

The Shape of the Tax

Round Prices and Bunching at New York City's 2019 Transfer-Tax Notches

Pablo Loschi*

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Abstract

New York's 2019 budget added buyer-paid transfer-tax notches at \$2 million and above. Using 434,108 sales, I compare the same prices before and after the reform, against never-taxed round prices and the unchanged \$1 million notch. The share of sales at exactly the new thresholds fell by about 60 percent, for jumps from \$5,000 to \$37,500 alike, as those sales moved just below. Standard bunching estimators read the moves at \$2 million as price cuts of over three times the tax saved; a before period corrects them. Beyond the round price, responses are smaller and larger at larger jumps.

JEL: H23, H71, K34, R31, R38. *Keywords:* transfer taxes; notches; bunching; round numbers; optimization frictions; housing transactions.

*Independent researcher, Berlin, Germany. ORCID 0009-0004-9455-4713. Correspondence: loschi.pablo@gmail.com. All data used here are public and free; retrieval and analysis scripts are archived and listed in the Supplemental Appendix. This research received no external funding. The author founded HolaCasaClara, a free comparator of Argentine mortgage loans, which has no connection to this study. No other competing interests are reported.

1 Introduction

Housing transaction taxes are often notches: the rate applies to the whole price once a threshold is crossed, so one more dollar of price can carry thousands of dollars of tax. New York’s 2019 budget created seven such notches at once. A buyer-paid supplemental tax took effect on 1 July 2019 at prices from \$2,000,000 to \$25,000,000, while the \$1,000,000 mansion tax, in place since 1989, did not change. The same thresholds can therefore be observed before and after they were taxed, next to an always-taxed control and never-taxed round prices. This paper uses that reform to measure how recorded prices respond to a notch when the same price can also be seen untaxed, and to show what that comparison changes about how such responses are measured.

The notches moved recorded prices, mostly by a little (Figure 1). Before the reform, \$2,000,000, \$3,000,000 and \$5,000,000 were ordinary round prices, each with a spike of sales at exactly that amount. After it, the share of sales at exactly those prices fell by about 60%, and the missing sales turn up just below the threshold, on average \$2,000 to \$3,000 below it, less than the buyer’s tax saving. The fall is the same, within sampling error, for jumps of \$5,000, \$15,000 and \$37,500, and it holds against never-taxed prices as round as the thresholds and in 2022–2025 alone (Table 3). Beyond the round price the response is smaller and larger where the jump is larger: net of never-taxed round prices and the unchanged \$1,000,000 notch, sales in the 5% of prices above the threshold fall by 22% at \$3,000,000 and 28% at \$5,000,000, and by an amount indistinguishable from zero at \$2,000,000.

The main contribution is to measurement. Round-price confounding is an established concern: Kopczuk and Munroe (2015, p. 237, footnote 20) discuss it and show that their response survives coarser bins; they also use New Jersey’s tax introduction for a before–after comparison. Here the 2019 New York reform permits the exact-price margin to be measured at several newly taxed thresholds, and compared with the remaining response after those observations are removed. Bunching estimators fit a smooth distribution outside a window around the threshold, with indicators for round numbers, and read the excess of sales below the threshold and the shortfall above it as a price response (Kleven and Waseem 2013; Kopczuk and Munroe 2015; Best and Kleven 2018). At a round-number threshold the fit cannot say how large the spike at the threshold itself would have been, so sales that leave the round price by a few dollars are counted as a response spread over a range of prices. A before period at the same prices measures that spike directly, and the difference is large. At \$2,000,000 the standard estimate rises by 0.223, more than at any never-taxed placebo; without the sales at exactly the threshold and one dollar below, it rises by 0.041, within placebo noise. Read as a width, the pile below \$2,000,000 is \$16,900 of price, against an

average move of about \$2,400. Notches in transaction taxes are almost always set at round prices, so the check carries over: count the sales at the threshold and one dollar below it, and estimate the response with and without them, against a before period or against never-taxed prices as round as the threshold.

Whether a round-price sale moves depends little on the size of the jump. The share that moved is about 0.4 at the mortgage recording tax’s \$625 loan notch, about 0.6 for jumps from \$5,000 to \$37,500, and close to one at the \$1,000,000 notch, in place since 1989. It is lower when a company sells, as developers do at prices set in advance, and it did not rise between 2020 and 2025. If leaving the round price has a cost that varies across sales, 40% of round-price transactions faced a cost above \$37,500 in each of the first six years. Writing a different number on a contract costs far less. A price fixed before anyone checked the schedule fits better, and so does a schedule nobody checked; the data cannot tell the two apart.

The evidence comes from 434,108 recorded residential sales above \$300,000 in New York City from 2016 to 2025, one row per deed, with the merged rows near the thresholds checked against the deed registry. Counts at exactly each threshold and just below it need no estimator (Tables 1–4). A difference-in-differences in counts by price zone nets out the always-taxed threshold and 24 never-taxed round prices, year by year and against a permutation of those placebos (Table 3, Figure 2). The bunching estimator, applied before and after the reform at every threshold, places the response against placebo noise and across the whole schedule (Tables 5–7).

For tax design, the round-price margin means that even a quarter-point notch moves most of the deals that would have closed at the threshold, and that the deals that do not move pay the whole jump for one dollar of price. Whole-price notches are common: New Jersey’s mansion tax, Washington, D.C.’s transfer taxes and the tax Los Angeles introduced in 2023 charge a higher rate on the whole price above a threshold (Kopczuk and Munroe 2015; Slemrod et al. 2017; Green et al. 2026), and the United Kingdom moved its stamp duty from whole-price to slice rates in December 2014 (Best and Kleven 2018; Kleven 2016). In New York the revenue at stake is small: the sales that moved avoid \$1.6 to \$2.9 million a year, under 1% of the \$296 million a year that the reform’s schedule implies. Slice schedules that raise the same revenue without jumps are feasible but need higher marginal rates (Table 11).

1.1 Relation to the literature

The notch evidence sits with Kopczuk and Munroe (2015), who study the New York and New Jersey transfer-tax notch and find both price bunching below the threshold and a

shortfall of transactions above it that exceeds the bunching, so part of the market unravels instead of simply repricing. Best and Kleven (2018) document bunching at UK stamp-duty thresholds, Kleven and Waseem (2013) supply the notch estimator, Saez (2010) the kink design it extends, and Kleven (2016) a review of both. Best et al. (2020) use notches in UK mortgage rates at loan-to-value limits, and DeFusco and Paciorek (2017) estimate bunching of loan amounts at the conforming loan limit. Pope et al. (2015) show that negotiated house prices cluster at round numbers, which is the margin this paper finds the notches act on. Bunching identifies a response only under assumptions about the counterfactual distribution near the threshold (Blomquist et al. 2021; Bertanha et al. 2023); a before period at the same prices supplies that counterfactual directly. Work on tax salience finds that consumers respond less to taxes that are less visible (Chetty et al. 2009; DellaVigna 2009; Taubinsky and Rees-Jones 2018; Gabaix 2019), which is one reading of the round-price sales that do not move. Slemrod et al. (2017) find sale prices moved to the lower-rate side of a notched change in Washington, D.C.’s transfer taxes and read the sales left in the dominated range as a measure of how many transactions were aware of the notch, a reading Section 4.4 returns to. Dachis et al. (2012) find that Toronto’s 1.1% land transfer tax reduced sales of single-family homes by 15% and lowered prices by about the amount of the tax, and Besley et al. (2014) that about sixty percent of the UK’s 2008–09 stamp duty holiday went to buyers. Fritzsche and Vandrei (2019) find fewer single-family home transactions where German states set higher transfer taxes, and Eerola et al. (2021) study the mobility effects of a Finnish reform that raised the tax on co-operative housing but not on directly owned houses. Green et al. (2026) show that the transfer tax Los Angeles imposed in 2023 on sales above \$5,000,000 sharply reduced transactions and eroded property-tax revenue through deferred reassessment; that channel is weak in New York City, where a sale does not reset a home’s capped assessment (New York City Department of Finance 2018; Horton et al. 2024). Relative to these studies, the 2019 reform turns a fitted-counterfactual design into a before-and-after comparison at the same round prices, and its seven thresholds on one date allow the response to be compared across jump sizes with the tax’s age held fixed.

Section 2 sets out the tax schedules and Section 3 the data. Section 4 presents the evidence on recorded prices and what it says about frictions. Section 5 turns to the loan notch of the mortgage recording tax. Section 6 states what the paper cannot settle, and Section 7 concludes. The Supplemental Appendix holds the fixed-window estimates, the split by payment method, counts and volume over equal periods, the recording tax’s gross charge on observed loans, and the check of the one-row-per-deed rule against the deed registry.

2 The tax schedules

2.1 The transfer-tax thresholds

The mansion tax under Tax Law §1402-a imposes 1% on residential conveyances of \$1,000,000 or more. It is a notch, not a bracket: the tax applies to the whole consideration, so a sale at \$1,000,000 owes \$10,000 that a sale at \$999,999 does not. The threshold has been in place, unchanged, since 1989.

The supplemental tax under §1402-b, on New York City residential conveyances, has seven brackets: 0.25% for consideration of \$2,000,000 or more, 0.50% from \$3,000,000, 1.25% from \$5,000,000, 2.25% from \$10,000,000, 2.50% from \$15,000,000, 2.75% from \$20,000,000 and 2.90% from \$25,000,000. Each rate applies to the whole consideration, so every step is a notch, and the grantee pays it. It took effect for conveyances on or after 1 July 2019, with conveyances under binding written contracts entered into on or before 1 April 2019 excluded. The state base rate under §1402 rose in the same budget from 0.40% to 0.65% for residential conveyances of \$3,000,000 or more, so the \$3,000,000 threshold carries a half-point discontinuity: 0.25 points paid by the buyer and 0.25 points imposed on the seller. In sponsor (new-development) sales, contracts commonly shift the seller's transfer taxes to the buyer, so the statutory payer need not be the one who pays.

The city's own Real Property Transfer Tax steps from 1% to 1.425% on consideration above \$500,000. Because a sale at exactly \$500,000 already pays the lower rate, a price one dollar below avoids nothing there, and that threshold is not used in any inference (Supplemental Appendix Table A11).

The 2019 reform is the source of identification. It created seven discontinuities on one date, from \$2,000,000 to \$25,000,000, and left the \$1,000,000 and \$500,000 discontinuities untouched.

