concern that any of these reforms would be too costly in terms of revenue, is to add additional brackets, and tax some lower-valued transactions at a lower rate. This last reform would bring ULA more in line with transfer taxes elsewhere.

II. Raising Revenue vs. Changing Behavior: A Taxation Primer

"The art of taxation," the French statesman Jean-Baptist Colbert once wrote, "consists in so plucking the goose as to obtain the largest possible amount of feathers with the smallest possible amount of hissing."

Pluck a goose lightly and it will quietly part with its plumage; treat it more aggressively and it will wriggle away, try to escape, and maybe even bite back. Translated into non-avian terms, this aphorism is a reminder that all taxes do some combination of two things: raise revenue and change behavior.[8] The more a tax does of one, the less it will be able to do of the other. As an example, a gasoline tax that raises a lot of revenue may do little to reduce gasoline consumption, because people only pay the tax if they keep consuming fuel. By the same logic, a gas tax that dramatically reduces fuel consumption won't raise as much revenue, because people who don't buy gas also don't pay the tax.

The tension between changing behavior and raising revenue gives rise to a longstanding piece of conventional wisdom in public finance: a tax designed to raise revenue should apply a low rate to a broad base. The low rate makes it less likely that people will change their behavior to avoid the tax, while the broad base compensates for the low rate. Each individual unit is taxed less, but there are more units, so the total revenue remains high. Not every tax is designed to raise revenue (as we discuss more below) but when revenue is the goal, the consensus is clear: the taxing authority should tax many things lightly.

This rule of thumb has an unpleasant political implication: it suggests that the surest path to revenue is taxing lots of people. People, of course, don't like taxes. It can be tempting, as a result, to reject the conventional wisdom, and assert that as a matter of simple arithmetic, taxing a few people heavily should yield the same amount of money as taxing many people lightly. Unfortunately, this intuition is often incorrect, because as tax rates rise people become more likely to change their behavior. To reinvoke the bird metaphor, at a certain point the goose stops sitting still, and starts to wriggle and hiss.

We can illustrate. Suppose (and this is a silly example but bear with us) that we wanted to raise $100 for street repair from a group of 100 people, and that we wanted to do so by placing a tax on crossing the street. One approach would be to charge everyone 10 cents every time they crossed. This is a low rate (just 10 cents) on a broad base (all people and crossings are taxed). If we assume everyone crosses the street at the same frequency, we'd raise $100 once everyone crossed ten times.

8 A public finance textbook will tell you that there are two taxes that can be levied without any behavior change: a pure tax on land value, and a flat tax paid per person (a "head tax"). Both these taxes, however, basically only exist in public finance textbooks, and are almost never implemented in real life.

Imagine, though, that we wanted the revenue faster than that. So we raise the rate, and charge $1 for each crossing. Now we raise $100 once everyone crosses the road once. The $1 tax, however, is larger and more noticeable than the 10-cent tax. Some people, as a result, might cross the road less frequently. This would slow our revenue collection, but not necessarily by much. It's hard to avoid crossing the street, and even if a few people cross less, the money will keep flowing.

A bigger problem is that now we're levying a higher tax on everyone, and some people might object. If we wanted to avoid political opposition, but still raise revenue quickly, we could — instead of charging everyone $1 apiece for crossing — charge the richest two of those people $50 apiece. Doing so solves the political problem: a tax that falls on only two people is much easier to sell than one that falls on everybody. But it might create two new problems. First is an issue of fairness: these two people might have more money than everyone else, but does this alone make them responsible for all street repair? Unless we have evidence that they benefit more from the street than the 98 other people, or that they are uniquely responsible for the street's problems, placing the entire burden on these two might seem inequitable.

Even if we ignore this fairness concern, we're still left with the second problem: our revenue now depends entirely on just two people. If the high rate makes them change their behavior, our revenue is jeopardized. If just one of these people leaves town, or stays on the sidewalk, our tax revenue is cut in half. If both do, we get no revenue at all.

Maybe that sounds implausible. For the sake of argument, in fact, let's make it maximally implausible, and assume these people are *really* rich, and can easily afford the tax. It turns out that doesn't matter. Taxes don't work simply because people can afford them. Taxes work when people *can't avoid* them. And while in general it's hard to avoid crossing the street — everyone has errands to run — as tax rates rise, actions that might otherwise seem cumbersome or convoluted start to look rational. For example: anyone who can afford $50 to cross the street and run errands can also afford to pay someone else, not subject to the tax, $25 to cross the street and run errands for them.[9]

The serious lesson embedded in this silly example is that when a tax rate is high and the base is narrow, behavior changes are both more likely (because the rate is more burdensome, might feel more unfair, and becomes more worthwhile to avoid) and more consequential (because each taxpayer who changes their behavior is a larger share of the total tax base).

9 Here's a real world example. New York City levies a progressive income tax on its residents. For most people this tax is unwelcome, but also unavoidable. For the very rich, however, the top rate of 3.9%, even though they can afford it, represents a lot of money — more money, it turns out, than the cost of avoiding it. The most reliable way to avoid the tax is to not legally be a resident of the city. For people who own property and work in the city, that's hard — but not impossible — to do. Anyone who is not sleeping overnight in the city for at least 182 days a year is not, for tax purposes, a New York resident. As such, there is a cottage industry that helps super-rich New Yorkers constantly document their location, and the end of the year often involves a flurry of nights spent on Long Island or in New Jersey. The inconvenience of such behavior pays itself back via millions of dollars of avoided taxes. This behavior may not be admirable, but it is predictable: people with the means to pay high tax bills often also have the means, and motivation, to avoid them. Progressive taxes for this reason must be designed to anticipate and mitigate such avoidance. See James Stewart, 2012. Tax Me if You Can. The New Yorker. March 12.

A few additional points. First, behavior changes are also more likely when tax rates rise sharply at a certain threshold (for instance, going from zero to $50 for a single crossing of the street). In public finance these thresholds are known as "notches."

Second, assuming a tax does substantially change people's behavior, that outcome could be good or bad, depending on whether we think society has too much or too little of the activity being taxed. If the answer is too much, then a tax rate high enough to change behavior might be beneficial, even if it means less revenue than would have been raised if rates were lower. Taxes on gasoline, for this reason, are sometimes seen as a win-win: they raise valuable revenue when Americans drive, but since it's also true that Americans burn more gas than is optimal, and create substantial pollution as a result, it isn't necessarily a loss if the tax deters some fuel consumption too.

An even starker example comes from "sin" taxes, such as taxes on cigarettes or alcohol. These taxes are often *intended* to change behavior, and the revenue is a secondary consideration. When New York City raised its cigarette taxes to $7 a pack in 2002, it did so with an explicit goal of reducing smoking. "'If it were totally up to me," the city's mayor said at the time, "I would raise the cigarette tax so high the revenues from it would go to zero." The harm done from smoking, in the mayor's mind, exceeded the benefit of more revenue, so it was better to tax smoking into extinction than to raise money off people continuing to smoke.

There are other activities, of course, that we worry don't occur often enough. Taxes levied on those activities might involve bigger tradeoffs. A portion of the U.S. income tax, for example, falls on savings. Since many Americans don't save enough, there is a good argument that this portion of the tax rate is worth being careful about. When a tax triggers a socially undesirable behavior change, that change itself, above and beyond any lost revenue, is considered a cost of the tax.

III. Measure ULA

We can use the ideas above to help think about Measure ULA. Specifically, we can ask if the tax rate is high or low and if the base is broad or narrow. From there we can think about whether the tax is likely to change real estate sales in the city of Los Angeles, and — if it does — whether that change would be good or bad.

We can make the following observations:

Measure ULA has high rates and a narrow base

Table 1 compares Measure ULA to a selection of other real estate transfer taxes in U.S. localities. Comparing such taxes can be tricky. Almost every city has a very low base rate designed to cover the cost of recording transactions (L.A.'s again, is 0.45%). Our interest is in the augmented rates designed to raise revenue for other purposes. As such, we ignore the recording-fee base rates for each locality, and compare the augmented rates. Thus we consider ULA's lowest rate to be the 4% it levies on transactions over $5 million, and San Francisco's lowest rate to be 0.71% it levies on transactions of over $250,000.

When we make this comparison, we see that ULA's rates are high.[10] ULA has the highest base rate, at 4%, and close to the highest top rate, at 5.5%. Consider that Culver City's top rate, of 4%, on sales of over $10 million, is lower than L.A.'s *bottom* rate, which applies to sales over $5 million. The other places with high top rates, moreover, also have more brackets and/or apply their top rates to higher thresholds. San Francisco has a 5.5% top tax rate, but this rate is the highest of five, and it applies only to sales of over $25 million. The same is true of New York City's top rate, of 3.9%. New York City's tax has six brackets, and the top rate applies only to sales of $25 million or more. L.A.'s top rate, in contrast, is one of only two brackets, and applies to sales over $10 million.

10 The table compares total transfer tax rates for each locality, e.g., for Los Angeles it adds the ULA rate to the pre-existing transfer tax rate of 0.45%.

Table 1. Selected Real Estate Transfer Taxes, USA

Jurisdiction	Lowest Rate	Highest Rate	Lowest Threshold	Highest Threshold	Taxable Brackets	Marginal Rates?
Los Angeles (ULA)	4.00%	5.50%	$5.1M	$10.3M	2	No
San Francisco	0.68%	5.50%	$100k	$25M	5	No
San Jose	0.75%	1.50%	$2M	$10M	3	No
Culver City	1.50%	4.00%	$1.5M	$10M	3	Yes
Santa Monica	0.60%	5.60%	None	$8M	3	No
Berkeley, CA	1.50%	2.50%	None	NA	2	No
New York State	1%	1%	$1M	NA	1	No
New York City	1%	3.90%	$1M	$25M	7	No
New Jersey	1%	1%	$1M	NA	1	No
Nantucket, MA	2%	2%	None	NA	1	NA
Washington, DC	1.10%	1.45%	None	$400k	2	No

Source: Compiled by authors from city websites. Table does not include any base documentary transfer rates.

ULA's high rates are paired with a narrow base. ULA has far and away the highest tax floor. Many jurisdictions begin levying their taxes when sales hit $1 million, and some jurisdictions tax almost all transactions (although every tax exempts some sales). The lowest threshold for taxation under ULA, in contrast, is over $5 million. This narrow base is by design: it was one of the measure's selling points. Measure ULA's campaign emphasized that the tax would fall on just 4% of all property transactions.[11]

Measure ULA has big notches

Measure ULA's tax rates aren't marginal. A property selling for just below its threshold pays no ULA tax, while a property selling for just above it is taxed on the entire sale amount. As Table 1 shows, this lack of marginality isn't unusual for real estate transfer taxes, but it does make ULA strikingly different from most taxes that have brackets. In a typical income tax, for example, crossing the threshold to another bracket increases your tax rate only on income above the threshold. That's so for a reason. The marginal rates keep the tax from triggering unintended behavior changes.

We can illustrate the role marginal rates play in minimizing unwanted behavior changes using the U.S. income tax code. In 2024, the United States taxed income between $11,600 and $47,150 at 11%, and taxed income above $47,150 at twice that rate (22%). That's a big jump, but the key is that *only the income above the threshold* was subject to the 22% tax. Anything below the threshold was still taxed at 11%. So if you earned $47,152, almost all

11 Proponents, for example, said that Measure ULA would affect "approximately 4% of real estate transactions in a given year and 72% of its revenue would come from properties sold for over $10 million. Under 3% of single-family homes or condos sold in a given year would be affected." See Dreier et al, 2022.

of your income was taxed at 11%, and only two dollars were taxed at 22%. Were this not the case, and exceeding $47,150 changed the rate on all of your income, the income tax would have a notch, and people earning just below the threshold could lose money by getting a raise (even a pretty big raise). These people would have a perverse incentive to keep their taxable income below $47,151 — by earning less, hiding their income, or working off the books.[12]

Now consider Measure ULA. When the measure was enacted, a property selling for exactly $5 million paid no ULA taxes, while a property that sold for a dollar more paid $200,000. That's a steep notch,[13] and it creates a strong incentive, for owners of properties that would sell at or near the threshold, to keep the price below it (or to not sell at all). Properties likely to sell for substantially more than $5 million, of course, are unlikely to be affected. But remember that ULA has a second notch near $10 million. A property selling for $1 less than $10 million paid just under $400,000 in taxes. One selling for a dollar above $10 million, in contrast, paid $550,000. In both absolute terms and as a proportion of the sale price, it is smaller than the notch at the lower threshold. But it isn't small. This leads to our next point.

Measure ULA's rates and notches are large enough to change behavior

Transfer taxes have not been extensively studied in the academic literature, but almost every study of them that exists suggests that they reduce the volume of real estate transactions in the jurisdiction that levies them, often by substantial amounts. Most of these studies, moreover, examine taxes with lower rates and smaller notches than ULA. An analysis of Toronto's transfer tax, for instance, which applied a rate of 1% to transactions over $400,000, found that it reduced sales volume by 15%.[14]

We reviewed 10 studies of transfer taxes, and all but one found that transfer taxes were associated with a significant reduction in either sales volume or residential mobility (a proxy for sales volume). The lone study that didn't find such an effect examined a very small increase in an existing transfer tax: Washington, D.C.'s decision to raise its tax by less than one percentage point during a booming market.[15] Overall, the literature suggests that the most common rates — which again, are notably lower than ULA's — tend to depress sales. *A priori*, this suggests that ULA's rates may depress sales as well.[16]

12 Here's how that would work. With marginal rates, an income of $47,150 implies taxes of about $5,000 (the first $11,000 taxed at 10%, and the remainder at 11%), leaving you with an after tax income of $42,150. If you got a 10% raise, bringing your earnings up to $51,900, the extra $4,700 would be taxed at 22% (about $1,000 in taxes), leaving you with a post-tax income of just under $45,000. The taxes take a bite, but the raise still leaves you with more money. If tax rates weren't marginal, moving from $47,150 to $51,900 would apply a 22% tax to all your income. Now your post-raise tax bill would be $11,400 (51,900 x 0.22), leaving your after-tax income at $40,500: lower than it was before your promotion. You were better off earning less.

