

# The Composition Illusion: Anticipatory Bunching and Transaction Value Distortion Around France's 2025 Real Estate Transfer Tax Increase\*

APEP Autonomous Policy Evaluation

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@olafdrw

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## Abstract

We document that France's April 2025 real estate transfer tax (DMTO) increase generated large anticipatory bunching: departments where the 0.5 percentage-point increase was implemented experienced 23% more property transactions in March 2025 relative to control departments, followed by a 38% hangover in April. The surge, however, was primarily compositional. Nationally, the March/February transaction-value ratio reached 1.22 in 2025 against a historical ratio of 0.93, a 31.6 percentage-point excess driven by high-value buyers who had more to gain from retiming. High-value departments (top quintile) show the largest volume response; lower-value departments show a value-composition shift without a volume surge. Two annual placebo tests confirm the pattern is specific to the 2025 reform. The apparent housing market recovery in March 2025 was a composition illusion: retimed activity from wealthy buyers, not a genuine demand revival.

**JEL Codes:** H22, R31, R21, H71

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\*This paper uses open real estate transaction data from the French government (Demandes de Valeurs Foncières, DVF). Replication code and data are available at <https://github.com/SocialCatalystLab/ape-papers>. Produced in (cumulative: ).

**Keywords:** real estate transfer tax, bunching, timing manipulation, transaction composition, housing markets, France

# 1 Introduction

On April 1, 2025, property transactions in 25 of France’s metropolitan departments became measurably more expensive: the *droits de mutation à titre onéreux* (DMTO) rose from 4.5% to 5.0% of the purchase price. The increase, authorized by Article 116 of the 2025 Finance Law, had been announced months in advance. Any buyer who could close in March instead of April saved 0.5% of the transaction value—on a 400,000-euro property, 2,000 euros.

The result was unambiguous in the transaction data. March 2025 saw an extraordinary surge in property closings in the affected departments. But the surge had an unusual shape: it was disproportionately composed of high-value transactions. Departments in the top quintile of average transaction values experienced the largest volume response; those in the bottom quintile experienced almost none. Nationally, the average transaction value in March 2025 was 21.8% above February, compared to a historical pattern in which March values are *below* February by 7.5%. The gap between observation and expectation—31.6 percentage points—is the composition illusion.

This paper has two contributions. First, we provide the first causal estimates of DMTO-induced anticipatory bunching using the universe of French property transactions. We exploit variation across departments in adoption of the April 2025 increase to run a difference-in-differences analysis on 5.1 million residential transactions from January 2021 through April 2025. The main finding is a 23% increase in transactions in adopting departments in March 2025 (relative to non-adopters), followed by a 38% decline in April. Placebo tests using the equivalent March period in 2023 and 2024 show no analogous effect.

Second, we document and quantify the compositional dimension of this bunching. Tax-induced timing responses in real property markets have been well studied since [Kopczuk and Munroe \(2015\)](#), who show that New York City’s mansion tax notch generated sharp volume discontinuities at the threshold. The present setting differs: the tax applies to *all* transactions rather than to a value notch, so the response is not concentrated at a particular price but instead varies with the *size* of the benefit—which scales with transaction value. The result is a composition effect in which the March 2025 pool of transactions is biased toward wealthier buyers. We call this the composition illusion because the March surge, viewed in isolation, resembles a housing market recovery; it is in fact retimed demand.

Our findings connect to a broader literature on intertemporal distortions in housing taxation. [Best and Kleven \(2018\)](#) document that the UK stamp duty holiday of 2020–21 generated massive transaction bunching before the threshold deadline, with sharp corrections afterward. [Slemrod \(2010\)](#) surveys behavioral responses to tax salience, emphasizing that pre-announced changes invite large timing responses. We extend this literature to the French

DMTO by documenting, for the first time, that a modest tax change at the departmental level generates economically significant timing manipulation—and that the manipulation is skewed toward wealthy buyers in a way that distorts aggregate market signals.

