Do Place-Based Tax Incentives Expand Housing Supply? Evidence from France

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Abstract

This paper estimates the causal impact of the French Pinel program—a geographically targeted demand-side tax incentive—on local housing market dynamics. Exploiting the spatial discontinuity in program eligibility and the staggered reclassification of municipalities, we implement a spatial difference-in-differences event-study design using high-resolution administrative data on real-estate transactions and building permits. The results show no statistically significant increase in new housing construction within treated areas. Instead, the policy induced a geographic reallocation of investment from non-eligible municipalities toward nearby eligible zones, together with a persistent shift in the composition of new housing toward smaller units. We also document a lasting rise in vacant-unit sales after the program's expiration, suggesting speculative behavior and limited rental absorption in affected markets. Overall, the findings indicate that spatially targeted tax incentives can reshape the geographic and typological structure of housing investment without expanding aggregate supply. These results highlight the limits of demand-side subsidies in markets with low supply elasticity and the need to design policies that better integrate fiscal incentives with land-use constraints and local regulatory conditions.

Keywords: Tax incentives; Housing supply; Spatial difference-in-differences; Place-based policy; France.

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1 Introduction

Housing affordability has become a central challenge in urban markets, driven by rising demand, regulatory constraints, and limited supply responsiveness. In response, governments increasingly deploy fiscal incentives to stimulate private rental housing investment. Yet the effectiveness of these demand-side subsidies in expanding net housing supply—rather than merely reshaping investment patterns or inflating prices—remains an open question with important implications for housing policy design.

A growing body of research highlights that geographically targeted interventions can reshape local economic outcomes without necessarily expanding aggregate activity. Recent studies emphasize that the effectiveness of place-based programs hinges on local absorption capacity, institutional constraints, and spatial spillovers (Caragliu and Landoni (2024); López-Villuendas and del Campo (2024)). These insights motivate our examination of the French Pinel program, a geographically targeted tax incentive aimed at stimulating new rental housing investment in intermediate and peri-urban markets.

Since 1984, France has introduced a series of tax-based programs to promote private rental housing, particularly for low- and middle-income households. These initiatives have grown in both scale and fiscal cost, with annual expenditures reaching nearly €2 billion by 2018 (Deniau et al. (2019)). Despite this sustained effort, there is little consensus on whether these incentives generate lasting supply-side benefits or instead exacerbate market distortions through land price inflation, investment misalignment, or spatial displacement.

This paper evaluates the impact of the Pinel tax incentive—France's most recent geographically targeted housing subsidy—on urban housing market outcomes. Introduced in 2014 and extended through 2024, the Pinel program offers income tax reductions to landlords who invest in newly built rental units located in designated eligible municipalities. Exploiting spatial discontinuities in program eligibility, we implement a spatial difference-in-differences (DiD) event-study design to estimate its causal effects on new construction, housing typology, price dynamics, and investment spillovers.

This paper makes three main contributions to the urban economics literature. First, we provide one of the first systematic, long-term evaluations of a geographically targeted tax incentive, covering both active policy years and post-policy adjustments. Second, we document how such incentives reshape the composition of new housing supply—specifically unit size and density—highlighting their role in reinforcing market segmentation. Third, we analyze spatial externalities by testing for investment reallocation across adjacent municipalities, thereby distinguishing redistribution from genuine supply expansion.

Our identification strategy compares outcomes in municipalities newly eligible for the Pinel program to adjacent municipalities that remained ineligible, leveraging the spatial boundary of eligibility. To strengthen causal inference, we limit the sample to a 0–5 km radius around the eligibility boundary and exclude a 1 km buffer to mitigate displacement effects from immediate border adjustments. This design ensures that treated and control municipalities are similar in pre-policy characteristics and subject to common macroeconomic trends.

The empirical results yield four main findings. First, the Pinel tax incentive did not generate a statistically significant increase in new housing construction across treated areas. Estimated effects on the number of authorized units are small and insignificant at all distances, with a modest decline observed within the 1 km zone. When this constrained boundary area is excluded, effects attenuate further, confirming the absence of any net expansion in housing supply.

Second, the policy significantly altered the composition of new housing by reducing the share of larger units. The proportions of three-room and four-or-more-room dwellings decline notably and persistently from Year 2 onward, particularly within 1 km of the eligibility boundary. These effects attenuate after excluding the immediate boundary zone. This typological shift appears to result from a combination of displacement effects and strategic developer behavior: in areas where regulatory or geographic constraints limited large-scale development, developers responded by reallocating projects toward eligible but less constrained zones and by favoring smaller, incentive-compatible units with higher per-square-meter rental yields.

Third, we document a sharp and sustained increase in vacant housing transactions near the boundary, beginning in Year 0 and continuing well after the policy's expiration. These effects are strongest within the 1–2 km zones but persist even when the 1 km area is excluded, indicating that speculative acquisitions and weak rental absorption contributed to a post-policy surge in resale activity.

Fourth, the policy induced transitory price increases in the new housing segment, concentrated within 1–2 km of the eligibility cutoff. New housing prices peaked between Years 2 and 3, consistent with heightened investor activity, and subsequently declined as the policy was phased out. Meanwhile, prices in the old housing segment began to fall significantly from Year 5 onward, particularly in the 3–5 km bands. This post-policy depreciation persists even after excluding the 1 km zone and is consistent with an overhang of investor-owned stock reentering the resale market.

Taken together, the results highlight the limits of demand-side tax incentives in structurally weaker markets. Rather than inducing a sustained expansion of housing supply, the Pinel program primarily reshaped the geography and typology of new construction. In the absence of complementary supply-side enablers or strong rental demand, such incentives may generate spatial distortions, compositional adjustments, and post-policy corrections—without alleviating structural housing shortages.

The remainder of this paper is organized as follows. Section 2 reviews the literature and provides institutional context on the Pinel program. Section 4 describes the data and key variables. Section 5 outlines the empirical strategy. Section 6 presents the main results. Section 7 concludes with policy implications.

2 Related Literature and Policy Overview

2.1 Related Literature

This paper contributes to a broad literature on the effectiveness of tax-based and geographically targeted incentives in shaping housing market outcomes. A key policy question is whether such instruments expand aggregate housing supply or primarily induce spatial and compositional reallocation.

In the United States, most evidence focuses on the Low-Income Housing Tax Credit (LIHTC), the principal federal mechanism for subsidizing rental housing. Early work by Sinai and Waldfogel (2005) and Eriksen and Rosenthal (2010) finds limited supply effects due to crowding out of unsubsidized development. More recent research emphasizes spatial sorting and regulatory frictions: McClure (2019) shows that LIHTC projects concentrate in lower-cost or less-regulated areas, reinforcing segregation rather than expanding access in high-demand markets. Similarly, Betz-Hamilton (2024) demonstrate that local land-use constraints significantly condition the program's capacity to stimulate new construction.

Institutional context further mediates program efficacy. Sportiche et al. (2024) evaluate Massachusetts's Chapter 40B and find that while it mitigates zoning constraints and improves locational access, net supply effects remain limited in highly regulated jurisdictions. Schirmer (2024) argues that broader coalitional politics and financialization trends shape the redistributive effects of housing subsidies, underscoring the importance of governance and institutional alignment.

Beyond the U.S., European studies show heterogeneous impacts of fiscal and regional development programs. Recent work has documented how cohesion and innovation subsidies often generate uneven spatial outcomes or reinforce pre-existing disparities (Psycharis et al. (2024); López-Villuendas and del Campo (2024)). These findings align with concerns that geographically targeted incentives may generate displacement rather than net externalities.

In the French context, the state has long relied on tax-based rental incentives, yet robust causal evidence remains sparse due to overlapping program periods and limited access to administrative data. An early contribution by Rigaud et al. (2008) finds localized construction increases under the Robien law, while Chapelle et al. (2018) show that its withdrawal led to price deflation without measurable supply contraction. Subsequent studies highlight unintended effects: Bono and Trannoy (2019) find capitalization into land values, and Chareyron et al. (2021) document diverging price dynamics between new and existing housing markets, suggesting compositional rather than aggregate effects. More recent work by Daly (2024) shows that renovation-targeted tax credits induce only short-lived and geographically concentrated transaction increases.

The design of geographically targeted programs raises concerns about spillovers and investment displacement. In parallel policy domains, Xu et al. (2024) show that China's carbon-trading pilot induced green innovation in treated cities but reduced it in nearby untreated ones. Similarly, Caragliu and Landoni (2024) demonstrate that spatial heterogeneity in urban density and pre-existing investment levels mediates the effectiveness of regional cooperation incentives in Italy. These studies underscore the need to account for cross-border effects and local absorptive capacity in evaluating spatially selective fiscal policies.

This paper advances the literature along three dimensions. First, it offers a long-run evaluation of a major demand-side housing incentive, capturing both policy-active and post-expiry dynamics. Second, it applies a spatial difference-in-differences framework that isolates reallocation effects across municipal boundaries. Third, it links changes in unit typology to incentive design, highlighting how fiscal instruments affect the composition—not just the volume—of new housing supply. Together, these contributions inform ongoing debates about the limits of place-based subsidies in markets constrained by regulatory and geographic frictions.

3 Policy Overview

Since the 1980s, France has relied extensively on tax-based incentives to stimulate private rental housing construction. Eight successive programs—ranging from the Méhaignerie law (1984) to the Pinel *ancien* (2019)—have offered income tax reductions to investors in new or rehabilitated dwellings.[1] Like the U.S. Low-Income Housing Tax Credit (LIHTC), these schemes rely on tax expenditures to incentivize the private provision of below-market rental units.

The Pinel program, introduced in 2014 to replace the more restrictive Duflot scheme, sought to revive investor participation by relaxing several constraints. It offered income tax reductions to landlords who rented newly built units at capped rents for six, nine, or twelve years. Compared with its predecessor, the Pinel law introduced (i) greater contractual flexibility in rental duration, (ii) authorization for intra-family rentals, and (iii) expanded spatial eligibility—all intended to improve take-up and liquidity in the rental investment market.

[1] The major programs include Méhaignerie (1984–1997), Périssol (1996–1999), Besson (1999–2002), Robien (2003–2006), Borloo (2006–2009), Scellier (2009–2012), Duflot (2013–2014), and Pinel (2014–2024).

Eligibility rested on the ABC zoning system[2], which classifies municipalities into Zones A bis, A, B1, B2, and C according to housing market pressure. Initially, only Zones A bis through B1 were eligible. In 2013, however, municipalities in Zone C could request reclassification into Zone B2, thereby gaining eligibility. Nearly 900 municipalities obtained this status by 2014, creating a discrete spatial boundary between newly eligible and persistently ineligible areas.

Subsequent reforms further tightened targeting. In 2018, eligibility was restricted to Zones A bis, A, and B1, effectively phasing out Zone B2. Transitional provisions allowed B2 projects to qualify until March 2019 if permits were filed before the end of 2017. These revisions aimed to contain fiscal costs and refocus subsidies on high-demand markets.

Overall, the Pinel program illustrates France's broader shift toward geographically targeted demand-side housing incentives. Despite a fiscal cost approaching €2 billion annually, its capacity to expand effective supply remains uncertain. The staggered reclassification of municipalities into—and later out of—eligibility creates a quasi-experimental setting to evaluate whether such tax incentives genuinely stimulate new construction or merely reallocate investment and distort spatial housing patterns.