13 To put this in perspective: a $1 increase in revenue triggers a $200,000 dollar increase in taxes. That $1 — the dollar that pushes the sale over the ULA threshold — is thus taxed at a marginal rate of 20 million percent.

14 Ben Dachis, Gilles Duranton, Matthew A. Turner. 2012. The effects of land transfer taxes on real estate markets: evidence from a natural experiment in Toronto, *Journal of Economic Geography*.

15 Joel Slemrod, Caroline Weber and Hui Shan. 2017. The Behavioral Response to Housing Transfer Taxes. *Journal of Urban Economics*. 100:137-153.

16 The studies we reviewed: (1) Carolin Fritzsche and Lars Vandrei. 2019. The German real estate transfer tax: Evidence for single-family home transactions. *Regional Science and Urban Economics*. 74:131-143, (2) Wojciech Kopczuk and David Munroe. 2015. "Mansion Tax: The Effect of Transfer Taxes on the Residential Real Estate Market." *American Economic Journal: Economic Policy*, 7 (2): 214–57. (3) Petkova, Kunka Petkova and Alfons Weichenrieder. 2017. Price and Quantity Effects of the German Real Estate Transfer Tax. *CESifo Working Paper Series No. 6538*. (4) Ben Dachis, Gilles Duranton, Matthew A. Turner. 2012. The effects of land transfer taxes on real

That conclusion might sound wrong. One might argue, for example, that "sellers have to sell" — people looking to buy a house often can't do so without selling the one they have. Selling a house, furthermore, already comes with numerous fees, such as payments to brokers or attorneys. The idea that one more tax could keep a house off the market might thus seem unlikely.

A few points are worth considering here. First, economic impacts occur on the margin. No one is suggesting that transfer taxes will drive real estate activity to zero. What they may do, for owners who are wavering about selling or holding, is tip the scale toward the latter. And many people *do* have choices about whether to sell their property. The "sellers have to sell" adage is most likely to hold for everyday owner-occupants whose circumstances make a move necessary: they've have taken a job in another state, or their family has outgrown the current unit. But not everyone pondering a move is in such a situation. Consider homeowners who might move across town because their kids are grown, and their house now feels too big. These empty nesters have much more discretion about if and when they put their home on the market. Consistent with this reasoning, some research shows that transfer taxes tend to reduce the volume of shorter local moves more than longer moves.[17]

A further and more important point is that not every property has an owner occupant. People own open land, vacation homes, investment homes, and commercial and industrial buildings. With property in these categories, the pressure to sell can be much lower, and the criteria by which an owner decides to sell are much different. Faced with a high transfer tax, the owner of a second home could simply operate it as a rental, while the owner of a commercial lot could maintain the existing use, or renovate it themselves rather than sell to someone else. On high value transactions like the sale of commercial buildings, moreover, brokers' fees become more negotiable, in contrast to the less flexible percentage amounts that accompany sales of single-family homes. It thus isn't the case that a seller of such land is bound, tax or no, to pay high ancillary charges.

Now consider Measure ULA: it *exempts* most sales by everyday owner-occupants. The typical owner doesn't have a $5 million property to sell. On the one hand (and this is important) ULA's high floor should blunt the tax's potential to reduce transactions. A tax that doesn't fall on most transactions cannot directly affect them. Within the small segment of properties that ULA targets, however, the high floor and high rate work in the other direction. ULA exempts the sellers with the least ability to avoid the tax, and explicitly targets those sellers with the most ability to do so. It is not unreasonable, therefore, to think that ULA will change behavior. It may not affect the typical single-family sale, but it should reduce higher-end transactions in Los Angeles.

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estate markets: evidence from a natural experiment in Toronto, *Journal of Economic Geography*. 12(2): 327–354. (5) Ian Davidoff and Andrew Leigh. 2013. How do stamp duties Affect the housing market? *Economic Record*. 89 (286):396-410. (6) Joel Slemrod, Caroline Weber and Hui Shan. 2017. The Behavioral Response to Housing Transfer Taxes. *Journal of Urban Economics*. 100:137-153. (7) Essi Eerola, Oskari Harjunen, Teemu Lyytikäinen, Tuukka Saarimaa. 2021. Revisiting the effects of housing transfer taxes, *Journal of Urban Economics*. (8) Christian Hilber and Teemu Lyytikäinen, 2015. Transfer Taxes and Household Mobility. London School of Economics, Spatial Economics Research Centre. (9) Michael Carlos Best, Henrik Jacobsen Kleven, 2018. Housing Market Responses to Transaction Taxes: Evidence From Notches and Stimulus in the U.K., *The Review of Economic Studies*, 85(1):157–193. (10) Timothy Besley, Neil Meads, Paolo Surico, 2014, The incidence of transaction taxes: Evidence from a stamp duty holiday, *Journal of Public Economics*,119, 61-70.

17 Hilber and Teemu Lyytikäinen (2015).

A Reduced volume of real estate transactions is bad for Los Angeles

Recall from above that when a tax changes behavior, our judgment of whether that change is good or bad will hinge on whether society has too much or too little of the activity in question. In the case of Measure ULA, then, the relevant question is whether the City of Los Angeles has too much or too little property turnover in general, and too much or too little high-value property turnover in particular.

The answer to both questions is almost certainly "too little." While house flipping and real estate speculation can be problems,[18] it is generally accepted that, as a result of Proposition 13, property in California doesn't change hands enough. This "lock-in" effect of Proposition 13 has many consequences.[19] It results in housing being misallocated, as empty nesters hold onto homes better used for families,[20] and labor being misallocated, as people become less likely to move to follow jobs. To the extent turnover is necessary for redevelopment, the lock-in effect can also impede the built environment's adaptation to changes in the economy (e.g., the repurposing of office space). Not least, lack of turnover, especially of higher-value properties, costs local governments (cities, counties and school districts) substantial revenue. It can also prevent housing development. We discuss the latter two issues in more detail below.

Less turnover means less local property tax revenue

Under existing California law, a property can only be reassessed — brought to full market value for property tax purposes — when it is sold or otherwise turns over (what the law calls "transfer of title") or when it is substantially improved (what the law calls "new construction"). Properties that do neither in any given year — which is most properties — are taxed based on the assessment they received at the date of their last transfer or substantial improvement. In practice, this can mean that many parcels' taxable values are decades out of date. Put simply, properties that haven't sold in a long time are often taxed far below what they are actually worth.

Table 2 shows both the prevalence and scale of this issue by tracking the average change in Net Taxable Value (or "NTV," the basis of the property tax calculation), before and after reassessment, for 10 years in the City of Los Angeles. Two points are obvious. First, reassessment is rare. In a given year, only about 4% of parcels are reassessed (the first column divided by the second column). Second and more important is that reassessment, when it occurs, is fiscally consequential. On average, reassessment results in a parcel's NTV almost doubling, which means that property tax revenues associated with that parcel will also almost double.

18 People can reasonably disagree about the harms of house-flipping, but flipping is most problematic when a home in a lower-income neighborhood, particularly one occupied by renters, is sold, rehabbed, and then quickly re-sold at a higher price. Measure ULA cannot realistically deter home flipping of this sort, because it is almost impossible to buy an inexpensive home in a low-income neighborhood and then quickly re-sell it for over $5 million. Measure ULA could only deter this kind of flipping if its tax threshold was much lower.

19 Nada Wasi and Michelle J White. 2005. "Property Tax Limitations and Mobility: Lock-in Effect of California's Proposition 13." *Brookings-Wharton Papers on Urban Affairs*: 59-97.

20 Proposition 13's impacts on residential mobility have been reduced, but not eliminated, by provisions allowing people over age 55 to carry their base tax rate to a new property when they move.

Table 2. City of L.A. Tax Roll Changes by Year

			A	B	C = B/A
Roll Year	Total Parcels	# of Reassessments (Sales & New Const.)	Average Starting NTV of Parcels Prior to Reassessment	Average NTV of Newly Assessed Parcels	NTV Multiple from Reassessment
2006	757,735	43,524	486,384	1,216,973	2.50
2007	763,090	35,245	563,691	1,427,612	2.53
2008	768,794	30,770	668,957	1,258,562	1.88
2009	774,066	37,212	651,588	941,615	1.45
2010	774,831	36,246	615,838	825,274	1.34
2011	777,831	38,544	601,144	1,062,725	1.77
2012	778,597	39,763	592,525	935,906	1.58
2013	779,143	40,028	593,690	1,209,390	2.04
2014	779,361	37,302	$632,133	1,712,072	2.71
2015	780,044	38,512	685,396	1,537,589	2.24
2016	781,305	38,061	740,579	1,637,792	2.21
2017	782,729	39,219	770,661	1,592,254	2.07
2018	783,792	34,528	832,805	1,490,825	1.79
2019	785,465	34,171	877,099	1,625,121	1.85
2020	786,759	32,488	967,564	1,622,623	1.68
2021	787,413	43,887	994,254	1,524,101	1.53
2022	788,099	33,531	1,000,447	1,708,551	1.71
2023	789,193	27,585	948,242	1,709,701	1.80
10-year Average		**35,928**	**844,918**	**1,616,063**	**1.91**

Source: County of Los Angeles. (2023). Assessor Parcel Summary (Rolls 2006 – Present) [Data set]. Los Angeles County Data Portal. https://data.lacounty.govhttps://arcg.is/1ujCim0, Los Angeles County

As context, California state law prevents the NTV of properties that *aren't* reassessed (that is, not sold or improved) from growing at more than 2% per year. As a result, L.A. County, like most counties in California, depends heavily on reassessments for most of the growth in its property tax revenue. Slowing the pace of reassessment, then, can slow the growth in revenue.

Table 3 further illustrates the importance of reassessment, using three years of City of L.A. data that show *how* NTV grows in the city. Columns A and B show that over these three years reassessments have accounted, on average, for about 70% of growth in the city's NTV (total NTV grew 5.4%, reassessment was responsible for 3.8%).[21]

Table 3. Growth in Aggregate Net Taxable Value (City of Los Angeles)

		A	B = MIN (A - 2%, 0)	C = MIN (A - 2% , 0)	D	E = D/C
Roll Year	**Aggregate NTV**	**Total NTV Growth**	**Min. NTV Growth Attributable to Sales & New Construction (%)**	**Min. NTV Growth Attributable to Sales & New Construction ($)**	**Total NTV of Sales >=$5mm**	**% of NTV Growth from Sales >=$5 million**
2021	$686,892,409,151	4.4%	2.4%	16,506,051,652	11,806,592,635	72%
2022	735,995,357,734	7.1%	5.1%	37,893,197,499	12,909,130,441	34%
2023	778,110,541,909	5.7%	3.7%	28,962,891,714	9,291,162,503	32%
Average		**5.4%**	**3.8%**	**27,787,380,288**	**11,335,628,526**	**41%**

Percent of NTV Growth Attributable to Reassessment (B/A) 70%

Source: County of Los Angeles. (2023). Assessor Parcel Summary (Rolls 2006 – Present) [Data set]. Los Angeles County Data Portal. https://data.lacounty.gov

The table's next columns show that not all reassessed parcels contribute equally to this growth. Properties that sold for $5 million or more represented just 4% of the city's transactions from 2021 to 2023, but these properties, because of their expense, represented 27% of the total transaction value in the city. As a result, they accounted for *41%* of the growth in NTV.

In sum, a tiny share of parcels are reassessed each year, and a tiny share of those reassessments account for a disproportionate share of the city's increase in taxable value. This small share of transactions is also the segment of transactions explicitly targeted by Measure ULA. The fiscal implications for the City of L.A. are obvious; if Measure ULA reduces these transactions, the city could lose property tax revenue. Nor are we alone in observing this tight relationship between real estate sales and property tax revenue. L.A.'s City Controller, in a 2023 revenue report, noted that the two phenomena have a "built-in connection." "In most years", the Controller wrote, "more than 50% of property tax growth is due to property transfers, since properties get reappraised upon change of ownership. Hence, high levels of property sales can be a huge driver of property tax growth."[22]

21 We say "the city's growth in taxable value" for ease of exposition. As we discuss below, parcels within the City of L.A. are actually taxed by multiple local governments: the City, the County, and L.A. Unified School District,as well as various special districts.

22 Kevin Meija, 2023. Revenue Forecast Report. Los Angeles City Controller's Office. March. https://controller.lacity.gov/revenueforecastfy2024.pdf

But the fiscal implications of property turnover extend beyond L.A.'s borders. Property taxes on parcels in the city of Los Angeles fund multiple local agencies: the city itself, the county, the L.A. Unified School District, the Los Angeles Community College District, and various special districts. The City of L.A., in fact, claims a minority (about a third) of the total property tax revenue raised within its borders. The rest goes to these other, overlapping government agencies.

If Measure ULA does in fact depress transactions, the presence of these overlapping tax authorities will make the measure's fiscal consequences complicated. Successful transactions will result, for the City of L.A., in a one-time deposit into an affordable housing fund. But when ULA deters sales, the city, county, schools and districts will all be deprived of the longer-term revenue streams that come from reassessment.[23] Because these latter streams compound, the revenue losses can quickly add up.

Measure ULA has been portrayed primarily as a new source of revenue — the fiscal analysis that accompanied it on the Los Angeles ballot said only that it would raise funds — but this portrayal is incorrect. If the measure deters some reassessments, it will raise revenue through one channel but reduce revenue through another. In Section IV of this report we offer a rough estimate of early property tax revenue losses from ULA, which we find to be approximately $25 million in the first year. Our point isn't that these losses are catastrophic — the county's total annual property tax revenues run into the billions — but that they *are* losses, and because they happen quietly, manifesting not as expenses but as revenues that go uncollected, they can be easy to overlook. Over time, moreover, they could become fiscally meaningful.

