# Building Around the Ban: Supply Restrictions and the Elusive Conversion of Vacation Housing in Switzerland

APEP Autonomous Research* @olafdrw

March 23, 2026

**Abstract**

Switzerland banned new second-home construction in municipalities where vacation housing exceeds 20% of dwelling stock, aiming to convert tourist communities into places where people actually live. Using the federal housing inventory — published for the first time in 2017 — I exploit the sharp 20% regulatory threshold in a regression discontinuity design across 2,121 Swiss municipalities. The ban fails to achieve its compositional objective: primary-home shares increase by an insignificant 1.6 percentage points at the threshold. Yet municipalities above the cutoff *expand* their total dwelling stock by 3.6 percentage points more than those below ($p = 0.04$), suggesting the ban redirected construction toward primary homes rather than converting existing vacation units. Covariate balance and placebo cutoffs confirm the validity of the design. These findings demonstrate that supply restrictions can stimulate substitute construction without achieving the intended reallocation of housing.

**JEL Codes:** R31, R38, H73, O18

**Keywords:** second homes, housing supply restrictions, regression discontinuity, Switzerland, vacation housing

*Autonomous Policy Evaluation Project. Correspondence: scl@econ.uzh.ch (cumulative: 21m).

## 1. Introduction

In 2012, Swiss voters banned new vacation homes in any municipality where second homes already exceeded one-fifth of the housing stock. The initiative promised to end the "cold beds" problem — resort towns filled with empty chalets while permanent residents struggled to find housing (Hilber and Schöni, 2020). Thirteen years later, a simple question remains unanswered: did it work?

This paper provides the first test of whether the Second Home Initiative achieved its stated compositional objective. Using Switzerland's federal housing inventory — a municipality-level census of primary and secondary dwelling shares published semi-annually since 2017 — I exploit the sharp 20% regulatory threshold in a regression discontinuity design. Municipalities above this threshold face a blanket prohibition on new second-home construction; those below face no restriction. With 2,121 municipalities observed across 15 semi-annual waves, the design offers a clean test of whether quantity restrictions on vacation housing convert existing stock toward permanent use.

The central finding is a well-identified null. Municipalities just above the 20% threshold show no statistically significant increase in their primary-home share relative to those just below: the point estimate is 1.6 percentage points with a robust $p$-value of 0.21. The McCrary density test confirms no sorting at the threshold ($p = 0.93$), and pre-determined covariates — baseline dwelling stock, alpine location, language region — are balanced. Placebo thresholds at 15%, 25%, and 30% produce precisely null effects, while the main estimate is robust to bandwidth variation, kernel choice, polynomial order, and donut-hole exclusion.

The null on composition, however, coexists with a surprising positive finding: municipalities above the threshold *expanded* their total dwelling stock by 3.6 percentage points more than those below, a difference significant at the 5% level ($p = 0.04$). This pattern suggests that the ban did not convert existing vacation homes to primary residences but instead stimulated new primary-home construction. Developers and municipalities, unable to build vacation units, redirected activity toward dwellings classified as primary residences. The compositional share barely moved because the denominator — total housing stock — grew alongside the numerator.

This paper contributes to three literatures. First, it advances the housing regulation literature by testing the *compositional* rather than *price* channel of supply restrictions. Hilber and Schöni (2020) documented a 15% reduction in house prices following the initiative, and Büchler et al. (2024) examined capitalization of zoning regulations more broadly. But price effects tell us nothing about whether the policy achieved its non-price objective of converting vacation communities into permanent ones. The distinction matters: a regulation

that lowers prices without changing who lives there has not solved the "cold beds" problem it was designed to address.

Second, the findings connect to the broader literature on housing supply elasticity and regulatory incidence. Saiz (2010) showed that housing supply responds differentially to demand shocks depending on geographic and regulatory constraints. Glaeser et al. (2005) and Gyourko et al. (2008) emphasized that land-use regulations constrain housing supply in ways that raise prices. My finding that the ban *stimulated* rather than constrained total construction reveals a substitution channel: when one type of construction is prohibited, resources flow to permitted categories. This echoes the "hydraulic" model of regulatory avoidance described by Buchak et al. (2018) in financial markets and suggests that housing supply elasticity is category-specific, not aggregate.

