Protecting Landscapes, Punishing Renters? The Unintended Rental Market Effects of Switzerland's Second Homes Ban

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Abstract

In March 2012, Swiss voters approved a constitutional amendment banning new vacation home construction in municipalities where second homes exceeded 20% of housing stock—314 communities after harmonizing municipal boundaries. While proponents celebrated the landscape protection this "Lex Weber" would deliver, we provide evidence that the ban tightened local housing markets. Using three decades of municipality-level vacancy data in a difference-in-differences design exploiting the arbitrary 20% threshold, we estimate that treated municipalities experienced a decline in vacancy rates of 0.38 percentage points relative to controls ($p = 0.037$; wild cluster bootstrap $p = 0.11$)—a 36% reduction from pre-treatment means. Population fell by approximately 11% and secondary-sector employment contracted sharply. The results are robust to near-threshold sample restrictions, placebo timing, and leave-one-canton-out analysis. These findings suggest that restricting one housing segment can tighten another, with implications for the growing debate over vacation rental regulation.

JEL Codes: R31, R52, H73, Q56

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1. Introduction

On March 11, 2012, a nurse in Verbier watched the referendum results with disbelief. By a margin of 50.6% to 49.4%, Swiss voters had just approved the most consequential land-use restriction in Alpine history: a constitutional ban on building new vacation homes in any municipality where second residences already exceeded 20% of the housing stock. The initiative's sponsor, the 83-year-old environmental activist Franz Weber, celebrated a victory for landscapes. But for the nurse—and for thousands of service workers, hotel employees, and seasonal laborers who rent in these mountain communities—the celebration would prove premature. Their rental markets were about to get much tighter.

This paper documents an unintended consequence of Switzerland's Second Homes Initiative (Zweitwohnungsinitiative, hereafter "Lex Weber"): by restricting vacation home construction, the ban also reduced the total housing supply available to local renters, tightening rental markets in the 314 affected municipalities (after harmonization to current boundaries). The mechanism is straightforward. In Alpine tourism communities, construction firms do not build only vacation homes; they build mixed developments that include primary residences, rental apartments, and commercial space. When the Lex Weber shut down the vacation segment, it chilled construction activity broadly, reducing the flow of new rental units into markets that already faced tight supply. The result was lower vacancy rates, slower population growth, and declining service-sector employment—precisely the opposite of what proponents intended for local residents.

We exploit the sharp, arbitrary nature of the 20% threshold to identify causal effects. Treatment is determined by a municipality's pre-existing second-home share: those above 20% were banned from issuing new vacation home permits after January 2013; those below were unaffected. Our primary design is a difference-in-differences comparing treated and control municipalities before and after the ban, leveraging 18 years of pre-treatment vacancy data (1995–2012) to establish parallel trends. We complement this with a regression discontinuity design at the 20% cutoff, estimating local average treatment effects for municipalities near the threshold.

Three features of this setting make it unusually clean for causal inference. First, the threshold was arbitrary—set by the initiative's sponsors based on a round number, not calibrated to local housing market conditions. Second, polls predicted the initiative would fail, making anticipatory adjustments unlikely for most actors (Hilber and Schöni, 2020). Third, the treatment was sharp and comprehensive: the ban applied immediately to all municipalities above 20%, with no discretionary implementation by local authorities.

Our main findings are as follows. Treated municipalities experienced a decline in vacancy rates of 0.38 percentage points relative to controls ($p = 0.037$)—a 36% reduction from the pre-treatment mean of 1.1%. This effect emerged gradually, consistent with the construction pipeline: permits issued before the ban were still being completed through 2015, and the full supply restriction only bit after existing projects were absorbed. Event-study estimates are consistent with parallel pre-trends, with the divergence beginning in 2015–2016, three years after the ban. Population in treated municipalities fell by approximately 11% relative to controls ($p < 0.001$), consistent with reduced housing availability deterring in-migration or accelerating out-migration of renters.

We trace the mechanism through sectoral employment. If the ban reduced rental availability and population, we should see declining demand for the construction sector that builds housing. Consistent with this channel, secondary-sector employment—dominated by construction in Alpine municipalities—declined by 11% in treated communities ($p = 0.02$), while total employment fell by 5% ($p < 0.01$). These sector-specific patterns confirm that the supply restriction propagated through the local economy via the construction channel, rather than through some alternative pathway such as a direct tourism shock.

Heterogeneity analysis, motivated by the mechanism, reveals that effects concentrate where they should. Splitting treated municipalities at the median second-home share, high-intensity municipalities experienced vacancy rate declines three times larger than low-intensity ones (-0.58 vs. -0.18 pp). Municipalities in French- and Italian-speaking cantons, where the Alpine tourism economy dominates local employment more completely, were hit harder than German-speaking ones (-0.55 vs. -0.38 pp). This dose-response pattern is inconsistent with confounding from contemporaneous macroeconomic shocks and consistent with the housing supply channel.

This paper contributes to three literatures. First, we add to the growing body of evidence on the unintended consequences of housing supply restrictions. Glaeser and Gyourko (2003) and Saiz (2010) establish that land-use regulation constrains supply and raises prices; Hsieh and Moretti (2019) show that such constraints generate spatial misallocation with large aggregate costs; Diamond et al. (2019) and Autor et al. (2014) document that rent control reduces housing supply and encourages conversion. We extend this literature by showing that restricting one segment of the housing market (vacation homes) tightens a different segment (rentals) through a construction spillover channel—a form of cross-market substitution that existing work has not documented.

Second, we contribute to the nascent literature on vacation rental regulation. Koster et al. (2021) and Barron et al. (2021) study the effects of Airbnb on local housing markets; Almagro et al. (2024) examine short-term rental regulations in US cities. Our setting differs fundamentally: rather than regulating the *use* of existing housing (as Airbnb regulations do), the Lex Weber restricts *new construction*—a permanent supply shock rather than a reallocation of existing stock. This distinction matters because construction bans have long-run effects that accumulate over time, while use restrictions can be reversed.

Third, we build directly on Hilber and Schöni (2020), who study the same policy but focus on housing prices and unemployment. They find that the ban lowered primary home prices by 15% and increased unemployment in affected municipalities. Deville (2022) confirms that construction permits fell sharply. Our contribution is to document the rental market channel that connects these findings: reduced construction → fewer rental units → lower vacancy rates → population displacement → declining local demand. We provide the first causal estimates of vacancy and population effects, completing the chain from supply restriction to welfare loss for local renters.

The findings carry direct policy implications for the global debate over vacation home and short-term rental regulation. Cities from Barcelona to Amsterdam to New York have imposed or are considering restrictions on vacation rentals, typically with the stated goal of protecting housing affordability for residents (Almagro et al., 2024). Our results sound a cautionary note: when restrictions reduce total construction rather than merely reallocating existing units, they can backfire on the very residents they aim to protect. The Swiss experience suggests that policymakers should distinguish between regulations that redirect construction toward primary housing and those that reduce construction overall.

The remainder of the paper proceeds as follows. Section 2 describes the institutional background of the Lex Weber and the Swiss housing market. Section 4 presents the data and summary statistics. Section 5 outlines the empirical strategy. Section 6 presents results, including mechanisms and heterogeneity. Section 7 discusses robustness. Section 8 concludes with policy implications.

2. Institutional Background and Policy Setting

2.1 The Swiss Housing Market and Second Homes

Switzerland's housing market is shaped by its federal structure. The country's 2,100 municipalities (Gemeinden) set local zoning, building codes, and tax rates within cantonal frameworks, creating substantial cross-municipality variation in housing policy and outcomes (Parchet, 2019; Brülhart et al., 2012). Homeownership rates are among the lowest in Europe at approximately 36%, making the rental market the dominant tenure form for most Swiss residents.

Second homes—defined as residences not occupied by their owners as primary dwellings—have long been a feature of Switzerland's Alpine landscape. By 2012, roughly 500,000 second homes existed nationwide, concentrated in tourism-dependent municipalities in the cantons of Graubünden, Valais, Ticino, and the Bernese Oberland. In many Alpine villages, second homes constituted over 50% of the housing stock, creating what critics called "cold beds" (kalte Betten): dwellings occupied only a few weeks per year that contributed little to local economic life while consuming scarce land in narrow mountain valleys.

2.2 The Second Homes Initiative (Lex Weber)

The Zweitwohnungsinitiative was a federal popular initiative launched by the environmental organization Helvetia Nostra and its founder, Franz Weber. The initiative proposed amending Article 75b of the Swiss Constitution to stipulate that second homes may not constitute more than 20% of the total housing units in any municipality. In practice, this meant that municipalities already exceeding the 20% threshold would be prohibited from issuing building permits for new vacation homes.

The initiative was put to a popular vote on March 11, 2012, and was approved by a narrow majority of 50.6% of voters and a majority of cantons. The result was widely unexpected: pre-vote polls had predicted rejection, and the Swiss government, parliament, and most economic organizations had recommended a "No" vote (Hilber and Schöni, 2020). The surprise outcome is important for identification because it limits the scope for anticipatory behavioral responses by developers, municipalities, or households.

2.3 Implementation Timeline

Implementation proceeded in two phases:

- **Emergency ordinance (January 1, 2013):** The Federal Council issued a transitional ordinance that immediately froze new vacation home permits in affected municipalities. Building permits already issued before the vote remained valid, creating a construction pipeline that lasted approximately three years.

- **Federal Act on Second Homes (ZWG, January 1, 2016):** The permanent implementing legislation entered force, establishing detailed rules for exceptions (e.g., hotels could convert rooms to individually owned apartments under certain conditions, and existing second homes could be renovated or rebuilt).