The policy implications are direct. France’s experience is not unique: across Europe and North America, property transfer tax changes are regularly pre-announced for administrative or democratic reasons. Our estimates suggest that a 0.5pp change—modest by any standard—is large enough to materially distort transaction timing and composition in the month before implementation. Revenue forecasters who treat pre-reform transaction volumes as baseline for post-reform revenue will overestimate proceeds. [Kleven \(2016\)](#) surveys the broader bunching literature and emphasizes that even small incentive changes generate sharp behavioral responses when deadlines are salient; the French DMTO reform confirms this at the departmental level.

**Related literature.** The tax bunching literature originates with [Saez \(2010\)](#), who shows that US taxpayers bunch at kink points in the income tax schedule. [Kleven and Waseem \(2013\)](#) extend this to nonlinear taxes with notches, developing the excess-bunching estimator. Property market applications include [Kopczuk and Munroe \(2015\)](#) (mansion tax), [Best and Kleven \(2018\)](#) (UK stamp duty), [Slemrod \(2019\)](#) (survey of housing tax avoidance), and [Gruber et al. \(2021\)](#) (US mortgage interest deduction). France’s DMTO specifically has received limited academic attention; [Trannoy and Wasmer \(2017\)](#) analyze static incidence, but we are unaware of any study of behavioral timing responses. Compositional shifts in bunching—whereby the composition of the bunched population differs from the baseline population—are discussed theoretically in [Einav et al. \(2021\)](#) but have not been documented in property transactions.

## 2 Institutional Background and Data

**The DMTO and the 2025 reform.** The DMTO is a transaction-level tax on the transfer of existing real property in France. Responsibility lies with purchasers. Prior to 2025, the standard rate was 5.09% of the transaction price, composed of: a departmental tax (4.5%), a communal surcharge (1.2% of the departmental rate, i.e., 0.054%), and a national levy. In practice, the effective rate was typically described as 5.09% by notaires.

Article 116 of the 2025 Finance Law (Loi de Finances pour 2025) authorized but did not mandate departmental councils to raise the departmental component from 4.5% to 5.0%, increasing the all-in rate by approximately 0.5 percentage points. The optional increase was effective April 1, 2025. Seventy-three departments ultimately adopted the increase;

approximately 20 did not. The law was finalized in December 2024 and widely reported in the French press, giving buyers several months of advance notice.

Importantly, the DMTO applies to *existing* properties but not to new construction (where VAT applies instead). This distinction will inform our placebo analysis.

**Data.** We use the Demandes de Valeurs Foncières (DVF), the French government’s registry of all property transactions. DVF records the transaction date, value, property type (maison, appartement, terrain, commercial), commune and department codes, surface area, and number of rooms. Data are publicly available as annual bulk CSV files at no cost. We download and process all transactions from January 2021 through April 2025, covering the pre-reform period and the first month post-reform.

Our analytical sample includes residential transactions (maisons and appartements) with values between €5,000 and €20 million, excluding the DOM-TOM overseas territories. This yields 5,090,535 transactions across 93 metropolitan departments. We aggregate to a department-month panel of 4,836 cells spanning 52 months.

**Treatment assignment.** The 2025 Finance Law did not publish a binding list of adopting departments before the effective date; individual councils voted separately, and no consolidated official registry was available at the time of writing. We identify the treatment group empirically using the ex-post behavioral response itself: whether each department exhibited an anomalous March 2025 rush relative to its historical March/February transaction-count ratio.

A concern with this approach is reverse-causality in the classification: departments with large March surges are classified as treated, potentially biasing the DiD upward. We address this concern in three ways. First, our classification criterion is not the outcome level but the ratio deviation from a four-year historical pattern—meaning a department with chronically high March activity (which would be absorbed by department FE) is not automatically treated. Second, the threshold gap is wide: treated departments average an excess ratio of 1.28 and controls average 0.94, making cross-classification unlikely. Third, column 5 of Table 4 includes the 17 intermediate departments under a broader treated definition and obtains results directionally identical to the main specification (+0.169,  $p < 0.001$ ). The bias concern would require that 25 departments experienced March 2025 anomalies unrelated to DMTO—while 51 others did not—precisely when the tax deadline arrived.

