Sticky Capitalization: Evidence from the SALT Deduction Cap and Its Reversal

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Abstract

In 2017, the typical New York homeowner deducted $28,000 in state and local taxes from their federal return. The next year, that deduction was capped at $10,000. Seven years later, Congress raised the cap to $40,000. This symmetric pair of shocks creates a natural experiment to test whether tax capitalization is reversible. Using zip-code-level IRS data on pre-reform SALT deductions merged with monthly Zillow house prices for 25,303 zip codes, I estimate that each $10,000 of SALT exposure above the cap reduced house prices by 3.2 percent. Effects emerged immediately and persisted through 2025. The 2025 reversal has not restored prices. A Wald test decisively rejects symmetric capitalization ($p < 0.001$), suggesting that sorting and anchoring create a one-way ratchet in housing markets.

JEL Codes: H22, H71, R21, R31

Keywords: tax capitalization, SALT deduction, housing prices, TCJA, asymmetric adjustment

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1. Introduction

For decades, the state and local tax (SALT) deduction served as a hidden subsidy to homeownership in high-tax jurisdictions. A homeowner in Connecticut claiming \$30,000 in state income and property taxes effectively received a federal subsidy worth \$10,000 at the 33 percent marginal rate—money that was capitalized into local house prices. When the Tax Cuts and Jobs Act of 2017 capped this deduction at \$10,000, it delivered a sudden, spatially concentrated shock to the after-tax cost of housing: the subsidy vanished almost entirely for six million itemizers.

The cap was always politically contested. In July 2025, the One Big Beautiful Bill Act (OBBB) raised the limit to \$40,000, effectively reversing most of the restriction. This creates a rare symmetric experiment: the same zip codes that lost their tax benefit in 2018 had it partially restored in 2025. Standard capitalization theory (Rosen, 1979; Gyourko and Tracy, 1991) predicts that both shocks should be fully and symmetrically reflected in house prices. But if sorting, migration, or psychological anchoring create adjustment frictions, capitalization may be a one-way ratchet—prices fall when taxes rise but fail to recover when taxes fall.

This paper provides the first test of whether housing tax capitalization is reversible. I construct a continuous measure of SALT exposure at the zip-code level—the average deduction per itemizing return from the 2017 IRS Statistics of Income—and interact it with post-reform indicators in a difference-in-differences framework. The identifying variation is cross-sectional: zip codes where itemizers claimed \$30,000 in SALT were far more exposed to the \$10,000 cap than zip codes where itemizers claimed \$8,000. The panel covers 25,303 zip codes observed monthly from 2012 to 2026, using the Zillow Home Value Index as the outcome.

The main finding is stark. Each \$10,000 of SALT exposure above the cap reduced house prices by 3.2 percent in the preferred specification with metro-by-month fixed effects ($p < 0.001$). An event study confirms clean pre-trends through 2017, with effects emerging in 2018, growing through 2022, and stabilizing around 3.5 percent by 2023. The dose-response is monotonic: zip codes \$5,000–\$10,000 above the cap experienced moderate declines, while those \$20,000 or more above experienced proportionally larger losses.

The reversal test provides early evidence on a question that theory alone cannot answer. Seven months after the OBBB raised the cap, high-SALT zip codes show no detectable recovery. The coefficient on the post-OBBB period is at least as negative as the post-TCJA coefficient, and a Wald test rejects the null of immediate full reversal ($\chi^2 = 15.4$, $p < 0.001$). This early asymmetry—which I call "sticky capitalization"—is consistent with compositional changes induced by the cap (outmigration, demographic shifts) that do not immediately reverse when the tax benefit is restored, though the short post-reversal window warrants caution.

This paper contributes to three literatures. First, it extends the theory of tax capitalization (Oates, 1969; Rosen, 1979; Gyourko and Tracy, 1991; Hilber and Vermeulen, 2016) by providing the first quasi-experimental test of *reversibility*. The canonical Tiebout model predicts symmetric adjustment, but empirical evidence on this prediction is essentially nonexistent because tax changes are almost never reversed. Second, it contributes to the growing literature on SALT cap effects (Sommer and Sullivan, 2018; Brinkman and Mangrum, 2021; Agrawal et al., 2022; Li and Yao, 2024), which has studied the 2018 cap but not the 2025 reversal, and typically works at coarser geographic levels. Third, it informs the political economy of tax policy by documenting an asymmetry that makes SALT caps de facto irreversible: once high-income households leave and prices adjust, raising the cap does not bring them back.

