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Threshold-Based Regulation and Redevelopment in a Land-Constrained Market: Evidence from Mixed-Use

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Threshold-Based Regulation and Redevelopment in a Land-Constrained Market: Evidence from Mixed-Use Mandates in the Paris Region

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Abstract

Urban land-use regulations often bind at project-size thresholds, yet little is known about how such rules operate in built-out metropolitan markets. Standard models predict bunching below thresholds and reductions in project initiation. I show that these predictions break down when development occurs primarily through redevelopment within a durable built environment. I study the 2019 tightening of the Paris Region's administrative approval procedure for large office developments, which introduced a discontinuous regulatory wedge within a sharply defined perimeter. Using a border-based difference-in-discontinuities design, I compare municipalities just inside and just outside the boundary before and after the reform. The results reject a quantity-based account of threshold regulation. I find no evidence of bunching, no robust decline in office permitting, and no increase in net housing supply. Instead, the adjustment operates through redevelopment margins. Residential permits become more likely to involve demolition, intervention on existing structures, housing loss, and office-related transformation, while office permits shift away from pure office expansion toward mixed-use and housing-generating forms. Consistent with a tightening of office-side constraints, office prices increase following the reform, whereas residential prices do not. These findings imply that in built-out urban environments, threshold-based regulation binds primarily through the reorganization of redevelopment within existing structures rather than through aggregate quantity adjustment. As a result, standard quantity-based measures can substantially mischaracterize the economic incidence of land-use regulation. More broadly, the paper highlights redevelopment as the central margin through which urban policy operates in constrained metropolitan markets.

Keywords: Land Use Regulation; Urban Economics; Real Estate Development; Housing Prices; Spatial DiD

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1. Introduction

Threshold-based land-use regulations are a pervasive feature of urban policy. A large literature studies their effects through the lens of regulatory notches, emphasizing two canonical responses: bunching below the threshold and reductions in project initiation. These predictions rely on environments in which developers can adjust project scale smoothly and where development occurs on relatively unconstrained land. In dense metropolitan areas, however, these conditions rarely hold. Land is scarce, most sites are already occupied, and new investment primarily takes the form of redevelopment within an inherited built environment.

This paper shows that in such settings, threshold-based regulation operates through a different margin. When development occurs within existing structures, avoiding regulatory thresholds does not simply require marginal adjustments in scale, but often entails costly redesign and reallocation across uses. As a result, regulation need not generate bunching or large quantity responses even when it binds. Instead, it reshapes the internal organization of redevelopment, inducing compositional adjustments within projects rather than changes in aggregate development volumes.

A key implication follows. In built-out environments, pressure toward mixed-use development need not translate into net housing expansion. When projects are constrained by an inherited built envelope, accommodating additional uses requires reallocating existing space rather than adding new construction. Policies that restrict office expansion may therefore alter project composition without increasing housing supply, and may do so alongside reductions in residential surface within reconfigured sites.

I study this mechanism in the context of the 2019 tightening of the Paris Region's *procédure d'agrément*, an administrative approval procedure governing large office developments within a sharply defined metropolitan perimeter. The reform increases the cost of projects that generate large net additions of office floor area, while leaving many forms of redevelopment within the existing stock comparatively less exposed. This setting combines a discontinuous regulatory rule with a land-constrained environment in which redevelopment is the dominant margin of adjustment.

To identify the causal effects of the reform, I exploit the fixed boundary of the regulated area in a border-based difference-in-discontinuities design comparing municipalities just inside and just outside the perimeter before and after 2019. This approach isolates the change in the cross-boundary discontinuity induced by the reform. I combine detailed geocoded building-permit data, which track creation, demolition, extension, and transformation by use, with transaction-level data from the DV3F database to analyze both project-level responses and price effects.

The results reject a quantity-based account of threshold regulation. I find no evidence of bunching below the relevant thresholds and no robust decline in office permitting. Nor does the reform generate a measurable increase in residential construction or housing floor area. Instead, the adjustment operates through redevelopment and compositional margins. Residential permits become more likely to involve intervention on existing structures, demolition, and redevelopment-oriented configurations, and they exhibit increased housing loss together with office-related transformation. At the same time, office permits shift away from pure office expansion toward mixed-use and housing-generating forms. Consistent with a tightening of office-side constraints, office prices increase following the reform, while residential prices show no systematic response.