Throughout, the jump T at a threshold is the change in all transfer taxes, the buyer's and the seller's together, and the rate change is the change in the total rate. At \$2,000,000, \$3,000,000 and \$5,000,000 the total rate rises by 0.25, 0.50 and 0.75 points and T is \$5,000, \$15,000 and \$37,500; at \$3,000,000 half of it, \$7,500, falls on the buyer. Where an exhibit uses the buyer's part only, it says so.

2.2 The mortgage recording tax

New York taxes the recording of a mortgage on real property (New York State 2026). For a one- to three-family home or a condominium unit in New York City the aggregate rate is 2.05% on mortgages below \$500,000 and 2.175% at or above, on the whole loan. The special

additional tax of 0.25% is payable by the lender for such properties under §253(1-a), so the borrower’s rate is 1.80% below \$500,000 and 1.925% at or above: a notch of \$625 at the threshold. A purchase without a recorded mortgage pays none of it, and neither does a co-operative purchase, because a share loan is not a mortgage of real property. Supplemental Appendix Section I sets out the components and measures the gross charge on observed loans.

3 Data

Everything used here is public and free.

3.1 Transactions

The New York City Department of Finance publishes annualized calendar sales through NYC Open Data (resource `w2pb-icbu`), covering 1 January 2016 to 31 December 2025. The residential building classes are one-, two- and three-family dwellings, condominiums and co-operatives. With recorded consideration above \$300,000 they give 442,497 transactions, of which 431,471 lie between \$300,000 and \$6,000,000. One formatting detail matters: calendar year 2016 writes the building-class category with a double space after the two-digit code, so a filter on the full literal strings silently drops the whole of 2016, the earliest pre-reform year. The filter used here matches on the two-digit code.

A deed that conveys several lots, such as a building sold whole or a unit with its storage lot, appears in the sales file once per lot, each row carrying the deed’s whole consideration. Counting such rows separately would multiply one sale, and would do so mostly at high prices. The analysis therefore keeps one row per deed: rows that share a sale date, borough, tax block and price are treated as one deed, while rows on different blocks are kept apart, so that two separate sales at the same round price on the same day are not merged. That removes 8,389 of the 442,497 rows above \$300,000, leaving 434,108 sales; above \$25,000,000, nearly two-thirds of the rows repeat a deed. Supplemental Appendix Section J checks the removed rows near the thresholds against the recorded deeds. The supplemental tables on the fixed-window design and on equal-period counts use every row, as do the robustness checks labeled that way.

3.2 Loans

New York City’s Automated City Register Information System (ACRIS) records deeds and mortgages in Manhattan, the Bronx, Brooklyn and Queens; Staten Island instruments are

recorded by the Richmond County Clerk. A purchase is classified as financed when a mortgage on the same parcel is dated within -15 to $+90$ days of the sale. Condominium units are matched to their unit lot through each sale’s own deed, because the Department of Finance and ACRIS index condominiums under different lot numbers (Supplemental Appendix Section I). The result is a loan amount for 172,725 financed house and condominium purchases in the four boroughs: 125,023 houses and 47,702 condominiums.

4 Recorded prices at the 2019 notches

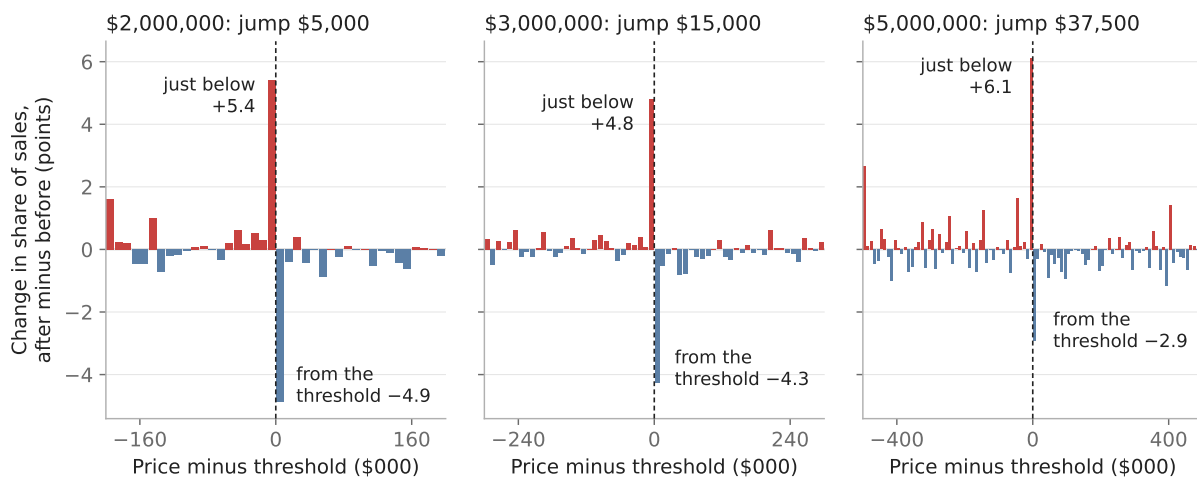


Figure 1: Recorded prices moved to just below the new thresholds. Each bar is the change in a \$10,000 price bin’s share of sales within 10% of the threshold, from January 2016–March 2019 to 2020–2025, one row per deed. The bin just below the threshold gains 5.4, 4.8 and 6.1 percentage points at \$2,000,000, \$3,000,000 and \$5,000,000; the bin starting at the threshold, which includes sales at exactly the round price, loses 4.9, 4.3 and 2.9. Prices are whole dollars, so the bin below the threshold includes sales at $X - 1$. Changes elsewhere, including at other round prices, are small. Replication package, `make_headline_figure.py`.

4.1 Design and measures

The 2019 budget created the notches effective 1 July 2019 and left the \$1,000,000 mansion tax, in place since 1989, untouched. Conveyances under contracts signed on or before 1 April 2019 were grandfathered, so April to December 2019 is dropped as a transition window. The pre-period runs January 2016 to March 2019 (142,352 sales) and the post-period January 2020 to December 2025 (258,647 sales). Each threshold X is compared with itself before the reform, with the always-taxed \$1,000,000 threshold, and with never-taxed round prices.

A price point whose estimation window contains no statutory threshold serves as a placebo. Four measures are used, from the most direct to the most model-based.

Counts by price zone. Let $s_{z,t}(X)$ be the share of sales in $[0.9X, 1.1X)$ that fall in price zone z in period t . The zones are $[0.95X, X - 5,000)$, $[X - 5,000, X)$, exactly X , and $(X, 1.05X)$, with the rest of the window as reference. The reform's change in zone z , in sales a year, is

$$\Delta_z(X) = [s_{z,\text{post}}(X) - s_{z,\text{pre}}(X)] \frac{N_{\text{post}}(X)}{6}, \quad (1)$$

where $N_{\text{post}}(X)$ counts post-reform sales in the window over six years, and the share of round-price sales that moved is $m(X) = 1 - s_{X,\text{post}}(X)/s_{X,\text{pre}}(X)$, with $z = X$ the zone of sales at exactly X .

A difference-in-differences in counts. Let n_{xtz} count sales in zone z around price point x in period t , for the three new thresholds, the \$1,000,000 control and the 24 never-taxed multiples of \$100,000 between \$1,200,000 and \$4,500,000 whose $\pm 10\%$ windows hold no threshold. The model is

$$\log E[n_{xtz}] = \alpha_{xt} + \gamma_{xz} + \delta_{zt} + \beta_z \mathbf{1}[x \text{ new}] \mathbf{1}[t = \text{post}], \quad (2)$$

estimated by Poisson regression, with the two outer zones $[0.9X, 0.95X)$ and $[1.05X, 1.1X)$ as reference. Here α_{xt} absorbs each price point's volume in each period, γ_{xz} its fixed shape, including its own spike at the round price, and δ_{zt} any change in shape common to all 28 price points. Then $\exp(\beta_z) - 1$ is the change in zone z at the new thresholds net of the same change at the control and the placebos. Intervals come from a block bootstrap over calendar months. The same model with year effects in place of the two periods traces the change year by year.

Bunching. The estimator follows Kleven and Waseem (2013) and Kopczuk and Munroe (2015). In bins j of width $0.005X$ (rounded down to \$10,000, \$25,000, \$50,000 or \$100,000) within $\pm 0.2X$, counts outside an excluded range $(X - 0.025X, X + 0.05X)$ are fitted by

$$c_j = \sum_{k=0}^5 \theta_k \left(\frac{p_j - X}{0.2X} \right)^k + \rho' r_j + u_j, \quad (3)$$

where p_j is the bin's midpoint and r_j marks round-number bins. The statistic is the share of counterfactual mass missing above the threshold, $b(X) = \sum_{j \in A} (\hat{c}_j - c_j) / \sum_{j \in A} \hat{c}_j$ over the bins A in $(X, 1.05X)$, and the reported change is $b_{\text{post}} - b_{\text{pre}}$. As implemented, r_j marks

the two \$10,000 bins on either side of each multiple of \$100,000; marking the bins that contain multiples of \$100,000, \$50,000 and \$25,000 instead changes the estimates by at most 0.017 (Section 4.6). Bootstrap intervals resample sales within the window in each period. A threshold with fewer than 1,000 pre-reform sales in its window is called thin.

A ratio with no counterfactual. $R(X) = N[X, 1.05X]/N[0.925X, 0.975X]$ compares sales just above the threshold with sales just below it, outside the range where bunching sits; a notch predicts a fall in $\log R$.

4.2 The round price

Round numbers attract transactions (Pope et al. 2015). A notch at a round number makes it the first taxed price, so a sale that would have closed there can avoid the jump by recording a slightly lower price. Table 1 counts this directly. Before the reform, \$2,000,000, \$3,000,000 and \$5,000,000 carried 7.8, 7.0 and 8.3 times the surrounding density of sales at exactly that price, comparable to the never-taxed \$2,500,000. After it the spikes fell to 2.3, 2.5 and 1.8, while those at \$1,500,000 and \$2,500,000 rose. Sales one dollar below went from about one a year to 21, 11 and 4 a year; at the never-taxed prices they stayed at one a year or fewer. At \$1,000,000, a notch since 1989, there is no spike at exactly the threshold in either period, and sales one dollar below are already common, 48 a year before the reform and 89 after.