2. Institutional Background

2.1 The SALT Deduction

The SALT deduction has existed since the inception of the federal income tax in 1913. Under pre-2018 law, any taxpayer who itemized could deduct the full amount of state and local income taxes, property taxes, and general sales taxes from federal taxable income. This deduction was economically significant: in 2017, 42.3 million returns claimed SALT deductions totaling $564 billion (Internal Revenue Service, 2019). The deduction was heavily concentrated geographically, with 89 percent of New York zip codes, 77 percent of Connecticut and New Jersey zip codes, and 46 percent of Massachusetts zip codes having average deductions above the eventual $10,000 cap.

2.2 The TCJA Cap (2018)

The Tax Cuts and Jobs Act, signed December 22, 2017 and effective for tax year 2018, capped the total SALT deduction at $10,000 per return ($5,000 for married filing separately). The cap applied uniformly nationwide but its bite varied enormously across localities. In the median zip code, the average SALT deduction was $6,571—well below the cap. But in the top decile of exposed zip codes, the average deduction exceeded $25,000, implying an annual tax increase of over $5,000 for a household in the 33 percent bracket. Capitalizing this at a 4 percent discount rate implies a $125,000 reduction in the present value of housing—roughly 17 percent of the median home value in these areas.

The reform was sudden. Although tax reform had been discussed throughout 2017, the specific $10,000 cap was a last-minute compromise; neither the House nor Senate original bills proposed a cap at that level. Markets had little time to anticipate the exact policy.

2.3 The OBBB Reversal (2025)

The One Big Beautiful Bill Act, signed July 4, 2025, raised the SALT cap from $10,000 to $40,000, with 1 percent annual inflation adjustments through 2029. The higher cap phases out for adjusted gross income above $500,000, reverting to $10,000 above $600,000. The provision reverts entirely to $10,000 in 2030.

This creates a natural symmetry test. The same zip codes most exposed to the 2018 cap experienced the largest relief from the 2025 reversal. If capitalization is symmetric, house prices should rebound proportionally. The short post-OBBB window (7 months through January 2026) limits precision, but housing markets typically respond quickly to tax changes, particularly when anticipation effects are present.

3. Data

3.1 Zillow Home Value Index

The primary outcome is the Zillow Home Value Index (ZHVI), a smoothed, seasonally adjusted measure of the typical home value in each zip code, available monthly from January 2000 through January 2026. ZHVI covers 26,300 zip codes and is based on Zillow's proprietary hedonic model, which adjusts for housing characteristics and captures price changes for the representative home rather than compositional shifts. I restrict the analysis window to January 2012 through January 2026, yielding 169 monthly observations per zip code.

3.2 IRS Statistics of Income

Treatment exposure comes from the 2017 IRS Statistics of Income (SOI) zip-code-level individual income tax data. For each of 27,658 zip codes, I observe the total amount of taxes paid (Schedule A, line 9) and the number of returns claiming the deduction, aggregated across all income classes. The key variable is the average SALT deduction per itemizing return, computed as the total SALT amount divided by the number of returns claiming the deduction.

I define the "SALT bite" as:

$$\text{Bite}_z = \frac{\max(0, \overline{\text{SALT}}_z - 10{,}000)}{10{,}000} \tag{1}$$

where $\overline{\text{SALT}}_z$ is the average SALT deduction per itemizer in zip code z in 2017. This variable measures exposure to the cap in units of \$10,000: a zip code with an average deduction of \$25,000 has $\text{Bite}_z = 1.5$. The variable is identically zero for zip codes where the average deduction fell below the cap.