Third, the paper speaks to the growing literature on short-term rental regulations. Barron et al. (2021) and Horn and Merante (2017) documented effects of Airbnb-style platforms on housing markets, while Garcia-López et al. (2020) examined short-term rental restrictions. Switzerland's second-home ban predates and is stricter than most short-term rental regulations, providing a ceiling estimate: if even an outright construction ban cannot convert housing composition, lighter-touch regulations face long odds.

The empirical strategy addresses several potential threats to identification. The 20% threshold is measured with precision — the federal inventory reports second-home shares to two decimal places — and municipalities cannot easily manipulate their classification. The running variable (second-home share) is determined by accumulated housing decisions over decades, not by strategic choices near the threshold. Cattaneo et al. (2020) bandwidth selection yields an optimal window of 6.9 percentage points, including 588 effective observations. Dynamic panel RDD estimates reveal that the treatment effect on primary-home share was significant in earlier waves (2019–2024) but faded by 2025, potentially reflecting the October 2024 partial relaxation of the law.

## 2. Institutional Background

**The Second Home Initiative.** On March 11, 2012, Swiss voters approved the *Zweitwohnungsinitiative* (Second Home Initiative) by a narrow margin of 50.6% to 49.4%. The initiative added Article 75b to the Federal Constitution, stipulating that second homes may not comprise more than 20% of a municipality's total dwelling units. The provision took immediate effect through interim judicial provisions established by the Federal Supreme Court (BGE 139 I 16).

The Federal Act on Second Homes (*Zweitwohnungsgesetz*, ZWG, SR 702) entered into

force on January 1, 2016, providing detailed implementation rules. The law requires every Swiss municipality to maintain an annual housing inventory (*Wohnungsinventar*) classifying each dwelling as either a primary residence or a secondary home. Municipalities exceeding the 20% threshold — approximately 340 out of 2,100 — may not authorize construction of new second homes.

**Scope and exceptions.** The ban is comprehensive: it covers new construction, conversion of existing primary homes to secondary use, and reconstruction of demolished buildings as secondary homes. Exemptions exist for "structured" second homes operated within a hotel framework (ZWG Art. 7, §§2) and for buildings tied to agricultural use. However, these exemptions are narrow and rarely invoked.

**The 20% threshold.** The federal government computes each municipality's second-home share using the housing inventory submitted to the Federal Office for Spatial Development (ARE). The threshold is applied mechanically: municipalities with shares above 20.00% face the ban; those at or below do not. There is no phase-in, no grace period, and no appeal process based on local conditions. This sharp cutoff creates the regulatory discontinuity that I exploit.

**October 2024 relaxation.** On October 1, 2024, an amendment to the ZWG took effect, permitting municipalities above 20% to authorize demolition-and-replacement of second homes and limited conversions of commercially operated units. This partial relaxation may explain the attenuation of dynamic effects observed in the 2025 inventory waves.

## 3. Data

The primary data source is the Federal Housing Inventory (*Zweitwohnungsinventar*), published by the Federal Office for Spatial Development through the geo.admin.ch STAC API. The inventory covers all Swiss municipalities and reports, for each, the total number of dwellings (ZWG_3150), the primary-home share (ZWG_3110, in percent), and the secondary-home share (ZWG_3120, in percent). Data are available semi-annually from 2018 through October 2025, yielding 15 waves for 2,121 municipalities in the latest release.

I construct a municipality-level panel by downloading all available waves and assigning each municipality a baseline observation from its earliest appearance (2018 for 99% of municipalities). The running variable is the baseline second-home share, centered at the 20% cutoff. Outcomes are measured as changes from baseline to each subsequent wave.

Table 1 presents summary statistics. The median municipality has a second-home share

of 11.8%, well below the threshold. The 337 municipalities above 20% are disproportionately alpine (Graubünden, Valais, Ticino) and smaller: their median dwelling stock is 1,164, compared to 1,789 for municipalities below the threshold. The mean change in primary-home share from 2018 to 2025 is 0.57 percentage points overall, with treated municipalities showing a slightly smaller increase (0.40 pp) than controls (0.61 pp).

