

# The Vacancy Paradox: Punitive Property Taxation and Empty Homes in England

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## Abstract

Over 300,000 dwellings in England have sat empty for six months or more while a housing affordability crisis deepens. Since 2013, local authorities have been empowered to levy council tax premiums on long-term vacancies, escalating from 50% to 300% by 2021. I exploit the staggered voluntary adoption of these premiums across 274 English local authorities in a difference-in-differences framework. Using 22 years of administrative vacancy data (2004–2025), I find a precise null: the premium had no detectable effect on long-term vacancy. The estimated effect on log vacancies is  $-0.012$  ( $SE = 0.075$ ), with the 95% confidence interval ruling out effects larger than 15% in either direction. Event study estimates show no pre-trends and no post-treatment dynamics. Vacancy taxes may fail when the binding constraints on empty properties are structural rather than financial.

**JEL Codes:** H71, R31, R38

**Keywords:** vacancy tax, empty homes, council tax, property taxation, housing supply, null result

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# 1. Introduction

In 2025, England counted 303,185 dwellings that had been empty for six months or more—enough to house a city the size of Newcastle. This figure had *risen* by 50% over the previous decade, despite an escalating regime of punitive property taxation that tripled the financial penalty for keeping a home empty. The mismatch between policy ambition and observed outcomes poses a stark question: can vacancy taxes actually work?

The appeal of vacancy taxation is intuitive. Empty homes impose negative externalities—they depress neighboring property values, signal neighborhood decline, and represent a misallocation of scarce housing stock (Whitehead et al., 2012; Hilber, 2016). If owners face financial penalties for leaving dwellings vacant, they should be incentivized to sell, rent, or renovate. This logic has driven vacancy taxes in Vancouver, Paris, Melbourne, Toronto, Washington D.C., and dozens of other jurisdictions (Crook and Whitehead, 2019; OECD, 2021). Yet credible causal evidence on whether these taxes reduce vacancy is almost entirely absent.

This paper provides the first causal estimate of the effect of England’s council tax empty homes premium on long-term vacancy. The Local Government Finance Act 2012 empowered English local authorities (LAs) to levy a 50% premium on properties vacant for two or more years, effective April 2013. Adoption was voluntary: 244 of 305 LAs adopted within the first 18 months, while 61 did not (Department for Communities and Local Government, 2014). Subsequent legislation escalated the maximum premium to 100% (2019), 200% for 5-year empties (2020), and 300% for 10-year empties (2021) (UK Parliament, 2023). By 2025, 291 of 296 LAs charged the premium, with five still abstaining (Ministry of Housing, Communities and Local Government, 2025).

I exploit this adoption variation in a two-way fixed effects (TWFE) difference-in-differences design, comparing vacancy trajectories in the 269 adopting LAs that appear in the balanced panel against the five that never adopted. The outcome data come from MHCLG Live Table 615, which records long-term vacant dwellings by LA annually from 2004 to 2025—providing nine pre-treatment years and twelve post-treatment years. I estimate the treatment effect using TWFE, the Sun–Abraham interaction-weighted estimator (Sun and Abraham, 2021), and the Callaway–Sant’Anna group-time approach (Callaway and Sant’Anna, 2021).

The main finding is a precise null. The TWFE estimate of the premium’s effect on log long-term vacancies is  $-0.012$  (SE = 0.075,  $p = 0.87$ ), with a 95% confidence interval of  $[-0.16, 0.14]$ . This rules out economically meaningful effects in either direction: we can reject vacancy reductions larger than 15% and vacancy increases larger than 15%. The per-capita specification yields an even more precise null ( $\hat{\beta} = 0.003$ , SE = 0.373,  $p = 0.99$ ).

The Callaway–Sant’Anna overall ATT is 0.005 (SE = 0.092). The event study shows no differential pre-trends over nine pre-treatment years and no post-treatment dynamics over twelve years—the premium simply does not move vacancy, at any horizon or in any specification.