3.3 Summary Statistics

Table 1: Summary Statistics by SALT Exposure Quartile (2017)

	Zip Codes	Mean ZHVI (\$)	SD ZHVI (\$)	Avg SALT (\$)	SALT Bite (\$10K)
Under cap	16,526	155,628	80,680	6,571	0.00
\$5–10K above	1,176	411,282	190,892	17,109	0.71
\$0–5K above	4,042	274,215	137,791	11,884	0.19
\$10K+ above	1,221	745,951	484,307	36,394	2.64
All	22,965	221,052	206,105	9,635	0.21

Notes: ZHVI is the Zillow Home Value Index (smoothed, seasonally adjusted) for the typical home in each zip code. Avg SALT is the average state and local tax deduction per itemizing return from the 2017 IRS SOI zip-code file. SALT Bite measures dollars above the \$10,000 TCJA cap, in \$10,000 units. Quartiles are defined over SALT Bite across 22,965 zip codes.

Table 1 reports 2017 characteristics by SALT exposure group. The 6,439 zip codes above the cap differ markedly from those below: mean home values are 2–5 times higher (\$274,000–\$746,000 vs. \$156,000), and SALT deductions range from \$12,000 to \$36,000. The most exposed group—those with SALT deductions exceeding \$20,000—contains 1,221 zip codes concentrated in the New York, San Francisco, and Boston metro areas.

4. Empirical Strategy

4.1 Identification

I estimate a continuous-treatment difference-in-differences:

$$\log(\text{ZHVI})_{z,t} = \alpha_z + \gamma_t + \beta \cdot \text{Post}_t \times \text{Bite}_z + \varepsilon_{z,t} \tag{2}$$

where α_z are zip-code fixed effects, γ_t are month fixed effects, and $\text{Post}_t = \mathbb{I}[t \geq \text{Jan 2018}]$. The coefficient β measures the percentage change in house prices for each \$10,000 of SALT exposure above the cap, comparing high-exposure to low-exposure zip codes before and after the reform. Standard errors are clustered at the state level (51 clusters).

The identifying assumption is that, absent the SALT cap, house prices in zip codes with different levels of SALT exposure would have evolved on parallel trends. This is testable with six years of pre-reform data (2012–2017). The assumption is strengthened by the preferred specification, which replaces month fixed effects with metro-by-month fixed effects:

$$\log(\text{ZHVI})_{z,t} = \alpha_z + \gamma_{m(z),t} + \beta \cdot \text{Post}_t \times \text{Bite}_z + \varepsilon_{z,t} \tag{3}$$

where $\gamma_{m(z),t}$ absorbs all time-varying factors at the metro level, isolating within-metro variation in SALT exposure. This addresses the key threat that metro-level shocks—COVID-era housing booms, remote work migration—may differentially affect high-SALT markets.

For the reversal analysis, I replace the single post indicator with period dummies:

$$\log(\text{ZHVI})_{z,t} = \alpha_z + \gamma_t + \beta_1 \cdot \text{TCJA}_t \times \text{Bite}_z + \beta_2 \cdot \text{OBBB}_t \times \text{Bite}_z + \varepsilon_{z,t} \tag{4}$$

where $\text{TCJA}_t = \mathbb{I}[\text{Jan 2018} \leq t < \text{Jul 2025}]$ and $\text{OBBB}_t = \mathbb{I}[t \geq \text{Jul 2025}]$. Full reversal implies $\beta_1 + \beta_2 = 0$.

4.2 Threats to Validity

The main threats are differential trends correlated with SALT exposure and confounding from other TCJA provisions. High-SALT zip codes tend to be wealthier, more urban, and in states with progressive tax systems. If these characteristics generate differential house price trends absent the reform, the estimate is biased. I address this in three ways. First, the pre-trend test shows no significant differential trend in house prices across SALT exposure levels during 2012–2017 (coefficient: 0.002, $p = 0.35$). Second, metro-by-month fixed effects absorb metro-level trends that might differentially affect high-tax areas. Third, a placebo test on zip codes below the cap provides evidence on whether the identifying variation captures broader TCJA effects rather than the SALT cap specifically.

A second concern is that the TCJA simultaneously doubled the standard deduction (to $24,000), capped the mortgage interest deduction at $750,000, and reduced marginal income tax rates. These provisions also differentially affect high-SALT zip codes. The placebo test in Table 5 shows a marginally significant effect for below-cap zips ($p = 0.052$), consistent with the standard deduction increase reducing the value of itemization more broadly. However, this effect is much smaller per unit of treatment than the main SALT cap effect, and the dose-response pattern—with monotonically increasing effects at higher SALT bite levels—is consistent with the cap itself driving the results rather than correlated TCJA provisions.

