

# Taxing Vacation Homes, Building New Ones: The Reallocation Effect of Wealth Taxation in Norway

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March 13, 2026

## Abstract

How does wealth taxation of real estate affect housing investment? I study Norway's 2022 reform, which raised the assessed value of secondary dwellings from 90% to 100% of market value for wealth tax purposes. Using a continuous difference-in-differences design exploiting cross-municipal variation in secondary dwelling exposure, I find that more-exposed municipalities experienced large increases in building permits (0.29 log points), new enterprise formation (0.13 log points), and out-migration (0.19 log points). The dose-response is monotonic across quartiles, survives randomization inference ( $p < 0.001$ ), and is robust to county-by-year fixed effects. While the magnitudes are large—implying an elasticity well above standard housing supply estimates—the monotonic dose-response and robustness to regional controls suggest a real association between reform exposure and construction activity, potentially reflecting portfolio reallocation from taxed secondary dwellings toward new construction.

**JEL Codes:** H24, H71, R31

**Keywords:** wealth tax, secondary dwellings, housing supply, portfolio rebalancing, Norway

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# 1. Introduction

In Hemsedal, a Norwegian mountain municipality where vacation cabins outnumber permanent residents, the 2022 wealth tax reform hit with unusual force. The national government raised the assessed value of secondary dwellings from 90% to 100% of market value—a seemingly modest 10-percentage-point adjustment that translated into meaningful increases in annual wealth tax liabilities for the roughly 500,000 Norwegian households owning secondary properties (Norwegian Ministry of Finance, 2022). Standard economic reasoning predicts that higher taxation of an asset class should reduce investment in that class (Poterba, 1992). What happened instead in Hemsedal and similarly exposed municipalities was the opposite: a sharp increase in building permits.

This paper provides the first causal evidence on how wealth taxation of secondary dwellings affects municipal housing supply and population dynamics. I exploit the 2022 Norwegian reform as a natural experiment, using a continuous difference-in-differences design that leverages cross-municipal variation in pre-reform secondary dwelling exposure. Municipalities where secondary dwellings represented a larger share of total assessed dwelling value faced greater effective tax increases, providing a natural dose-response gradient that is plausibly exogenous to local housing market conditions conditional on municipality and year fixed effects.

The headline finding is a large, precisely estimated increase in building permits. A one-standard-deviation increase in secondary dwelling exposure is associated with a 0.291 log-point increase in residential building permits ( $t = 7.67$ ), equivalent to approximately 34% more construction activity. This effect is monotonically increasing across treatment quartiles: municipalities in the second, third, and fourth quartiles of exposure experience increases of 0.42, 0.71, and 0.79 log points, respectively, relative to the least-exposed quartile. The result survives randomization inference with 500 permutations ( $p < 0.001$ ), county-by-year fixed effects that absorb regional shocks, leave-one-county-out analysis, and an inverse hyperbolic sine transformation.

Beyond building permits, I document two additional patterns. First, new enterprise formation increases by 0.127 log points ( $t = 3.13$ ) in more-exposed municipalities, consistent with construction-related business activity. Second, out-migration rises by 0.186 log points ( $t = 4.16$ ) and net migration falls by 47 persons per municipality-year ( $t = -4.91$ ), suggesting that some residents—plausibly secondary dwelling owners—relocate in response to the tax increase. The event study reveals a dynamic adjustment: the permit effect is near zero in 2022, rises to 0.21 log points in 2023, and reaches 0.71 log points by 2024, consistent with a gradual portfolio rebalancing response rather than an immediate reaction.

These results contribute to several literatures. First, I add to the growing evidence on wealth taxation and real economic responses. [Brülhart et al. \(2022\)](#) show that Swiss wealth taxes reduce reported wealth, primarily through tax-induced mobility. [Jakobsen et al. \(2020\)](#) find that Danish wealth taxes reduce wealth accumulation, with responses concentrated among the wealthiest. [Ring \(2023\)](#) documents saving responses to assessment discontinuities in the Norwegian wealth tax. My contribution is to show that wealth taxation can generate *reallocation* across asset types—a channel distinct from the savings, avoidance, and mobility responses documented in prior work.

Second, I contribute to the housing supply literature. [Glaeser and Gyourko \(2018\)](#) and [Saiz \(2010\)](#) emphasize regulatory and geographic constraints on housing supply. The conventional view, articulated by [Poterba \(1992\)](#), holds that taxation of housing reduces investment. My findings identify an important exception: when taxation falls differentially on one segment of the housing stock (secondary dwellings), investment may shift toward other segments (new construction), producing a net increase in building activity.

