Supplemental Appendix

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

Pablo Loschi · October 2026

This appendix holds the robustness tables the paper cites, including the thresholds above $5,000,000 (Table A3) and the split by seller (Table A8), the recording-tax calculation and the replication manifest. Table and section numbers in the paper are cited as "of the paper" throughout. Like the paper, Tables A3 and A6–A8 use one row per deed; Tables A1, A10, A11 and A12 use every row of the sales file, and Table A2 the matched four-borough panel.

A. Robustness of the differenced bunching estimates

Referenced in Section IV.F of the paper and Section F below; robustness of Table A10.

Table A1: The differenced result across period definitions

Specification	$1M (control)	$2M	$3M
Baseline	$+0.005$	$+0.220$	$+0.461$
Drop COVID, post = 2022–2025	$+0.000$	$+0.213$	$+0.514$
Clean pre-period, 2016–2018 only	$+0.007$	$+0.221$	$+0.465$
Tight window, 2017–18 vs 2020–21	$+0.014$	$+0.202$	$+0.311$
Late only, post = 2023–2025	-0.006	$+0.216$	$+0.531$

Notes. Across eight estimator variants at $2,000,000 (window ±$300,000/$400,000/$500,000, bins of $10,000 or $20,000, polynomial degree 3, 5 or 7, a wider exclusion band, and round-number controls on or off), the *difference* ranges from 0.136 to 0.233 and is never negative, while the *level* in the pre-period ranges from -0.032 to $+0.223$. Differencing absorbs most of the specification error and levels do not, which is the methodological reason for preferring the differenced design.

B. Charm pricing by buyer type

Referenced in Section G below. Reported there with its limitations, since the cells are small and the corresponding split of the bunching magnitudes is not distinguishable from specification noise.

Table A2: Charm pricing by buyer type

Threshold	Buyer-side jump	Cash buyers	Financed buyers	Ratio
$500,000	$0	4.2% (27 of 644)	**7.1%** (80 of 1,125)	1.70
$1,000,000	$10,000	70.8% (131 of 185)	**88.1%** (379 of 430)	1.24
$2,000,000	$5,000	12.1% (26 of 214)	**22.2%** (70 of 316)	1.82
$3,000,000	$7,500	10.9% (12 of 110)	**32.0%** (40 of 125)	2.93
$1,500,000 placebo	—	0.4% (2 of 462)	0.5% (5 of 921)	—
$2,500,000 placebo	—	0.0% (0 of 199)	0.0% (0 of 268)	—

Notes. Computed on the matched four-borough panel, since a financing flag is needed; counts are therefore smaller than in Table A11 and Staten Island is absent. **At every threshold, financed buyers price one dollar below it more often than cash buyers do**, by a factor of 1.2 to 2.9, while the two never-taxed placebos show nothing for either type. Pricing at $X - 1$ avoids a tax jump at the three buyer-paid thresholds, but not at $500,000. The counts are descriptive and do not identify an effect of financing. The bunching estimates do not split the same way. By buyer type they differ by about as much at four never-taxed placebo prices as at the thresholds (Section G).

C. The full 2019 schedule, who responds, and the replacement: detail

This section supports Section IV.E of the paper and Sections G and I below. The extended price sample is a fresh, count-verified retrieval of the same Department of Finance resource with the same building-class filter and no upper price limit (729,047 residential records; 442,497 above $300,000). Its $300,000–$6,000,000 slice is identical, sale for sale, to the archived 431,471-sale sample used in Tables A10 and A11. The paper and Table A3 keep one row per deed.

Table A3: Mass just below each threshold, before and after the reform

Threshold	Statute	Pre: below/above	Post: below/above	$\Delta \log S$ [95% CI]
$500,000	NYC RPTT, 1989	1367/1229	1685/1829	-0.188 $[-0.290, -0.087]$
$1,000,000	mansion 1402-a, 1989	2363/90	6460/192	$+0.248$ $[-0.007, +0.503]$
$2,000,000	1402-b, 2019	167/383	1011/408	$+1.737$ $[+1.522, +1.953]$
$3,000,000	1402-b + base, 2019	101/223	527/213	$+1.698$ $[+1.414, +1.982]$
$5,000,000	1402-b, 2019	52/76	295/64	$+1.908$ $[+1.463, +2.352]$
$10,000,000	1402-b, 2019	22/21	83/11	$+1.974$ $[+1.107, +2.842]$
$15,000,000	1402-b, 2019	7/25	24/23	$+1.316$ $[+0.301, +2.330]$
$20,000,000	1402-b, 2019	5/11	14/14	$+0.788$ $[-0.502, +2.079]$
$25,000,000	1402-b, 2019	1/4	12/12	$+1.386$ $[-0.947, +3.719]$
$1,500,000	placebo	270/653	629/1547	-0.017 $[-0.186, +0.153]$
$1,750,000	placebo	138/418	278/941	-0.111 $[-0.345, +0.123]$
$2,250,000	placebo	100/237	200/604	-0.242 $[-0.526, +0.041]$
$2,500,000	placebo	145/271	315/646	-0.093 $[-0.335, +0.150]$
$4,000,000	placebo	72/135	123/236	-0.023 $[-0.383, +0.337]$
$6,000,000	placebo	44/69	86/144	-0.066 $[-0.529, +0.397]$
$7,000,000	placebo	29/47	63/93	$+0.093$ $[-0.469, +0.656]$
$8,000,000	placebo	23/39	33/85	-0.418 $[-1.072, +0.235]$
$12,000,000	placebo	6/25	11/56	-0.200 $[-1.301, +0.900]$

S compares sales in $[0.99X, X)$ with sales in $[X, 1.01X)$, so it captures pricing a few thousand dollars below a threshold as well as one dollar below. At $5,000,000, sales at $4,995,000 rose from 7 before the reform to 77 after. One row per deed. Pre is January 2016 to March 2019, post January 2020 to December 2025; Poisson intervals. The always-treated $500,000 and $1,000,000 rows measure drift over the same years; the $500,000 row falls, but no tax on prices changed there in 2019. On every row of the sales file, without collapsing deeds (`collapsed_counts.py`), the just-below shift is $+1.68$ at $2,000,000, $+0.93$ at $10,000,000 and $+1.31$ at $15,000,000.

Table A4: Charm pricing by loan-to-value, financed house purchases

Threshold	Payer	N at $X-1$ or X	Charm share (%)	By LTV tercile, low / mid / high (%)	Median LTV
$500,000	seller	868	7.3	5.2 / 6.5 / 10.7	0.82
$1,000,000	buyer	335	88.7	87.3 / 96.6 / 81.0	0.77
$2,000,000	buyer	213	23.5	16.9 / 17.3 / 37.3	0.70
$3,000,000	buyer	79	29.1	22.2 / 37.0 / 28.0	0.67

Financed house purchases recorded at exactly $X-1$ or X with a positive loan amount and $0 < \text{LTV} \leq 1$. This table uses houses, and Table A9 adds condominium loans from the separate deposited file. Pooling the three buyer-paid thresholds with threshold and year fixed effects and parcel-clustered errors, the charm probability changes by +0.3 points per ten points of LTV (s.e. 0.9), and by -12.3 points for LTV of 0.80 or more (s.e. 3.4). At $500,000, where pricing one dollar below saves no tax (the city's higher rate applies only above $500,000), the slope is +1.0 (s.e. 0.6).

Table A5: Mean change in tax per purchase under each replacement, by price band ($)

	Flat 0.87%		1.55% above $500k		Two brackets	
Price band	Financed	Cash	Financed	Cash	Financed	Cash
$300k–500k	$-2,632$	+3,549	$-6,305$	+0	$-6,305$	+0
$500k–1M	$-4,055$	+6,491	$-6,725$	+3,778	$-7,349$	+3,170
$1M–2M	$-5,198$	+12,405	$-3,607$	+14,263	$-5,807$	+11,965
$2M–6M	$-9,889$	+28,221	+2,997	+42,299	+10,365	+51,520

New tax minus the gross schedule-implied recording charge on the purchase, averaged within band and payment method; static, statutory incidence; four boroughs (Staten Island excluded). Schedules as in Section I below.

D. Placebo inference, markets and deed screens: detail

This section supports Sections IV.E and IV.G of the paper. Table A6 lists the post-minus-pre statistics at each of the sixteen never-taxed prices behind Table A21 below, next to the four thresholds. Table A7 gives both statistics, with window sizes, for every row of Table A23 below.