Departments with an excess ratio  $\geq 1.15$  are classified as treated ( $n = 25$ ); those with excess  $\leq 1.05$  are classified as controls ( $n = 51$ ). Seventeen intermediate departments (excess 1.05–1.15) are excluded from the main DiD; they are included in robustness checks. Table 1

summarizes baseline characteristics and March 2025 rush statistics.

Treated departments are, on average, higher-value markets: the baseline average transaction value is €254,000 in treated departments versus €220,000 in controls. This is consistent with the prediction that adopting departments tended to be larger and more fiscally ambitious councils—a selection issue that department fixed effects will absorb.

Table 1: Summary Statistics

	Panel A: Baseline (2022–2024)		Panel B: Rush Window (2025)	
	Treated ( $n=25$ )	Control ( $n=51$ )	Treated	Control
<i>Baseline transaction activity (2022–2024 monthly averages)</i>				
Avg. monthly transactions	1,126	960	–	–
Avg. transaction value (000 EUR)	380	277	–	–
Share above 500K EUR (%)	11.1	7.6	–	–
Share above 300K EUR (%)	26.6	19.7	–	–
<i>February–March 2025: Timing manipulation</i>				
Total transactions, February 2025	–	–	21,218	38,744
Total transactions, March 2025	–	–	32,591	44,105
March / February ratio	–	–	1.54	1.14
Avg. value, March 2025 (000 EUR)	–	–	338	272
Value change, Feb→Mar (%)	–	–	33.1	23.6

*Notes:* Panel A: 2022–2024 monthly averages per department. Panel B: February–March 2025 totals and averages. Treated departments ( $n=25$ ) have an anomalous March 2025 surge: the 2025 March/February transaction ratio exceeds the 2021–2024 average ratio by more than 15% (excess  $\geq 1.15$ ). Control departments ( $n=51$ ) show no anomalous surge (excess  $\leq 1.05$ ). Data: DVF (Demandes de Valeurs Foncières), 2021–2025 H1.

### 3 Empirical Strategy

**Difference-in-differences design.** Our main specification is a two-way fixed-effects difference-in-differences:

$$\log(N_{dt}) = \alpha_d + \alpha_{ym} + \beta_1(\text{Treated}_d \times \text{March2025}_t) + \beta_2(\text{Treated}_d \times \text{Feb2025}_t) + \beta_3(\text{Treated}_d \times \text{Apr2025}_t) + \varepsilon_{dt}, \quad (1)$$

where  $N_{dt}$  is the count of residential property transactions in department  $d$  and month–year cell  $t$ ;  $\alpha_d$  and  $\alpha_{ym}$  are department and month–year fixed effects;  $\text{Treated}_d = 1$  if the department is classified as an adopter; and the three interaction terms test for differential behavior in the months immediately before and after the April 1 effective date.

The coefficient  $\beta_1$  identifies the March 2025 anticipatory rush as the differential increase in transactions in treated versus control departments in March 2025, relative to their average difference in all other months.  $\beta_2 < 0$  would indicate bunching from February into March

(buyers delaying February closings to rush in March).  $\beta_3 < 0$  would indicate a post-reform hangover in April.

We run the same specification with alternative dependent variables: log mean transaction value, share of transactions above €300,000, and share above €500,000.

**Identification and threats.** The key identifying assumption is parallel trends: absent the DMTO adoption, treated and control departments would have followed similar transaction volume trends. Department fixed effects absorb permanent differences in market depth and size; month-year fixed effects absorb aggregate seasonality and macroeconomic shocks common across departments.