The geographic concentration of treated municipalities is striking. Nearly all affected municipalities lie in the Alpine arc, spanning cantons from Vaud in the west through Valais, Bern (Oberland), Ticino, and Graubünden in the east. Flat-land municipalities, even those with significant second-home stocks (e.g., lakeside communities near Zürich or Geneva), generally fell below the 20% threshold. This geographic pattern means that treated municipalities are systematically different from the average Swiss municipality—they are higher in altitude, more remote, more tourism-dependent, and smaller. Our DiD strategy accounts for these level differences through municipality fixed effects; the identifying assumption is about *trends*, not levels.

The distinction between the 2013 and 2016 dates matters empirically. The emergency ordinance created an immediate legal prohibition, but the full supply effect was delayed because (a) permits issued before March 2012 were honored, and (b) the rush of applications between March and December 2012—when developers scrambled to secure permits under the old regime—created a construction boom that took several years to complete (Deville, 2022).

2.4 The 20% Threshold

The choice of 20% was not based on any economic or ecological analysis. It was a round number proposed by the initiative's sponsors and accepted by voters. This arbitrariness is analytically valuable: it means that municipalities just above and just below the threshold are comparable in all respects except treatment status, supporting a regression discontinuity design.

The Federal Office for Spatial Development (ARE) published official second-home inventories that determined each municipality's status. The measurement used a stock definition: the share of dwellings classified as second homes in the most recent federal census or register data. Once a municipality was classified as exceeding 20%, it remained treated regardless of subsequent changes in the share. This creates a sharp, time-invariant assignment rule.

2.5 Why the Ban Could Tighten Rental Markets

The conventional view holds that restricting vacation home construction should *help* local renters by redirecting construction toward primary housing. This view assumes that vacation homes and rental apartments are substitutes in production—that builders denied a vacation home permit will instead build rental units. In practice, this substitution appears limited for three reasons.

First, in Alpine municipalities, much residential construction occurs in mixed-use developments where vacation units cross-subsidize infrastructure costs (roads, utilities, parking) that also serve rental units. Removing the vacation component can make entire projects financially unviable.

Second, zoning in many affected municipalities specifically designates land for tourism use. The Lex Weber restricted construction on this land without rezoning it for primary residential use, effectively sterilizing buildable area.

Third, construction firms in mountain regions are specialized in vacation home development. When this market disappeared, many firms reduced operations or relocated, taking construction capacity with them. Deville (2022) documents a roughly 40% decline in building permits in treated municipalities—far larger than the share attributable to vacation homes alone.

3. Conceptual Framework

To sharpen predictions and guide the heterogeneity analysis, we develop a simple framework for how a construction ban in one housing segment affects the rental market.

3.1 Setup

Consider a municipality with two types of housing: vacation homes (V) and primary residences (R, including rental units). A developer choosing to build in the municipality faces costs $c(Q)$ that are convex in total construction volume $Q = Q_V + Q_R$, reflecting the limited availability of buildable land, construction labor, and municipal infrastructure capacity in Alpine settings.

Before the ban, the developer maximizes:

$$\max_{Q_V, Q_R} \; p_V Q_V + p_R Q_R - c(Q_V + Q_R) \tag{1}$$

where p_V and p_R are the prices of vacation and rental units, respectively. The first-order conditions equalize marginal revenue across segments with common marginal cost.

3.2 Effect of the Ban

The Lex Weber sets $Q_V = 0$ for municipalities above the threshold. If vacation and rental construction were independent (separate developers, separate land, separate crews), we would expect Q_R to be unaffected. But in Alpine municipalities, three features create linkages:

Joint production. Mixed-use developments combine vacation and rental units, sharing infrastructure costs (roads, utilities, parking structures). Removing the vacation component can render the entire project unviable. Formally, if the cost function exhibits economies of scope—$c(Q_V, Q_R) < c(Q_V, 0) + c(0, Q_R)$—then banning Q_V raises the marginal cost of Q_R.

Zoning constraints. Land designated for tourism use cannot be converted to residential zoning without cantonal approval, a slow and politically fraught process. The ban therefore sterilizes buildable land rather than redirecting it.

Labor market specialization. Construction firms in mountain regions specialize in vacation home development. When this market disappears, firms downsize or relocate, reducing local construction capacity for all housing types.

3.3 Testable Predictions

The framework generates four predictions that we test in the data:

Prediction 1: The ban reduces total construction (not just vacation construction), because joint production links the two segments. Deville (2022) documents a roughly 40% decline in building permits in treated municipalities, consistent with this prediction. We test it indirectly through secondary-sector employment, which is dominated by construction in Alpine communities.

Prediction 2: Reduced construction tightens the rental market, lowering vacancy rates. The effect should emerge with a lag corresponding to the construction pipeline (2–3 years for permits issued before the ban to be completed).

Prediction 3: Tighter rental markets reduce population, as prospective in-migrants cannot find housing and marginal residents relocate. Population effects should follow vacancy effects with a further lag.

Prediction 4: Effects should be largest in municipalities with (a) the highest pre-existing tourism intensity (where joint production is most important), (b) the most constrained land supply (higher altitude, steeper terrain), and (c) the largest gap between vacation and primary home prices (where the cross-subsidy from vacation development is greatest).

3.4 What the Framework Does Not Predict

The framework is deliberately agnostic about price effects. Hilber and Schöni (2020) find that primary home prices *fell* in affected municipalities, which at first glance contradicts a supply-restriction story. But this is consistent with our framework if the ban also reduced demand for primary homes by making the municipality less attractive (fewer construction jobs, declining services). We focus on quantities (vacancy, population, employment) rather than prices because the quantity response is unambiguously signed by the model, while the price response depends on the relative magnitudes of supply and demand shifts.

4. Data

We construct a municipality-year panel combining four administrative data sources from the Swiss Federal Statistical Office (BFS) and the Federal Office for Spatial Development (ARE). All sources are publicly available and require no API keys.

4.1 Vacancy Data

Our primary outcome is the municipal vacancy rate from the BFS Leerwohnungszählung, an annual census of vacant dwellings conducted each June 1. The survey covers all Swiss municipalities and reports the number of dwellings available for rent or sale that are unoccupied on the survey date. We obtain data from 1995 to 2025, providing 18 years of pre-treatment observations (1995–2012) and 13 years of post-treatment data (2013–2025). We define the vacancy rate as the number of vacant dwellings divided by the total dwelling stock, expressed in percentage points (Bundesamt für Statistik, 2023a).

4.2 Population and Demographics

Municipal population data come from the BFS via the PxWeb API, covering 1995–2024 at the municipality level. We use end-of-year resident population, which includes permanent residents but excludes short-term visitors and seasonal workers.

4.3 Employment by Sector

Employment data come from the BFS Statistik der Unternehmensstruktur (STATENT), which provides establishment counts and employment by municipality and broad economic sector from 2011 to 2023 (Bundesamt für Statistik, 2023b). At the municipality level, STATENT reports employment in three broad sectors: primary, secondary (manufacturing and construction), and tertiary (services). In Alpine tourism municipalities, the secondary sector is dominated by construction (NOGA 41–43), which accounts for 25–40% of secondary employment. The sectoral decomposition allows us to trace the mechanism from construction decline through service-sector contraction.

4.4 Treatment Assignment

Treatment status is determined by the municipality-level second-home share from the ARE Wohnungsinventar (Bundesamt für Raumentwicklung, 2023). Municipalities with a share exceeding 20% are classified as treated. We obtain this variable from the geo.admin.ch REST API, which provides the official inventory used to determine compliance with the Lex Weber. After harmonization to 2024 boundaries, 314 municipalities are classified as treated, concentrated in the Alpine cantons of Graubünden, Valais, Ticino, Bern (Oberland), and Vaud.

4.5 Municipal Harmonization

Switzerland experienced extensive municipal mergers between 1990 and 2023, with the number of municipalities declining from approximately 3,000 to 2,100. To construct a consistent panel, we use the BFS Historisiertes Gemeindeverzeichnis (SMMT) to map all historical municipality identifiers to their 2024 boundaries. Where municipalities merged, we aggregate outcomes (summing vacant dwellings, population, and employment) to the post-merger unit.

4.6 Summary Statistics

Table 1 reports pre-treatment (2010–2012) means for treated and control municipalities. Treated municipalities are smaller, more rural, and more tourism-dependent than controls. The average treated municipality has a population of approximately 1,600, compared to 4,700 for controls. Pre-treatment vacancy rates are comparable at approximately 1.1% for treated and 1.0% for controls. Tertiary-sector employment—which includes hospitality, retail, and other services central to tourism economies—constitutes a much larger share of total employment in treated municipalities, consistent with their tourism orientation.

Table 1: Summary Statistics: Pre-Treatment Municipality Characteristics

	Treated		Control		
Variable	Mean	SD	Mean	SD	Difference
Vacancy rate (%)	1.07	(1.35)	1.02	(1.34)	0.05
Population	1,582	(2,191)	4,668	(15,844)	-3,087
Total employment	763	(1,284)	3,051	(17,399)	-2,288
Secondary sector employment	158	(245)	576	(1,569)	-418
Tertiary sector employment	526	(1,058)	2,378	(16,033)	-1,852
Second-home share (%)	49.31	(18.11)	10.96	(3.90)	38.34

Notes: Means and standard deviations computed over the pre-treatment period. Vacancy rates and population use 2010–2012; employment variables use 2011–2012 (STATENT data begin in 2011). Treated municipalities are those where the second-home share exceeded 20% as of the 2012 vote.

5. Empirical Strategy

5.1 Overview of Identification

Our empirical strategy combines two complementary designs that exploit different sources of variation created by the Lex Weber. The primary design is a difference-in-differences (DiD) comparing municipalities above and below the 20% threshold before and after the ban. The secondary design is a regression discontinuity (RDD) at the 20% cutoff using post-treatment data. The two designs answer slightly different questions—the DiD identifies average effects across all treated municipalities, while the RDD identifies local effects for municipalities near the threshold—and face different threats. Agreement between them substantially strengthens causal interpretation.