4 Data

The analysis draws on multiple high-resolution administrative datasets covering real estate transactions, construction activity, and spatial characteristics for metropolitan France between 2010 and 2022. The sample excludes the departments of Alsace-Moselle and Mayotte due to differences in cadastral and fiscal administration. All variables are harmonized at the municipality and transaction levels to enable consistent panel construction and spatial matching.

Real Estate Transactions – The primary source of transaction data is the *Demande de Valeur Foncière Version 3F* (DV3F), compiled by the French General Directorate of Public Finance (DGFiP) and Cerema. DV3F is a confidential administrative dataset that records all notarized residential property transactions in mainland France. Compared to the publicly available DVF dataset, DV3F includes harmonized property characteristics, enriched metadata, and superior geolocation accuracy.

Each transaction record reports the sale price, lot surface, construction year, property type, and precise geographic coordinates, along with cadastral references that allow for spatial linkage to zoning and eligibility regimes. The dataset distinguishes between new and existing housing units, enabling disaggregated analysis of the differential effects of tax incentives on various housing market segments.

The sample is restricted to residential sales, excluding commercial properties, undevel-

[2]Defined in Article D304–1 of the French Construction and Housing Code.

oped land, and atypical transfers (e.g., sales below €20,000 or intra-family transactions). These filters ensure that the estimation sample reflects market-based transactions relevant to the housing supply response.

Building Permits –To evaluate supply-side responses to the Pinel tax incentive, we use annual municipality-level building permit data from the *SITADEL2* database, maintained by the French Ministry for Ecological Transition. This administrative dataset provides exhaustive coverage of construction permits issued throughout France and records detailed project characteristics, including the number of units, project type (e.g., new construction vs. renovation), structural form (e.g., single-family vs. collective housing), and unit size.

The SITADEL2 data are linked to the policy treatment via spatial assignment of municipalities to eligibility zones. Specifically, permit observations are matched to Pinel fiscal zones to trace how changes in eligibility status affect local construction activity over time. Aggregation is conducted at the municipality-year level to ensure consistency with treatment timing and zoning reclassifications across the sample period.

Geolocation and Spatial Assignment– The identification strategy exploits spatial discontinuities in program eligibility, which requires precise geographic matching of properties and municipalities to treatment boundaries. All property transactions are geocoded using cadastral references and GPS coordinates provided in the DV3F dataset. For building permits, which report only address-level information, we use official administrative geocoding services from *data.gouv.fr* to convert permit addresses into geographic coordinates[3] .

Using this geospatial information, we assign each observation to distance bands relative to the nearest Pinel eligibility boundary (e.g., 0–1 km, 1–2 km, 2–5 km). Official boundary shapefiles from the Institut Géographique National (IGN) and the Directorate for Housing, Urban Planning, and Landscape (DGALN) define both municipal borders and fiscal eligibility zones (A bis, A, B1, B2). Observations within one kilometer of the policy boundary are excluded in certain specifications to mitigate the influence of spillovers and cross-border displacement effects. This spatial assignment procedure underpins the construction of treatment and control groups in the spatial differences-in-differences design.

Vacant Units – The DV3F dataset also records whether a transaction involves a property previously identified as vacant, based on administrative tax data. This information allows for the construction of municipal-year aggregates of transactions involving underutilized housing stock. In years with no vacant property sales, a zero is recorded to preserve panel balance. These data are used to assess whether the tax incentive mobilized

[3]Permit addresses are geocoded using the official open data platform of the French government (`https://www.data.gouv.fr`), which provides APIs and datasets for converting administrative addresses to geographic coordinates.

idle housing resources in addition to stimulating new construction.

5 Empirical Framework

To assess the causal impact of geographically targeted tax incentives on housing outcomes, we construct a counterfactual using municipalities just outside the policy's eligibility boundary. Our empirical strategy leverages both the spatial discontinuity in Pinel program eligibility and variation in treatment timing resulting from municipality-level reclassification. This allows us to isolate treatment effects while controlling for time-varying confounders and unobserved heterogeneity.

The identification strategy relies on two key sources of variation. First, we exploit the reclassification of municipalities under the ABC zoning scheme, which governs housing policy eligibility in France. In particular, we compare municipalities that were initially excluded (Zone C) but later became eligible for the Pinel program (via reclassification to Zone B2) to those that remained ineligible throughout the study period.[4][5] This provides quasi-experimental variation in exposure to the policy, conditional on observable and unobservable characteristics common to Zone C municipalities.

Second, we implement a spatial differences-in-differences (DiD) framework that compares treated municipalities with geographically adjacent untreated municipalities. This spatial design follows the boundary discontinuity approach developed by Overman and Einio (2012) and extended in housing contexts by Chapelle et al. (2018). Because treated and control municipalities are selected from a narrow geographic band and share similar initial zoning, this approach reduces the likelihood of systematic differences in pre-treatment trends. Spatial proximity further mitigates concerns about differential exposure to macroeconomic conditions or national housing market shocks, supporting the plausibility of the parallel trends assumption.

To estimate dynamic treatment effects, we adopt an event-study specification. Let $Y_{i,t}$ denote the housing market outcome of interest in municipality i and year t, such as the number of new housing permits, housing transactions, or the share of specific unit types. We estimate the following model:

$$Y_{i,t} = \delta_t + \theta_i + \phi_{b(i)} + \sum_{\substack{y=-n \\ y\neq -1}}^{m} \gamma_y \cdot \text{Treatment}_i \cdot \mathbb{I}(t - t_i^* = y) + \beta X_{i,t} + \varepsilon_{i,t} \tag{1}$$

[4]Treated and untreated municipalities were initially classified within the same category (Zone C), according to national criteria of housing supply–demand imbalances defined in Article D304-1 of the French Construction and Housing Code, and thus exhibited comparable housing market fundamentals.

[5]Under the national ABC zoning scheme, municipalities are classified based on supply–demand imbalances (Article D304-1, Construction and Housing Code). Several Zone C municipalities were reclassified into Zone B2 by the *Arrêté du 1er août 2014* (*Journal officiel*, 6 août 2014), later amended by the *Arrêté du 30 septembre 2014*. Zone C municipalities could apply for reclassification to benefit from the program.

In this specification, δ_t denotes year fixed effects that control for national shocks and time trends common to all municipalities. The term θ_i captures municipality fixed effects, accounting for time-invariant unobserved heterogeneity at the municipality level, such as baseline housing demand, regulatory constraints, or geographic characteristics. We also include boundary fixed effects, denoted $\phi_{b(i)}$, which control for persistent unobservable features shared across municipalities located on the same side of a given eligibility threshold. The indicator variable Treatment_i equals one if municipality i ever becomes eligible for the Pinel tax incentive, while $\mathbb{I}(t - t_i^* = y)$ is an event-time dummy equal to one if year t is y years from the first year of eligibility t_i^*. We omit the year immediately prior to treatment, $y = -1$, which serves as the reference period. The coefficients γ_y trace out the dynamic treatment effects for each relative year. The vector $X_{i,t}$ includes time-varying controls for local economic conditions or spatial characteristics, such as proximity to urban centers or transport infrastructure. Standard errors are clustered at the municipality level to account for heteroskedasticity and serial correlation in the residuals.

Under the identifying assumption of parallel trends, we expect the estimated coefficients γ_y to be close to zero and statistically insignificant for all pre-treatment years $y < 0$. A statistically significant divergence post-treatment would then indicate the causal impact of the policy. We use ordinary least squares to estimate Equation 1 and report robust standard errors clustered at the municipality level.

Treatment timing is based on the date at which a municipality first became eligible for the Pinel tax credit. For municipalities benefiting exclusively from tax incentives for new housing, we define t_i^* as January 1, 2015, the official introduction date of the program.[6] The outcome variables are drawn from three administrative data sources. Real estate transaction data are from the DV3F database, which provides property-level information on sales, prices, and geolocation. Building permit records are sourced from SITADEL2, a national database covering permit issuance by municipality and year. Vacancy and distressed property transactions are captured using the LOVAC database, which reports vacant unit sales based on administrative tax records.[7]

A key empirical concern in spatial policy evaluation is the potential violation of the Stable Unit Treatment Value Assumption (SUTVA), particularly through spillover or displacement effects. The implementation of the Pinel program may attract investment away from nearby untreated municipalities, especially those in close proximity to eligible areas. To address this concern, we exclude municipalities located within one kilometer of the treatment boundary. This exclusion follows the strategy of Kline and Moretti (2014),

[6] The reclassification was later amended by the *Arrêté du 30 septembre 2014*, but new construction projects typically take time to be approved and completed. In France, the average time between building permit approval and completion of a new dwelling ranged from 13 to 17 months between 1990 and 2010 (Boutier (2012)). See Section 3 for additional details on program eligibility and rollout.

[7] The LOVAC dataset is compiled as part of the national vacant housing initiative and is accessible to local governments and housing agencies such as ANAH.

which recommends removing zones of high spatial integration to reduce contamination across treatment groups. As shown in Panel A in Table 2, real estate characteristics such as average prices and transaction volumes are highly similar within this narrow band, increasing the risk of cross-border interactions. Excluding the 1-kilometer zone enhances the credibility of our causal estimates by minimizing potential spillovers.

Consistent with displacement dynamics, we find that treatment effects are strongest in municipalities immediately adjacent to the eligibility boundary and fade once the 1-kilometer ring is excluded. The main results remain robust to this exclusion and to alternative buffer widths ranging from 2 to 5 kilometers, confirming that our estimates are not driven by local spillovers or boundary contamination. Taken together, these findings underscore the importance of explicitly accounting for spatial dependence and interference when implementing spatial difference-in-differences designs.

6 Results

6.1 New Housing Units Constructed

We begin by estimating the impact of the Pinel tax incentive on new housing construction using a spatial difference-in-differences event-study design. Figure 1 displays the dynamic treatment effects by distance from the eligibility boundary, with coefficients measuring yearly deviations in the number of newly authorized units relative to the pre-policy baseline.

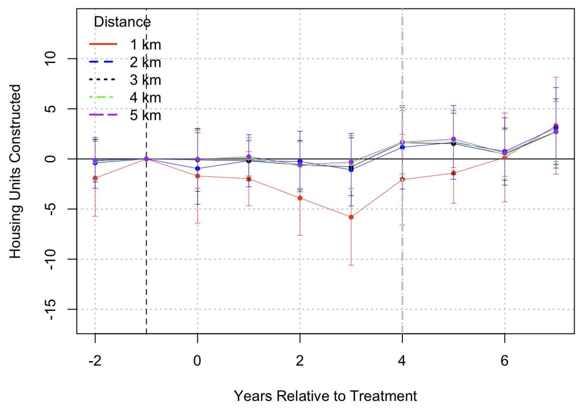


Figure 1: Dynamic Effects of Tax Incentives on New Housing Units Constructed, by Distance from Policy Boundary

As shown in Table 3, the estimated treatment effects are consistently close to zero and statistically insignificant across most distance bands and years, with the exception of a short-term decline at 1 km. This pattern reinforces the interpretation that the policy failed to generate a measurable increase in new construction.

The pre-treatment period shows no significant divergence in construction trends between treated and control municipalities, supporting the parallel trends assumption.