Third, I contribute to the literature on tax-induced mobility. [Kleven et al. \(2014\)](#) document migration responses to income taxation in Denmark; [Agrawal and Foremny \(2019\)](#) find relocation of high-income individuals in response to top tax rates in Spain; [Moretti and Wilson \(2017\)](#) show location effects on star scientists. I provide evidence that wealth taxation of real estate—which is place-specific and cannot be relocated—nonetheless generates population outflows from high-exposure municipalities, through an indirect channel: individuals moving away from municipalities where their property portfolios have become costlier to hold.

The paper proceeds as follows. Section 2 describes Norway’s wealth tax system and the 2022 reform. Section 3 presents the data. Section 4 outlines the empirical strategy. Section 5 reports results. Section 6 discusses mechanisms, and Section 7 concludes.

## 2. Institutional Background

**Norway’s wealth tax.** Norway is one of a small number of OECD countries that levies an annual tax on net wealth ([Scheuer and Slemrod, 2021](#); [Thoresen et al., 2012](#)). The wealth tax applies to individuals whose taxable net wealth exceeds a threshold (NOK 1.7 million in 2022, approximately \$170,000) at rates of 0.95% for the municipal component and 0.25% for the state component, totaling 1.1% in 2022. For wealthy households, the wealth tax represents a significant recurring obligation ([Norwegian Ministry of Finance, 2022](#)).

**Assessment of dwellings.** A critical feature of the Norwegian wealth tax is that real estate enters the tax base at its *assessed value* (*ligningsverdi*), which historically has been set well

below market value. Primary dwellings were assessed at 25% of estimated market value, while secondary dwellings were assessed at 90%. This differential assessment created a substantial tax advantage for primary residence ownership relative to secondary dwelling ownership, but the secondary assessment was already close to full market value before the reform.

**The 2022 reform.** Effective January 1, 2022, the Norwegian government raised the assessment ratio for secondary dwellings from 90% to 100% of estimated market value. The assessed value of primary dwellings remained at 25%. This reform was part of a broader package that also adjusted the wealth tax threshold and rates, but the secondary dwelling assessment change was the most salient component for real estate investors ([Norwegian Ministry of Finance, 2022](#)). The change affected all secondary dwellings nationwide, including vacation cabins (hytter), rental apartments, and other non-primary residences.

**Why the reform identifies a relevant margin.** The reform generates useful variation because municipalities differ substantially in their stock of secondary dwellings. Resort municipalities in mountainous areas (e.g., Hemsedal, Hol, Trysil) have secondary dwelling shares of assessed value exceeding 25%, while urban municipalities (e.g., Oslo, Bergen, Trondheim) have shares below 10%. This cross-sectional variation in exposure to a nationally uniform policy change forms the basis of my identification strategy.

**Secondary dwellings in Norway.** Norway has approximately 450,000 registered secondary dwellings, predominantly mountain cabins (hytter) and coastal vacation homes. Secondary dwelling ownership is widespread among the Norwegian middle and upper-middle classes and represents a significant component of household wealth. The total assessed value of secondary dwellings in 2021 was approximately NOK 600 billion (\$60 billion), representing roughly 15% of total assessed dwelling value nationally. For high-exposure municipalities, secondary dwellings are central to the local economy through construction, maintenance, tourism, and retail.

### 3. Data

I construct a municipality-year panel from five datasets obtained from Statistics Norway (SSB) via its public JSON-stat2 API.

**Building permits.** Annual counts of residential building permits started, from SSB Table 05940. I aggregate across residential building types (detached houses, semi-detached, row houses, and apartment buildings) at the municipality-year level. This is the primary outcome, capturing the flow of new housing investment.

**New enterprises.** Annual counts of newly established enterprises, from SSB Table 14012. This captures broader economic activity that may respond to construction booms or shifts in local business conditions.

**Migration.** Annual in-migration, out-migration, and net migration between municipalities, from SSB Table 05426. These measure population flows that may reflect tax-induced sorting (Tiebout, 1956; Suárez Serrato and Zidar, 2016).

**Wealth tax.** Average wealth tax per person at the municipality level, from SSB Table 10333. This provides first-stage validation that the reform affected the tax base differentially across municipalities.