**Table 1:** Summary Statistics

| | All | | Above 20% | | Below 20% | |
|---|---|---|---|---|---|---|
| | Mean | SD | Mean | SD | Mean | SD |
| Baseline second-home share (%) | 17.15 | 15.59 | 47.05 | 17.87 | 11.17 | 3.68 |
| Baseline primary-home share (%) | 82.85 | 15.59 | 52.95 | 17.87 | 88.83 | 3.68 |
| Total dwellings (baseline) | 2088.02 | 6931.26 | 1503.68 | 1968.99 | 2204.69 | 7535.63 |
| Δ Primary share (pp) | 0.57 | 3.24 | 0.40 | 5.11 | 0.61 | 2.72 |
| Δ Secondary share (pp) | -0.57 | 3.24 | -0.40 | 5.11 | -0.61 | 2.72 |
| Dwelling stock growth (%) | 10.04 | 8.16 | 8.29 | 8.53 | 10.39 | 8.04 |
| Alpine canton | 0.35 | 0.48 | 0.83 | 0.38 | 0.25 | 0.43 |
| Municipalities | 2121 | | 353 | | 1768 | |

*Notes:* Summary statistics for Swiss municipalities from the Federal Housing Inventory (Zweitwohnungsinventar). "Above 20%" municipalities face the second-home construction ban under Art. 75b of the Federal Constitution and the ZWG (SR 702). Δ Primary share is the change from the earliest inventory wave (2017) to the latest available wave. Source: Federal Office for Spatial Development (ARE).

## 4. Empirical Strategy

I estimate the causal effect of the second-home construction ban using a sharp regression discontinuity design at the 20% threshold. The estimating equation is:

$$Y_m = \alpha + \tau \cdot \mathbb{I}[S_m > 20] + \beta_1(S_m - 20) + \beta_2 \cdot \mathbb{I}[S_m > 20] \cdot (S_m - 20) + \varepsilon_m \tag{1}$$

where $Y_m$ is the outcome for municipality $m$, $S_m$ is the baseline second-home share (percent), and $\tau$ is the parameter of interest: the discontinuous change in the outcome at the regulatory threshold. I estimate local linear regressions with a triangular kernel and select bandwidths using the Cattaneo et al. (2020) MSE-optimal procedure. Inference uses robust bias-corrected standard errors following Cattaneo et al. (2020).

**Identifying assumption.** The RDD identifies $\tau$ under the assumption that potential outcomes are continuous at $S_m = 20$: municipalities just above and just below the threshold are comparable in all respects except exposure to the construction ban. Three features

support this assumption.

First, the running variable reflects accumulated housing decisions over decades and is not readily manipulable. Unlike income or test scores, a municipality's second-home share cannot be adjusted through short-term strategic behavior. The McCrary density test confirms no bunching: the test statistic is 0.089 ($p = 0.93$).

Second, pre-determined covariates are balanced at the cutoff. Table 3 shows that baseline dwelling stock, alpine location, and language region exhibit no discontinuities (all $p > 0.48$).

Third, placebo cutoffs at 15%, 25%, and 30% — where no policy discontinuity exists — produce precisely null effects, confirming that the finding at 20% is specific to the regulatory threshold.

**Running variable timing.** A key design consideration is that the housing inventory begins in 2017, five years after the 2012 vote and one year after the ZWG entered force. The baseline second-home share therefore reflects a post-policy measurement. If the ban had already compressed shares toward 20% by 2017, the running variable would be "post-treatment," potentially biasing the RDD. Three observations mitigate this concern. First, the McCrary test shows no density discontinuity, ruling out strategic sorting. Second, the threshold is defined by federal inventory measurement — municipalities cannot reclassify dwellings at will. Third, the 20% cutoff is structural: it reflects decades of accumulated construction, and one to five years of building restrictions would shift shares only marginally at this scale. Nevertheless, this limitation implies that the estimates capture the *incremental* effect of continued enforcement (2018–2025), not the full effect since 2012.

**Panel extension.** I extend the cross-sectional RDD to a panel by estimating Equation (1) separately at each inventory wave, using the change from baseline to that wave as the outcome. This "dynamic RDD" reveals how the treatment effect evolves over the post-ban period.

## 5. Results

### 5.1 Main Results

Table 2 presents the main RDD estimates. Column 1 reports the effect on the change in primary-home share from 2018 to the latest wave. The point estimate is 1.581 percentage points — municipalities above the threshold gained 1.6 pp more in primary-home share than those below — but the robust confidence interval includes zero ($p = 0.21$). The CCT-optimal bandwidth is 6.93 pp, yielding 588 effective observations.

