



Taxing Second Homes: Recorded Stock Responses and Tax-Base Reclassification under Council Tax Premiums in England

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Taxing Second Homes: Recorded Stock Responses and Tax-Base Reclassification under Council Tax Premiums in England

Abstract

Purpose: This study examines the causal effects of the effects of England's second-home council tax premium, effective from April 2025, on the recorded number of second homes.

Design/methodology/approach: Using Council Taxbase returns for 2019-2025 and a panel of billing-authority adoption dates, the study applies the Callaway and Sant'Anna difference-in-differences estimator and assesses robustness to alternative estimands, spatial correlation, and deviations from the parallel trends assumption.

Findings: Adoption is associated with a 6.6% decline in the average recorded second-home rate among adopting authorities in the first year of charging. The estimate is stable across comparison groups and functional forms, and remains robust to smooth departures from parallel trends, although it attenuates when anticipation is allowed and is not statistically significant when authorities are weighted by dwelling stock. No evidence suggests a net increase in recorded empty properties sufficient to offset the decline.

Research limitations/implications: The evidence is limited to a single charging year and a single treated cohort. Available data cannot yet distinguish genuine change in use from reclassification onto the non-domestic self-catering base, which is identified as the priority for further work.

Practical implications: The premium functions as a broad-based local revenue instrument. A decline in the recorded second-home count should not be interpreted as an increase in housing returning to permanent occupation, and authorities should monitor shifts between the domestic and non-domestic tax bases.

Originality/value: The paper offers the first causal evaluation of this policy and contributes a validated adoption panel that corrects errors in official and press classifications.

Keywords: second homes; council tax premium; property taxation; tax-base reclassification; difference-in-differences; housing affordability

JEL classification: H71, R31, H26, R38

1. Introduction

Housing markets across many developed countries have tightened over the past decade, and governments have increasingly responded not by increasing supply but by steering existing stock back towards permanent residents. Switzerland has restricted new second-home construction; Canada, Singapore, and China have taxed or restricted foreign and multiple-property buyers; and many European and North American cities have taxed short-term rentals and vacant dwellings. These measures differ in design but share a common aim: to discourage non-primary use of housing where residents face acute affordability pressures (Belgodère and Casamatta, 2026; Hilber and Schöni, 2020; Segù, 2020). Great Britain has moved decisively into this space. Under the Local Government Finance Act 1992, as amended by the Levelling-up and Regeneration Act 2023, English billing authorities have been able to levy a premium of up to 100% on furnished dwellings that are not a sole or main residence since 1 April 2025. The Council Taxbase adoption flag records 211 of 296 billing authorities charging the premium in its first year, with further cohorts in 2026 and 2027. Wales has held comparable powers since 2017, raising its maximum premium to 300% in April 2023, and Scotland introduced discretionary premiums in April 2024.

The policy rests on the promise that taxing second homes will return dwellings to permanent occupation and ease affordability. That promise, in turn, rests on an untested assumption that owners will sell or convert properties to primary use rather than find ways to avoid the charge. Avoidance is not a marginal worry here. A dwelling's status as a second home is an administrative classification rather than a directly observed fact. Accordingly, a premium can prompt owners or authorities to re-designate properties with no change in use, whether as a main residence, as self-catering accommodation on the non-domestic rating list, or as an empty dwelling. In each case, the recorded number of second homes falls, although the housing available to residents remains unchanged. Distinguishing genuine reallocation from administrative relabelling is therefore the first-order question in evaluating this policy (Belgodère and Casamatta, 2026; Shao and Gao, 2025).

To our knowledge, this paper provides the first causal evidence on second-home council tax premiums in England. We assemble a local-authority panel of recorded second homes, empty properties, and dwelling stock from Council Taxbase returns for 2019 to 2025, hand-code the

adoption cohort and announcement date for each English billing authority, and estimate the first-year effect using the staggered difference-in-differences (DiD) estimator of Callaway and Sant’Anna (2021), treating 2026 and 2027 adopters as not-yet-treated controls. Adoption is associated with a decline of roughly 0.9 per 1,000 dwellings in the recorded second-home rate of the average adopting authority, about 6.6% relative to the 2025 cohort's pre-treatment mean, with no offsetting rise in recorded empty properties. The estimate is robust across specifications, though its magnitude and interpretation are bounded by issues we set out in Section 4. These findings parallel and extend those of Belgodère and Casamatta (2026) on France's municipal surcharge, which they attribute most of the decline to strategic reclassification rather than genuine changes in occupancy.

This study’s contribution is threefold. First, we provide the first causal evidence for England, whose council-tax institutions, near-universal adoption, and statutory twelve-month notice differ materially from the French setting. Unlike the one-off transaction taxes that dominate the empirical literature on housing-tax responses (Agarwal et al., 2020; Best and Kleven, 2018; Kopczuk and Munroe, 2015), England’s system involves a recurring tax. It is therefore closer to the question at the centre of optimal property-tax analysis (Balke et al., 2020; Best and Kleven, 2018; Kopczuk and Munroe, 2015). Second, we identify a reclassification margin with no counterpart in France. Specifically, this involves the movement of dwellings between the domestic council-tax base and the non-domestic business-rates base as self-catering accommodation. We find no net increase in recorded empty properties sufficient to offset the decline. We also document substantial national growth in the self-catering rating stock. A local-authority test of this margin remains an open question pending the requisite data. Third, we build and validate a dated, sourced adoption panel for every English billing authority, correcting errors in the administrative flag and published press compilations, and provide a resource for later research.

2. Institutional background and related literature

2.1 Policy timeline and the no-anticipation assumption

The ability to charge a second-home premium and the decision sequence behind it are important for identification because owners and authorities could respond once the policy becomes foreseeable, not only when charging actually starts. In England, the enabling power was introduced

through the *Levelling-up and Regeneration Bill*, presented to Parliament in May 2022 and receiving Royal Assent as the *Levelling-up and Regeneration Act 2023* in October 2023. The Act permits, but does not require, a billing authority to charge a premium of up to 100% on dwellings that are furnished but not a sole or main residence. It also requires the authority to make its determination at least one year before the beginning of the financial year to which the premium relates. Authorities intending to charge on 1 April 2025 therefore had to make that decision by the end of March 2024. In practice, most signalled their intention through budget-setting documents in 2023 and early 2024.

Three implications follow for the study design. First, the treatment date (the first charging year) is when the premium is levied, not when it was first announced. The statutory twelve-month notice made the policy foreseeable to owners in advance. Second, any behavioural response during the notice window is captured in the pre-treatment period rather than in the first year of the premium. Therefore, our first-year estimate captures the adjustment that remains after this anticipatory response. Third, because the 2026 cohort were in their own determination-and-notice window in 2025, using them as not-yet-treated controls could understate the contemporaneous response; we return to these issues in Sections 4.2 and 4.3, where we explicitly allow for anticipation.