Post-policy estimates reveal that the Pinel tax incentive did not induce a statistically significant increase in new housing starts. Across all distance bands beyond 1 km, treatment effects remain small and insignificant throughout the policy period. The sole exception is the 1 km zone, where a temporary decline in construction is observed. This counterintuitive effect—given that the policy's objective was to stimulate building activity—may reflect short-term investment displacement, an issue explored further in the next subsection.

Between Years 0 and 3, the muted response suggests limited elasticity of supply in the face of the tax incentive. A moderate rebound is visible only after the policy's phase-out in Year 4, with some increase in construction observed from Year 5 onward, particularly beyond the 2 km threshold. However, these post-policy dynamics likely reflect broader macroeconomic or market-driven adjustments, rather than direct policy effects.

The spatial variation in treatment effects underscores the influence of local market conditions and supply-side frictions on policy responsiveness. The absence of any sustained positive effect—despite geographic targeting—raises questions about the ability of demand-side incentives to foster net new development.

Overall, the findings suggest that the Pinel program reallocated investment geographically without meaningfully expanding the housing supply. These results align with the broader literature questioning the effectiveness of fiscal subsidies in addressing structural housing shortages when underlying market fundamentals are weak or unresponsive.

6.2 Changes in Housing Unit Typology

We next examine whether the Pinel tax incentive affected the composition of new housing supply by altering the distribution of unit sizes. Figure 2 plots dynamic treatment effects on the share of newly authorized 1-, 2-, 3-, and 4-or-more-room units, across municipalities located 1 to 5 km from the eligibility boundary. Full regression estimates are reported in Tables 4–7.

Prior to the policy, the share of each unit type remained stable across all distance bands, with no evidence of divergent pre-trends. This supports the validity of the parallel trends assumption and reinforces the credibility of the event-study design.

Following policy implementation in Year 0, we observe a sharp and persistent decline in the share of larger units—specifically 3-room and 4-or-more-room dwellings. These effects become statistically significant from Year 2 onward and reach a peak between Years 2 and 3, with reductions ranging from 10 to 15 percentage points depending on the distance band. While the decline is consistently negative across the 2 to 5 km zones, it is even more pronounced within the 1 km band, where the estimated reductions are larger in magnitude and statistically significant not only for 3- and 4-room units, but also for 2-room dwellings. This pattern suggests that the immediate boundary area experienced the strongest typological contraction, affecting all but the smallest unit size.

In contrast, there is no clear upward adjustment in the share of smaller units during the policy window. Estimates for these categories remain small, statistically insignificant, and generally flat over time. The absence of compensatory increases in smaller unit shares implies that the shift in composition reflects a net contraction in average unit size, rather than a redistribution across categories.

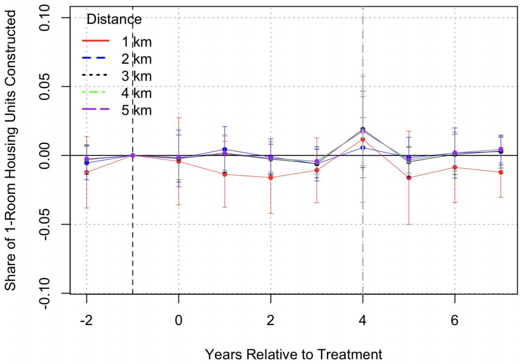
The typological effects of the policy persist well beyond its phase-out in Year 4. Between Years 5 and 7, the share of 4-or-more-room units remains significantly below pre-policy levels in nearly all distance bands, suggesting that the incentive may have produced a lasting alteration in the structure of new housing supply.

These patterns point to a strategic response by developers to the incentive design. By emphasizing rental profitability within capped rent ceilings, the Pinel scheme may have implicitly favored smaller units with higher per-square-meter yields. Developers thus appear to have adjusted not by increasing density, but by scaling down unit size within projects to maximize returns under program constraints.

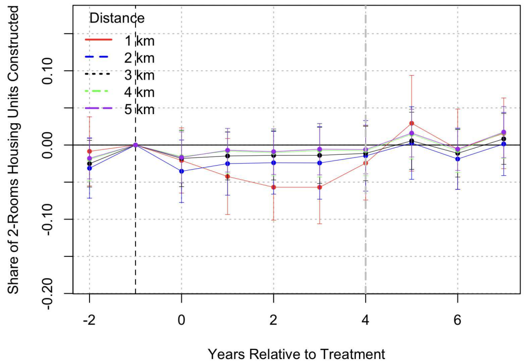
Taken together, these results provide compelling evidence that the Pinel tax incentive significantly altered the typology of new housing units, even in the absence of aggregate increases in construction volume. This mechanism—through compositional rather than quantitative adjustment—constitutes a key channel through which place-based tax subsidies shape urban development patterns.

This typological shift is particularly striking given the underlying demographic structure of treated municipalities. As shown in Table 1, Pinel-eligible areas have significantly larger populations, higher household counts, and more residents in family-forming age groups (30–59) than control areas. These characteristics are typically associated with higher demand for larger housing units. Yet, the policy appears to have induced a compositional shift in the opposite direction—reducing the share of 3-room and 4-or-more-room dwellings—despite this demographic profile. This suggests that the typological adjustment was not driven by local housing needs, but rather by financial optimization within the program's rent ceiling and profitability constraints. The divergence between household composition and the typology of new supply raises important questions about the alignment between fiscal incentives and actual spatial housing demand.

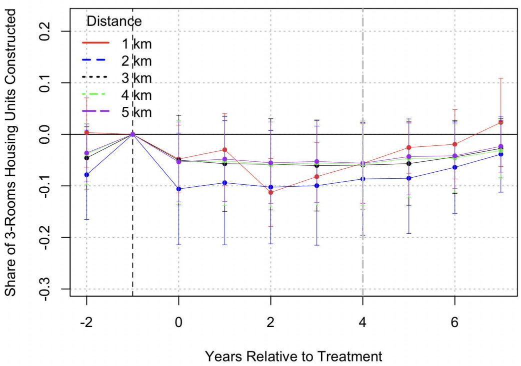
From a policy perspective, this typological shift raises important distributional concerns. While the expansion of smaller units may improve access for single-person households or low-income renters, the sustained decline in larger units risks exacerbating supply shortages for families or multigenerational households. These findings underscore the need to align fiscal incentives not only with overall supply goals, but also with broader objectives of housing diversity and demographic adequacy.



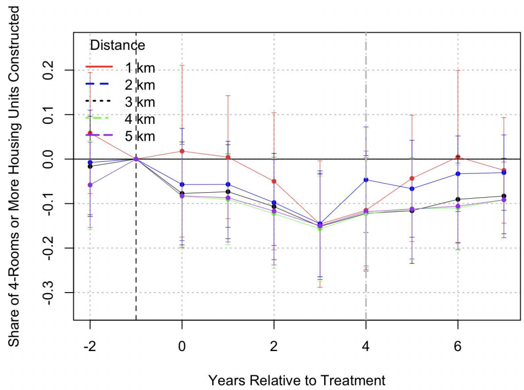
(a) Share 1-Room in New Units



(b) Share 2-Rooms in New Units



(c) Share 3-Rooms in New Units



(d) Share 4-Rooms or More in New Units

Figure 2: Distribution of Room Shares in New Units

6.2.1 Displacement Effects

The typological shifts documented in the previous section—most notably the decline in the share of larger housing units—may not only reflect adjustments in developer preferences, but also localized displacement of construction activity within treated areas. The spatial heterogeneity observed near the eligibility boundary raises the possibility that developers strategically reallocated projects to nearby zones that were still eligible for the incentive but less constrained by land availability or regulatory frictions.

As shown earlier, municipalities located within 1 km of the eligibility boundary experience both a significant decline in overall housing construction and the sharpest reductions in the share of larger units—namely three- and four-room dwellings. In contrast, areas situated between 2 and 5 km from the boundary display more modest impact in construction activity and milder typological adjustments.

This asymmetric response suggests that developers may have reallocated investment within the same local housing market—shifting projects just beyond the most constrained zone—in order to benefit from the tax incentive. Within the 1 km band, high land prices, greater building density, and intensified competition for developable parcels likely constrained the feasibility of large-scale or spacious developments. As capital moved to adjacent but still eligible zones, this localized surge in demand encountered low supply elasticity, prompting developers to prioritize smaller, higher-density units. This mechanism plausibly explains the particularly sharp typological contraction observed within the 1 km zone, where statistically significant declines are recorded not only for three- and four-room dwellings, but also for two-room units.

These findings indicate that the Pinel program may have induced a spatial reallocation of investment rather than a net expansion in supply. In the presence of localized constraints, the incentive appears to have reinforced endogenous densification dynamics—encouraging smaller-unit developments in the most pressured areas.

To isolate these displacement effects more formally, we re-estimate the event-study specifications after excluding the 1 km distance band. This adjustment removes the area most exposed to boundary-driven distortions and allows us to test whether the estimated effects persist farther from the eligibility frontier. Figure 3 and Table 8 present the corresponding results.

The revised estimates reveal a weakening of the treatment effect once the 1 km zone is excluded. Coefficients for municipalities located 2 to 5 km from the boundary are generally small and statistically insignificant. This attenuation suggests that the limited and imprecise effects observed in the baseline specification were at least partially driven by construction activity displaced toward the immediate boundary area, rather than by a net increase in new development across eligible zones.

We further explore whether unit typology is affected by the exclusion of the 1 km zone. Figure 4 shows the dynamic evolution of unit size composition. The decline in the share of larger units (three- and four-room dwellings) becomes less pronounced, while the share of smaller units remains stable or slightly positive—although these changes are not statistically significant. This attenuation reinforces the interpretation that the typological shifts observed in the full sample were driven by localized spatial displacement, concentrated within the most constrained treated areas.

Taken together, these results provide consistent evidence that the Pinel incentive altered the geographic distribution and composition of new housing developments without substantially increasing aggregate supply. In the presence of binding local constraints—whether regulatory, geographic, or political—developers appear to have responded by reallocating investment to adjacent areas, often substituting toward smaller, denser units. This behavior underscores a key limitation of geographically targeted tax incentives: without complementary measures to address land and permitting frictions, place-based subsidies may simply shift investment spatially without generating additional housing.

From a policy standpoint, these findings raise important concerns. While demand-side incentives like the Pinel program are designed to stimulate supply in specific areas, their effectiveness ultimately depends on the absorptive capacity of local housing markets. If developers face barriers to building in targeted zones, the policy may yield compositional changes and spatial distortions rather than genuine supply expansions. Ensuring the success of such instruments thus requires better alignment between fiscal incentives and local regulatory conditions.