Table 1: Round prices before and after the 2019 reform

Price point	Sales at exactly X , relative to local density		Sales at $X - 1$, a year	
	Before Apr 2019	From Jan 2020	Before	From Jan 2020
\$1,000,000 — taxed throughout	0.1	0.1	48.0	89.3
\$2,000,000 — new in 2019	7.8	2.3	1.2	21.2
\$3,000,000 — new in 2019	7.0	2.5	0.6	11.3
\$5,000,000 — new in 2019	8.3	1.8	0.0	4.3
\$1,500,000 — never taxed	4.9	5.8	0.9	0.7
\$2,500,000 — never taxed	6.7	10.0	0.3	0.0

Notes. One row per deed (Section 3). The periods are January 2016 to March 2019 and January 2020 to December 2025; the nine months in between are left out, because conveyances under contracts signed on or before 1 April 2019 were grandfathered. Local density is the number of sales between \$5,000 and \$25,000 from X on either side whose price is not a multiple of \$10,000, per \$10,000 of price; the \$5,000 on either side, where moved sales land or go missing, is left out. Sales at $X - 1$ are divided by the length of each period (3.25 and 6 years). The table uses counts only, with no fitted counterfactual. Replication package, `focal_table.py`.

Table 2 follows the moved sales by price zone, using equation (1). At \$2,000,000 the reform added about 89 sales a year in the \$5,000 below the threshold and 33 between \$1,900,000 and \$1,995,000. It removed 78 a year at exactly \$2,000,000 and 31 from the rest of the 5% above. About seven in ten of the sales missing above the threshold were at the round price itself; at \$3,000,000 and \$5,000,000 about half and a third were. The share of window sales recorded at exactly the threshold fell by 61% at \$2,000,000, 64% at \$3,000,000 and 61% at \$5,000,000, with 95% intervals of 55–67%, 55–72% and 42–73%; equality of the three is not rejected ($p = 0.86$), although the total jumps are \$5,000, \$15,000 and \$37,500. The never-taxed round prices’ own spikes grew by 6% on average, so against them the moved shares are slightly larger, 63% to 66%. The sales added just below sit on average \$2,400 below \$2,000,000, \$2,200 below \$3,000,000 and \$3,000 below \$5,000,000, less than the buyer’s saving at each threshold. The never-taxed round prices show none of this. The always-taxed \$1,000,000 threshold also gained sales just below it, 138 a year, partly (48 a year) from the 5% above; its tax did not change, and the next subsection nets that drift out.

Table 2: Where the moved sales came from

Threshold	Change in sales a year, by recorded price				Mean distance below X of added sales	Sales at X that moved
	$[0.95X, X - 5,000)$	$[X - 5,000, X)$	Exactly X	$(X, 1.05X)$		
\$2,000,000	+33 [+2, +58]	+89 [+79, +100]	−78 [−91, −64]	−31 [−50, −11]	\$2,446 [\$1,972, \$2,881]	61% [55, 67]
\$3,000,000	+19 [+1, +40]	+42 [+33, +48]	−37 [−46, −26]	−35 [−50, −19]	\$2,214 [\$1,598, \$2,738]	64% [55, 72]
\$5,000,000	+10 [−3, +22]	+23 [+19, +28]	−12 [−17, −7]	−23 [−33, −12]	\$2,985 [\$2,355, \$3,542]	61% [42, 73]
\$1,000,000 (control)	−29 [−79, +15]	+138 [+102, +171]	−3 [−10, +2]	−48 [−73, −21]	\$2,644 [\$1,330, \$3,686]	—
\$1,500,000 (never taxed)	+15 [−19, +48]	−8 [−20, +3]	+8 [−15, +27]	−8 [−37, +22]	—	—
\$2,500,000 (never taxed)	−17 [−38, +6]	−2 [−8, +4]	+7 [−4, +20]	−11 [−31, +8]	—	—

Notes. Each cell is the change in a price zone’s share of sales between $0.9X$ and $1.1X$, from January 2016–March 2019 to 2020–2025, multiplied by post-reform sales a year in that range: the number of sales a year the zone gained or lost. The mean distance is the mean of $X - P$ over the sales added to $[X - 10,000, X)$: post-reform sales there, less pre-reform sales rescaled to the post-reform window size. The share that moved is one minus the ratio of the post-reform to the pre-reform share of window sales recorded at exactly X ; equality across the three thresholds is not rejected ($p = 0.86$). Prices are whole dollars, so the zone just below X includes $X - 1$. 95% bootstrap intervals (500 draws; 1,000 for the share that moved), resampling sales within the window in each period, in brackets. Replication package, `focal_robustness.py`, `focal_inference.py`.

4.3 Netting out the control and never-taxed round prices

Table 3 estimates equation (2). Pooled over the three new thresholds, sales at exactly the threshold fall by 64% relative to the outer zones, net of the control and the placebos, and sales in the \$5,000 below it more than triple. Sales between $0.95X$ and $X - 5,000$ rise by 9%, and sales in the 5% above the threshold, excluding the round price, fall by 17% (95% interval 11% to 23%), or by 12% on every row of the sales file. The last line is the response beyond the round price. By threshold it is -10% at \$2,000,000, with an interval from -19% to $+2\%$, and -22% and -28% at \$3,000,000 and \$5,000,000, each interval excluding zero. The comparison group matters more for this line than for the round price. Against the control alone, the change at the round price is -54% and the change above it only -9% ; against the placebos alone, -64% and -18% . The control is a weak benchmark for the zone above the threshold, since it gained 138 sales a year just below its own threshold (Table 2), so most of the evidence beyond the round price comes from the never-taxed prices. Those mix prices of different roundness. Against the five that are multiples of \$500,000, as round as the thresholds, the changes are -65% at the round price and -14% above it, with an interval from -21% to -6% (Table 3, Panel B); treating each of the five in turn as if it had been taxed gives at most -13% at the round price and -4% above it. With the post-period restricted to 2022–2025 the changes are -64% and -18% , and in sales without a company seller -67% and -26% .

Two checks address inference. Treating each of the 24 placebos in turn as if it had been taxed, against the control and the other placebos, gives changes at exactly the round price between -11% and $+68\%$, none as large in magnitude as the -64% at the new thresholds; above the round price no placebo change is as large as those at \$3,000,000 and \$5,000,000, and one exceeds the \$2,000,000 change. And Figure 2 estimates the model year by year, relative to the 2016–2018 mean. Sales at the round price move by $+5\%$, -14% and $+10\%$ in the three pre-reform years, fall by 43% in 2019, as grandfathered contracts gave way to new ones, and stay between 60% and 69% below the pre-reform mean in every year from 2020. The response above the round price is -6% in 2020 and between -12% and -24% from 2021, so it built up after the round-price response, as it would if a smaller share of those transactions adjusted at first.

4.4 What the round-price margin says about frictions

Some sales that would have closed at exactly a threshold now close just below it, and others still close at it and pay the jump on the whole price. Which ones move says something about the frictions that keep sales where a notch makes them costly (Kleven and Waseem 2013).

Table 3: Bin-count difference-in-differences: the new thresholds against the control and never-taxed round prices

Price zone	Pooled, \$2M, \$3M, \$5M		By threshold		
	One row per deed	Every row	\$2,000,000	\$3,000,000	\$5,000,000
$[0.95X, X - 5,000)$	+9 [+2, +16]	+11 [+3, +20]	+10 [+1, +21]	+7 [-2, +19]	+9 [-7, +30]
$[X - 5,000, X)$	+257 [+197, +337]	+249 [+185, +323]	+248 [+175, +345]	+202 [+133, +303]	+625 [+340, +1922]
Exactly X	-64 [-68, -58]	-60 [-66, -54]	-63 [-68, -56]	-66 [-73, -58]	-63 [-75, -49]
$(X, 1.05X)$	-17 [-23, -11]	-12 [-22, -1]	-10 [-19, +2]	-22 [-32, -10]	-28 [-38, -15]
<i>Panel B. Other comparisons and samples</i>					
	Pooled		$(X, 1.05X)$, by threshold		
	Exactly X	$(X, 1.05X)$	\$2,000,000	\$3,000,000	\$5,000,000
\$1M control only	-54 [-71, -31]	-9 [-19, +3]	—	—	—
24 placebos only	-64 [-68, -58]	-18 [-24, -11]	—	—	—
Control and placebos as round as X^a	-65 [-69, -59]	-14 [-21, -6]	-7 [-17, +6]	-19 [-31, -8]	-25 [-35, -11]
Placebos as round as X only	-65 [-70, -60]	-16 [-22, -8]	—	—	—
Post-period 2022–2025 only	-64 [-69, -59]	-18 [-26, -9]	-10 [-19, +4]	-22 [-33, -7]	-30 [-41, -18]
No company seller	-67 [-74, -60]	-26 [-36, -14]	-28 [-39, -13]	-22 [-42, +5]	-30 [-55, +3]

Notes. Panel A: percentage change in the number of sales in each zone after the reform (2020–2025 against January 2016–March 2019), relative to the two outer zones $[0.9X, 0.95X)$ and $[1.05X, 1.1X)$ and net of the same change at the always-taxed \$1,000,000 threshold and at the 24 never-taxed multiples of \$100,000 between \$1.2 and \$4.5 million whose $\pm 10\%$ windows hold no threshold. Poisson regression on zone counts with price point $\times$ period, price point $\times$ zone and zone $\times$ period effects; reported as $\exp(\beta) - 1$. 95% intervals from 500 block-bootstrap draws that resample calendar months within each period. One row per deed except in the second column, which uses every row of the sales file (Section 3). Treating each placebo in turn as if it had been taxed gives changes at exactly X between -11% and $+68\%$, and in $(X, 1.05X)$ between -13% and $+22\%$. Panel B re-estimates the pooled and by-threshold models with other comparison groups and samples, one row per deed. ^aThe never-taxed multiples of \$500,000 among the placebos (\$1.5M, \$2.5M, \$3.5M, \$4M, \$4.5M). Treating each of these in turn as if it had been taxed, against the control and the other four, gives changes at exactly X between -13% and $+54\%$ and in $(X, 1.05X)$ between -4% and $+7\%$. Panel B intervals from 300 draws. Replication package, `focal_inference.py`, `did_round_placebos.py`.