The paper makes two contributions. First, it shows that in built-out metropolitan markets, the economic incidence of threshold-based land-use regulation does not primarily operate through aggregate quantity adjustments in the targeted use. Instead, regulation reshapes the organization of redevelopment within constrained sites, generating cross-use reallocation and compositional changes that are largely invisible in standard quantity measures. Weak or null responses on aggregate outcomes should therefore not be interpreted as evidence of limited policy impact, but as reflecting adjustment along margins that standard frameworks do not capture.

Second, the paper highlights a systematic divergence between the administrative target of regulation and its economic incidence. Although the policy directly applies to large office developments, some of its clearest effects emerge in residentially classified permits. This pattern reflects the fact that redevelopment decisions are made at the level of sites that embed multiple uses and physical constraints, so that regulations targeting one use can propagate across other margins of adjustment.

More broadly, the paper contributes to the literature on land-use regulation in constrained urban environments. While existing interpretations of threshold regulation implicitly fit settings in which project scale can adjust smoothly, this paper shows that in built-out cities the relevant margin of adjustment is the internal reallocation of space within inherited structures. Evaluating land-use regulation through aggregate construction outcomes alone may therefore substantially understate its effects.

The remainder of the paper proceeds as follows. Section 2 presents the institutional background. Section 3.1 describes the data and empirical strategy. Section 4 reports the main findings. Section 6 concludes.

2. Institutional Background and Conceptual Framework

2.1. Institutional setting: a threshold-based constraint on net additions to office space

The Paris Region is characterized by persistent spatial imbalances between employment and housing. Office and tertiary activities are highly concentrated in the western part of the region, while residential development is relatively more prevalent in the east. These imbalances have long generated commuting pressures and housing scarcity near major employment centers, and have motivated repeated policy interventions aimed at rebalancing land use within an already dense metropolitan environment.

In this context, the regional Prefect tightened in October 2018 the long-standing *procédure d'agrément*, an administrative approval process governing large office developments within a predefined perimeter covering the most employment-intensive areas of the region. For sufficiently large projects, accreditation must be obtained prior to the issuance of a building permit, and approval is assessed in light of residential compensation requirements.

A threshold-based regulatory wedge. A central feature of the reform is that it targets *net additions* to office floor space. Let Δb denote the increase in office surface relative to the pre-existing structure. The reinforced regulatory orientations apply to projects involving at least 1,000 m^2 of additional office space and become substantially more stringent once project size exceeds thresholds such as 10,000 m^2 or 20,000 m^2, depending on local conditions.

For projects above these thresholds, accreditation is conditional on the provision of residential floor space, often expressed as a multiple of the additional office surface and potentially including minimum shares of social housing. In reduced-form terms, the reform introduces a discontinuous increase in the effective cost of expanding office space beyond these thresholds within the regulated area.

Asymmetric treatment of expansion and redevelopment. The regulatory framework distinguishes sharply between projects that increase office capacity and those that operate within the existing built stock. New construction and large extensions are subject to the strongest compensation requirements, which can reach several times the additional office surface. By contrast, projects that do not increase total office space—such as rehabilitation or demolition-reconstruction without net expansion—are generally exempt from these reinforced obligations or subject only to qualitative expectations of functional mixity.

More broadly, even when redevelopment involves some reallocation across uses, the regulatory burden remains tied to *net expansion* rather than to the level of office space per se. As a result, the policy does not impose a uniform constraint on development, but instead disproportionately penalizes expansion-oriented strategies while leaving non-expanding redevelopment comparatively less constrained.

This asymmetry is central for the economic interpretation of the reform. By increasing the cost of expanding office capacity while preserving the feasibility of redevelopment within the existing built envelope, the policy alters the relative profitability of development strategies. In particular, it makes pure office expansion less attractive, while leaving mixed-use configurations and transformation-based redevelopment comparatively less affected.

In a metropolitan setting where most land is already developed and investment primarily occurs through the transformation of existing sites, such a rule is likely to affect not only whether projects proceed, but also how they are configured and which forms of redevelopment are selected.

The reform applies within a fixed geographic perimeter that remained unchanged over the study period. Importantly, the thresholds and regulatory requirements depend on pre-existing indicators of local housing–employment imbalance rather than on contemporaneous development outcomes, limiting concerns about endogenous policy targeting at the margin of the boundary.

The reform formally entered into force on November 1, 2018. In the empirical analysis, however, the post-reform period begins on January 1, 2019. This timing accounts for the administrative pipeline embedded in the accreditation process, as approval precedes permit issuance and processing times typically span several weeks to months. Permits issued in late 2018 are therefore unlikely to reflect the new regulatory constraints.

