The Shape of the Tax

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

Pablo Loschi

Independent researcher, Berlin, Germany

loschi.pablo@gmail.com

ORCID 0009-0004-9455-4713

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Abstract

New York's 2019 budget added whole-price transfer-tax notches from $2 million. Using 434,108 sales, I compare each threshold before and after the reform against never-taxed round prices. The share of sales at exactly a new threshold fell 61 to 64 percent whether the tax jump was $5,000 or $37,500, and the missing sales appear $2,200 to $3,000 below. At $2 million this shift drives most of the standard bunching estimate, which drops from 0.223 to 0.041 without sales at the threshold and one dollar below. In New Jersey, where exactly $1 million owes no fee, the share fell 91 percent.

JEL: H26, H71, R31. *Keywords:* transfer taxes; notches; bunching; round numbers; optimization frictions; housing transactions.

I 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. I use 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.

I find that 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 percent, and the missing sales turn up just below the threshold, on average $2,200 to $3,000 below it, less than the buyer's tax saving. The fall is similar, with differences within sampling error, for jumps of $5,000, $15,000 and $37,500, and it holds against the never-taxed multiples of $500,000 and in 2022–2025 alone. Beyond the round price the response is smaller and appears only at the two larger jumps. Net of never-taxed round prices and the unchanged $1,000,000 notch, sales in the 5 percent of prices above the threshold fall by 22 percent at $3,000,000 and 28 percent at $5,000,000, and by an amount indistinguishable from zero at $2,000,000.

My main contribution is to measurement, where 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, and 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, which is within placebo noise. I use this exclusion as a diagnostic of the exact-price margin: it removes tax-responsive sales as well, so the remaining estimate does not recover a structural elasticity. Read as a width, the pile below $2,000,000 is $16,900 of price, against an average move of about $2,400. Because transaction-tax notches are almost always set at round prices, the same check should apply elsewhere. It amounts to estimating the response with and without the sales at the threshold and one dollar below it, against a before period or against never-taxed prices as round as the threshold.

New Jersey offers a related case, because its 1 percent fee on residential transfers, in place since August 2004, applies when the consideration is in excess of $1,000,000, so as written a sale at exactly the round price is untaxed (State of New Jersey, 2004). In the state's assessment rolls for all 21 counties, the share of sales near $1,000,000 recorded at exactly that price fell from 9.6 percent in 1999–2003 to 0.8 percent from August 2004 to July 2009, a moved share of 0.91, while never-taxed round prices changed little. Sales at $999,999, $999,000 and $995,000 rose instead. The decline at the untaxed round price is consistent with a response to a salient price boundary, and suggests that brokers or buyers treat a round cutoff as binding even when the statute exempts exact-round transfers. Because the assessment rolls do not show the fee paid or how participants understood the rule, the pattern could reflect misperception or cautious rounding behavior (Section IV.K).

Among the new thresholds, 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 markedly lower whenever a corporate or LLC entity appears on either side of the deed

(51 percent versus 70 percent for individual-to-individual resales), whereas newly built condominium offering plans rarely price at exact millions even before the reform; and across 2020–2025 the moved share did not converge toward full avoidance in either group, even though both corporate and individual deals aggressively pulled closings forward into June 2019 before the tax took effect. A flat response across jumps of $5,000 to $37,500 gives little support to a small fixed cost of rewriting the price when adjustment opportunities are comparable. Those opportunities can differ across property tiers and offset higher tax stakes, so the pattern does not identify a single cost distribution. Corporate governance or entity-pricing norms, routine broker pricing, and buyer inattention may each leave a round price unchanged, and administrative deed records cannot tell those channels apart among the remaining individual-to-individual stayers.

My evidence comes from 434,108 recorded residential sales above $300,000 in New York City from 2016 to 2025, one row per inferred 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. 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 (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.

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, Weber, and Shan, 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 percent 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.

Relation to the literature. The notch evidence sits with Kopczuk and Munroe (2015), who study the New York and New Jersey transfer-tax notches 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. Dowd and McClelland (2019) adapt the method to the one-year holding threshold for capital gains and find that realizations are delayed until the lower rate applies, a real but modest response. 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, Pope, and Sydnor (2015) show that negotiated house prices cluster at round numbers, which is the margin on which I find the notches act. In labor markets, Dube, Manning, and Naidu (2025) similarly find that round-number wage bunching can reflect firm misoptimization blunted by market power rather than counterparty left-digit bias, suggesting why pre-reform housing prices can sit tightly on round benchmarks when bilateral surpluses vary slowly near a match price. Bunching identifies a response only under assumptions about the counterfactual distribution near the threshold (Blomquist et al., 2021; Bertanha, McCallum, and Seegert, 2023). The same prices before the reform, together with round prices the reform left untaxed, give that assumption a more direct check. Work on tax salience finds that consumers respond less to taxes that are less visible (Chetty, Looney, and Kroft, 2009; DellaVigna, 2009; Taubinsky and Rees-Jones, 2018; Gabaix, 2019), which is one reading of the round-price sales that do not move. On recurring liabilities, Koster and Pinchbeck (2022) estimate from English property taxes that homebuyers capitalize future tax streams close to prevailing market discount rates of 3 to 4 percent, whereas in German pension discontinuities Seibold (2021) finds statutory thresholds themselves act as reference points whose effects exceed pure financial incentives sevenfold. Across policy reforms, Gelber, Jones, and Sacks (2020) show how adjustment frictions attenuate cross-sectional bunching relative to underlying responsiveness, while Bergstrom, Dodds, and Rios (2025) derive nonparametric welfare bounds for notch reforms

directly from observed mass shifts. Slemrod, Weber, and Shan (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 IV.D returns to. Dachis, Duranton, and Turner (2012) find that Toronto's 1.1 percent land transfer tax reduced sales of single-family homes by 15 percent and lowered prices by about the amount of the tax, and Besley, Meads, and Surico (2014) that about 60 percent of the UK's 2008–09 stamp duty holiday went to buyers, while Ke et al. (2025) find that the 2020–21 holiday raised both prices and volumes, with about 80 percent of the surplus going 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 likely 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 II sets out the tax schedules and Section III the data. Section IV presents my evidence on recorded prices and what it says about frictions. Section V turns to the loan notch of the mortgage recording tax. Section VI states what my evidence cannot settle, and Section VII 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.

II The tax schedules

A The transfer-tax thresholds

The mansion tax under Tax Law §1402-a imposes 1 percent on residential conveyances of $1,000,000 or more, and it is a notch: 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, and 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 percent for consideration of $2,000,000 or more, 0.50 percent from $3,000,000, 1.25 percent from $5,000,000, 2.25 percent from $10,000,000, 2.50 percent from $15,000,000, 2.75 percent from $20,000,000 and 2.90 percent 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 percent to 0.65 percent 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 percent to 1.425 percent 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).

My identification comes from the 2019 reform, which 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.

B 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 percent on mortgages below $500,000 and 2.175 percent at or above, on the whole loan. The special additional tax of 0.25 percent is payable by the lender for such properties under §253(1-a), so the borrower's rate is 1.80 percent below $500,000 and 1.925 percent at or above, which makes 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.

III Data

Everything I use is public and free.

A 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. I group rows sharing a sale date, borough, tax block and price, keeping one row per group. This proxy for a deed keeps sales on different blocks apart but can merge distinct same-price unit sales within a block. Section IV.F and Supplemental Appendix Section J quantify those collisions and compare results using every sales row. The grouping 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.

B Loans

New York City's Automated City Register Information System (ACRIS) records deeds and mortgages in Manhattan, the Bronx, Brooklyn and Queens, while 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). A match does not establish purchase purpose, and a missing match does not establish a cash purchase. The result is a loan amount for 174,732 financed house and condominium purchases in the four boroughs: 125,023 houses and 49,709 condominiums.

IV Recorded prices at the 2019 notches

A 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 I drop April to December 2019 (33,109 sales) as a transition window in the main before–after comparisons and examine that window directly in Section IV.D. The pre-period runs January 2016 to March 2019 (142,352 sales) and the post-period January 2020 to December 2025 (258,647 sales). I compare each threshold X with itself before the reform, with the always-taxed $1,000,000 threshold, and with never-taxed round prices. I treat any price point whose estimation window contains no statutory threshold as a placebo. I use four measures, 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

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

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 ± 10 percent windows hold no threshold. The model is

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

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. I compute intervals with a block bootstrap over calendar months, and 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

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

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 IV.F). 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, and a notch predicts a fall in $\log R$.