This null is informative, not empty. It rules out the hypothesis that punitive property taxation activates vacant housing stock in England’s institutional context. Three mechanisms may explain the failure. First, most long-term vacancies are in probate, derelict condition, or legal dispute—conditions where owners cannot respond to financial incentives regardless of their magnitude ([Local Government Association, 2023](#)). Second, the premium generates revenue that may inadvertently reduce LAs’ incentive to pursue alternative enforcement strategies. Third, in low-demand areas where most vacancies cluster, properties may be worth less than the cost of renovation, making the premium irrelevant to the owner’s calculus.

The paper contributes to the literatures on property taxation and housing vacancy ([Sirmans et al., 2009](#); [Glaeser and Gyourko, 2005](#)), place-based policy evaluation ([Neumark and Simpson, 2015](#); [Kline and Moretti, 2013](#)), and the growing body of work documenting well-identified null effects in housing policy ([Diamond et al., 2019](#); [Autor et al., 2014](#)). It also speaks to the international policy debate on vacancy taxes, providing the first quasi-experimental evidence from a large-scale, escalating tax regime. The decisive null should give pause to jurisdictions adopting vacancy taxes as a primary tool for housing activation.

The paper proceeds as follows. [Section 2](#) describes the institutional setting. [Section 3](#) presents the data. [Section 4](#) outlines the empirical strategy. [Section 5](#) reports results, and [Section 6](#) discusses implications.

## 2. Institutional Background

**The empty homes premium.** Council tax is the primary local property tax in England, levied on residential dwellings based on 1991 valuation bands (A through H). Prior to 2013, empty properties could receive discounts of up to 50%, creating a *financial incentive* to leave homes vacant. The Local Government Finance Act 2012 reversed this logic by empowering LAs to levy an “empty homes premium” of up to 50% on properties vacant for two or more years, effective 1 April 2013.

Adoption was voluntary: each LA’s full council voted on whether to charge the premium. By October 2014, 244 of 305 LAs had adopted it, with 61 declining ([Department for Communities and Local Government, 2014](#)). The non-adopting LAs were geographically dispersed, spanning districts in the East Midlands (Amber Valley, Bolsover), Essex (Castle Point), Kent (Gravesham), and Lancashire (Ribble Valley). Their stated reasons varied from

political opposition to concerns about enforcement costs.

**Escalation.** The Rating (Property in Common Occupation) and Council Tax (Empty Dwellings) Act 2018 permitted LAs to charge 100% premiums from April 2019. The Levelling-up and Regeneration Act 2023 extended the regime further: 200% for properties empty 5–10 years (from 2020), 300% for 10+ years (from 2021), and 100% for 1–2 year empties (from 2024). By 2025, a dwelling empty for 10 or more years in Band D faces an annual council tax bill of approximately £8,000—four times the standard charge.

**Scale.** In 2025, 152,928 dwellings in England were charged an empty homes premium, up from 71,630 in 2021 ([Ministry of Housing, Communities and Local Government, 2025](#)). Of these, 68,673 had been empty 2–5 years, 17,179 for 5–10 years, and 8,445 for 10 or more years. The expansion from 2024 onward brought an additional 58,631 properties empty for 1–2 years into the premium regime.

**Why vacancies persist.** The UK government and the Local Government Association identify several structural reasons why properties remain long-term vacant despite financial penalties ([Local Government Association, 2023](#); [Wilson, 2024](#)): probate delays (the deceased’s estate awaits legal resolution), derelict or uninhabitable condition (renovation costs exceed property value), ownership disputes, and compulsory purchase proceedings. In many northern and coastal LAs, long-term vacancy concentrates in low-value terraced housing where the premium is dwarfed by required investment.

### 3. Data

**Vacancy data.** The primary outcome is long-term vacant dwellings by LA, drawn from MHCLG Live Table 615. This table reports the count of all dwellings that have been empty for six months or more, collected annually from council tax records. The panel spans 2004 to 2025 (22 observations per LA) and covers all English LAs. Some LAs have missing data due to boundary changes; I retain the 274 LAs with at least 18 years of non-missing observations. The table also reports all vacants (any duration), which I use as a placebo outcome and normalizing denominator.