Less turnover means less new housing

Measure ULA could reduce the production of new and much-needed multifamily housing, by making it difficult for builders to buy sites. Unlike the fiscal concerns we outline above, which were not discussed before ULA's approval, the concern that Measure ULA might impede housing development did come up. Proponents dismissed this concern, however, based on the following reasoning. If one looks at the typical moderate density development built in the early 2010s, it began with a land purchase of less than $5 million (making it exempt from the tax) and by 2022 it had not been sold (also avoiding the tax). Based on this evidence, proponents concluded that Measure ULA would not affect most new residential projects.[24]

Two aspects of this reasoning make it unreliable for gauging ULA's impact. The first mistake is the assumption that housing development on medium density parcels is a reasonable proxy for multifamily development overall. The second mistake is the assumption that a transfer tax can only affect developments that are sold. We will discuss each in turn.

23 Although we do not examine it here, it's worth noting that a reduced volume of higher-end transactions can also have implications for state and federal revenues, because real estate transactions are also subject to state and federal capital gains taxes. A recent analysis of a proposed transfer tax in Massachusetts, for example, predicts that the tax would raise considerable funds for affordable housing, but reduce total property tax revenues, and reduce state and federal capital gains revenues See Gilles Duranton, Lu Han, and Matthew Turner. 2024. *The Negative Impact of a Massachusetts Real Estate Transfer Tax*. Massachusetts Real Estate Community Alliance. May 25.

24 Shane Phillips and Maya Ofek. 2022. How Will the Measure ULA Transfer Tax Initiative Impact Housing Production in Los Angeles? October 10.

Medium density development is not representative of development overall. While it is true that many multifamily *projects* are built in moderate density zones, most new multifamily *units* are not. Projects in moderate density zones account for only 10-20% of multifamily units built each year. Most multifamily units are instead built in high density developments, on high density parcels. These projects almost always begin with a land purchase exceeding $5 million,[25] and — because of financing practices common to large developments — often end with a sale exceeding $5 million as well.

That latter sale is what matters most. Investors in large residential projects (often pension funds, university endowments and other institutional sources) typically require that the developer sells the building as soon as it is complete. This requirement subjects those developments to ULA, and means ULA can have a large deterrent effect on them. Developers of these projects, anticipating the tax they will have to pay when the building is complete, will have little choice but to bid less for parcels they need. Specifically, they must reduce their bids by at least much as their anticipated ULA tax burden. That might not sound like much, given that ULA is a roughly 5% tax, but the key here is that the tax is levied on the *total value of the finished development*, and the land cost may only be 20-30% of that total value. The tax, as a result, can force the developer to make an uncompetitive bid.

Table 4 shows an example. Suppose a developer needs a $6 million site, on which he plans to build a project that he will then sell (per the agreement with his investors) to a long-term owner. He anticipates spending $18 million to build the project. If he builds in the normal expected profit margin of 20%, then the expected value when he sells the finished building is just over $31 million. Crucially, *that* number, not the $6 million, is the denominator the ULA tax is applied to. A 5.5% tax on a $31 million sale is $1.7 million. The developer must therefore lower his bid by $1.7 million, which is almost 30%, not 5%, less than what he could have bid before Measure ULA.

Table 4.

Acquisition		6,000,000
Development		20,000,000
Total Project Cost		**26,000,000**
Required Margin	20%	5,200,000
Required Proceeds from Sale		**31,200,000**
ULA Tax	5.50%	(1,716,000)
Reduced Residual Land Value		4,284,000
Discount Needed for Feasiblity		**29%**

25 There are instances, of course, where large high density developments begin with the assembly of many lower-density parcels. In 2023, Carmel Partners purchased over 20 single family parcels to build a multifamily development of over 600 units. Such instances are the exception rather than the rule. See Steven Sharp. 2023. Single Family Houses Cleared for 600-plus Apartments. *Urbanize LA*. July 19.

In a city with large amounts of vacant land, this lower bid may not matter as much. But Los Angeles has almost no vacant land. The vast majority of sites in Los Angeles already hold a going concern. In this situation, so long as there are other bidders for a site who are *not* developers, and who plan to simply to keep and operate whatever is already there, a would-be housing developer is at a serious disadvantage. Suppose the developer wants a site that currently holds a carwash; if he buys it, he'll demolish the carwash and construct an apartment building. Because of ULA, his maximum bid for that site could easily be 30% lower than the bid of a competitor who just wants to operate the carwash. His competitors can pay more, and are likely to emerge with the parcel. This is an important point: even if these high value parcels are sold, ULA makes it less likely that they will be sold *for new housing, or new construction of any kind*.[26]

Let's now turn to the second mistake. The discussion above assumes that the developer sells the property and thus must pay the tax. With many smaller projects, however, the developer does not sell the property after building it. Is it correct to think that ULA has no impact on these projects? It is not. A tax liability can be costly even for developers who never pay it, because it changes the terms on which they can secure equity and debt.

Nearly all developers need equity investors, and investors typically use a hypothetical sale price to gauge their prospective returns. They judge the value of an investment by what they could get if they did sell it. ULA reduces that price, and thus reduces the appeal of a project.

When a developer needs debt, meanwhile, ULA rears its head in a different way: it changes the lender's worst-case scenario. A bank making a development loan does so considering what it is likely to get back if the project goes bust, and they need to foreclose on the property and sell it at an auction. Since Measure ULA does not exempt foreclosure sales, the tax changes the bank's calculus. The bank will pay the tax when it sells the property, which means it will recover fewer of its losses. The development thus becomes riskier in the bank's eyes, and the bank will revise downward the amount they are willing to loan. Some projects will as a result be unable to get financing at all, while other projects will become more expensive. In both cases fewer units are built in L.A.[27]

In summary, the case for ULA not affecting housing production is constructed on two mistakes. It is not correct to think that development on medium density parcels is representative of most development in the city, and it is not correct to think that the tax has no bearing on projects where the developer has no plans to sell.

26 To extend this point: the seller will still capture a windfall profit, and the windfall will be bigger if the owner avoids selling to a housing developer. In a city with a housing shortage, this is a suboptimal incentive.

27 One might argue that it doesn't matter if ULA reduces market rate housing production, because market rate housing does little to promote affordability. Such an assertion is wrong: neither theory nor evidence support it. New market rate housing has strong social benefits, and contributes substantially to affordability in places where the housing supply is constrained. (See Shane Phillips, Michael Manville and Michael Lens, 2021. Research Roundup: The Effect of Market Rate Development on Neighborhood Rents. UCLA Lewis Center. February. Also Michael Manville, 2021, Value Capture Reconsidered. UCLA Lewis Center.) Even if one views this evidence skeptically, and believes that only below-market housing is beneficial, it's worth remembering that in Los Angeles market rate housing is an important source of below-market housing. A large share of market rate development in L.A. uses either the Transit Oriented Communities or Density Bonus incentive programs, and is thus subject to inclusionary requirements. Given the high per-unit costs of building affordable housing in Los Angeles, if ULA deters high density market rate construction it may prevent as many below-market units as it enables. (For this latter point see the RAND/UCLA Lewis Center analysis of ULA by Shane Phillips and Jason Ward.)

ULA taxes work as well as windfalls, and bad luck alongside good

Real estate transfer taxes seem most fair when they are levied on transactions whose value comes mostly from a general phenomenon of rising prices, rather than from the effort of individual property owners. Someone who buys a vacant lot in a depressed downtown, leaves it unimproved, and then years later sells to a developer during boom times will profit handsomely, but not through his own industry. It was other people who exerted effort to bring the downtown back to life and make his property more valuable.[28] He profits from their work, not his.

The developer who buys the lot and puts a building on it, conversely, will probably be able to sell it at a profit, but much of that increased value *will* owe to her own industry: she converted vacant land into a building, and she doesn't benefit from the original windfall appreciation (indeed, she is the one who *delivered* the windfall profit to the speculator, when she paid market rate to buy the land from him). That doesn't mean that none of her profit should be taxed, or that luck plays no role in her success,[29] but it does suggest that an ideal tax would draw a distinction between her industry and the previous owner's idleness. A well-designed tax might also account for the risk she took on. Development is never a sure thing: a developer can pour money into a project, complete a building, and nevertheless lose money. When this occurs the *site* becomes more valuable but the *person* (or at least the investment) loses rather than gains.

A tax that draws such distinctions perfectly may be impossible. ULA, however, doesn't attempt to draw these distinctions at all. ULA is triggered by a high price, and treats all high-priced transactions the same. But many properties that transact for large amounts of money yield relatively little in profit, and many properties that change hands for smaller total dollar amounts are highly profitable. Similarly, many parcels that gain value do so because the owner improved them, not because (or only because) an overheated real estate market delivered a windfall.

When a vacant lot is turned into an apartment building and sold, ULA will tax the total value of the improved parcel. When empty office space is repurposed and sold, ULA will tax the full value of that sale. Even situations that appear at first blush to be straightforward windfalls may be deceptive. Consider an example that Measure ULA's proponents cited in the months leading up to the election. The picture they painted, of a huge and opulent hillside estate, seemed to perfectly embody the idea that ULA would tax excessive luxury and unearned profit. Here is how the proponents described it:

> *"In 2014 the 14,000 square foot mansion at 1369 Londonderry Place sold for $6 million. In 2021, it sold again for $26 million. In just seven years, this 6 bed/10 bath home located in the Hollywood Hills increased in value by $20 million. If Measure ULA had been in place ... this sale would have generated $1.4 million ... just 7% of the property's appreciation value gained during that period."*[30]

28 The canonical explication of this logic is Henry George's 1849 book *Progress and Poverty*.

29 If nothing else, prices might continue to rise while she permits and constructs the building, and this would represent good fortune more than good work.

30 Dreier et al. 2022.

The numbers here are mostly correct, but many of the details aren't. And the details matter. In 2014, a developer paid $6 million not for the mansion, but for the *land*, which at that time was largely vacant. The parcel's assessed value at the time of purchase was about $380,000. The developer then proceeded to *build* an absurdly lavish mansion, which included not just the 10 bathrooms but also a cryo chamber, yoga studio and a full-size movie theater. (When the mansion was complete, he threw multiple parties in it, in violation of the city's COVID-19 rules). And while the property did in fact sell for $26 million, the developer had spent $30 million building it. The sale, which was a loss, was executed amidst his various bankruptcy proceedings.[31]

The point here isn't that the developer was a victim. (Few people, we expect, will lose sleep over his troubles.) The point is only that big numbers don't automatically mean big gains, let alone big unearned gains. A transfer tax on 1369 Londonderry Place, as unsympathetic as its owner may be, would be a tax on a business loss. Similarly, a post-COVID vacant office building might trade at well above $5 million dollars, but nevertheless represent a large loss for the seller. In 2016, Rising Realty Group purchased the historic Trust Building in downtown Los Angeles for $80 million, and invested another $40 million in it. In 2023, amid a wave of post-COVID vacancies, UCLA purchased the building for a reported $40 million.[32] High prices don't always mean high profits.

Note too that there's another side to this problem. ULA does not just wrongly tax some work as though it was windfall. It also chooses *not* to tax many, and perhaps most, of the city's property windfalls. The majority of windfall property appreciation in L.A. occurs in properties under $5 million, simply because the vast majority of properties are worth less than this threshold. For every transaction with a truly eye-popping price tag, many more sales occur that involve lower absolute numbers but large percentage increases in capital gains. And these gains, unlike situations where an entire building was constructed, often cannot be chalked up to large improvements. Consider a home we chose more or less at random: the single-family dwelling at 732 N Vista St. In 2011 this property sold for $949,000. In 2019 it sold again for $1.6 million — a 45% increase even in inflation-adjusted terms. While some of this increase could be the result of remodelling, tax records make clear that the owner did not add any square footage between sales. Thus at least some and perhaps most of this profit was pure windfall. Similar situations play out daily across Los Angeles. But none of these windfalls are taxed by Measure ULA.

Measure ULA's exemptions are preferential and uneven

Not all sales above the ULA threshold are subject to the tax. The fact that ULA has exemptions isn't unusual: most taxes do. The specific nature of ULA's exemptions, however, is puzzling. A seller is exempt from Measure ULA if the *buyer* is a nonprofit organization with "a history of affordable housing development and/or affordable housing property management" or a nonprofit without such a history that partners with one that does.[33]

The idea here is that the exemption will encourage property owners to sell their land to affordable housing developers, and give those developers a leg up in the city's overheated land market. This claim may or may not

31 Christopher Bokum. 2021. 1369 Londonderry Place Files for Chapter 11 Bankruptcy Amid Controversy. *Levelset*. July 30.

32 Teresa Watanabe and Roger Vincent. 2023. UCLA to Expand in Downtown. *Los Angeles Times*. June 29.

33 This language is from the ULA statute.

be accurate, but regardless of its efficacy the exemption as written has two problems: it doesn't require that any affordable housing actually be built, and it doesn't extend to the largest single source of deed-restricted affordable housing in the city.

With respect to the first issue: Measure ULA only says that if a nonprofit with a "history of developing affordable housing" buys land, that transaction will be exempt from the tax. It does not specify that the nonprofit needs to *build affordable housing* on that land. Pointing this out might seem like nitpicking, and undoubtedly it will be rare for a housing nonprofit to buy land and not build. But rare instances do arise.[34]

The bigger issue is the second one. The buyer exemption is restricted to *nonprofit* organizations that build affordable housing. That clause might sound benign or even redundant, but a signature attribute of America's largest affordable housing program — the Low Income Housing Tax Credit (LIHTC) program — is that it enables both nonprofit and for-profit development of deed-restricted affordable housing. The latter, moreover, reliably creates more affordable housing than the former. LIHTC is the single-largest source of affordable units in the city of Los Angeles, and the majority of those units have been delivered by for-profit developers.[35] Both types of developers, it should be emphasized, deliver below market-rate units. The "profit" in for-profit LIHTC doesn't come from charging higher rents, but what the developer chooses to do with the revenues it collects.[36] The tax status of the developer has *no bearing* on the affordability of the LIHTC housing being delivered. Yet ULA's exemption is denied to the for-profit providers. This denial is puzzling. If the goal of the exemption is to encourage the production of deed-restricted affordable housing, there is no reason not to extend the exemption to all experienced developers of such housing, and to require that the purchased property actually result in new units.