B The round price

Round numbers attract transactions (Pope, Pope, and Sydnor, 2015). Evidence from the car market also shows responses to leading digits (Lacetera, Pope, and Sydnor, 2012). 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. I count this directly in Table 1. 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, while 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.

Here, "moved" sales are inferred from aggregate changes in price-zone counts; the records do not follow individual deals. Table 2 reports those changes, 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 percent above. About seven in ten of the sales missing above the threshold were at the round price itself, against about half at $3,000,000 and a third at $5,000,000. The share of window sales recorded at exactly the threshold fell by 61 percent at $2,000,000, 64 percent at $3,000,000 and 61 percent at $5,000,000, with 95 percent intervals of 55–67 percent, 55–72 percent and 42–73 percent. Equality of the three is not rejected ($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 percent on average, so against them the moved shares are slightly larger, 63 percent to 66 percent. 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, while the always-taxed $1,000,000 threshold also gained sales just below it, 138 a year, partly (48 a year) from the 5 percent above. Its tax did not change, and the next subsection nets that drift out.

C Netting out the control and never-taxed round prices

I estimate Equation (2) in Table 3. Pooled over the three new thresholds, sales at exactly the threshold fall by 64 percent 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 percent, and sales in the 5 percent above the threshold, excluding the round price, fall by 17 percent (95 percent interval 11 percent to 23 percent), or by 12 percent on every row of the sales file. The last line is the response beyond the round price. By threshold it is -10 percent at $2,000,000, with an interval from -19 percent to $+2$ percent, and -22 percent and -28 percent 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 percent and the change above it only -9 percent, and against the placebos alone the figures are -64 percent and -18 percent. 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. Because those mix prices of different roundness, I also compare with the five that are multiples of $500,000, the roundest never-taxed prices available. Against the control and these five placebos, the changes are -65 percent at the round price and -14 percent above it, with an interval from -21 percent to -6 percent (Supplemental Appendix Table A18, Panel B). Treating each of the five in turn as if it had been taxed gives at most -13 percent at the round price and -4 percent above it. With the post-period restricted to 2022–2025 the changes are -64 percent and -18 percent. That sample also leaves out 2020 and 2021, the first two years in which the control's own ratio dipped (Section IV.E). In sales without a company seller the changes are -67 percent and -25 percent.

I run two checks on 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 percent and $+68$ percent; no placebo falls by more than 11 percent, against -64 percent 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 $+5$ percent, -14 percent and $+10$ percent in the three pre-reform years, fall by 43 percent in 2019, as grandfathered contracts gave way to new ones, and stay between 60 percent and 69 percent below the

pre-reform mean in every year from 2020. The response above the round price is -6 percent in 2020 and between -12 percent and -24 percent from 2021, so it built up after the round-price response, as it would if a smaller share of those transactions adjusted at first.

D What the round-price margin says about frictions

Recording a lower price may require noticing the notch, reopening a contract or pricing approval, and giving up a focal price (Kleven and Waseem, 2013). Three predictions follow. First, among transactions with comparable adjustment opportunities, the moved share should rise with the tax saving when more sales can overcome adjustment costs, and remain flat otherwise. Second, sales above the threshold within the dominated range $D = T/(1 + t)$ should move no more often than round-price sales if reopening a non-focal price costs at least as much. Third, beyond that range, the response should be larger where the tax jump is larger.

Table 4, Panel A, gives moved shares of 0.61, 0.64 and 0.61, 0.62 on average (95 percent interval 0.55 to 0.68). Equality is not rejected ($p = 0.86$), although the jump rises 7.5-fold, from \$5,000 to \$37,500. Supplemental Appendix Table A19, Panel C, reports about 0.40 of the loans leaving the mortgage recording tax's \$500,000 notch, with its \$625 jump (0.32 to 0.43, depending on the benchmark). At the \$1,000,000 notch, in place since 1989 with a \$10,000 jump, the share is 0.99. These comparisons mix stakes, age and market differences and do not identify adjustment costs.

Panel B shows little change over time: the moved share is 0.60 in 2022–2025, after contracts signed before the April 2019 grandfathering date have closed. The response is between 60 percent and 69 percent in every year from 2020 (Figure 2); on every row of the sales file, shares are 0.58, 0.60 and 0.62. It is lower with a company seller, 0.40 against 0.67 without one (difference 28 points, 95 percent interval 9 to 62). Rather than reflecting rigid developer offering plans—which, as Table 8 shows next, rarely price units at exact millions to begin with—this seller gap reflects broader corporate and LLC transaction structure on both sides of resales. Financed buyers record one-dollar-below prices 1.2 to 2.9 times as often as cash buyers at taxed thresholds (Supplemental

Appendix Table A2). That ratio is also 1.7 at untaxed $500,000, which suggests that lender underwriting or rounding norms outside the notch contribute to the gap alongside broker coordination and buyer-lender negotiation before closing. Administrative deed records cannot distinguish inattention from prices fixed before anyone compared them with the schedule.

Table 8 and Figure 3 (Panel A) probe the flat 61–64 percent moved share across seller and buyer entity status, property type, financing, and composition standardization (full counts, intervals, and contrasts in Supplemental Appendix Tables A27 and A28). Three empirical patterns sharpen the mechanism. First, corporate counterparty status lowers responsiveness on *both* sides of the deed, not only for developer sellers. Averaged across $2,000,000, $3,000,000 and $5,000,000, individual buyers move 65.9 percent of exact-round sales against 43.9 percent for company buyers (difference 21.9 percentage points, 95 percent interval 3.4 to 47.7, $p = 0.052$), closely mirroring the 27.8-point seller gap (67.4 versus 39.6 percent, $p = 0.045$). In the two-by-two split, individual-to-individual deeds average 69.8 percent (68.8, 72.4 and 68.2 percent; equality $p = 0.90$) against 50.6 percent when either party is a company (difference 19.2 points, $p = 0.048$), while company-to-company deeds move only 23.2 percent at $2,000,000 and 29.5 percent at $3,000,000 (too few pre-reform exact-$5,000,000 sales, 3, allow a precise $5,000,000 rate). Among the 467 post-reform deeds still recorded at exact $2,000,000, $3,000,000 or $5,000,000, 200—61.7 percent of the 324 linked deeds—involve at least one company party, whereas newly built condominiums (at most three years old) carry almost no pre-reform exact-round spike (0.8 to 1.4 percent; only 12 post-reform exact-round deeds across all three thresholds). Offering-plan price lists thus keep new-development units off exact round integers to begin with, while ongoing entity ownership and entity purchases account for most linked stayers. Second, property-type and financing contrasts are directionally positive but imprecise: houses (67.9 percent) and co-operatives (68.5 percent) average 11–12 points above condominiums (56.7 percent; $p = 0.14$), and financed purchases average 65.2 percent against 52.3 percent for cash purchases (difference 12.9 points, 95 percent interval -7.3 to $+31.2$, $p = 0.16$; among individual sellers, 72.6 versus 60.6 percent, $p = 0.20$). Third, higher price tiers

shift markedly toward lower-response segments: from $2,000,000 to $5,000,000, condominiums rise from 44.5 to 60.3 percent of post-reform window sales, company sellers from 42.4 to 56.2 percent of linked deeds, and cash purchases from 37.8 to 53.3 percent of observed financing records. Holding composition fixed at the $2,000,000 pre-reform mix tilts the $5,000,000-minus-$2,000,000 difference mildly upward, from 0.0 points raw to $+7.3$ points by property type ($p = 0.32$, equality $p = 0.58$) and $+8.6$ points by seller–buyer entity class ($p = 0.26$, equality $p = 0.38$), and houses ($+17.8$ points, $p = 0.068$; individual-seller houses $+21.2$ points, $p = 0.056$) and co-operatives ($+17.2$ points, $p = 0.18$) point upward. Even after composition standardization, however, equality across the 7.5-fold tax jump is never rejected, and roughly 30 percent of individual-to-individual round-price deals still close at the exact threshold.