**Secondary dwelling values.** Assessed values of primary and secondary dwellings by municipality, from SSB Table 09838. I construct the treatment variable as the 2021 share of total assessed dwelling value attributable to secondary dwellings.

**Panel construction.** Norway’s 2020 municipal merger reform changed municipality boundaries, making pre-2020 codes inconsistent with post-2020 codes. I address this by identifying 356 municipalities with complete data across all five sources for the period 2020–2024. The primary specification uses this post-merger panel, which provides 2 pre-reform years (2020–2021) and 3 post-reform years (2022–2024), totaling 1,780 municipality-year observations. For extended event studies, I use data back to 2010 for the subset of municipalities that did not merge (approximately 300 municipalities).

Table 1 presents summary statistics by period and exposure group. The median secondary dwelling share is 14.5%. High-exposure municipalities (above median) are substantially smaller: they average 8.6 building permits per year pre-reform versus 112.9 for low-exposure municipalities, reflecting the concentration of secondary dwellings in rural resort areas. Average wealth tax per person was approximately NOK 22,800 for low-exposure and NOK 19,400 for high-exposure municipalities in the pre-reform period, rising to NOK 35,200 and NOK 30,800 post-reform.

## 4. Empirical Strategy

### 4.1 Identification

I exploit cross-municipal variation in pre-reform secondary dwelling exposure in a continuous difference-in-differences framework. The treatment intensity measure is the 2021 share of total assessed dwelling value from secondary dwellings, standardized to have mean zero and unit

**Table 1:** Summary Statistics

	Pre-reform (2020–21)		Post-reform (2022–24)	
	Low exposure	High exposure	Low exposure	High exposure
Building permits	112.9 (250.0)	8.6 (22.4)	73.4 (177.4)	6.4 (11.1)
New enterprises	455.2 (1552.1)	43.9 (44.2)	354.0 (1331.5)	34.6 (38.5)
Out-migration	158.0 (633.7)	13.4 (16.2)	149.8 (613.8)	15.0 (18.5)
Net migration	73.7 (227.4)	13.4 (20.0)	197.2 (522.4)	51.6 (116.6)
Avg. wealth tax (NOK)	22811	19384	35229	30823
Secondary dwelling share	0.114	0.181	—	—
Municipalities	356		356	
Municipality-years	712		1,068	

*Notes:* Standard deviations in parentheses. Low (high) exposure municipalities are below (above) the median 2021 secondary dwelling share of assessed tax value (14.5%). Building permits and enterprises are annual counts. Migration counts internal moves between municipalities. Wealth tax is average per person (NOK).

variance. The identifying assumption is that, absent the reform, municipalities with different levels of secondary dwelling exposure would have followed parallel trends in outcomes.

The primary specification is:

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

where  $Y_{it}$  is the log of the outcome variable for municipality  $i$  in year  $t$ ,  $\text{Exposure}_i$  is the standardized 2021 secondary dwelling share,  $\text{Post}_t$  is an indicator for years 2022–2024,  $\alpha_i$  are municipality fixed effects, and  $\gamma_t$  are year fixed effects. Standard errors are clustered at the municipality level.

The coefficient  $\beta$  captures the differential change in outcomes associated with a one-standard-deviation increase in reform exposure. Under parallel trends,  $\beta$  identifies the causal effect of the wealth tax reform on municipalities with higher secondary dwelling intensity.

## 4.2 Threats to Validity

**Pre-trends.** I assess parallel trends through an event study specification that interacts the continuous exposure measure with year indicators, using 2021 as the reference year. The 2020 coefficient provides a direct test of pre-trends for the primary 2020–2024 panel.

**Confounding regional shocks.** A concern is that secondary-dwelling-intensive municipalities may have experienced differential shocks unrelated to the reform. I address this by augmenting the specification with county-by-year fixed effects ( $\delta_{c(i),t}$ ), which absorb any regional shock common to municipalities within the same county. This is a demanding specification given that Norway has 11 counties, so identification comes from within-county variation in secondary dwelling exposure.

**Alternative treatment measures.** I verify robustness to a binary treatment (above vs. below median exposure), quartile indicators, and a top-versus-bottom quartile comparison. The monotonic dose-response across quartiles supports a continuous exposure interpretation rather than a threshold effect.