2.2 The UK second-home premium in comparative context

The English premium belongs to a well-defined international family of use-based housing interventions, all of which tax or restrict non-primary use of dwellings. Two broad rationales support taxing housing on this basis. The first is efficiency. The relatively inelastic demand for and supply of housing imply that recurrent taxes on housing are comparatively less distortionary than taxes levied on more responsive tax bases (Arnold et al., 2011; Balke et al., 2025). The quantitative public-finance literature suggests that the welfare-optimal long-run property tax may lie well above current levels (Balke et al., 2025; Eerola and Määtänen, 2013), with the case strongest for the least-mobile component, land (Bonnet et al., 2021). The second rationale is allocative. Housing usually enjoys preferential tax treatment relative to other assets, and reducing that preference alters tenure, prices, and housing composition (Gervais, 2002; Sommer and Sullivan, 2018). Second-home premiums draw on both rationales, raising revenue from a lightly taxed, immobile base while pushing dwellings towards permanent occupation. A broader literature documents the economic effects of second homes across tourism and gateway regions, from local

price and employment pressures to planning and amenity trade-offs (de Oliveira et al., 2015; Marcouiller et al., 2013), helping to explain the affordability concern underlying the English reform.

A causal precedent that specifically addresses second homes is Hilber and Schöni (2020). They treat the 2012 Swiss Second Home Initiative, a constitutional ban on new second-home construction in municipalities where second homes exceed 20% of the housing stock, as a quasi-natural experiment. Using a general-equilibrium framework in which primary and second homes are poor substitutes, they find that the ban reduced primary-home price growth by about 15%, increased second-home price growth by about 26%, and raised local unemployment growth. Although their study examines a construction ban rather than a tax and focuses on prices and labour markets rather than the recorded stock, it provides the poor-substitutes framework and the political-economy backdrop for our analysis.

2.3 Taxing under-used housing: vacancy, second homes and compliance

Another piece of evidence on a tax targeting under-occupied housing is Segù (2020), who evaluates France’s vacant-dwelling tax using a DiD design and finds that it reduced the vacancy rate. The most direct precedent is Belgodère and Casamatta (2026), whose analysis of the French second-home surcharge aligns with our policy type, estimator family, headline magnitude, and compliance concern. Their advantage is dwelling-level transition microdata, which allows them to observe status changes without ownership transfers. On that basis, they attribute most of the recorded decline to relabelling. From a theoretical perspective, Shao and Gao (2025) model how second-home taxes generate endogenous avoidance and how suppliers incorporate that avoidance into prices, formalising the intuition that a tax’s recorded effect may not match its real effect.

A parallel body of literature on empty homes and vacancy taxation reaches much the same conclusion about their limited real effects. Evidence on Vancouver’s empty-homes tax shows reductions in recorded vacancy but no effect on rents (Caracciolo and Miglino, 2024). Notably, the Vancouver evidence rests on the explicit assumption that owners’ status declarations are truthful, an assumption the authors themselves flag as impossible to verify. That is precisely the compliance concern at the heart of our own design. These studies motivate examining several administrative margins jointly rather than the second-home count in isolation.

2.4 Behavioural and compliance responses to housing taxes

Related literature indicates that households respond strongly to housing taxes at the margin, often through avoidance, misreporting, or timing rather than genuine reallocation. In the United Kingdom, Best and Kleven (2018) show that UK property owners respond strongly and strategically to property-tax thresholds. Besley et al. (2014) and Kopczuk and Munroe (2015) document bunching and incidence responses to transfer taxes in the UK and US. In China, Agarwal et al. (2020) find that a rise in capital-gains taxes on housing produced only a small real response in holding periods but a large increase in price under-reporting, so that the administrative response far outpaced the behavioural one. This pattern, in which a housing tax moves recorded outcomes more than underlying behaviour, is what our model is intended to detect.

2.5 Related instruments: foreign-buyer, purchase-restriction and short-term-rental taxes

Broader evidence on taxes and restrictions targeting specific sub-uses of housing repeatedly shows that such measures reallocate activity across differently taxed categories more readily than they shift aggregate outcomes. On foreign-buyer and purchase-restriction taxes, Favilukis and Van Nieuwerburgh (2021) show that out-of-town buyers affect city welfare through channels that a simple purchase tax only partly captures. Somerville et al. (2020), using a within-market design, find that purchase restrictions in China shifted the composition of activity rather than its level. Broader reviews of foreign-buyer taxes report mixed and often modest price effects (Du et al., 2022; Hartley et al., 2024) and, more directly, effects on affordability (Pavlov et al., 2023).

Furthermore, the short-term rental literature identifies the reallocation of housing between long-term and short-term rental markets as an important behavioural margin through which regulatory interventions affect local housing markets (Koster et al., 2021). Garz and Schneider (2023) show that a short-term rental tax changes behaviour mainly when enforcement makes reclassification costly. Dejean et al. (2025) also find that the effect of mandatory short-term-rental registration in France depends on the extent of enforcement. The recurring theme that a housing-use tax's effect is blunted by movement into adjacent, differently taxed categories motivates our focus on the margins along which second homes are reclassified: the empty-property category, which we test directly, and the non-domestic self-catering list, which we identify as the key unresolved channel.

3. Data and empirical strategy

3.1 Data

Our main data source is the annual Council Taxbase (CTB) dwelling stock return, published as accredited official statistics by the Ministry of Housing, Communities and Local Government (MHCLG, 2025). All outcomes are based on a single early- October snapshot return. On that form, exempt dwellings (line 2), chargeable dwellings (lines 4 and 7), second homes (line 11), and empty properties (lines 12 to 15) are recorded sequentially, with second homes and empty properties defined as subsets of the same chargeable base. Line 11 is classified identically across all years of our sample (2019-2025), so the second-home count is measured consistently rather than drifting with recording practices; the only change over the period is the premium sub-line (11c), introduced with the policy in 2025. For the years 2019 to 2025, we analyse data at the local-authority level, focusing on recorded second homes (furnished dwellings that are not primary residences), vacant properties, and overall dwelling stock. We calculate second-home and vacancy rates per 1000 chargeable dwellings. England-level totals match the published figures exactly, with 253,510 second homes in 2019 and 267,894 in 2025. One distinction matters for interpretation, and the official statistics make it explicit: the number of dwellings recorded as second homes (268,000 in 2025) is larger than the number actually charged the premium (170,000), because exemptions, exception classes, and the statutory notice period mean that not every recorded second home attracts the charge. Our outcome is the recorded count, which is the relevant object for studying reclassification. We use the published local authority tables rather than the unavailable property-level records; therefore, our analysis is conducted at the authority level throughout. Local authority boundaries shifted over the period, most notably in the 2019, 2021, and 2023 reorganisations that created unitary authorities in Buckinghamshire, Northamptonshire, North Yorkshire, Somerset, Cumbria, and elsewhere. We harmonise every year to 2025 Government Statistical Service codes using a documented crosswalk based on ONS local government restructuring records and GSS code histories.