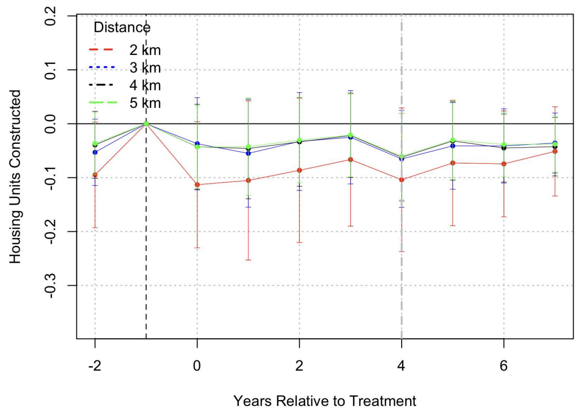
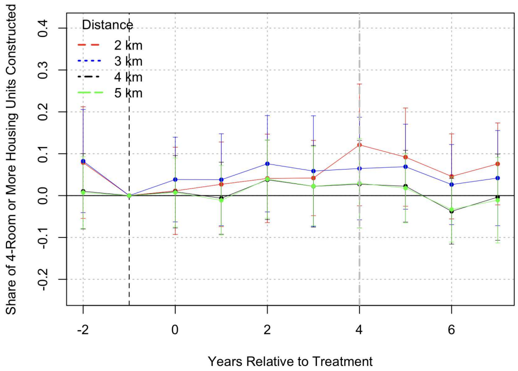
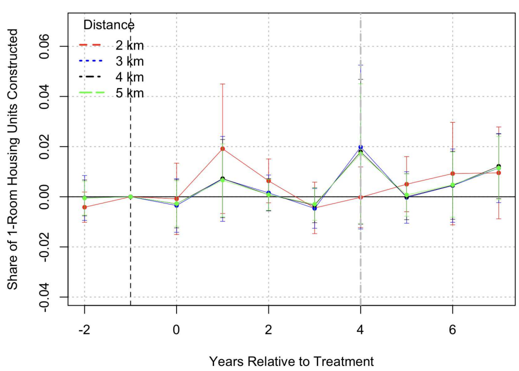


Figure 3: Housing Units Constructed (Excluding 1 km Zone)

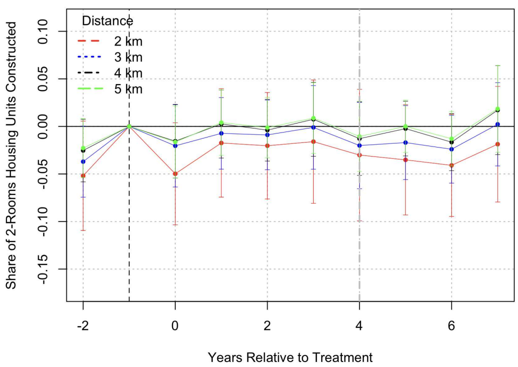
The revised results also provide additional insights into the composition of newly constructed units. Figure 4 presents the distribution of unit sizes across different distances, excluding the 1 km band.



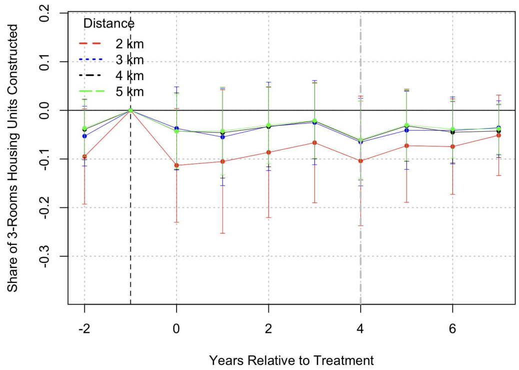
(a) Share 1-Room in New Units



(b) Share 2-Rooms in New Units



(c) Share 3-Rooms in New Units



(d) Share 4-Rooms or More in New Units

Figure 4: Distribution of Room Shares in New Units (Excluding 1 km Zone)

Overall, these results underscore the importance of accounting for spatial displacement when evaluating geographically targeted housing incentives. The Pinel policy appears to have shaped *where* housing was built, rather than increasing the total volume of new construction. This suggests that such demand-side subsidies, if not carefully designed, can produce spatial distortions that undermine their intended supply-side objectives. The findings thus raise important implications for the formulation of place-based tax incentives in constrained urban housing markets.

6.3 Vacant Housing Units Sold

Beyond its impact on new construction, a key question is whether the Pinel incentive generated additional housing that was effectively absorbed by the market, or whether it led to the accumulation of underutilized stock—particularly in smaller municipalities with structurally weaker housing demand. This question is especially relevant as the treated municipalities are in Zone B2,[8] where the policy targeted intermediate-sized towns and peri-urban areas, often characterized by lower rental pressure and slower demographic growth.

To assess market absorption, we analyze the dynamic effect of the policy on the number of vacant housing units sold, using log-transformed transaction counts to account for skewness in the distribution (Figure 5). This measure captures whether newly built or acquired dwellings remained vacant and were subsequently resold, potentially signaling speculative dynamics or demand-side frictions.

Prior to the policy's introduction (Years -4 to 0), trends in vacant unit sales were flat and statistically indistinguishable across distance bands, confirming the validity of the parallel trends assumption. However, following policy implementation (Year 0), we observe a sharp increase in vacant unit transactions within the 1 km and 2 km zones. These effects become statistically significant from Year 0 onward and persist throughout the treatment window.

This pattern is consistent with the displacement effects documented in previous sections. The 1 km and 2 km zones were most affected by spatial reallocation of construction activity, as developers and investors redirected new housing supply toward eligible areas just beyond the policy discontinuity. The observed surge in vacant transactions in these areas is therefore unsurprising: it likely reflects speculative acquisition of new or underutilized housing units, motivated by the expectation of future rental income under the tax incentive scheme.

Importantly, the upward trend in vacant unit transactions continues well after the end of the policy in Year 4. From Years 5 to 7, we observe a further increase in the number of vacant units sold, particularly in the 1 km and 2 km bands, where treatment effects remain positive and statistically significant. This dynamic suggests that the local housing markets may not have fully absorbed the additional supply induced by the policy, leading to excess stock being resold once the incentive ended.

This interpretation is consistent with concerns about policy-induced overbuilding in low-demand areas. In Zone B2 municipalities—unlike in high-pressure urban cores—the marginal unit added by the Pinel scheme may have outpaced effective rental demand, especially in contexts where households are more mobile or rental yields are lower. As a result, the post-policy period may reflect a correction phase, with unsold or unrented units returning to the market.

[8] In the French housing policy system, Zone B2 includes medium-sized municipalities and peri-urban areas with moderate housing demand. These areas are eligible for subsidies only under certain conditions, and are typically more sensitive to local supply–demand imbalances.

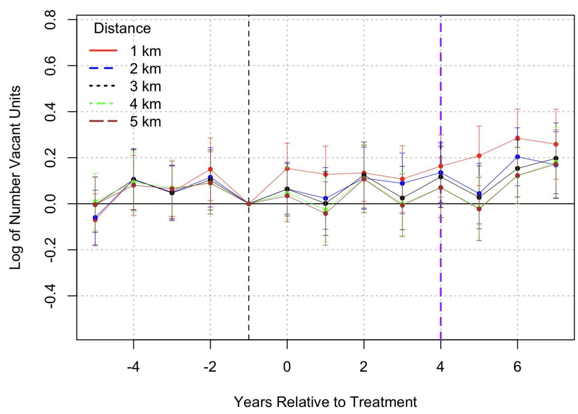


Figure 5: Log Number of Vacant Housing Units Sold

6.3.1 Displacement Effects

To assess whether the observed increase in vacant unit sales was concentrated near the policy boundary, we re-estimate the model after excluding the 1 km zone—the area most likely affected by spatial frictions and investment reallocation. Table 15 and Figure 6 present the corresponding estimates.

Once the 1 km band is removed from the sample, the treatment effects on vacant housing transactions weaken substantially. The post-policy coefficients beyond 2 km are no longer statistically significant and remain close to zero throughout the treatment window. This attenuation suggests that the sharp increase in vacant unit sales observed in the full sample was primarily driven by activity concentrated within the immediate boundary zone.

These findings lend further support to the displacement mechanism identified in earlier sections: developers and investors may have responded to the tax incentive by concentrating their activity in the most eligible, yet geographically constrained, areas—leading to a surge in transactions that was not necessarily supported by structural rental demand. The exclusion of the 1 km zone removes this localized effect, revealing the absence of significant policy impact on vacancy turnover in less constrained municipalities.

Overall, these results suggest that the Pinel program influenced the geography of investment more than its volume. In the context of Zone B2 municipalities—characterized by moderate rental demand and limited absorptive capacity—the incentive appears to have encouraged speculative acquisitions near the boundary, some of which may have failed to translate into long-term occupancy. This highlights a key risk of geographically targeted demand-side subsidies: when local market fundamentals are weak, such incentives may exacerbate spatial distortions without generating durable improvements in housing utilization.

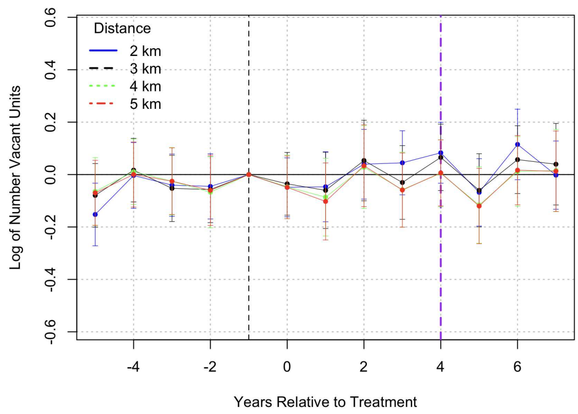


Figure 6: Log Number of Vacant Housing Units Sold (Excluding 1 km Zone)

6.3.2 New Housing Prices

Figure 7 and Table 16 present the dynamic treatment effects of the Pinel tax incentive on the log of new housing prices across distance bands from the eligibility boundary. The specification includes year, municipality, and boundary fixed effects, and standard errors are clustered at the municipality level.

Pre-treatment coefficients (Years -4 to -1) are flat and statistically indistinguishable across all distance bands, supporting the credibility of the identification strategy and the parallel trends assumption.

Following policy implementation in Year 0, we observe a marked increase in new housing prices in areas located closest to the policy boundary. In the 1 km zone, prices rise significantly from Year 1 onward, peaking between Years 2 and 3 with cumulative increases exceeding 25%. The 2 km band also exhibits a moderate but statistically significant price increase during the same period, while zones farther from the boundary (3 to 5 km) show no meaningful deviation from pre-policy levels.

This localized price appreciation is consistent with a surge in investor demand induced by the tax incentive, concentrated in areas where eligibility was clearly defined and immediately salient to market participants. The spatial concentration of price effects suggests that developers and buyers strategically responded to the geographic cutoff, thereby amplifying price pressures in the immediate eligibility zone.

Importantly, the magnitude and persistence of price effects are closely aligned with the zones that experienced the highest construction intensity and the most pronounced increase in vacant unit transactions (see Section 4). These patterns reinforce the interpretation that investor expectations—rather than structural changes in supply or demand—were the primary drivers of observed price dynamics.

After the policy's discontinuation in Year 4, the treatment effects fade across all distance bands. Coefficients become statistically insignificant, and prices gradually revert toward pre-policy levels, indicating that the effects of the incentive were transitory and did not permanently alter the price structure of the new housing market.

Overall, these findings highlight how geographically targeted tax incentives can generate short-term price distortions in local housing markets without producing sustained affordability gains. The results underscore the importance of considering spatially differentiated demand elasticities and investment dynamics when designing place-based housing policies—especially in markets characterized by weak underlying demand, such as those in Zone B2.