Two dynamic tests sharpen this interpretation further (Figure 3, Panels B and C; full estimates in Supplemental Appendix Tables A29 and A30). First, examining the excluded April–December 2019 transition window directly uncovers sharp intertemporal forestalling right before the 1 July 2019 effective date, mirroring the timing surge documented by Best and Kleven (2018). Comparing monthly deed volumes at or above $2,000,000 against unaffected $1,000,000–$1,999,999 sales and 2016–2018 same-calendar-month baselines, affected volume jumped $+83.4$ percent in June 2019 (95 percent interval $+62.7$ to $+106.7$ percent, $+378.8$ excess deeds, $p < 10^{-22}$; $+83.7$ percent, $t = +4.33$ in a 48-month calendar-month fixed-effects regression, and $+81.8$ percent against an interior $1,050,000–$1,950,000 control). The June surge scaled monotonically with the rate jump—$+51.3$ percent at $2,000,000–$2,999,999, $+85.7$ percent at $3,000,000–$4,999,999, and $+136.2$ percent at $5,000,000 or more—and concentrated in the final two weeks of June ($+133.9$ percent in June 16–30; $+196.5$ percent in June 24–30), before reversing immediately in July (-47.3 percent overall and -63.9 percent at $5,000,000 or more) and July–September (-28.2 percent, -331.2 deeds). Over April–September 2019 combined, net affected volume was virtually unchanged ($+1.9$ percent, 95 percent interval -3.7 to $+8.0$ percent, $p = 0.51$), confirming pure retiming. Crucially, corporate counterparties accelerated closings just as vigorously as individuals in June 2019 ($+70.6$ percent for deeds with

a company party versus $+76.3$ percent for individual-to-individual deeds, both $p < 10^{-6}$), indicating that lower corporate round-price shifting after the reform is not simple unawareness of the 2019 tax. Across the transition quarters, the share of window sales priced just below the threshold ($[X - \$5{,}000, X)$) stayed flat at 1.64 percent in April–June 2019 (pre-reform 1.65 percent), jumped nearly fourfold to 6.20 percent in July–September as the tax took effect while grandfathered contracts closed, and by October–December the exact-round moved share reached 66.0 percent (95 percent interval 38.7 to 85.9 percent), matching its 2020–2025 steady state. Second, splitting post-reform moved shares across 2020–2021, 2022–2023, and 2024–2025 shows no convergence toward complete avoidance over six years (Figure 3, Panel B; Supplemental Appendix Table A30). On the three-notch average, individual-to-individual deals stay flat at 70.4, 66.3, and 73.0 percent (2024–2025 minus 2020–2021 difference $+2.6$ percentage points, $p = 0.78$; three-period equality $p = 0.86$), while deals with any company party stay lower at 61.3, 45.4, and 47.2 percent (difference -14.1 points, $p = 0.19$; equality $p = 0.30$), leaving the entity gap intact or wider ($+9.1$, $+20.9$, and $+25.8$ points across the three periods; trend $p = 0.26$). Pooling the three threshold windows for higher precision yields a modest, statistically imprecise upward drift for individual-to-individual sales (64.5 to 69.1 to 74.9 percent; $+10.4$ points, $p = 0.13$) alongside a decline for company-party deeds (57.9 to 54.6 to 42.9 percent; -15.0 points, $p = 0.066$). Even five to six years after enactment, roughly a quarter to three-tenths of individual-to-individual round-price deals and over half of corporate-party deals remain at the exact threshold, pointing to persistent bargaining, institutional, or attention frictions rather than a gradual initial adjustment lag.

The second prediction holds on average: the share of sales in the dominated range that disappeared is 0.40, below 0.62 at the round price. The difference, 22 points, has an interval from 1 to 79 points; estimates by threshold, 0.47, 0.55 and 0.17, are imprecise. For the third prediction, D covers 5 percent, 10 percent and 14 percent of the zone $(X, 1.05X)$ in Table 3. Responses there are -10 percent, -22 percent and -28 percent for jumps of \$5,000, \$15,000 and \$37,500. With three thresholds, how the response scales is not identified (Section IV.F). Meanwhile 40 percent

of round-price sales still pay between $5,000 and $37,500 more tax for one extra dollar of price.

E Bunching across the schedule

The bunching estimator puts the response on a common scale at every threshold. I report it in Supplemental Appendix Table A20 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, while 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 percent of price below each threshold against the 1 percent above, gives the same reading. 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 (Supplemental Appendix Figure A1). 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, and 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 IV.F shows that the shortfall from proportionality is the round-price margin.

Inference against never-taxed prices. Supplemental Appendix Table A21 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. There are sixteen such prices, from $1,300,000 to $1,650,000 and from $3,800,000 to $4,150,000. The placebo windows hold more sales than the

treated ones, so the main comparison resamples each larger placebo window to a treated threshold's own sample size. Against those draws the \$2,000,000 and \$3,000,000 changes lie 3.5 and 4.6 standard deviations from the 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 percent of draws as large. The shortfall from proportionality is not unusual against the same draws: 11 percent of placebo contrasts are at least as extreme (12 percent when the two placebo windows must not overlap), and 37 percent against a wider set of 41 thinner placebos up to \$8,300,000. Without resampling, the sixteen placebo changes average $+0.003$, with a standard deviation of 0.046 and a maximum of $+0.072$, and each change at the new thresholds is larger than all sixteen, as each log change in R is lower. Because neighboring windows share sales and sixteen placebos give a smallest possible rank of $1/17$, the rank $(1+k)/17$ is a reference comparison rather than a test with known size. 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 percent 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, and bunching lies above it in five. At \$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, a dip that 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.

F Beyond the round price, and other robustness

In Table 5 I re-estimate 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 $+0.143$ [$+0.019$, $+0.265$] 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, since 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 I read the evidence as showing that the response beyond the round price is larger at the larger jumps; three thresholds cannot say how it scales. The shortfall 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, and much less 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. The \$5,000,000 estimate beyond the round price thus falls by almost half and one placebo matches it, and the difference-in-differences above the round price is -12 percent instead of -17 percent, with an interval from -22 percent to -1 percent. The round-price margin is insensitive to this choice. On every row the moved shares are 58 percent, 60 percent and 62 percent (Supplemental Appendix Table A19, Panel B). The rule was checked

against the deed records (Supplemental Appendix Table A15). Within 10 percent 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 percent above $3,000,000 and $5,000,000 after the 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 ±$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 percent before the reform to 11.8 percent 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 I use only differences. Differencing assumes the bias is the same in both periods, and the placebos, whose own round-number spikes also changed, are the check on that assumption.

G 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. I split the bunching change by each in Supplemental Appendix Table A23, 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 they are $+0.225$, $+0.355$ 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 ($+0.091$), and 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.

Corporate entities—including LLCs, investment vehicles, and condominium sponsors whose pre-April 2019 contracts were grandfathered—face different approval and pricing conventions than individual sellers. The linked deed shows whether a company sold. In sales without a company seller the changes are $+0.342$, $+0.381$ and $+0.437$, each larger than every placebo in that sample; the control changes by $+0.019$. In company-seller sales the response is clear at \$3,000,000 ($+0.430$) and is matched by 4 placebos at \$2,000,000 ($+0.074$) and 1 at \$5,000,000 ($+0.207$; 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 percent 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 $+0.228$, $+0.387$ and $+0.291$.

H 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 (Supplemental Appendix Table A24). 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 total tax 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.

Related evidence from income taxation shows that targeting a refund-maximizing point can make standard bunching elasticities misleading (Mortenson and Whitten, 2020). Here the reform changes the tax treatment of prices that were already focal.

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, 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 percent have a loan equal to 50 percent, 55 percent, …, or 95 percent of $2,000,000, the same as before the reform (8 percent of 37), while 43 percent have a loan equal to one of those fractions of the recorded price instead. At exactly $2,000,000 the two coincide, and 71 percent of loans are such fractions. At $3,000,000 the shares are 7 percent and 43 percent of 156. Many moved sales were therefore priced below the threshold by the time the loan was sized. That timing argues against a last-minute trim after underwriting, though the evidence on bargaining remains indirect. Because lenders size loans to the signed contract price, buyer counteroffers or early broker coordination could produce the same alignment, much as brokerage commission conflicts shape matching speed and deal probability without large direct price effects in Barwick, Pathak, and Wong (2017), and part of the reduction may reflect consideration shifted off the deed into closing credits or personal property. Montalvo, Piolatto, and Raya (2020) propose comparing declared prices with appraisals, which are unavailable 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 6), 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 percent 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 percent of the $296 million a year that the reform's residential schedule implies on post-reform sales (Table 7). These are static counts of sales that moved below a threshold, and they leave out any sales that did not take place.

I 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, I compute in Table 7 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 percent 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 percent of these sales would pay more. The statutory steps scaled into marginal rates, from 0.48 percent above $2,000,000 to 6.11 percent 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 percent 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 IV.D 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.