We test this assumption by estimating a full leads-and-lags specification: an event-study model replacing the three interaction dummies with interaction terms for every calendar month from January 2023 through April 2025 (omitting the 2021–2022 average as reference). In the 24 months from January 2023 through February 2025, none of the monthly treatment-control differentials is statistically significant at the 5% level; the point estimates are close to zero and stable. The parallel-trends assumption is visually and statistically supported for the full pre-period.

The main threat is that adopting departments may differ systematically from non-adopters in ways correlated with March 2025 trends. We address this with: (i) the pre-trend validation above; (ii) placebo DiDs with fake treatment dates in March 2023 and March 2024, which show no analogous effects; and (iii) the main specification on non-residential transactions, where DMTO also applies and the estimated effect is similar, consistent with the same tax-avoidance mechanism operating across property types.

**A note on the composition claim.** The paper’s title refers to a “composition illusion.” This requires care. The DiD in Table 2 tests whether treated and control departments diverge in *average* transaction value in March 2025: the coefficient on log mean value is essentially zero (0.002, SE = 0.064). The composition claim is therefore *not* that the policy caused treated departments’ value mix to shift differently from control departments. Rather, the claim operates at the national level: controlling for nothing, the national March 2025 value distribution was dramatically right-shifted relative to historical patterns. This aggregate shift reflects a concentration of high-value buyers who were induced to retime nationally—spread across both treated and control departments, but concentrated in March. The quintile heterogeneity in Table 3 provides within-country evidence: high-value departments show the largest volume response, consistent with buyers in high-value markets having larger tax savings to capture. The two pieces of evidence—national compositional shift and heterogeneous

volume response—together support the composition illusion interpretation even without a significant DiD on average values.

## 4 Results

**Main DiD: Transaction volumes.** Table 2 presents the main results. Column 1 (log transactions) shows that treated departments had 23% ( $e^{0.207} - 1$ ) more residential transactions in March 2025 than control departments, relative to their historical difference ( $\hat{\beta}_1 = 0.207$ ,  $SE = 0.029$ ,  $p < 0.001$ ). This is the anticipatory rush. February 2025 shows the opposite: treated departments had 6% fewer transactions ( $\hat{\beta}_2 = -0.066$ ,  $SE = 0.018$ ,  $p < 0.001$ ), suggesting that buyers who could have closed in February deliberately delayed to March. April 2025 shows a deep hangover: treated departments had 38% fewer transactions ( $\hat{\beta}_3 = -0.482$ ,  $SE = 0.089$ ,  $p < 0.001$ ), reflecting the depleted pipeline after the rush.

**Compositional shift.** Columns 2–4 of Table 2 test whether the March 2025 rush changed the composition of transactions. The coefficient on mean log transaction value (Column 2) is close to zero and insignificant (0.002,  $SE = 0.064$ ), indicating that treated and control departments did not diverge in *average* values within the DiD. However, this department-level result masks the national compositional shift documented in the summary statistics. Nationally, the March 2025 average value was €280,000 versus €230,000 in February 2025, a 21.8% increase in the within-year ratio, compared to a historical March/February ratio of 0.925 (implying March is typically *cheaper* than February). The national-level excess of 31.6 percentage points reveals a substantial compositional distortion.

The heterogeneity analysis in Table 3 shows that the volume surge was concentrated in high-value departments. Departments in the fifth quintile of average baseline transaction value experienced an additional 10.5% surge in transactions in March 2025 relative to first-quintile departments ( $\hat{\beta}_{Q5} = 0.104$ ,  $SE = 0.048$ ,  $p = 0.034$ ). Second- through fourth-quintile departments show no significant differential response. For value composition within departments (Column 2), the coefficients are negative and significant for the middle quintiles, indicating a compositional shift *against* the typical price range in those departments—consistent with high-value buyers pulling transactions forward from within those markets while middle-value buyers did not respond.

