The Shape of the Tax

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

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October 2026

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 recorded at exactly a new threshold fell 61 to 64 percent, with similar estimates across jump sizes, and extra sales appear $2,200 to $3,000 below, less than the tax saved. At $2 million, standard bunching estimates read these moves as a pile $16,900 wide. Under New Jersey's 2004 fee, which as written exempts exactly $1 million, that share still fell 91 percent. Avoided tax is under 1 percent of revenue.

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. 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. The assessment rolls do not show the fee paid or the parties' understanding of the rule, so the comparison cannot establish its mechanism (Section IV.J).

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 lower when a company sells, as developers do at prices set in advance, and it did not rise between 2020 and 2025. A flat response across jumps of $5,000 to $37,500 is hard to square with a cost of rewriting the price, which is small next to either jump. It suggests instead that prices were committed before anyone compared them with the schedule, or that nobody checked the schedule, and with these data I cannot tell the two apart.

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. 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. 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 as a transition window. The pre-period runs January 2016 to March 2019 (142,352 sales) and the post-period January 2020 to December 2025 (258,647 sales). 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

Some sales that would have closed at exactly a threshold now close just below it, and others still close at it and pay the jump on the whole price. Which ones move says something about the frictions that keep sales where a notch makes them costly (Kleven and Waseem, 2013). Consider sales that would close near X without the tax. Some would be agreed at exactly the round price X, and the rest have prices spread smoothly around it. A jump of T dollars in tax on the whole price at X gives every sale at or just above X a reason to record a lower price. Suppose that recording a price below X costs the parties to a sale an amount F, which varies across sales with distribution G. F stands for everything that comes between a sale and a lower recorded price: noticing the notch in time, reopening a price already agreed or written into a contract or an offering plan, and settling on a figure other than a focal one. A sale agreed at exactly X can move by giving up little, and the added sales sit about $2,400 below the threshold on average (Table 2). It moves when F is below the tax saved, so the share of round-price sales that move is about $m(T) = G(T)$.

Three predictions follow. First, m rises with T over the range where G has mass and is flat where it has none. Second, a sale agreed at a price δ above X but within the dominated range $D = T/(1+t)$, which is dominated in the frictionless price-adjustment benchmark, has to reopen a price that was not focal and change it by more. If that costs at least as much, the share of such sales that disappear is at most m. Third, beyond the dominated range a sale moves only if the tax saved exceeds the value of the price cut to the parties, as in the frictionless notch model of Kleven and Waseem (2013), so the response there should be larger where T is larger.

Table 4 sets out the evidence. In Panel A the round-price share is 0.61, 0.64 and 0.61 at the three new thresholds, 0.62 on average (95 percent interval 0.55 to 0.68), and equality is not rejected ($p = 0.86$), although the total jump rises 7.5-fold, from $5,000 to $37,500. Supplemental Appendix Table A19, Panel C, adds the other two notches in the data, as cross-sectional comparisons. At the mortgage recording tax's $500,000 loan notch, where the jump is $625, about 0.40 of the loans that would have been written at exactly $500,000 are written just below it (0.32 to 0.43, depending on which untaxed round amount is the benchmark). At the $1,000,000 mansion-tax notch, in place since 1989 with a $10,000 jump, there is no spike at the threshold at all, and the share is 0.99. Read through G, and treating G as the same across these markets, which is an assumption, about 40 percent of round-price transactions face costs below $625, about 60 percent face costs below $5,000, and the rest face costs above $37,500 in each of the first six years of the new notches, far more than the cost of writing a different number on a contract. The near-complete response at the $1,000,000 notch in Supplemental Appendix Table A19, Panel C, shows that G is not in fact common across these markets, so these shares are illustrative, not estimated cost quantiles. The share that moves rises with small stakes, does not change between $5,000 and $37,500, and is close to one at a notch that has been in place for thirty years. The last comparison mixes the notch's age with everything else that differs between the $1,000,000 market and the markets above $2,000,000, and cannot separate them.