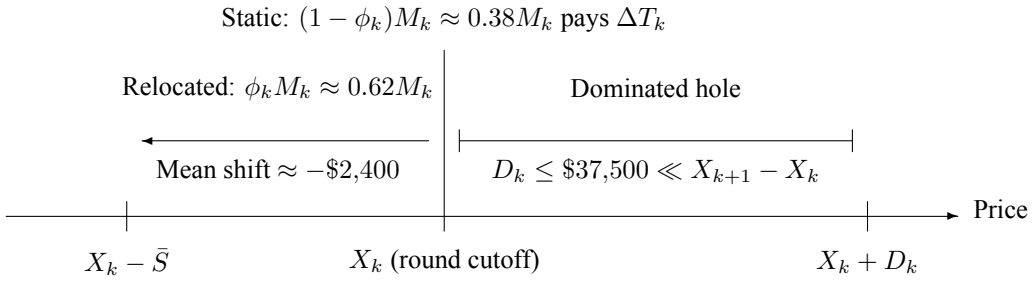
J Optimal notch spacing under round-number shifting

Why do closing-price drops of $2,200 to $3,000 coexist with tax jumps up to $37,500, and what does that combination imply for notch design relative to Kopczuk and Munroe (2015) and Best and Kleven (2018)? Consider transactions drawn from a smooth latent density $h_0(p)$ near a cutoff X_k, augmented by a pre-reform point mass $M_k > 0$ at the focal round price X_k. Discrete notches $\{X_k\}_{k=1}^{K}$ raising tax liability at X_k by $\Delta T_k = \Delta\tau_k X_k$ create dominated price intervals $(X_k, X_k + D_k)$ above each cutoff (Kopczuk and Munroe, 2015; Best and Kleven, 2018). With

round-price mass sitting directly on the cutoff, excess mass B_k below X_k combines two margins:

$$B_k \approx \phi_k(\Delta T_k)\, M_k + h_0(X_k)\, \Delta p_k(\Delta T_k), \tag{4}$$

where ϕ_k denotes the moved round-price share and Δp_k summarizes continuous adjustment off the interior. Suppose a round-price match can relocate by $s_i \in [\$1, \bar{S}]$ (averaging about \$2,400) at a bargaining or relabeling cost c_i, shifting whenever $\Delta T_k - s_i > c_i$. Equation (4) illustrates why standard notch statistics tend to overstate continuous elasticities: dividing total B_k by smooth density $h_0(X_k)$ attributes relocated round mass $\phi_k M_k$ to the continuum, inflating implied bunching widths. That mechanism matches why dropping the \$5,000 strip below \$2,000,000 shrinks the pile from \$16,900 to \$5,200, and why raw estimates near \$1,000,000 approach Kopczuk and Munroe (2015)'s unraveling region when the round mass stays in. Meanwhile, a nearly flat moved share $\phi_k \in [0.61, 0.64]$ across jumps from \$5,000 to \$37,500 points to a bimodal cost distribution in which roughly three-fifths of deals clear modest coordination costs once attentive while two-fifths may remain bound by entity-pricing norms, underwriting, or inattention unless unobserved deal traits offset higher stakes, echoing how adjustment frictions attenuate cross-sectional bunching in Gelber, Jones, and Sacks (2020).



Notes: Conceptual decomposition at a focal round cutoff X_k. Approximately three-fifths of pre-reform round matches $\phi_k M_k$ relocate just below X_k by an average of \$2,400, whereas two-fifths remain at X_k paying ΔT_k. Because realized shifts ($\approx \$2{,}400$) and dominated regions ($D_k \leq \$37{,}500$) stay narrow compared with million-dollar bracket gaps ($X_{k+1} - X_k \geq \$1{,}000{,}000$), adjacent notches remain decoupled.

Relative to a smooth slice schedule raising equivalent revenue, placing a discrete notch at a

round integer X_k generates three distinct welfare-loss terms:

$$\mathcal{L}_k(X_k, \Delta T_k) \approx \underbrace{h_0(X_k) \int_{X_k}^{X_k + D_k} \psi(p)\, dp}_{\text{dominated-region distortion}} + \underbrace{\phi_k M_k \left(\bar{c}_k + \bar{s}_k^{\text{real}}\right)}_{\text{round-price shifting cost}} + \underbrace{(1-\phi_k) M_k\, \Delta T_k\, \mu_k}_{\text{horizontal penalty on fixed matches}}, \quad (5)$$

where $\psi(p) \geq 0$ captures efficiency loss per distorted continuum trade, $\bar{c}_k$ denotes mean coordination or relabeling cost among shifters, $\bar{s}_k^{\text{real}}$ allows for real price or financing-limit concessions beyond pure relabeling, and $\mu_k \geq 0$ weights horizontal inequity or liquidity strain borne by non-adjusting round matches $(1-\phi_k)M_k$ paying ΔT_k for one extra dollar of price. Equations (4) and (5) suggest two design lessons beyond Kopczuk and Munroe (2015) and Best and Kleven (2018). First, setting discrete jumps on focal round integers maximizes both the horizontal penalty $(1-\phi_k)M_k \Delta T_k \mu_k$ and relabeling or bargaining effort on $\phi_k M_k$. Shifting cutoffs slightly off round numbers, or adopting slice schedules (Section IV.I), decouples M_k from ΔT_k, although non-round cutoffs could still invite errors under coarse heuristics (Chetty, Looney, and Kroft, 2009). Rather than trusting parametric utility calibrations to map bunching into welfare (Kleven, 2016), observed mass shifts around reforms can also bound discrete notch welfare under revealed preference (Bergstrom, Dodds, and Rios, 2025), while shallow bilateral surpluses around focal prices (Dube, Manning, and Naidu, 2025) help explain why pre-reform round matches can relocate by a few thousand dollars at low bargaining cost. Second, because realized shifts ($\approx \$2{,}400$) and dominated intervals ($D_k \leq \$37{,}500$) stay narrow relative to New York's million-dollar bracket gaps ($X_{k+1} - X_k \geq \$1{,}000{,}000$), adjacent holes do not overlap here as they can in denser schedules (Best and Kleven, 2018). Widening notch spacing while leaving jumps on round numbers would likely change little compared with moving to slice schedules or non-round cutoffs.

K An untaxed round price: New Jersey, 2004

New Jersey introduced a buyer fee of 1 percent of the whole consideration on 1 August 2004. The initial rule referred to residential zoning, and an amendment effective 1 February 2005 specified

eligible property classes, including Class 2 residential property (State of New Jersey, 2004; 2005). Both rules placed the additional fee above $1,000,000. A covered, non-exempt transfer at exactly $1,000,000 therefore owes no additional 1 percent fee, while the charge just above it is about $10,000. Kopczuk and Munroe (2015, pp. 221–222) describe the threshold as starting at $1,000,000; the Division of Taxation's buyer affidavit, which county recording officers collect with the deed, likewise applies the fee only to consideration in excess of $1,000,000 (New Jersey Division of Taxation, 2004). The rolls do not record fees paid. I use the MOD IV assessment rolls distributed by Rutgers (Rutgers University, Bloustein School and New Jersey Agricultural Experiment Station, 2026). Each annual roll gives every parcel's most recent deed date and price, so reading the eleven rolls of 2000–2010 for all 21 counties and keeping each distinct deed recovers most sales, including parcels sold more than once. Class 2 sales of at least $100,000 number 722,309 with deeds dated 1999–2003 and 667,783 dated August 2004 to July 2009. A parcel sold twice between two rolls still appears once.

Within 10 percent of the threshold, 431 of 4,512 sales were recorded at exactly $1,000,000 before the fee and 97 of 11,566 after, so the share fell from 9.6 percent to 0.8 percent. The moved share, defined as in Table 2, is 0.91 (bootstrap 95 percent interval 0.89 to 0.93), and 0.91 after dividing by the average change at six never-taxed round prices between $800,000 and $1,500,000. It is 0.92 among sales the assessor coded as usable for ratio studies, and 0.91 in both the ten northern counties and the other eleven. The same comparison one period earlier, 1994–1998 against 1999–2003, gives -0.05 (95 percent interval -0.32 to 0.14). Because the rolls begin in 2000, the 1994–1998 comparison contains only deeds still recorded as the latest sale in those later rolls; it is a selected sample. Sales at $999,999 went from 4 to 341, sales at $999,000 from 13 to 446, and sales at $995,000 from 91 to 434. The share of window sales in $(\$1{,}000{,}000, \$1{,}050{,}000]$ fell from 14.7 percent to 7.9 percent, and Figure 5 shows the change year by year.