The CTB records domestic dwellings from an October snapshot; the non-domestic rating list, which holds self-catering and other business-rated properties, is a separate register maintained by the Valuation Office Agency (VOA) and measured on 31 March. The two are distinct legal registers with different units of assessment (a council-tax dwelling versus a non-domestic hereditament), different valuation bases (council-tax band versus rateable value), and reference

dates six months apart; a property appears on one register or the other, not both. No common property identifier links an individual dwelling across the two lists in the published data, so property-level matching is not possible; nor can their counts simply be added, because a dwelling moving from the domestic to the non-domestic list leaves one register and enters the other at a different reference date. We therefore analyse the council-tax base directly and treat movement onto the non-domestic list as an external margin we characterise nationally but cannot yet track locally. This also qualifies our recommendation that authorities monitor the two bases jointly, which would require a linked register that does not currently exist in published form.

Treatment is defined by whether and when an authority adopts the second-home premium. Based on council determinations and budget documents, we record each English billing authority's adoption cohort (2025, 2026, 2027, or never) and its announcement date. This dated, sourced panel corrects two errors in existing sources. The first concerns the CTB adoption flag, which records 211 first-year adopters but misrecords South Tyneside, whose own determination confirms a 100% premium from April 2025. The second concerns published press compilations, which misclassified the cohorts of at least four authorities. Throughout, our coding rule treats the 212 authorities for which council evidence confirms a premium from April 2025 as the treated cohort, and we report results that exclude South Tyneside as a robustness check. These 212 authorities hold roughly 88% of England's recorded second-home stock (235,784 dwellings), compared with about 3.2% (8,514 dwellings) in the 34 clean, never-adopting authorities. Table 1 and Figure 1 document this concentration, which is central to our choice of comparison group. The 291 authorities in Table 1 (212 first-year adopters, 36 in 2026, 9 in 2027, and 34 never-adopters) fall short of the 296 English billing authorities by five (North East Lincolnshire, North Hertfordshire, Nuneaton and Bedworth, Runnymede, and Wyre Forest), which we exclude from the clean cohort classification because their determinations are withdrawn, legally irregular, or unresolved.

Table 1. about here

Figure 1. about here

3.2 Empirical strategy

We estimate the premium's first-charging-year effect using the staggered difference-in-differences estimator of Callaway and Sant'Anna (2021), which suits our setting for two reasons.

Adoption is staggered across the 2025, 2026, and 2027 cohorts, and treated and untreated authorities differ sharply in baseline second-home levels, a pattern that two-way fixed effects handle poorly. Although we describe the design as staggered, only the 2025 cohort has a post-treatment observation in the current data; the 2026 and 2027 cohorts function solely as controls, so the reported effect is effectively a comparison of the 2025 cohort against later- and never-adopting authorities. The full staggered structure will become available only as later cohorts accrue post-treatment years. The estimator computes group-time average treatment effects using not-yet-treated authorities as the comparison group, then aggregates them into an overall average treatment effect on the treated (ATT). We report estimates for two comparison groups: not-yet-treated controls, preferred because later-adopting authorities resemble treated authorities and differ mainly in timing, and never-treated controls. Agreement between the two serves as evidence against a comparison-group artefact. Standard errors are obtained via the multiplier bootstrap and are clustered at the local-authority level.

Let $Y_{i,t}$ denote the recorded second-home rate for authority i in year t , and let G_i denote the first year in which authority i charges the premium (its cohort), with $G_i = 0$ for never-adopters. The building block is the group-time average treatment effect,

$$ATT(g,t) = E[Y_t - Y_{g-1} | G = g] - E[Y_t - Y_{g-1} | G \in C_{g,t}]$$

which is the difference between the change in the outcome from the last pre-treatment year ($g-1$) to year t for cohort g and the corresponding change for a comparison set $C_{g,t}$. Under the not-yet-treated definition, $C_{g,t}$ comprises authorities not yet treated by t (those with $G > t$); under the never-treated definition, it comprises authorities with $G = 0$. Identification requires conditional parallel trends: absent the premium, the expected change in the outcome from $g-1$ to t would have been equal for cohort g and its comparison group. Our headline is the cohort-size-weighted aggregate over first charging years, in which $P(G = g | G > 0)$ weights each cohort by its size,

$$ATT_{overall} = \sum_g P(G = g | G > 0) \times ATT(g,g)$$

The dynamic (event-study) coefficients are the analogous averages at event time $e = t - G_i$, where the first charging year (2025) is event time 0 and 2024 is the last pre-treatment year ($e = -1$). We use the universal base period, which normalises 2024 to zero and holds this reference fixed across all event times; the overall ATT is invariant to this choice. We normalise to 2024

because it is the last observation before the premium is levied and therefore the natural comparison point for a first-year effect. Because 2024 may itself lie within the announcement window, we do not treat it as a guaranteed clean pre-period: we also estimate an anticipation-adjusted specification that effectively takes 2023, the year of Royal Assent and before most determinations, as the last clean reference. That specification (Figure 4, Section 4.3) is the more conservative timing test, and we weigh it alongside the 2024-normalised event study when interpreting the pre-treatment path.

Identification therefore hinges on parallel trends: without the premium, treated and comparison authorities would have followed common paths in the recorded second-home rate. We probe this in three ways: the event study, the anticipation-adjusted specification (Section 4.3), and a placebo test that assigns a counterfactual treatment date of 2023. We also apply the same specification to a second outcome, the empty-property rate, to assess whether any decline in recorded second homes is matched by a rise in recorded empty properties, the most immediate form of administrative reclassification. Because adopting authorities have much higher second-home density than non-adopters, we do not reweight to a common support; with so little overlap, entropy balancing collapses onto a handful of control authorities and tells us little. Instead, the analysis relies on the stability of the Callaway and Sant'Anna estimate across comparison-group definitions to address concerns about baseline-level differences.

A natural concern is that adoption is itself endogenous: authorities experiencing rural decline, for example, might be systematically less likely to charge the premium, making treated and comparison authorities non-comparable. We assess this directly. Merging the Office for National Statistics 2011 Rural-Urban Classification and pre-policy population change (2011 to 2022) onto the full panel of 294 classified authorities, we find that adoption is near-universal (87%) and not significantly predicted by either characteristic. In a linear probability model, neither the ordinal rural-urban score ($p = 0.17$) nor population change ($p = 0.35$) is significant; the result is unchanged under a binary rural indicator ($p = 0.73$) and a logit specification, and the two covariates are jointly insignificant. Because adoption is so widespread, there is little scope for selection on these observables, and the estimator further conditions on baseline second-home levels. We therefore find little evidence that the endogeneity of adoption drives the results, though we cannot rule out selection on unobservables.

Table 2 summarises the specification in compact form before we turn to the estimates.

Table 2. about here

4. Results

4.1 The recorded stock of second homes

Table 3 reports the overall effect. In the first charging year, adoption of the premium is associated with a decline of about 0.90 per 1,000 dwellings in the recorded stock of second homes. The interval is negative throughout, with only a narrow exclusion of zero. Set against the 2025 charging cohort's pre-treatment mean of 13.70 second homes per 1,000 dwellings, this represents a first-year decline of about 6.6%. The estimation sample includes the 2025, 2026, and 2027 adoption cohorts, but only the 2025 cohort contributes treated post-policy observations in the 2019-2025 panel; the later cohorts enter as not-yet-treated controls. For context, MHCLG (2025) reports that the recorded number of second homes in England fell by 4.3% nationally between 2024 and 2025, from 280,000 to 268,000. Our causal estimate, identified by comparing adopting authorities with not-yet-treated controls, is larger than the national aggregate, which we would expect if the decline concentrates where the premium was actually charged. The magnitude is economically modest. Doubling the council tax bill on affected dwellings shifts the recorded stock by less than one dwelling per thousand in the first year, consistent with a relatively modest short-run adjustment in the recorded second-home stock over this horizon.