Supplemental Appendix Table A19, Panel B, shows where the sales that stay are found, and the share does not rise over the post-reform years. Restricting the post-period to 2022–2025, when contracts signed before the April 2019 grandfathering date have long since closed, gives 0.60, and the round-price response in Figure 2 is between 60 percent and 69 percent in every year from 2020. On every row of the sales file the shares are 0.58, 0.60 and 0.62. It is lower in sales with a company seller, 0.40 against 0.67 without one (difference 28 points, 95 percent interval 9 to 62). Company sellers include developers, who set prices in offering plans before units are sold, and a price fixed in advance is one form of a high F. Financed buyers also record a price one dollar below the threshold 1.2 to 2.9 times as often as cash buyers at the taxed thresholds

(Supplemental Appendix Table A2), as they would if a lender, an appraiser or a closing attorney makes the notch harder to miss, although the ratio is also 1.7 at $500,000, where the price carries no jump, so part of the gap is not specific to the notch.

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

The second prediction holds on average: the share of sales in the dominated range that disappeared is 0.40, below the round-price share of 0.62, and the difference, 22 points, has an interval from 1 to 79 points. The estimates by threshold, 0.47, 0.55 and 0.17, are imprecise because the dominated range is narrow and holds few sales, and they do not rise with the jump. They are too imprecise to say how the cost of reopening a non-focal price scales, and no cost is inferred from them. The third prediction concerns the zone $(X, 1.05X)$ of Table 3, which lies mostly beyond the dominated range: D covers 5 percent, 10 percent and 14 percent of its width at the three thresholds. There the response is -10 percent, -22 percent and -28 percent for jumps of $5,000, $15,000 and $37,500, larger where the jump is larger. With three thresholds, how it scales is not identified (Section IV.F). Meanwhile 40 percent of the round-price sales still close at exactly the threshold and pay between $5,000 and $37,500 more tax for one extra dollar of price.

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 3 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.

Developers set condominium prices in advance, and some sponsor sales that closed after the reform were contracted before the April 2019 cut-off. The linked deed shows whether a company sold, a proxy that includes sponsor sales. In sales without a company seller the changes are $+0.342$, $+0.381$ 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.

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 is evidence against a trim made after the loan was arranged, though weak evidence, since loans are commonly sized on the final contract price, and it says nothing about consideration moved off the deed during negotiation. Montalvo, Piolatto, and Raya (2020) propose comparing declared prices with appraisals, which are not in the public record.

What the notches cost. A sale that moves below a notch avoids at least the jump in tax. With the bunching estimator, about 129 extra sales a year sit just below $2,000,000, 45 below $3,000,000 and 30 below $5,000,000 (Table 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 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; whether county offices collected the fee at exactly that price is not known. 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 4 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.

Because the additional fee is zero at exactly $1,000,000, leaving that price is not avoidance of the fee as written. The pattern fits a salient round-number boundary, but the rolls record neither fee payments nor what the parties understood, so the mechanism remains open. In the replication package, `second_notch/nj_extract_sales.py` builds the sales file, `second_notch/nj_mansion_tax_statewide.py` reproduces the estimates above, and `second_notch/check_2005_scope_sensitivity.py` supplies the eligibility-period checks. County and monthly-block checks are in `second_notch/nj_bounded_checks.py`.

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, by borrowing a little less or moving financing outside the recorded mortgage, which the records cannot tell apart. One untaxed amount, $550,000, also shows some loans just below it, for reasons not known.

The tax itself is large: applied to each observed loan, the borrower-side schedule implies about $248 million a year on purchases between $300,000 and $6,000,000 in the four ACRIS boroughs, and a purchase without a recorded mortgage pays none of it. Supplemental Appendix Section I documents the calculation, compares it with the Department of Finance's reported liability (New York City Department of Finance, 2025) and costs continuous schedules that would raise the same amount from every purchase.

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 is weak evidence against a trim made after the loan was arranged, but some value may still have moved off the deed into personal property or credits during negotiation, and I cannot say whether a buyer, a seller or both gave up the difference.