Starting in February 2005 gives a 91.6 percent decline (sales-bootstrap 95 percent interval 89.6 percent to 93.5 percent). Fixing the county mix at preperiod weights gives 91.2 percent; omitting each county in turn gives 90.6 percent to 92.2 percent. The central 95 percent ranges

from three- and six-month circular block resampling are 88.1 percent to 94.4 percent and 88.2 percent to 94.5 percent, respectively (2,999 draws each), and assessor-usable sales give 92.9 percent (90.6 percent to 94.9 percent). Assessment class does not verify zoning at the date of sale. The public MOD IV rolls omit buyer and seller names, so corporate status cannot be tested directly, but they do identify condominium units and tax-escrow bank codes. The exact-million collapse is pervasive across observable segments: 97.3 percent (95 percent interval 95.0 to 99.0 percent) among condominiums and 89.7 percent (87.1 to 91.9 percent) among non-condominium residences, and 91.9 percent (88.3 to 94.9 percent) on parcels carrying a tax-escrow bank code against 90.9 percent (88.2 to 93.1 percent) without one (Supplemental Appendix Table A28, Panel B).

Because the additional fee is zero at exactly $1,000,000, leaving that price is not avoidance of the fee as written. Two distinct mechanisms fit the decline. One is statutory misperception: brokers, attorneys, or buyers may treat a "$1 million mansion tax" as starting *at* $1,000,000 rather than strictly *above* it, a salient boundary framing that parallels evidence from German pension discontinuities where statutory thresholds shift behavior beyond financial incentives (Seibold, 2021). The second is closing-adjustment or title-software caution: even parties who know the exact statutory rule may set consideration at $999,999, $999,000, or $995,000 as a safety margin so that settlement adjustments, closing credits, or title-company software checks cannot inadvertently push recorded consideration above $1,000,000 and trigger a $10,000 charge. Because the public assessment rolls lack fee-payment receipts and closing statements, the data cannot separate statutory misperception from precautionary pricing below the line.

V 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 percent to 1.925 percent 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 174,732 matched mortgages with an observed amount, 1,503 are for exactly $500,000 and 1,287 lie in the $1,000 below it. At untaxed round amounts from $300,000 to $700,000, between 79 percent and 95 percent of loans in the last $1,000 up to and including the round amount sit at exactly that amount, against 54 percent at $500,000. Relative to loans in the $25,000 below, loans just under $500,000 are more common than loans just under the other round amounts (0.175 against 0.018 to 0.062). Fitted against a smooth counterfactual in the manner of DeFusco and Paciorek (2017), the excess below the threshold is 0.47 (95 percent interval 0.42 to 0.53), equivalent to $5,900 of loan and a recorded-loan elasticity of 0.059 (0.047 to 0.073), with widths of $5,200 to $6,900 across 27 specifications. These widths describe recorded amounts and

can overstate the change in borrowing for the reason Section IV.H gives for prices.

Two other explanations fit less well. 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, and 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 0.52 and 0.98 in every year, wherever the baseline limit stood. And only 5 percent of the loans just below $500,000 finance a price within $1,000 below a transfer-tax threshold, against 2 percent 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, plausibly by borrowing slightly less or shifting financing outside the recorded mortgage, two channels that deed and loan records cannot tell apart. One untaxed amount, $550,000, also shows some loans just below it, which could reflect unrecorded lender pricing rules or conventional cutoffs.

Applying the borrower-side schedule to recorded loans, with charges imputed where loan amounts are unavailable, gives about $248 million a year on purchases between $300,000 and $6,000,000 in the four ACRIS boroughs. A purchase without a recorded mortgage pays none of this charge. 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.

VI What these data cannot settle

How the moved deals were reached. I observe recorded prices, not the bargaining behind them. The loan check of Section IV.H points against a trim made after the loan was arranged, though the test remains indirect: early broker coordination or buyer counteroffers could leave both the contract price and loan below the threshold, and some value may still have shifted off the deed into personal property or credits, leaving open whether a buyer, a seller, or both absorbed the

difference.

Why the other 40 percent do not move. The round-price sales that still close at the threshold could reflect buyers or brokers who never compared the price with the schedule, or prices constrained by corporate approval or bargaining conventions. Table 8 and Figure 3 show that corporate involvement on *either* side of the deed accounts for over three-fifths of linked post-reform stayers, whereas newly built condominiums rarely price at exact round integers even before the reform; moreover, corporate deals accelerated closings just as sharply as individual deals in June 2019 ($+70.6$ versus $+76.3$ percent), ruling out simple unawareness of the tax as the sole corporate explanation. Yet roughly 25 to 30 percent of individual-to-individual round-price deals still close at the threshold five to six years after enactment (70.4, 66.3, and 73.0 percent moved across 2020–2021, 2022–2023, and 2024–2025; $p = 0.78$), so administrative records cannot separate persistent inattention from bargaining or institutional constraints.

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 only three thresholds I cannot estimate how it scales with the jump.

Local responses, so no optimal rate. The widths and elasticities of Supplemental Appendix Table A24 describe responses at discrete jumps, dominated by the round-price margin and by frictions. They need not carry over to a continuous schedule, and the slice schedules of Table 7 are static. A welfare analysis would also need the economic incidence of each tax, which I do 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 IV.G drops them using a companion paper's classifier (Loschi, 2026b). The estimates move slightly but retain the same pattern (Section IV.G). The main estimates do not use that classifier.

VII Conclusion

Most of the recorded price response to New York's 2019 notches occurred at a single margin: roughly 60 percent of the spike at each newly taxed round price vanished and reappeared a few thousand dollars below. That share barely varied between a $5,000 and a $37,500 jump. Splitting the moved share across transaction types (Table 8, Figure 3) shows that corporate counterparties on either side of the deed move far less than individual-to-individual deals (50.6 versus 69.8 percent) and account for over three-fifths of linked post-reform stayers, whereas newly built condominium offering plans rarely price at exact millions even before the reform, and higher corporate, condominium, and cash shares at $5,000,000 mildly dampen the raw $5,000,000 aggregate relative to $2,000,000. Dynamic tests reinforce this reading: affected sales surged $+83.4$ percent in June 2019 ($+136.2$ percent at $5,000,000 or more) across both corporate and individual counterparties before reversing in July–September (-28.2 percent; April–September net $+1.9$ percent), yet over 2020–2025 the moved share did not converge toward full avoidance in either segment (70.4 to 73.0 percent for individual-to-individual deeds, $p = 0.78$; 61.3 to 47.2 percent when a company is involved, $p = 0.19$). Even after holding composition fixed, equality across the 7.5-fold tax jump is not rejected, and roughly a quarter to three-tenths of individual-to-individual round-price deals remain at the exact threshold six years on. Under comparable adjustment opportunities, a flat, persistent response across stakes lends little support to a small fixed cost of rewriting the price as the sole friction, though registry fields alone cannot separate persistent inattention from institutional or bargaining constraints. Away from the round price, missing mass above the threshold appears only at the two larger jumps, where volume in the 5 percent above fell 22 percent at $3,000,000 and 28 percent at $5,000,000, with no clear

change at $2,000,000.

For tax design the notch has a cost that a revenue table does not show. Sales that move avoid $1.6 to $2.9 million a year, under 1 percent of the $296 million the schedule implies on recorded prices, so the fiscal leakage is small. The roughly 40 percent of round-price deals that do not move each pay between $5,000 and $37,500 for one dollar of price, a payment with no counterpart under a slice schedule. A revenue-neutral slice schedule that scales the statutory steps into marginal rates would lower the average tax in every band below $10,000,000. That calculation holds recorded prices fixed and ignores volume responses of the kind Green et al. (2026) find in Los Angeles, as well as the choice between owning and renting. In a search model calibrated to Toronto's 2008 land transfer tax, Han, Ngai, and Sheedy (2026) put the welfare loss at 111 percent of the extra revenue, and incidence is beyond what my design can estimate.

For measurement, these patterns argue for judging bunching at a round-number notch against a pre-reform period at the same price or with the focal price modeled directly. Dropping sales at the threshold and one dollar below gauges sensitivity to that focal point without recovering a structural response. Where standard smooth counterfactuals turn a $2,400 shift below $2,000,000 into a pile $16,900 wide, separating the round-price margin materially changes how wide the behavioral distortion appears, and loans at the recording tax's $500,000 notch show the same spike-clearing pattern. New Jersey's loss of its exact-million spike despite a statutory exemption—pervasive across condominiums, houses, and escrow statuses—suggests public assessment rolls alone cannot distinguish statutory misperception from closing-adjustment or title-software caution below the cutoff.