Taken together, these features imply that the reform generates a discontinuous increase in the cost of office expansion above specific thresholds within a fixed spatial boundary, while leaving redevelopment within the existing built stock comparatively less constrained. This structure closely maps to the conceptual framework below: the policy targets the *expansion margin*, but in a setting where adjustment occurs primarily through redevelopment within a constrained urban fabric.

2.2. Conceptual framework

This section develops a stylized framework of redevelopment under office-targeted threshold regulation in a built-out urban environment. The objective is to isolate the economic mechanism through which a regulation that targets *net office expansion* reshapes development outcomes when projects occur within an inherited built structure.

The key departure from standard threshold models is that developers do not choose project scale from vacant land. Instead, they face a discrete choice over redevelopment strategies, followed by a constrained allocation of uses within a pre-existing structure. This generates a two-stage decision problem.

Initial condition: a durable built structure. Consider a site characterized by an inherited structure

$$s_0 = (b_0, h_0),$$

where $b_0 \geq 0$ denotes existing office floor space and $h_0 \geq 0$ denotes existing residential floor space. The site is already developed. As in vintage models of urban capital, the existing structure is durable and cannot be costlessly reshaped.

Let $L_0 = b_0 + h_0$ denote the inherited envelope, and let $\bar{L} \geq L_0$ denote the maximal feasible post-project envelope. In a dense metropolitan environment, expansion beyond L_0 is possible but costly and limited.

Stage 1: choice of redevelopment strategy. The developer chooses a strategy

$$a \in \{K, R, E\},$$

where:

- K (keep): no major intervention, the inherited structure is retained;
- R (reconfigure): redevelopment within the existing or slightly modified envelope, with no large net office expansion;
- E (expand): expansion-oriented redevelopment with substantial net additions to office space.

Each strategy entails a different cost structure:

$$F(a) = \begin{cases} 0 & \text{if } a = K, \\ F_R > 0 & \text{if } a = R, \\ F_E > F_R & \text{if } a = E. \end{cases}$$

The fixed cost F_E captures the higher engineering, administrative, and financial burden associated with large-scale expansion projects.

Stage 2: within-site allocation. Conditional on choosing $a \in \{R, E\}$, the developer chooses post-project uses (b, h) and envelope L solving

$$\max_{b,h,L} \quad p_B b + p_H h - C(b, h; s_0) - \phi(L - L_0) - T(\Delta b; a),$$

subject to

$$b + h \leq L, \qquad L \leq \bar{L}, \qquad b \geq 0, \quad h \geq 0.$$

Prices p_B and p_H denote office and residential values.

The adjustment cost is

$$C(b, h; s_0) = \frac{c_b}{2}(b - b_0)^2 + \frac{c_h}{2}(h - h_0)^2,$$

which captures demolition, restructuring, and redesign costs. These costs make large deviations from the inherited structure increasingly expensive.

The function $\phi(L - L_0)$ captures the cost of expanding the built envelope, with $\phi' > 0$, $\phi'' \geq 0$.

Office-targeted threshold regulation. Regulation applies to net additions of office space,

$$\Delta b = b - b_0,$$

through a discontinuous burden

$$T(\Delta b; a) = \begin{cases} 0 & \text{if } a \neq E, \\ \tau \cdot \mathbf{1}(\Delta b > \bar{b}) & \text{if } a = E, \end{cases} \qquad \tau > 0.$$

This formulation captures the key institutional asymmetry: the policy penalizes *expansion-oriented strategies* that generate sufficiently large net office additions, while leaving reconfiguration strategies comparatively less exposed.

Two margins of adjustment. The developer's decision therefore involves two distinct margins:

1. a **strategy margin**: whether to keep, reconfigure, or expand;

2. an **allocation margin**: conditional on intervention, how to allocate space between office and residential uses.

This structure is central. The policy acts primarily on the first margin (expansion vs. reconfiguration), while adjustment within projects occurs along the second margin.

Why standard threshold predictions break down. In standard notch models, agents can reduce scale smoothly to remain just below the threshold. Here, avoiding the regulatory burden requires switching from strategy E to R, which involves a discrete change in project design and potentially large adjustment costs.

Moreover, conditional on redevelopment, reallocating space within a constrained envelope requires moving away from (b_0, h_0), which is costly. As a result, reducing Δb is not a frictionless scalar adjustment.