Table 3. about here

4.2 Estimate across control group, coding, and sample

Table 4 summarises the robustness of the second-home estimate across three dimensions. The first concerns whether adopters differ systematically from non-adopters, or whether a single large authority drives the result. Here, the estimate is reassuringly stable: it is identical across the not-yet-treated and never-treated comparison groups (both -0.903), changes only slightly when South Tyneside is dropped, and remains stable after removing each of the five largest-stock authorities in turn. Dropping Westminster, an outlier with a high recorded second-home count and an atypical

trajectory, actually strengthens the effect to -1.20 , indicating that the headline is conservative and is certainly not an artefact of a single authority. The control-group sensitivity observed under naive two-way fixed-effects and doubly-robust two-period specifications does not carry over to the staggered estimator, confirming that the earlier instability was an artefact of the estimator.

The second and third dimensions are more demanding, and we report them plainly because they set the limits of what a single post-treatment year can establish. Under a varying ($g-1$) base period rather than the universal one, the point estimate remains unchanged (-0.903), but the confidence interval just includes zero ($p = 0.052$). The magnitude of the effect is unchanged, but its statistical significance depends on the choice of reference period. More substantively, when we allow for one period of anticipation (in which the estimator treats the year before charging as potentially already affected), the first-year ATT falls to -0.35 and is no longer distinguishable from zero ($p = 0.42$). This is evidence about timing rather than about an effect: given the twelve-month notice, part of the adjustment falls within the pre-treatment period, so the charging-year estimate captures the response remaining after any anticipation, and the two cannot be separately identified with a single post-year.

Table 4. about here

Because treatment and second-home stock are strongly clustered geographically, we assess whether spatial correlation across neighbouring authorities affects inference. On the treatment-year cross-section (289 authorities: 210 treated, 79 control), the first-difference estimate reproduces the headline (-0.914 ; HC1 standard error 0.371 ; $p = 0.014$). Conley (1999, 2008) spatial-HAC standard errors, computed with a Bartlett kernel at distance cutoffs from 50 to 200 km, are no larger than this baseline, and the effect remains significant throughout ($p = 0.010$ at 50 km, falling below 0.01 at wider cutoffs). Spatial correlation therefore does not weaken inference; we report this as a stress test on the cross-section rather than as a replacement for the staggered estimate.

The headline reports second homes per 1,000 dwellings, with authorities treated as equal units, so it estimates the effect on the average adopting authority rather than the change in England's total stock. We therefore report alternative estimands. A log specification implies a semi-elasticity of about 8% (-0.086 , $p = 0.037$), and a fixed 2024 dwelling denominator leaves the estimate essentially unchanged (-0.876 , $p = 0.036$). When authorities are weighted by dwelling stock,

however, the estimate falls to -0.776 and is no longer significant ($p = 0.197$); a second-home-stock-weighted estimate, a distinct estimand answering a different question, is larger (-2.424 , $p = 0.019$). The effect is thus robust to functional form but sensitive to population weighting. Accordingly, we frame the headline in terms of the effect on the average adopting authority and do not interpret it as a precise England-wide aggregate.

4.3 Parallel trends

Figure 2 plots the event-study coefficients. Under the universal base period, which normalises the year before charging (2024) to zero, the coefficients in the two periods immediately before treatment are negative and statistically significant (-0.49 at event time -3 , $p = 0.03$; -0.62 at event time -2 , $p = 0.001$). Because 2024 may itself contain anticipatory responses, the universal-base event study should be read as a diagnostic relative to the pre-treatment year rather than as evidence based on an untreated reference period. The anticipation specification therefore provides the more conservative timing test. Relative to its 2024 level, the treated cohort's recorded second-home rate was somewhat higher in the two preceding years. The raw series in Figure 3 rises through much of the pre-period before falling between 2024 and 2025, so the concern is one of relative pre-trends against the comparison groups rather than a simple decline in treated levels.

Figure 2. about here

Figure 3 plots the raw group-mean rate for each cohort and shows the same pattern in levels: the four cohorts sit at very different baselines, and the treated cohort declines in 2024-25, whereas the never-adopters do not. We do not treat this as a clean pre-period; instead, we assess how much it directly threatens the first-year estimate rather than setting it aside. A placebo treatment assigned to 2023, ahead of any announcement, yields a null effect (0.18 , $p = 0.27$), so the pre-treatment drift does not, by itself, generate a spurious treatment-year jump. Specifically, we apply the sensitivity analysis of Rambachan and Roth (2023), which asks how large a post-treatment violation of parallel trends would have to be, relative to the pre-treatment violation we observe, for the first-year effect to be indistinguishable from zero. Under a smoothness restriction, the effect remains negative and excludes zero across the full range we examine (deviations from a linear counterfactual trend up to $M = 0.6$ per 1,000). This conclusion holds under both the fixed-length

confidence interval and the more conservative conditional least-favourable construction. Under the more agnostic relative-magnitude restriction, the effect is distinguishable from zero for post-treatment violations up to roughly 0.3 times the largest pre-treatment violation, and loses significance beyond that.

Figure 3. about here

Table 5 reports both restrictions in full. The first-year estimate is robust if one assumes the pre-existing trend would have continued smoothly rather than reversed sharply, and more fragile if one allows large, arbitrary departures. We regard the smoothness benchmark as more plausible, given the shape of the pre-period, but report both transparently. Because the universal base period normalises 2024 to zero, all pre-treatment coefficients are therefore measured relative to 2024. A joint Wald test rejects the null of parallel pre-trends ($p = 0.022$), consistent with the significant coefficients at $e = -2$ and $e = -3$. We therefore do not rely on parallel trends for identification, but assess the robustness of the first-year effect to their violation using Rambachan and Roth's (2023) sensitivity analysis.

Table 5. about here

To assess the timing of the response, Figure 4 re-estimates the event study with one period of anticipation, treating 2024 as potentially already affected and making 2023 the last clean pre-treatment reference. Two features stand out. First, the pre-treatment coefficients are individually indistinguishable from zero and show no systematic trend, in contrast to the main specification; this is consistent with the pre-trend documented above, which reflects anticipatory movement in 2024 rather than a long-run differential trend between treated and comparison authorities. Second, the first-charging-year coefficient is correspondingly smaller (about -0.35) and no longer distinguishable from zero, consistent with part of the response occurring during the twelve-month notice period rather than in the charging year itself. Figure 4 supports the timing interpretation but does not resolve the parallel-trends concern: the joint pre-test still rejects under the main specification, and the two specifications differ mainly in how they handle the potentially contaminated 2024 reference.