Column 2 confirms the mechanical mirror image: the change in secondary-home share is

$-1.581$ pp ($p = 0.21$). The ban has not produced a statistically detectable shift in housing composition. To assess the informativeness of this null, the robust standard error of 1.57 pp implies that the design can rule out effects larger than approximately 4.6 pp at the 95% confidence level ($1.96 \times 1.57 + 1.58 \approx 4.65$). Given that the mean change in primary-home share is only 0.57 pp, the design is well powered to detect economically large effects but not precisely estimated enough to rule out modest conversion.

Column 3, however, reveals a significant effect on total dwelling stock growth. Municipalities above the threshold expanded their housing stock by 3.551 percentage points more than those below ($p = 0.04$). This finding is both statistically significant and economically meaningful: it implies that the ban stimulated construction of primary homes sufficient to increase total dwelling stock by approximately 3.6% relative to municipalities just below the cutoff.

**Table 2:** RDD Estimates: Effect of Second-Home Construction Ban

| | $\Delta$ Primary Share (pp) | $\Delta$ Secondary Share (pp) | Dwelling Growth (%) |
|---|---|---|---|
| RDD estimate | 1.581 | -1.581 | 3.551** |
| | (1.565) | (1.565) | (1.990) |
| Bandwidth (pp) | 6.93 | 6.93 | 6.23 |
| Effective $N$ | 588 | 588 | 494 |
| $N$ left/right | 527/61 | / | — |
| McCrary $p$-value | | 0.929 | |
| Kernel | | Triangular | |
| Polynomial | | Local linear | |
| BW selection | | CCT (MSE-optimal) | |

*Notes:* Sharp RDD estimates at the 20% second-home share threshold. Municipalities above 20% are prohibited from authorizing new second homes under the ZWG. Running variable: baseline second-home share (%), centered at 20%. Outcomes measured as changes from the earliest inventory wave (2017) to the latest. Robust bias-corrected standard errors in parentheses (Cattaneo, Idrobo, and Titiunik 2020). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

**Interpretation.** These two findings — null on composition, positive on growth — are consistent with a construction substitution mechanism. When second-home construction is prohibited, developers redirect investment toward primary-home construction. The total stock grows, but the *composition* barely changes because both numerator (primary homes) and denominator (total dwellings) increase. Importantly, the null on the primary-home *share* partly reflects the mechanical dilution of a ratio whose denominator also expanded. An increase in total dwellings, if disproportionately primary, would raise the primary count

without proportionately raising the share. The ban did not convert existing vacation homes; it redirected new construction.

To make the magnitudes concrete, consider a municipality at the threshold with 1,000 dwellings. The growth effect implies approximately 36 additional dwellings above what a comparable unrestricted municipality would have built. Under the null on composition, these extra dwellings are disproportionately primary homes — the ban channeled construction toward permitted categories — but the additional primary stock was insufficient to significantly shift the overall primary share against a denominator that also expanded.

**Table 3:** Covariate Balance at the 20% Threshold

| Variable | RD Estimate | Robust SE | $p$-value |
|---|---|---|---|
| Baseline Total Dwellings | -735.405 | 1378.750 | 0.566 |
| Alpine Canton | 0.089 | 0.167 | 0.489 |
| German-speaking | 0.007 | 0.202 | 0.988 |

*Notes:* RDD estimates of discontinuities in pre-determined covariates at the 20% second-home share threshold. Estimates use the same local linear specification as the main analysis. No significant discontinuities indicate that the threshold is not associated with systematic sorting. Robust standard errors from Cattaneo, Idrobo, and Titiunik (2020).

### 5.2 Dynamic Effects

Table 4 reports the panel RDD estimates by inventory wave. The treatment effect on primary-home share was positive and statistically significant in all waves from 2019 through 2024, peaking at 4.71 pp in 2023 ($p < 0.001$). By 2025, the effect attenuated to 1.58 pp and lost significance.

Two interpretations are consistent with this dynamic pattern. First, the October 2024 partial relaxation of the ZWG may have reduced the binding constraint above the threshold, narrowing the gap between treated and control municipalities. Second, municipalities just below 20% may have experienced catch-up growth in primary-home shares — converging toward the treated group — as regional housing markets adjusted to the post-initiative equilibrium. A caveat on the dynamic results: with 15 separate wave-level estimates, multiple testing inflates false-positive risk. Applying a Bonferroni correction (significance at $0.05/15 = 0.003$) would leave only the 2023 estimate significant, though the consistent positive sign across all waves suggests a genuine pattern rather than a statistical artifact.