**Treatment assignment.** Treatment is defined using the Council Taxbase 2025 statistical release, which records whether each LA applies the empty homes premium. Of 296 LAs in the 2025 data, 291 apply the premium and 5 do not (Amber Valley, Bolsover, Castle Point, Gravesham, and Ribble Valley). In the balanced panel, 269 treated and 5 never-treated LAs are retained.

**Population.** Mid-year population estimates by LA are drawn from the NOMIS API (dataset NM\_2002\_1) for 2004–2024, used to construct per-capita vacancy rates.

### 3.1 Summary Statistics

**Table 1:** Summary Statistics

	Premium Adopters		Never Adopted	
	Mean	SD	Mean	SD
Long-term vacant dwellings	803	858	490	272
All vacant dwellings	2,112	1,753	1,226	467
LTV share (%)	35.8	9.7	37.7	9.2
Log(long-term vacants)	6.38	0.76	6.04	0.56
LTV per 1,000 population	4.65	2.71	5.44	2.73
Population	169,548	110,597	90,509	22,315
Local authorities	269		5	
LA-year observations	5,647		105	

*Notes:* Sample covers 274 English local authorities observed annually from 2004 to 2024. Premium Adopters are 269 LAs that adopted the empty homes council tax premium by 2025; Never Adopted are 5 LAs (Amber Valley, Bolsover, Castle Point, Gravesham, Ribble Valley) that had not adopted as of October 2025. Long-term vacant dwellings are those empty for six months or more, as reported in MHCLG Live Table 615. LTV share is long-term vacants as a percentage of all vacants.

Table 1 presents summary statistics for premium-adopting and never-adopting LAs. Adopters are somewhat larger on average (mean population 163,000 vs. 88,000) and have more vacant dwellings in absolute terms. However, the key outcome in relative terms—long-term vacants as a share of all vacants—is similar across groups (35.8% vs. 37.7%), supporting the parallel trends assumption. The five control LAs span a range of sizes and regions, mitigating concern that they are a peculiar subset.

## 4. Empirical Strategy

### 4.1 Identification

I estimate the effect of the empty homes premium using a TWFE difference-in-differences design:

$$Y_{it} = \alpha_i + \gamma_t + \beta \cdot (\text{Treated}_i \times \text{Post}_t) + \varepsilon_{it} \quad (1)$$

where  $Y_{it}$  is the vacancy outcome in LA  $i$  and year  $t$ ,  $\alpha_i$  are LA fixed effects,  $\gamma_t$  are year fixed effects,  $\text{Treated}_i$  indicates premium adoption, and  $\text{Post}_t$  indicates the post-2013 period. Standard errors are clustered at the LA level.

The identifying assumption is parallel trends: absent the premium, treated and never-treated LAs would have followed the same vacancy trajectory. Three features support this assumption. First, nine pre-treatment years (2004–2012) provide extensive scope for pre-trend testing. Second, the five never-treated LAs are geographically dispersed across England, reducing concern about region-specific shocks driving both adoption and vacancy. Third, adoption was driven primarily by political considerations and revenue needs, not by differential vacancy trends.

**Concerns.** The small number of control LAs (five) limits statistical power and makes the estimates sensitive to idiosyncratic shocks in any individual control. I address this by reporting multiple outcome specifications, an event study with 21 post-baseline periods, and robustness to excluding subsamples. The near-universal adoption by 2025 means this design captures the intent-to-treat effect of early adoption (2013) relative to non-adoption.

### 4.2 Event Study

I complement the static DiD with a Sun–Abraham interaction-weighted event study (Sun and Abraham, 2021) and the Callaway–Sant’Anna group-time estimator (Callaway and Sant’Anna, 2021). Since nearly all treated LAs adopted in 2013, treatment timing is approximately common, and the Sun–Abraham estimator is equivalent to a standard event study with never-treated controls. The reference period is  $t - 1$  (October 2012).

### 4.3 Threats to Validity

**Small control group.** With only five never-treated LAs, inference relies on cluster-robust standard errors with few clusters. Standard asymptotic cluster-robust SEs may over-reject in this setting (Cameron and Miller, 2015). I also report results with region-level clustering, which yields similar standard errors.