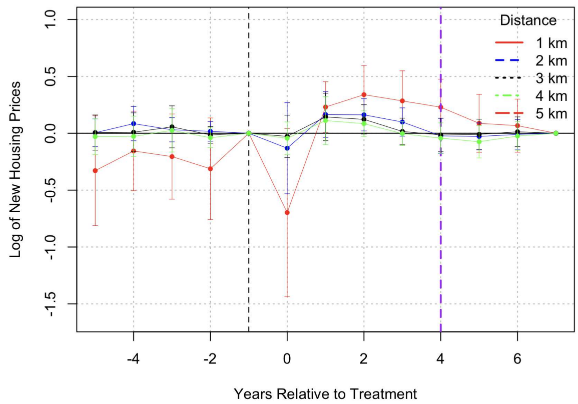


Figure 7: Log New Housing Prices

6.3.3 Old Housing Prices

Figure 8 displays the estimated effects of the Pinel tax incentive on old housing prices, measured as the log of transaction values for existing dwellings across municipalities located 1 to 5 km from the policy boundary. Full regression results are reported in Table 17.

Prior to the policy's implementation (Years -5 to -1), old housing prices exhibit stable trends across all distance bands, supporting the parallel trends assumption. During the early years of the policy (Years 0 to 3), price dynamics remain flat and statistically insignificant across all zones, suggesting that the introduction of the tax incentive had limited immediate spillover effects on the resale market.

However, a notable downward trend emerges after Year 4, coinciding with the policy's phase-out. From Year 5 onward, old housing prices begin to decline significantly—particularly in the 3 km to 5 km bands—with effects ranging from -3% to -7.5% and reaching statistical significance in Years 6 and 7. This delayed response points to structural imbalances in local housing markets following the termination of the incentive.

One plausible mechanism behind this decline lies in the interaction between excess supply and weakened demand in intermediate-sized municipalities. As discussed in Section 6.3, the post-policy period saw a sharp increase in vacant housing sales, especially in the zones closest to the policy boundary. The Pinel scheme had previously stimulated investment displacement at the boundary, much of it speculative, with investors anticipating favorable rental returns. Yet once the incentive was withdrawn, the disappearance of the tax advantage combined with structurally weak rental demand in Zone B2 likely resulted in a surplus of unsold or unrented units.

Importantly, even if these newly constructed dwellings were never occupied, they entered the resale market as ”old” housing once the initial transaction occurred. This influx of near-new but vacant housing into the secondary market likely increased the effective supply of old units, amplifying competition and exerting downward pressure on prices. The result is a localized depreciation in old housing prices that materialized after the policy ended—not due to substitution effects during the policy, but due to excess supply lingering in its aftermath.

These findings suggest that demand-side subsidies such as the Pinel policy may generate medium-run distortions in the structure and segmentation of local housing markets. In municipalities with low absorption capacity, tax-driven construction booms can outpace underlying demand, producing residual inventory that later depresses prices in adjacent market segments. From a policy perspective, this highlights the importance of aligning fiscal incentives with local market fundamentals to avoid misallocations of housing investment that persist beyond the policy window.

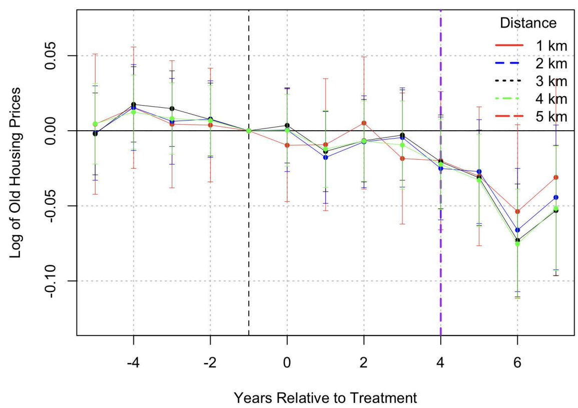


Figure 8: Log Old Housing Prices

6.4 Displacement Effects on Housing Prices

To further isolate the spatial incidence of the Pinel policy, we re-estimate dynamic treatment effects after excluding the 1 km band, which—as previously documented—was the area most affected by boundary-related distortions. Figures 9 and 10 present the estimated policy impacts on new and old housing prices, respectively, while Tables 18 and 19 report the corresponding coefficients.

The exclusion of the 1 km zone yields a markedly different pattern for new housing prices. In contrast to the full-sample estimates, where new housing prices rose sharply between Years 1 and 3, the trajectory becomes flatter and statistically insignificant beyond the immediate eligibility boundary. This suggests that the earlier price appreciation was largely concentrated within close proximity to the policy cutoff, where investment intensity was highest. Once this boundary zone is removed, the treatment effect on new housing prices largely dissipates, pointing to a localized price response rather than a generalized market-wide effect.

For old housing prices, however, the exclusion of the 1 km zone reveals a more persistent and statistically significant decline beginning around Year 5. The negative effects, which reach up to 8.3% by Year 6 in the 3–5 km bands, are stronger than those observed in the full sample. This amplified post-policy decline suggests that the depreciation of older housing stock was not simply a result of proximity to the boundary, but rather a broader structural adjustment following the withdrawal of the tax incentive.

A key mechanism behind this pattern appears to be the reintegration of vacant investor-owned properties into the resale market. As discussed in Section 6.3, the Pinel policy stimulated substantial investment in new housing, particularly in lower-demand zones such as Zone B2. However, rental demand in these areas remained weaker than anticipated. Once the policy was phased out, investors who failed to secure tenants may have opted to exit the market by selling their properties—many of which, although newly built, were classified as part of the old housing stock upon resale. This influx of supply likely exacerbated price pressures in the secondary market.

Overall, the exclusion of the 1 km band strengthens the interpretation that the Pinel policy had highly localized effects on new housing prices, but more diffuse and delayed consequences for old housing prices. The findings underscore the importance of considering both spatial and temporal spillovers when evaluating geographically targeted tax incentives. In particular, they highlight a key limitation of demand-side subsidies in structurally weak markets: without strong rental absorption, such incentives may induce temporary investment surges that are later followed by market corrections in the resale segment.

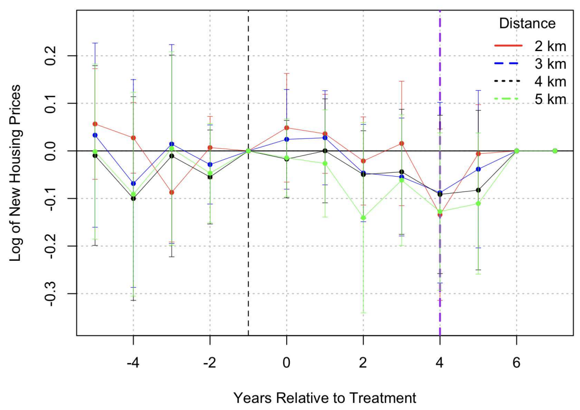


Figure 9: Log New Housing Prices (Excluding 1 km Zone)

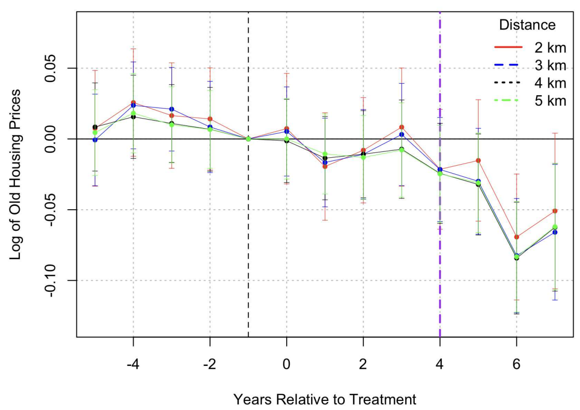


Figure 10: Log Old Housing Prices (Excluding 1 km Zone)

7 Conclusion

This paper evaluates the effects of the Pinel tax incentive on housing market outcomes in intermediate-demand areas of France using a spatial difference-in-differences event-study around eligibility boundaries. We ask whether the policy expanded new construction, altered the composition of supply, and generated spillovers in vacancy and prices.

Three main findings emerge. First, the policy did not generate a statistically significant increase in the number of newly authorized units. Estimated effects are near zero across distance bands, with a short-lived decline within 1 km of the boundary. When this most constrained band is excluded, effects attenuate further, indicating spatial reallocation rather than net supply expansion.

Second, the policy reshaped the typology of new construction. The share of larger dwellings—three-room and four-or-more-room units—declines markedly and persistently, especially near the boundary, with no compensating increase in the share of smaller units. This implies a genuine downsizing of average unit size, driven by incentives that favor products with higher rent-per-square-meter returns within rent ceilings. Typological effects persist beyond the policy window.

Third, we document a sustained increase in vacant-unit transactions near the boundary, beginning during the policy and continuing into the post-policy period. This is followed by a significant decline in prices for old housing units. The post-policy depreciation, is consistent with the reintegration of investor-owned, under-occupied units into the resale market once tax advantages expired and rental demand proved insufficient.

Taken together, the Pinel program altered where and what was built—promoting smaller units near the eligibility boundary—without expanding the total supply of housing. In structurally weaker markets, such as Zone B2 municipalities, demand-side subsidies appear to have induced spatial displacement, compositional shifts, and speculative inventory that later depressed secondary-market prices.

Place-based tax incentives should be paired with supply-side facilitators—such as faster permitting, serviced land, and predictable local planning—to convert fiscal incentives into genuine additions to supply. Incentive design should also address both quantity and composition, for example by conditioning support on the inclusion of larger units. Finally, eligibility should account for local absorptive capacity to avoid speculative overbuilding and post-policy market corrections. Without these safeguards, geographically targeted, demand-side subsidies risk shifting investment spatially, compressing unit size, and ultimately failing to alleviate underlying housing shortages.

Tables

Table 1: Descriptive Statistics (2010 Census): Treated vs. Control Municipalities

Variable	**Mean (Treated)**	**SD (Treated)**	**Mean (Control)**	**SD (Control)**	**t-stat**
Total population	6,857	7,546	1,292	1,138	8.54
Population aged 0–14	1,239	1,327	272	254	8.43
Population aged 15–29	1,264	1,549	197	185	7.99
Population aged 30–44	1,298	1,350	281	255	8.71
Population aged 45–59	1,376	1,445	278	230	8.80
Population aged 60–74	997	1,099	175	153	8.67
Total dwellings	3,448	4,110	573	527	8.11
Main residences	3,019	3,561	500	441	8.20
Secondary residences	164	424	41.2	168	3.30
Vacant dwellings	265	404	31.6	33.6	6.70
Occupied dwellings	2,701	2,748	586	510	8.90
Active population (15–64)	3,110	3,308	630	549	8.68
Unemployed (15–64)	430	617	47.3	45.6	7.20
Salaried workers (15+)	1,196	1,236	249	218	8.86
Non-salaried workers (15+)	89.3	90.6	23.0	20.4	8.45
Higher education (15+)	227	264	38.6	38.1	8.28
Number of households	3,019	3,561	500	441	8.20
Number of municipalities	**135**		**508**		

Notes: This table reports mean values, standard deviations, and t-statistics comparing treated (Pinel-eligible) and control municipalities. The t-statistic tests equality of means. The treated sample includes 135 municipalities and the control sample 508. All variables are drawn from the 2010 French Population Census (INSEE).