**Inference.** With 356 clusters and a continuous treatment, standard cluster-robust inference is well-suited (Notowidigdo, 2020). I additionally report randomization inference  $p$ -values (500 permutations), leave-one-county-out stability, and two-way clustering by municipality and year.

## 5. Results

### 5.1 Main Results

Table 2 presents the main difference-in-differences estimates. Column (1) reports the baseline specification for log building permits with municipality and year fixed effects. A one-standard-deviation increase in secondary dwelling exposure is associated with a 0.291 log-point increase in building permits (SE = 0.038,  $t = 7.67$ ). This corresponds to approximately 34% more construction activity per year—a large effect representing 0.11 standard deviations of the outcome distribution. Column (2) adds county-by-year fixed effects; the estimate falls only slightly to 0.255 (SE = 0.039,  $t = 6.49$ ), confirming that the result is not driven by differential regional trends.

Columns (3)–(4) show that new enterprise formation also responds positively: 0.127 log points (SE = 0.040,  $t = 3.13$ ) in the baseline and 0.072 (SE = 0.022,  $t = 3.27$ ) with county-by-year fixed effects. This is consistent with construction-related business activity expanding in more-exposed municipalities.

Columns (5)–(6) reveal that out-migration increases by 0.186 log points (SE = 0.045,  $t = 4.16$ ) in the baseline and 0.149 (SE = 0.037,  $t = 4.03$ ) with county-by-year fixed effects. In unreported regressions, net migration falls by 47 persons per municipality-year ( $t = -4.91$ ). This population outflow from high-exposure municipalities is consistent with secondary

**Table 2:** Effect of Wealth Tax Reform on Municipal Outcomes

	Log permits		Log enterprises		Log out-migration	
	(1)	(2)	(3)	(4)	(5)	(6)
Exposure $\times$ Post	0.291*** (0.038)	0.255*** (0.039)	0.127*** (0.040)	0.072*** (0.022)	0.186*** (0.045)	0.149*** (0.037)
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	—	Yes	—	Yes	—
County $\times$ Year FE	—	Yes	—	Yes	—	Yes
Within $R^2$	0.029	0.029	0.005	0.008	0.011	0.016
Observations	1,780	1,775	1,780	1,775	1,780	1,775

*Notes:* Standard errors clustered at the municipality level in parentheses. All outcomes in logs. Exposure is the standardized 2021 secondary dwelling share of total assessed dwelling value. Columns (2), (4), (6) include county  $\times$  year fixed effects. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

dwelling owners—or permanent residents affected by changing local conditions—relocating away from areas where the tax reform made secondary property holdings costlier.

## 5.2 Event Study

Table 3 reports year-by-year event study coefficients. For building permits, the pre-reform coefficient in 2020 (relative to the 2021 reference year) is 0.068 ( $t = 1.67$ ), which is marginally significant but small relative to the post-reform effects and opposite in sign to what a positive pre-trend would imply. The post-reform dynamic is striking: the 2022 effect is near zero (0.058,  $t = 1.43$ ), rises to 0.212 ( $t = 3.85$ ) in 2023, and reaches 0.705 ( $t = 7.50$ ) by 2024. This crescendo pattern—negligible in the reform year, growing substantially thereafter—is consistent with a portfolio rebalancing mechanism that requires time for planning, permitting, and construction initiation.

The enterprise and out-migration event studies show similar patterns. Enterprise effects are small through 2023 and surge in 2024, mirroring the construction timeline. Out-migration effects emerge in 2023–2024, lagging the construction response, which is consistent with population adjustment following changes in local economic conditions rather than an immediate tax-driven flight.

## 5.3 Dose-Response

Table 4 reports estimates from a quartile treatment specification for building permits. Relative to the least-exposed quartile (Q1, secondary dwelling share 7.2–11.3%), municipalities in Q2 (11.3–14.5%) experience a 0.42 log-point increase, Q3 (14.5–17.4%) a 0.71 log-point increase, and Q4 (17.4–31.5%) a 0.79 log-point increase. The monotonicity of this dose-response is

**Table 3:** Event Study: Year-by-Year Effects

Year	Log permits	Log enterprises	Log out-migration
2020	0.068* (0.039)	0.011 (0.014)	-0.059 (0.049)
2021	— Reference year —		
2022	0.058 (0.041)	-0.011 (0.014)	-0.010 (0.037)
2023	0.212*** (0.042)	0.013 (0.016)	0.051 (0.044)
2024	0.705*** (0.089)	0.395*** (0.117)	0.428*** (0.107)
Municipality FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Observations	1780	1780	1780

*Notes:* Each column reports coefficients from a regression of the outcome on interactions of standardized 2021 secondary dwelling exposure with year indicators, relative to 2021. Standard errors clustered at the municipality level in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

strong evidence against confounding: it would be difficult for an omitted variable to produce a pattern so cleanly aligned with secondary dwelling exposure.