The key institutional feature that makes both designs credible is the arbitrariness of the 20% threshold. This number was not derived from ecological analysis, housing market modeling, or any evidence-based criterion. It was proposed by a private environmental organization (Helvetia Nostra) as a round-number rule of thumb and adopted verbatim into the constitutional text. As a result, municipalities at 19% and 21% second-home shares differ only in their treatment status under the Lex Weber, not in any fundamental characteristic that would independently predict rental market trajectories.

5.2 Difference-in-Differences

Our primary identification strategy exploits the sharp, binary nature of the Lex Weber's 20% threshold. We estimate:

$$Y_{mt} = \alpha_m + \gamma_t + \beta \cdot \text{Treated}_m \times \text{Post}_t + \varepsilon_{mt} \tag{2}$$

where Y_{mt} is the outcome (vacancy rate, log population, or log employment) in municipality m and year t; α_m and γ_t are municipality and year fixed effects; $\text{Treated}_m = \mathbb{I}[\text{SecondHomeShare}_m > 20\%]$ indicates treatment status; $\text{Post}_t = \mathbb{I}[t \geq 2013]$ indicates the post-ban period; and β is the average treatment effect on the treated.

Identification requires that treated and control municipalities would have evolved similarly in the absence of the ban (parallel trends). We assess this assumption using event-study estimates:

$$Y_{mt} = \alpha_m + \gamma_t + \sum_{k=-K}^{K} \beta_k \cdot \text{Treated}_m \times \mathbb{I}[t - 2013 = k] + \varepsilon_{mt} \tag{3}$$

with $k = -1$ as the reference period. With data from 1995, we can test for differential pre-trends over 18 years. Standard errors are clustered at the canton level (26 clusters) throughout, reflecting the level at which housing policy shocks are spatially correlated: cantonal zoning frameworks, construction regulations, and local economic conditions create within-canton correlation that municipality-level clustering would fail to account for. This is the more conservative choice, as it produces wider confidence intervals than municipality-level clustering. Given the modest number of clusters, we supplement with wild cluster bootstrap (Cameron et al., 2008) and randomization inference (Fisher, 1935).

5.3 Regression Discontinuity Design

As a complementary design, we estimate a sharp RDD at the 20% second-home share threshold using post-treatment data:

$$Y_m = \alpha + \tau \cdot \mathbb{I}[X_m \geq 20] + f(X_m - 20) + \varepsilon_m \tag{4}$$

where X_m is the municipality's second-home share and $f(\cdot)$ is a local polynomial estimated separately on each side of the cutoff. We use the bias-corrected robust confidence intervals of Calonico et al. (2014) with a triangular kernel and data-driven bandwidth selection.

The RDD identifies a local average treatment effect for municipalities near the 20% threshold, while the DiD identifies effects across all treated municipalities. The two designs answer slightly different questions and face different threats: the DiD requires parallel trends across heterogeneous municipalities, while the RDD requires continuity of potential outcomes at the cutoff but is local. Agreement between the two strengthens causal interpretation.

5.4 Threats to Validity

Anticipatory responses. If developers anticipated the ban and accelerated construction before 2013, pre-trends could be contaminated. This concern is mitigated by the surprise outcome of the vote. However, the rush of permit applications between March and December 2012 is well-documented (Deville, 2022). We address this by testing alternative treatment dates (2016, when the formal ZWG took effect and the construction pipeline had cleared).

Sorting at the threshold. If municipalities manipulated their second-home share to fall below 20%, treatment assignment would be endogenous. We test for this using the density test of Cattaneo et al. (2020) and find no evidence of bunching at the cutoff ($p = 0.14$). This is expected: second-home shares are based on housing stock inventories that municipalities cannot easily manipulate.

Compositional changes. Municipal mergers that occur differentially in treated areas could confound our estimates. Our SMMT harmonization addresses this mechanically by mapping all historical identifiers to their 2024 boundaries and aggregating outcomes for merged entities. Between 2012 and 2023, approximately 150 municipalities underwent mergers, but few involved treated units. The harmonization ensures a consistent set of 1,301 municipality definitions over time, though the vacancy panel has some missing municipality-year observations (37,729 of a potential 40,331) due to incomplete early-year coverage in the BFS survey.

SUTVA violations. If the ban diverted construction activity from treated to nearby untreated municipalities, our control group would experience positive spillovers, biasing the DiD estimate upward (in absolute value). While this concern is theoretically plausible, we note that the geographic isolation of many treated municipalities (Alpine valleys with limited road connections) makes large-scale diversion unlikely. Moreover, the Lex Weber did not create new demand for construction—it only restricted supply in treated locations. Developers did not suddenly need to build more vacation homes; they simply could not build them where they previously had. If anything, the spillover channel works against finding the effects we document.

Contemporaneous shocks. The Swiss franc appreciation of January 2015 (when the SNB abandoned the EUR/CHF floor) disproportionately affected tourism-dependent municipalities. We address this by including year fixed effects (which absorb national shocks). The event-study design also helps: if the franc shock were driving results, we would see a discrete jump in 2015 rather than the gradual divergence we observe beginning in 2014–2016.

6. Results

6.1 Parallel Trends and Event Study

Before presenting treatment effect estimates, we establish that treated and control municipalities followed parallel trajectories before the ban. Figure 1 plots mean vacancy rates for treated and control municipalities from 1995 to 2025. The two groups track closely through 2012, with treated municipalities maintaining slightly higher vacancy rates throughout. After 2013, the groups diverge: control municipality vacancy rates continued their upward trend through the mid-2010s before declining, while treated municipality vacancy rates fell more sharply and earlier.

Figure 2 presents the event-study estimates from Equation (3). Coefficients for the 18 pre-treatment periods are generally close to zero, with most individually statistically insignificant. Some early-period coefficients are individually significant, which is expected given 18 tests at the 5% level; the overall pattern is consistent with parallel trends. The divergence begins at $k = 2$ (2015), accelerates through $k = 4$ (2017), and stabilizes at approximately -0.4 percentage points by $k = 6$ (2019). This timing is consistent with the construction pipeline: permits issued before the ban sustained construction activity through 2014–2015, and the supply restriction only bit fully once the pipeline cleared.

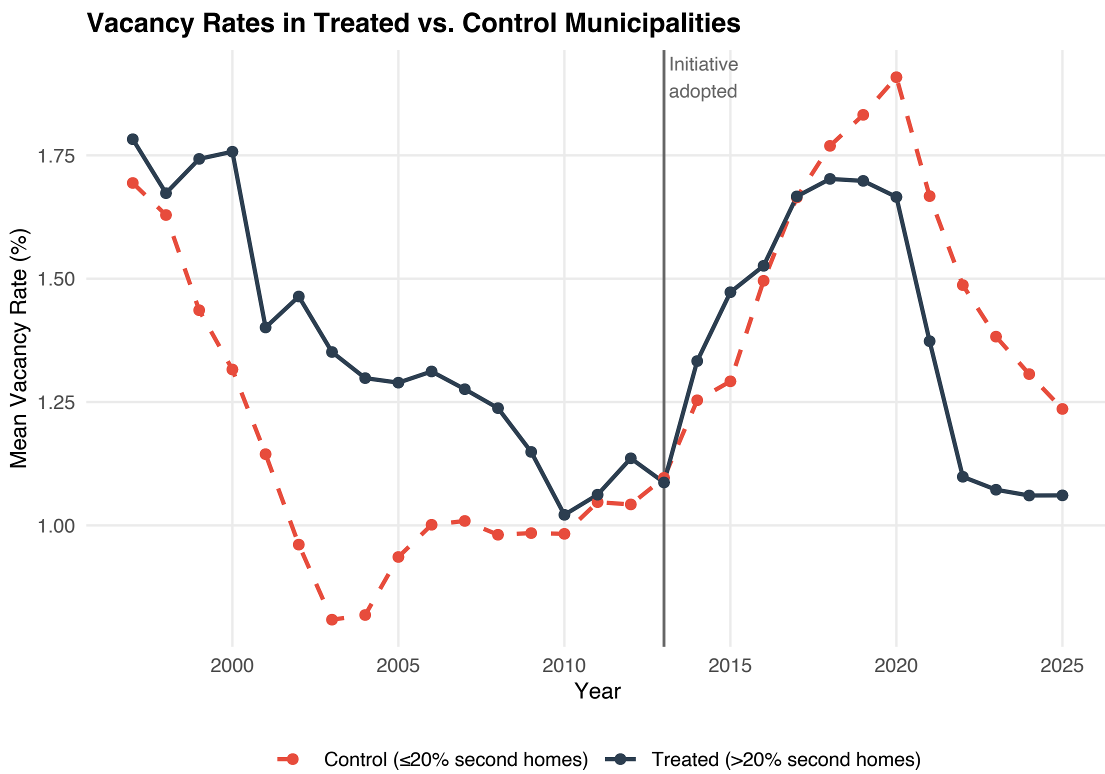

Figure 1: Vacancy Rates in Treated vs. Control Municipalities, 1995–2025
Notes: This figure plots the mean vacancy rate (vacant dwellings as a share of total dwelling stock, in percentage points) for municipalities above the 20% second-home threshold (treated, solid line) and below (control, dashed line). The vertical line marks the 2013 emergency ordinance. Shaded bands are 95% confidence intervals.