Figure 4. about here

4.4 The decline is not reclassification into empty properties

If the recorded decline in second homes were simply a reclassification into the empty-property category, it would appear as a rise in the number of recorded empty properties. It does not. The second row of Table 3 reported the effect of adoption on the empty-property rate: an ATT of +0.08 per 1,000 dwellings, which is statistically indistinguishable from zero and is stable across the same robustness checks as the headline. The fall in recorded second homes is not offset by a rise in empty properties. Figure 5 places the two event studies side by side: a recorded second-home step-down in the first charging year, while the empty-property effect at treatment is null. This informative null rules out the most mechanical form of reclassification and narrows the interpretation to three remaining channels: genuine change in use (sale or conversion to a main residence); reclassification into non-domestic self-catering (business rates) status; and administrative reassessment, whereby the introduction of a premium prompts authorities to audit and correct their council-tax records, so that a dwelling leaves the recorded second-home count through data cleaning rather than any change in owner behaviour. Because the dependent variable is itself an administrative classification, this last channel is particularly relevant and cannot be separated from genuine change with the present data.

Figure 5. about here

4.5 Interpreting the decline: remaining channels

With the empty-property route excluded, the recorded decline must reflect a combination of two broad responses, and distinguishing them is the central open question. The first is genuine change in use: an owner sells the dwelling or converts it to a main residence, so it leaves the second-home category through a real change in occupancy. The second is reclassification onto the non-domestic rating list as self-catering accommodation (VOA special category 131), which removes the dwelling from the council-tax base without any change in its use. Conversion to short-term holiday letting straddles the two: if a property is let for enough days to qualify, it moves onto the non-domestic list, and enters the business-rates margin.

We cannot separate these channels with the present data, as we observe authority-level counts rather than individual dwellings. Testing the business-rates margin would require a consistent

local-authority self-catering series linked to adopting authorities, and distinguishing sales from other exits would require dwelling-level transaction records; neither can be constructed from the published Council Taxbase tables, which report only authority totals. At the national level, the English self-catering stock rose from 50,960 in 2019 to 66,500 in 2025, consistent with some movement into business rates, but this cannot be attributed to the premium or localised to adopting authorities. We therefore qualify accordingly: a validated local-authority self-catering series and Land Registry transaction data would allow the disposal and holiday-let channels to be tested directly, and we identify both as priorities for further work.

The estimate reflects a change in a recorded administrative count, and the balance among sales, conversions, and re-designations behind that change cannot be determined from the current data.

5. Discussion and policy implications

This study assesses whether taxing second homes returns them to permanent use or alters how they are recorded. The evidence points to a genuine but modest reduction in the recorded stock of the average adopting authority, about 6.6% in the first charging year, stable across comparison group, sample, and coding. We show that it is robust to smooth departures from parallel trends but materially more fragile to unrestricted relative-magnitude deviations. At the same time, there is no offsetting rise in the number of recorded empty properties, so the adjustment does not run through the empty-property category.

These findings, in a separate setting, corroborate the evidence reported by Belgodère and Casamatta (2026) on France's municipal surcharge: a recorded decline of comparable magnitude, larger at higher rates and in tourist areas, with a central role for strategic reclassification. Using dwelling-level transition microdata, they attribute most of the French decline to owners re-designating their primary residence rather than to changes in actual occupancy. We cannot observe such transitions directly in the UK data, but our results are consistent with a similar mechanism operating outside the empty-homes list, most plausibly a mix of genuine change in use (sale or conversion to a main residence, as documented in high-premium Welsh authorities) and reclassification to non-domestic self-catering. The results echo broader evidence that housing-tax

responses often operate as much through avoidance and misreporting as through real reallocation (Agarwal et al., 2020; Best and Kleven, 2018).

If the objective is to expand housing available to permanent residents, the evidence counsels caution: the first-year quantity response is modest, and part of it may reflect reclassification rather than dwellings returning to the market. A fall in the recorded second-home count should not be assumed to translate one-for-one into homes for local residents. If the objective is instead to raise revenue from lightly used property with limited market distortion, a legitimate aim given fiscal pressures on English authorities, the findings read more favourably. The premium is levied on a large base (88% of England's recorded second homes), and the modest response within that base means revenue is relatively stable. This tension is sharpened by the incidence of property taxation over time. Quantitative work finds that higher property taxes are borne by current owners through immediate capital losses, while benefiting future cohorts (Balke et al., 2025). This helps explain why second-home premiums, concentrated on a politically vocal group of existing owners, are contested even where their revenue rationale is strong. The business-rates margin, which we cannot yet measure at the local-authority level, is where the clearest avoidance risk lies. Self-catering accommodation that meets the letting thresholds is assessed for non-domestic rates and may qualify for small-business relief, allowing owners to avoid the premium without changing use. Authorities and the VOA should therefore closely monitor movement between the domestic and non-domestic bases. The six-month tightening of self-catering eligibility in Wales, relative to England, provides a useful setting for studying this.

The fiscal scale of the policy is non-trivial. MHCLG reports that 170,299 second homes were charged the premium in 2025, compared with 267,894 dwellings recorded as second homes for council tax purposes. Using the 2025-26 average Band D council tax in England of £2,280, the mechanical gross premium-revenue exposure is approximately £388 million per year (at 100% premium); the total council tax bill on these dwellings, including the standard charge and premium, is roughly £777 million. This is a gross exposure, not net receipts. It ignores variations in local Band D rates, property-band composition, exceptions, and discounts, and uses the national Band D average. Because second homes are concentrated in shire and unitary authorities, where the average Band D price (£2,344 and £2,366, respectively, in 2025-26) exceeds the London figure of £1,982, a stock-weighted average across adopting authorities would, if anything, be higher. The

£388 million figure is therefore best read as an approximate static exposure, not realised revenue. It may understate gross exposure if second homes are concentrated in higher-tax authorities, but it may overstate receipts once band mix, exceptions, discounts, and behavioural responses are taken into account.

The behavioural response documented above also implies some erosion of the recorded second-home base, though the revenue implication is not mechanically equal to the estimated percentage decline. Some properties may leave the premium base by being reclassified onto the non-domestic rating list, where liability depends on rateable value and relief eligibility rather than disappearing from taxation altogether. And because English authorities had to announce the premium before charging it, the 2025 charged base may already embed anticipatory reclassification or changes of use. The figure should therefore be read as the static scale of the tax base exposed to the premium, not as an estimate of realised net revenue. Even so, it makes clear that the premium is a material local revenue instrument, and that its fiscal significance does not depend on its success as a housing-supply measure.

Several tax-design implications follow. First, a premium of this kind alters the recorded second-home stock, but only modestly in its first year, so it should not be relied on as a substantial short-run instrument for housing supply. Second, because the measured effect depends on the administrative boundary of the tax base, the design of that boundary matters as much as the rate: what counts as a second home, what qualifies for non-domestic self-catering treatment, and how the two lists are reconciled will shape both the behavioural response and the revenue. Third, local authorities and the VOA should jointly monitor council tax and non-domestic rating movements rather than in isolation, since a dwelling leaving the premium base may simply reappear on the rating list. Fourth, exemptions and business-rate relief can weaken the intended effect, both by narrowing the charged base and by offering a low-liability exit; tightening the self-catering letting thresholds, as Wales has done, is one lever. Taken together, these points suggest that, on current evidence, the premium is more defensible as a local revenue instrument levied on a broad and relatively inelastic base than as a large intervention in housing supply.