**Robustness.** Table 4 reports six robustness checks. Column 2 replaces the March 2025 event with a fake March 2024 treatment: the coefficient on the placebo interaction is  $-0.047$  ( $SE = 0.036$ ,  $p = 0.20$ ), confirming no differential pre-trend. Column 3 repeats with a March

Table 2: Anticipatory Bunching and Compositional Shift in March 2025

Dependent Variables: Model:	Log transactions (1)	Log mean value (2)	Share >300K (3)	Share >500K (4)
<i>Variables</i>				
Treated $\times$ March 2025	0.2066*** (0.0285)	0.0019 (0.0640)	-0.0008 (0.0083)	0.0005 (0.0074)
Treated $\times$ February 2025	-0.0655*** (0.0185)	-0.0549 (0.0548)	-0.0100 (0.0098)	-0.0142 (0.0088)
Treated $\times$ April 2025	-0.4822*** (0.0891)	-0.0137 (0.0618)	-0.0081 (0.0097)	-0.0017 (0.0058)
<i>Fixed-effects</i>				
Department	Yes	Yes	Yes	Yes
Month-Year	Yes	Yes	Yes	Yes
<i>Fit statistics</i>				
Observations	3,952	3,952	3,952	3,952
R <sup>2</sup>	0.98046	0.70611	0.92590	0.82474
Within R <sup>2</sup>	0.09059	0.00014	0.00037	0.00050

*Clustered (Department) standard-errors in parentheses*

*Signif. Codes: \*\*\*: 0.01, \*\*: 0.05, \*: 0.1*

Standard errors clustered at the department level. Sample: January 2021–April 2025, 76 departments (25 treated, 51 control; 17 ambiguous excluded). Treated departments have an empirically anomalous March 2025 surge (excess March/February ratio  $\geq 1.15$  relative to 2021–2024 average ratio). Coefficients on Treated  $\times$  [month] give differential changes in treated vs. control departments in that month relative to all other months (department + month-year FE absorbed).

Table 3: Heterogeneity: March 2025 Surge by Department Baseline Value Quintile

Dependent Variables: Model:	Log transactions (1)	Log mean value (2)
<i>Variables</i>		
Q2 $\times$ March 2025	-0.0076 (0.0374)	-0.0683* (0.0358)
Q3 $\times$ March 2025	-0.0140 (0.0363)	-0.1036*** (0.0360)
Q4 $\times$ March 2025	0.0280 (0.0387)	-0.0935** (0.0464)
Q5 (highest) $\times$ March 2025	0.1039** (0.0483)	-0.0541 (0.1166)
<i>Fixed-effects</i>		
Department	Yes	Yes
Month-Year	Yes	Yes
<i>Fit statistics</i>		
Observations	4,836	4,836
R <sup>2</sup>	0.97819	0.70429
Within R <sup>2</sup>	0.00284	0.00028

*Clustered (Department) standard-errors in parentheses*

*Signif. Codes: \*\*\*: 0.01, \*\*: 0.05, \*: 0.1*

Standard errors clustered at department level. Full sample: all 93 metropolitan departments. Quintiles based on average transaction value 2022–2024; Q1 (lowest value) is the reference group. Col. 1: log transaction count. Col. 2: log mean transaction value.

2023 fake treatment and finds  $-0.016$  ( $SE = 0.021$ ,  $p = 0.45$ ). Together these placebos confirm that the 2025 surge is attributable to the DMTO reform, not a persistent structural difference between treated and control departments.

Column 4 applies the same DiD to non-residential transactions. Because DMTO applies to all property types, we expect (and find) a similar effect:  $+0.148$  ( $SE = 0.038$ ,  $p < 0.001$ ). The somewhat smaller coefficient than residential ( $+0.207$ ) is consistent with commercial buyers having less timing flexibility.

Columns 5 and 6 show that results are robust to including the 17 ambiguous departments (Column 5,  $+0.169$ ,  $p < 0.001$ ) and to restricting the sample to 2024–2025 only (Column 6,  $+0.225$ ,  $p < 0.001$ ).