Why the other 40 percent do not move. The round-price sales that still close at the threshold could be parties who never compared the price with the schedule, or prices committed before anyone did, as in a developer's offering plan. The lower share when a company sells is consistent with the second, but the records cannot separate the two.

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, and the companion paper describes its agreement with AI-assisted document readings.

VII Conclusion

I find that the 2019 notches changed recorded prices mainly at one margin: the share of sales recorded at exactly a new threshold fell by about 60 percent, and extra sales appeared a few thousand dollars below it. The decline was similar whether the jump was $5,000 or $37,500. A response that does not grow with the stakes is hard to reconcile with a fixed cost of rewriting a price, which is trivial next to these jumps. The records cannot tell whether the sales that stay reflect inattention to the schedule or prices committed before anyone compared them with it. The lower response when a company sells is consistent with commitments such as sponsors' offering

plans, although seller type does not identify that channel. One-dollar-below prices are also more frequent among financed buyers, but the records do not show who checked the schedule. Beyond the round price the response is smaller and appears only at the two larger jumps. Sales in the 5 percent above the threshold fell by 22 percent at $3,000,000 and 28 percent at $5,000,000, and not detectably 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, my results suggest that bunching at a round-number notch should be read against a before period at the same price or with the round price modeled directly. New Jersey also loses the exact-million spike despite the statutory exemption; the records leave the payment and attention mechanisms unresolved. Against a smooth counterfactual, sales that moved about $2,400 below $2,000,000 register as a pile $16,900 wide, and loans at the recording tax's $500,000 notch show the same pattern.

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`. The New York analysis inputs, code and outputs accompany the preprint from version 3.4, `10.5281/zenodo.23666456`. 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 the accompanying `second_notch` directory; reconstructing the original sales extract requires those historical rolls. The county and monthly-count sensitivity checks can be rerun from their deposited inputs. The earlier replication archive is under concept DOI `10.5281/zenodo.22880953`; it holds earlier versions of the code. Preprint versions are under concept DOI `10.5281/zenodo.22925301`.

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

Funding. This research received no external funding.

Disclosures

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

Pablo Loschi

The author founded HolaCasaClara, a free comparator of Argentine mortgage loans, which has no connection to New York housing records or this study. This research received no external funding. The author reports no other financial arrangements or competing interests with respect to this research.

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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.