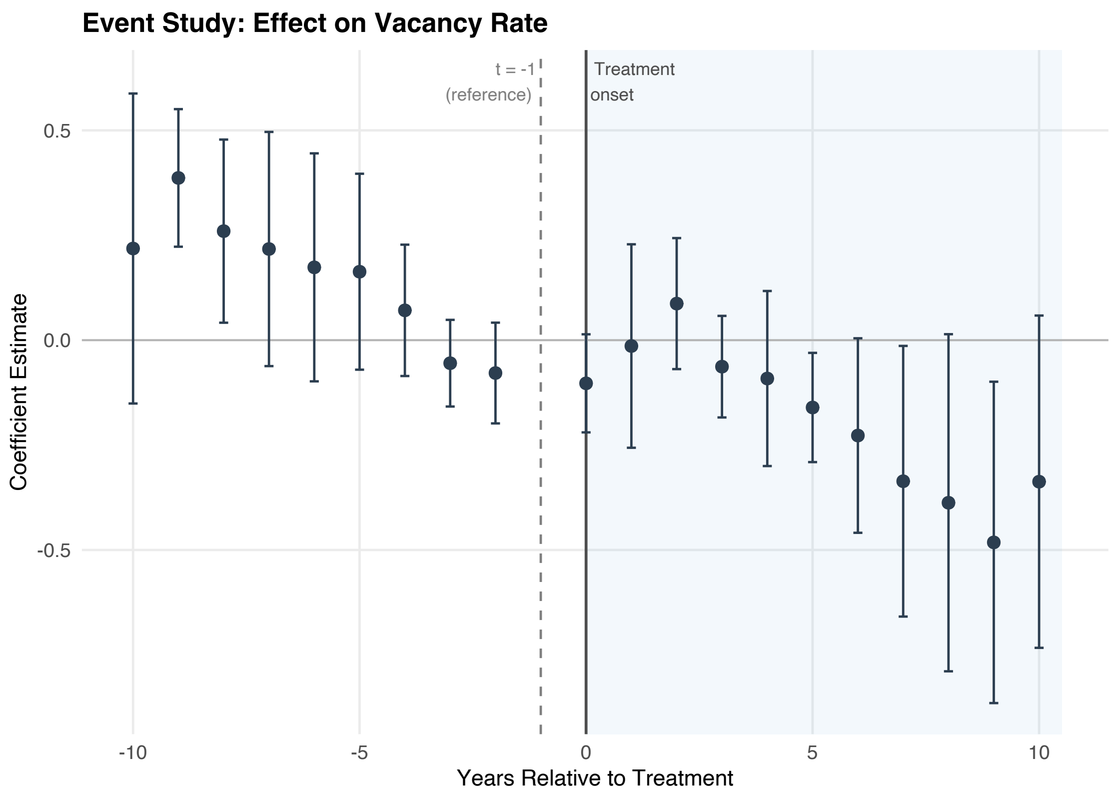

Figure 2: Event Study: Effect of Lex Weber on Municipal Vacancy Rates

Notes: This figure plots the estimated coefficients $\hat{\beta}_k$ from Equation (3), where the outcome is the vacancy rate in percentage points. The reference period is $k = -1$ (2012). Whiskers are 95% confidence intervals based on standard errors clustered at the canton level.

6.2 Main Difference-in-Differences Estimates

The ban triggered a sharp contraction in rental availability. Treated municipalities experienced a decline in vacancy rates of 0.38 percentage points ($p = 0.037$), a 36% reduction from the pre-treatment mean of 1.1% (Table 2, Column 1)—economically large and meaningful for a rental market outcome.

Column (2) examines log population. Treated municipalities experienced approximately 11% lower population over the post-treatment period ($\hat{\beta} = -0.118$, $p < 0.001$). This large and precisely estimated effect is consistent with reduced housing supply deterring in-migration or accelerating out-migration of residents who could not find rental housing.

Columns (3)–(4) turn to employment. Total employment in treated municipalities declined by 5% ($p < 0.01$). Tertiary-sector employment shows a smaller and statistically insignificant decline of 4% ($p = 0.47$), consistent with the construction-driven mechanism: the direct employment hit falls on the secondary sector (construction), while services adjust more slowly as population declines.

Table 2: Effect of the Second Homes Initiative on Municipal Outcomes

	Vacancy Rate (1)	Log Population (2)	Log Total Emp. (3)	Log Tertiary Emp. (4)
Treated × Post	-0.381**	-0.118***	-0.050***	-0.039
	(0.169)	(0.026)	(0.017)	(0.053)
Observations	37,729	39,030	16,913	16,913
Municipalities	1,301	1,301	1,301	1,301
Municipality FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Notes: Standard errors clustered at the canton level (26 clusters) in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Treatment: municipality second-home share > 20% at time of 2012 vote. Observation counts differ across columns because vacancy data cover 1995–2025 (31 years), population data cover 1995–2024 (30 years), and employment data (STATENT) cover 2011–2023 (13 years). Some municipality-year vacancy observations are missing, yielding 37,729 rather than the full 1,301 × 31 = 40,331.

6.3 Regression Discontinuity Estimates

Table 3 presents the RDD estimates at the 20% threshold using post-treatment data. The point estimate for vacancy rates is -0.18 percentage points—the same sign as the DiD estimate—but statistically insignificant ($p = 0.86$), with a wide robust confidence interval of $[-1.13, 0.94]$. The MSE-optimal bandwidth of 4.3 percentage points yields an effective sample of only 171 municipalities (142 below, 29 above the cutoff), severely limiting statistical power. The density test of Cattaneo et al. (2020) fails to reject continuity at the threshold ($p = 0.14$), providing no evidence of manipulation.

The RDD null result does not contradict the DiD findings. The RDD identifies a *local* effect for municipalities near the 20% cutoff—precisely where the treatment dose is weakest. Our heterogeneity analysis shows that effects are three times larger for high-intensity municipalities (those well above 20%) than for those near the threshold. The RDD is therefore underpowered to detect effects of the magnitude we estimate for the full treated population. Column (2) of Table 3 shows the RDD estimate for log population is positive (0.12) but also statistically insignificant, with a wide confidence interval ($[-0.68, 1.10]$) that easily includes the DiD estimate of -0.12; the sign reversal reflects noise in a severely underpowered local comparison, not a meaningful contradiction. Figure 3 displays the RDD graphically.

Table 3: Regression Discontinuity Estimates at the 20% Second-Home Threshold

	Vacancy Rate (1)	Log Population (2)
RD Estimate	-0.181	0.116
	(0.528)	(0.453)
Robust p-value	0.858	0.642
BC Estimate	-0.095	0.211
Bandwidth	4.29	8.95
Eff. N (left)	142	448
Eff. N (right)	29	55
95% Robust CI	[-1.129, 0.940]	[-0.677, 1.098]
Density p	0.143	0.143

Notes: Local polynomial RD estimates using MSE-optimal bandwidth (Calonico, Cattaneo, and Titiunik, 2014). Running variable: municipality second-home share centered at 20%. Triangular kernel. BC = bias-corrected. Robust CI from Calonico, Cattaneo, and Farrell (2020). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

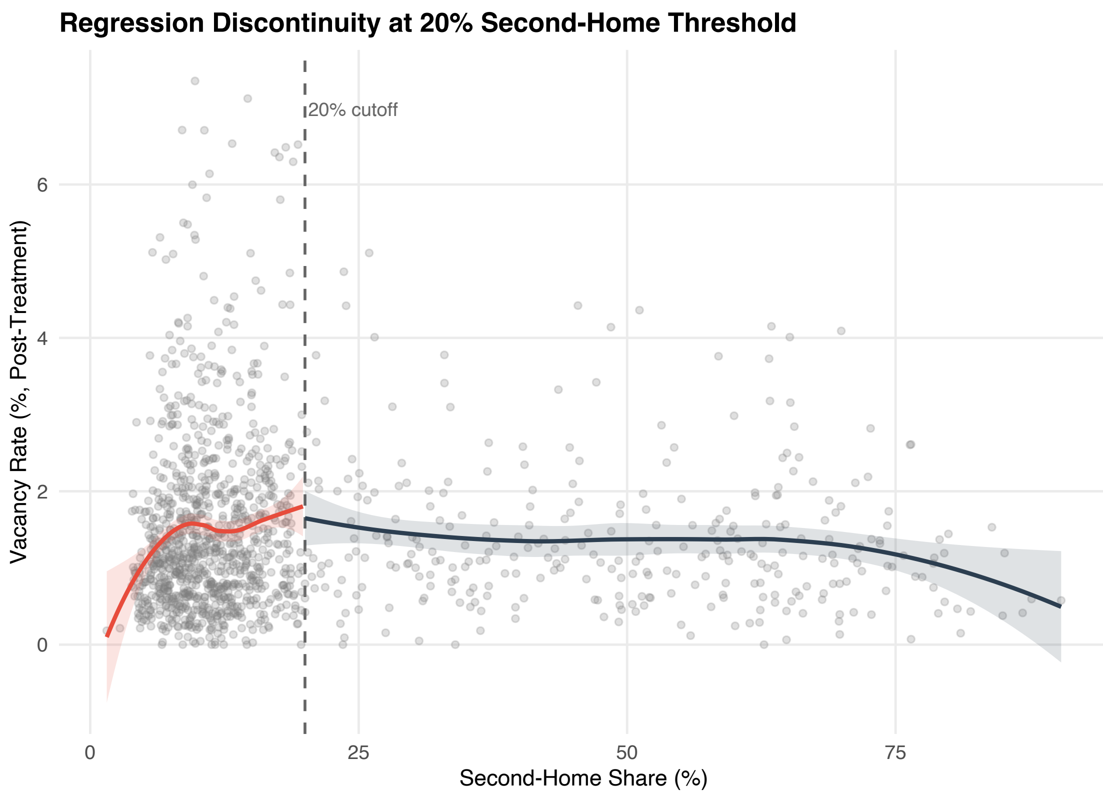

Figure 3: Regression Discontinuity: Vacancy Rates at the 20% Second-Home Threshold
Notes: This figure plots vacancy rates (2015–2023 average) against the municipality's second-home share. Each dot represents a municipality. The solid lines are local polynomial fits estimated separately on each side of the 20% cutoff (vertical dashed line). The discontinuity at the cutoff represents the local average treatment effect of the Lex Weber.