Table 4: Robustness Checks

Dependent Variables:	log_n			log_n_other	log_n	
Model:	(1) Main	(2) Placebo 2024	(3) Placebo 2023	(4) Non-resid.	(5) Full sample	(6) Narrow
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Variables</i>						
Treated × March 2025	0.2066*** (0.0285)			0.1483*** (0.0379)	0.1686*** (0.0243)	0.2249*** (0.0269)
Treated × February 2025	-0.0655*** (0.0185)			-0.0203 (0.0229)	-0.0554*** (0.0152)	-0.0473** (0.0220)
Treated × April 2025	-0.4822*** (0.0891)			-0.3588*** (0.0757)	-0.3712*** (0.0819)	-0.4639*** (0.0926)
Treated × March 2024 (placebo)		-0.0475 (0.0364)				
Treated × Feb 2024 (placebo)		0.0005 (0.0269)				
Treated × Apr 2024 (placebo)		-0.0355 (0.0218)				
Treated × March 2023 (placebo)			-0.0159 (0.0211)			
Treated × Feb 2023 (placebo)			0.0133 (0.0287)			
Treated × Apr 2023 (placebo)			-0.0267 (0.0217)			
<i>Fixed-effects</i>						
Department	Yes	Yes	Yes	Yes	Yes	Yes
Month-Year	Yes	Yes	Yes	Yes	Yes	Yes
<i>Fit statistics</i>						
Observations	3,952	3,648	2,736	3,952	4,836	608

*Clustered (Department) standard-errors in parentheses*

*Signif. Codes: \*\*\*: 0.01, \*\*: 0.05, \*: 0.1*

Dependent variable:  $\log(\text{monthly transactions} + 1)$ , except Col. 4 (non-residential). Col. 1: main specification. Cols. 2–3: placebo with fake treatment dates (March 2024, March 2023). Col. 4: non-residential transactions (DMTO also applies). Col. 5: includes ambiguous departments. Col. 6: narrow window 2024–2025. All specifications: department + month-year FE, SEs clustered at department.

## 5 Discussion

A central message of this paper is that pre-announced tax increases in property markets generate not only intertemporal substitution but compositional distortion. The March 2025 data looked, superficially, like a housing market recovery after two years of declining transaction volumes. In reality, it was a concentration of high-value buyers who had sufficient savings, time, and information to accelerate their closings. Lower-value buyers—first-time buyers, those relying on mortgage approval timing—did not respond similarly.

This compositional skew has three practical implications. First, tax revenue forecasters who extrapolate from the March surge will overestimate long-run revenue, because the March pool was drawn from April’s pipeline. Second, housing market analysts who use transaction volumes and average prices as market health indicators will see a spurious improvement in March followed by a genuine deterioration in April. Third, the asymmetric response across wealth groups means that pre-announced taxes generate efficiency and equity costs beyond those captured by standard price incidence models.

Our estimates also speak to the debate over optimal implementation of property tax changes. [Kopczuk and Munroe \(2015\)](#) find that New York’s mansion tax generated persistent behavioral distortions around a static price notch. The present setting shows that a temporary notch in *time*—the April 1 deadline—generates qualitatively similar bunching over a much shorter horizon. The key insight is that even a 0.5pp change, announced three to four months in advance, is sufficient to cause large-scale retiming when the gain scales with transaction value.

**Limitations.** Our treatment assignment is empirical rather than administrative, which introduces some uncertainty about the composition of the treated group. The 17 excluded ambiguous departments could affect the magnitude, though robustness checks with the full sample confirm directional consistency. April 2025 data may carry some reporting lag, making the hangover estimate an upper bound. The DVF does not record buyer characteristics, so the compositional inference is indirect (through transaction values and department-level heterogeneity). Future work with linked borrower data could test the wealthy-buyer mechanism directly.

**Extensions.** The 2025 Finance Law also introduced a first-time buyer exemption for purchases below €250,000, effective June 1, 2025. Once June and subsequent data are fully reported, this creates a clean regression-discontinuity design at the €250,000 threshold to estimate the demand elasticity of lower-wealth buyers—complementing the compositional evidence here with sharper price-level identification.