Table 2: Mean Differences (Treated − Control) and t-Statistics by Distance Band

Panel	Variable	Mean Difference	t-stat
Panel A: ≤ 1 km Band			
	Log of property value	-0.12	-19.10
	Living area (m^2)	-32.37	-44.57
	Number of rooms	-0.40	-30.91
	Floor level	0.53	24.82
	Bathroom surface (m^2)	-0.07	-12.45
	Number of outbuildings	0.14	7.42
	Housing units sold	0.15	1.03
	Number of garages	-0.19	-38.83
Panel B: ≤ 3 km Band			
	Log of property value	-0.19	-51.21
	Living area (m^2)	-39.23	-96.91
	Number of rooms	-0.50	-66.54
	Floor level	0.53	36.62
	Bathroom surface (m^2)	-0.10	-32.76
	Number of outbuildings	0.19	14.28
	Housing units sold	0.31	4.91
	Number of garages	-0.22	-79.94
Panel C: ≤ 5 km Band			
	Log of property value	-0.18	-53.79
	Living area (m^2)	-38.49	-104.86
	Number of rooms	-0.49	-71.05
	Floor level	0.53	42.87
	Bathroom surface (m^2)	-0.10	-33.53
	Number of outbuildings	0.25	3.73
	Housing units sold	0.41	2.81
	Number of garages	-0.21	-83.42
Panel D: Full Sample			
	Log of property value	-0.18	-56.09
	Living area (m^2)	-37.73	-106.46
	Number of rooms	-0.48	-71.62
	Floor level	0.54	45.69
	Bathroom surface (m^2)	-0.09	-34.41
	Number of outbuildings	0.26	3.78
	Housing units sold	0.42	2.91
	Number of garages	-0.21	-84.41

Notes: This table reports mean differences (treated − control) and associated t-statistics for key property characteristics across distance bands. Panels correspond to cumulative distances from the Pinel eligibility boundary (≤ 1 km, ≤ 3 km, ≤ 5 km). All values are computed using the DV3F dataset.

Table 3: Dynamic treatment effects by distance (in number of housing units authorized)

	Distance: 1 km	**Distance: 2 km**	**Distance: 3 km**	**Distance: 4 km**	**Distance: 5 km**
Year -2	-1.912	-0.390	-0.172	-0.222	-0.140
	(1.944)	(1.291)	(1.087)	(0.994)	(0.960)
Year -1 (omitted)			Reference category		
Year 0	-1.703	-0.957	-0.099	-0.017	-0.040
	(2.395)	(1.818)	(1.600)	(1.493)	(1.466)
Year 1	-1.972	-0.180	-0.191	0.090	0.220
	(1.380)	(1.327)	(1.022)	(0.952)	(0.977)
Year 2	-3.903*	-0.253	-0.615	-0.685	-0.577
	(1.900)	(1.543)	(1.258)	(1.196)	(1.196)
Year 3	-5.803*	-1.076	-0.787	-0.477	-0.304
	(2.451)	(1.845)	(1.471)	(1.362)	(1.347)
Year 4	-2.064	1.160	1.613	1.596	1.676
	(2.312)	(2.127)	(1.800)	(1.633)	(1.609)
Year 5	-1.417	1.652	1.523	1.832	1.982
	(1.536)	(1.874)	(1.550)	(1.509)	(1.455)
Year 6	0.142	0.743	0.475	0.432	0.610
	(2.265)	(1.712)	(1.328)	(1.230)	(1.201)
Year 7	3.324	3.107	2.719	2.666	2.729
	(2.465)	(2.048)	(1.668)	(1.572)	(1.529)
Fixed Effects			Year, Municipality, and Boundary Fixed Effects		
Observations			6,430		
Municipalities			643		
Adjusted R^2	0.020	0.067	0.084	0.086	0.087

Notes: Standard errors in parentheses.
+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.
Standard errors clustered at the municipality level. All regressions include year, municipality, and boundary fixed effects. The omitted category is Year -1.

Table 4: Dynamic Treatment Effects by Distance – Share of 1-Room Units in New Housing

	Distance: 1 km	Distance: 2 km	Distance: 3 km	Distance: 4 km	Distance: 5 km
Year -2	-0.012	-0.005	-0.003	-0.003	-0.003
	(0.013)	(0.006)	(0.005)	(0.005)	(0.005)
Year -1			Reference category		
Year 0	-0.004	-0.002	-0.002	-0.001	-0.001
	(0.016)	(0.011)	(0.009)	(0.008)	(0.008)
Year 1	-0.014	0.004	0.001	0.002	0.002
	(0.012)	(0.008)	(0.007)	(0.007)	(0.006)
Year 2	-0.016	-0.001	-0.003	-0.002	-0.002
	(0.013)	(0.007)	(0.006)	(0.005)	(0.005)
Year 3	-0.011	-0.006	-0.006	-0.005	-0.004
	(0.012)	(0.006)	(0.005)	(0.005)	(0.005)
Year 4	0.012	0.006	0.019	0.018	0.018
	(0.023)	(0.011)	(0.014)	(0.013)	(0.013)
Year 5	-0.016	-0.002	-0.005	-0.004	-0.003
	(0.017)	(0.007)	(0.005)	(0.005)	(0.005)
Year 6	-0.009	0.002	0.001	0.002	0.002
	(0.013)	(0.009)	(0.007)	(0.007)	(0.007)
Year 7	-0.012	0.003	0.003	0.004	0.004
	(0.009)	(0.006)	(0.005)	(0.005)	(0.005)
Fixed Effects			Year, Municipality, and Boundary Fixed Effects		
Observations			6,430		
Municipalities			643		
Adjusted R^2	0.010	0.062	0.091	0.096	0.097

Notes: Standard errors in parentheses. Standard errors are clustered at the municipality level.
The omitted category is Year -1. All regressions include year, municipality, and boundary fixed effects.
$p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 5: Dynamic Treatment Effects by Distance – Share of 2-Room Units in New Housing

	Distance: 1 km	Distance: 2 km	Distance: 3 km	Distance: 4 km	Distance: 5 km
Year -2	-0.009	-0.031	-0.025	-0.019	-0.018
	(0.024)	(0.021)	(0.016)	(0.015)	(0.014)
Year -1			Reference category		
Year 0	-0.021	-0.035	-0.018	-0.016	-0.017
	(0.022)	(0.021)	(0.019)	(0.018)	(0.018)
Year 1	-0.042	-0.025	-0.015	-0.008	-0.007
	(0.026)	(0.022)	(0.017)	(0.015)	(0.015)
Year 2	-0.057*	-0.024	-0.014	-0.010	-0.009
	(0.023)	(0.022)	(0.017)	(0.016)	(0.016)
Year 3	-0.057*	-0.024	-0.014	-0.007	-0.006
	(0.025)	(0.025)	(0.019)	(0.018)	(0.018)
Year 4	-0.024	-0.015	-0.011	-0.008	-0.006
	(0.025)	(0.024)	(0.019)	(0.017)	(0.017)
Year 5	0.029	0.003	0.006	0.015	0.016
	(0.033)	(0.025)	(0.020)	(0.017)	(0.016)
Year 6	-0.006	-0.019	-0.011	-0.008	-0.006
	(0.028)	(0.021)	(0.016)	(0.015)	(0.015)
Year 7	0.016	0.001	0.008	0.017	0.017
	(0.024)	(0.022)	(0.017)	(0.018)	(0.018)
Fixed Effects			Year, Municipality, and Boundary Fixed Effects		
Observations			6,430		
Municipalities			643		
Adjusted R^2	0.08	0.08	0.08	0.08	0.09

Notes: Standard errors in parentheses. Standard errors are clustered at the municipality level.
The omitted category is Year -1. All regressions include year, municipality, and boundary fixed effects.
$p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 6: Dynamic Treatment Effects by Distance – Share of 3-Room Units in New Housing

	Distance: 1 km	Distance: 2 km	Distance: 3 km	Distance: 4 km	Distance: 5 km
Year -2	0.004	-0.078	-0.046	-0.038	-0.036
	(0.034)	(0.044)	(0.031)	(0.030)	(0.029)
Year -1			Reference category		
Year 0	-0.048	-0.106	-0.050	-0.053	-0.054
	(0.033)	(0.055)	(0.044)	(0.041)	(0.039)
Year 1	-0.030	-0.094	-0.057	-0.052	-0.048
	(0.036)	(0.061)	(0.047)	(0.044)	(0.042)
Year 2	-0.113***	-0.102	-0.058	-0.059	-0.055
	(0.034)	(0.056)	(0.045)	(0.042)	(0.040)
Year 3	-0.082*	-0.100	-0.061	-0.056	-0.053
	(0.034)	(0.059)	(0.045)	(0.042)	(0.041)
Year 4	-0.056	-0.087	-0.060	-0.058	-0.056
	(0.039)	(0.056)	(0.044)	(0.041)	(0.040)
Year 5	-0.025	-0.085	-0.057	-0.047	-0.043
	(0.026)	(0.055)	(0.041)	(0.038)	(0.038)
Year 6	-0.019	-0.064	-0.044	-0.046	-0.042
	(0.034)	(0.046)	(0.036)	(0.034)	(0.033)
Year 7	0.023	-0.039	-0.027	-0.031	-0.023
	(0.044)	(0.038)	(0.029)	(0.027)	(0.026)
Fixed Effects			Year, Municipality, and Boundary Fixed Effects		
Observations			6,430		
Municipalities			643		
Adjusted R^2	0.020	0.067	0.074	0.083	0.089

Notes: Standard errors in parentheses. Standard errors are clustered at the municipality level.
The omitted category is Year -1. All regressions include year, municipality, and boundary fixed effects.
$p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 7: Dynamic Treatment Effects by Distance – Share of 4-Room or More Units in New Housing

	Distance: 1 km	Distance: 2 km	Distance: 3 km	Distance: 4 km	Distance: 5 km
Year -2	0.058	-0.007	-0.016	-0.059	-0.058
	(0.069)	(0.060)	(0.058)	(0.051)	(0.049)
Year -1			Reference category		
Year 0	0.018	-0.057	-0.077	-0.084	-0.083
	(0.098)	(0.064)	(0.059)	(0.060)	(0.059)
Year 1	0.004	-0.057	-0.073	-0.091	-0.087
	(0.071)	(0.049)	(0.054)	(0.052)	(0.051)
Year 2	-0.050	-0.098*	-0.107	-0.123	-0.117
	(0.079)	(0.049)	(0.061)	(0.063)	(0.062)
Year 3	-0.146*	-0.145*	-0.151*	-0.157**	-0.150*
	(0.073)	(0.061)	(0.061)	(0.060)	(0.059)
Year 4	-0.115	-0.046	-0.122	-0.123*	-0.118
	(0.068)	(0.061)	(0.066)	(0.062)	(0.062)
Year 5	-0.043	-0.067	-0.116	-0.112	-0.112
	(0.073)	(0.056)	(0.060)	(0.058)	(0.057)
Year 6	0.005	-0.033	-0.091	-0.110*	-0.106*
	(0.099)	(0.043)	(0.049)	(0.049)	(0.049)
Year 7	-0.025	-0.030	-0.083	-0.092*	-0.092*
	(0.061)	(0.043)	(0.043)	(0.044)	(0.043)
Fixed Effects			Year, Municipality, and Boundary Fixed Effects		
Observations			6,430		
Municipalities			643		
Adjusted R^2	0.032	0.057	0.060	0.061	0.061

Notes: Standard errors in parentheses. Standard errors are clustered at the municipality level. The omitted category is Year -1. All regressions include year, municipality, and boundary fixed effects. $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 8: Dynamic Treatment Effects by Distance – Excluding 1 km Zone
(Number of Housing Units Authorized)