Table A6: Every core placebo, with the treated thresholds for comparison

Price	Sales in window, pre/post	Bunching, post − pre	$\Delta \log R$
Never-taxed placebos			
\$1,300,000	17,449/41,222	−0.011	−0.016
\$1,350,000	17,297/40,829	+0.042	−0.032
\$1,400,000	16,683/39,442	+0.004	+0.006
\$1,450,000	16,051/38,118	−0.077	+0.072
\$1,500,000	15,624/36,973	+0.015	+0.006
\$1,550,000	14,697/35,097	+0.011	−0.021
\$1,600,000	13,815/32,900	−0.045	+0.026
\$1,650,000	12,885/30,867	−0.021	+0.030
\$3,800,000	3,563/7,453	+0.038	−0.109
\$3,850,000	3,503/7,401	+0.064	−0.122
\$3,900,000	3,414/7,211	+0.031	−0.075
\$3,950,000	3,325/7,046	+0.072	+0.002
\$4,000,000	3,321/7,063	−0.056	+0.091
\$4,050,000	3,257/6,902	+0.031	+0.142
\$4,100,000	3,176/6,703	+0.024	+0.125
\$4,150,000	3,074/6,500	−0.071	+0.163
Thresholds (\$1,000,000 always taxed; the others created in 2019)			
\$1,000,000	27,575/61,880	+0.059	−0.113
\$2,000,000	9,250/22,421	+0.223	−0.350
\$3,000,000	5,233/11,607	+0.384	−0.459
\$5,000,000	2,367/5,024	+0.287	−0.479

Estimator, windows and pre/post periods as in Table A20 below. Placebos are the \$50,000 multiples between \$1,200,000 and \$6,000,000 whose window $[0.8X, 1.2X)$ holds no statutory threshold. Placebo bunching changes: mean +0.003, standard deviation 0.046, maximum +0.072. Source: `placebo_inference.py`.

Table A7: The response by market and after deed screens, both statistics

	Pre-reform sales in window	Bunching, post − pre (placebos as large)	$\Delta \log R$ (placebos as low)	z against placebo SD
All sales (placebo SD 0.046)				
$1,000,000	27,575	+0.059 (2)	−0.113 (1)	+1.2
$2,000,000	9,250	+0.223 (0)	−0.350 (0)	+4.8
$3,000,000	5,233	+0.384 (0)	−0.459 (0)	+8.3
$5,000,000	2,367	+0.287 (0)	−0.479 (0)	+6.2
Houses (placebo SD 0.159)				
$1,000,000	15,170	+0.005 (8)	+0.068 (12)	−0.1
$2,000,000	2,642	+0.292 (2)	−0.639 (0)	+1.7
$3,000,000	1,003	+0.485 (0)	−0.611 (0)	+2.9
$5,000,000	299	+0.438 (0)	−0.849 (0)	+2.6
Condominiums (placebo SD 0.083)				
$1,000,000	7,578	+0.151 (1)	−0.235 (0)	+1.8
$2,000,000	4,258	+0.196 (0)	−0.235 (0)	+2.3
$3,000,000	2,939	+0.376 (0)	−0.439 (0)	+4.5
$5,000,000	1,571	+0.247 (0)	−0.312 (0)	+2.9
Co-operatives (placebo SD 0.173)				
$1,000,000	4,827	+0.035 (7)	−0.110 (2)	+0.2
$2,000,000	2,350	+0.192 (3)	−0.208 (2)	+1.1
$3,000,000	1,291	+0.352 (0)	−0.398 (0)	+2.1
$5,000,000	497	+0.275 (1)	−0.737 (0)	+1.6
Manhattan (placebo SD 0.060)				
$1,000,000	7,429	+0.091 (0)	−0.165 (0)	+1.4
$2,000,000	5,431	+0.225 (0)	−0.265 (0)	+3.6
$3,000,000	3,846	+0.355 (0)	−0.421 (0)	+5.8
$5,000,000	2,037	+0.220 (0)	−0.367 (0)	+3.5
Other four boroughs (placebo SD 0.076)				
$1,000,000	20,146	+0.040 (3)	−0.048 (3)	+0.6
$2,000,000	3,819	+0.212 (0)	−0.423 (0)	+2.9
$3,000,000	1,387	+0.499 (0)	−0.536 (0)	+6.7
$5,000,000	330	+0.680 (0)	−0.994 (0)	+9.1
No company seller (placebo SD 0.059)				
$1,000,000	15,813	+0.019 (7)	−0.034 (3)	+0.1
$2,000,000	4,100	+0.342 (0)	−0.592 (0)	+5.6
$3,000,000	1,980	+0.381 (0)	−0.474 (0)	+6.2
$5,000,000	669	+0.437 (0)	−0.600 (0)	+7.2
Company seller (sponsor proxy) (placebo SD 0.151)				
$1,000,000	5,105	+0.205 (1)	−0.302 (0)	+1.4
$2,000,000	2,647	+0.074 (4)	−0.146 (2)	+0.6
$3,000,000	1,873	+0.430 (0)	−0.551 (0)	+2.9
$5,000,000	1,155	+0.207 (1)	−0.322 (0)	+1.4
Without legal-rule deeds (placebo SD 0.046)				
$1,000,000	27,411	+0.055 (2)	−0.103 (2)	+1.1
$2,000,000	9,236	+0.227 (0)	−0.355 (0)	+4.9
$3,000,000	5,231	+0.389 (0)	−0.459 (0)	+8.4
$5,000,000	2,367	+0.288 (0)	−0.482 (0)	+6.2
Without any lender or referee deed (placebo SD 0.047)				
$1,000,000	27,240	+0.054 (2)	−0.102 (2)	+1.1
$2,000,000	9,220	+0.228 (0)	−0.357 (0)	+4.8
$3,000,000	5,223	+0.387 (0)	−0.457 (0)	+8.2
$5,000,000	2,366	+0.291 (0)	−0.487 (0)	+6.2

Placebo counts are out of sixteen, computed within each row's sample. Source: `market_splits.py`.

E. Sales with and without a company seller, and the loan-to-value test with condominium loans

This section supports Section IV.G of the paper and Section G below. Table A8 splits the response by whether the linked deed names a company as seller, a proxy that includes sponsor sales and other company sales, and adds condominiums in new buildings and a late post-period. Each group is normalized within its own sample, so the rows do not divide the aggregate shift between groups. Table A9 reruns the loan-to-value test of Section G with condominium loans from the linked dataset.

Table A8: Sales with and without a company seller

	Sales in window pre/post	Bunching, post − pre (placebos as large)	$\Delta \log R$ (placebos as low)	z against placebo SD
All sales, no company seller (placebo SD 0.059)				
$1,000,000	15,813/34,166	+0.019 (7)	−0.034 (3)	+0.1
$2,000,000	4,100/10,041	+0.342 (0)	−0.592 (0)	+5.6
$3,000,000	1,980/4,255	+0.381 (0)	−0.474 (0)	+6.2
$5,000,000	669/1,655	+0.437 (0)	−0.600 (0)	+7.2
All sales, company seller (placebo SD 0.151)				
$1,000,000	5,105/12,013	+0.205 (1)	−0.302 (0)	+1.4
$2,000,000	2,647/6,743	+0.074 (4)	−0.146 (2)	+0.6
$3,000,000	1,873/4,279	+0.430 (0)	−0.551 (0)	+2.9
$5,000,000	1,155/2,142	+0.207 (1)	−0.322 (0)	+1.4
Condominiums, no company seller (placebo SD 0.207)				
$1,000,000	4,319/7,803	+0.058 (6)	−0.119 (3)	+0.1
$2,000,000	2,218/4,514	+0.331 (2)	−0.411 (0)	+1.4
$3,000,000	1,281/2,435	+0.381 (1)	−0.367 (0)	+1.6
$5,000,000	464/996	+0.251 (3)	−0.345 (0)	+1.0
Condominiums, company seller (placebo SD 0.113)				
$1,000,000	3,211/7,001	+0.301 (0)	−0.355 (0)	+2.8
$2,000,000	2,005/4,986	+0.059 (4)	−0.101 (3)	+0.7
$3,000,000	1,628/3,492	+0.395 (0)	−0.554 (0)	+3.6
$5,000,000	1,086/1,878	+0.206 (1)	−0.257 (0)	+2.0
Condominiums, company seller, post-period 2022–2025 (placebo SD 0.125)				
$1,000,000	3,211/4,402	+0.288 (0)	−0.281 (0)	+2.5
$2,000,000	2,005/3,370	+0.050 (4)	−0.078 (3)	+0.6
$3,000,000	1,628/2,454	+0.418 (0)	−0.661 (0)	+3.6
$5,000,000	1,086/1,282	+0.282 (0)	−0.328 (0)	+2.5
Condominiums in buildings at most three years old (placebo SD 0.234)				
$1,000,000	1,231/2,646	+0.330 (2)	−0.197 (4)	+1.3
$2,000,000	567/1,475	−0.091 (12)	+0.048 (8)	−0.5
$3,000,000	471/801	+0.532 (1)	−0.194 (4)	+2.2
$5,000,000	309/319	+0.071 (6)	+0.027 (7)	+0.2
Condominiums in older buildings (placebo SD 0.081)				
$1,000,000	6,347/12,312	+0.130 (1)	−0.246 (0)	+1.6
$2,000,000	3,691/8,256	+0.241 (0)	−0.280 (0)	+2.9
$3,000,000	2,468/5,332	+0.345 (0)	−0.462 (0)	+4.2
$5,000,000	1,262/2,685	+0.283 (0)	−0.368 (0)	+3.5