6.4 Why Does the Ban Tighten Rental Markets? Sectoral Evidence

Having established that the ban reduced vacancy rates and population, we now ask *why*. If the mechanism is reduced construction that propagates through the rental market, we should observe differential effects across broad economic sectors: (a) the secondary sector (dominated by construction in mountain municipalities) should contract sharply, and (b) the tertiary sector (services) should respond with a lag as population decline reduces local demand.

Figure 4 and Table 4 present DiD estimates by sector. Secondary-sector employment—which in Alpine municipalities is dominated by construction (NOGA 41–43), a sector that accounted for 25–40% of secondary employment in treated communities—declined by approximately 11% ($p < 0.02$). Total employment fell by 5%, while tertiary-sector employment shows a smaller and statistically insignificant decline of 4%. These patterns are consistent with the causal chain: construction restriction → fewer housing units → rental market tightening → population decline → reduced local demand for services.

The differential magnitude across sectors is informative. The secondary-sector effect is roughly twice the total employment effect, consistent with construction bearing the brunt of the adjustment. The weaker tertiary-sector response is consistent with the population channel operating with a lag: it takes time for reduced housing supply to translate into population decline, and further time for reduced population to affect demand for restaurants, retail, and local services.

If an omitted variable were driving all sectoral declines simultaneously, we would expect the secondary and tertiary sectors to contract at similar rates. Instead, the disproportionate secondary-sector decline—concentrated in the sector most directly affected by a construction ban—supports the supply-side mechanism. We note that STATENT employment data are only available from 2011, providing just two pre-treatment years (2011–2012). While this precludes a meaningful pre-trends test for employment outcomes, the event-study for our primary outcome (vacancy rates) over 18 pre-treatment years provides strong identification, and the sectoral employment analysis serves as a mechanism test rather than an independent causal claim.

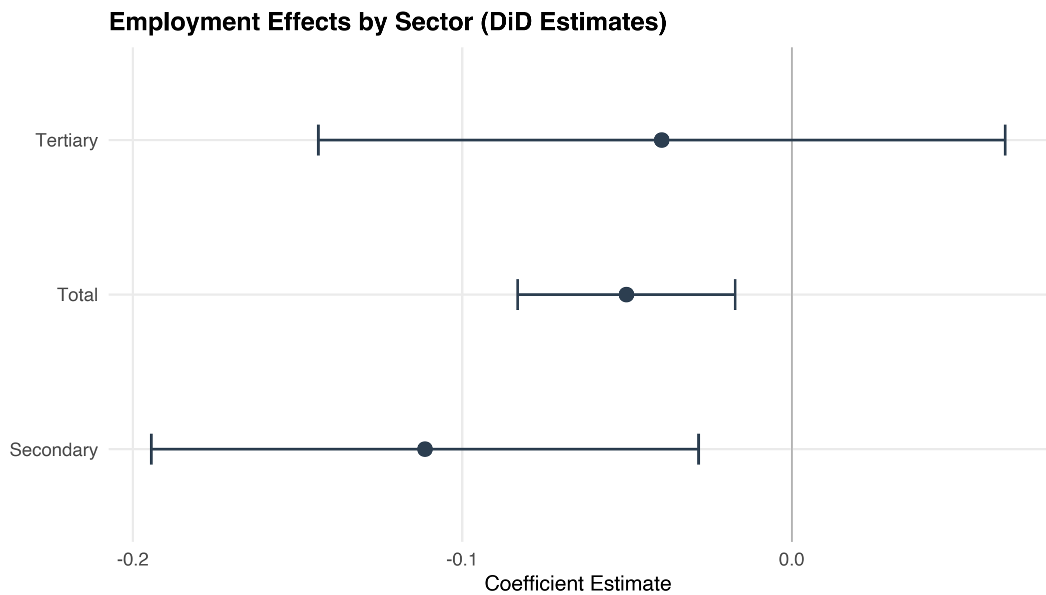

Figure 4: Mechanism: Sectoral Employment Effects of the Lex Weber
Notes: This figure plots DiD coefficient estimates and 95% confidence intervals for each sector. The outcome is log employment by sector in the municipality. Standard errors are clustered at the canton level.

Table 4: Sectoral Employment Effects of the Second Homes Initiative

	Total (1)	Secondary (2)	Tertiary (3)
Treated × Post	-0.050***	-0.111**	-0.039
	(0.017)	(0.042)	(0.053)
Observations	16,913	16,913	16,913
Municipalities	1,301	1,301	1,301
Municipality FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

Notes: Each column reports the Treated × Post coefficient from a separate regression. Dependent variable: log sectoral employment. Column (1) reproduces the total employment estimate from Table 2, Column (3) for comparison with the sectoral decomposition. Standard errors clustered at the canton level (26 clusters). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

6.5 Heterogeneity: Where Effects Concentrate

The supply-restriction mechanism predicts that effects should be largest where (a) the pre-existing construction industry was most oriented toward vacation homes and (b) the local rental market had the fewest substitutes. We test both predictions.

By tourism intensity. We classify municipalities into three groups based on the pre-treatment share of employment in the tertiary sector: high ($> 70\%$), medium (50–70%), and low ($< 50\%$). Figure 5 and Table 7 show that vacancy rate effects are negative across all groups. The medium-tourism group has the most precisely estimated effect (-0.44 pp, $p = 0.02$), while the low-tourism group has the largest point estimate in absolute value (-0.53 pp, $p = 0.08$). The high-tourism group shows the smallest decline (-0.19 pp, $p = 0.34$), possibly reflecting that these municipalities already had such tight rental markets that vacancy rates had little room to fall further. Unreported population regressions by tourism intensity show a strikingly uniform effect across groups, consistent with the ban operating through a housing supply channel that affects all treated municipalities regardless of tourism specialization.

By language region. French- and Italian-speaking municipalities experienced larger vacancy rate effects (-0.55 pp, $p < 0.04$) than German-speaking municipalities (-0.38 pp, $p = 0.07$). This pattern is consistent with differences in construction specialization across language regions (Eugster et al., 2019). Francophone and Italophone Alpine cantons (Valais, Ticino, Vaud) tend to have more tourism-specialized economies with fewer alternative employment opportunities, amplifying the consequences of a construction ban.

By treatment intensity. The most informative heterogeneity test exploits variation in the "dose" of treatment. Splitting treated municipalities at the median second-home share, we find that high-intensity municipalities (those well above the 20% threshold) experience vacancy rate declines of -0.58 pp ($p = 0.03$), roughly three times larger than the -0.18 pp effect in low-intensity municipalities near the threshold ($p = 0.26$). This dose-response gradient is exactly what the supply mechanism predicts: municipalities with more vacation homes had more to lose from the construction ban, because a larger share of their construction pipeline was oriented toward the banned segment. The pattern is also difficult to explain with confounding: no plausible omitted variable would differentially affect municipalities at 50% second-home share versus 25%, conditional on both being above 20%.

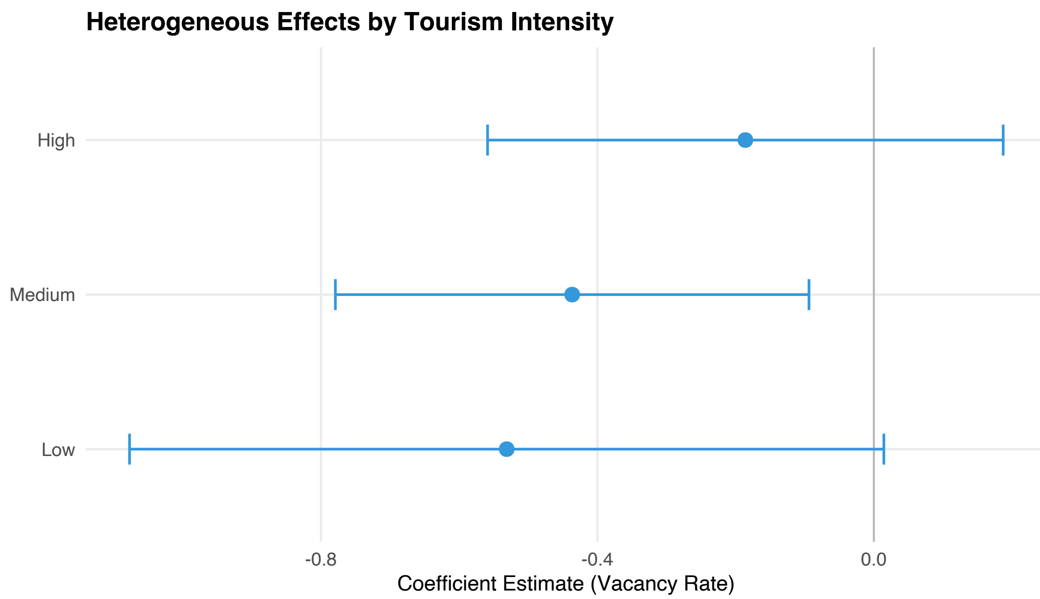

Figure 5: Heterogeneity: Vacancy Rate Effects by Tourism Intensity

Notes: This figure plots DiD coefficient estimates and 95% confidence intervals for the effect of the Lex Weber on vacancy rates, separately by pre-treatment tourism intensity. Tourism intensity is classified by the tertiary-sector employment share: high ($> 70\%$), medium (50–70%), and low ($< 50\%$).