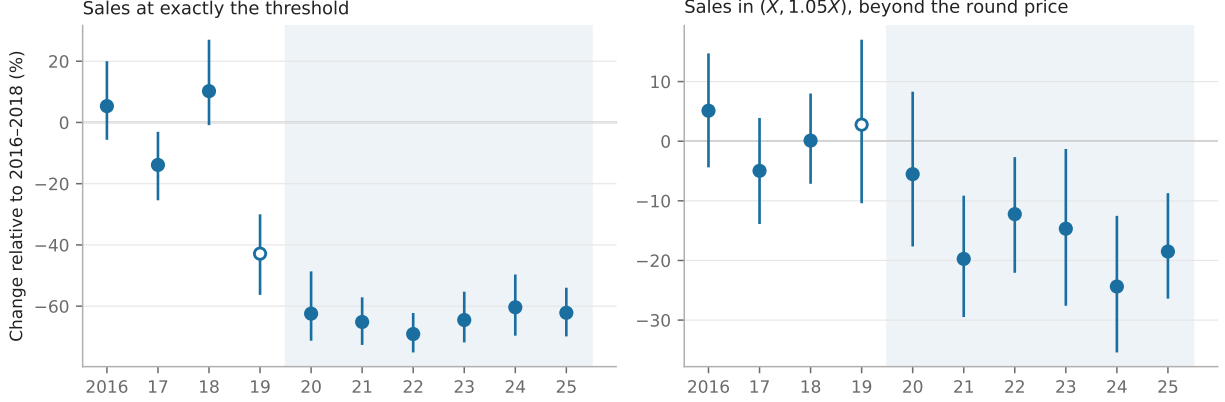


Figure 2: The round price emptied with the reform, and the response beyond it built up after. Percentage change in sales at exactly the threshold (left) and in $(X, 1.05X)$ excluding X (right) at the three new thresholds, relative to the 2016–2018 mean and net of the same change at the always-taxed \$1,000,000 threshold and 24 never-taxed round prices (equation (2) with year effects). 95% intervals from 300 block-bootstrap draws over calendar months. The hollow point is 2019, when grandfathered contracts were still closing; the shaded years follow the reform. One row per deed. Replication package, `focal_inference.py`, `make_did_figure.py`.

Consider sales that would close near X without the tax. Some would be agreed at exactly the round price X , and the rest have prices spread smoothly around it. A jump of T dollars in tax on the whole price at X gives every sale at or just above X a reason to record a lower price. Suppose that recording a price below X costs the parties to a sale an amount F , which varies across sales with distribution G . F stands for everything that comes between a sale and a lower recorded price: noticing the notch in time, reopening a price already agreed or written into a contract or an offering plan, and settling on a figure other than a focal one. A sale agreed at exactly X can move by giving up little: the added sales sit about \$2,400 below the threshold on average (Table 2). It moves when F is below the tax saved, so the share of round-price sales that move is about $m(T) = G(T)$.

Three predictions follow. First, m rises with T over the range where G has mass and is flat where it has none. Second, a sale agreed at a price δ above X but within the dominated range $D = T/(1 + t)$, where a buyer who noticed the notch would never choose to transact, has to reopen a price that was not focal and change it by more. If that costs at least as much, the share of such sales that disappear is at most m . Third, beyond the dominated range a sale moves only if the tax saved exceeds the value of the price cut to the parties, as in the frictionless notch model of Kleven and Waseem (2013), so the response there should be larger where T is larger.

Table 4 sets out the evidence. In Panel A the round-price share is 0.61, 0.64 and 0.61 at the three new thresholds, 0.62 on average (95% interval 0.55 to 0.68), and equality is not

rejected ($p = 0.86$), although the total jump rises 7.5-fold, from \$5,000 to \$37,500. Panel C adds the other two notches in the data, as cross-sectional comparisons. At the mortgage recording tax’s \$500,000 loan notch, where the jump is \$625, about 0.40 of the loans that would have been written at exactly \$500,000 are written just below it (0.32 to 0.43, depending on which untaxed round amount is the benchmark). At the \$1,000,000 mansion-tax notch, in place since 1989 with a \$10,000 jump, there is no spike at the threshold at all: the share is 0.99. Read through G , and treating G as the same across these markets, which is an assumption, about 40% of round-price transactions face costs below \$625, about 60% face costs below \$5,000, and the rest face costs above \$37,500 in each of the first six years of the new notches. Writing a different number on a contract costs far less than that. The share that moves rises with small stakes, does not change between \$5,000 and \$37,500, and is close to one at a notch that has been in place for thirty years. The last comparison mixes the notch’s age with everything else that differs between the \$1,000,000 market and the markets above \$2,000,000, and cannot separate them.

Panel B shows where the sales that stay are found. The share does not rise over the post-reform years: restricting the post-period to 2022–2025, when contracts signed before the April 2019 grandfathering date have long since closed, gives 0.60, and the round-price response in Figure 2 is between 60% and 69% in every year from 2020. It is lower in sales with a company seller, 0.42 against 0.68 without one (difference 26 points, 95% interval 8 to 62). Company sellers include developers, who set prices in offering plans before units are sold, and a price fixed in advance is one form of a high F . Financed buyers also record a price one dollar below the threshold about twice as often as cash buyers (Supplemental Appendix Table A2), as they would if a lender, an appraiser or a closing attorney makes the notch harder to miss. On every row of the sales file the shares are 0.58, 0.60 and 0.62.

The data cannot say how much of the remaining 40% is inattention, parties who never compared the price with the schedule, and how much is prices committed before anyone did. Both are frictions in the sense of Kleven and Waseem (2013). Slemrod et al. (2017) read the sales left in the dominated range of Washington, D.C.’s 2006 notch as a measure of how many transactions were aware of it and find a share of similar size. Field and experimental evidence finds that taxes that are less visible get less of a response (Chetty et al. 2009; DellaVigna 2009; Taubinsky and Rees-Jones 2018), and models of costly attention predict that attention rises with the stakes (Gabaix 2019), which fits the rise from \$625 to \$5,000 but not the flat share beyond it.

The second prediction holds on average. The share of sales in the dominated range that disappeared is 0.40, below the round-price share of 0.62, and the difference, 22 points, has an interval from 1 to 79 points. The estimates by threshold, 0.47, 0.55 and 0.17, are imprecise

because the dominated range is narrow and holds few sales, and they do not rise with the jump. They are too imprecise to say how the cost of reopening a non-focal price scales, and no cost is inferred from them. The third prediction concerns the zone $(X, 1.05X)$ of Table 3, which lies mostly beyond the dominated range: D covers 5%, 10% and 14% of its width at the three thresholds. There the response is -10% , -22% and -28% for jumps of \$5,000, \$15,000 and \$37,500, larger where the jump is larger. With three thresholds, how it scales is not identified (Section 4.6). Meanwhile 40% of the round-price sales still close at exactly the threshold and pay between \$5,000 and \$37,500 more tax for one extra dollar of price.

4.5 Bunching across the schedule

The bunching estimator puts the response on a common scale at every threshold. Table 5 reports it for the seven thresholds created in 2019 and for never-taxed round prices whose windows hold no threshold. At \$2,000,000, \$3,000,000 and \$5,000,000 the share of counterfactual mass missing above the threshold rises by 0.223, 0.384 and 0.287, and $\log R$ falls by 0.350, 0.459 and 0.479; sales at \$4,999,999 go from none to 26. Every placebo has an interval that includes zero on the bunching measure. Above \$5,000,000 the thresholds are thin and the bunching estimates are imprecise. The simplest count, sales in the 1% of price below each threshold against the 1% above, still reads: it shifts by $+1.97$ log units at \$10,000,000 and $+1.32$ at \$15,000,000, against $+1.74$ at \$2,000,000, but on fewer than 30 pre-reform sales in each band (Supplemental Appendix Table A3).

With all sales, the response is not proportional to the rate (Figure 3). The total rate rises by 0.25, 0.50 and 0.75 points at the three thresholds. The point estimate at \$5,000,000, where the jump is \$37,500, is close to the one at \$2,000,000, where it is \$5,000: bootstrapping the two jointly, the difference is $+0.064$ $[-0.136, +0.269]$. A response proportional to the total rate would make the \$5,000,000 change three times the \$2,000,000 one; the shortfall is -0.383 $[-0.731, -0.059]$ ($p = 0.027$). The \$3,000,000 change exceeds the \$2,000,000 one by $+0.161$ $[-0.000, +0.340]$, and equal responses at all three thresholds are not rejected ($p = 0.18$). Section 4.6 shows that the shortfall from proportionality is the round-price margin.

Inference against never-taxed prices. Table 6 judges each estimate against the same statistic at every \$50,000 multiple between \$1,200,000 and \$6,000,000 whose window contains no threshold: sixteen prices, from \$1,300,000 to \$1,650,000 and from \$3,800,000 to \$4,150,000. Their bunching changes average $+0.003$, with a standard deviation of 0.046 and a maximum of $+0.072$. The changes at the three new thresholds are each larger than all sixteen, and each log change in R is lower than all sixteen. Neighbouring windows share sales, so the rank

Table 4: The share of round-price sales that moved, and the dominated range

<i>Panel A. The 2019 thresholds, one row per deed</i>					
Threshold	Dominated range D	Sales at exactly X that moved		Sales in $(X, X + D]$ that disappeared	
		Raw	Against placebos	Raw	Against placebos
\$2,000,000	\$4,900	61%	63%	47%	33%
		[55, 67]	[57, 69]	[-21, 75]	[-62, 68]
\$3,000,000	\$14,500	64%	66%	55%	46%
		[55, 72]	[57, 74]	[18, 76]	[1, 71]
\$5,000,000	\$35,900	61%	63%	17%	7%
		[42, 73]	[45, 75]	[-125, 63]	[-155, 60]
Average of the three		62%		40%	
		[55, 68]		[-17, 60]	
<i>Panel B. Sales at exactly X that moved, other samples</i>					
		\$2,000,000	\$3,000,000	\$5,000,000	Average
Every row of the sales file		58%	60%	62%	60%
Post-reform years 2022–2025 only		61%	63%	57%	60%
No company seller on the deed		65%	69%	70%	68%
Company seller on the deed		44%	51%	32%	42%
<i>Panel C. Other notches</i>					
	Jump	Comparison		Moved	
Recording tax, \$500,000 loan	\$625	other round loan amounts		40%	
Mansion tax, \$1,000,000, since 1989	\$10,000	new thresholds before 2019		99%	

Notes. Each share is one minus the ratio of the post-reform to the pre-reform share of sales in $[0.9X, 1.1X)$ recorded at exactly X (Panel A, columns 3–4, and Panel B) or in the dominated range $(X, X + D]$, excluding X itself (Panel A, columns 5–6). $D = T/(1 + t)$, with T the jump in all transfer taxes and t the total rate just above X : no buyer who noticed the notch would record a price in that range. Against placebos divides the post-to-pre ratio by its average over the 24 never-taxed multiples of \$100,000 of Table 3 (for the dominated range, over the same proportional band above each). The difference between the two averages in Panel A is +22 points (95% interval +1 to +79). 95% bootstrap intervals (1,000 draws) in brackets. Panel B: the company-seller flag comes from the linked deed and includes sponsor sales; the average without a company seller exceeds the average with one by 26 points (95% interval 8 to 62). Panel C compares levels, not periods. For loans, the share of loans in the last \$1,000 up to and including \$500,000 that sit at exactly \$500,000 (54%) against the same share at the untaxed round amounts from \$300,000 to \$700,000 (90% on average; 32% to 43% against any one of them). For \$1,000,000, the post-reform spike at exactly the threshold relative to local density (0.1, Table 1) against the average pre-reform spike at the three new thresholds (7.7). Replication package, `focal_inference.py`, `moved_share_checks.py`.