A company seller on the linked deed is a proxy that includes sponsor (developer) sales and other company sales; sales without a linked deed are in neither group. Buildings at most three years old use the Department of Finance year built. Share of

condominium window sales with a company seller: $1,000,000, 42% before and 47% after; $2,000,000, 47% before and 51% after; $3,000,000, 55% before and 57% after; $5,000,000, 69% before and 63% after. Placebo counts are out of sixteen, computed within each row's sample. Source: `cost_and_sponsors.py`.

Table A9: Loan-to-value and the charm price, with condominium loans

Financed purchases at $X-1$ or X	N	Charm (%)	Per 10 LTV points	LTV $\geq 80\%$
Houses (as in the paper)	627	59.0	$+0.3$ (0.9)	-12.3 (3.4)
Condominiums	219	44.7	$+0.3$ (1.3)	-17.4 (6.4)
Houses and condominiums	846	55.3	$+0.3$ (0.7)	-13.2 (3.0)
Seller-paid $500,000 notch (placebo)	1,043	7.1	$+0.7$ (0.6)	-2.0 (1.7)

Linear probability of a price of $X-1$, in percentage points, with threshold and year effects (and a condominium effect where both types are pooled); standard errors clustered by parcel in parentheses. Buyer-paid thresholds $1,000,000, $2,000,000 and $3,000,000; the $500,000 row pools houses and condominiums at the seller-paid notch. Condominium loans come from `data/condo_loans.csv.gz`, matched as in the costing. Source: `cost_and_sponsors.py`.

F. The fixed-window design and the four residential notches

Referenced in Sections II.A and IV.F of the paper.

Table A10 applies a fixed ±$400,000 window with $10,000 bins and an exclusion band of −$50,000 to +$100,000. At the always-treated control the missing share is 0.508 before the reform and 0.513 after; at $2,000,000 it rises from 0.094 to 0.314 and at $3,000,000 from −0.117 to 0.344. Table A1 shows the same differences across period definitions. Table A11 counts sales one dollar below and at each of the four residential notches, including the city's $500,000 threshold, where a sale at exactly $500,000 already pays the lower rate.

Table A10: The fixed-window design: bunching at $1,000,000, $2,000,000 and $3,000,000, differenced across the 2019 reform

Panel A. Share of counterfactual mass missing above the threshold

Threshold	Status in 2019	Pre	Post	Difference	95% CI
$1,000,000	taxed throughout	0.508	0.513	**+0.005**	[−0.020, +0.034]
$2,000,000	**untaxed → taxed**	0.094	0.314	**+0.220**	[+0.133, +0.320]
$3,000,000	**untaxed → taxed**	−0.117	0.344	**+0.461**	[+0.287, +0.645]
$1,500,000	never taxed	−0.049	−0.076	−0.027	[−0.103, +0.055]
$2,500,000	never taxed	−0.080	−0.096	−0.016	[−0.148, +0.148]

Panel B. The same result with no counterfactual fitted

Threshold	R pre	R post	Log change	95% CI
$1,000,000	0.289	0.302	+0.042	[−0.011, +0.092]
$2,000,000	0.738	0.531	**−0.328**	[−0.431, −0.222]
$3,000,000	0.933	0.517	**−0.591**	[−0.764, −0.425]
$1,500,000	0.763	0.792	+0.036	[−0.031, +0.101]
$2,500,000	0.882	0.900	+0.021	[−0.097, +0.148]

Notes. Panel B defines $R(X)$ as the number of transactions in $[X, X+100{,}000)$ divided by the number in $[X-150{,}000, X-50{,}000)$. A notch at X predicts lower R, but changing density or transaction composition can also move it. The $1,000,000 row checks the design. Nothing changed at that threshold in 2019, and the estimate stays at 0.508 before and 0.513 after, on 64,066 and 134,996 transactions in the estimation window. The small fixed-window control change reduces, without eliminating, the concern that the before-and-after comparison picks up general drift in the price distribution. Same conclusion in both panels, and Panel B needs no polynomial. This table uses a fixed ±$400,000 window and $10,000 bins (bootstrap intervals, 600 draws). The paper's estimates use windows that scale with the threshold ($\pm 0.2X$); the two coincide at $2,000,000, and at $3,000,000 the scaled window gives +0.321 on every row of the sales file. Never-taxed round prices whose fixed window would contain the $2,000,000 threshold ($1,750,000 and $2,250,000) are not shown.

G. Who responds: payment method and loan-to-value

Section IV.D of the paper notes that financed buyers record a price one dollar below a threshold more often than cash buyers. This section reports the counts and a test of one explanation.

Splitting the charm-price counts of Table A11 by financing status, financed buyers price one dollar below the threshold more often than cash buyers at all four thresholds, by factors of 1.2 to 2.9, while both never-taxed placebos show nothing for either type. Table A2 reports the counts. Three caveats apply. At $500,000 the charm price avoids no tax (Table A11), so there the split measures charm pricing alone. The cells are small. The $3,000,000 comparison rests on 12 of 110 against 40 of 125, so the ordering is better

Table A11: Four residential notches, and the buyer's share of each

Panel A. Charm pricing at each threshold, all transactions 2016–2025

Threshold	Notch	In force since	Sales at $X-1$	Sales at X	**Charm share**
\$500,000 (city RPTT)†	0.425 pt	1989	249	2,911	7.9%
\$1,000,000 (mansion)	1.00 pt	1989	728	200	**78.4%**
\$2,000,000 (§1402-b)	0.25 pt	Jul 2019	140	674	**17.2%**
\$3,000,000 (§1402-b + base)	0.50 pt	Jul 2019	73	322	**18.5%**
\$1,500,000 placebo	—	—	7	1,885	0.4%
\$2,500,000 placebo	—	—	1	802	0.1%

Panel B. Statutory payer of each jump

Threshold	Total jump	Of which the buyer owes	Charm share
\$1,000,000	\$10,000	**\$10,000**	78.4%
\$2,000,000	\$5,000	**\$5,000**	17.2%
\$3,000,000	\$15,000	**\$7,500**	18.5%
\$500,000†	\$2,125	\$0	7.9%

Notes. Charm share is sales at $X-1$ as a percentage of sales at $X-1$ or X. †**Not comparable with the other rows.** The city's higher rate applies to consideration *above* \$500,000, so both \$499,999 and \$500,000 pay 1%, and pricing one dollar below saves nothing; the jump is avoided by a price of \$500,000 or less, which is itself a round number. The row's charm share therefore measures round-number pricing below a salient price, which is a different thing from avoidance, and a boundary-aligned test would need round-number controls this table does not have. Panel B's incidence follows the statutes: the city's Real Property Transfer Tax and the state base tax under §1402 are imposed on the *grantor* (Administrative Code §11-2104; Tax Law §1404(a)), while the mansion tax under §1402-a and the supplemental tax under §1402-b are paid by the *grantee*; §1402-b says that "the tax imposed by this section shall be paid by the grantee." 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. Among the three thresholds where a price one dollar below avoids the jump, the charm share is highest at \$1,000,000, the oldest threshold, which also differs from the others in price level and market, so the comparison cannot isolate an effect of age.

read as a pattern than as an estimate. And the corresponding split of the *bunching* magnitudes is a null. Applying the Kleven–Waseem estimator separately by buyer type gives differences (financed minus cash) of $+0.063$, $+0.053$, $+0.165$ and -0.069 at the four thresholds, and $+0.054$, $+0.054$, $+0.004$ and -0.137 at four never-taxed placebo prices by the same procedure. That split is within specification noise. The charm-price comparison survives only because it is a count of transactions at two prices with no counterfactual to misspecify.