Acknowledgments and Disclaimers

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 dataset version 1.2.0 (Loschi, 2026a), version DOI `10.5281/zenodo.23557585`. This release uses the unchanged empirical inputs, code and outputs archived at `10.5281/zenodo.23761331`, in `The_Shape_of_the_Tax_v3_6_source_and_checks.zip`, plus the mechanism-split and dynamic scripts (`scripts/mechanism_split_v3_11.py`, `scripts/nj_modiv_split_v3_11.py`, `scripts/dynamics_forestalling_v3_12.py`, `scripts/make_fig_mechanism_dynamics_v3_12.py`). The New Jersey analysis uses the MOD IV historical assessment rolls distributed by Rutgers (Rutgers University, Bloustein School and New Jersey Agricultural Experiment Station, 2026); its extraction scripts, saved estimates, and scope checks are in `second_notch` (`nj_extract_sales.py`, `nj_mansion_tax_statewide.py`, `check_2005_scope_sensitivity.py`, `nj_bounded_checks.py`). Earlier code is under concept DOI `10.5281/zenodo.22880953` and preprints under `10.5281/zenodo.22925301`.

Declaration of generative AI and AI-assisted technologies. I designed the study, specified all empirical and robustness designs, verified all estimation and consistency checks against public administrative records and deposited outputs, and take full responsibility for all empirical design, interpretation, citations, and text. No AI document reading is used as evidence anywhere in the paper. Generative AI coding and editing tools (OpenAI ChatGPT/Codex, Anthropic Claude, and Google Gemini) were used solely for script assistance, formatting, and copy-editing.

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Disclosures

The Shape of the Tax: Round Prices and Bunching at New York City's 2019 Transfer-Tax Notches

Pablo Loschi

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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

Note. One row per inferred deed. Pre: January 2016–March 2019; post: January 2020–December 2025. Local density uses sales $5,000–$25,000 from the threshold on either side, excluding multiples of $10,000. Annual counts divide by 3.25 and 6 years. Full definitions and source notes: Supplemental Appendix Table A16.

Table 2: Where the moved sales came from

	Change in sales a year, by recorded price				Mean distance	Sales at
Threshold	$[0.95X,$ $X-5{,}000)$	$[X-5{,}000,$ $X)$	Exactly X	$(X,$ $1.05X)$	below X of added sales	X that moved
\$2,000,000	$+33$	$+89$	-78	-31	\$2,446	61%
\$3,000,000	$+19$	$+42$	-37	-35	\$2,214	64%
\$5,000,000	$+10$	$+23$	-12	-23	\$2,985	61%
\$1,000,000 (control)	-29	$+138$	-3	-48	\$2,644	—
\$1,500,000 (never taxed)	$+15$	-8	$+8$	-8	—	—
\$2,500,000 (never taxed)	-17	-2	$+7$	-11	—	—

Note. One row per inferred deed. Changes in price-zone shares are multiplied by post-reform annual window sales. These are aggregate shifts, not tracked negotiations. Distance is the mean price difference among additional sales in the \$10,000 below the threshold. Full uncertainty intervals and definitions appear in Supplemental Appendix Table A17.

Table 3: Bin-Count Difference-in-Differences (Percentage Changes)

	Pooled, $2M, $3M, $5M		By threshold		
Price zone	One row per deed	Every row	$2,000,000	$3,000,000	$5,000,000
$[0.95X, X - 5{,}000)$	$+9$	$+11$	$+10$	$+7$	$+9$
	$[+2, +16]$	$[+3, +20]$	$[+1, +21]$	$[-2, +19]$	$[-7, +30]$
$[X - 5{,}000, X)$	$+257$	$+249$	$+248$	$+202$	$+625$
	$[+197,$ $+337]$	$[+185,$ $+323]$	$[+175,$ $+345]$	$[+133,$ $+303]$	$[+340,$ $+1922]$
Exactly X	-64	-60	-63	-66	-63
	$[-68, -58]$	$[-66, -54]$	$[-68, -56]$	$[-73, -58]$	$[-75, -49]$
$(X, 1.05X)$	-17	-12	-10	-22	-28
	$[-23, -11]$	$[-22, -1]$	$[-19, +2]$	$[-32, -10]$	$[-38, -15]$

Note. Entries are percentage changes, with 95 percent month-block-bootstrap intervals in brackets. Poisson zone-count regressions compare the three reformed thresholds with the unchanged $1 million threshold and 24 never-taxed prices, net of changes in the outer zones. One row per inferred deed except Column 2. Additional comparisons and full notes: Supplemental Appendix Table A18.

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

Panel A. The 2019 thresholds, one row per deed

	Dominated	Sales at exactly X that moved		Sales in $(X, X + D]$ that disappeared	
Threshold	range D	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]	

Note. Percentages are one minus the post-to-pre ratio of window shares. The dominated range $D = T/(1 + t)$ is a frictionless price-adjustment benchmark, not a test of individual awareness. Intervals are from 1,000 bootstrap draws. The two raw averages differ by 22 percentage points, with interval 1 to 79. Other samples, benchmarks and full definitions: Supplemental Appendix Table A19.

Table 5: The response with and without the round price

Panel A. Change in bunching, post − pre (placebos at least as large, of 16)				
	\$1,000,000 (control)	\$2,000,000	\$3,000,000	\$5,000,000
All sales (Supplemental Appendix Table A20)	$+0.059$ (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)
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)
Without sales at X and $X-1$:				
Net of the control	—	-0.015 [-0.086, $+0.060$]	$+0.143$ [$+0.019$, $+0.265$]	$+0.123$ [-0.032, $+0.283$]

Note. Bunching estimates use the same periods, windows and sixteen placebos as Supplemental Appendix Table A20. Each screen is applied in both periods to treated and placebo prices alike. Parentheses count placebos at least as large. Bootstrap intervals are in the full table. Additional screens, log-ratio results and tests across thresholds: Supplemental Appendix Table A22.

Table 6: 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]

Note. Annual excess mass below the threshold, January 2020–December 2025, minus the pre-reform excess rescaled to post-reform density. Intervals are conditional on the estimator. Tax avoided assumes each additional sale crossed the threshold; the $3 million jump includes buyer and seller taxes. Alternative counts and full notes: Supplemental Appendix Table A25.

Table 7: The 2019 schedule and two revenue-neutral slice schedules

	2019 notches (whole price)	Slice A (one marginal rate)	Slice B (scaled steps)
Panel A. Tax added by the 2019 reform at a recorded price of			
$1,999,999	0.00	0.00	0.00
$2,000,000	5.00	0.00	0.00
$3,000,000	22.50	23.61	4.85
$5,000,000	75.00	70.82	33.95
$10,000,000	250.00	188.86	179.45
$20,000,000	600.00	424.93	688.68
Panel B. Totals			
Revenue, $ million a year	296	296	296
Sales paying more than under the notches	—	77%	7%

Note. Panel A tax amounts are in thousands of dollars, rounded to two decimals; recorded prices are in dollars. Post-reform prices are held fixed. Slice A charges 2.36 percent above $2 million. Slice B scales the statutory increments into marginal rates, preserving the same schedule-implied revenue. It has no whole-price jumps. Band-level changes and exact amounts: Supplemental Appendix Table A26. These are static calculations, not forecasts.

Table 8: Moved shares at exact $2,000,000, $3,000,000 and $5,000,000 by transaction type

Subgroup or standardization	$2M	$3M	$5M	3-notch avg.	$5M − $2M	Eq. p
All sales (baseline)	61.1%	63.9%	61.2%	62.1%	+0.0	0.86
Panel A. Seller and buyer entity status (linked deeds)						
Individual seller	64.9%	70.4%	66.8%	67.4%	+2.0	0.73
Company seller	40.5%	48.6%	29.6%	39.6%	−10.8	0.84
Individual buyer	66.1%	70.2%	61.2%	65.9%	−4.9	0.77
Company buyer	48.3%	47.0%	36.5%	43.9%	−11.8	0.91
Individual to individual	68.8%	72.4%	68.2%	69.8%	−0.6	0.90
Any company party	53.7%	54.0%	44.2%	50.6%	−9.5	0.86
Panel B. Property type and financing						
1–3 family houses	62.7%	60.5%	80.5%	67.9%	+17.8	0.18
Condominiums	64.8%	63.2%	42.1%	56.7%	−22.7	0.46
Co-operatives	58.2%	71.7%	75.5%	68.5%	+17.2	0.25
Financed (ACRIS)	62.4%	71.9%	61.2%	65.2%	−1.2	0.47
Cash (ACRIS)	60.5%	48.9%	47.4%	52.3%	−13.1	0.52
Panel C. Direct standardization to $2,000,000 pre-reform mix						
By property type	62.1%	65.2%	69.4%	65.6%	+7.3	0.58
By seller–buyer entity class	58.8%	64.8%	67.5%	63.7%	+8.6	0.38
By financing × seller (ACRIS)	60.3%	65.7%	62.9%	63.0%	+2.6	0.70

Note. One row per inferred deed (434,108 sales; pre: January 2016–March 2019; post: January 2020–December 2025). Entries are moved shares $m(X) = 1 - s_{X,\text{post}}/s_{X,\text{pre}}$ within $[0.9X, 1.1X)$, the simple three-threshold average, the $5M-minus-$2M difference in percentage points, and the Wald $\chi^2(2)$ equality p-value from 2,000 joint window bootstrap draws. Panel C reweights subgroup exact-round shares at each threshold to the $2,000,000 pre-reform window weights. Full counts, 95 percent intervals, pairwise contrasts, and New Jersey MOD IV splits appear in Supplemental Appendix Tables A27 and A28. Source: `scripts/mechanism_split_v3_11.py`.