Table 5: The full 2019 schedule: every threshold, before and after the reform

Threshold	Buyer's jump (rate / dollars)	Sales in window pre/post	Bunching, post – pre [95% CI]	$\Delta \log R$, no counterfactual [95% CI]	Sales at $X - 1$ pre $\rightarrow$ post
<i>Panel A. Thresholds created by the 2019 reform</i>					
\$2,000,000	0.25 pt / \$5,000	9,250/22,421	+0.223 [+0.131, +0.323]	–0.350 [–0.457, –0.244]	4 $\rightarrow$ 127
\$3,000,000	0.25 pt / \$7,500 ^a	5,233/11,607	+0.384 [+0.256, +0.532]	–0.459 [–0.596, –0.322]	2 $\rightarrow$ 68
\$5,000,000	0.75 pt / \$37,500	2,367/5,024	+0.287 [+0.126, +0.463]	–0.479 [–0.687, –0.271]	0 $\rightarrow$ 26
\$10,000,000 ^b	1.00 pt / \$100,000	809/1,392	+0.294 [–0.010, +0.612]	–0.397 [–0.781, –0.012]	3 $\rightarrow$ 7
\$15,000,000 ^b	0.25 pt / \$37,500	414/685	+0.083 [–0.456, +0.652]	–0.248 [–0.732, +0.235]	0 $\rightarrow$ 2
\$20,000,000 ^b	0.25 pt / \$50,000	218/431	+0.363 [–0.166, +1.087]	–0.591 [–1.256, +0.073]	0 $\rightarrow$ 0
\$25,000,000 ^b	0.15 pt / \$37,500	164/258	–0.607 [–1.630, +0.430]	+0.151 [–0.694, +0.995]	0 $\rightarrow$ 0
<i>Panel B. Round-number placebos, never taxed</i>					
\$4,000,000	none	3,321/7,063	–0.056 [–0.197, +0.098]	+0.091 [–0.074, +0.255]	1 $\rightarrow$ 5
\$7,000,000	none	1,490/2,922	+0.079 [–0.225, +0.402]	–0.170 [–0.414, +0.073]	0 $\rightarrow$ 0
\$8,000,000	none	1,227/2,217	–0.147 [–0.464, +0.181]	+0.067 [–0.214, +0.348]	0 $\rightarrow$ 0

Notes. Pre is January 2016 to March 2019; post is January 2020 to December 2025. One row per deed. Windows scale with the threshold so one design applies across the schedule. Placebos are never-taxed round prices whose window contains no threshold. Bunching is the Kleven–Waseem share of counterfactual mass missing above X (window $\pm 0.2X$, bins of $0.005X$ rounded down to \$10,000, \$25,000, \$50,000 or \$100,000, exclusion band $(-0.025X, +0.05X)$, degree-five polynomial with round-number indicators), post minus pre, with bootstrap percentile intervals. R is sales in $[X, 1.05X)$ over sales in $[0.925X, 0.975X)$; its log change has a Poisson interval. ^aAt \$3,000,000 the seller's base rate also rises 0.25 points. ^bFewer than 1,000 pre-reform sales in the estimation window. On samples this small the estimates are imprecise, and no conclusion is drawn from these rows.

$(1 + k)/17$, where k counts placebos at least as extreme, is a reference comparison rather than a calibrated p -value. Resampling each larger placebo window to a treated threshold's own sample size, the \$2,000,000 and \$3,000,000 changes lie 3.5 and 4.6 standard deviations from the draws' mean and exceed every draw; the \$5,000,000 change, on a quarter of the sample, lies 2.2 standard deviations out, with 1.4% of draws as large. Against these draws the shortfall from proportionality is not unusual: 11% of placebo contrasts are at least as extreme (12% when the two placebo windows must not overlap), and 37% against a wider set of 41 thinner placebos up to \$8,300,000. The control does not stand out on bunching, and its log change in R , -0.113 , is exceeded by one placebo; the event study locates that change.

Timing. Figure 4 computes both bunching statistics year by year, each normalized to its own 2016–2018 mean, against the 90% range of the same series at size-matched placebos. Before the reform the treated series stay inside the band in every year. From 2020 the change at \$2,000,000 is immediate and does not fade: $\log R$ lies below the band in all six post-reform years, between -0.28 and -0.41 , and bunching lies above it in five. At

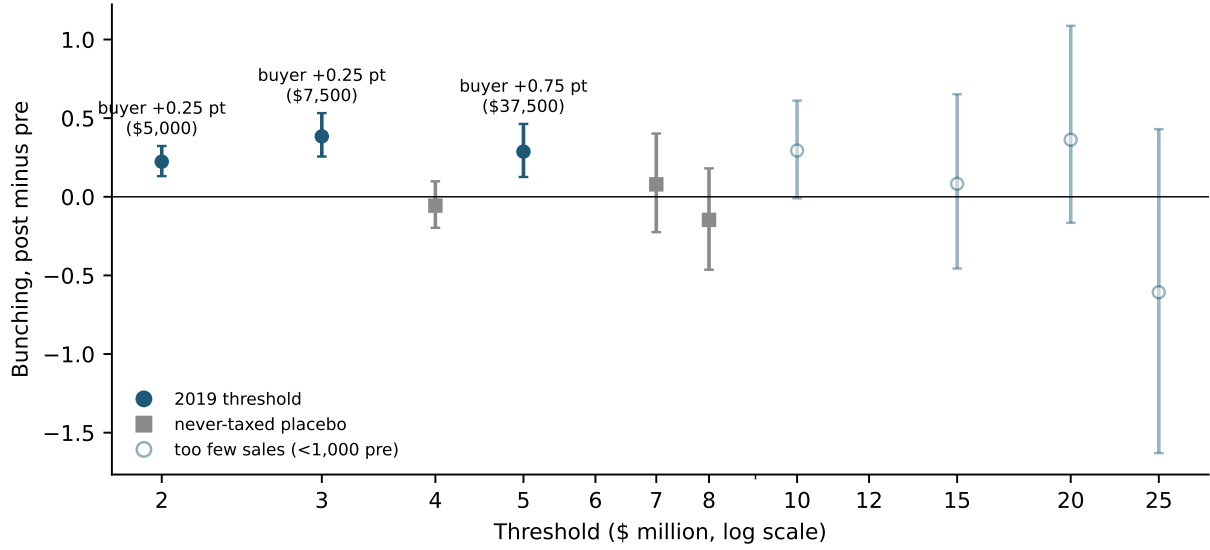


Figure 3: With all sales, the response does not rise in proportion to the jump. Change in bunching after the 2019 reform at each threshold of the schedule, with 95% bootstrap intervals (Table 5). Without the sales at the threshold and one dollar below it, proportionality is not rejected (Table 7). Filled circles are reformed thresholds with enough sales to estimate; squares are never-taxed placebos whose windows hold no threshold; as in Table 5, the jump shown is the buyer’s; hollow markers have fewer than 1,000 pre-reform sales in the window.

\$3,000,000 both statistics are outside the band in all six years; at \$5,000,000, the smallest sample, log R is outside in five and bunching in one. Moving the reform back to January 2018 gives bunching changes of -0.095 , -0.058 and -0.024 , all inside their placebo distributions (shares of 0.43 or more). The control’s log R falls in 2020–2022, by 0.33 in 2020, and is back near its baseline from 2023; the dip coincides with the pandemic. From 2023 to 2025, with the control near its baseline, log R stays between -0.28 and -0.41 at \$2,000,000, between -0.37 and -0.58 at \$3,000,000 and between -0.41 and -0.61 at \$5,000,000.

4.6 Beyond the round price, and other robustness

Table 7 re-estimates the bunching statistics without the round-price sales, at the thresholds and at every placebo alike. Dropping only the sales at exactly X and $X - 1$ cuts the change at \$2,000,000 from 0.223 to 0.041 $[-0.027, +0.110]$, exceeded by six of the sixteen placebos and below the control’s 0.056; dropping everything within \$1,000 or \$5,000 of the threshold gives the same answer. At \$2,000,000 the measured bunching is therefore almost entirely the round price. At \$3,000,000, 0.198 $[+0.080, +0.314]$ remains and at \$5,000,000, 0.179 $[+0.028, +0.333]$, each above every placebo, and about 0.20 and 0.18 when sales within \$1,000 or \$5,000 are dropped. Net of the control’s own change, the \$3,000,000 change is

Table 6: Placebo-calibrated inference for the 2019 notches

Threshold	Change,	Placebos at	Rank	Size-matched placebos		
	post – pre	least as extreme	$(1 + k)/17$	Resampling SD	z	As large
<i>Panel A. Bunching (share of counterfactual mass missing above X)</i>						
\$1,000,000 (control)	+0.059	2 of 16	0.18	—	—	—
\$2,000,000	+0.223	0 of 16	0.06	0.067	3.5	0 of 400
\$3,000,000	+0.384	0 of 16	0.06	0.086	4.6	0 of 400
\$5,000,000	+0.287	0 of 16	0.06	0.124	2.2	11 of 800
<i>Panel B. Log change in R, no counterfactual fitted</i>						
\$1,000,000 (control)	–0.113	1 of 16	0.12	—	—	—
\$2,000,000	–0.350	0 of 16	0.06	—	—	—
\$3,000,000	–0.459	0 of 16	0.06	—	—	—
\$5,000,000	–0.479	0 of 16	0.06	—	—	—
<i>Panel C. Tests across thresholds (bunching)</i>						
	Estimate	Bootstrap p		Size-matched placebo share		
\$5M – \$2M	+0.064	0.535		0.644		
\$5M – $3 \times$ \$2M (proportional)	–0.383	0.027		0.112		
\$3M – \$2M	+0.161	0.061		0.145		

Notes. One row per deed. Estimator and windows as in Table 5. Placebos are the sixteen \$50,000 multiples between \$1,200,000 and \$6,000,000 whose window $[0.8X, 1.2X)$ contains none of the thresholds at \$500,000, \$1,000,000, \$2,000,000, \$3,000,000, \$5,000,000 and above. The rank column is one-sided in the direction a notch predicts (bunching up, R down): $(1 + k)/17$, where k is the number of placebos at least as extreme. It is a reference ranking rather than an exact p -value: a calibrated rank test would need an exchangeability argument the design does not supply, and neighboring placebo windows overlap. Size-matched placebos resample each placebo window with more sales than the treated window, with replacement, to the treated window’s pre- and post-reform counts, fifty times per placebo (eight placebos for \$2,000,000 and \$3,000,000, sixteen for \$5,000,000); z is the estimate’s distance from the draws’ mean in their standard deviations. No placebo has as many sales as the \$1,000,000 window, so the control has no size-matched row. Panel C reports the two-sided share of differences between draws at two distinct placebos that are at least as large (for the proportional contrast, 0.118 when the two placebo windows must not overlap); these shares are reference comparisons, not calibrated p -values. Bootstrap p -values are from the joint bootstrap of Section 4.5. Supplemental Appendix Table A6 lists every placebo. Replication package, `placebo_inference.py`.