On cells this small the ordering is suggestive at most. Pricing below a notch is *successful avoidance*, so on this evidence cash buyers pay the notch more often. The claim concerns responsiveness and says nothing about burden. Two readings of *why* are available. Payment method may proxy liquidity: buyer-side transfer taxes add to the funds due at closing, alongside the down payment for a financed purchase. Or a financed transaction involves more professionals (a lender, an appraiser, an attorney preparing a cash-to-close statement), who are more likely to notice the threshold. Loan-to-value provides one check of the liquidity prediction, which is that the most leveraged buyers avoid the notch most. Among 627 financed house purchases recorded at exactly X or $X - 1$ at the three buyer-paid thresholds, with threshold and year fixed effects, the probability of the charm price rises by 0.3 percentage points per ten points of loan-to-value (standard error 0.9), and buyers borrowing 80% or more of the price choose it 12 points *less* often (standard error 3.4). The loan-to-value pattern does not support the specified liquidity prediction, and professional intermediation remains one possible explanation. Adding the 219 financed condominium purchases at these prices, with loans from the linked dataset (Loschi, 2026a), gives a pooled charm-price association of 0.3 percentage points per ten points of loan-to-value (standard error 0.7). Buyers borrowing 80% or more choose it 13 points less often (standard error 3.0; Table A9).

H. Counts and composition over equal periods

A fixed-window count comparison adds a transparent accounting check to the density estimates. It compares 2016–2018, 2020–2022 and 2023–2025, omitting 2019, in windows from $0.95X$ to $1.05X$. At the new \$2,000,000, \$3,000,000 and \$5,000,000 notches, the share strictly below the threshold rises by 10.8–16.4 percentage points (Table A12). Standardizing each post-period share to the pre-period property-type composition gives increases of 10.8–16.1 points, and the largest composition component is 0.81 points in absolute value. Separate standardization by company-seller status, with missing flags retained as a category, gives 11.3–17.4 points. Missing seller flags account for about 20–40% of these window samples and cannot be treated as individual sellers. These standardizations address broad observed categories and leave composition within them unaddressed.

Total sales in each new-notch window rise after the reform, as they do around the untaxed references. The counts therefore supply no estimate of transactions prevented by the tax. Pandemic conditions, price growth and other market changes prevent a causal reading of the equal-period volume comparison. Wider $0.90X$–$1.10X$ windows give smaller below-share increases of 4.5–12.1 points, or 4.4–11.4 after property-type standardization. This check confirms the local concentration of recorded prices, but transaction losses and incidence are outside its reach.

Transaction volume. Green et al. (2026) estimate that Los Angeles' 4% tax on sales above \$5,000,000, 5.5% from \$10,000,000, reduced transactions by about half. Nothing of that size is visible here. Sales between \$2,000,000 and \$10,000,000 were 35.2% of sales between \$1,000,000 and \$10,000,000 from January 2016 to March 2019 and 32.7% in 2020–2025, a decline of 2.5 points although rising nominal prices alone would push that share up. The 2019 steps were an order of magnitude smaller than the Los Angeles rates, and this comparison does not identify a volume effect. Pandemic-era demand shifted across price segments, and counts over equal three-year periods on every row show total sales rising around both taxed and untaxed thresholds (Table A12).

Table A12: Transaction counts and below-threshold shares over equal periods

	Sales in fixed window			Share below threshold (%)		
Threshold ($m)	2016–18	2020–22	2023–25	2016–18	2020–22	2023–25
1	6,694	8,943	8,231	86.5	89.1	87.8
1.5	3,257	4,457	3,992	49.0	50.1	47.6
2	1,846	2,588	2,553	55.2	67.7	69.1
2.5	1,582	1,965	2,032	50.2	48.2	48.6
3	1,164	1,458	1,454	52.7	63.4	68.0
5	537	794	686	50.1	61.2	66.5

Notes. Same all-residential NYC sales universe and $300,000 lower cutoff as the threshold analysis. Window $[0.95X, 1.05X)$, with sales exactly at X included in the denominator but not the below-threshold count. The $1m tax is unchanged; $1.5m and $2.5m are untaxed reference prices. Counts include unlinked sales. All periods are three years; 2019 is omitted. Detailed composition, missingness and $\pm 10\%$ window results are in replication package, `transaction_accounting.py`. This table is descriptive, with no counterfactual for the absence of the reform.

I. The mortgage recording tax: statute, loans and the gross charge

Referenced in Sections II.B, III.B and V of the paper.

The statute. New York imposes a tax on the recording of a mortgage on real property (New York State, 2026). The components are a basic tax of 50 cents per $100 under Tax Law §253(1); a special additional tax of 25 cents per $100 under §253(1-a); and, within the Metropolitan Commuter Transportation District, an additional 30 cents per $100 under §253(2). Section 253-a authorizes a city of one million or more to impose its own by local law, and New York City does.

For a one- to three-family dwelling or an individual condominium unit the aggregate rate is 2.05% on mortgages below $500,000 and 2.175% at or above. The special additional tax of 25 cents per $100 is, by the terms of §253(1-a), payable by the mortgagee for property containing not more than six residential dwelling units. What remains with the borrower is therefore **1.80% below $500,000 and 1.925% at or above**.

A purchaser paying cash records no mortgage and pays none of it.

The magnitude is material relative to the transaction. On a $900,000 condominium purchased with a $720,000 mortgage, the unadjusted borrower-side schedule implies $13,860 before credits, exemptions or consolidation adjustments. Two buyers offering the same price for the same property face different statutory closing charges, and the law assigns the difference to the one who records a purchase mortgage.

The co-operative gap. Article 11 of the Tax Law, which imposes the mortgage recording tax, reaches mortgages *of real property*. A co-operative apartment is not real property. The purchaser acquires shares in a corporation and a proprietary lease, and a loan secured by those shares is a security interest in personalty, perfected by filing under the Uniform Commercial Code, to which no mortgage recording tax attaches. A financed co-operative purchase therefore escapes the tax while a financed condominium purchase of the same value does not, and co-operative purchases appear in the deed-based data as though they were almost all cash. The mansion tax under §1402-a, by contrast, expressly includes "a cooperative apartment unit," so the two articles draw the boundary of taxable residential property differently.

Loans and financing. New York City's Automated City Register Information System (ACRIS) records every instrument affecting real property **in Manhattan, the Bronx, Brooklyn and Queens**. Richmond County is not in it: Staten Island instruments are recorded by the Richmond County Clerk, and the city's own guidance directs users there. Applying a deed-based method to Staten Island therefore returns a

financed share of 0.0% on 41,445 sales, an artifact of the administrative boundary. Staten Island is therefore excluded from the loan-level statutory-burden measurement of this section. It is retained in Section IV of the paper, which studies prices rather than financing and needs no ACRIS match.

Two ACRIS tables are used. **Real Property Master** (`bnx9-e6tj`) supplies document type, consideration or loan amount, document date and recording date; restricted to plain `DEED` and `MTGE` documents recorded between 2015 and 2026 it gives **1,354,279 records**. **Real Property Legals** (`8h5j-fqxa`) supplies borough, block and lot for each document; restricted to the same period, **5,460,074 rows**, of which 3,609,058 resolve to a single parcel. Documents touching several parcels are dropped, since a mortgage spanning multiple lots cannot be attributed to one sale.

Restricting to plain `DEED` excludes `DEED, LE` (life estate), `DEED, TS`, `DEEDO`, `DEED, RC` and `DEED COR`. Correction deeds and life-estate transfers are therefore out of the sample before any further cleaning. This restriction does not remove foreclosure transfers, since referee's deeds and deeds in lieu of foreclosure are recorded as plain `DEED` and must be identified from their parties (Loschi, 2026b). Section VI of the paper returns to what this implies.