	Distance: 2 km	Distance: 3 km	Distance: 4 km	Distance: 5 km
Year -2	0.356	0.088	0.112	0.208
	(1.506)	(1.216)	(1.091)	(1.024)
Year -1	Reference category			
Year 0	-0.665	0.337	0.418	0.393
	(1.443)	(1.390)	(1.269)	(1.243)
Year 1	0.956	0.457	0.969	1.177
	(1.888)	(1.371)	(1.259)	(1.292)
Year 2	1.597	0.019	0.018	0.238
	(1.705)	(1.296)	(1.206)	(1.211)
Year 3	1.138	0.651	1.197	1.343
	(1.856)	(1.334)	(1.212)	(1.205)
Year 4	2.947	2.793	2.655	2.741
	(2.827)	(2.266)	(2.006)	(1.940)
Year 5	2.549	2.012	2.464	2.633
	(2.364)	(1.792)	(1.735)	(1.646)
Year 6	-0.400	-0.243	-0.151	0.123
	(1.946)	(1.236)	(1.092)	(1.022)
Year 7	3.275	3.104	3.100	3.165
	(2.096)	(1.770)	(1.673)	(1.612)
Fixed Effects	Year, Municipality, and Boundary Fixed Effects			
Observations	6,430			
Municipalities	643			
Adjusted R^2	0.070	0.077	0.074	0.076

Notes: Standard errors in parentheses. Standard errors are clustered at the municipality level. The omitted category is Year -1.
$p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 9: Dynamic Treatment Effects by Distance - Excluding 1 km Zone – Share of 1-Room Units in New Housing

	Distance: 2 km	Distance: 3 km	Distance: 4 km	Distance: 5 km
Year -2	-0.004	0.000	0.000	-0.001
	(0.003)	(0.005)	(0.004)	(0.003)
Year -1	Reference category			
Year 0	-0.001	-0.003	-0.003	-0.003
	(0.007)	(0.005)	(0.005)	(0.005)
Year 1	0.019	0.007	0.007	0.007
	(0.013)	(0.009)	(0.008)	(0.007)
Year 2	0.006	0.002	0.001	0.001
	(0.004)	(0.004)	(0.003)	(0.003)
Year 3	-0.004	-0.005	-0.003	-0.003
	(0.005)	(0.004)	(0.004)	(0.003)
Year 4	0.000	0.020	0.018	0.017
	(0.006)	(0.017)	(0.015)	(0.014)
Year 5	0.005	0.000	0.000	0.001
	(0.006)	(0.005)	(0.005)	(0.004)
Year 6	0.009	0.004	0.005	0.005
	(0.010)	(0.007)	(0.007)	(0.007)
Year 7	0.010	0.011	0.012	0.011
	(0.009)	(0.007)	(0.007)	(0.006)
Fixed Effects	Year, Municipality, and Boundary Fixed Effects			
Observations	6,430			
Municipalities	643			
Adjusted R^2	0.070	0.077	0.084	0.086

Notes: Standard errors in parentheses. Standard errors are clustered at the municipality level. The omitted category is Year -1.
$p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 10: Dynamic Treatment Effects by Distance - Excluding 1 km Zone – Share of 2-Room or More Units in New Housing

	Distance: 2 km	Distance: 3 km	Distance: 4 km	Distance: 5 km
Year -2	-0.052	-0.037	-0.025	-0.023
	(0.029)	(0.019)	(0.017)	(0.016)
Year -1		Reference category		
Year 0	-0.050	-0.020	-0.016	-0.016
	(0.027)	(0.022)	(0.020)	(0.019)
Year 1	-0.017	-0.007	0.003	0.004
	(0.029)	(0.019)	(0.018)	(0.018)
Year 2	-0.020	-0.009	-0.004	-0.001
	(0.029)	(0.019)	(0.017)	(0.016)
Year 3	-0.016	-0.001	0.007	0.009
	(0.033)	(0.022)	(0.020)	(0.019)
Year 4	-0.030	-0.020	-0.013	-0.011
	(0.035)	(0.023)	(0.020)	(0.019)
Year 5	-0.035	-0.017	-0.002	0.000
	(0.030)	(0.020)	(0.015)	(0.014)
Year 6	-0.041	-0.024	-0.017	-0.013
	(0.027)	(0.018)	(0.015)	(0.015)
Year 7	-0.019	0.002	0.017	0.018
	(0.031)	(0.022)	(0.024)	(0.023)
Fixed Effects	Year, Municipality, and Boundary Fixed Effects			
Observations	6,430			
Municipalities	643			
Adjusted R^2	0.101	0.107	0.114	0.116

Notes: Standard errors in parentheses. Standard errors clustered at the municipality level. The omitted category is Year -1.

$p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 11: Dynamic Treatment Effects by Distance - Excluding 1 km Zone – Share of 3-Room or More Units in New Housing

	Distance: 2 km	Distance: 3 km	Distance: 4 km	Distance: 5 km
Year -2	-0.095	-0.053	-0.039	-0.037
	(0.050)	(0.031)	(0.032)	(0.030)
Year -1	Reference category			
Year 0	-0.113	-0.037	-0.043	-0.043
	(0.060)	(0.044)	(0.040)	(0.039)
Year 1	-0.105	-0.055	-0.046	-0.043
	(0.075)	(0.051)	(0.048)	(0.046)
Year 2	-0.086	-0.033	-0.034	-0.030
	(0.068)	(0.046)	(0.042)	(0.040)
Year 3	-0.066	-0.025	-0.022	-0.021
	(0.063)	(0.044)	(0.040)	(0.039)
Year 4	-0.104	-0.065	-0.062	-0.060
	(0.068)	(0.046)	(0.041)	(0.041)
Year 5	-0.073	-0.041	-0.032	-0.030
	(0.059)	(0.041)	(0.037)	(0.037)
Year 6	-0.074	-0.041	-0.045	-0.039
	(0.050)	(0.035)	(0.032)	(0.031)
Year 7	-0.051	-0.036	-0.043	-0.038
	(0.042)	(0.028)	(0.028)	(0.026)
Fixed Effects	Year, Municipality, and Boundary Fixed Effects			
Observations	6,430			
Municipalities	643			
Adjusted R^2	0.069	0.087	0.084	0.96

Notes: Standard errors in parentheses. Standard errors clustered at the municipality level. The omitted category is Year -1.

$p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 12: Dynamic Treatment Effects by Distance - Excluding 1 km Zone – Share of 3-Room or More Units in New Housing

	Distance: 2 km	Distance: 3 km	Distance: 4 km	Distance: 5 km
Year -2	-0.095	-0.053	-0.039	-0.037
	(0.050)	(0.031)	(0.032)	(0.030)
Year -1	Reference category			
Year 0	-0.113	-0.037	-0.043	-0.043
	(0.060)	(0.044)	(0.040)	(0.039)
Year 1	-0.105	-0.055	-0.046	-0.043
	(0.075)	(0.051)	(0.048)	(0.046)
Year 2	-0.086	-0.033	-0.034	-0.030
	(0.068)	(0.046)	(0.042)	(0.040)
Year 3	-0.066	-0.025	-0.022	-0.021
	(0.063)	(0.044)	(0.040)	(0.039)
Year 4	-0.104	-0.065	-0.062	-0.060
	(0.068)	(0.046)	(0.041)	(0.041)
Year 5	-0.073	-0.041	-0.032	-0.030
	(0.059)	(0.041)	(0.037)	(0.037)
Year 6	-0.074	-0.041	-0.045	-0.039
	(0.050)	(0.035)	(0.032)	(0.031)
Year 7	-0.051	-0.036	-0.043	-0.038
	(0.042)	(0.028)	(0.028)	(0.026)
Fixed Effects	Year, Municipality, and Boundary Fixed Effects			
Observations	6,430			
Municipalities	643			
Adjusted R^2	0.110	0.117	0.114	0.126

Notes: Standard errors in parentheses. Standard errors clustered at the municipality level. The omitted category is Year -1.

$p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 13: Dynamic Treatment Effects by Distance - Excluding 1 km Zone – Share of 4-Room or More Units in New Housing

	Distance: 2 km	Distance: 3 km	Distance: 4 km	Distance: 5 km
Year -2	0.079	0.082	0.010	0.007
	(0.068)	(0.063)	(0.046)	(0.044)
Year -1	Reference category			
Year 0	0.011	0.038	0.009	0.008
	(0.053)	(0.052)	(0.044)	(0.042)
Year 1	0.027	0.038	-0.006	-0.011
	(0.052)	(0.056)	(0.044)	(0.042)
Year 2	0.041	0.076	0.038	0.039
	(0.054)	(0.059)	(0.048)	(0.048)
Year 3	0.042	0.059	0.022	0.022
	(0.046)	(0.067)	(0.050)	(0.048)
Year 4	0.121	0.065	0.027	0.029
	(0.074)	(0.063)	(0.053)	(0.055)
Year 5	0.092	0.069	0.022	0.018
	(0.060)	(0.052)	(0.044)	(0.042)
Year 6	0.046	0.026	-0.038	-0.033
	(0.052)	(0.049)	(0.040)	(0.040)
Year 7	0.076	0.042	-0.004	-0.011
	(0.050)	(0.058)	(0.053)	(0.052)
Fixed Effects	Year, Municipality, and Boundary Fixed Effects			
Observations	6,430			
Municipalities	643			
Adjusted R^2	0.110	0.111	0.111	0.112

Notes: Standard errors in parentheses. Standard errors clustered at the municipality level. The omitted category is Year -1.

$p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 14: Dynamic Treatment Effects by Distance – Log of Number of Vacant Units

	Distance: 1 km	**Distance: 2 km**	**Distance: 3 km**	**Distance: 4 km**	**Distance: 5 km**
Year -5	-0.070	-0.060	-0.003	0.009	-0.004
	(0.057)	(0.061)	(0.061)	(0.063)	(0.061)
Year -4	0.105	0.106	0.104	0.094	0.081
	(0.066)	(0.068)	(0.067)	(0.067)	(0.066)
Year -3	0.051	0.049	0.047	0.069	0.065
	(0.058)	(0.059)	(0.061)	(0.062)	(0.061)
Year -2	0.150*	0.115	0.103	0.089	0.091
	(0.069)	(0.066)	(0.067)	(0.069)	(0.069)
Year -1			Reference category		
Year 0	0.153**	0.064	0.064	0.046	0.034
	(0.056)	(0.056)	(0.059)	(0.058)	(0.057)
Year 1	0.128*	0.024	0.001	-0.026	-0.042
	(0.063)	(0.068)	(0.071)	(0.071)	(0.070)
Year 2	0.134*	0.111	0.125	0.109	0.107
	(0.063)	(0.069)	(0.073)	(0.075)	(0.074)
Year 3	0.107	0.089	0.025	0.000	-0.007
	(0.074)	(0.067)	(0.070)	(0.070)	(0.069)
Year 4	0.163*	0.135*	0.117	0.070	0.070
	(0.069)	(0.066)	(0.068)	(0.067)	(0.066)
Year 5	0.208**	0.044	0.028	-0.018	-0.023
	(0.066)	(0.067)	(0.070)	(0.071)	(0.070)
Year 6	0.285***	0.204**	0.153*	0.121	0.123*
	(0.064)	(0.064)	(0.063)	(0.063)	(0.062)
Year 7	0.259***	0.169*	0.197*	0.181*	0.172*
	(0.078)	(0.073)	(0.079)	(0.078)	(0.076)
Fixed Effects			Year, Municipality, and Boundary Fixed Effects		
Observations			8,359		
Municipalities			643		
Adjusted R^2	0.642	0.589	0.578	0.547	0.539

Notes: Standard errors clustered at the municipality level.