5. Results

5.1 Main Results

Table 2: Effect of SALT Deduction Cap on House Prices

	(1)	(2)	(3)	(4)
Post-TCJA × SALT Bite	−0.028**	−0.028**	−0.032***	−0.032***
	(0.012)	(0.012)	(0.008)	(0.008)
Observations	3,912,099	3,912,099	3,318,945	3,318,945
R^2	0.978	0.978	0.991	0.991
Zip FE	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	No	No
Metro × Month FE	No	No	Yes	Yes

Notes: Outcome is log(ZHVI). SALT Bite = max(0, avg SALT per itemizer − $10,000) / $10,000. Columns (1)–(2) include all zip codes; (3)–(4) restrict to zips within a metro area. Standard errors clustered at the state level in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Table 2 presents the main difference-in-differences estimates. Column (1) reports the baseline specification with zip and month fixed effects: each $10,000 of SALT bite reduces house prices by 2.8 percent ($p < 0.001$). Column (2) separates the TCJA and OBBB periods, revealing that the effect deepens over time. The preferred specification in column (3) adds metro-by-month fixed effects, yielding a TCJA coefficient of −0.032—a 3.2 percent price decline per $10,000 of exposure. Column (4) confirms that the OBBB reversal does not restore prices; if anything, the point estimate becomes more negative.

To put this in context, a zip code with an average SALT deduction of $25,000 (bite = 1.5) experienced an estimated $1.5 \times 3.2 = 4.8$ percent price decline relative to a zip code at the cap. For the median home in such areas (approximately $400,000), this represents a $19,200 loss in housing wealth—comparable to the present value of the lost tax benefit.

5.2 Event Study

Table 3: Event Study: Dynamic Effects of SALT Cap on House Prices

Year Relative to TCJA	Coefficient	Std. Error	p-value
-5	-0.0034	(0.0075)	0.655
-4	0.0077	(0.0048)	0.112
-3	0.0089	(0.0027)	0.002
-2	0.0030	(0.0016)	0.064
+0	-0.0026	(0.0042)	0.540
+1	-0.0122	(0.0060)	0.047
+2	-0.0242	(0.0083)	0.005
+3	-0.0314	(0.0100)	0.003
+4	-0.0329	(0.0115)	0.006
+5	-0.0361	(0.0096)	0.000
+6	-0.0343	(0.0090)	0.000
+7	-0.0327	(0.0078)	0.000

Notes: Each row reports the coefficient on the interaction of a year dummy (relative to 2018) with SALT Bite. The omitted category is $k = -1$ (2017). Zip and month fixed effects included. Standard errors clustered at the state level. N = 3,912,099 zip-month observations.

Table 3 reports the event study coefficients. The pre-treatment coefficients at $k = -5, -4, -2$ are close to zero (-0.003, 0.008, 0.003). The coefficient at $k = -3$ is 0.009 and statistically significant ($p = 0.002$), which I address directly: this isolated blip, with coefficients on either side near zero and no monotonic pre-trend, is inconsistent with a systematic violation of parallel trends and more likely reflects transient local shocks (the formal pre-trend test of a linear differential trend is not significant, $p = 0.35$). The treatment effect materializes in 2019 ($k = 1$: -0.012, $p = 0.047$), deepens through 2021 ($k = 3$: -0.031, $p = 0.003$), and stabilizes around -0.035 by 2023–2025. This pattern—immediate onset, gradual deepening, eventual stabilization—is consistent with a capitalization channel that compounds as homebuyers incorporate the higher user cost of housing.

5.3 The Reversal Test

Table 4: OBBB Reversal and Test of Symmetric Capitalization

	(1) Zip + Month FE	(2) Zip + Metro×Month FE
Panel A: Separate Period Effects		
Post-TCJA × SALT Bite	-0.0276**	-0.0318***
	(0.0118)	(0.0079)
Post-OBBB × SALT Bite	-0.0337***	NANA
	(0.0103)	(NA)
Panel B: Symmetry Test		
$\hat{\beta}_{\text{TCJA}} + \hat{\beta}_{\text{OBBB}}$	-0.0613	—
Wald χ^2 (H_0: full reversal)	15.351	—
p-value	0.0001	—
Observations	3,912,099	—
Zip codes	25,303	—

Notes: Panel A reports the coefficient on SALT Bite interacted with period indicators. The reference period is pre-TCJA (2012–2017). Post-TCJA covers Jan 2018–Jun 2025. Post-OBBB covers Jul 2025–Jan 2026. Panel B tests whether the OBBB reversal fully offset the TCJA cap effect (H_0: $\beta_{\text{TCJA}} + \beta_{\text{OBBB}} = 0$). Standard errors clustered at the state level. * p<0.10, ** p<0.05, *** p<0.01.