6. Conclusion

This study provides the first causal evidence on second-home council tax premiums in England. Exploiting the staggered adoption of England's 100% premium from April 2025 and a panel of

every billing authority's adoption cohort, we find that the premium is associated with a modest first-year decline in the recorded second-home stock of about 6.6% for the average adopting authority, with no offsetting rise in recorded empty properties. The result corroborates, in an independent institutional setting, the recorded stock decline documented by Belgodère and Casamatta (2026) for France, with reclassification as a live component of the measured response.

Several limitations shape the agenda for further work. We observe a single post-treatment year, so the estimate captures a first-charging-year effect and is silent on dynamics. The statutory twelve-month notice makes this more precise. Owners are warned a year in advance, so part of the response is anticipatory, and the estimate attenuates once anticipation is accounted for. Our charging-year figure thus captures the adjustment remaining after any response during the notice period, and separating anticipatory from post-charging responses will require additional years, which the 2026 and 2027 cohorts will enable.

The effect is also modest. Though stable across comparison groups, samples, and the exclusion of any single authority, it is estimated to have an upper confidence bound near zero, suggesting it crosses zero as the base period varies. It is early rather than definitive evidence.

Most importantly for the mechanism, we find no net increase in recorded empty properties sufficient to offset the decline. Future studies should test reclassification to non-domestic self-catering status that would allow a direct test of the business-rates 'flipping' margin and is the single most valuable extension.

Other extensions would sharpen the picture. Richer outcomes, including Land Registry transactions for the disposal channel, prices in high-second-home markets, and heterogeneity by second-home density and coastal location, would help apportion the decline between genuine change and relabelling. Furthermore, Wales, with a wider range of premium rates and a longer post-adoption window, offers scope for the continuous-treatment design that England's uniform rate and single post-year cannot yet support.

Responses may also vary with the distance between an owner's main residence and their second home, and with whether a property is in a tourism hotspot rather than a rural area, since proximity and local letting demand shape how easily a dwelling can be sold, re-occupied, or converted to short-term letting. The Council Taxbase records neither owner location nor these property attributes, so we flag this heterogeneity as a further avenue rather than one we can test here.

Data availability statement

The data that support the findings of this study are derived from public sources. Local-authority second-home, empty-property, and dwelling-stock counts are taken from the Ministry of Housing, Communities and Local Government Council Taxbase (CTB) returns for 2019-2025, published as accredited official statistics and openly available on gov.uk. The adoption panel (first charging year and announcement date for each English billing authority) was hand-coded by the authors from published council determinations and budget documents.

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Conflict of interest disclosure

The author(s) declare that they have no conflicts of interest.

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Table 1. Descriptive statistics by adoption cohort

Cohort	N	2nd homes /1,000, 2024 (SD)	Empty /1,000, 2024 (SD)	2nd homes /1,000: 2019/2024/2025	Total dwellings	Total 2nd homes 2025
2025 adopters	212	13.70 (21.87)	13.18 (4.33)	12.71 / 13.70 / 12.82	20,118,164	235,784
2026 adopters	36	6.42 (13.81)	13.69 (4.89)	5.64 / 6.42 / 6.42	2,537,928	17,980
2027 adopters	9	5.25 (3.44)	12.65 (5.71)	4.88 / 5.25 / 5.25	752,058	4,145
Never-adopters	34	4.30 (3.41)	11.82 (5.62)	3.88 / 4.30 / 4.34	1,982,370	8,514

Note: Rates are per 1,000 chargeable dwellings, with 2024 standard deviations in parentheses. The three-year sequence (2019, 2024, and 2025) reports the recorded second-home rate before the announcement window, at the last pre-charging year, and in the first charging year. Total dwellings and total second homes are cohort sums; all other columns are per-authority means. Source: MHCLG Council Taxbase and the authors' adoption panel.

Table 2. Summary of the estimation specification

Element	Specification
Estimator	Callaway and Sant'Anna (2021) staggered difference-in-differences (doubly robust)
Outcome	Recorded second homes per 1,000 chargeable dwellings (and, separately, empty properties per 1,000)
Unit and period	English billing authorities, annual, 2019-2025 (authority-level)
Treatment	First charging year of the premium (cohort G = 2025, 2026, 2027, or never)
Comparison group	Not-yet-treated authorities (preferred); never-treated as robustness
Estimand	Overall ATT = cohort-size-weighted average of first-charging-year ATT(g,g); with data to 2025 this is the 2025-cohort effect
Base period	Universal, normalising the year before charging (2024, $e = -1$) to zero; overall ATT invariant to this choice
Identification	Conditional parallel trends; probed by event study, 2023 placebo, and Rambachan and Roth (2023) sensitivity
Inference	Multiplier bootstrap, clustered by local authority; Conley spatial standard errors as robustness

Table 3. First-year effect of the premium

Outcome	ATT	SE	95% CI	p	Base	%	2025	Later	Never-
							effect treated	adopters	treated
Second homes / 1,000	−0.90	0.432	[−1.75, −0.06]	0.037	13.70	−6.6	212	45	34
Empty properties / 1,000	+0.08	0.38	[−0.66, 0.81]	0.836	—	—	212	45	34

Note: ATT under the Callaway-Sant'Anna estimator with not-yet-treated controls; standard errors clustered by local authority via the multiplier bootstrap. Base is the 2025 charging cohort's pre-treatment (2024) mean, and the ATT is expressed relative to it because only that cohort has entered its first charging year within the 2019-2025 panel. Columns give the three disjoint groups: 212 first-year (2025) treated authorities, 45 later adopters (36 in 2026, 9 in 2027) that enter as not-yet-treated controls, and 34 never-treated controls. The empty-property effect is a precise null; no base or percentage is reported for it, since a proportional effect would be meaningless.

Table 4. Robustness of the second-home ATT

Specification	ATT	SE	95% CI	p	2025	Later
					treated	adopters
Comparison group and coding						
All treated, not-yet-treated controls	−0.903	0.432	[−1.75, −0.06]	0.037	212	45
All treated, never-treated controls	−0.903	0.437	[−1.76, −0.05]	0.039	212	45
Excl. South Tyneside, not-yet-treated	−0.895	0.424	[−1.73, −0.06]	0.035	211	45
Excl. South Tyneside, never-treated	−0.895	0.422	[−1.72, −0.07]	0.034	211	45
Leave-one-out (largest-stock authorities)						
Excl. Cornwall	−0.904	0.426	[−1.74, −0.07]	0.034	211	45
Excl. Westminster	−1.198	0.222	[−1.63, −0.76]	<0.001	211	45
Excl. Kensington and Chelsea	−0.876	0.443	[−1.74, −0.01]	0.048	211	45
Excl. Tower Hamlets	−0.905	0.445	[−1.78, −0.03]	0.042	211	45
Excl. North Yorkshire	−0.888	0.418	[−1.71, −0.07]	0.034	211	45
Alternative modelling choices						
Base period: varying (g−1)	−0.903	0.465	[−1.82, 0.01]	0.052	212	45
One-period anticipation allowed	−0.354	0.435	[−1.21, 0.50]	0.417	212	45

Note: Callaway-Sant'Anna ATT on second homes per 1,000 dwellings; SEs clustered by authority, 95% CIs in brackets; 34 never-treated controls throughout. "2025 treated" counts first-year treated authorities; "Later adopters" (45: 36 in 2026 and 9 in 2027) enter as not-yet-treated controls; "Never-treated" controls number 34. The leave-one-out and South Tyneside rows drop one 2025-cohort authority, lowering the first column by one.