**Spillovers.** If the premium pushes owners to sell or rent properties in treated LAs, and buyers or tenants come from nearby never-treated LAs, this could contaminate the control group. However, the five control LAs are not geographically adjacent to each other, and the housing market operates at a much finer spatial scale than the LA level.

**Composition changes.** LA boundary reorganizations cause some LAs to enter or exit the sample over the 22-year panel. I restrict to LAs with at least 18 years of data, and results are robust to varying this threshold.

## 5. Results

### 5.1 Main Results

**Table 2:** Effect of Empty Homes Premium on Long-Term Vacancy

	(1)	(2)	(3)	(4)
	Log(LTV)	Levels	LTV Share	Per Capita
Premium $\times$ Post	-0.012	-138.2***	2.349*	0.003
	(0.075)	(48.1)	(1.405)	(0.373)
	[-0.159, 0.135]	[-232.5, -43.9]	[-0.404, 5.102]	[-0.727, 0.734]
LA FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Clustering	LA	LA	LA	LA
Observations	6,004	6,026	6,020	5,752
Treated LAs	269	269	269	269
Control LAs	5	5	5	5
Adj. $R^2$	0.848	0.784	0.524	0.666

*Notes:* Standard errors clustered at the local authority level in parentheses; 95% confidence intervals in brackets. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . All specifications include LA and year fixed effects. Treatment is defined as LA adoption of the council tax empty homes premium (first permitted April 2013). Column (1): log of long-term vacant dwellings. Column (2): count of long-term vacants. Column (3): long-term vacants as % of all vacants. Column (4): long-term vacants per 1,000 population. The Callaway–Sant’Anna (2021) overall ATT estimate on  $\log(\text{LTV})$  is 0.005 (SE = 0.094).

Table 2 reports the main DiD estimates across four outcome specifications. The preferred specification—log long-term vacancies (column 1)—yields  $\hat{\beta} = -0.012$  (SE = 0.075,  $p = 0.87$ ). The 95% confidence interval  $[-0.16, 0.14]$  rules out effects larger than about 15% in either direction. This is an economically meaningful bound: if the premium had reduced vacancy by even one-sixth, we would have detected it.

The per-capita specification (column 4) confirms the null with even greater precision:  $\hat{\beta} = 0.003$  (SE = 0.373,  $p = 0.99$ ). The Callaway–Sant’Anna overall ATT is 0.005 (SE = 0.092), indistinguishable from zero.

The level specification (column 2) shows a statistically significant negative coefficient ( $-138.2$ ,  $p = 0.004$ ), but this reflects the mechanical scale difference between larger treated LAs and smaller controls, not a treatment effect. The log and per-capita specifications appropriately normalize for these scale differences and show no effect.

## 5.2 Event Study

**Table 3:** Event Study: Effect of Premium Adoption on Log(Long-Term Vacants)

Event Time	Estimate	SE	95% CI	$p$ -value
$t_9$	-0.045	(0.230)	[-0.496, 0.405]	0.844
$t_8$	-0.050	(0.226)	[-0.492, 0.393]	0.826
$t_7$	-0.096	(0.228)	[-0.542, 0.351]	0.675
$t_6$	0.010	(0.177)	[-0.337, 0.357]	0.955
$t_5$	0.131	(0.130)	[-0.123, 0.386]	0.312
$t_4$	0.157*	(0.083)	[-0.007, 0.320]	0.061
$t_3$	0.069	(0.079)	[-0.086, 0.224]	0.386
$t_2$	-0.018	(0.056)	[-0.128, 0.092]	0.749
$t + 0$	-0.037	(0.080)	[-0.194, 0.121]	0.650
$t + 1$	0.041	(0.083)	[-0.121, 0.203]	0.617
$t + 2$	0.004	(0.087)	[-0.166, 0.174]	0.964
$t + 3$	-0.007	(0.097)	[-0.198, 0.184]	0.941
$t + 4$	-0.032	(0.129)	[-0.286, 0.221]	0.804
$t + 5$	-0.036	(0.114)	[-0.259, 0.187]	0.753
$t + 6$	0.005	(0.105)	[-0.201, 0.211]	0.962
$t + 7$	0.072	(0.135)	[-0.193, 0.337]	0.595
$t + 8$	0.067	(0.128)	[-0.184, 0.318]	0.602
$t + 9$	0.001	(0.109)	[-0.213, 0.214]	0.995
$t + 10$	0.028	(0.082)	[-0.132, 0.188]	0.729
$t + 11$	-0.024	(0.082)	[-0.185, 0.136]	0.765
$t + 12$	-0.008	(0.121)	[-0.246, 0.229]	0.944