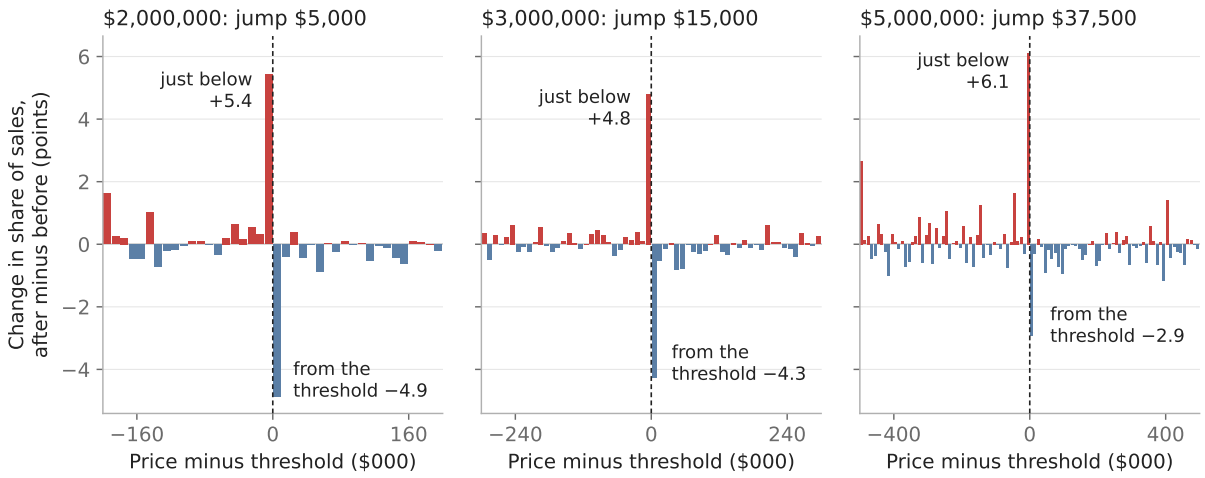

Figure 1: Recorded prices moved to just below the new thresholds. Each bar is the change in a $10,000 price bin's share of sales within 10% of the threshold, from January 2016–March 2019 to 2020–2025, one row per deed. The bin just below the threshold gains 5.4, 4.8 and 6.1 percentage points at $2,000,000, $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`.

Alt text. Three price-bin charts compare recorded-price shares before and after the reform. At each newly taxed threshold, the bin just below gains sales while the bin beginning at the threshold loses sales.

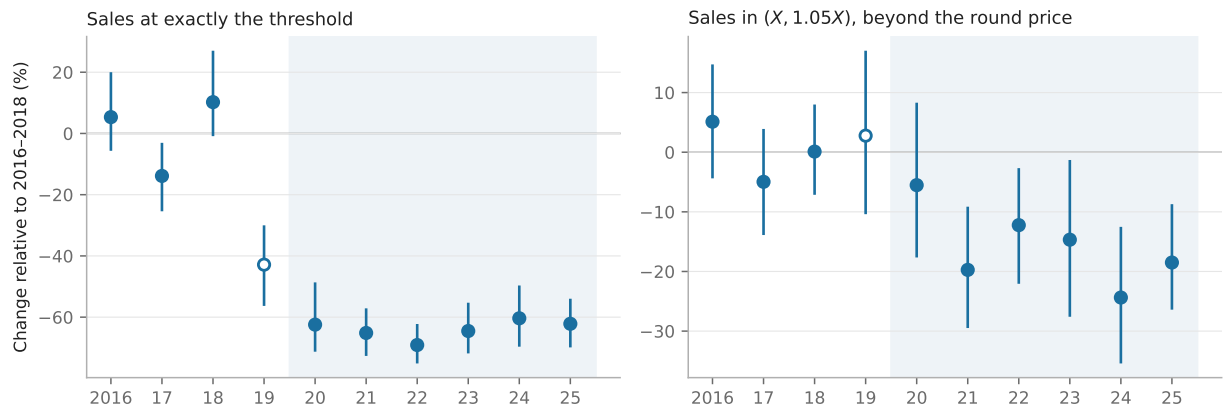

Figure 2: The round price emptied with the reform, and the response beyond it built up after. Percentage change in sales at exactly the threshold (left) and in $(X, 1.05X)$ excluding X (right) at the three new thresholds, relative to the 2016–2018 mean and net of the same change at the always-taxed $1,000,000 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`.

Alt text. Two event-study panels show a sharp decline in sales at the exact threshold during 2019 and a later, smaller decline in the zone above it. Intervals show sampling uncertainty.

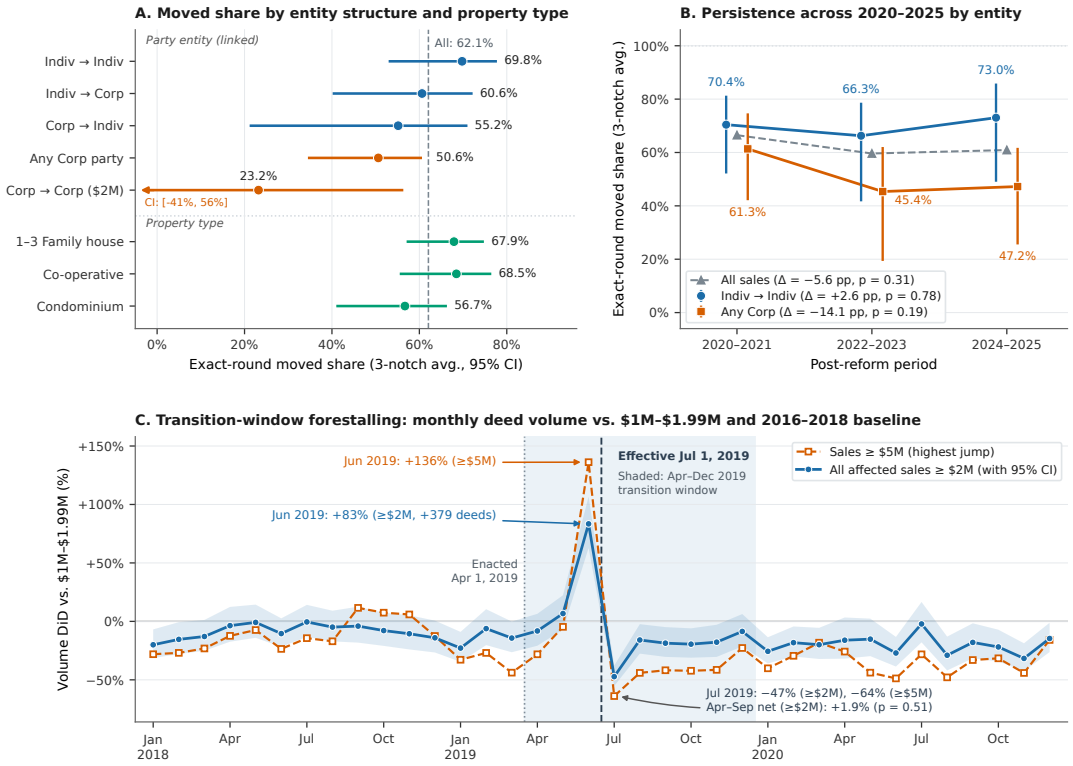

Figure 3: Corporate counterparties move less, stayers persist through 2025, and closings surged right before 1 July 2019. Panel A plots three-threshold average moved shares ($m(X) = 1 - s_{X,\text{post}}/s_{X,\text{pre}}$ across \$2M, \$3M, and \$5M, 95% bootstrap intervals; company-to-company at \$2M). Panel B splits moved shares across 2020–2021, 2022–2023, and 2024–2025 by entity status. Panel C plots monthly volume differences-in-differences for ≥\$2M and ≥\$5M sales against \$1M–\$1.99M controls and 2016–2018 same-month baselines. Sources: `mechanism_split_v3_11.py`, `dynamics_forestalling_v3_12.py`.