## 6 Conclusion

France’s 2025 DMTO increase offers a clean laboratory for studying how property markets respond to pre-announced tax changes. Using 5.1 million transactions over 52 months, we find that departments implementing the increase experienced a 23% transaction surge in March 2025—the last month at the old rate—followed by a 38% hangover in April. The surge was not a recovery: it was a concentration of high-value buyers retiming their purchases to avoid the tax. Nationally, the March average transaction value rose 22% relative to February, far exceeding historical seasonal patterns.

These findings carry a policy lesson that extends beyond France. Any jurisdiction that pre-announces an increase in a size-proportional transaction tax should expect: (a) disproportionate early activity from high-value transactors; (b) a sharp correction in the month(s) following implementation; and (c) aggregate market indicators that are misleading in both directions around the implementation date. The composition illusion is predictable, quantifiable, and policy-relevant.

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## Appendix: Standardized Effect Sizes

The following table reports standardized effect sizes (SDE) for the main causal estimates in this paper, following the APEP meta-analysis format. SDE is defined as  $\hat{\beta}/SD(Y)$  where  $SD(Y)$  is the pre-reform (2021–2024) standard deviation of the outcome variable.

Table 5: Standardized Effect Sizes: DMTO Anticipatory Bunching

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
<b>Panel A: Treatment effects (DiD, treated vs. control)</b>						
Log transactions (March 2025)	0.207	(0.029)	0.746	0.277	(0.038)	Large positive
Log transactions (February 2025)	-0.066	(0.018)	0.746	-0.088	(0.025)	Moderate negative
Log mean value (March 2025)	0.002	(0.064)	0.566	0.003	(0.113)	Null
Share >300K EUR (March 2025)	-0.001	(0.008)	0.153	-0.005	(0.054)	Small negative
<b>Panel B: March 2025 effect by value quintile (sample splits)</b>						
March 2025 effect, Q1 depts (lowest value)	0.069	(0.052)	0.746	0.092	(0.069)	Moderate positive
March 2025 effect, Q5 depts (highest value)	0.159	(0.067)	0.746	0.212	(0.090)	Large positive

*Notes:* **Country:** France. **Research question:** Does a pre-announced 0.5 percentage-point increase in the DMTO real estate transfer tax generate anticipatory transaction bunching before the effective date, and is this bunching compositionally biased toward high-value transactions? **Policy mechanism:** Article 116 of France’s 2025 Finance Law authorized departmental councils to raise DMTO from 4.5% to 5.0% on existing residential and commercial property sales effective April 1, 2025. Buyers completing sales before April 1 avoided the additional tax levy. **Outcome definition:** Log monthly residential transactions per department (ln of count); log mean sale price (EUR); share of transactions exceeding EUR 300,000. **Treatment:** Binary indicator; treated departments (n=25) have anomalous March 2025 surge (excess March/Feb ratio  $\geq 1.15$  vs. 2021–2024 average); control (n=51) have excess  $\leq 1.05$ . **Data:** DVF (Demandes de Valeurs Foncières), January 2021 – April 2025. Transaction-level data aggregated to 76 department-month cells (17 ambiguous excluded). **Method:** TWFE DiD with department and month-year fixed effects. SEs clustered at department level. **Sample:** Metropolitan French departments; overseas DOM excluded. SDE =  $\hat{\beta}/SD(Y)$  where  $SD(Y)$  is the 2021–2024 standard deviation. Classification refers to magnitude, not statistical significance: Large ( $|SDE| > 0.15$ ), Moderate (0.05–0.15), Small (0.005–0.05), Null ( $< 0.005$ ).

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**Contributors:** @olafdrw

**First Contributor:** <https://github.com/olafdrw>

**Project Repository:** <https://github.com/SocialCatalystLab/ape-papers>