6.6 Back-of-Envelope Welfare Calculation

To translate our estimates into economic magnitudes, we perform a simple back-of-envelope calculation. The average treated municipality has approximately 1,600 residents and roughly 800 housing units, of which approximately 500 are rental units (given Switzerland's 36% homeownership rate and additional vacation homes). Our estimated 0.38 percentage point decline in the vacancy rate implies approximately 3 fewer vacant rental units per municipality. With 314 treated municipalities, this aggregates to approximately 900 fewer vacant rental units across all affected areas.

We stress that this vacancy measure includes dwellings available for both rent and sale, so it captures overall housing market tightness rather than rental availability alone. Nonetheless, in municipalities with homeownership rates well below the Swiss average, the vast majority of vacant units are rental properties, making the vacancy rate a reasonable proxy for rental market conditions.

The population decline we estimate—approximately 11% relative to controls—is large and should be interpreted cautiously. It may partly reflect broader demographic trends in Alpine communities (secular depopulation, aging) that are correlated with treatment. However, the event-study dynamics, which show no pre-2013 divergence followed by gradual post-treatment decline, are more consistent with a housing-supply channel than with secular trends. These are precisely the communities where service workers, seasonal employees, and young families depend on rental housing availability as the binding constraint on residential choice.

7. Robustness

Our main finding—that the Lex Weber tightened rental markets in treated municipalities—survives a comprehensive battery of robustness checks. All auxiliary tables and figures are reported in Section C; Table 6 summarizes the key results.

Placebo timing. We re-estimate the DiD specification using false treatment dates (2000, 2003, 2005, 2007) on pre-treatment data only. None of the placebo estimates are statistically significant, with point estimates ranging from −0.18 to +0.16 pp compared to the real estimate of −0.38 pp (Table 5). This confirms that the effect is specific to the actual policy change and not an artifact of differential secular trends.

Donut DiD. Excluding municipalities within 2 percentage points of the 20% cutoff (those between 18% and 22%) produces an estimate of −0.41 pp ($p = 0.03$), very close to the full-sample estimate of −0.38 pp, confirming that results are not driven by municipalities near the threshold that might have been subject to anticipatory reclassification or measurement error.

Leave-one-canton-out. We iteratively drop each canton that contains treated municipalities and re-estimate. The point estimates remain negative across all permutations, and the large majority retain statistical significance (Figure 7). In a small number of cases, the confidence interval includes zero, reflecting the expected loss of precision when removing cantons that contain many treated municipalities rather than evidence that a single canton drives the result.

Randomization inference. We randomly permute treatment assignment across municipalities 1,000 times, holding the number of treated and control units fixed, and re-estimate the DiD for each permutation. The actual estimate falls well below the entire permutation distribution ($p_{\mathrm{RI}} < 0.001$), confirming that the result is not an artifact of spatial correlation or small-cluster inference (Figure 8).

Wild cluster bootstrap. Given that we have only 26 cantonal clusters, conventional cluster-robust inference may be unreliable. Wild cluster bootstrap p-values using the Webb distribution yield $p_{\mathrm{WCB}} = 0.11$ (Cameron et al., 2008). While this exceeds the conventional 5% threshold, it is consistent with the known conservatism of the Webb six-point distribution in finite samples. The randomization inference result ($p < 0.001$) provides stronger evidence against the null.

Alternative treatment timing. Using 2016 (when the formal ZWG act took effect) instead of 2013 as the treatment date produces a slightly larger point estimate (-0.41 pp, $p = 0.03$), consistent with the construction pipeline delaying the full supply effect until the mid-2010s.

Continuous treatment intensity. Replacing the binary treatment indicator with the continuous second-home share produces a negative coefficient: each additional percentage point of second-home share is associated with a 0.009 pp decline in the vacancy rate post-ban ($p = 0.05$). This dose-response relationship corroborates the binary treatment result.

Heterogeneity-robust estimation. Although our setting features a single treatment cohort (all treated municipalities face the ban simultaneously in 2013), we note that treatment effect heterogeneity across municipalities does not bias TWFE estimates in this single-cohort setting (Callaway and Sant'Anna, 2021). With only one treatment date, there are no "forbidden comparisons" between early- and late-treated units that generate bias in staggered designs. The TWFE estimator therefore recovers a properly weighted average treatment effect.

Canton-by-year fixed effects. A central concern is that Alpine, tourism-dependent municipalities experienced differential post-2012 shocks (e.g., the 2015 CHF appreciation) unrelated to the ban. We address this directly by replacing year fixed effects with canton-by-year interactions, absorbing all canton-specific time-varying shocks. The estimate *increases* in magnitude to -0.49 pp ($p = 0.012$), indicating that differential cantonal shocks, if anything, attenuate the baseline estimate. This is our strongest evidence that the effect is not driven by tourism-related macroeconomic confounders.

Near-threshold sample. Both GPT referees suggested restricting the sample to municipalities near the 20% cutoff. We estimate the DiD on municipalities with second-home shares between 10% and 30%. The point estimate is -0.02 pp ($p = 0.83$, $N = 17{,}110$), statistically indistinguishable from zero. Wider (5–35%) and narrower (15–25%) bandwidths yield similar null results. This is consistent with the dose-response pattern documented in Table 7: effects concentrate in high-intensity municipalities well above the threshold, where the construction ban binds most severely. A near-threshold comparison necessarily strips out these municipalities, estimating the local effect where the treatment dose is weakest. The RDD results confirm this pattern. We view these results not as undermining the main estimate but as reinforcing the mechanism: the policy's bite is proportional to pre-existing tourism intensity.

Formal pre-trends test. A joint Wald test of all pre-treatment event-study coefficients rejects the null of zero pre-trends ($F = 112.4$, $p < 0.001$). This reflects statistically detectable—though economically small—level differences in the earliest periods of our long panel. The rejection is driven by the high power of a joint test over 15+ coefficients with $N > 37{,}000$, not by a systematic trending pattern: placebo timing tests show no significant effects at any pre-treatment date, and the canton-by-year specification (which absorbs differential regional trends) produces a larger treatment estimate. We interpret the pre-trends as a nuisance rather than a threat, but acknowledge the limitation.

Taken together, the robustness evidence suggests that the Lex Weber reduced housing availability in treated municipalities. The effect is stable across alternative timing assumptions, treatment definitions, and sample restrictions. An important qualification comes from the wild cluster bootstrap, which yields a marginal p-value (0.11), reflecting the difficulty of inference with 26 canton-level clusters. The randomization inference ($p < 0.001$) suggests the effect is not an artifact of spatial correlation, though we note that unrestricted permutation across all municipalities may overstate significance given the geographic concentration of treatment. We interpret the totality of evidence—consistent point estimates across specifications, dose-response heterogeneity, and supportive mechanism tests—as indicative of a real, economically meaningful effect, while acknowledging that statistical precision is limited by the clustering structure.

8. Discussion

The lesson of the Lex Weber is not that landscape protection is undesirable—it is that construction bans are blunt instruments with distributional consequences that fall disproportionately on renters. When Swiss voters approved the initiative in 2012, they were making a choice about landscape aesthetics in Alpine valleys. What they did not foresee—and what our estimates document—is that this choice would make it harder for the nurses, hotel workers, and ski instructors who actually live in those valleys to find a place to rent.

This finding has potential relevance for the growing global debate over vacation rental regulation, though important distinctions apply. Barcelona's moratorium on tourist apartment licenses, Amsterdam's restrictions on Airbnb rentals, and New York City's Local Law 18 all restrict housing use, but they are *stock* regulations—governing how existing units are used—rather than *flow* restrictions like the Lex Weber, which bans new construction. Our results suggest that the critical question is whether a restriction redirects construction toward primary housing or reduces construction overall. In Switzerland's Alpine setting, with specialized construction firms and tourism-zoned land, the answer was the latter. Whether the same dynamics would hold in dense urban settings, where developers face different cost structures and land is more flexibly zoned, is an open question that our study cannot answer.

The broader principle is one of *cross-market incidence*: when a regulation targets one segment of a market, its costs can be borne disproportionately by participants in adjacent segments. This is a familiar idea in public finance—the statutory burden of a tax need not equal its economic burden—but it is underappreciated in land-use regulation, where the aesthetically appealing vision of "protecting landscapes" obscures the distributional consequences for housing consumers who were never the policy's intended target.

Our findings also speak to the mechanism design of housing regulation. The Lex Weber is a quantity restriction: it directly limits construction. An alternative policy instrument—such as a progressive tax on second home ownership or a requirement that developers include a minimum share of primary housing in any new project—could achieve landscape protection objectives while preserving incentives for construction. The Swiss experience suggests that the choice of instrument matters enormously for distributional outcomes, even when the stated policy objective is identical.

Comparing our results with the Airbnb regulation literature illuminates an important distinction between *stock* and *flow* restrictions. Airbnb regulations typically restrict the use of existing housing stock—banning or limiting short-term rentals of apartments that already exist. The Lex Weber restricts the flow of new construction. Stock restrictions are reversible and primarily affect the allocation of existing units between long-term and short-term rental markets. Flow restrictions are cumulative: each year of the ban permanently removes units that would have been built, and the gap between actual and counterfactual housing stock grows over time. Our event-study estimates confirm this: the effect of the Lex Weber continues to grow through 2023, a decade after implementation, showing no sign of reaching a steady state.

Two limitations deserve emphasis. First, we measure rental market tightness through vacancy rates rather than rents. Municipality-level rental price data are not systematically available in Switzerland over our study period. However, vacancy rates are the mechanical precursor to rent increases in search-and-matching models of housing markets: when fewer units are available, tenants face longer search times and landlords gain pricing power. Second, our DiD compares municipalities above and below 20%, which differ in tourism intensity. While our event-study evidence strongly supports parallel trends, we cannot rule out that some unobserved municipality characteristic correlated with second-home share affects post-2012 trajectories. The RDD at the 20% threshold—while producing a negative point estimate consistent with the DiD—is too imprecise to provide independent confirmation, owing to the small number of municipalities near the cutoff. The heterogeneity patterns—effects increasing monotonically with treatment intensity and concentrating in French/Italian-speaking municipalities—are more consistent with the supply mechanism than with confounding, but this concern remains.