+0.143 [+0.019, +0.265] and the \$5,000,000 change +0.123 [–0.032, +0.283].

Without the round-price sales, the responses at \$3,000,000 and \$5,000,000 are larger than at \$2,000,000, by 0.157 ($p = 0.026$) and 0.138 ($p = 0.09$), and neither departs from a response proportional to the total rate: the differences from twice and three times the \$2,000,000 change are +0.116 ($p = 0.22$) and +0.056 ($p = 0.67$). With three thresholds and a \$2,000,000 response close to zero, these tests have little power, so the evidence is that the response beyond the round price is larger at larger jumps, not how it scales. The shortfall

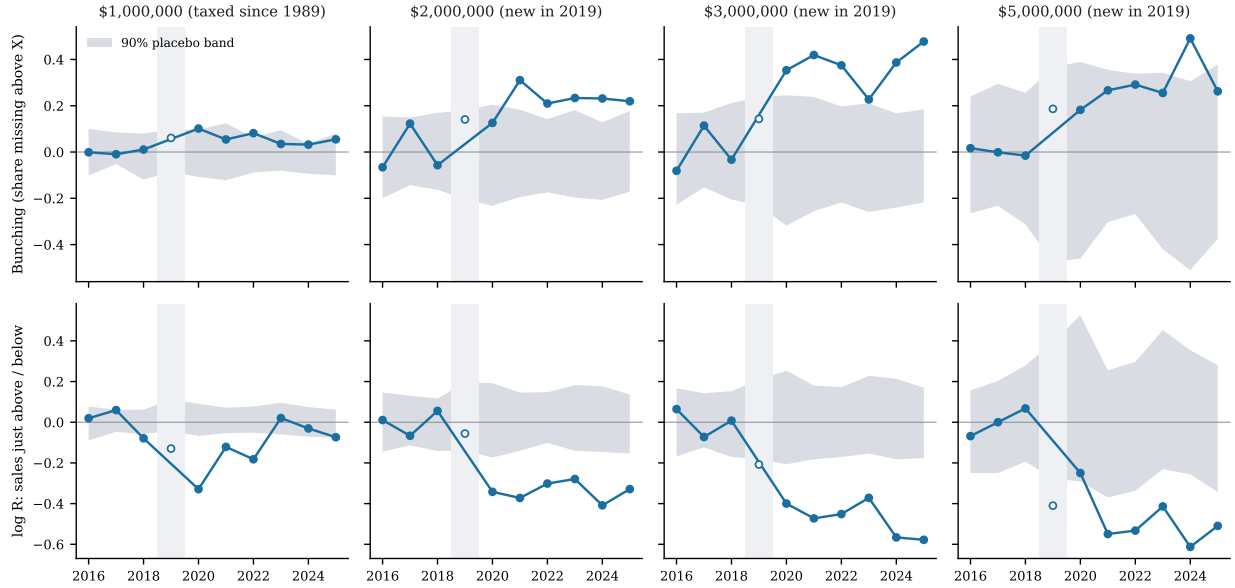


Figure 4: The response starts with the reform. Bunching (top) and log R (bottom) by year, each normalized to its 2016–2018 mean, at the always-treated \$1,000,000 threshold and the three thresholds created in 2019. Grey bands are the 90% range of the same statistic at never-taxed placebos resampled with replacement to each threshold’s yearly sales (for the control, the seven placebos with at least half its sales, without subsampling). Hollow points mark 2019, the transition year, which no test uses. Replication package, `event_study.py`.

from proportionality in the full sample is the round-price margin, whose size is set by how many deals sit at the round number and by the share that moves, not by the jump.

One row per deed. The deed rule matters most beyond the round price. On every row of the sales file the bunching changes are 0.220, 0.321 and 0.219 with all sales and 0.044, 0.146 and 0.101 without the round-price sales, so the \$5,000,000 estimate beyond the round price falls by almost half and one placebo matches it; the difference-in-differences above the round price is -12% instead of -17% , with an interval from -22% to -1% . The round-price margin is not sensitive: on every row the moved shares are 58%, 60% and 62% (Table 4). The rule was checked against the deed records (Supplemental Appendix Table A14). Within 10% of the three new thresholds it removes 1,354 rows, almost all after the reform, when the sales file repeats multi-lot deeds far more often. Of these, 567 belong to groups whose lots all appear on one recorded deed in ACRIS, 278 repeat the same unit, and 386 are co-operative apartments in one building recorded on the same day at a price at least twice the building’s usual sale price, a price for several apartments together. Only 51 are separate deeds merged in error; another 51 are co-operative apartments at ordinary prices, which could be separate sales, and 21 have no deed in ACRIS. In the 5% above \$3,000,000 and \$5,000,000 after the

Table 7: The response with and without the round price

<i>Panel A. Change in bunching, post – pre (placebos at least as large, of 16)</i>				
	\$1,000,000 (control)	\$2,000,000	\$3,000,000	\$5,000,000
All sales (Table 5)	+0.059 (2)	+0.223 (0)	+0.384 (0)	+0.287 (0)
Without sales at X and $X - 1$	+0.056 (5)	+0.041 (6)	+0.198 (0)	+0.179 (0)
	[+0.028, +0.087]	[−0.027, +0.110]	[+0.080, +0.314]	[+0.028, +0.333]
Without sales within \$1,000	+0.058 (5)	+0.034 (6)	+0.203 (0)	+0.179 (0)
Without sales within \$5,000	+0.068 (4)	+0.035 (6)	+0.206 (0)	+0.188 (0)
<i>Without sales at X and $X - 1$:</i>				
Net of the control	—	−0.015	+0.143	+0.123
		[−0.086, +0.060]	[+0.019, +0.265]	[−0.032, +0.283]
Houses only	+0.003 (6)	+0.046 (4)	+0.222 (1)	+0.112 (3)
Every row of the sales file	+0.058 (2)	+0.044 (2)	+0.146 (0)	+0.101 (1)
Indicators on containing bin	+0.061	+0.041	+0.191	+0.166
<i>Panel B. Log change in R (placebos at least as low, of 16)</i>				
All sales	−0.113 (1)	−0.350 (0)	−0.459 (0)	−0.479 (0)
Without sales at X and $X - 1$	−0.110 (3)	−0.137 (1)	−0.305 (0)	−0.385 (0)
<i>Panel C. Tests across thresholds (bunching)</i>				
	All sales		Without X and $X - 1$	
	Estimate	Bootstrap p	Estimate	Bootstrap p
\$5M – 3 × \$2M (proportional)	−0.383	0.027	+0.056	0.67
\$3M – 2 × \$2M (proportional)	−0.062	0.607	+0.116	0.22
\$5M – \$2M	+0.064	0.535	+0.138	0.09
\$3M – \$2M	+0.161	0.061	+0.157	0.03

Notes. Estimator, windows, periods and placebos as in Tables 5 and 6. Each row drops the stated sales in both periods, at the thresholds and at every placebo alike; the dropped prices lie inside the range left out of the counterfactual fit, and where a bin loses part of its price range its counterfactual count is scaled by the share kept. 95% bootstrap intervals (500 draws) in brackets. Net of the control subtracts the \$1,000,000 change, with independent draws for the two windows, which do not overlap. Houses are building classes 01–03, with placebos recomputed among houses. The analysis keeps one row per deed; the every-row line uses all 442,497 rows of the sales file above \$300,000, 8,389 more. The estimator marks, for \$10,000 bins, the two bins on either side of each multiple of \$100,000 (Section 4.6); the last row of Panel A marks instead the bin that contains each multiple of \$100,000, \$50,000 and \$25,000. R is sales in $[X, 1.05X)$ over sales in $[0.925X, 0.975X)$. Panel C: the proportional rows test a response proportional to the change in the total transfer-tax rate, 0.25, 0.50 and 0.75 points at \$2, \$3 and \$5 million; all-sales p -values from 1,000 joint draws (Section 4.5); without X and $X - 1$, from 500. Replication package, `focal_robustness.py`.

reform, where the two counts differ most, every removed row is one deed, a repeated row or a bulk co-operative price, so the every-row estimates there count the same sales more than once. The rule is less accurate at the \$1,000,000 control: 151 of the 382 rows it removes there are separate deeds, mostly developers’ sales of identical units on the same day.

Other checks. Marking the round-number bins that contain multiples of \$100,000, \$50,000 and \$25,000, rather than the bins on either side of each multiple of \$100,000, changes the estimates by at most 0.017. A fixed $\pm\$400,000$ window with \$10,000 bins on every row gives changes of 0.220 and 0.461 at \$2,000,000 and \$3,000,000 and +0.005 at the control, stable across period definitions (Supplemental Appendix Tables A1 and A10). All estimates use nominal consideration, to which the statute attaches; the share of sales above \$2,000,000 rose from 10.2% before the reform to 11.8% after, but a 2017–18 against 2020–21 comparison, over which prices changed little, leaves the \$2,000,000 response at +0.202 in the fixed window. In the fixed window’s pre-period the estimator returns a missing share of 0.094 at the then-untaxed \$2,000,000, with an interval excluding zero: levels are biased, which is why only differences are used. Differencing assumes the bias is the same in both periods; the placebos, whose own round-number spikes also changed, are the check on that assumption.

4.7 Where the response comes from

The linked dataset (Loschi 2026a) attaches property type, borough, the seller on the deed and the deed’s classification to the same sales. Table 8 splits the bunching change by each, against placebos recomputed within the same sample. Among houses the changes at the three new thresholds are +0.292, +0.485 and +0.438, and among condominiums +0.196, +0.376 and +0.247; in Manhattan +0.225, +0.355 and +0.220, and in the other four boroughs +0.212, +0.499 and +0.680. Of these twelve, only houses at \$2,000,000 are matched by any placebo, by two of sixteen. The house estimates at \$3,000,000 and \$5,000,000 rest on 1,003 and 299 pre-reform sales, and the last outer-borough estimate on 330, so they are imprecise. The control’s movement in the pooled data comes from condominiums (+0.151) and Manhattan (+0.091); among houses it does not move (+0.005, with eight placebos as large). Among condominiums the new thresholds’ changes exceed their own control’s by +0.04, +0.23 and +0.10, so the condominium evidence rests mainly on \$3,000,000. Co-operatives’ changes are +0.192, +0.352 and +0.275, in a noisy sample.