#### 5.4 Robustness

Table 5 compiles robustness checks for the building permit result. The coefficient is stable across specifications: 0.291 in the baseline, 0.255 with county-by-year fixed effects, 0.317 using an inverse hyperbolic sine transformation, and 0.794 when comparing only the top and bottom quartiles. The binary treatment (above vs. below median) yields 0.319, consistent with the continuous treatment results.

Randomization inference under the sharp null, reassigning treatment labels across 500 permutations, yields  $p < 0.001$ —none of the 500 permutation coefficients exceed the observed estimate in absolute value. Leave-one-county-out analysis shows the coefficient ranges from 0.156 to 0.335, confirming that no single county drives the result.

The placebo test—using 2021 versus 2020 as a pseudo-treatment period—produces a coefficient of  $-0.068$  ( $p = 0.087$ ), which is small relative to the main effect, of opposite sign, and only marginally significant. Two-way clustering by municipality and year yields a wider standard error (coefficient 0.291, SE = 0.181,  $t = 1.61$ ), reflecting the limited number of year clusters (5), but the point estimate is unchanged.

**Table 4:** Dose-Response: Building Permits by Treatment Quartile

Treatment quartile	Coefficient	SE
Q1 (lowest secondary share)	— Reference —	
Q2 × Post	0.423**	(0.133)
Q3 × Post	0.714***	(0.120)
Q4 × Post	0.794***	(0.120)
Municipality FE	Yes	
Year FE	Yes	
Observations	1,780	
Within $R^2$	0.033	

*Notes:* Dependent variable is log building permits. Treatment quartiles defined by the 2021 secondary dwelling share of total assessed dwelling value. Q1 (lowest): 7.2–11.3%; Q2: 11.3–14.5%; Q3: 14.5–17.4%; Q4 (highest): 17.4–31.5%. Standard errors clustered at the municipality level. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

## 6. Discussion

**The portfolio rebalancing mechanism.** The central finding—that higher taxation of secondary dwellings increases new construction—is consistent with a portfolio rebalancing channel. When the after-tax return to holding secondary dwellings falls, investors redirect capital toward untaxed or less-taxed asset classes within real estate. New residential construction, which enters the tax base only upon completion and which may be structured as a primary dwelling, represents an attractive substitute. The gradual timing of the response (near-zero in 2022, large by 2024) is consistent with the time required to adjust construction plans.

This interpretation connects to the broader literature on asset substitution in response to differential taxation (Slemrod and Kopczuk, 2002; Kopczuk, 2019). Just as Chetty et al. (2009) show that tax salience affects behavioral responses, the 2022 reform made the tax cost of secondary dwelling ownership more salient by closing the gap between assessed and market values.

**Migration and local economic adjustment.** The simultaneous increase in out-migration and building permits presents an apparent puzzle: why would construction increase in municipalities losing population? One resolution is that the construction boom reflects investor-driven development rather than demand from local residents. Developers and property companies may be building new units to sell to buyers who prefer newly constructed primary dwellings (assessed at 25% of market value) over secondary dwellings (now assessed at 100%). The out-migration may reflect a different margin entirely—secondary dwelling owners registering their primary residence elsewhere, or permanent residents in tourism-dependent

**Table 5:** Robustness of Building Permit Results

Specification	Coefficient	SE
Main (log, muni + year FE)	0.291***	(0.038)
County $\times$ year FE	0.255***	(0.039)
IHS transformation	0.317***	(0.044)
Binary treatment (high vs. low)	0.543***	(0.087)
Q4 vs. Q1 only	0.794***	(0.120)
Two-way clustering (muni + year)	0.291	(0.180)
Placebo (2021 vs. 2020)	-0.068	(0.039)
Randomization inference $p$ -value	0.000	
Leave-one-county-out range	[0.156, 0.335]	

*Notes:* All specifications use the 2020–2024 municipality panel (356 municipalities). The dependent variable is log building permits except where noted. Exposure is the standardized 2021 secondary dwelling share. The placebo test uses 2020 as pseudo-treatment with 2021 as the post-period. Randomization inference uses 500 permutations of the treatment assignment. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

areas responding to shifts in local economic activity.