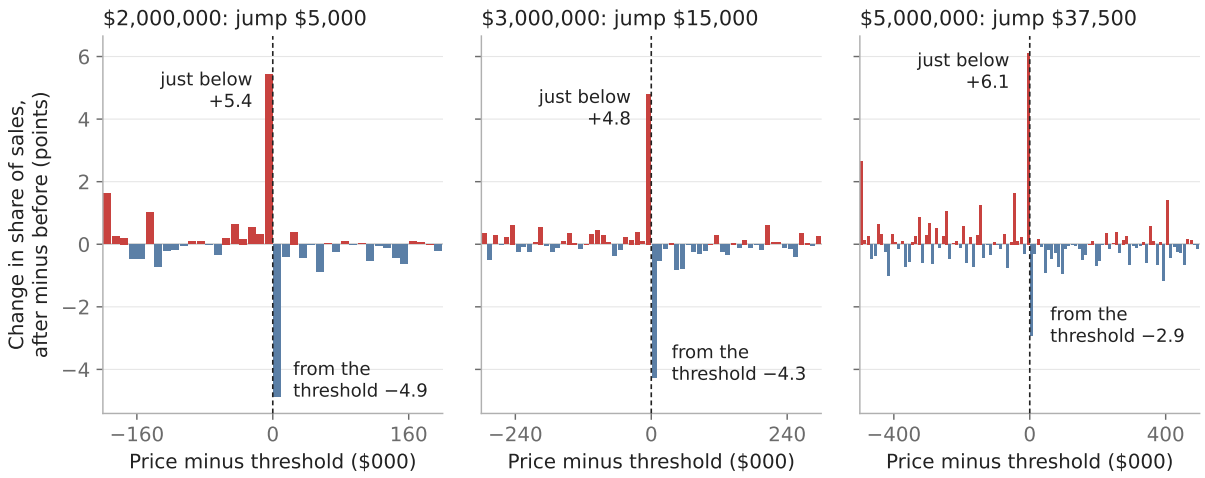


Figure 1: Recorded prices moved to just below the new thresholds. Each bar is the change in a $10,000 price bin's share of sales within 10% of the threshold, from January 2016–March 2019 to 2020–2025, one row per deed. The bin just below the threshold gains 5.4, 4.8 and 6.1 percentage points at $2,000,000,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

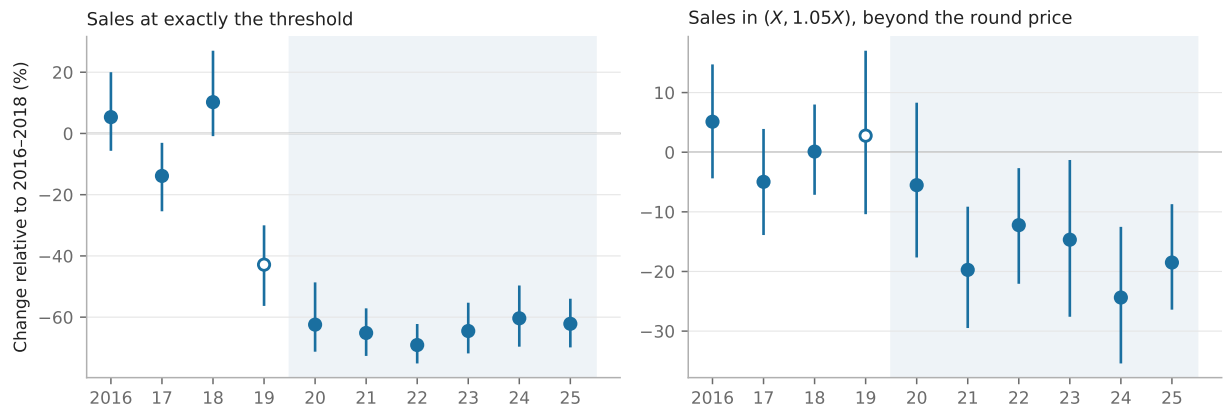


Figure 2: The round price emptied with the reform, and the response beyond it built up after. Percentage change in sales at exactly the threshold (left) and in $(X, 1.05X)$ excluding X (right) at the three new thresholds, relative to the 2016–2018 mean and net of the same change at the always-taxed $1,000,000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