Developers set condominium prices in advance, and some sponsor sales that closed after the reform were contracted before the April 2019 cut-off. The linked deed shows whether a company sold, a proxy that includes sponsor sales. In sales without a company seller the changes are +0.339, +0.387 and +0.504, each larger than every placebo in that sample,

and the control does not move (+0.021). In company-seller sales the response is clear at \$3,000,000 (+0.431) and within placebo noise at \$2,000,000 (+0.079, six placebos as large) and \$5,000,000 (+0.124, five placebos as large; Supplemental Appendix Table A8). Referee’s deeds and deeds in lieu of foreclosure are recorded as plain deeds and stay in the sales file, but they are rare near these prices (0.2–1.2% of window sales have a lender or referee party). Dropping the deeds whose consideration a companion paper’s classifier marks as set by a legal rule (Loschi 2026b) leaves the changes at +0.227, +0.389 and +0.288, and dropping every deed with a lender or referee party at +0.229, +0.387 and +0.291.

Table 8: Where the response comes from

<i>Change in bunching, post – pre (placebos at least as large, of 16)</i>					
	\$1,000,000 (control)	\$2,000,000	\$3,000,000	\$5,000,000	Placebo SD
All sales	+0.059 (2)	+0.223 (0)	+0.384 (0)	+0.287 (0)	0.046
Houses	+0.005 (8)	+0.292 (2)	+0.485 (0)	+0.438 (0)	0.159
Condominiums	+0.151 (1)	+0.196 (0)	+0.376 (0)	+0.247 (0)	0.083
Co-operatives	+0.035 (7)	+0.192 (3)	+0.352 (0)	+0.275 (1)	0.173
Manhattan	+0.091 (0)	+0.225 (0)	+0.355 (0)	+0.220 (0)	0.060
Other four boroughs	+0.040 (3)	+0.212 (0)	+0.499 (0)	+0.680 (0)	0.076
No company seller	+0.021 (7)	+0.339 (0)	+0.387 (0)	+0.504 (0)	0.070
Company seller (sponsor proxy)	+0.082 (6)	+0.079 (6)	+0.431 (0)	+0.124 (5)	0.178
Without legal-rule deeds	+0.055 (2)	+0.227 (0)	+0.389 (0)	+0.288 (0)	0.046
Without any lender or referee deed	+0.054 (2)	+0.229 (0)	+0.387 (0)	+0.291 (0)	0.047

Notes. One row per deed. Estimator, windows and periods as in Table 5. Property types follow the Department of Finance building-class category (houses 01–03; condominiums 04, 12, 13, 15; co-operatives 09, 10, 17) and come from the linked dataset (Loschi 2026a), whose sales are the same records as this paper’s sample. Each row’s placebos are the sixteen of Table 6, recomputed within that row’s sample. The last two market rows split sales by whether the linked deed names a company as seller, a proxy that includes sponsor (developer) sales and other company sales; sales without a linked deed are in neither row. Without legal-rule deeds drops deeds whose consideration the companion paper’s classifier (Loschi 2026b) marks as set by a legal rule (referee’s deeds to lenders, deeds in lieu of foreclosure, lender-to-lender transfers); the last row drops every deed with a lender or referee party; sales with no linked deed (co-operatives, Staten Island sales, and houses and condominiums whose deed was not matched) are kept. Supplemental Appendix Table A7 adds log changes in R and window sizes. Replication package, `market_splits.py` and `cost_and_sponsors.py`.

4.8 How far prices moved, how the deals were struck, and what it cost

How far. Dividing the excess mass below a threshold and the missing mass above it by the counterfactual density at the threshold expresses each as a width of price (Table 9). With all sales, the pile below \$2,000,000 grew by the equivalent of \$16,900 of price, more than three times the \$5,000 jump. Read literally, the moved sales cut their prices by far more than the tax they saved. Table 2 shows why that reading is wrong: the added sales sit on average \$2,400 below the threshold, and most came from the round price itself. A width measures how much price range a pile would empty if its sales came from a smooth distribution; when they come from a mass point at the threshold, it overstates the price change. Without sales within \$5,000 of each threshold, the pile at \$2,000,000 shrinks to \$5,200 and the hole above it to \$400, and at \$3,000,000 and \$5,000,000 the holes are \$12,500 and \$35,600, no wider than the dominated range at each threshold. At those two thresholds many sales therefore stay where the notch leaves a buyer who noticed it strictly worse off, the signature of optimization frictions (Kleven and Waseem 2013). At \$2,000,000 the dropped \$5,000 covers the whole dominated range, so the hole there says nothing about it. The long-standing \$1,000,000 notch looks the same: three-quarters of its post-reform pile width lies within \$5,000 of the threshold.

The usual parameters inherit the problem. The local incidence of Kopczuk and Munroe (2015), the pile width relative to the jump, is 3.38 at \$2,000,000 with all sales; for the sales that moved off the round price, the recorded price fell by about half the buyer's \$5,000 saving at \$2,000,000 and by less than a fifth of the jump at \$3,000,000 and \$5,000,000. Their unraveling statistic, the hole width less the pile width, is not informative here: its interval includes zero at the new thresholds, and at \$1,000,000 it is significantly negative with this paper's estimator ($-\$4,581$) and significantly positive with Kopczuk and Munroe's level-shift counterfactual ($\$26,800$). Read through the frictionless model of Kleven and Waseem (2013), with $1 + t$ in place of $1 - t$, the all-sales widths imply recorded-price elasticities of 0.004 to 0.017 at the new notches and 0.04 to 0.05 at \$1,000,000; without the sales within \$5,000 of each threshold, those at the new notches fall to 0.006 or less. Because the round-price margin dominates the widths and frictions dominate beyond it, these numbers summarize local responses in recorded prices rather than structural elasticities, and the gap between new and old notches cannot be read as an effect of a notch's age.

How the deals were struck. A sale recorded at \$1,997,600 may have been negotiated at that price, or agreed at \$2,000,000 with value moved off the deed into personal property,

Table 9: How far sales moved: bunching widths with and without the round price

Threshold	Jump T	Dominated range	Pile width		Hole width		Elasticity, all sales	
			All	Without	All	Without	Pile	Hole
			(\$ thousand)					
\$2,000,000	5.0	4.9	16.9	5.2	10.8	0.4	0.017	0.008
\$3,000,000	15.0	14.5	14.1	1.2	25.9	12.5	0.004	0.010
\$5,000,000	37.5	35.9	59.9	13.2	57.7	35.6	0.013	0.013
\$1,000,000 (level)	10.0	—	30.1	7.8	25.6	21.4	0.053	0.040

Notes. Widths are the excess mass in $(0.975X, X)$ (pile) and the missing mass in $(X, 1.05X)$ (hole), each divided by the counterfactual density of sales per dollar at X ; at the 2019 thresholds they are post-reform minus pre-reform, at \$1,000,000 the post-reform level. “Without” drops sales within \$5,000 of X in both periods, as in Table 7. T is the jump in all transfer taxes at X ; the dominated range is the band above X in which a buyer who saw the notch could never prefer to transact, $T/(1+t)$ with t the total transfer-tax rate just above X . The elasticity is that of the recorded price with respect to one plus the transfer-tax rate, solved from the indifference condition of the marginal buncher (Kleven and Waseem 2013) with $1+t$ in place of $1-t$, all transfer taxes counted and no optimization frictions, from the all-sales widths; counting only buyer-paid taxes changes them by at most 0.005. Replication package, `focal_robustness.py`, `notch_elasticity.py`.

credits or closing adjustments. The records cannot observe side payments, but loan amounts give one check. If deals were agreed at the round price and trimmed afterwards, loans sized on that price would often be round fractions of it. Among 353 financed post-reform purchases recorded in the \$10,000 below \$2,000,000, 10% have a loan equal to 50%, 55%, ..., or 95% of \$2,000,000, the same as before the reform (8% of 37), while 43% have a loan equal to one of those fractions of the recorded price instead. At exactly \$2,000,000 the two coincide, and 71% of loans are such fractions. At \$3,000,000 the shares are 7% and 43% of 156. Most moved sales were therefore priced below the threshold by the time the loan was sized. That is evidence against a trim made after the loan was arranged, though weak evidence, since loans are commonly sized on the final contract price; it says nothing about consideration moved off the deed during negotiation; Montalvo et al. (2020) propose comparing declared prices with appraisals, which are not in the public record.

What the notches cost. A sale that moves below a notch avoids at least the jump in tax. With the bunching estimator, about 129 extra sales a year sit just below \$2,000,000, 45 below \$3,000,000 and 30 below \$5,000,000 (Table 10), each pile larger as a share of its window than at any placebo. If they crossed the threshold, as Table 2 indicates, they avoid \$2.4 million a year at the jump alone (95% interval \$1.9–\$3.0 million, conditional on the estimator). Counting the moved sales directly gives \$2.9 million, every row of the sales file \$2.2 million, and netting out the control’s own excess \$1.6 million. Each is under 1% of

the \$296 million a year that the reform’s residential schedule implies on post-reform sales (Table 11). These are static counts of sales that moved below a threshold; they leave out any sales that did not take place.

Table 10: What the 2019 notches cost in avoided tax

Threshold	Jump at X	Extra sales just below X , a year	Missing minus extra, a year	Tax avoided at the jump, \$M a year
\$1,000,000 (control)	—	106 [18, 182]	−27 [−121, +75]	—
\$2,000,000	\$5,000	129 [100, 157]	−47 [−101, +16]	0.65 [0.50, 0.79]
\$3,000,000	\$15,000	45 [25, 62]	+37 [−4, +81]	0.67 [0.38, 0.94]
\$5,000,000	\$37,500	30 [18, 40]	−1 [−27, +25]	1.11 [0.67, 1.52]
Total, new thresholds				2.43 [1.88, 2.96]

Notes. Post-reform period, January 2020 to December 2025. Extra sales just below X are the excess mass in $(0.975X, X)$ after the reform minus the pre-reform excess rescaled to the post-reform counterfactual density; missing minus extra is the corresponding missing mass in $(X, 1.05X)$ less that excess. Estimator and windows as in Table 5; 95% bootstrap intervals in brackets, covering sampling uncertainty conditional on the estimator. Each extra sale is assumed to have crossed the threshold from above (Table 2 shows most came from the round price itself) and to avoid the statutory jump at X (at \$3,000,000 the buyer’s 0.25 points and the seller’s 0.25-point base-rate rise); it also lowers the other transfer taxes by about 3–4% of a price reduction that is mostly under \$5,000, which is not counted. Other counts give \$2.9 million when the moved sales are counted directly as those lost at and above X (Table 2), \$2.2 million on every row of the sales file, and \$1.6 million when each pile is reduced by the control’s own excess as a share of its window. The reform’s whole residential schedule implies \$296 million a year on post-reform sales from \$2,000,000 (Table 11). At the \$1,000,000 control, whose tax did not change, the extra sales are within the range of the never-taxed placebos. Replication package, `cost_and_sponsors.py`, `focal_inference.py`, `benchmarks_v29.py`.