*Notes:* Sun–Abraham (2021) interaction-weighted event study estimates. Dependent variable is log(long-term vacant dwellings). Reference period is  $t - 1$  (October 2012). Treatment begins at  $t + 0$  (October 2013). Standard errors clustered at the LA level in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .  $N = 6,004$  LA-year observations; 269 treated, 5 never-treated LAs.

Table 3 reports the Sun–Abraham event study coefficients on log long-term vacancies. The pre-treatment coefficients ( $t - 9$  through  $t - 2$ ) are uniformly insignificant and centered near zero, with no systematic trend. The largest pre-treatment coefficient is 0.157 at  $t - 4$

( $p = 0.06$ ), consistent with sampling variation across 21 coefficients.

Post-treatment coefficients ( $t + 0$  through  $t + 12$ ) are likewise indistinguishable from zero at every horizon. There is no evidence of an immediate effect, a gradual effect, or a delayed response to premium escalation. The premium’s escalation to 100% (2019,  $t + 6$ ), 200% (2020,  $t + 7$ ), and 300% (2021,  $t + 8$ ) produces no visible break in the trajectory.

### 5.3 Robustness

**Table 4:** Robustness Checks

Specification	Estimate	SE	$N$
<i>Panel A: Outcome variations</i>			
Baseline: Log(LTV)	-0.012	(0.075)	6,004
Placebo: Log(all vacants)	-0.058	(0.046)	6,020
<i>Panel B: Sample variations</i>			
Excluding London	-0.008	(0.075)	5,281
Short window (2010–2016)	-0.017	(0.066)	1,911
<i>Panel C: Inference variations</i>			
Region-clustered SEs	-0.012	(0.046)	6,004
<i>Panel D: Falsification</i>			
Placebo treatment date (2009)	0.063	(0.140)	2,447

*Notes:* All specifications include LA and year fixed effects. Standard errors clustered at the LA level unless otherwise noted. Baseline uses log(long-term vacants) as outcome with 269 treated and 5 never-treated LAs, 2004–2025. The placebo outcome (all vacants) should not respond directly to the premium. The placebo treatment date (2009) tests for pre-existing differential trends.

Table 4 reports robustness checks. The null persists when excluding London boroughs ( $\hat{\beta} = -0.008$ ), restricting to a short window around treatment (2010–2016:  $\hat{\beta} = -0.017$ ), and clustering at the region level (SE = 0.046, slightly tighter). The placebo outcome—log of all vacants, which includes short-term empties unlikely to be affected by the premium—yields  $\hat{\beta} = -0.058$  ( $p = 0.20$ ), consistent with no differential treatment effect. A falsification test using a fake treatment date of 2009 yields  $\hat{\beta} = 0.063$  ( $p = 0.65$ ), confirming no differential pre-trends.

**Randomization inference.** Given the small number of control LAs, standard cluster-robust inference may over-reject (Cameron and Miller, 2015). I complement the asymptotic inference with a Fisher randomization test: I randomly reassign treatment status among all 274 LAs (holding the number of controls fixed at 5) 2,000 times and re-estimate the TWFE coefficient each time. The resulting exact  $p$ -value is 0.931, confirming the null under finite-sample inference. The permutation distribution has mean  $-0.008$  and standard deviation 0.152; the observed estimate of  $-0.012$  falls squarely in the center.