A purchase is classified **financed** if a same-parcel `MTGE` instrument is dated within -15 to $+90$ days of the sale, using the document date where available and the recording date otherwise, and **cash** otherwise. Cash therefore means that no such mortgage was matched, and a match says nothing about the loan's purpose. Purchase-money mortgages are normally recorded with or shortly after the deed. Two variants are reported throughout, -15 to $+30$ and -15 to $+180$. A refinancing that falls inside the window and a purchase mortgage that falls outside it both cause classification errors, which can run in either direction.

The condominium problem, and its solution. The parcel identifier does not join directly for condominiums. The Department of Finance bills a condominium unit under a billing lot in the 7501-and-above range, while ACRIS records instruments against the unit lot (1001, 1002 and so on). The two identifiers never meet, and a naive join returns a financed share of 0.0% for every condominium category. PLUTO does not bridge them. It contains 11,253 billing lots and only 2,188 unit lots, and its `condono` field numbers a condominium within a borough rather than linking its constituent units.

The match uses the deed's borough and block, consideration within 0.5%, and date within 45 days, searching unit lots 1001–6999. A retained match requires a unique qualifying unit parcel among single-parcel deeds. Several deeds on the accepted unit are resolved by retaining the lowest document ID; this is a selection rule, not verification that the deed records the purchase. Several qualifying units remain ambiguous. The comparative panel retains sales with an original parcel identifier and a recorded price strictly between \$300,000 and \$6,000,000. These restrictions are narrower than the linked dataset's full positive-price condominium universe.

An earlier field-agreement check applied the matching procedure to 211,394 house sales, where the sales parcel supplies a comparison field. Of these, 160,918 had a unique match, 44,954 had none, 3,042 had no parcel identifier and 2,480 were ambiguous. Recovered lots agreed with the sales parcel in 160,339 of the unique matches (99.6%). This is agreement between administrative fields, not an accuracy rate against verified instruments. The corrected condominium panel contains 86,381 sales. The broader linked dataset's coverage and guarded condominium rules are documented separately by Loschi (2026a).

The analysis sample. The deed-based panel contains **253,288 transactions in the four ACRIS boroughs**: 166,907 houses and 86,381 condominiums, on 2016–2025 sale dates, each with a price, a date, a parcel and a financing flag. A further 41,445 Staten Island house sales are in the sales file but carry no usable financing flag, because ACRIS holds no Staten Island instruments, and are excluded. The observed-loan calculations use financed purchases from this panel with a usable loan amount. The gross costing also imputes an amount for financed purchases without one, as described below. Section IV of the paper works instead on the sales file, one row per deed, including its extension above \$6,000,000, since it

needs prices and dates but no financing flag.

The gross schedule-implied charge on observed loans. The recording tax is levied on the loan rather than the sale, so the gross schedule-implied charge as a share of price depends on the recorded loan-to-price ratio. The ACRIS match records the loan amount alongside the price for most financed purchases, so that ratio can be measured instead of assumed. It is measured separately for each market, since houses and condominiums have different price levels and different loan-to-value distributions and the tax schedule is not proportional. House loan amounts are carried in the deposited linked panel. Condominium loan amounts come from the linked dataset (Loschi, 2026a), which rebuilds them from public ACRIS records with a documented script: 49,709 condominium purchases whose loan matches on unit lot, sale date and price.

Table A13: Gross schedule-implied borrower charge on observed loans, before credits, exemptions and CEMA adjustments

	Loans with an	Mean	Median	**MRT as a share of price**			
	observed amount	LTV	LTV	**Mean**	Median	p10	p90
Houses	125,023	0.745	0.774	**1.41%**	1.44%	0.91%	1.86%
Condominiums	49,709	0.677	0.737	**1.28%**	1.36%	0.78%	1.59%

Notes. The borrower's schedule, 1.80% below $500,000 and 1.925% at or above, is applied to each loan at its own size rather than at a representative loan-to-value ratio, and expressed as a share of the recorded purchase price. The lower condominium benchmark accompanies a lower mean recorded loan-to-price ratio. Why loan amounts or financing choices differ is a separate question. A cash buyer pays none of it in either market, and a co-operative buyer pays none of it at all, since a share loan is personalty and outside Article 11. The schedule is applied to recorded principal. The calculation does not establish taxable new money and applies no credits or exemptions, such as the exclusion of the first $10,000 of one- and two-family mortgages from the 0.30% component or the credit on first sales of condominium units (New York City Department of Finance, 2025).

Consolidation agreements. A New York purchase can be financed by a Consolidation, Extension and Modification Agreement (CEMA), under which the buyer takes an assignment of the seller's existing mortgage and pays the recording tax only on new money. Where such agreements are used, recorded principal overstates the taxable amount. Retrieving the 29,511 `M&CON` documents ("MORTGAGE AND CONSOLIDATION" in the ACRIS code table) recorded since 2015 and matching them to parcels and dates, 56 of 72,830 purchases flagged cash in the panel before the condominium-linkage correction, 0.08%, have a concurrent consolidation agreement. The consolidation screen concerns financing classification and does not bound the adjustment from recorded principal to taxable new money among purchases already classified as financed.

Costing a notch-free change. Three statutory changes would narrow the difference in liability between a financed and a cash purchase: exempting owner-occupied residential purchase money from Article 11; imposing an equivalent charge on purchases that record no mortgage; or reassigning the borrower-borne components to the lender, as §253(1-a) already does for the special additional tax. Their economic incidence is not estimated here. The first forgoes revenue, including the §253(2) component that Tax Law §261 directs to the Metropolitan Transportation Authority, so it needs a replacement. The costing below covers all observed house and condominium purchases, because owner occupancy is not identified. Summing the schedule over every financed purchase in the linked panel, each observed loan at its own size, gives **$248 million a year** of gross schedule-implied borrower charge on residential purchase money in the four ACRIS boroughs between $300,000 and $6,000,000, of which $39.0 million is the MTA-dedicated

§253(2) component (Table A14). Of the total, $243.6 million rests on observed loan amounts and $4.1 million on imputed ones, for the 3,470 houses and 391 condominiums without a usable loan amount. This is a bottom-up figure for the population the paper observes, before credits, exemptions and consolidation adjustments, and it should not be read as net revenue forgone.

The Department of Finance publishes a benchmark. Its total mortgage-recording-tax liability on residential mortgages, which covers purchase and refinance loans in all five boroughs and includes the lender-paid special additional tax, averaged $541 million a year over 2016–2024 (New York City Department of Finance, 2025, Table 10). Put on the same all-levies basis, the panel's purchase-money charge averages $281 million a year over those years, 52% of the city's figure and between 45% and 61% in each year. The remainder is refinancing, Staten Island, purchases outside the panel's price range or without a matched loan, and exemptions, which the panel's gross basis does not apply.

Table A14: The gross schedule-implied charge on residential purchase money

Gross schedule-implied borrower charge on purchase money, $ million a year	
Houses, loan amount observed (125,023 loans)	161.2
Houses, loan amount imputed (3,470)	3.4
Condominiums, loan amount observed (49,709 loans)	82.5
Condominiums, loan amount imputed (391)	0.7
Total	**247.8**
of which the §253(2) MTA component	39.0
Total on observed loan amounts only	243.6

Notes. Merged house and condominium panel: 253,288 purchases between $300,000 and $6,000,000 in the four boroughs ACRIS covers, of which 178,593 financed. Staten Island sales, whose deeds and mortgages ACRIS does not record, are excluded. The charge is the borrower-side schedule applied to each observed loan's recorded principal (1.80% below $500,000, 1.925% at or above), before credits, exemptions and consolidation (CEMA) adjustments, so the totals are a fixed-sample gross benchmark, not net revenue. Condominium loan amounts come from the linked dataset (Loschi, 2026a), matched to the panel on unit lot, sale date and price; sales whose unit, date and price match more than one record are left to imputation. Financed purchases without a usable loan amount carry the observed mean for their property type and price band. Replication package, `revenue_neutral.py`; the comparison with the Department of Finance in the text is from `benchmarks_v29.py`.