4.9 A schedule without jumps

The jumps exist because each 2019 rate applies to the whole price. A slice schedule, which charges each rate only on the part of the price above its threshold, has none. Holding recorded prices fixed, Table 11 gives two slice schedules that raise the same \$296 million a year from post-reform sales of \$2,000,000 or more. One marginal rate of 2.36% on the part above \$2,000,000 does it, but it raises the average tax between \$2,000,000 and \$10,000,000 and lowers it above, so that 77% of these sales would pay more. The statutory steps scaled into marginal rates, from 0.48% above \$2,000,000 to 6.11% above \$25,000,000, keep the schedule’s progressivity: on average they lower the tax in every band below \$10,000,000 and raise it above, and 7% of sales would pay more. The remaining kinks change only marginal rates, and the bunching literature generally finds far smaller responses at kinks than at notches (Kleven 2016). Section 4.4 adds a reason: under a slice schedule the round-price sales that do not move no longer pay a jump for one dollar of price. The calculation is static.

Table 11: The 2019 schedule and two revenue-neutral slice schedules

	2019 notches (whole price)	Slice A (one marginal rate)	Slice B (scaled steps)
<i>Panel A. Tax added by the 2019 reform at a recorded price of</i>			
\$1,999,999	\$0	\$0	\$0
\$2,000,000	\$5,000	\$0	\$0
\$3,000,000	\$22,500	\$23,607	\$4,850
\$5,000,000	\$75,000	\$70,822	\$33,949
\$10,000,000	\$250,000	\$188,857	\$179,446
\$20,000,000	\$600,000	\$424,929	\$688,684
<i>Panel B. Mean tax added, by price band</i>			
\$2–3 million (2,528 a year)	\$6,103	\$10,415	\$2,140
\$3–5 million (1,486 a year)	\$28,639	\$42,931	\$16,760
\$5–10 million (788 a year)	\$101,053	\$111,824	\$84,491
\$10–25 million (238 a year)	\$392,004	\$299,682	\$415,612
\$25 million and above (48 a year)	\$1,363,299	\$974,488	\$2,096,701
<i>Panel C. Totals</i>			
Revenue, \$M a year	296	296	296
Sales paying more than under the notches	—	77%	7%

Notes. Post-reform sales (2020–2025) from \$2,000,000, 5,087 a year, one row per deed (Section 3), with recorded prices held fixed. The 2019 column is the supplemental tax under §1402-b (0.25% from \$2,000,000 to 2.90% from \$25,000,000) and the 0.25-point state base-rate rise at \$3,000,000, each charged on the whole price. A slice schedule charges each marginal rate only on the part of the price above its threshold. Slice A charges 2.36% on the part above \$2,000,000. Slice B applies the statutory increments as marginal rates scaled by a common factor of 1.94, giving cumulative marginal rates of 0.48%, 1.45%, 2.91%, 4.85%, 5.33%, 5.82%, 6.11% above \$2, 3, 5, 10, 15, 20 and 25 million. Both raise the same revenue as the 2019 notches on these sales. A static calculation: it ignores the bunching that removing the jumps would undo and any other behavioral response. Replication package, `benchmarks_v29.py`.

5 The mortgage recording tax’s loan notch

The recording tax is a notched tax on the loan: the borrower’s rate rises from 1.80% to 1.925% of the whole loan at \$500,000, a jump of \$625. Since the threshold did not change in the sample period, the evidence is cross-sectional, and \$500,000 is itself the roundest loan amount in this range. The comparison that matters is therefore with other round loan amounts.

Among the 172,725 purchase mortgages with an observed amount, 1,488 are for exactly \$500,000 and 1,277 lie in the \$1,000 below it. At untaxed round amounts from \$300,000 to \$700,000, between 79% and 95% of loans in the last \$1,000 up to and including the round amount sit at exactly the round amount; at \$500,000 only 54% do. Relative to loans in the \$25,000 below, loans just under \$500,000 are 3 to 10 times as common as loans just under the

other round amounts (0.175 against 0.018 to 0.063). The pattern repeats the one in prices: a large share of the loans that would have been written at the round amount are written a few dollars or a few hundred dollars below it, and many are still written at the round amount. Fitted against a smooth counterfactual in the manner of DeFusco and Paciorek (2017), the excess below the threshold is 0.47 (95% interval 0.42 to 0.52), equivalent to \$5,900 of loan and a recorded-loan elasticity of 0.059 (0.047 to 0.072), with widths of \$5,100 to \$6,900 across 27 specifications. Those widths overstate the change in borrowing for the reason Section 4.8 gives for prices.

Two other explanations do not fit. Lenders price loans differently above the conforming loan limit, and the national baseline limit moved from \$417,000 in 2016 through \$484,350 in 2019 and \$510,400 in 2020 to \$806,500 in 2025; the high-cost limit that applies in New York City was above \$625,000 throughout (Federal Housing Finance Agency 2025). The ratio of loans just below \$500,000 to loans just above stays between 0.53 and 0.97 in every year, wherever the baseline limit stood. And only 5% of the loans just below \$500,000 finance a price within \$1,000 below a transfer-tax threshold, against 2% of those just above, so the pile is not inherited from bunching in prices. The pile is consistent with borrowers responding to the recording tax, by borrowing a little less or moving financing outside the recorded mortgage, which the records cannot tell apart. One untaxed amount, \$550,000, also shows some loans just below it, for reasons not known.

The tax itself is not small: applied to each observed loan, the borrower-side schedule implies about \$244 million a year on purchases between \$300,000 and \$6,000,000 in the four ACRIS boroughs, and a purchase without a recorded mortgage pays none of it. Supplemental Appendix Section I documents the calculation, compares it with the Department of Finance’s reported liability (New York City Department of Finance 2025) and costs continuous schedules that would raise the same amount from every purchase.

6 What this paper cannot settle

How the moved deals were reached. The records show recorded prices, not the bargaining behind them. The loan check of Section 4.8 is weak evidence against a trim made after the loan was arranged. It cannot rule out value moved off the deed into personal property or credits during negotiation, and it cannot say whether a buyer, a seller or both gave up the difference.

Why the other 40% do not move. The round-price sales that still close at the threshold could be parties who never compared the price with the schedule, or prices committed before

anyone did, as in a developer’s offering plan. The share is lower when a company sells, but the records cannot separate the two explanations.

Responses beyond the round price. The response beyond the round price is clear at \$3,000,000, including net of the control’s own change, and at \$5,000,000 in the count-based difference-in-differences, but the \$5,000,000 bunching change is not significant net of the control and falls by almost half on every row of the sales file. Its size is uncertain, and with three thresholds the data cannot say how it scales with the jump.

Local responses, so no optimal rate. The widths and elasticities of Table 9 describe responses at discrete jumps, dominated by the round-price margin and by frictions. They do not identify responses to a continuous schedule, and the slice schedules of Table 11 are static. A welfare analysis would also need the economic incidence of each tax, which this paper does not estimate.

Foreclosure transfers are not removed from every sample. Referee’s deeds and deeds in lieu of foreclosure are recorded as plain deeds and remain in the sales file, and their consideration can follow a legal rule. They are rare near the 2019 thresholds, and Section 4.7 drops them using a companion paper’s classifier (Loschi 2026b). No estimate changes, so no result here depends on that classifier, whose validation against sampled deed documents read with AI tools is described in the companion paper.

7 Conclusion

New York’s 2019 reform allows a before-and-after comparison at the same transfer-tax thresholds. At each well-populated new notch, sales moved to just below the threshold when the reform took effect, and nothing similar happened at never-taxed round prices. Most of the movement was at the round price. The share of sales at exactly the threshold fell by about 60%, and those sales closed a few thousand dollars below it instead, less than the buyer’s tax saving. The fall was the same for a \$5,000 jump as for a \$37,500 one. The sales that stayed at the round price were more common when a company sold, and their share did not shrink between 2020 and 2025. Each of them paid the higher rate on the whole price for one extra dollar. Beyond the round price the response is smaller and larger at larger jumps: sales just above the threshold fell by 22% at \$3,000,000 and 28% at \$5,000,000, and not detectably at \$2,000,000. The avoided tax is \$1.6 to \$2.9 million a year, under 1% of what the reform’s schedule implies.

For measurement, bunching at a round-number notch should be read against a before period or with the round price modeled explicitly. Against a smooth counterfactual, sales moved about \$2,400 below \$2,000,000 register as a pile \$16,900 wide, and loans at the recording tax’s \$500,000 notch show the same pattern. For design, a quarter-point notch at a round price is enough to move most of the deals that would have closed there, and the deals that do not move pay a jump for one dollar. A slice schedule could raise the same revenue without jumps, with the burden across price bands set by its marginal rates.

Declarations

Data availability. All data are public and free. Sales are New York City Department of Finance annualized calendar sales, NYC Open Data resource `w2pb-icbu`. Deeds and mortgages are ACRIS Real Property Master and Legals, resources `bnx9-e6tj` and `8h5j-fqxa`, linked to the sales in a published dataset (Loschi 2026a), concept DOI 10.5281/zenodo.22977532. The replication package, concept DOI 10.5281/zenodo.22880953, holds the analysis data and the code for every table and figure and reproduces them without network access. Preprint versions are under concept DOI 10.5281/zenodo.22925301.

Declaration of generative AI use. Anthropic’s Claude and OpenAI’s ChatGPT, including Codex, were used for coding, analysis scripts, reference formatting, drafting and editing, and to cross-check each other’s analyses. The author checked the reported results against the deposited outputs and is responsible for the submitted version.

Declaration of competing interest. The author founded HolaCasaClara, a free comparator of Argentine mortgage loans, which has no connection to New York housing records or this study. No other competing interests are reported.

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