34 An additional oddity in ULA's exemption rules, though perhaps a more defensible one, is that ULA does not exempt nonprofit housing providers who sell land to finance their operations. This too might seem like a rare occurrence, but it happened multiple times in ULA's first year, and housing nonprofits paid millions to the city in transfer taxes. See Jack Flemming. 2024. Why Us? Housing Nonprofits are Paying Millions in ULA Tax. *Los Angeles Times*. August 24.

35 Exact counts of affordable housing in Los Angeles vary, but Appendix 2.6 of the City of L.A.'s Housing Element suggests that LIHTC is the largest single source of affordable units, accounting for 43% of the total. The Department of Housing and Urban Development maintains a LIHTC database that offers slightly different numbers, but shows that 55% of LIHTC units in Los Angeles were built by for profit developers. Nationwide, about 75-80% of LIHTC units are built by for-profit developers.

36 To elaborate a bit: For-profit LIHTC developers are less likely to reinvest developer fees and more likely to use those fees, and their (restricted) rent payments, as profit. Nonprofits are more likely to reinvest both. Both groups, however, must abide by LIHTC's affordability requirements.

IV. Examining Measure ULA's Impact

We now turn to the question of whether Measure ULA has reduced the volume of real estate sales in Los Angeles. Empirically answering this question isn't simple. Sales in Los Angeles certainly fell after ULA was implemented, but ULA didn't occur in a vacuum. Interest rates rose during this time, causing sales to fall nationwide. L.A.'s decrease in transactions, then, could in part be the local manifestation of a national trend, not the result of a specific local policy.[37] Real estate transactions also fell in Sacramento and New York, for example, and those places were obviously not affected by ULA.

On the other hand, the fact that real estate transactions declined elsewhere doesn't mean ULA didn't contribute to the decline we see in Los Angeles. While transactions fell in both Los Angeles and New York, for example, ULA may have made L.A.'s decline larger than it would otherwise have been. We cannot assess this possibility simply by looking at transaction volumes across big cities. Cities and their real estate markets differ in many ways, and simply comparing L.A. to other big cities risks making apples-to-oranges comparisons.

Fortunately, the nature of Measure ULA offers us a more reliable way to isolate its impacts. ULA only applies to a subset of county transactions: sales over $5 million, in the City of L.A., on or after April 1, 2023. Sales at this price point before April 2023 were not affected. And no sales, at any time or any price, were directly affected by ULA in the remainder of L.A. County's cities. ULA thus created a "natural experiment" — it offers a "treatment" group (the City of L.A.), and a "control" group (the rest of the county), and allows us to observe how real estate activity changed in both groups before and after the measure was enacted. This "before-and-after, within-and-without" research design is a gold standard for policy evaluation. By focusing on variation within L.A. County, moreover, we are able to hold many factors constant: interest rates are the same for all of Los Angeles County, and the county's cities share the same labor market and climate. Holding these factors constant helps us isolate the specific impact of Measure ULA. If transactions change in Los Angeles after ULA and *don't* change (or change as much) in the rest of the county, we have strong evidence that ULA was responsible.[38]

To carry out this analysis, we collected data on property transactions in Los Angeles County from January 2020 through December 2024. The data are primarily from the L.A. County Assessor, though they were provided to us by Commonwealth Land and Title Company. The data include the date and location of all property transactions, as well as an array of other information, such as the size of both the building and parcel, its tax status, and the property's zoning and as-built land use designation.[39]

37 Dreier et al, 2024.

38 A potential confound is that Santa Monica and Culver City also enacted transfer taxes during this time. Culver City's Measure RE became effective on April 1, 2021. Santa Monica's Measure GS took effect on March 1, 2023. Measure RE and Measure GS have different thresholds, tax rates and exemptions than Measure ULA (as shown in Table 1). In all our regression analyses we either drop these two cities or explicitly control for their presence.

39 Some transactions involved a single buyer purchasing multiple parcels. When this occurs, the dataset can overcount, by assigning a new row for every parcel involved, but keeping the same document (transaction) number and price. In these instances we deleted any duplicate rows, so that each single transaction was counted only once.

We used the data on the price, time and address of each transaction to first identify sales above the ULA threshold ($5 million initially, and $5.15 million 15 months later) and to then identify the subset of those sales that occurred in the City of Los Angeles on or after April 1, 2023 — the transactions that would generally be subject to Measure ULA.

We validated our data by comparing our list of ULA sales to a list of transactions that paid the tax, which we obtained from the City of L.A. via a Public Records Act Request. The two lists were largely identical, although the city list included approximately 98 transactions that the Assessor data did not. We are unsure what caused this discrepancy, because the city data were difficult to check further: unlike the assessor data, the city data do not consistently contain information about the address of the property being sold or its owner; sometimes an entry contains little more than a date and an amount paid.[40] As a check on our main results, we add these unmatched ULA sales to the Assessor data in parts of the analysis we carry out below.

Our main dataset contains about 338,000 transactions, 34% of which (about 115,000) were in the City of L.A.[41] The vast majority of all transactions (276,000) are single-family homes,[42] another 35,000 are multifamily properties and 12,000 are commercial or industrial. The vast majority of sales — 96% — are also not above the ULA threshold. Within Los Angeles, just over two percent of all sales that occurred after ULA went into effect were above the ULA threshold. Of these ULA-eligible transactions, approximately 60% were single family residential, another 18% were multifamily residential, and 20% were commercial or industrial.[43]

Using these data, we can first descriptively examine trends in real estate activity, and then examine them with a statistical analysis called a difference-in-difference regression.

40 Our confusion about why the two lists don't match is amplified by the fact that some addresses on the City's list appear to be outside the City of L.A. We checked these addresses and their presence cannot be explained by location on parcels that cross city boundaries. It's possible that some transactions are simply not recorded quickly by the assessor, or that some recorded transactions do not make it into Commonwealth's data. If we assume that such errors are equally likely for all jurisdictions, then when we append the City data to the assessor data (as we do in some regressions in the Appendix) we will overcount post-ULA transactions in L.A. relative to the rest of the county, and bias the regression toward a false negative result.

41 Los Angeles has about 38% of the County's housing units, so this proportion seems sensible.

42 We include both fee-simple single family homes as well as individual residential condominium units under the header "single family residential."

43 These proportions differ slightly from those recorded on the ULA dashboard for the period ending December 31, 2024. The city shows a single family share of 57%, a commercial share of 22%, and a multifamily share of 11%. This discrepancy probably has multiple explanations. First, as discussed above, the city includes 98 transactions that do not appear in our data. Second, the city's dashboard categorizes its ULA transactions differently from how the county categorizes transactions. For example, the city categorizes 8% of transactions as "unknown" and another 3% as "vacant," neither of which are categories in the county data. Third, the city and county may classify some multifamily buildings differently: an apartment building with retail on the ground floor may be "commercial" in one data set and "multifamily residential" in another. Fourth, some transactions we record as above the ULA threshold may have received exemptions, if their buyers were affordable housing nonprofits.

Descriptive results

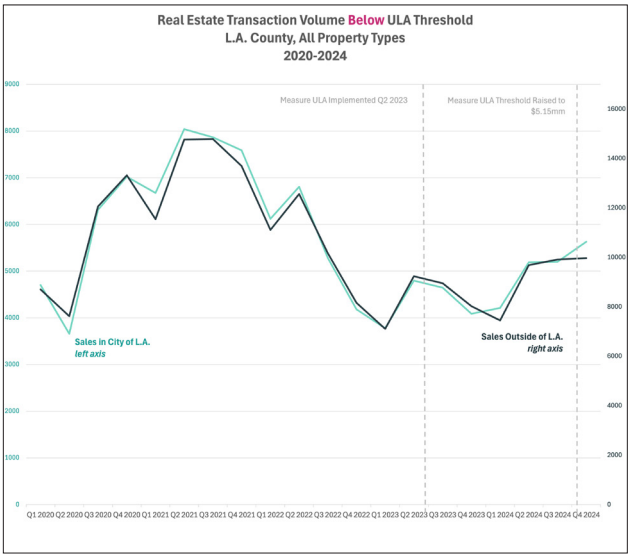
Measure ULA didn't apply to most transactions, so in raw numbers its introduction, if one looks at total sales countywide, wasn't accompanied by large changes. In our data, total real estate transactions in the City of Los Angeles fell 56% after ULA's introduction, while in the remainder of the county they fell 54%. This two percentage point difference isn't nothing, but it also isn't large.

A different story emerges, however, if we examine sales above the Measure ULA price threshold — and these transactions are, as we have discussed, particularly important for housing, employment and tax revenue growth. From January 1, 2020 to March 31, 2023 (prior to ULA), there were 3,257 sales above $5 million in the City of Los Angeles, accounting for 4% of the city's transactions. In the period after ULA, the number of sales above the threshold fell to 758, a 77% decline. In the rest of the county, meanwhile, sales above the threshold actually grew.

Figures 1–4 graphically depict how different types of transactions changed (or didn't change) in L.A. County as Measure ULA came online. Each figure has two panels, and each panel plots two trend lines: the trend in transactions below the ULA price threshold inside and outside the City of L.A. (left panel) and the trend inside and outside the City of L.A. for transactions above the ULA price threshold (right panel). Figure 1 shows these trends for all types of transactions, while Figures 2–4 break the transactions down by the largest property types (multifamily residential, commercial and industrial, and single-family residential).

The left panels show that sales under the ULA price threshold, inside and outside the City of L.A., moved in sync before *and* after ULA's introduction. ULA has no noticeable effect on them. The right panels show something different. In all figures, higher-priced sales inside and outside Los Angeles move largely in sync with each other *before* Measure ULA. This common movement does include a decline: throughout 2022, as interest rates rose and other macroeconomic conditions turned, sales inside and outside L.A. were falling. When ULA begins, however, the two lines diverge. The decline in the City of L.A., relative to the rest of the county, gets steeper and deeper, and sales in L.A. City level off well below sales elsewhere. We thus see unique downward movement that occurs only in the City of L.A., only for sales of $5 million or more, and only after Measure ULA is introduced. The strong implication is that Measure ULA is responsible for this movement, and that it depressed higher-priced transactions in Los Angeles.

Figure 1.



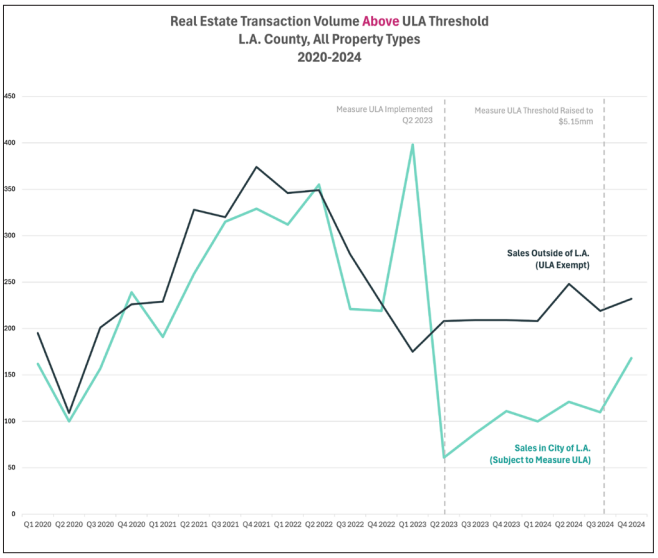
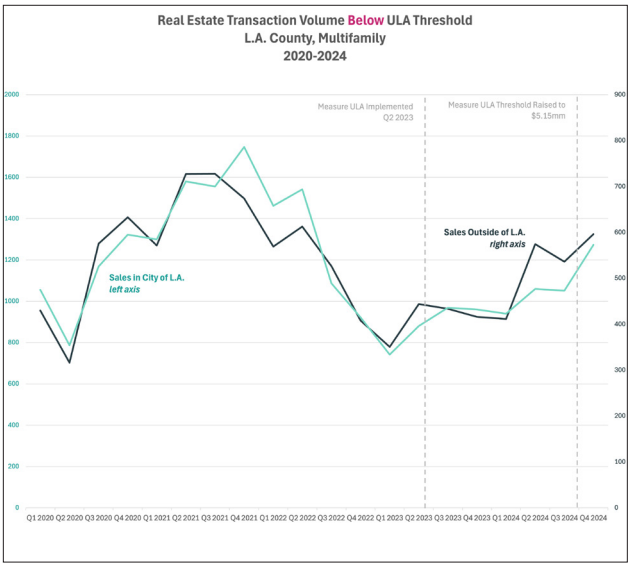
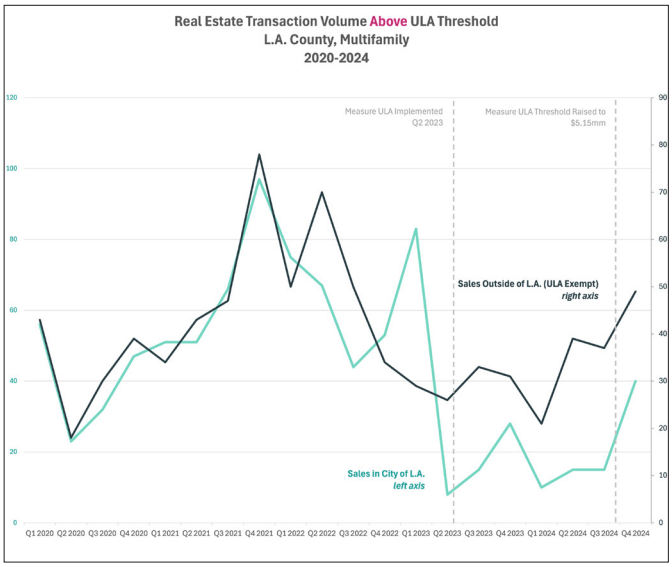


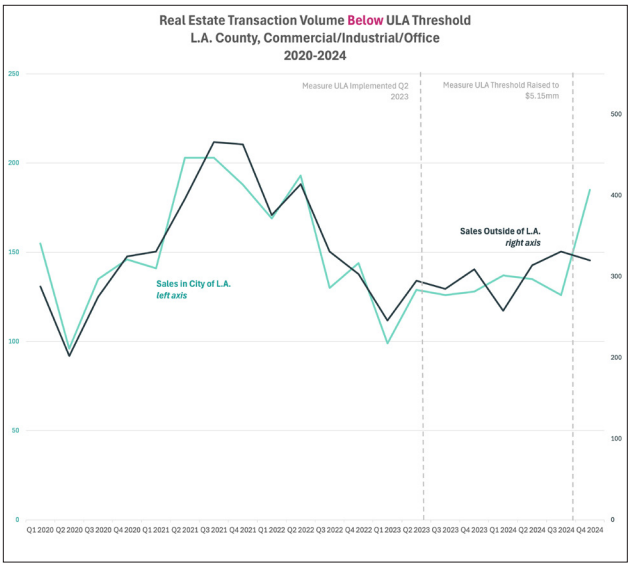
Figure 2.