Because notches move transactions (Section IV of the paper), the illustrative replacements are continuous schedules paid by every house and condominium buyer regardless of payment method. Raising the same $248 million from the same purchases takes 0.87% of the whole price; 1.55% of the price above $500,000; or 1.30% between $500,000 and $2,000,000 and twice that above (Table A5). Each is continuous in price. The marginal rate changes at a breakpoint, but total liability never jumps. Between 90% and 91% of financed buyers would pay less, by about $5,000 on average, and cash buyers about $11,000 to $13,000 more. Keyed on property class, the flat rate would be 1.02% for houses and 0.67% for condominiums. These figures hold transactions fixed and assign statutory incidence, so they measure the size of the swap and do not forecast its revenue.

J. Checking the one-row-per-deed rule

This section supports Sections III.A and IV.F of the paper. The sales file lists a deed that conveys several lots once per lot, each row carrying the whole consideration, and the paper keeps one row per sale date, borough, tax block and price. Table A15 classifies every row that rule removes within 10% of the four thresholds. Near the three new thresholds almost all removed rows are one deed, repeated rows for one unit, or co-operative apartments sold together at a bulk price. At the $1,000,000 control, many are separate sales of identical units on the same day, so the paper's every-row estimates are the relevant check there.

Table A15: What the one-row-per-deed rule removes near the thresholds

	Rows removed within 10% of				In $(X, 1.05X)$, post-reform	
Removed rows belong to	\$1M	\$2M	\$3M	\$5M	\$3M	\$5M
All lots on one recorded deed (ACRIS)	70	151	189	227	32	55
Repeated rows for one unit	33	55	66	157	41	47
Co-op apartments, bulk price	50	139	110	137	29	32
Separate recorded deeds	151	31	16	4	0	0
Co-op apartments, unit price	47	37	11	3	0	0
No deed found in ACRIS	31	7	14	0	0	0
All	382	420	406	528	102	134
Removed a year, before / after	21 / 45	7 / 54	3 / 60	3 / 81		

Notes. Rows of the sales file removed by the rule of Section III.A of the paper (one row per sale date, borough, tax block and price) in groups priced within 10% of each threshold, all periods. Groups whose rows carry different lots are checked against ACRIS: all lots listed on one deed dated within 30 days of the sale date, or on separate deeds. Groups on one lot are repeated rows for one unit when the address or apartment is the same; otherwise they are co-operative apartments in one building, whose share sales ACRIS does not record, and are classed as a bulk price when the price is at least twice the median of the building's other, unrepeated sales. Staten Island groups on several lots are among those with no deed found, because ACRIS does not cover the borough. Replication package, `deed_merge_check.py`, from `data/acris_merge_check.json`.

K. Replication manifest

Retrieval. `fetch_annualized.py` obtains the Department of Finance sales file; `fetch_acris.py` and `fetch_legals2.py` obtain the ACRIS master and legals tables. The legals table carries no date column, but its `document_id` begins with the four-digit recording year, which is what makes it pageable by period; cursor paging on `document_id` rather than offset paging is necessary because the table has 22.7 million rows.
Linkage. `build_panel.py` constructs the house panel and the financing flags. `condo_match.py` recovers condominium unit lots via each sale's own deed and reports the validation on houses. `merge_rerun.py` combines the two into the four-borough, loan-amount-linked panel of Section I. Co-operatives are outside the recording tax and contribute nothing to it, and the notch analysis needs prices and dates from the full sales file but no financing flag.
One row per deed. `fetch_full_price_range.py` re-retrieves the sales file with no upper price limit, paging on the stable row identifier and asserting the total against a server-side count; `build_deed_groups.py` reads that extract, checks that its date–price records are the deposited ones, and writes `data/deed_groups.csv.gz`, the groups of rows that share a sale date, borough, tax block and price. `deeds.py` keeps one row per deed in every script; setting `SHAPE_ALL_ROWS=1` keeps every row.
Fixed window and charm counts. `prepost.py`, `diffbunch.py`, `robust.py` and `verify.py` produce Tables A1, A10 and A11 on every row; `notch_by_financing.py` produces Table A2; `focal_table.py` produces Table 1 of the paper.
The full 2019 schedule. `threshold_extension.py` produces Table A20 below and Table A3 (seed `20260923`); `equality_test.py` the joint bootstrap tests of Section IV.E (seed `20260924`); `build_v11_exhibits.py` and `build_supp_v11.py` render Table A20, Figure A1 in Section M below and Section C; `collapsed_counts.py` compares the Table A3 counts on one row per deed and on every row, and `deed_merge_check.py` produces Table A15.
Placebos, timing and markets. `placebo_inference.py` produces Table A21 below and Table A6 (seed `20260926`); every placebo window is resampled with replacement when it is matched to a treated threshold's sample size. `event_study.py` produces Figure 3 of the paper (seed `20260927`). `build_linked_fields.py` attaches property type, borough, deed classification, company flags and year built from the linked dataset to the same sales, asserting that their date–price records are identical, and writes the matched condominium loans. `market_splits.py` and `cost_and_sponsors.py` (seed `20260929`) produce Table A23 below, the counts behind Table 6, and Tables A7, A8 and A9; `build_v15_exhibits.py` and `build_v24_exhibits.py` render them. `ltv_charm.py` produces the loan-to-value test of Section G and Table A4.
The round price, the model, widths and cost. `focal_robustness.py` (seed `20261013`) produces the price-zone counts of Table 2 of the paper, Table 5 of the paper and the complete estimates in Table A22 (without the round-price sales, net of the control, among houses, on every row and with alternative round-number indicators) and Table A24 (widths). `focal_inference.py` (seed `20261013`) produces Table 3 (the difference-in-differences, its permutation over placebos and its year-by-year version), the moved shares of Table 2, Panel A of Table 4, the avoided-tax variants of Table 6 and the loan-sizing check of Section IV.H; `make_did_figure.py` draws Figure 2, and `moved_share_checks.py` (seed `20261014`) Panels B and C of Table A19, and `did_round_placebos.py` (seed `20261015`) Panel B of Table A18. `notch_elasticity.py` and `km_unraveling.py` supply the elasticities and the Kopczuk–Munroe counterfactual. `benchmarks_v29.py` produces the revenue and slice schedules of Table 7 and the Department of Finance comparison of Section I; `volume_shares.py` the volume comparison; `make_headline_figure.py` Figure 1; and `build_v29_exhibits.py` renders the source exhibits for Tables 2, 3, 4, 5, 6 and 7 of the paper and Table A24 below.
The corrected condominium panel searches unit lots 1001–6999 in the frozen extracts. Common sale keys

retain their archived financing labels; newly recovered units use the same mortgage windows. The price-based core sample is unchanged. The corrected panel and its construction are included with the replication files.

Loans and the recording tax. `loan_notch.py`, `loan_notch_grid.py` and `loan_notch_checks.py` produce the loan-notch estimates of Section V of the paper, their specification grid, the year-by-year and price checks and the comparison with other round loan amounts. `revenue_neutral.py` produces Tables A13 and A14, the replacement schedules and Table A5; `compute_ltv.py` reproduces the house row of Table A13; `verify3.py` documents the consolidation screen and `fetch_cema.py` retrieves the consolidation agreements it uses.

Equal periods. `5_transaction_accounting/transaction_accounting.py` produces Table A12 and the composition and missingness detail.

New Jersey. Section IV.J and Figure 4 of the paper come from the scripts in `second_notch`: `nj_extract_sales.py` builds the sales file from the MOD IV rolls, `nj_mansion_tax_statewide.py` and `make_nj_figure.py` produce the estimates and Figure 4, and `check_2005_scope_sensitivity.py` and `nj_bounded_checks.py` produce the February-2005, county and block checks.

Checks. `check_text_against_outputs.py` confirms every number the paper and this appendix quote against the outputs, and `1_verify/verify_manuscript_numbers.py` audits the derived arithmetic. Seeds are `20260903` for Table A10, `20260923` for Table A20 and Table A3, `20260924` for the joint tests of Section IV.E, `20260926` and `20260927` for the placebo draws, `20260929` for the cost and company-seller bootstraps, `20261013` for the round-price analysis, `20261014` for the moved-share checks in Table A19, Panels B and C, and `20261015` for Panel B of Table A18. Every table reproduces from these scripts and the public endpoints named above.

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L. Extended Results and Full Table Notes

The tables in this section retain the complete estimates and notes behind the main paper's shorter exhibits. Their original estimation samples and uncertainty intervals are unchanged.