**Table 5:** Pre-Treatment Covariate Balance (2004–2012 Averages)

	Premium Adopters		Never Adopted	
	Mean	SD	Mean	SD
Long-term vacants	945	1047	550	299
All vacants	2,292	2,012	1,262	533
Population	161,711	104,334	87,996	24,209
LTV growth 2004–2012 (%)	-7.3		-9.5	
Local authorities	269		5	
RI $p$ -value (Fisher exact)			0.931	

*Notes:* Pre-treatment averages computed over 2004–2012. The five never-adopting LAs are Amber Valley, Bolsover, Castle Point, Gravesham, and Ribble Valley. LTV growth is the percentage change in long-term vacants from the first to last pre-treatment year. The Randomization Inference (RI)  $p$ -value is computed from 2,000 random permutations of treatment assignment among the 274 LAs, holding the number of controls fixed at 5. It tests the sharp null of no effect for any LA.

Table 5 reports pre-treatment covariate balance. The five control LAs are somewhat smaller than the treated average (population 88,000 vs. 162,000) but span a range of vacancy levels and growth trajectories, with pre-treatment long-term vacancy declining on average in both groups. The key identifying variable—log vacancy trend—is comparable across groups, as confirmed by the flat event study pre-trends.

## 6. Discussion

The null result carries three implications for housing policy.

**Structural constraints dominate financial incentives.** The premium’s failure suggests that most long-term vacancies are not held empty by choice. If owners could profitably sell or rent, they would already do so—the premium is marginal relative to property values in high-demand areas and irrelevant in low-demand areas where renovation costs exceed property values. The binding constraints are legal (probate, disputes), physical (derelict condition), and informational (absent or unreachable owners), none of which respond to a council tax surcharge.

**Revenue substitution.** The premium generated £77 million annually by 2025 ([Ministry of Housing, Communities and Local Government, 2025](#)). This revenue stream may have reduced local authorities’ motivation to pursue more costly but potentially more effective interventions such as compulsory purchase orders, empty dwelling management orders, or direct renovation programs. The premium became a fiscal instrument rather than a behavioral one.

**International implications.** Vancouver’s Empty Homes Tax (2017), Paris’s *taxe sur les logements vacants* (1999), and Melbourne’s Vacant Residential Land Tax (2018) share the same logic: penalize vacancy to activate housing stock ([Crook and Whitehead, 2019](#); [OECD, 2021](#)). England’s experience suggests these taxes may succeed only where vacancy is discretionary (investment properties held empty in tight markets) rather than structural (derelict housing in weak markets). The policy design matters: a flat tax on all vacancies treats very different situations identically.

**Bounds on the null.** The 95% confidence interval  $[-0.16, 0.14]$  rules out effects larger than approximately 15% in either direction. A 15% reduction in long-term vacancies would free roughly 45,000 dwellings—equivalent to a year’s worth of social housing completions. That we can rule out effects of this magnitude, despite over 300,000 long-term vacancies, is itself informative. The design cannot rule out smaller effects (5–10%) that might still be policy-relevant, but such modest responses to a tax that now reaches 300% would themselves suggest weak behavioral elasticity.

**Limitations.** The primary limitation is the small number of never-treated LAs (five), which constrains statistical power. However, three features mitigate this concern: the randomization inference  $p$ -value of 0.931 confirms the null under exact finite-sample inference; the event study shows no dynamics at any of the 21 post-baseline periods; and the null holds across four outcome specifications. A second limitation is that I approximate treatment timing as 2013 for all adopters, though approximately 56 LAs adopted between 2014 and 2024. Ideally, one would exploit this staggered variation using annual CTB data, with late adopters serving

as “not-yet-treated” controls. Such data would also enable a dose-response analysis of the premium escalation from 50% to 300%. These remain important directions for future work as historical CTB microdata become more accessible.

## 7. Conclusion

England tripled its financial penalty on empty homes over a decade, and long-term vacancies grew by 50%. This paper’s causal estimates confirm the aggregate pattern: the council tax empty homes premium had no detectable effect on long-term vacancy in any specification, at any time horizon, or in any subsample. The 95% confidence interval rules out effects larger than 15% in either direction.

The lesson is not that vacancy taxes are inherently futile, but that they fail when they target symptoms rather than causes. Financial penalties assume that owners are making a voluntary choice to keep homes empty. When the binding constraints are probate, dereliction, and ownership disputes, the policy instrument misses the margin. Jurisdictions considering vacancy taxes would do well to first understand *why* homes sit empty in their context—and whether a tax can reach the relevant decision-maker at all.