Table 5. Identification diagnostics and HonestDiD sensitivity (second-home rate)

Diagnostic	Result
Event-study coefficient, e = −3	−0.49 (SE 0.23; p = 0.03)

Event-study coefficient, $e = -2$	-0.62 (SE 0.19; $p = 0.001$)
Placebo treatment at 2023 (pre-announcement)	0.18 (SE 0.17; $p = 0.27$); null
ATT, not-yet-treated vs never-treated	-0.903 vs -0.903 (invariant)
ATT, excl. South Tyneside	-0.895 (95% CI excludes zero)
HonestDiD, smoothness (FLCI)	Excludes zero across full grid (to $M = 0.6$)
HonestDiD, smoothness (C-LF cross-check)	Excludes zero across full grid (to $M = 0.6$)
HonestDiD, relative magnitudes	Breakdown near $M = 0.3$

Note: Diagnostics for the second-home ATT (Callaway-Sant'Anna, universal base period). Lower rows report the Rambachan-Roth sensitivity: the effect excludes zero across the full smoothness grid and up to $M \approx 0.3$ in relative magnitudes.

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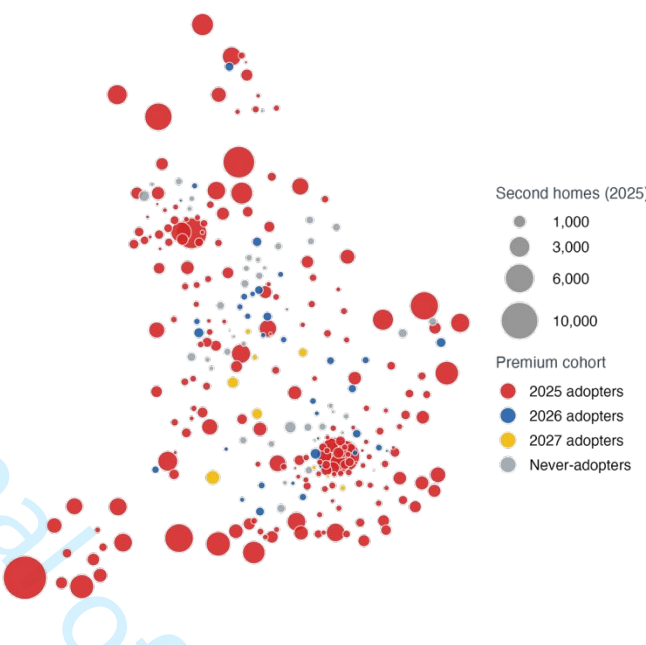


Figure 1. Second-home premium adoption and second-home stock

Note: Each bubble is one English local authority at its centroid, coloured by adoption cohort and sized by 2025 recorded second-home count.

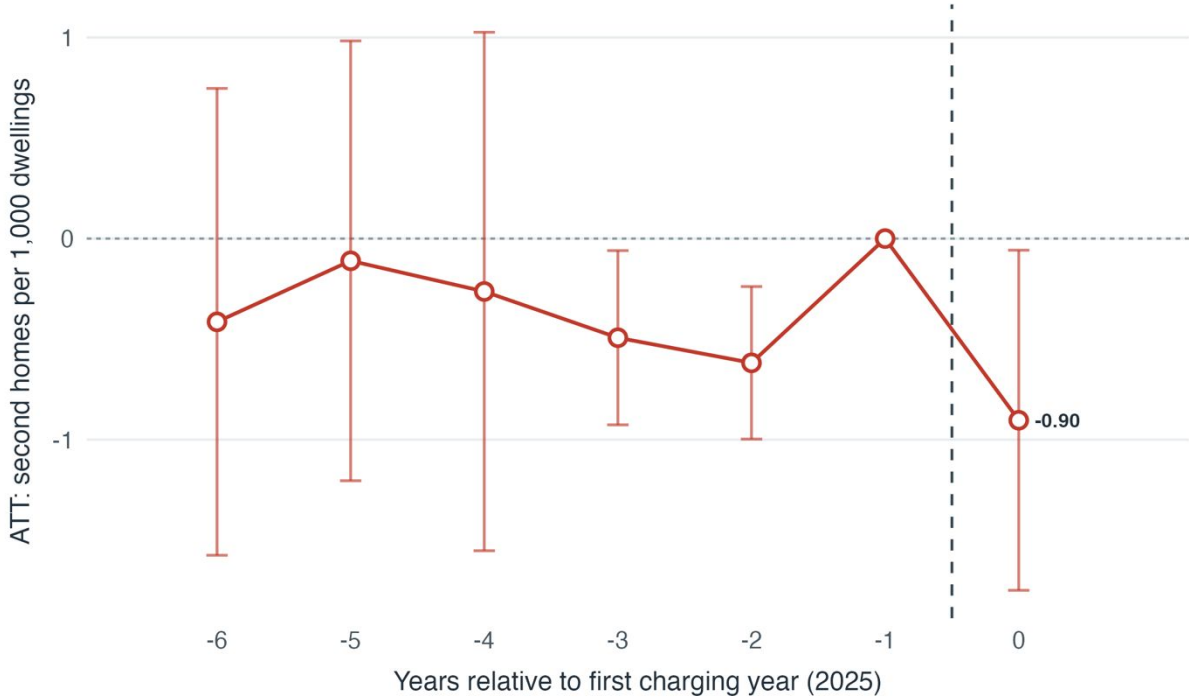


Figure 2. Event-study estimates of the premium's effect on recorded second homes

Note: Callaway-Sant'Anna estimator, not-yet-treated controls, 95% confidence intervals. Universal base period, normalising the year before charging (2024, $e = -1$) to zero.