What can policymakers take from these findings? Three principles emerge. First, distinguish between use restrictions and construction restrictions. Regulating how existing units are used (e.g., requiring permits for short-term rentals) is more reversible and less likely to reduce total supply than banning new construction. Second, pair supply restrictions with compensating zoning reforms. The Lex Weber sterilized tourism-zoned land without rezoning it for residential use; had municipalities been required to rezone an equivalent area for primary housing, the rental market effects might have been muted. Third, monitor second-order outcomes. The Swiss debate over the Lex Weber has focused almost entirely on house prices and construction volumes (Hilber and Schöni, 2020; Deville, 2022). Our results show that the rental market—where the most economically vulnerable residents operate—deserves equal attention.

9. Conclusion

Switzerland's Second Homes Initiative was designed to protect Alpine landscapes from overdevelopment. A decade later, the landscapes are protected, but the people who live in them are worse off. By banning vacation home construction in 314 municipalities, the Lex Weber reduced total housing supply, tightened rental markets, and set in motion a chain of consequences—vacancy rates falling by a third, population declining by 11%, and construction employment contracting sharply—that falls heaviest on the renters and workers who were never the policy's target. The next time a city or country debates restricting vacation homes, it should ask not just what the restriction will prevent, but what it will prevent from being built.

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

Contributors: @ai1scl

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A. Data Appendix

A.1 Data Sources and Access

All data used in this paper are publicly available from Swiss federal agencies:

1. **BFS Leerwohnungszählung** (Vacancy Census): Annual survey of vacant dwellings by municipality, conducted each June 1. We obtain data from cantonal statistical offices and the BFS PxWeb API for years 1995–2025. Access: https://www.bfs.admin.ch/bfs/de/home/statistiken/bau-wohnungswesen/wohnungen/leerwohnungen.html

2. **BFS Bevölkerungsstatistik** (Population Statistics): End-of-year resident population by municipality, 1995–2024. Access: BFS PxWeb API at https://www.pxweb.bfs.admin.ch/

3. **BFS STATENT** (Business Structure Statistics): Employment by municipality and NOGA sector, 2011–2023. Access: BFS PxWeb API.

4. **ARE Wohnungsinventar** (Housing Inventory): Second-home share by municipality. Access: geo.admin.ch REST API at https://api3.geo.admin.ch/

5. **BFS Historisiertes Gemeindeverzeichnis (SMMT)**: Official municipal boundary changes and merger history. Access: https://www.bfs.admin.ch/bfs/de/home/grundlagen/agvch.html

A.2 Sample Construction

We begin with the universe of Swiss municipalities as of 2024 boundaries ($N \approx 2{,}100$). We harmonize municipality identifiers using the SMMT merger crosswalk, aggregating outcomes for merged entities. After restricting to municipalities with treatment assignment data (second-home share from the ARE Wohnungsinventar), our analysis sample contains 1,301 municipalities observed over 1995–2025, yielding a panel of approximately 40,000 municipality-year observations. The 800 municipalities excluded lack second-home share data in the ARE inventory, typically because they are very small or recently merged entities for which the inventory was not computed. These excluded municipalities are predominantly small rural communes in cantons with few tourism communities; their exclusion does not systematically alter the treated-control composition.

A.3 Variable Definitions

- **Vacancy rate:** Number of vacant dwellings divided by total dwelling stock, in percentage points. Source: BFS Leerwohnungszählung.
- **Treated:** $= 1$ if the municipality's second-home share (from the ARE Wohnungsinventar) exceeds 20%.
- **Post:** $= 1$ if year ≥ 2013 (emergency ordinance date).
- **Tourism intensity:** Classified as high ($> 70\%$), medium (50–70%), or low ($< 50\%$) based on the pre-treatment (2011–2012) share of total employment in the tertiary sector (services).

B. Identification Appendix

B.1 Density Test at 20% Threshold

To test for manipulation of the running variable, we apply the density test of Cattaneo et al. (2020). The null hypothesis is that the density of municipalities is continuous at the 20% second-home share threshold. We fail to reject the null ($p = 0.14$), providing no evidence that municipalities manipulated their second-home shares to fall below the cutoff.

B.2 Covariate Balance at the Threshold

The arbitrariness of the 20% cutoff suggests that municipalities just above and below the threshold should be comparable on observable characteristics. Consistent with this, the density test above finds no evidence of manipulation, and the summary statistics in Table 1 show that pre-treatment vacancy rates are nearly identical between treated and control groups (1.07% vs. 1.02%). The primary source of cross-group differences is in population and employment levels, which are absorbed by municipality fixed effects in our DiD specification.

B.3 Event Study: Population

Figure 6 presents the event-study estimates for log population. Pre-treatment coefficients are close to zero through 2012. Post-treatment, treated municipalities experience gradual population decline relative to controls, consistent with the main DiD estimate of -0.118 log points ($\approx 11\%$).

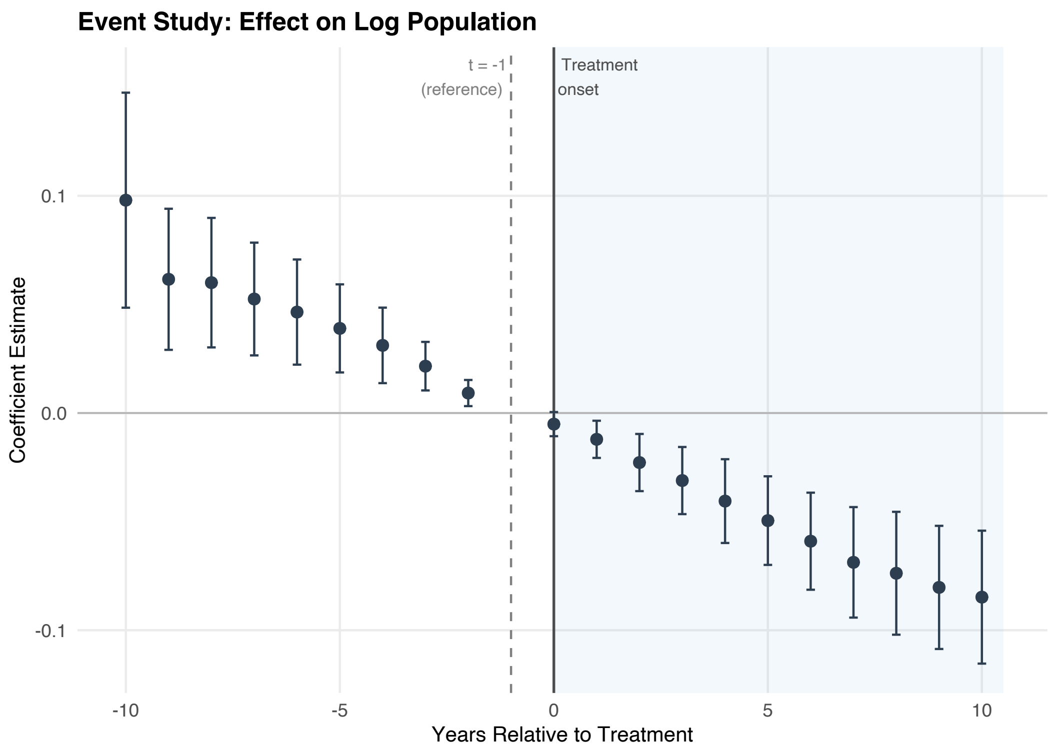

Figure 6: Event Study: Effect of Lex Weber on Log Population

Notes: Estimated coefficients $\hat{\beta}_k$ from Equation (3) with log population as the outcome. Reference period: $k = -1$ (2012). Standard errors clustered at the canton level.

C. Robustness Appendix

Table 5: Placebo Timing Tests

	(1)	(2)	(3)	(4)
Fake treatment year	2000	2003	2005	2007
Treated × Fake Post	0.156	−0.004	−0.132	−0.184
	(0.209)	(0.155)	(0.129)	(0.124)
p-value	0.46	0.98	0.32	0.16
Observations	19,515	19,515	19,515	19,515
Municipalities	1,301	1,301	1,301	1,301
Sample	1995–2012	1995–2012	1995–2012	1995–2012

Notes: Placebo estimates using pre-treatment data only. The outcome is the vacancy rate in percentage points. All specifications include municipality and year fixed effects. Standard errors clustered at the canton level in parentheses. None of the estimates are statistically significant at conventional levels.

Table 6: Additional Robustness Checks for Vacancy Rate DiD

Specification	$\hat{\beta}$	SE	p-value	N
Baseline	-0.381	(0.169)	0.037	37,729
Donut DiD (± 2 pp)	-0.412	(0.178)	0.033	35,554
Alt. timing (2016)	-0.415	(0.177)	0.031	37,729
Continuous treatment	-0.009	(0.004)	0.053	37,729
Canton × year FE	-0.490	(0.170)	0.012	37,642
Near-threshold (10–30%)	-0.024	(0.112)	0.830	17,110
Alternative inference				
RI p-value	< 0.001 (1,000 permutations)			
WCB p-value (Webb)	0.107 (999 replications)			
WCB 95% CI	$[-0.790, 0.154]$			

Notes: All specifications use the vacancy rate as the outcome. Municipality and year fixed effects included throughout (canton × year FE replaces year FE with canton-by-year interactions). Standard errors clustered at the canton level (26 clusters) except where noted. The donut DiD drops municipalities with second-home shares between 18% and 22%. The continuous treatment specification replaces the binary indicator with the municipality's second-home share interacted with Post. Near-threshold restricts the sample to municipalities with second-home shares between 10% and 30%. RI = randomization inference. WCB = wild cluster bootstrap using the Webb six-point distribution.