Alt text. Three-panel chart showing lower moved shares for corporate deals than individual resales (A), flat moved shares across 2020–2025 (B), and a June 2019 closing surge followed by a July–September trough (C).

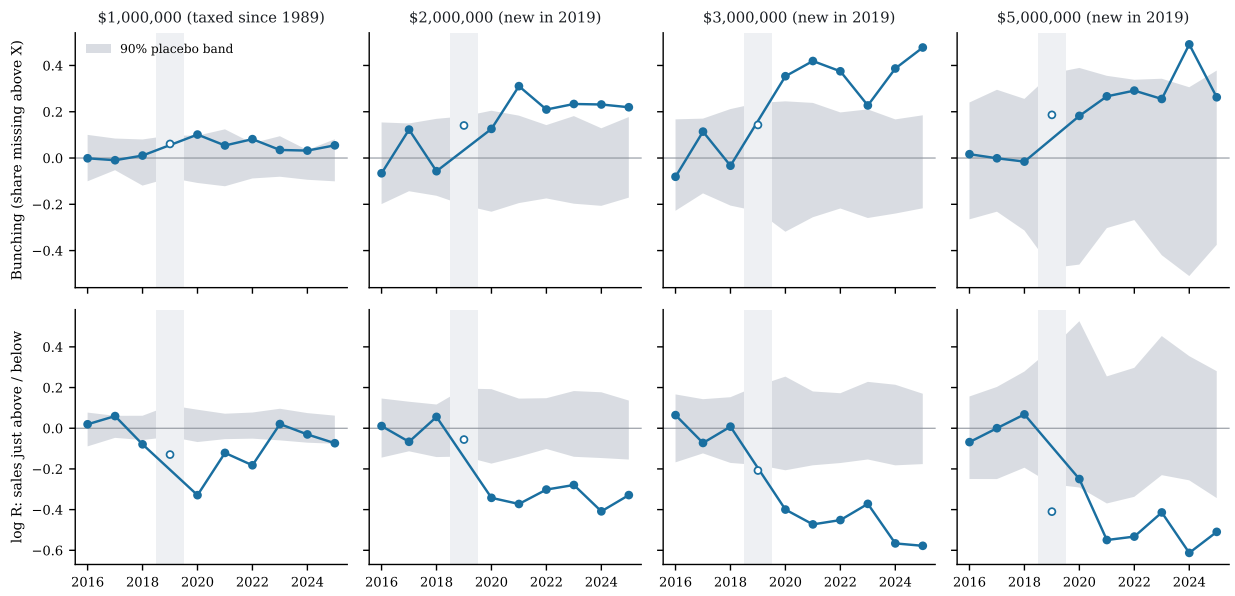

Figure 4: The response starts with the reform. Bunching (top) and log R (bottom) by year, each normalized to its 2016–2018 mean, at the always-treated $1,000,000 threshold and the three thresholds created in 2019. Gray bands are the 90% range of the same statistic at never-taxed placebos resampled with 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`.

Alt text. Eight small panels plot annual bunching and log-ratio estimates around four thresholds, with placebo ranges for comparison. The new thresholds respond around the reform; the older control also varies over time.

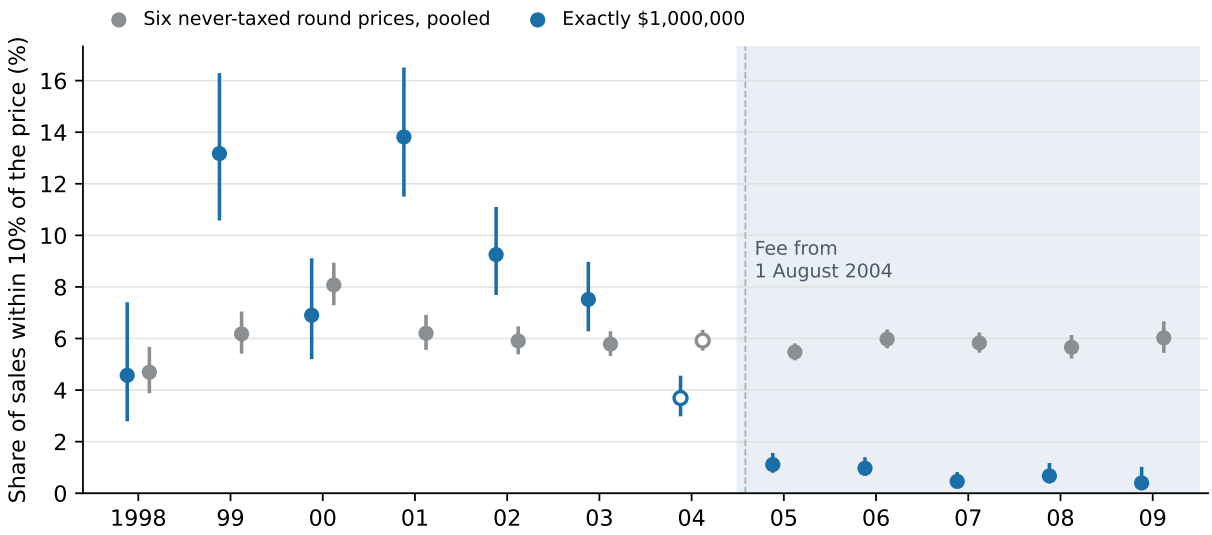

Figure 5: In New Jersey the untaxed round price emptied when the fee began. Share of Class 2 sales within 10% of $1,000,000 recorded at exactly that price, by deed year (blue), and the same share at six never-taxed round prices between $800,000 and $1,500,000, each in its own window and pooled (gray). Wilson 95% intervals. The fee applies to consideration in excess of $1,000,000 from 1 August 2004, so a sale at exactly $1,000,000 owes no additional 1% fee; 2004 is drawn hollow because it mixes both regimes. The share at $1,000,000 averaged 9.6% in 1999–2003 and was 1.1% or less in every year from 2005, while the never-taxed round prices stayed near 6%. Deeds from late 2009 are not yet in the last roll used. MOD IV rolls 2000–2010, all 21 counties; `second_notch/make_nj_figure.py`.

Alt text. The exact-million share in New Jersey falls after the fee begins in 2004, while the pooled never-taxed round-price share changes much less. Error bars show Wilson intervals.

Nontechnical Summary

In 2019 New York State introduced higher transfer taxes at several prices for homes in New York City. The rates apply to the whole price once a threshold is reached. A sale at $2 million therefore incurs $5,000 more tax than one at $1,999,999. Such thresholds are called notches.

Using 434,108 residential sales from 2016 to 2025, the paper compares newly taxed round prices with their own earlier distributions, the unchanged $1 million threshold and never-taxed round prices.

The share recorded at exactly $2 million, $3 million and $5 million fell by 61 to 64 percent, with extra sales a few thousand dollars below. These are distributional changes; the records do not follow individual negotiations. Declines were similar across tax jumps of $5,000 to $37,500. Sales farther above the threshold declined at $3 million and $5 million, but the $2 million response was indistinguishable from zero.

Standard methods compare sales below a threshold with a smooth curve. At $2 million, they represent the additional sales as a price range $16,900 wide, although the observed excess sits about $2,400 below the threshold. Much of the response reflects movement away from an already popular price.

New Jersey offers a further comparison. Its buyer fee applied above $1 million, leaving a sale at exactly that amount outside the additional charge. The exact-million share nevertheless fell substantially. The finding survives starting the comparison after the February 2005 eligibility amendment. The assessment rolls do not show actual fee payments or what the parties understood, so the explanation remains unresolved.

Avoided tax at the new New York notches is estimated at $1.6 million to $2.9 million a year, under one percent of the roughly $296 million implied by the reform's schedule. Illustrative continuous schedules raise the same amount on observed sales without jumps. These are static calculations. They cannot establish revenue after buyers and sellers respond, who ultimately bears the tax, or an optimal tax rate.