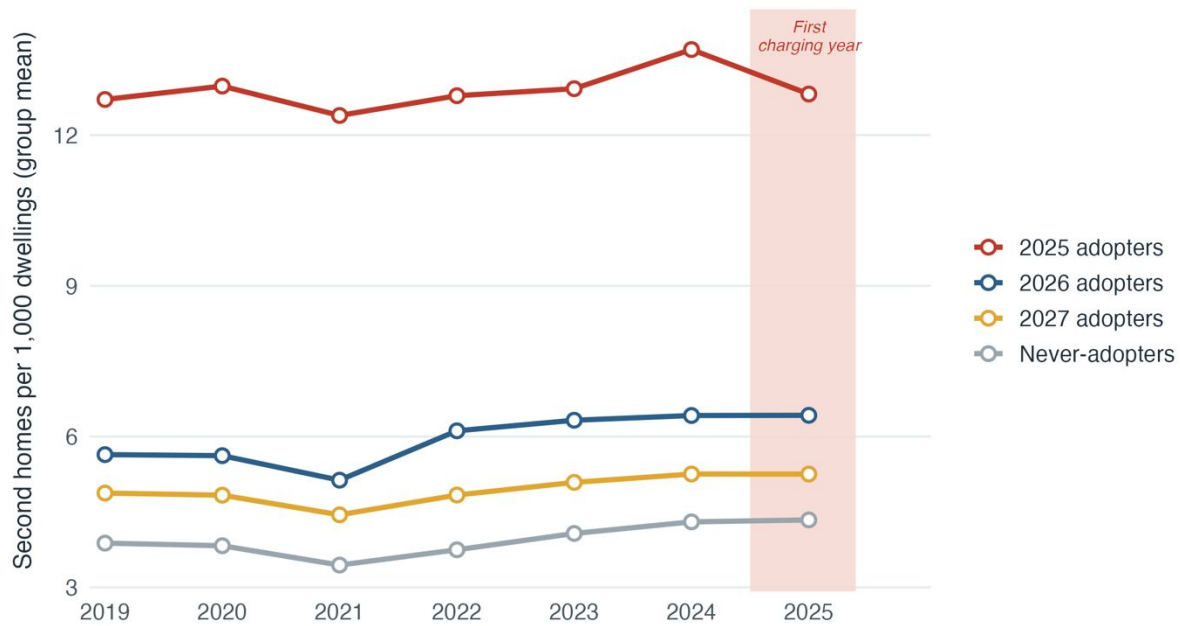


Figure 3. Raw group-mean second-home rate by adoption cohort, 2019-2025.

Note: Group means of second homes per 1,000 dwellings. The shaded region marks the 2025 first charging year.

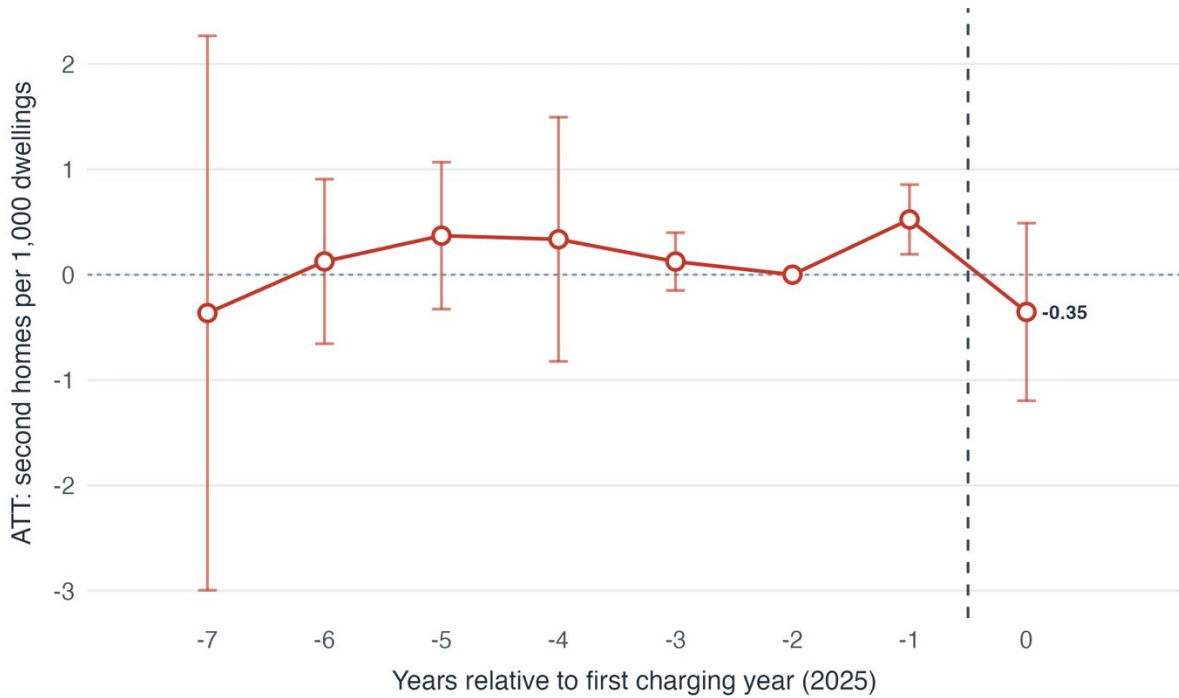


Figure 4. Anticipation-adjusted event-study estimates for recorded second homes

Note: As Figure 2, allowing one period of anticipation, so that 2024 is treated as potentially affected and 2023 is the last clean pre-charging reference.

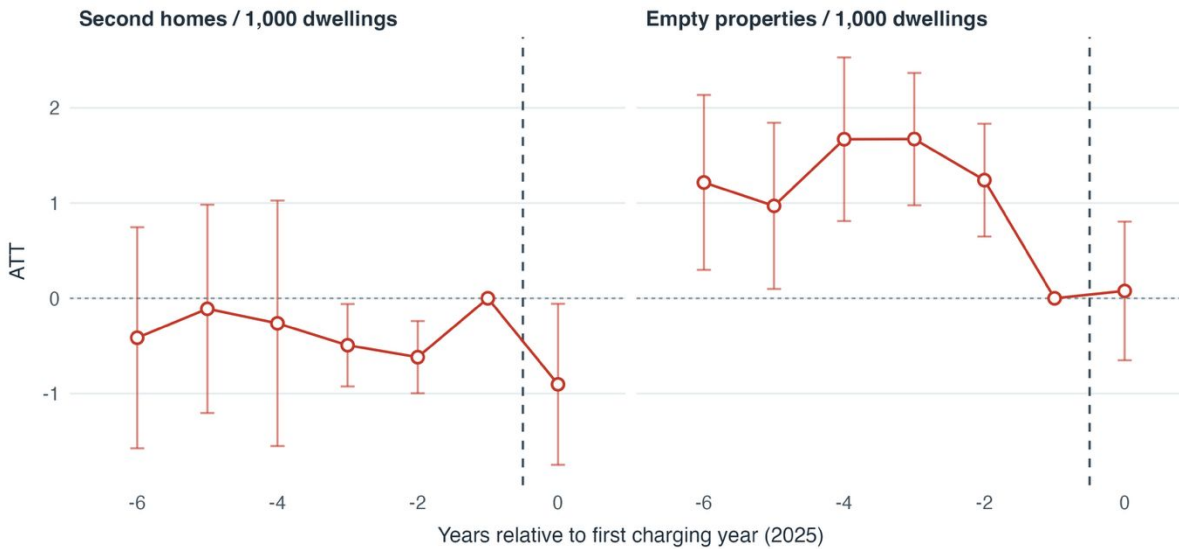


Figure 5. Event-study estimates for recorded second homes and empty properties

Note: Callaway-Sant'Anna estimator, not-yet-treated controls, 95% confidence intervals. The dashed line marks the first charging year.