Source for all figures: L.A. County Assessor Data, provided by Commonwealth Land Title Insurance Company

Figure 3.



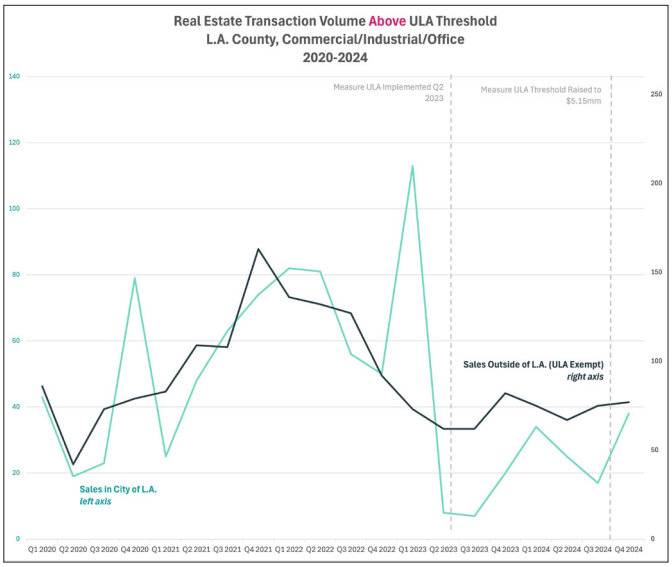
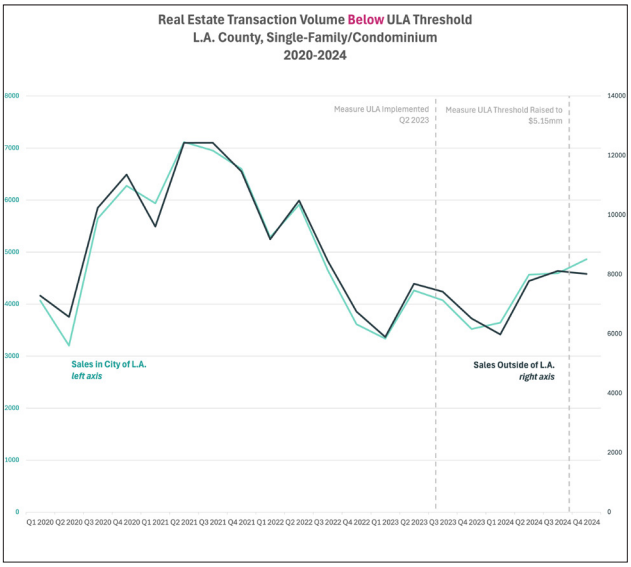
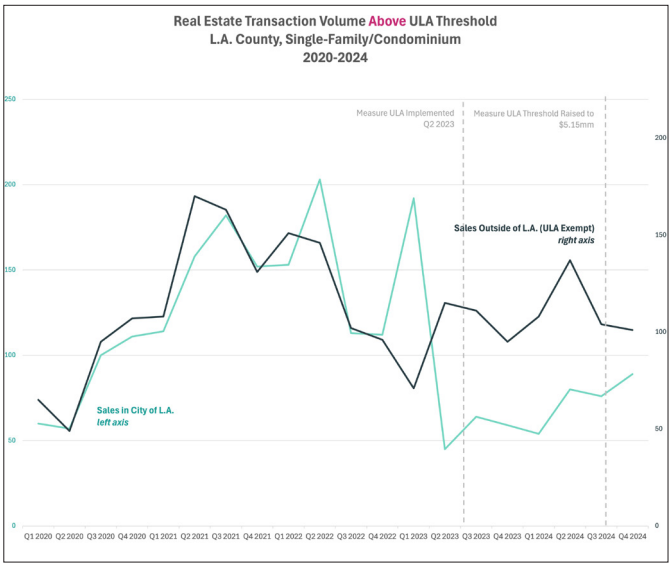


Figure 4.





Source for all figures: L.A. County Assessor Data, provided by Commonwealth Land Title Insurance Company

Figures 1–4 show a *temporal* notch: a behavior change when the tax is enacted. A second way to graphically examine patterns around Measure ULA is to look for a *price* notch. We do so in Figures 5–8. These figures plot the absolute number of sales at different price points, before and after ULA's introduction, inside and outside the City of L.A.. Once again a pattern is clear. Prior to Measure ULA, Los Angeles prices show little discontinuity at $5 million (Figure 5). If anything, sales just above $5 million are slightly more common than sales just below. After ULA, in contrast, a large notch forms at $5 million (Figure 6). Tellingly, this notch does *not* form in the remainder or L.A. County (Figures 7 and 8).[44] The implication, again, is that ULA reduced higher end real estate activity in Los Angeles.

Figures 5-8. Notching at $5 million after Measure ULA, Only in Los Angeles

Figure 5.

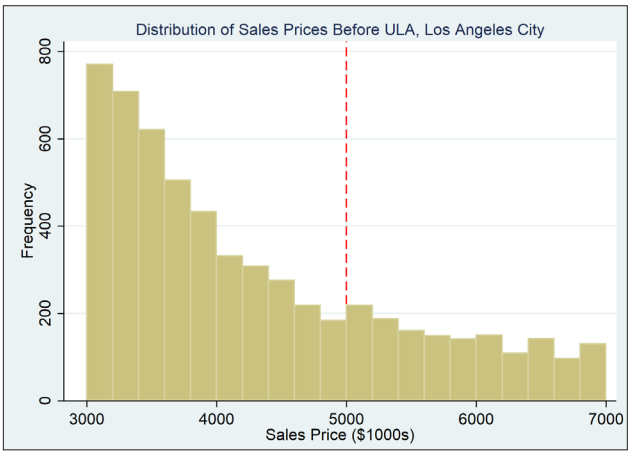


Figure 6.

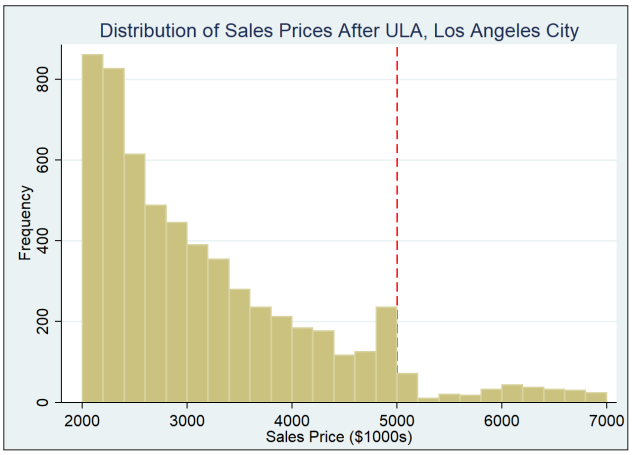
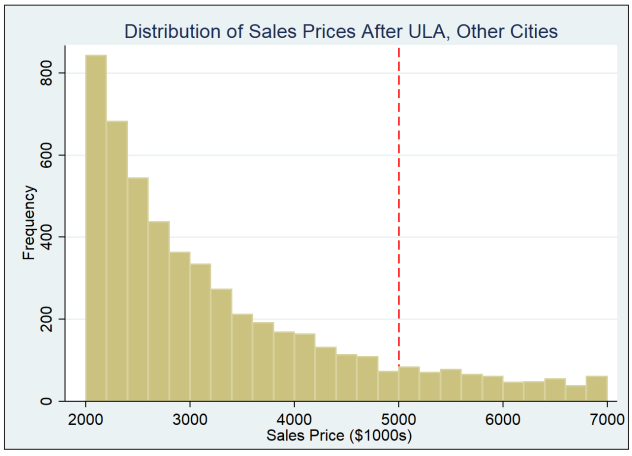


Figure 7.

Figure 8.



44 In the appendix we show graphs that exhibit a similar, but smaller, pattern around ten million dollars.

These graphs are of course only suggestive. They show that sales fell more sharply in L.A. after Measure ULA than in other places, and that sales bunched near the ULA thresholds after ULA and did not in neighboring cities. The graphs thus imply that interest rates alone cannot explain the decline we see in the City of Los Angeles. The graphs do not, however, tell us definitively that Measure ULA *does* explain them. It is possible, for example (though arguably not plausible), that expensive properties in Los Angeles were located in neighborhoods that became less desirable for reasons unrelated to ULA, or that properties in Los Angeles differ systematically from properties in the remainder of the county, in size or other attributes, and that this made them less likely to be sold.

A more plausible idea is that while Measure ULA reduced real estate transactions in L.A., its impact could be one-time and short-lived. Figures 1–4, for example, don't just show that $5 million sales in L.A. plunged after Measure ULA. They also show that such sales dramatically *rose* right *before* ULA. The explanation for this surge is simple: property owners rushed to sell before the tax kicked in. One could argue, based on this fact, that ULA was responsible for the transaction decline, but that the decline was a one-time occurrence. The tax, after all, only begins once; there are no more opportunities to rush sales forward in time. Sales should recover as time goes on.[45]

What should we make of this argument? Almost certainly some of the post-ULA decline is explained by the pre-ULA increase. But research on transfer taxes generally suggests that when transactions bunch right before the tax begins, the size of that pre-tax bunch tends to be smaller than the volume of "missing" transactions on the other side of the notch. The transfer tax, in other words, doesn't just move sales around (either forward in time or to one side of the notch), but can actually reduce the overall size of the market and potentially destroy more value than it creates.[46] This can occur through different channels. The largest factor, discussed above, is that some property owners who were unsure about selling might choose not to. Other owners might engage in more elaborate forms of tax avoidance. Owners could turn their property into an Limited Liability Corporation (LLC), and sell shares in the LLC to multiple buyers rather than sell the property itself. Or they could convert the property into a Tenancy-in-Common and sell the separate tenancies for under $5 million each.[47] Such avoidance behavior is cumbersome and arguably antisocial, but ULA's high non-marginal rates can make it worthwhile.

The fact that market unraveling is common in the literature doesn't automatically mean it occurred in the case of Measure ULA. Whether it did, moreover, is impossible to discern from graphs alone. We turn to regression analysis to explore that question further.

45 See Dreier et al, 2024 for a fuller version of this argument.

46 See Wojciech Kopczuk and David Munroe. 2015. "Mansion Tax: The Effect of Transfer Taxes on the Residential Real Estate Market." *American Economic Journal: Economic Policy*, 7 (2): 214–57.

47 See e.g., Geoffrey Gold. 2023. Nine Ideas to Avoid the Effect of Measure ULA. *The Real Dirt Blog*. April 13. Also Jack Flemming. 2022. LA's Rich are Scheming Ways to Avoid New Mansion Tax. *Los Angeles Times*. December 15. Note that some of these workarounds may not hold up in court, if challenged. Case law in California, for example, including *926 North Ardmore Avenue, LLC v. County of Los Angeles* (2017) has confirmed that in some cases, property ownership that gets transferred via the transfer of an LLC can still be considered a "sale" for the purposes of collecting transfer taxes.

Regression results

We use two sets of regression analyses to examine Measure ULA's impact. We examine both the *quantity* of transactions and the *price*. The first set of regressions analyzes the total number of real estate transactions at all price points. The second set examines the odds that a given transaction took place at a price above the ULA threshold.These two approaches represent different ways of capturing ULA's potential impact. Analyzing quantities lets us directly test the idea that ULA is preventing some sales from occurring; we should see the quantity of sales in L.A. decline after its introduction, even after controlling for other factors. Analyzing prices, meanwhile, offers a direct test of tax avoidance (e.g, sellers strategically pricing properties just below the threshold), and an *in*direct test of the quantity effect (if the prevalence of sales above the threshold declines sharply, that may be because such sales are not occurring).

All of our equations are difference-in-difference (DiD) regressions — a form of statistical analysis that seeks to isolate the causal effect of an intervention by comparing the before-and-after change in treated units to the before-and-after change in similar, untreated units (the "control group"). The disparity between those two changes (the "difference in differences") can, under some assumptions, be interpreted as the causal effect of the treatment.

The assumptions that make a DiD analysis valid are fourfold: that trends in treated and untreated cities are largely parallel beforehand; that the market does not anticipate the treatment before it occurs; that no other relevant events uniquely affect the the treatment and control groups during this time; and that no spillovers exist between the treatment and control. Figures 1–4 show common trends in real estate transaction activity prior to ULA, meaning the first assumption is satisfied.[48] The second assumption, that the market did not anticipate the tax, is satisfied by the existence of the temporal notch — the rush to sell immediately before ULA. That might seem counterintuitive, but had the market "anticipated" Measure ULA, it would have smoothly folded the expense of the tax into normal business, such that its beginning caused no detectable changes, such as the notch we observe.[49]

The third assumption, that no other events will influence sales, is violated because (as mentioned earlier) two other Los Angeles County cities, Culver City and Santa Monica, introduced real estate transfer taxes during this time. We address this violation in some regressions by dropping those cities, and in others by specifically controlling for their presence.

The fourth assumption, that the effect of the treatment will not spill over onto the control, is more complicated. ULA only applies to higher-priced transactions in Los Angeles, but real estate markets are not bound by municipal borders, and a change in one segment of the market could affect other segments.