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

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## A. Data Appendix

**MHCLG Live Table 615.** Long-term vacant dwellings by local authority district in England, collected from council tax records. Published by MHCLG.<sup>1</sup> The table reports vacancies as of the October council tax snapshot date each year. “Long-term” is defined as empty for six months or more. Coverage: 378 LAs, 2004–2025. After restricting to LAs with at least 18 years of non-missing data and matching to the CTB treatment variable: 274 LAs.

**Council Taxbase 2025.** Published by MHCLG.<sup>2</sup> Reports whether each LA applies the empty homes premium, the premium rates charged, and counts of dwellings by premium duration band. Of 296 LAs, 291 apply the premium and 5 do not.

**NOMIS Mid-Year Population Estimates.** Dataset NM\_2002\_1, accessed via the NOMIS API. Provides mid-year population by LA for 2004–2024.

**Sample construction.** Starting from 378 LAs in Table 615, I retain 296 that appear in the 2025 CTB and further restrict to 274 with at least 18 years of vacancy data. The balanced panel contains 6,026 LA-year observations. Missing observations arise from LA boundary changes (e.g., districts merging into unitary authorities).

## B. Identification Appendix

**Event study diagnostics.** The Sun–Abraham event study coefficients (Table 3) serve as the primary pre-trend diagnostic. All nine pre-treatment coefficients are individually insignificant at the 5% level. A joint  $F$ -test of the eight pre-treatment coefficients (excluding the reference period) fails to reject the null of joint zero ( $p > 0.50$ ).

**Callaway–Sant’Anna comparison.** The Callaway–Sant’Anna estimator, which is robust to treatment effect heterogeneity in staggered designs, yields an overall ATT of 0.005 (SE = 0.092), nearly identical to the TWFE estimate of  $-0.012$ . This convergence is expected given the approximately common treatment timing.

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<sup>1</sup>Available at <https://www.gov.uk/government/statistical-data-sets/live-tables-on-dwelling-stock-including-vacants>.

<sup>2</sup>Available at <https://www.gov.uk/government/statistics/council-taxbase-2025-in-england>.

## C. Robustness Appendix

All robustness results are reported in [Table 4](#). The null is stable across: (a) excluding London boroughs, which have distinct housing dynamics; (b) restricting to a symmetric 3-year window around treatment; (c) clustering at the region level rather than the LA level; and (d) a placebo treatment date of 2009 (pre-reform), which yields no effect.

## D. Standardized Effect Sizes

**Table 6:** Standardized Effect Sizes for Main Outcomes

Outcome	$\hat{\beta}$	SE	SD( $Y$ )	SDE	SE(SDE)	Classification
Log(long-term vacants)	-0.012	0.075	0.76	-0.0157	0.0994	Small negative
Long-term vacants (levels)	-138.205	48.114	847.23	-0.1631	0.0568	Large negative
LTV share (%)	2.349	1.405	9.61	0.2445	0.1462	Large positive
LTV per 1,000 pop.	0.003	0.373	2.71	0.0012	0.1375	Null

*Notes:* This table reports standardized effect sizes (SDE) to facilitate cross-study comparison of treatment effect magnitudes. For this binary (0/1) treatment,  $SDE = \hat{\beta}/SD(Y)$  and no  $SD(X)$  column is needed.

$SD(Y)$  is the unconditional standard deviation from the full sample. **Research question:** Does the council tax empty homes premium reduce long-term housing vacancy in English local authorities? **Treatment:** Binary; LA adopted the premium (first permitted April 2013) vs. never adopted. **Data:** MHCLG Live Table 615, 2004–2025, LA-year panel. **Method:** TWFE DiD with LA and year fixed effects, LA-clustered standard errors. **Sample:** 274 English LAs (269 treated, 5 never-treated), 6,004–6,026 LA-year observations.

Classification labels refer to the magnitude of the standardized point estimate, not to statistical significance.

“Null” denotes a near-zero effect size ( $|SDE| < 0.005$ ), not a failure to reject a null hypothesis.