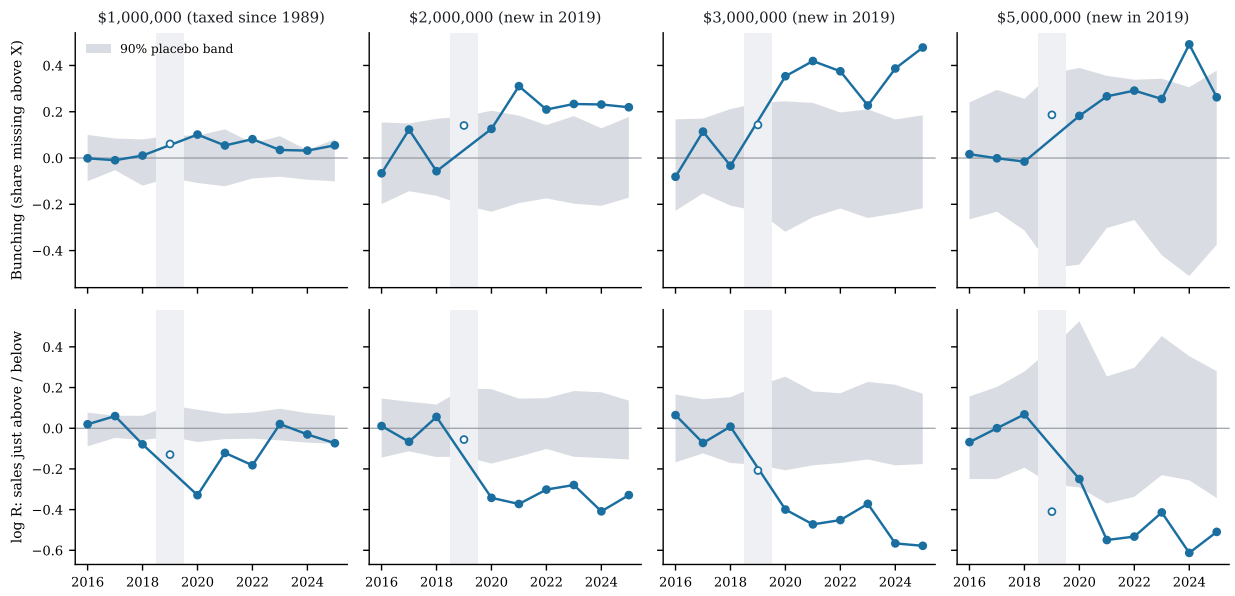


Figure 3: The response starts with the reform. Bunching (top) and log R (bottom) by year, each normalized to its 2016–2018 mean, at the always-treated $1,000,000 threshold and the three thresholds created in 2019. Gray bands are the 90% range of the same statistic at never-taxed placebos resampled with 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.

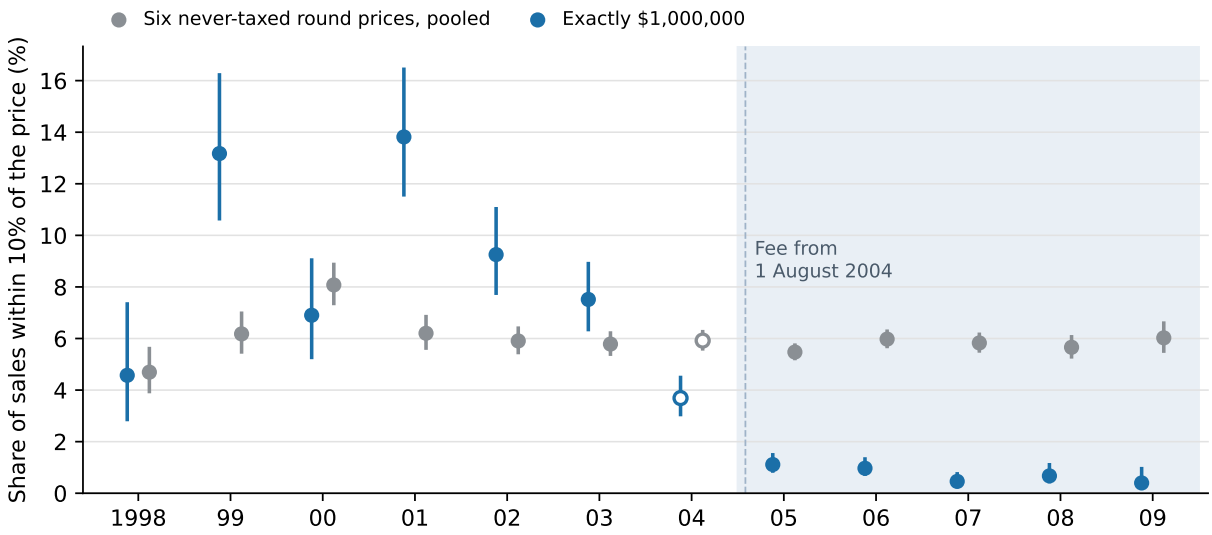


Figure 4: In New Jersey the untaxed round price emptied when the fee began. Share of Class 2 sales within 10% of $1,000,000 recorded at exactly that price, by deed year (blue), and the same share at six never-taxed round prices between $800,000 and $1,500,000, each in its own window and pooled (gray). 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.