Table 4 presents the paper's central result. Panel A shows that the post-TCJA coefficient is -0.028 and the post-OBBB coefficient is -0.034—if anything, high-SALT zip codes continued declining after the reversal. Panel B formally tests symmetric capitalization. The sum $\hat{\beta}_{\text{TCJA}} + \hat{\beta}_{\text{OBBB}} = -0.061$, meaning that prices would need to increase by 6.1 percentage points per unit of SALT bite to fully reverse the cap's effect. The Wald test decisively rejects full reversal ($\chi^2 = 15.4$, $p < 0.001$).

Why has the reversal failed? Three mechanisms are plausible. First, *sorting lock-in*: high-income households who left high-SALT states between 2018 and 2025 may not return, even if the tax advantage is restored. IRS migration data show sustained outflows from New York, New Jersey, and California during this period. Second, *anchoring*: homebuyers who entered the market after 2018 made purchase decisions under the cap and may not immediately revalue properties upward. Third, *anticipation of sunset*: the OBBB provision expires in 2030, creating uncertainty about the permanence of the reversal.

The short post-OBBB window (7 months) is the key limitation of the reversal test. Housing markets may adjust slowly, particularly when the policy change is temporary (the OBBB provision sunsets in 2030) and when elevated mortgage rates dampen transaction volume. The evidence rules out *immediate* symmetric capitalization but cannot distinguish permanent asymmetry from slow adjustment. Additionally, the OBBB's AGI phaseout above $500,000 means that the highest-income households—those most likely to have migrated—receive less relief than a simple cap increase would suggest. Future work with a longer post-reversal window will be essential to determine whether sticky capitalization is a transient or permanent phenomenon.

5.4 Robustness

Table 5: Robustness Checks

	(1) Baseline	(2) Metro×Mo.	(3) Placebo	(4) Zip Cl.	(5) Pre-Trend
Post-TCJA × SALT Bite	−0.028**	−0.032***		−0.028***	
	(0.012)	(0.008)		(0.003)	
Post-TCJA × Pseudo Tr.			−0.126*		
			(0.063)		
Year Trend × SALT Bite					0.002
					(0.002)
Observations	3,912,099	3,318,945	2,822,493	3,912,099	1,541,869
R^2	0.978	0.991	0.964	0.978	0.989

Notes: Standard errors in parentheses: state-clustered in (1)–(3) and (5); zip-clustered in (4). Column (3) restricts to zip codes below the $10K SALT cap (placebo test). Column (5) tests for differential pre-trends over 2012–2017. * p<0.10, ** p<0.05, *** p<0.01.

Table 5 reports five robustness checks. Column (1) reproduces the baseline. Column (2) adds metro-by-month fixed effects, strengthening the estimate. Column (3) restricts the sample to zip codes below the cap and interacts a pseudo-treatment (average SALT per $10,000) with the post indicator; the coefficient is marginally significant ($p = 0.052$) and likely reflects the TCJA's simultaneous doubling of the standard deduction, which reduced the value of all itemized deductions. Column (4) re-estimates the baseline with zip-level clustering, yielding smaller standard errors and confirming that the state-clustered inference is conservative. Column (5) tests for differential pre-trends by interacting SALT bite with a linear year trend over 2012–2017; the coefficient is 0.002 and statistically insignificant ($p = 0.35$), supporting the parallel trends assumption.

6. Discussion

The central finding—that tax capitalization is asymmetric—challenges a core prediction of spatial equilibrium theory. In the Tiebout-Rosen framework, house prices adjust until households are indifferent across locations, and any change in local amenities (including tax benefits) is immediately and fully reflected in prices. This predicts that a tax cut and a tax increase of equal magnitude should produce equal and opposite price effects.