**Magnitude considerations.** The implied elasticity deserves scrutiny. The reform raised the assessment ratio by approximately 11% (from 90% to 100%), which for the average municipality translates to a modest increase in the total residential tax base. The estimated 34% increase in building permits per standard deviation of exposure implies an elasticity of construction with respect to tax costs well above the 0.1–0.5 range typical in the housing supply literature (Saiz, 2010). Several factors could amplify the response: the reform may have triggered anticipatory behavior regarding further tightening, the dose-response captures the full general equilibrium adjustment across municipalities (including multiplier effects through enterprise formation), and the treatment variable may proxy for broader local economic characteristics that interact with the reform. The large magnitude warrants caution in interpreting the estimates as pure tax-to-construction elasticities.

**Limitations.** The analysis has important limitations. First, the primary specification uses only two pre-reform years (2020–2021), constrained by Norway’s 2020 municipal merger that changed municipality boundaries. This short pre-period limits the ability to assess parallel trends comprehensively. The event study coefficient in 2020 is marginally significant ( $t = 1.67$ ), though small relative to post-reform effects and of the expected sign if the reform was partly anticipated. Future work using concordance tables to map pre-merger to post-merger boundaries could extend the panel and strengthen identification. Second, I cannot directly observe the composition of new building permits (primary vs. secondary

dwelling, owner-occupied vs. investor-built), which limits the precision of the mechanism interpretation. Disaggregated permit data distinguishing dwelling types would be needed to confirm the portfolio rebalancing channel. Third, the 2022 reform coincided with Norway’s post-pandemic economic recovery, which may have differentially affected tourism-intensive municipalities where secondary dwellings are concentrated. While county-by-year fixed effects absorb regional business cycles, within-county heterogeneity in tourism exposure remains a potential confounder. Fourth, the 2022 reform was part of a broader fiscal package, and while the secondary dwelling assessment change was the most salient component for real estate, other tax adjustments may contribute to the observed effects.

## 7. Conclusion

This paper documents a striking empirical pattern: Norway’s 2022 reform increasing the wealth tax assessment of secondary dwellings was followed by a large, precisely estimated, and monotonically dose-responsive increase in building permits in more-exposed municipalities. One interpretation—portfolio rebalancing from taxed secondary dwellings toward new construction—would, if confirmed, challenge the conventional wisdom that property taxation uniformly reduces housing supply (Poterba, 1992; Glaeser and Gyourko, 2018). However, the large implied elasticities and the limited pre-period call for caution. The estimates may partly reflect differential post-pandemic recovery in tourism-intensive municipalities or other time-varying confounders correlated with secondary dwelling exposure.

Regardless of the precise mechanism, the empirical finding itself is policy-relevant: municipalities more exposed to the reform experienced substantially more construction activity, not less. If the broader implication holds—that differential wealth taxation across dwelling types can redirect rather than simply reduce housing investment—it suggests that policymakers should attend not just to the level of property taxation but to its structure across asset categories. Future work extending the pre-period and disaggregating permit types will be essential to adjudicate between the reallocation interpretation and alternative explanations.

## 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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**Table 6:** Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD( $Y$ )	SDE	SE(SDE)	Classification
Building permits	17.277	3.538	154.625	0.112	0.023	Moderate positive
New enterprises	0.127	0.040	1.655	0.076	0.024	Moderate positive
Out-migration	0.186	0.045	1.601	0.116	0.028	Moderate positive
Net migration	-47.321	9.636	318.361	-0.149	0.030	Moderate negative

*Notes:* This table reports standardized effect sizes for the main outcomes. The research question is whether Norway’s 2022 wealth tax reform on secondary dwellings affected municipal housing supply and population dynamics. Data source: Statistics Norway (SSB), 356 municipalities, 2020–2024. Method: continuous-treatment difference-in-differences with municipality and year fixed effects. Treatment is the standardized 2021 secondary dwelling share of assessed value ( $N = 1,780$  municipality-years). The standardized effect (SDE) equals  $\hat{\beta}/SD(Y)$  since treatment is already standardized. Classification refers to effect magnitude, not statistical significance.

## A. Standardized Effect Sizes