We can think of three potential spillovers from Measure ULA. The first involves the temporal notch discussed above. If L.A. owners rush to sell right before ULA goes into effect, the difference in transactions pre- and post-tax may be artificially large in Los Angeles. The second involves tax avoidance. If L.A. owners respond to

48 While trends inside and outside L.A. were largely similar, there are times, particularly for commercial and industrial space, where higher-end sales in Los Angeles exhibit volatility and their trend intersects with other lines. We correct for this in some models by adding controls for each individual month.

49 See Dachis et al (2012) for more.

ULA by subdividing properties into Tenancies in Common, and then sell those TICs at prices below the ULA threshold, the effect of the tax could appear artificially small. A regression examining the total number of transactions would find that the number of sales had *grown* (multiple properties sold rather than one) but the growth would represent efforts to avoid ULA. The third potential spillover could arise if some property owners in L.A. choose not to place their parcels on the market. If this happens, people in other parts of the county who would have bought those parcels may not sell their own. In other words, when some people choose not to sell, other people lose trading partners, and the entire market shrinks. To the extent this occurs, it reduces the number of transactions in the control area, and makes the effect of ULA seem smaller than it actually is.[50]

We are not overly concerned about any of these potential spillovers. In some of our equations we specifically control for the effect of the notch, by dropping all observations from the three months before and two months after ULA starts.[51] We cannot control for the other potential spillovers, but we don't anticipate them being large, and more importantly they would create a bias *against* the regressions finding significant results. To the extent these biases pose a risk, in other words, they would wrongly suggest that ULA was *not* influencing sales.

Analysis of Total Transactions

We now turn to our first set of regressions, where we analyze the total number of transactions. Table 5 shows our first set of models. To estimate these equations, we divide the number of sales occurring in each quarter into two bins. Sales that occur above the ULA price threshold, and that are in the City of Los Angeles, are categorized as "Treated," and coded 1 in the binary variable by that name. All other sales that quarter are coded 0. Similarly, quarters that occur after Measure ULA's implementation are coded 1 in the binary variable "Post". The third coefficient, "Post * Treated", is an interaction term, and it is our coefficient of interest — it is the difference in difference. The variable captures any unique association that occurs if the geography is the City of Los Angeles, the price is above the ULA threshold, and the time period is after ULA, compared to transactions where not all of those factors hold.

50 Eeriola et al. 2021. This effect is likely larger for single family homes, but may manifest in transactions of other property as well, via 1031 Exchanges and other instruments that rely on one real estate acquisition to help buy another. One might wonder if such a spillover could run in the other direction: for instance, could a person moving to the L.A. region be deterred from buying in the City of L.A. and buy in Burbank instead, thereby artificially increasing the count of transactions outside the City? Were this to occur, it would not be because the buyer was trying to avoid the tax at the time of purchase–ULA falls, statutorily and economically, on the seller. A forward-looking buyer might purchase in another city in anticipation of eventually selling, but this difference would be a function of tax avoidance, and thus not = a spillover that would compromise the DiD.

51 It's worth noting that while the presence of a large temporal notch can help explain a subsequent decline in property taxes, the decline remains an effect of the tax, and a sign that the tax is poorly designed. People designing taxes needn't approve of the human tendency toward self-interested behavior, but they should anticipate it. Marginal rates can reduce the strategic actions when taxes begin.

Table 5. Associations With Total Sales by Quarter, Negative Binomial Regressions

	All Sales	SFR	Comm/Ind	MFR
Treatment	0.01257*** (-13.00)	0.00888*** (-16.98)	0.0851*** (-12.07)	0.0314*** (-30.12)
Post	0.868051701 (-0.3328)	0.8513* (-0.474)	0.86038078 (-0.604)	0.863769163 (-1.123)
Post*Treatment	0.61206107 (-0.8232)	0.642300517 (-0.917)	0.4889* (-1.996)	0.3780*** (-4.698)
N	**42**	**42**	**42**	**40**

*Coefficents are Incident rate Ratios. Z scores in parentheses. * p<0.05, ** p<0.01, *** p<0.001.*
SFR = Single Family Residential. Comm/Ind =Commercial/Industrial. MFR= multifamily residential.

We present four models: one that analyzes the total quantity of sales by quarter, and three subsequent models that separately examine single family, commercial and industrial, and multifamily transactions. We present the coefficients as incident rate ratios (IRRs); subtracting an IRR from one yields the percentage change in transactions.

The first two models are quite similar, which is unsurprising given that single-family transactions are the vast majority of all transactions. In both cases, the Post*Los Angeles coefficient suggests a decline of 32-36% (1 - 0.64 = 0.36 or 36%), but neither of these coefficients is statistically significant. For reasons we discussed above, this result is largely expected. Recall that across the entire 2020-2024 time period, Los Angeles sales over the ULA price threshold were less than 4% of the sample. The vast majority of transactions, both inside and outside the City of L.A., are for detached single-family homes, and only a tiny share of single-family homes trades for over $5 million. In this context, even a real impact on total transactions would likely be difficult to detect, so long as the size of that impact is small compared to the overall variability in transaction volume. In technical terms, we would say that these models are underpowered, because the exposure to the treatment is so limited.[52]

The final two models, which examine transactions other than single-family homes, are less likely to suffer from being underpowered. These transactions are particularly likely to be affected by Measure ULA. Countywide, these transactions are only 16% of total sales, but they are almost 50% of sales whose price exceeds the ULA threshold. Similarly, fewer than 2% of single-family home sales transact for over the ULA threshold, compared to over 14% of non-single family sales. In sum, because a much larger share of these transactions occur at or near the ULA threshold, a regression that focuses on them is likely to have sufficient power to detect ULA's effect. About two-thirds of these non-single family transactions are multifamily residential, another 23% are commercial or industrial, and the remaining are a mixture of institutional, religious, agricultural or government uses.

52 As an analogy, imagine a drug trial featuring 100 people, where only 4 participants were given the drug. In such a trial even a drug that worked well would likely have undetectable effects.

These models do in fact show a large, statistically significant association between transaction volume and Measure ULA. Specifically, they suggest that ULA's introduction is associated with a 52% reduction in commercial and industrial transactions, and a 62% reduction in multifamily transactions.

The advantage of the models in Table 5 is their relative simplicity. We compare Los Angeles to other places, and see a strong and intuitive association between ULA's introduction and the volume of transactions. This association gives us some confidence that a relationship is really there. At the same time, the models' relative simplicity also comes with limitations. With only 42 observations, there is a risk that these regressions may overestimate the magnitude of any regression they detect (that the coefficients might be too big).[53] A skeptic might also argue that the temporal and especially the geographic units in these regressions are too large. By treating all areas outside the City of L.A. as essentially the same, we neglect the possibility of submarket variation — that the real estate markets in particular cities or neighborhoods may have changed in different ways after ULA's introduction, and in ways that would affect the volume of transactions. It may not be accurate, as an example, to implicitly assume that a neighborhood in Malibu and a neighborhood in Palmdale will maintain the same post-ULA real estate markets they had prior to ULA. Finally, the structure of these models makes it hard to distinguish between transactions that are lost and transactions that are simply redistributed in time or price. Because transactions in Los Angeles are divided by price (i.e., only coded "Treated" if they are above the threshold) the interaction term could grow if sellers nudged their prices below $5 million, or rushed to sell right before ULA began.

We address these potential limitations by running a series of more conservative, restrictive models. These are our preferred estimates. As a first step we reshape the data by place and time period. Our primary method of doing so is to measure transactions by ZIP code and month, although we also run some models using cities rather than ZIP codes. Reshaping the data gives us our dependent variable: the number of transactions per month in a given geography.

Table 6 shows results from four regression models. To account for the fact that real estate transactions are count data, and that the number of transactions per month has a high variance (with many months being zero in some ZIP codes and others having hundreds of transactions) we estimate a model called a Poisson Pseudo-Maximum Likelihood (PPML, or Quasi-Poisson) regression.[54] The structure of these equations is similar to Table 5, but with a few differences. The first two coefficients shown are again binary variables, and "Post-ULA" is again coded 1 if the month is after Measure ULA's implementation. "Los Angeles", however, is coded 1 if the ZIP code is in the City of L.A. and 0 otherwise, and does not include any price information. As such, in these models we consider the City of L.A. to be the treated unit; the advantage of doing so is that if some sellers nudge prices just below the tax threshold, this model will not erroneously count that as a lost transaction.[55] The third

53 Andrew Gelman and John Carlin. 2014. Beyond Power Calculations: Assessing Type S (Sign) and Type M (Magnitude) Errors. *Perspectives on Psychological Science*, 9(6), 641-651.

54 See Santos Silva, J.M.C. and Tenreyro, Silvana (2006), The Log of Gravity, *The Review of Economics and Statistics*, 88(4):641-658.

55 A potential problem with this approach is that some ZIP codes cross municipal boundaries. We assign these ZIP codes to L.A. if their modal transaction, across all the months in the sample, is in Los Angeles. As a result, there are some ZIP codes with Los Angeles sales that are not counted as Los Angeles sales, and vice-versa. The latter case does not worry us because it creates a nullifying bias: some sales that occur outside L.A. are counted as sales inside it. The former case, however, could artificially depress sales in L.A. In some specifications (not shown) we add an additional variable that controls for the share of Los Angeles transactions in each ZIP code, as a check on our results. The results do not change, because the amount of boundary crossing is rather low: the median share of

coefficient, "Post * Los Angeles", is again the interaction term, and again our coefficient of interest. It captures a unique association between transaction volumes and a situation where the zip code is in Los Angeles after Measure ULA, as opposed to situations where only one or neither of those conditions is met (e.g., a ZIP code is in Glendale after ULA, in Los Angeles before ULA, or in Glendale before ULA).

The results in Table 6, on balance, replicate those in the first two models of Table 5: we see no consistent association between ULA and total transactions. Because these regressions are PPMLs, the coefficients are so-called semi-elasticities, and can be interpreted as percent changes. In Model 1, the Post*ULA interaction term is positive and statistically significant, and it implies that total transactions in Los Angeles rose 5% as a result of Measure ULA. This result is implausible, both because it contradicts the simpler model above, and because taxes almost never increase the taxed activity. The most likely explanation is that Model 1 does not account for the fact that ZIP codes differ along many dimensions relevant for real estate activity, and that the real estate market, especially for larger transactions, can be volatile, and fluctuate from one month to the next. To account for these factors, all the subsequent models use fixed effects to control for each ZIP code and each year-month combination. Model 3, in addition, controls for location in each specific city in the county.

Once we apply these fixed effects that control for factors unique to months and neighborhoods, the interaction coefficient becomes negative, but also smaller and statistically insignificant. (Because the fixed effects are just more precise measures of location and time period, using them drops the coefficients on "Post" and "Los Angeles" but does not affect the interaction term).[56]

We experimented with a variety of other approaches to this regression, and the results were consistent: in almost every model the interaction term is negative, but in most cases not statistically significant.[57] This result is consistent with what we saw in Table 5. The evidence, in sum, suggests that while ULA may have had a negative impact on total sales, regardless of price, that impact is hard to statistically detect.

transactions that take place in L.A. in non-LA ZIP codes is 0, and the mean is 0.33.

56 To elaborate a bit: the "Los Angeles" variable is constructed so that a ZIP code is either in the city or not. Controlling for ZIP codes thus removes all variance associated with "Los Angeles" (that variance is instead measured by the ZIP code controls). The regression will therefore not estimate a separate Los Angeles coefficient, and instead absorb it into the ZIP code fixed effect. Similarly, every year-month combination is either before or after ULA, so the time period fixed effects will supersede and preclude a separate "Post" coefficient. All fixed effects are absorbed rather than assigned.

57 Estimating linear (OLS) or Poisson regressions yields negative but statistically insignificant coefficients. If we switch geographies and analyze the total number of sales per city, rather than per ZIP code, Poisson and Quasi-Poisson regressions return a small negative coefficient that is not statistically significant. If we estimate these city regressions as OLS, the coefficient is negative and highly statistically significant, but implausibly large — suggesting that ULA resulted in hundreds of lost transactions per month. We do not consider this credible, and OLS is not the correct functional form for these regressions. Finally, if we use larger aggregates, by analyzing quarters and dividing the county simply into Los Angeles and otherwise, we find similar results.

Table 6. Associations With Total Sales by Month and ZIP Code, PPML Regressions

	(1)	(2)	(3)	(4)
Post ULA	-0.286*** (-20.18)			
Los Angeles	0.0608 (0.78)			
Post ULA * Los Angeles	0.0474* (2.36)	-0.0226 (-1.14)	-0.0179 (-0.96)	-0.0196 (-0.93)
Zip Code Fixed Effects	No	Yes	Yes	Yes
Year and Month Fixed Effects	No	Yes	Yes	Yes
City Fixed Effects	No	No	Yes	No
Notch Omitted	No	No	No	Yes
Constant	3.003*** (49.37)	3.212*** (1502.07)	3.065*** (1412.85)	3.233*** (1394.41)
N	**17212**	**16783**	**14557**	**15387**

*Standard errors clustered at zip code level. Santa Monica and Culver City zip codes omitted, although results change little if they are included. In models 2-4 fixed effects completely absorb coefficients on Post and Los Angeles. "Notch Omitted" means observations from the quarter prior quarter prior to ULA and first two months of ULA are dropped. t statistics in parentheses, * p<0.05, **p<0.01, *** p<0.001*

In Table 7 we use the PPML regressions to analyze non-single family transactions specifically (we roll all these transactions — commercial, industrial, multifamily, etc. — into a single category). In Model 1 the interaction term Post*Los Angeles is negative, and is statistically significant at the 10% level — suggestive, but short of conventional levels in social science. Once we apply ZIP code and time fixed effects, however, the interaction term grows and becomes statistically significant, and suggests that ULA was associated with a 15% reduction in non-single family sales. If we add in a control for each individual city, this estimate falls to 9%, but remains statistically significant.