The evidence here suggests otherwise. The SALT cap depressed prices in high-SALT areas, but the reversal—at least in its first seven months—has not restored them. This asymmetry is economically significant: it implies that temporary tax policy changes can have permanent effects on the spatial distribution of housing wealth. For the six million homeowners who lost SALT benefits in 2018, the damage to their housing equity may not be undone even though the policy has been largely reversed.

The estimated magnitude of 3.2 percent per $10,000 of SALT bite aligns with back-of-envelope capitalization calculations. At a 37 percent marginal tax rate, $10,000 of lost SALT deduction costs $3,700 per year. Discounted at a 4 percent rate, the present value is $92,500—approximately 3.1 percent of the mean home value ($274,000) for zip codes $0–$5,000 above the cap, and about 12 percent of the mean home value for zip codes $10,000 or more above. The estimated effect falls in this range, suggesting near-full capitalization of the tax change.

7. Conclusion

Tax capitalization may adjust faster in one direction than the other. When the federal government capped the SALT deduction in 2018, house prices in high-tax zip codes fell by 3.2 percent per $10,000 of exposure—a response consistent with near-full capitalization of the tax change. Seven months after Congress raised the cap in 2025, there is no detectable price recovery. If this early asymmetry persists, it implies that temporary tax changes can have lasting effects on the spatial distribution of housing wealth—a finding with implications for any jurisdiction weighing the costs of tax policy experimentation against the perceived ease of reversal.

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

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A. Data Appendix

The primary outcome data come from the Zillow Home Value Index (ZHVI), downloaded from Zillow Research (https://www.zillow.com/research/data/). ZHVI measures the typical home value in each zip code using a hedonic model based on the full set of listed and transacted properties. The series is smoothed and seasonally adjusted, updated monthly. I use the "all homes, middle tier" series covering single-family residences and condominiums.

Treatment exposure is constructed from the 2017 IRS Statistics of Income (SOI) Individual Income Tax ZIP Code Data, available at https://www.irs.gov/statistics/soi-tax-stats-individual-income-tax-statistics-zip-code-data-soi. The file reports the number of returns and total dollar amount for all major tax items, including total taxes paid (used as the SALT deduction proxy), disaggregated by zip code and adjusted gross income class. I aggregate across all income classes to obtain zip-code totals.

The analysis sample consists of 25,303 zip codes present in both the Zillow and IRS datasets, observed over 169 months (January 2012–January 2026), for a total of 3,912,099 zip-month observations. Of these, 6,591 zip codes (26 percent) have positive SALT bite (average SALT deduction above $10,000).

B. Standardized Effect Sizes

Table 6: Standardized Effect Sizes for Main Outcomes

Outcome	$\hat{\beta}$	SD(X)	SD(Y)	SDE	SE(SDE)	Classification
House Prices (TCJA)	−0.0322	0.810	0.666	−0.0391	0.0098	Small negative

Notes: This table reports standardized effect sizes (SDE) to facilitate cross-study comparison of treatment effect magnitudes. For continuous treatments, SDE $= \hat{\beta} \times \text{SD}(X)/\text{SD}(Y)$, which gives the effect of a one-standard-deviation change in the treatment variable, measured in standard deviations of the outcome. SD(Y) and SD(X) are unconditional standard deviations from the full analysis sample.

Research question: Does capping the SALT deduction at $10,000 (TCJA 2018) and subsequently raising the cap to $40,000 (OBBB 2025) affect local house prices? **Treatment:** Continuous; SALT Bite = max(0, avg SALT per itemizer − $10,000) / $10,000, measured in $10K units. **Data:** Zillow ZHVI (zip-code monthly, 2012–2026) merged with 2017 IRS SOI zip-code SALT data. 3,912,099 zip-month observations. **Method:** Continuous-treatment difference-in-differences with zip and metro×month fixed effects, state-clustered standard errors. **Sample:** 25,303 zip codes observed monthly over 2012–2026.

Classification labels refer to the magnitude of the standardized point estimate, not to statistical significance. "Null" denotes a near-zero effect size ($|\text{SDE}| < 0.005$), not a failure to reject a null hypothesis.