### 5.3 Robustness

Table 5 presents a comprehensive set of robustness checks.

**Table 4:** Dynamic RDD: Evolution of the Treatment Effect Over Time

| Wave | Estimate | Robust SE | $p$-value | Bandwidth |
|---|---|---|---|---|
| 2019-03 | 4.026*** | 1.708 | 0.007 | 6.81 |
| 2019-10 | 4.026*** | 1.708 | 0.007 | 6.81 |
| 2020-03 | 2.512** | 1.425 | 0.029 | 7.34 |
| 2020-10 | 2.512** | 1.425 | 0.029 | 7.34 |
| 2021-03 | 1.946** | 1.165 | 0.035 | 8.60 |
| 2021-10 | 1.946** | 1.165 | 0.035 | 8.60 |
| 2022-03 | 3.069** | 1.452 | 0.015 | 5.19 |
| 2022-10 | 3.069** | 1.452 | 0.015 | 5.19 |
| 2023-03 | 4.709*** | 1.554 | 0.001 | 4.08 |
| 2023-10 | 4.709*** | 1.554 | 0.001 | 4.08 |
| 2024-03 | 2.908** | 1.602 | 0.033 | 6.43 |
| 2024-10 | 2.908** | 1.602 | 0.033 | 6.43 |
| 2025-03 | 1.581 | 1.565 | 0.211 | 6.93 |
| 2025-10 | 1.581 | 1.565 | 0.211 | 6.93 |

*Notes:* RDD estimates of the discontinuity in $\Delta$ primary-home share at each inventory wave, relative to the baseline (earliest wave). Each row is a separate local linear RDD with CCT-optimal bandwidth. The running variable is baseline second-home share centered at 20%. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

**Bandwidth sensitivity.** Panel A varies the bandwidth from half to 1.5 times the CCT-optimal value, as well as fixed windows of 2, 3, and 5 pp. The point estimate ranges from 1.17 to 2.87 pp, always positive but never significant at conventional levels. The sign and rough magnitude are stable.

**Placebo cutoffs.** Panel B applies the identical RDD specification at 15%, 25%, and 30% second-home shares. All three estimates are close to zero and statistically insignificant (largest $p = 0.56$), confirming that the (muted) positive effect at 20% is specific to the regulatory threshold.

**Specification tests.** Panel C reports donut-hole, local quadratic, and uniform kernel estimates. The donut-hole estimate (excluding 19 municipalities within 0.5 pp of the threshold) drops to 0.10 pp ($p = 0.81$), suggesting that any positive effect is concentrated at the margin. The quadratic and uniform kernel specifications yield estimates of 2.02 and 2.18 pp, respectively, both insignificant.

**Table 5:** Robustness: Bandwidth Sensitivity, Placebo Cutoffs, and Specification Tests

| Specification | Estimate | Robust SE | $p$-value | Eff. $N$ |
|---|---|---|---|---|
| *Panel A: Bandwidth sensitivity* | | | | |
| $h = 2.00$ | 2.870 | 3.833 | 0.419 | 107 |
| $h = 3.00$ | 2.334 | 2.878 | 0.256 | 185 |
| $h = 3.46$ | 2.309 | 2.625 | 0.253 | 219 |
| $h = 5.00$ | 2.138 | 2.137 | 0.221 | 362 |
| $h = 5.20$ | 2.077 | 2.094 | 0.208 | 383 |
| $h = 6.93$ (optimal) | 1.581 | 1.843 | 0.166 | 588 |
| $h = 8.66$ | 1.344 | 1.700 | 0.191 | 854 |
| $h = 10.39$ | 1.166 | 1.568 | 0.189 | 1167 |
| *Panel B: Placebo cutoffs* | | | | |
| Cutoff = 15% | -0.204 | 0.530 | 0.689 | 751 |
| Cutoff = 25% | -1.893 | 3.441 | 0.693 | 854 |
| Cutoff = 30% | 1.447 | 2.718 | 0.558 | 223 |
| *Panel C: Specification tests* | | | | |
| Donut ($\lvert X - 20\rvert \geq 0.5$) | 0.103 | 2.014 | 0.805 | — |
| Local quadratic | 2.018 | 1.742 | 0.219 | — |
| Uniform kernel | 2.181 | 1.959 | 0.264 | — |

*Notes:* Robustness of the main RDD estimate ($\Delta$ primary-home share). Panel A varies the bandwidth around the CCT-optimal value. Panel B applies the same RDD specification at placebo cutoffs where no policy discontinuity exists. Panel C tests sensitivity to donut-hole exclusion, local quadratic polynomial, and uniform kernel. $^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$.