Table A16: Round prices before and after the 2019 reform

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

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

Table A17: 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%
	$[+2, +58]$	$[+79, +100]$	$[-91, -64]$	$[-50, -11]$	[\$1,972, \$2,881]	[55, 67]
\$3,000,000	$+19$	$+42$	-37	-35	\$2,214	64%
	$[+1, +40]$	$[+33, +48]$	$[-46, -26]$	$[-50, -19]$	[\$1,598, \$2,738]	[55, 72]
\$5,000,000	$+10$	$+23$	-12	-23	\$2,985	61%
	$[-3, +22]$	$[+19, +28]$	$[-17, -7]$	$[-33, -12]$	[\$2,355, \$3,542]	[42, 73]
\$1,000,000 (control)	-29	$+138$	-3	-48	\$2,644	—
	$[-79, +15]$	$[+102, +171]$	$[-10, +2]$	$[-73, -21]$	[\$1,330, \$3,686]	
\$1,500,000 (never taxed)	$+15$	-8	$+8$	-8	—	—
	$[-19, +48]$	$[-20, +3]$	$[-15, +27]$	$[-37, +22]$		
\$2,500,000 (never taxed)	-17	-2	$+7$	-11	—	—
	$[-38, +6]$	$[-8, +4]$	$[-4, +20]$	$[-31, +8]$		

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

Table A18: Bin-count difference-in-differences: the new thresholds against the control and never-taxed round prices

	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
	$[-23, -11]$	$[-22, -1]$	$[-19, +2]$	$[-32, -10]$	$[-38, -15]$

Panel B. Other comparisons and samples

	Pooled		$(X, 1.05X)$, by threshold		
	Exactly X	$(X, 1.05X)$	$2,000,000	$3,000,000	$5,000,000
$1M control only	-54	-9	—	—	—
	$[-71, -31]$	$[-19, +3]$			
24 placebos only	-64	-18	—	—	—
	$[-68, -58]$	$[-24, -11]$			
Control and $500k-multiple placebos[a]	-65	-14	-7	-19	-25
	$[-69, -59]$	$[-21, -6]$	$[-17, +6]$	$[-31, -8]$	$[-35, -11]$
$500k-multiple placebos only	-65	-16	—	—	—
	$[-70, -60]$	$[-22, -8]$			
Post-period 2022–2025 only	-64	-18	-10	-22	-30
	$[-69, -59]$	$[-26, -9]$	$[-19, +4]$	$[-33, -7]$	$[-41, -18]$
No company seller	-67	-25	-28	-19	-29
	$[-74, -60]$	$[-35, -12]$	$[-39, -14]$	$[-37, +8]$	$[-51, +0]$

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

Table A19: 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% [55, 67]	63% [57, 69]	47% [−21, 75]	33% [−62, 68]
\$3,000,000	\$14,500	64% [55, 72]	66% [57, 74]	55% [18, 76]	46% [1, 71]
\$5,000,000	\$35,900	61% [42, 73]	63% [45, 75]	17% [−125, 63]	7% [−155, 60]
Average of the three		62% [55, 68]		40% [−17, 60]	

Panel B. Sales at exactly X that moved, other samples

	\$2,000,000	\$3,000,000	\$5,000,000	Average
Every row of the sales file	58%	60%	62%	60%
Post-reform years 2022–2025 only	61%	63%	57%	60%
No company seller on the deed	65%	70%	67%	67%
Company seller on the deed	40%	49%	30%	40%

Panel C. Other notches

	Jump	Comparison	Moved
Recording tax, \$500,000 loan	\$625	other round loan amounts	40%
Mansion tax, \$1,000,000, since 1989	\$10,000	new thresholds before 2019	99%

Notes. Each share is one minus the ratio of the post-reform to the pre-reform share of sales in $[0.9X, 1.1X)$ recorded at exactly X (Panel A, columns 3–4, and Panel B) or in the dominated range $(X, X+D]$, excluding X itself (Panel A, columns 5–6). $D = T/(1+t)$, with T the jump in all transfer taxes and t the total rate just above X: this is the frictionless price-adjustment benchmark, not a test of each buyer's awareness. Against placebos divides the post-to-pre ratio by its average over the 24 never-taxed multiples of \$100,000 of Table 3 (for the dominated range, over the same proportional band above each). The difference between the two averages in Panel A is +22 points (95% interval +1 to +79). 95% bootstrap intervals (1,000 draws) in brackets. Panel B: the company-seller flag comes from the linked deed and includes sponsor sales; the average without a company seller exceeds the average with one by 28 points (95% interval 9 to 62). Panel C compares levels, not periods. For loans, the share of loans in the last \$1,000 up to and including \$500,000 that sit at exactly \$500,000 (54%) against the same share at the untaxed round amounts from \$300,000 to \$700,000 (90% on average; 32% to 43% against any one of them). For \$1,000,000, the post-reform spike at exactly the threshold relative to local density (0.1, Table 1) against the average pre-reform spike at the three new thresholds (7.7). Replication package, `focal_inference.py`, `moved_share_checks.py`.

Table A20: The full 2019 schedule: every threshold, before and after the reform

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

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

Table A21: Placebo-calibrated inference for the 2019 notches

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

Notes. One row per deed. Estimator and windows as in Table A20. Placebos are the sixteen \$50,000 multiples between \$1,200,000 and \$6,000,000 whose window $[0.8X, 1.2X)$ contains none of the thresholds at \$500,000, \$1,000,000, \$2,000,000, \$3,000,000, \$5,000,000 and above. The rank column is one-sided in the direction a notch predicts (bunching up, R down): $(1 + k)/17$, where k is the number of placebos at least as extreme. It is a reference ranking rather than an exact p-value: a calibrated rank test would need an exchangeability argument the design does not supply, and neighboring placebo windows overlap. Size-matched placebos resample each placebo window with more sales than the treated window, with replacement, to the treated window's pre- and post-reform counts, fifty times per placebo (eight placebos for \$2,000,000 and \$3,000,000, sixteen for \$5,000,000); z is the estimate's distance from the draws' mean in their standard deviations. No placebo has as many sales as the \$1,000,000 window, so the control has no size-matched row. Panel C reports the two-sided share of differences between draws at two distinct placebos that are at least as large (for the proportional contrast, 0.118 when the two placebo windows must not overlap); these shares are reference comparisons, not calibrated p-values. Bootstrap p-values are from the joint bootstrap of Section IV.E. Supplemental Appendix Table A6 lists every placebo. Replication package, `placebo_inference.py`.

Table A22: 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 (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)
	[+0.028, +0.087]	[−0.027, +0.110]	[+0.080, +0.314]	[+0.028, +0.333]
Without sales within \$1,000	+0.058 (5)	+0.034 (6)	+0.203 (0)	+0.179 (0)
Without sales within \$5,000	+0.068 (4)	+0.035 (6)	+0.206 (0)	+0.188 (0)
Without sales at X and $X-1$:				
Net of the control	—	−0.015	+0.143	+0.123
		[−0.086, +0.060]	[+0.019, +0.265]	[−0.032, +0.283]
Houses only	+0.003 (6)	+0.046 (4)	+0.222 (1)	+0.112 (3)
Every row of the sales file	+0.058 (2)	+0.044 (2)	+0.146 (0)	+0.101 (1)
Indicators on containing bin	+0.061	+0.041	+0.191	+0.166
Panel B. Log change in R (placebos at least as low, of 16)				
All sales	−0.113 (1)	−0.350 (0)	−0.459 (0)	−0.479 (0)
Without sales at X and $X-1$	−0.110 (3)	−0.137 (1)	−0.305 (0)	−0.385 (0)

Panel C. Tests across thresholds (bunching)

	All sales		Without X and $X-1$	
	Estimate	Bootstrap p	Estimate	Bootstrap p
\$5M − 3 × \$2M (proportional)	−0.383	0.027	+0.056	0.67
\$3M − 2 × \$2M (proportional)	−0.062	0.607	+0.116	0.22
\$5M − \$2M	+0.064	0.535	+0.138	0.09
\$3M − \$2M	+0.161	0.061	+0.157	0.03

Notes. Estimator, windows, periods and placebos as in Tables A20 and A21. Each row drops the stated sales in both periods, at the thresholds and at every placebo alike; the dropped prices lie inside the range left out of the counterfactual fit, and where a bin loses part of its price range its counterfactual count is scaled by the share kept. 95% bootstrap intervals (500 draws) in brackets. Net of the control subtracts the \$1,000,000 change, with independent draws for the two windows, which do not overlap. Houses are building classes 01–03, with placebos recomputed among houses. The analysis keeps one row per deed; the every-row line uses all 442,497 rows of the sales file above \$300,000, 8,389 more. The estimator marks, for \$10,000 bins, the two bins on either side of each multiple of \$100,000 (Section IV.F); the last row of Panel A marks instead the bin that contains each multiple of \$100,000, \$50,000 and \$25,000. R is sales in $[X, 1.05X)$ over sales in $[0.925X, 0.975X)$. Panel C: the proportional rows test a response proportional to the change in the total transfer-tax rate, 0.25, 0.50 and 0.75 points at \$2, \$3 and \$5 million; all-sales p-values from 1,000 joint draws (Section IV.E); without X and $X-1$, from 500. Replication package, `focal_robustness.py`.