All regressions include year and municipality fixed effects. The omitted year is Year -1.

p < 0.10, * p < 0.05, ** p < 0.01, *** p < 0.001.

Table 15: Dynamic Treatment Effects by Distance – Excluding 1 km Zone (Log of Number of Vacant Units)

	Distance: 2 km	**Distance: 3 km**	**Distance: 4 km**	**Distance: 5 km**
Year -5	-0.152*	-0.079	-0.065	-0.070
	(0.061)	(0.062)	(0.066)	(0.063)
Year -4	-0.003	0.017	0.011	0.000
	(0.064)	(0.062)	(0.064)	(0.063)
Year -3	-0.040	-0.053	-0.025	-0.025
	(0.061)	(0.064)	(0.066)	(0.065)
Year -2	-0.045	-0.057	-0.067	-0.060
	(0.063)	(0.064)	(0.069)	(0.069)
Year -1	Reference category			
Year 0	-0.049	-0.035	-0.047	-0.049
	(0.057)	(0.061)	(0.062)	(0.061)
Year 1	-0.047	-0.060	-0.086	-0.103
	(0.068)	(0.074)	(0.076)	(0.075)
Year 2	0.040	0.054	0.028	0.034
	(0.068)	(0.078)	(0.081)	(0.080)
Year 3	0.045	-0.030	-0.058	-0.059
	(0.062)	(0.072)	(0.073)	(0.072)
Year 4	0.083	0.066	0.005	0.006
	(0.059)	(0.065)	(0.065)	(0.064)
Year 5	-0.068	-0.060	-0.116	-0.120
	(0.066)	(0.071)	(0.074)	(0.073)
Year 6	0.115	0.057	0.012	0.017
	(0.069)	(0.066)	(0.068)	(0.067)
Year 7	-0.002	0.039	0.016	0.013
	(0.066)	(0.079)	(0.080)	(0.078)
Fixed Effects	Year, Municipality, and Boundary Fixed Effects			
Observations	8,359			
Municipalities	643			
Adjusted R^2	0.635	0.635	0.605	0.602

Notes: Standard errors are clustered at the municipality level.

All regressions include year and municipality fixed effects. The omitted year is Year -1.

p $<$ 0.10, * p $<$ 0.05, ** p $<$ 0.01, *** p $<$ 0.001.

Table 16: Dynamic Treatment Effects by Distance – Log of New Housing Prices

	Distance: 1 km	Distance: 2 km	Distance: 3 km	Distance: 4 km	Distance: 5 km
Year -5	-0.329	0.005	0.006	-0.029	-0.029
	(0.246)	(0.063)	(0.079)	(0.081)	(0.079)
Year -4	-0.156	0.085	0.009	-0.027	-0.027
	(0.178)	(0.077)	(0.087)	(0.090)	(0.088)
Year -3	-0.205	0.031	0.056	0.026	0.030
	(0.191)	(0.054)	(0.094)	(0.097)	(0.093)
Year -2	-0.312	0.017	-0.015	-0.039	-0.040
	(0.228)	(0.044)	(0.037)	(0.044)	(0.042)
Year -1			Reference category		
Year 0	-0.698	-0.131	-0.027	-0.044	-0.029
	(0.377)	(0.204)	(0.095)	(0.073)	(0.058)
Year 1	0.231*	0.165	0.143	0.112	0.091
	(0.113)	(0.102)	(0.106)	(0.107)	(0.096)
Year 2	0.339*	0.162*	0.122	0.084	0.005
	(0.131)	(0.072)	(0.066)	(0.057)	(0.067)
Year 3	0.285*	0.099	0.016	-0.003	-0.015
	(0.135)	(0.063)	(0.060)	(0.054)	(0.051)
Year 4	0.229	-0.023	-0.015	-0.043	-0.068
	(0.125)	(0.079)	(0.075)	(0.069)	(0.066)
Year 5	0.087	-0.028	-0.010	-0.075	-0.090
	(0.130)	(0.060)	(0.069)	(0.073)	(0.068)
Year 6	0.067	-0.010	0.013	-0.024	-0.034
	(0.119)	(0.066)	(0.067)	(0.064)	(0.061)
Fixed Effects			Year, Municipality, and Boundary Fixed Effects		
Observations	1,295	2,984	3,730	3,984	4,034
Municipalities	76	136	160	167	172
Adjusted R^2	0.715	0.767	0.742	0.748	0.742
Within R^2	0.663	0.675	0.641	0.648	0.640

Notes: Standard errors in parentheses. Standard errors are clustered at the municipality level. All regressions include year, municipality, and Boundary fixed effects. The omitted year is Year -1. $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 17: Dynamic Treatment Effects by Distance – Log of Old Housing Prices

	Distance: 1 km	Distance: 2 km	Distance: 3 km	Distance: 4 km	Distance: 5 km
Year -5	0.004	-0.002	-0.002	0.005	0.002
	(0.024)	(0.016)	(0.014)	(0.014)	(0.013)
Year -4	0.015	0.015	0.018	0.012	0.014
	(0.021)	(0.015)	(0.013)	(0.013)	(0.012)
Year -3	0.004	0.006	0.015	0.008	0.007
	(0.022)	(0.015)	(0.013)	(0.012)	(0.012)
Year -2	0.004	0.008	0.007	0.007	0.006
	(0.019)	(0.013)	(0.012)	(0.012)	(0.011)
Year -1			Reference category		
Year 0	-0.010	0.001	0.004	0.000	0.001
	(0.019)	(0.014)	(0.013)	(0.012)	(0.012)
Year 1	-0.009	-0.018	-0.014	-0.012	-0.010
	(0.022)	(0.016)	(0.014)	(0.013)	(0.013)
Year 2	0.005	-0.007	-0.007	-0.007	-0.009
	(0.022)	(0.016)	(0.014)	(0.014)	(0.013)
Year 3	-0.018	-0.004	-0.003	-0.010	-0.010
	(0.022)	(0.017)	(0.015)	(0.015)	(0.014)
Year 4	-0.020	-0.025	-0.021	-0.022	-0.023
	(0.023)	(0.017)	(0.016)	(0.015)	(0.015)
Year 5	-0.030	-0.027	-0.031	-0.033*	-0.032*
	(0.024)	(0.018)	(0.016)	(0.016)	(0.016)
Year 6	-0.054	-0.066**	-0.073***	-0.075***	-0.075***
	(0.029)	(0.021)	(0.019)	(0.019)	(0.018)
Year 7	-0.031	-0.044	-0.053*	-0.052*	-0.052*
	(0.033)	(0.025)	(0.022)	(0.021)	(0.021)
Fixed Effects			Year, Municipality, and Parcel Fixed Effects		
Observations	59,111	138,183	182,947	199,164	206,810
Municipalities	440	472	476	478	479
Adjusted R^2	0.591	0.593	0.589	0.584	0.584
Within R^2	0.427	0.447	0.448	0.441	0.437

Notes: Standard errors in parentheses. Standard errors are clustered at the municipality level. All regressions include year, municipality, and parcel fixed effects. The omitted year is Year -1.
$p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 18: Dynamic Treatment Effects by Distance – Excluding 1 km Zone (Log of New Housing Prices)

	Distance: 2 km	Distance: 3 km	Distance: 4 km	Distance: 5 km
Year -5	0.057	0.033	-0.010	-0.002
	(0.059)	(0.099)	(0.096)	(0.094)
Year -4	0.027	-0.069	-0.100	-0.091
	(0.038)	(0.112)	(0.109)	(0.109)
Year -3	-0.087	0.014	-0.011	0.005
	(0.053)	(0.107)	(0.108)	(0.104)
Year -2	0.007	-0.029	-0.055	-0.047
	(0.033)	(0.042)	(0.050)	(0.053)
Year -1	Reference category			
Year 0	0.049	0.024	-0.017	-0.015
	(0.058)	(0.054)	(0.041)	(0.042)
Year 1	0.036	0.028	0.000	-0.026
	(0.042)	(0.051)	(0.056)	(0.057)
Year 2	-0.021	-0.047	-0.050	-0.140
	(0.047)	(0.052)	(0.047)	(0.102)
Year 3	0.016	-0.055	-0.044	-0.062
	(0.067)	(0.063)	(0.067)	(0.070)
Year 4	-0.134	-0.088	-0.091	-0.128
	(0.092)	(0.097)	(0.085)	(0.085)
Year 5	-0.006	-0.038	-0.082	-0.111
	(0.053)	(0.084)	(0.085)	(0.076)
Fixed Effects	Year, Municipality, and Boundary Fixed Effects			
Observations	1,689	2,435	2,689	2,739
Municipalities	98	125	133	139
Adjusted R^2	0.827	0.775	0.781	0.773
Within R^2	0.744	0.679	0.685	0.674

Notes: Standard errors in parentheses. Standard errors are clustered at the municipality level. All regressions include year, municipality, and Boundary fixed effects. The omitted year is Year -1. $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 19: Dynamic Treatment Effects by Distance – Excluding 1 km Zone (Log of Old Housing Prices)

	Distance: 2 km	Distance: 3 km	Distance: 4 km	Distance: 5 km
Year -5	0.007	-0.001	0.008	0.005
	(0.021)	(0.017)	(0.016)	(0.015)
Year -4	0.026	0.024	0.016	0.018
	(0.019)	(0.016)	(0.015)	(0.014)
Year -3	0.017	0.021	0.011	0.010
	(0.019)	(0.015)	(0.014)	(0.014)
Year -2	0.014	0.008	0.007	0.007
	(0.018)	(0.016)	(0.015)	(0.014)
Year -1	Reference category			
Year 0	0.007	0.005	-0.001	0.000
	(0.020)	(0.016)	(0.015)	(0.015)
Year 1	-0.019	-0.017	-0.014	-0.011
	(0.019)	(0.016)	(0.015)	(0.014)
Year 2	-0.008	-0.011	-0.011	-0.013
	(0.019)	(0.016)	(0.016)	(0.015)
Year 3	0.008	0.003	-0.007	-0.008
	(0.021)	(0.018)	(0.018)	(0.017)
Year 4	-0.021	-0.022	-0.024	-0.025
	(0.022)	(0.019)	(0.018)	(0.018)
Year 5	-0.015	-0.030	-0.032	-0.031
	(0.022)	(0.019)	(0.018)	(0.018)
Year 6	-0.069**	-0.083***	-0.084***	-0.083***
	(0.023)	(0.021)	(0.020)	(0.020)
Year 7	-0.051	-0.066**	-0.062**	-0.062**
	(0.028)	(0.024)	(0.023)	(0.023)
Fixed Effects	Year, Municipality, and Parcel Fixed Effects			
Observations	79,072	123,836	140,053	147,699
Municipalities	455	465	469	470
Adjusted R^2	0.611	0.599	0.592	0.591
Within R^2	0.458	0.455	0.445	0.439

Notes: Standard errors in parentheses. Standard errors are clustered at the municipality level. All regressions include year, municipality, and parcel fixed effects. The omitted year is Year -1. $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

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