This result, however, could still be driven by the spike in expensive sales immediately prior to ULA, and the trough immediately after it. To address this possibility, the last two models (Models 4 and 5) omit that temporal notch: we drop all transactions that occurred in the three months before ULA, and the first two months after it. The resulting coefficients are smaller, but not much so: the smaller one implies that Measure ULA was associated with a 7% reduction in non-single family transactions, the larger one a 13% decline.[58]

58 We experimented with different definitions of the notch (e.g., only removing the month prior to ULA) and the results don't change, suggesting that almost all of the strategic behavior occurred in March 2023.

Table 7. Associations With Non-Single Family Sales by Month and ZIP Code, PPML Regressions

	(1)	(2)	(3)	(4)	(5)
Post ULA	-0.169***				
	(-3.98)				
Los Angeles	-0.167				
	(-1.36)				
Post ULA * Los Angeles	-0.0768	-0.147**	-0.0924**	-0.131*	-0.0732*
	(-1.52)	(-2.92)	(-2.94)	(-2.54)	(-2.25)
Zip Code Fixed Effects	No	Yes	Yes	Yes	Yes
Year and Month Fixed Effects	No	Yes	Yes	Yes	Yes
City Fixed Effects	No	No	Yes	No	Yes
Notch Omitted	No	No	No	Yes	Yes
Constant	1.235***	1.630***	1.282***	1.649***	1.296***
	(12.47)	(360.66)	(394.43)	(344.74)	(372.79)
N	**17212**	**16543**	**14799**	**15112**	**13445**

Standard errors clustered at zip code level. Santa Monica and Culver City zip codes omitted, although results change little if they are included. In models 2-5 fixed effects completely absorb coefficients on Post and Los Angeles. "Notch Omitted" means observations from the quarter prior quarter prior to ULA and first two months of ULA are dropped.
*t statistics in parentheses, * p<0.05, **p<0.01, *** p<0.001,*

Regression analyses can sometimes err by detecting false positives — mistaking "noise" for "signal." Particularly when there are many observations, a difference-in-difference interaction term can return a statistically significant result by chance alone. Were this to occur, we would mistakenly conclude that ULA is having impacts it is not.[59] To examine this possibility, in Table 8 we carry out placebo tests: we estimate regressions with a fictitious Measure ULA that began one year earlier. Nothing of consequence actually happened on April 1, 2022, so if an interaction term based on this placebo ULA returns a statistically significant coefficient, that is a sign that our modelling approach may be flawed. The table shows, however, that the placebos are consistently insignificant, with and without fixed effects, and varying the end date of the sample as well. This gives us some confidence that the results above are robust.[60]

How should we interpret these results? A 7-15% monthly reduction translates, via compounding, into a 30-50% reduction in transaction volume over the time period for which we have data.[61] The regressions suggest, in

59 See for example Marianne Bertrand, Esther Duflo and Sendhil Mullainathan. 2004. How Much Should We Trust Difference in Differences Estimates? *Quarterly Journal of Economics*. February: 249-274.

60 We also re-estimated these regressions as linear (OLS) models. As mentioned in note 56, OLS models are not ideal for this type of data because the sample has many zero values and because the errors are heteroskedastic, but estimating the OLS equation can be useful as a check. The results are similar: the Post*Los Angeles coefficient is always negative, and statistically significant in the presence of year and ZIP code fixed effects.

61 There are different ways to convert a monthly decline into a cumulative decline, which rely on assumptions about how constant

short, that as a result of Measure ULA, non-single family transactions between April 2023 and December 2024 were between 50% and 70% of what they would have been in the tax's absence. Because such transactions are a large proportion of higher-end sales, we should expect to see the probability of such sales fall when ULA is introduced. We turn to that question next.

Table 8. Placebo Tests of Non-Single Family Transactions, PPML Regressions

	(1)	(2)	(3)	(4)
Placebo Post-ULA	-0.151*** (-6.42)		-0.191*** (-5.67)	
Los Angeles	-0.166 (-1.35)		-0.166 (-1.35)	
Placebo * Los Angeles	0.0376 (0.78)	0.0398 (0.83)	-0.0243 (-0.51)	-0.0671 (-1.40)
Zip Code Fixed Effects	No	Yes	No	Yes
Year and Month Fixed Effects	No	Yes	No	Yes
End Date	Mar-23	Mar-23	Dec-24	Dec-24
Constant	1.268*** (12.99)	1.633*** (417.30)	1.268*** (12.99)	1.615*** (227.26)
N	**11232**	**10947**	**17568**	**16904**

*t statistics in parentheses, * p<0.05, **p<0.01, *** p<0.001. Standard errors clustered at ZIP code.*
Results unchanged with or without Santa Monica and Culver City. In all models Placebo ULA begins April 1, 2022.

Odds of a transaction exceeding the ULA threshold

Our second set of regressions focuses on price rather than quantity. Table 9 shows the results of three logistic regressions. Each regression predicts the odds that a property will transact for a price above the ULA threshold. The difference-in-difference setup remains the same. The first two coefficients represent the time period and the location, and the coefficient of interest is the interaction variable. In these regressions, however, the coefficients are odds ratios. Subtracting the interaction term from 1 yields the change in the odds of a sale in Los Angeles after ULA being $5 million or more. A larger coefficient, therefore, implies a smaller negative effect associated with Measure ULA.

the rate of decline is. We use the standard formula for exponential decay, which is 1−(eβT, where βs the monthly loss, T is the number of months, and e is Euler's number. Not all economic phenomena will decay exponentially, so this formula only yields an estimate.

Table 9. Associations With Sales Above ULA Thresholds (Odds Ratios)

Los Angeles	1.821* (2.22)	0.949 (-0.10)	0.876 (-0.25)
Post-ULA	0.976 (-0.49)	0.549*** (-7.13)	0.598*** (-8.83)
Post-ULA * Los Angeles	0.523*** (-11.13)	0.496*** (-12.66)	0.556*** (-10.88)
Transfer Tax Control	No	Yes	Yes
Year Trend	No	Yes	Yes
Zip Code Fixed Effects	No	Yes	Yes
Notch Omitted	No	No	Yes
N	**328929**	**322767**	**301978**

Exponentiated coefficients; t-statistics in parentheses. All regressions estimated with standard errors clustered at city level. (L.A. observations clustered by neighborhood). p<0.05, ** p<0.01, *** p<0.001.*
"Transfer tax control" is a dummy variable for Santa Monica or Culver City
"Notch Omitted" means observations from the quarter prior to ULA, and first two months after ULA, are dropped.
Sample is transactions over $100,000, 2020-2024.

In Table 9's first regression, the relevant coefficient is 0.52, implying that in Los Angeles, relative to the rest of the county, the odds of a transaction being $5 million or more were about 48% lower after Measure ULA. As with the initial models in Tables 6 and 7, however, one could criticize this model for being overly parsimonious. Model 2 thus adds controls for the ZIP code of each transaction and a variable measuring a linear year trend in sales.[62] The coefficient of interest changes slightly, and suggests that Measure ULA was associated with a 51% reduction in the odds that a Los Angeles property would transact for $5 million or more.

In Model 3 we drop the months immediately before and after Measure ULA's introduction. This regression implies that, even controlling for the temporal notch, ULA is associated with a 44% reduction in the odds of a Los Angeles property transaction occurring at $5 million or more.

Table 10 presents results from eight more versions of this regression. These models differ primarily by including a variety of controls about the parcel being sold: its size, its zoned land use (commercial, residential, etc), and whether it was tax delinquent at the time of sale. In some regressions we restrict the sample to residential properties and control for the number of bedrooms. Two of these regressions, moreover, again omit the temporal notch. The coefficients fluctuate, in part because including some of these additional controls forces us to drop some observations, but the overall result is consistent, and suggests that Measure ULA is associated with a 43-53% reduction in the odds that a sale in Los Angeles will transact at $5 million or more.

62 We use a trend variable because the model will not converge with year fixed effects; we address year fixed effects in robustness checks.

Table 10. Associations With Sales Above ULA Thresholds (Odds Ratios)

Los Angeles	2.300**	2.297	1.084	0.875	2.129*	2.139	0.991	0.824
	(2.75)	(1.86)	(0.11)	(-0.15)	(2.48)	(1.69)	(-0.01)	(-0.22)
Post-ULA	1.010	1.195	0.549***	0.600***	1.014	1.195	0.608***	0.635***
	(0.19)	(1.93)	(-7.06)	(-4.95)	(0.28)	(1.96)	(-8.52)	(-5.37)
Post-ULA * Los Angeles	0.511***	0.522***	0.479***	0.465***	0.573***	0.580***	0.535***	0.506***
	(-9.21)	(-7.20)	(-10.44)	(-7.78)	(-7.87)	(-6.03)	(-9.16)	(-7.01)
Transfer Tax Control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Site and Building Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bedrooms Control	No	Yes	No	Yes	No	Yes	No	Yes
Year Trend	No	No	Yes	Yes	No	No	Yes	Yes
Zip Code Fixed Effects	No	No	Yes	Yes	No	No	Yes	Yes
Notch Omitted	No	No	No	No	Yes	Yes	Yes	Yes
N	**306812**	**291414**	**300306**	**210889**	**288433**	**274180**	**281667**	**194702**

*Exponentiated coefficients; t-statistics in parentheses. Standard errors clustered at city level (neighborhood level within L.A. city). * p<0.05, ** p<0.01, *** p<0.001. Site and building controls include land use type, size of lot, and tax delinquency status. Models that control for number of bedrooms only include residential parcels. "Notch Omitted" means observations from the quarter prior to ULA and first two months of ULA are dropped. "Transfer tax control" is a dummy variable for Santa Monica or Culver City. Sample is sales of $100,000 or more, 2020-2024.*

Table 11 shows that these regressions also pass placebo tests. In the Appendix, furthermore, we present regressions that restrict the sample to sales of $1 million or more. These models allow us to use year fixed effects rather than a year trend, and in this sample we also add in the 98 transactions that appeared in the city's ULA data but not our assessor data. The results remain largely unchanged.[63] As such, we can plausibly state that Measure ULA strongly reduced the odds of higher-priced transactions occurring in Los Angeles.

63 All of our regressions outside the placebo tests use clustered standard errors, to reduce the risk that serial autocorrelation will return false significance tests (See Bertrand et al 2004). We cluster at the city level, and within L.A. at the neighborhood level. Relative to using robust standard errors, clustering can cost some observations, because some clusters may either have only one observation (unlikely in our data) or may not have much variance (e.g., a city with very few sales above the threshold). If we do estimate these regressions with robust standard errors, our sample sizes are larger and the results are much the same (but the standard errors predictably shrink). Most of the placebo tests use regular standard errors, to maximize the chances of a false positive.

Table 11. Placebo Tests for Sales Above ULA Threshold

	(1)	(2)	(3)	(4)
Placebo Post-ULA	0.250***	0.250***	0.248***	0.297***
	(6.23)	(6.23)	(5.50)	(6.91)
Los Angeles	0.501***	0.501***	0.501***	-0.162*
	(16.50)	(16.50)	(16.50)	(-2.15)
Placebo * Los Angeles	0.0826	0.0826	0.0610	0.0483
	(1.42)	(1.42)	(0.93)	(0.78)
Zip Code Fixed Effects	No	No	No	Yes
End Date	Jan 1 2023	Jan 1 2023	Nov 1 2022	Jan 1 2023
N	**218803**	**218803**	**206016**	**213478**

*t statistics in parentheses, * p<0.05, **p<0.01, *** p<0.001.*

Odds are not probabilities, but they can be converted into probabilities. When we do this, we see that the regression results (using an average coefficient of about 0.5) suggest that Measure ULA reduced the incidence of higher-value sales by 49-55%.[64]

This loss of higher-end sales will have fiscal implication. If we assume that most of this reduction is the result of lost transactions, rather than transactions pushed just below the price threshold,[65] this result implies not just a sharp decline in sales activity but a sizeable loss in property tax revenue. We can use the county tax roll data we presented in Tables 2 and 3 to explain. Recall that properties in the City of Los Angeles almost double in Net Taxable Value (NTV) when reassessed. Also recall that from 2021 to 2023, sales over $5 million contributed disproportionately to the growth in NTV, contributing about $11.3 billion to the property tax base. If half of those transactions had never occurred, roughly $5.6 billion in property value would remain at its lower, pre-sale valuation. That lower property tax base translates to an estimated $25 million per year in forgone property tax growth — money that schools, city services, and other critical programs would have otherwise received. Slowed revenue growth, moreover, compounds. If ULA continues to reduce the probability of higher-end transactions occurring in subsequent years, within a decade its annual cost in lost property tax revenues could total in the low hundreds of millions.[66]

64 Converting a change in odds to a change in probability involves knowing the prior probability (in this case the share of transactions that were above the threshold before ULA) and then (optionally) accounting for any change in volume that occurred when the odds changed. These calculations yield the 49-55% decline.

65 This assumption is not unreasonable, given that we control for temporal bunching, and that our estimates of lost transactions also suggest substantial declines.

66 Calculating future losses of tax revenue always involves some assumptions, but generally the loss will be a function of the rate at which ULA reduces high end transactions and the growth in property value over time. If ULA reduced high-end transactions by the same dollar amount (say, $25 million) every year, by year ten the City of Los Angeles would be forfeiting some $250 million per year in property tax revenue. It's possible, of course, that the rate at which the tax deters transactions could itself decline (e.g., in year 2 transactions fall by 45-40%, not 50% as we find in the first years). This could reduce the amount of lost revenue if the average transaction size stays constant. But it may not. If real estate continues to appreciate over time, even a smaller rate of deterred transactions could result in a steady or even increasing amount of foregone revenue (because the average price of a ULA transaction is rising). We are not attempting a precise calculation of the losses: our point is simply that the losses *will* compound so long as ULA continues to deter sales.

V. Conclusion: Fixing ULA

Our analysis suggests that Measure ULA is depressing higher-priced real estate sales in the City of L.A. We find suggestive evidence that ULA has depressed the quantity of sales overall, strong evidence that it has reduced the quantity of non-single family sales, and very strong evidence that it has reduced the incidence of transactions priced above the ULA threshold.