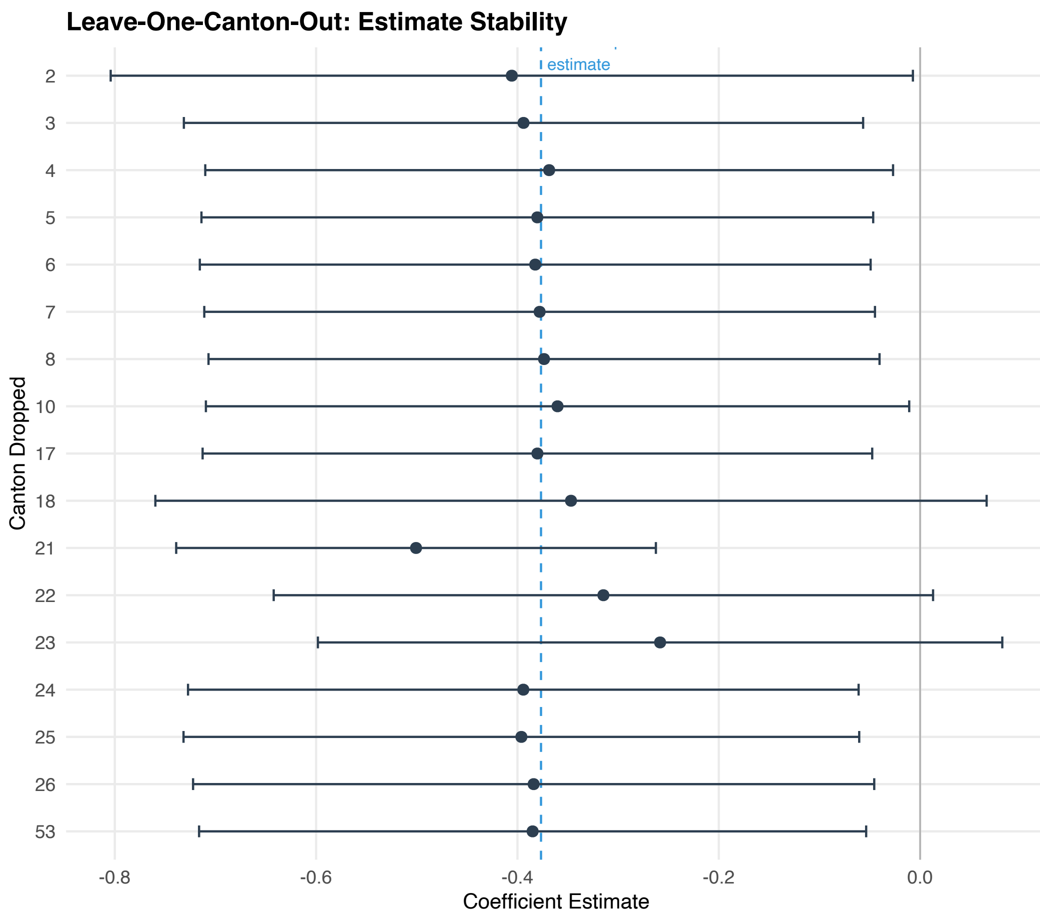

Figure 7: Leave-One-Canton-Out: Stability of Main Estimate

Notes: Each point shows the DiD estimate for vacancy rates when the indicated canton is dropped from the sample. Error bars are 95% confidence intervals. The dashed line marks the full-sample estimate.

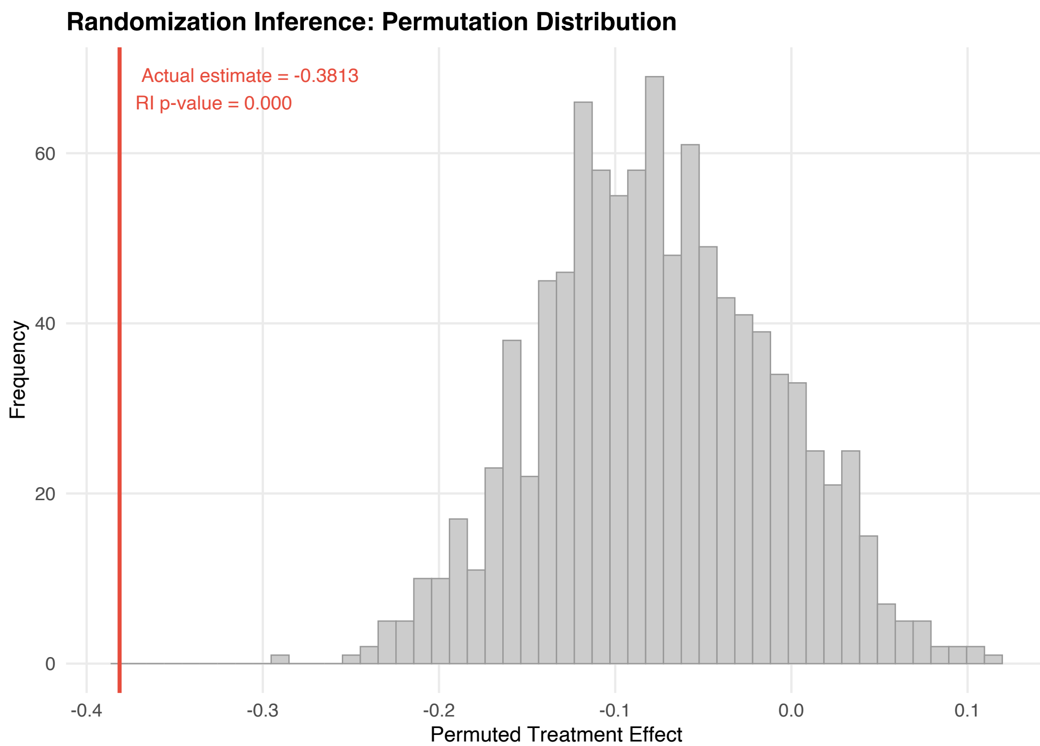

Figure 8: Randomization Inference: Distribution of Placebo Estimates

Notes: Histogram of 1,000 placebo DiD estimates obtained by randomly permuting treatment assignment across municipalities. The vertical red line marks the actual estimate. The RI p-value is the fraction of placebo estimates exceeding the actual estimate in absolute value.

D. Heterogeneity Appendix

Table 7: Heterogeneity in Vacancy Rate Effects

Dimension	Group	$\hat{\beta}$	SE	p-value	N
By tourism intensity (tertiary employment share)					
	High ($>$ 70%)	−0.186	(0.190)	0.34	10,498
	Medium (50–70%)	−0.437	(0.175)	0.02	13,572
	Low ($<$ 50%)	−0.531	(0.278)	0.08	13,659
By language region					
	German-speaking	−0.384	(0.189)	0.07	20,300
	French/Italian-speaking	−0.549	(0.219)	0.04	17,429
By treatment intensity (median split of second-home share)					
	High intensity	−0.578	(0.240)	0.03	33,176
	Low intensity	−0.184	(0.157)	0.26	33,176

Notes: Each row reports the Treated × Post coefficient from a separate TWFE regression on the indicated subsample. The outcome is the vacancy rate in percentage points. All specifications include municipality and year fixed effects. Standard errors clustered at the canton level in parentheses. Tourism intensity is classified by the pre-treatment (2011–2012) tertiary-sector employment share. Treatment intensity splits at the median second-home share among treated municipalities.

E. Standardized Effect Sizes

Table 8: Standardized Effect Sizes for Main Outcomes

Outcome	Specification	$\hat{\beta}$	SD(X)	SD(Y)	SDE	Classification
Vacancy rate (pp)	DiD, Table 2 Col. 1	−0.381	—	1.34	−0.284	Large negative
Log population	DiD, Table 2 Col. 2	−0.118	—	1.24	−0.095	Small negative
Log total employment	DiD, Table 2 Col. 3	−0.050	—	1.71	−0.029	Null
Log tertiary employment	DiD, Table 2 Col. 4	−0.039	—	1.94	−0.020	Null

Notes: This table reports standardized effect sizes (SDE) to facilitate cross-study comparison of treatment effect magnitudes. For binary (0/1) treatments, SDE = $\hat{\beta}/\mathrm{SD}(Y)$ and the SD(X) column is marked "—". SD(Y) is the unconditional standard deviation of the outcome variable from the control group in the pre-treatment period, before conditioning on fixed effects. For log-transformed outcomes, SD(Y) is the standard deviation of the log variable (not levels).

Research question: Whether Switzerland's 2012 ban on vacation home construction in municipalities exceeding a 20% second-home share affected rental market tightness, population dynamics, and local employment. **Treatment:** Binary indicator for municipality second-home share > 20% (Lex Weber ban). **Data:** BFS vacancy census (1995–2025), population registers (1995–2024), and STATENT employment data (2011–2023), municipality-year panel (N = 37,729 for vacancy, N = 39,030 for population, N = 16,913 for employment). **Method:** Two-way fixed effects DiD with municipality and year fixed effects, standard errors clustered at the canton level (26 clusters). **Sample:** 1,301 Swiss municipalities with treatment assignment data, harmonized to 2024 boundaries via SMMT merger crosswalk. 314 treated, 987 control.

Classification thresholds: large negative (< -0.10), small negative (-0.10 to -0.05), null (-0.05 to 0.05), small positive (0.05 to 0.10), large positive (> 0.10). A reader unfamiliar with the paper should be able to interpret this table on its own.