Table A23: Where the response comes from

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	Placebo SD
All sales	+0.059 (2)	+0.223 (0)	+0.384 (0)	+0.287 (0)	0.046
Houses	+0.005 (8)	+0.292 (2)	+0.485 (0)	+0.438 (0)	0.159
Condominiums	+0.151 (1)	+0.196 (0)	+0.376 (0)	+0.247 (0)	0.083
Co-operatives	+0.035 (7)	+0.192 (3)	+0.352 (0)	+0.275 (1)	0.173
Manhattan	+0.091 (0)	+0.225 (0)	+0.355 (0)	+0.220 (0)	0.060
Other four boroughs	+0.040 (3)	+0.212 (0)	+0.499 (0)	+0.680 (0)	0.076
No company seller	+0.019 (7)	+0.342 (0)	+0.381 (0)	+0.437 (0)	0.059
Company seller (sponsor proxy)	+0.205 (1)	+0.074 (4)	+0.430 (0)	+0.207 (1)	0.151
Without legal-rule deeds	+0.055 (2)	+0.227 (0)	+0.389 (0)	+0.288 (0)	0.046
Without any lender or referee deed	+0.054 (2)	+0.228 (0)	+0.387 (0)	+0.291 (0)	0.047

Notes. One row per deed. Estimator, windows and periods as in Table A20. Property types follow the Department of Finance building-class category (houses 01–03; condominiums 04, 12, 13, 15; co-operatives 09, 10, 17) and come from the linked dataset (Loschi, 2026a), whose sales are the same records as this paper's sample. Each row's placebos are the sixteen of Table A21, recomputed within that row's sample. The last two market rows split sales by whether the linked deed names a company as seller, a proxy that includes sponsor (developer) sales and other company sales; sales without a linked deed are in neither row. Without legal-rule deeds drops deeds whose consideration the companion paper's classifier (Loschi, 2026b) marks as set by a legal rule (referee's deeds to lenders, deeds in lieu of foreclosure, lender-to-lender transfers); the last row drops every deed with a lender or referee party; sales with no linked deed (co-operatives, Staten Island sales, and houses and condominiums whose deed was not matched) are kept. Supplemental Appendix Table A7 adds log changes in R and window sizes. Replication package, `market_splits.py` and `cost_and_sponsors.py`.

Table A24: How far sales moved: bunching widths with and without the round price

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

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

Table A25: What the 2019 notches cost in avoided tax

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

Notes. Post-reform period, January 2020 to December 2025. Extra sales just below X are the excess mass in $(0.975X, X)$ after the reform minus the pre-reform excess rescaled to the post-reform counterfactual density; missing minus extra is the corresponding missing mass in $(X, 1.05X)$ less that excess. Estimator and windows as in Table A20; 95% bootstrap intervals in brackets, covering sampling uncertainty conditional on the estimator. Each extra sale is assumed to have crossed the threshold from above (Table 2 shows most came from the round price itself) and to avoid the statutory jump at X (at $3,000,000 the buyer's 0.25 points and the seller's 0.25-point base-rate rise); it also lowers the other transfer taxes by about 3–4% of a price reduction that is mostly under $5,000, which is not counted. Other counts give $2.9 million when the moved sales are counted directly as those lost at and above X (Table 2), $2.2 million on every row of the sales file, and $1.6 million when each pile is reduced by the control's own excess as a share of its window. The reform's whole residential schedule implies $296 million a year on post-reform sales from $2,000,000 (Table 7). At the $1,000,000 control, whose tax did not change, the extra sales are within the range of the never-taxed placebos. Replication package, `cost_and_sponsors.py`, `focal_inference.py`, `benchmarks_v29.py`.

Table A26: 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	$0	$0
$2,000,000	$5,000	$0	$0
$3,000,000	$22,500	$23,607	$4,850
$5,000,000	$75,000	$70,822	$33,949
$10,000,000	$250,000	$188,857	$179,446
$20,000,000	$600,000	$424,929	$688,684
Panel B. Mean tax added, by price band			
$2–3 million (2,528 a year)	$6,103	$10,415	$2,140
$3–5 million (1,486 a year)	$28,639	$42,931	$16,760
$5–10 million (788 a year)	$101,053	$111,824	$84,491
$10–25 million (238 a year)	$392,004	$299,682	$415,612
$25 million and above (48 a year)	$1,363,299	$974,488	$2,096,701
Panel C. Totals			
Revenue, $M a year	296	296	296
Sales paying more than under the notches	—	77%	7%

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

M. Bunching Across the Full Schedule

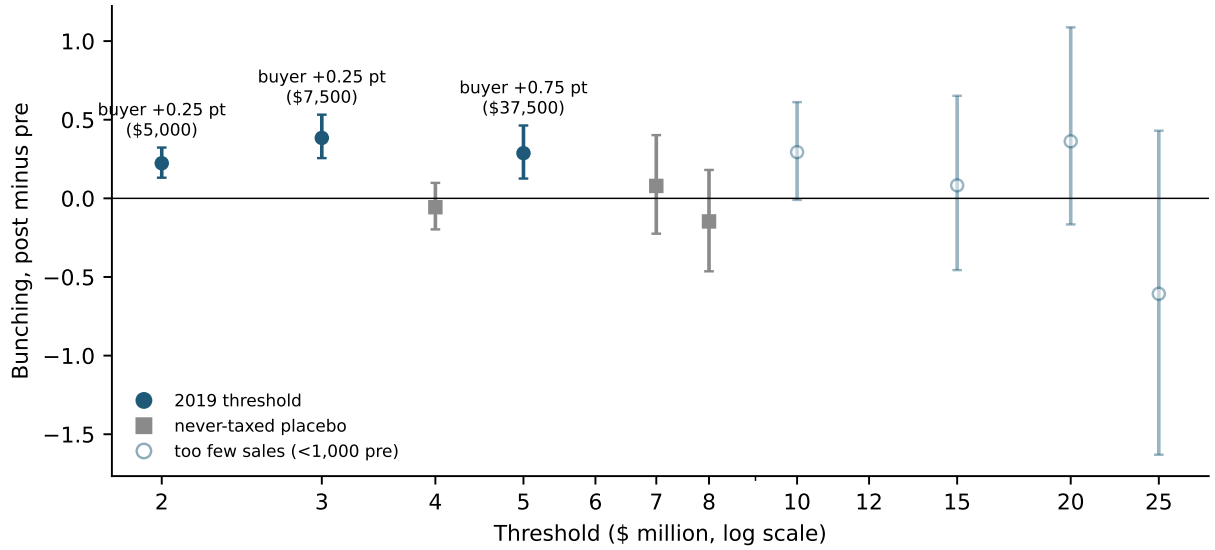


Figure A1: With all sales, the response does not rise in proportion to the jump. Change in bunching after the 2019 reform at each threshold of the schedule, with 95% bootstrap intervals (Table A20). Without the sales at the threshold and one dollar below it, proportionality is not rejected (Table A22, Panel C). Filled circles are reformed thresholds with enough sales to estimate; squares are never-taxed placebos whose windows hold no threshold; as in Table A20, the jump shown is the buyer's; hollow markers have fewer than 1,000 pre-reform sales in the window.

Alt text. The chart compares bunching changes at each new threshold with never-taxed prices. Estimates are imprecise at the highest thresholds, where transaction counts are small.