Specifically, we show regressions suggesting that ULA reduced the sale of commercial, industrial and multifamily residential property in Los Angeles by 7-14% per month, and that it reduced the odds of any sale exceeding in Los Angeles exceeding its tax floor by 50%.

These results align with both economic theory and existing evidence about transfer taxes. Transfer taxes tend to reduce the volume of real estate sales. Measure ULA differs from other transfer taxes by falling on fewer transactions (which could decrease its effects) but also by taxing the transactions it does fall on at a higher rate (which could increase its effects). The results we see — of Measure ULA having little effect on the overall volume of sales, but notable effects on the subset of sales most likely to be taxed — is consistent with these expectations.

Could our results have an alternative explanation? Our difference-in-difference design controls away the effect of interest rates and other macroeconomic factors, and includes specific controls for the potentially unobserved influence of different cities, ZIP codes, parcels and time periods. We use placebo tests to check the possibility that our models are returning false positives. We explicitly control for the introduction of transfer taxes in Culver City and Santa Monica, and for the pre-ULA rush to sell in Los Angeles. And we do *not* control for the possibility that some L.A. owners subdivided and sold property to avoid Measure ULA, or that a reduced volume of transactions within L.A. would also reduce transactions outside it. If either of these possibilities occurred, our results would be biased against, not toward, the findings we have. This suggests that our findings are, if anything, conservative.

This doesn't mean we have addressed every imaginable objection. We are unable, for example, to test the idea that property owners in Los Angeles have simply been holding out until all political and legal challenges to ULA have been exhausted. Measure ULA was challenged in court, and lost. From there opponents mounted a ballot initiative against it, and that too was struck down in court. But these proceedings took time, and one might argue that property owners were waiting for all these efforts to run their course (or, less plausibly, that the broader real estate community conspired not to sell until these challenges ended). In this interpretation of events, legal and political challenges to ULA extended the one-time downturn associated with it. While not impossible, we do consider this scenario unlikely. The anti-ULA ballot measure was struck down in June 2024, so our data contain six months of transactions after the final challenge to ULA failed. In addition, many

realtors and attorneys, while the challenges to ULA were ongoing, were not suggesting that their clients hold properties off the market, but instead suggesting that they keep careful records, so they could collect a refund if ULA was overturned.[67]

Skeptics might also point to our analysis of total transactions (in, e.g, the first models Table 5 and in Table 6). The results here are only suggestive, with coefficients that are negative but not statistically significant. We recognize this limitation to our argument, but its most plausible explanation lies in the fact that ULA does not apply to the vast majority of transactions, which makes detecting its effect in a sample of all transactions difficult. One could also argue that single-family home sales (which are over 80% of total transactions) are less likely to be deterred by Measure ULA. We think this is probably true as a general rule, but likely *less* true for single-family homes of over $5 million, which often look more like investment properties and less like dwellings for everyday owner-occupancy. In any case, if we are wrong, and the lack of statistical significance in these regressions implies that ULA really has no effect, we would expect to find similar null results in our other regressions. But we do not. All other models imply substantial reductions.

In summary, we believe a fair reading of the evidence should give cause for concern. Measure ULA is plausibly reducing a small but disproportionately important segment of real estate transactions. These transactions — the trade in more expensive non-single family property — are pivotal for increasing our housing supply, and for repurposing many spaces that the new economy has rendered obsolete. These transactions also drive growth in the property tax base, which means they have strong fiscal implications for not just the City of L.A. but also the other governments (the county, the school district, and various special districts) that rely on revenue from property in the City of L.A. Not least, discouraging these transactions also reduces ULA's own revenue.

A final point is that Measure ULA, in targeting these transactions, departs from the spirit in which it was advertised. ULA was heavily promoted as a "Mansion Tax" and a tax on unearned gains, but our evidence suggests it is landing heavily on the trade in commercial, industrial and multifamily residential property. Put another way, Measure ULA was advertised as a tax on what might be called "consumer" real estate — high-value single-family residences — but we see that in practice it lands heavily on *investor* real estate, which is to say people trying to build, or to operate properties as businesses.

The solution to these problems is *not* to abolish or end Measure ULA. The spirit of Measure ULA is an admirable one, and Los Angeles undeniably needs revenue to help its less-advantaged residents. In a world where raising property taxes is politically difficult, transfer taxes can provide some of that revenue. The ideal outcome, then, is one that preserve's ULA's ability to raise revenue for affordable housing without jeopardizing new development or property tax growth.

We propose a number of ways to achieve that outcome, none mutually exclusive and some overlapping. We acknowledge, before outlining these ideas, that reforming ULA is legally and politically difficult. Measure ULA is a dense, 29-page law, and included in its language are provisions that restrict the Los Angeles City Council's

67 This suggestion is of course in their interest (realtors and attorneys would prefer to keep collecting fees) but the self-interest doesn't change the fact that this was the advice being given, and also suggests that a conspiracy is unlikely — people rarely conspire to make their own incomes streams smaller and less stable.

ability to amend it. That takes the most direct path to reform off the table, and leaves the courts, the state Legislature, or another direct vote of Angelenos. A direct vote is difficult and in many ways undesirable. ULA itself exemplifies the risks of passing complex legislation through the ballot box. Attempts to change or end ULA via the courts have already failed; while it may be unwise to have poorly designed taxes, it isn't illegal. That leaves the state.

The simplest reform would be to exempt from Measure ULA any property that isn't single-family residential. This would, in one fell swoop, turn Measure ULA into the tax many voters thought they were getting: a tax on mansions. As our analysis shows, most transactions subject to Measure ULA today are single-family residential, and there is some reason to think ULA would deter these transactions less than it would the sales of other types of property. To the extent Measure ULA does slow single-family transactions, moreover, the follow-on impacts of those lost transactions are arguably smaller. A large house failing to change hands will reduce tax revenue, but will not have adverse large implications for new multifamily housing or local economic development.

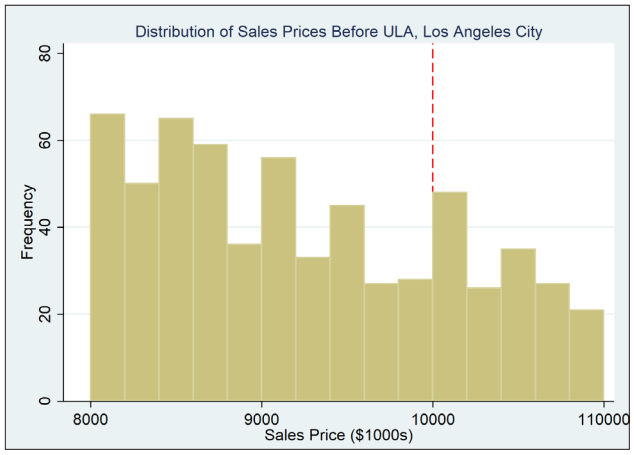
Another straightforward fix would involve exempting any transaction where the property has been reassessed relatively recently. With this reform, ULA would apply only to properties that had not been reassessed in the previous (for example) 20 years. In making this change, the city would effectively exempt developers of new housing (including for-profit affordable housing) as well as people who buy and upgrade commercial and industrial properties. Like the proposal above, this exemption would move ULA much closer to its stated vision (a tax on mansions and passive property appreciation), and would reduce its dedicated revenue only modestly. It also could, plausibly, even increase public revenues overall, by not deterring as many sales or improvements that trigger large reassessments.

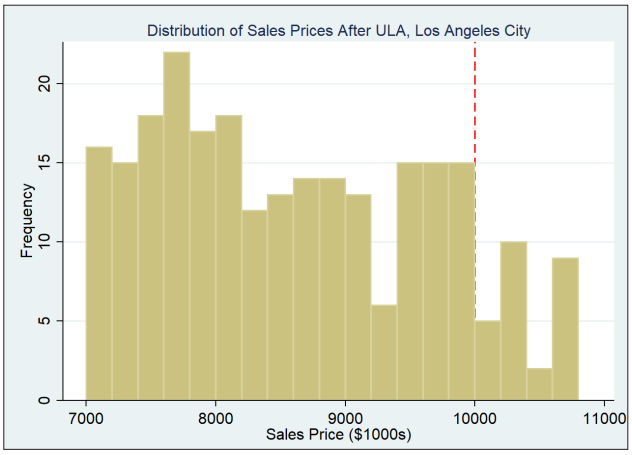
A third potential reform, which could be done on its own or paired with either of the reforms above, is to make Measure ULA's tax rates marginal. Currently these rates are constructed as notches, where a dollar of additional sales revenue can result in hundreds of thousands of dollars of additional tax burden. The notches create strong incentives for tax avoidance, and convert what could be revenue into behavior change. Smoothing at each notch by making rates marginal would cost some revenue per transaction. It might also increase the number of transactions, which would bring revenue up, but this reform, compared to the two reforms above, would be more likely to bring ULA's revenue down. It is also a more complicated reform. Rather than simply exempting some transactions, it changes the nature of the tax itself.

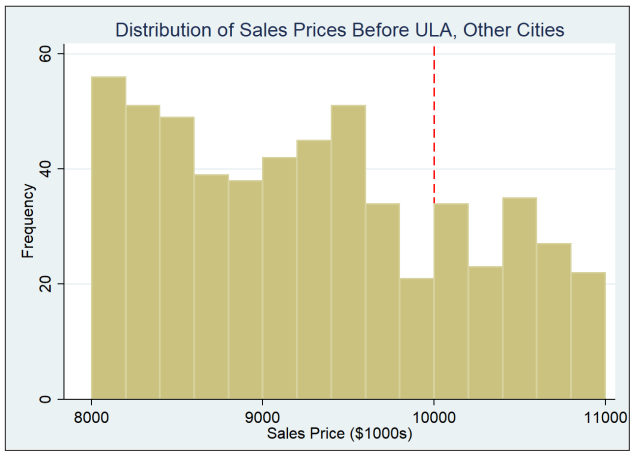
A fourth potential reform, which could be paired with marginal rates, is to add another bracket: lower Measure ULA's floor and tax those transactions at a lower rate (for example, taxing sales above $2 million at 1%, or 0.8%). A lower rate will be less likely to deter transactions (although the broader base would expand the reach of the tax) and the added revenue could compensate for any revenue lost as a result of the other reforms we suggest. A lower could also offer a more stable revenue stream, since sales above $5 million tend to be volatile.

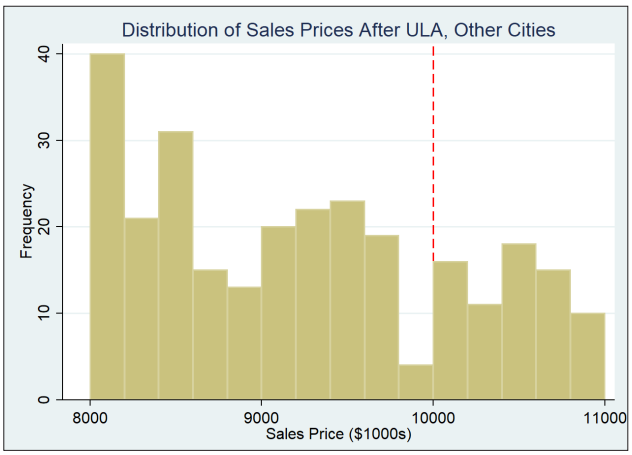
Finally — and this reform is perhaps least important, but also easiest — we see no reason not to extend ULA's current exemption to for-profit LIHTC developers. If the goal of the Measure ULA exemption is to encourage the production of deed-restricted affordable housing, it makes little sense to exclude a group that produces the preponderance of such housing in the city.

Appendix A: Notches at $10 Million in Los Angeles, After ULA









Appendix B: Logistic Regressions with Year Fixed Effects and Additional ULA Transactions

The models below, in Tables B1 and B2, re-estimate the logistic regressions in the main text, but do so with two added robustness checks: we add in the transactions that appeared in the city's records but not our records, and we use year fixed effects rather than a year trend variable. The magnitudes of the coefficients remain quite similar: relative to other cities in the county, after Measure ULA the odds of a higher-end sale (>$1 million) being $5 million or more in Los Angeles fall by 34-43%.

Table B1. Associations With Sales of $5 Million or More (Odds Ratios)

	(1)	(2)	(3)
Los Angeles	1.063*	0.816**	0.762***
	(2.43)	(-3.18)	(-4.04)
Post-ULA	5.889***	0.513***	1.109*
	(82.25)	(-10.26)	(2.11)
Post-ULA * Los Angeles	0.0990***	0.541***	0.603***
	(-51.79)	(-10.94)	(-8.72)
Year Fixed Effects	No	Yes	Yes
Zip Code Fixed Effects	No	Yes	Yes
Notch Omitted	No	No	Yes
Constant	0.0957***	0.0486***	0.0544***
	(-132.91)	(-5.88)	(-5.67)
N	**121455**	**111984**	**104968**

Exponentiated coefficients; t-statistics in parentheses. p<0.05, ** p<0.01, *** p<0.001*
"Notch Omitted" means observations from the quarter prior to ULA and first two months of ULA are dropped.
All regressions estimated with robust standard errors.

Table B2. Associations With Sales of $5 Million or More (Odds Ratios)

	(1)	(2)	(3)
Los Angeles	1.275***	1.192***	0.700***
	(6.73)	(4.62)	(-4.24)
Post-ULA	1.069	1.068	2.006***
	(1.47)	(1.40)	(9.56)
Post-ULA * Los Angeles	0.590***	0.655***	0.510***
	(-7.50)	(-5.87)	(-8.10)
Site and Building Controls	Yes	Yes	Yes
Year Fixed Effects	No	No	Yes
Zip Code Fixed Effects	No	No	Yes
Notch Omitted	No	Yes	Yes
N	**96464**	**90653**	**77614**

Exponentiated coefficients; t-statistics in parentheses. p<0.05, ** p<0.01, *** p<0.001*
Site and building controls include bedrooms, land use type, size of lot, and tax delinquency status.
"Notch Omitted" means observations from the quarter prior to ULA and first two months of ULA are dropped.
Sample is restricted to sales of $1 million or more. All regressions estimated with robust standard errors.

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