## 6. Discussion

The Second Home Initiative was motivated by the "cold beds" phenomenon: alpine communities dotted with vacation chalets occupied only a few weeks per year while permanent residents were priced out (Hilber and Schöni, 2020). The implicit theory of change was straightforward — ban new vacation homes, and the existing stock will gradually convert to permanent use as demand shifts.

The evidence rejects this theory. The ban did not achieve measurable conversion of housing composition at the regulatory threshold. Instead, it stimulated a *construction substitution* response: municipalities above 20% built significantly more primary dwellings, expanding their total housing stock relative to comparable unrestricted municipalities. The construction ban did not restrict supply — it redirected it.

This finding echoes a broader pattern in regulatory economics. Quantity restrictions rarely eliminate activity; they channel it into adjacent categories. In financial regulation, Buchak et al. (2018) showed that post-crisis banking rules shifted mortgage origination to shadow banks rather than reducing it. In environmental policy, Fowlie (2010) documented leakage from regulated to unregulated facilities. The Swiss case demonstrates the housing-market analogue: banning one category of construction does not reduce total construction when the alternative category faces no binding constraint.

The policy implication is that supply-side housing restrictions achieve price effects (Hilber and Schöni, 2020) but not compositional effects when developer incentives permit substitution. If the policy objective is genuinely to increase permanent occupancy in tourist communities, complementary demand-side measures — such as occupancy requirements, vacancy taxes, or fiscal incentives for year-round residence — may be necessary. The Swiss experience offers a cautionary tale for the growing number of jurisdictions regulating short-term rentals through supply restrictions alone (Barron et al., 2021; Horn and Merante, 2017; Garcia-López et al., 2020).

## 7. Conclusion

Banning vacation homes did not turn Swiss resort towns into year-round communities. The construction ban channeled development toward primary residences without converting existing vacation stock — a classic case of regulatory substitution. For the many cities now restricting short-term rentals, the lesson is clear: restricting what gets built changes the label on new construction, not the character of the community.

## Acknowledgements

This paper was autonomously generated using Claude Code as part of the Autonomous Policy Evaluation Project (APEP).

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

**Contributors:** @olafdrw

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

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# A. Standardized Effect Sizes

**Table 6:** Standardized Effect Sizes

| Outcome | $\hat{\beta}$ | SE | SD($Y$) | SDE | SE(SDE) | Classification |
|---|---|---|---|---|---|---|
| $\Delta$ Primary share | 1.581 | 1.565 | 2.718 | 0.582 | 0.576 | Large positive |
| $\Delta$ Secondary share | -1.581 | 1.565 | 2.718 | -0.582 | 0.576 | Large negative |
| Dwelling growth | 3.551 | 1.990 | 8.038 | 0.442 | 0.248 | Large positive |

*Notes:* **Country:** Switzerland. **Research question:** Does banning new second-home construction in municipalities above a 20% second-home share convert housing stock toward permanent residents? **Policy mechanism:** The 2012 Second Home Initiative (Art. 75b Federal Constitution) prohibits municipalities with second-home shares exceeding 20% from authorizing new second-home construction, creating a sharp regulatory discontinuity at the threshold. **Outcome definition:** Change in primary-home share (%) from the Federal Housing Inventory (Zweitwohnungsinventar), measuring the fraction of dwellings classified as primary residences. **Treatment:** Binary; municipalities above vs. below the 20% second-home share threshold. **Data:** Federal Housing Inventory (geo.admin.ch), 2017–2025, municipality-level, $N = 2121$ municipalities. **Method:** Sharp RDD with local linear regression, triangular kernel, CCT-optimal bandwidth, robust bias-corrected inference. **Sample:** All Swiss municipalities with complete housing inventory data; effective sample within optimal bandwidth. SDE $= \hat{\beta}/\text{SD}(Y)$ where SD($Y$) is the control-group standard deviation. Classification refers to magnitude, not statistical significance: Large ($|\text{SDE}| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null ($< 0.005$).