Therefore, even when the regulatory wedge τ is large, the policy need not generate bunching or large aggregate quantity responses.

Cross-use reallocation within constrained sites. Within reconfiguration or expansion, the envelope constraint implies that increases in office-oriented components may require reductions elsewhere. Let

$$h = h^{new} - \delta(\Delta b; L), \qquad \frac{\partial \delta}{\partial \Delta b} > 0.$$

The function $\delta(\cdot)$ captures the fact that accommodating office-related transformation within a constrained site may require removing residential space through demolition or conversion.

This implies that redevelopment can involve *housing loss together with office-related transformation*, rather than a simple increase in mixed-use configurations.

Implications. The framework yields four implications.

Proposition 1 (Weak bunching). A threshold on net office expansion does not necessarily generate bunching below the threshold.

Intuition. Avoiding the threshold requires switching from expansion to reconfiguration, which is a discrete and costly change in strategy. Bunching is therefore not a necessary implication.

Proposition 2 (Limited aggregate quantity response). Regulation need not produce strong changes in aggregate construction or housing supply.

Intuition. Adjustment occurs primarily through strategy selection and within-site reallocation rather than through unconstrained expansion or contraction of total built volume.

Proposition 3 (Reorientation of redevelopment strategies). The policy shifts development away from expansion-oriented projects and toward reconfiguration within the existing built stock.

Intuition. The regulatory wedge applies only to expansion, reducing the relative profitability of strategy E and increasing the attractiveness of R.

Proposition 4 (Cross-use reallocation and price effects). Office prices increase, while residential adjustments occur through reallocation within redevelopment, potentially involving housing loss and transformation.

Intuition. The policy constrains the supply of additional office space, raising office prices. Residential supply does not expand proportionally because adjustment occurs within a constrained envelope.

Mapping to empirics. These implications guide the empirical analysis. First, weak bunching responses are expected around the threshold. Second, aggregate residential quantities need not increase. Third, redevelopment margins—including works on existing structures, demolition, and transformation—should become more prevalent. Fourth, cross-use reallocation within residential permits should manifest through housing loss and office-related transformation. Finally, price effects should be asymmetric, with stronger responses in office markets than in residential markets.

The broader implication is that in a built-out metropolitan environment, the economic incidence of threshold-based regulation operates primarily through the reorganization of redevelopment strategies rather than through aggregate quantity adjustment.

3. Literature Review

This paper relates to three strands of literature.

First, it connects to the large literature on land-use regulation, housing supply, and urban prices. A substantial body of work shows that regulatory constraints reduce construction and raise housing prices in high-demand metropolitan areas (Quigley and Rosenthal (2005); Glaeser et al. (2005)). Related research documents that housing supply elasticities vary widely across cities as a result of both geographic constraints and local regulation (Saiz (2010); Hilber and Vermeulen (2016)), while broader reviews emphasize the central role of land-use controls in shaping urban development (Gyourko and Molloy (2015)). Most of this literature studies continuous constraints on development intensity, such as density restrictions, height limits, or floor-area-ratio rules, and evaluates their effects through aggregate outcomes including construction volumes, supply elasticities, and prices.

Second, the paper relates to the literature on redevelopment in cities with durable built capital. In mature metropolitan areas, adjustment often occurs not through expansion onto vacant land, but through the transformation of already-developed sites. Classic models of durable housing and urban redevelopment show that the timing and form of development depend on the interaction between land values, depreciation, and the option value of replacing existing structures (Brueckner (1980); Braid (2001); Rosenthal and Helsley (1994)). Related work distinguishing land value from structure value emphasizes that in land-constrained urban markets, rising values are increasingly capitalized into land, which can weaken the match between inherited structures and the highest-valued use of the site (Davis and Palumbo (2008); Diewert et al. (2015); Tideman and Plassmann (2018)). This perspective implies that regulatory shocks in built-out cities may operate through redevelopment margins even when aggregate quantity responses appear weak.

Third, the paper speaks to a more limited literature on policies that generate discontinuous regulatory exposure. Unlike continuous zoning constraints, threshold-based rules impose sharp changes in compliance costs once projects exceed specific cutoffs. In standard settings, such rules are expected to generate bunching below the threshold or reductions in project initiation. But in dense metropolitan markets, where sites are already occupied and project resizing is constrained by inherited structures, adjustment may instead occur through project redesign, use recomposition, and reallocation within existing sites. Despite the prevalence of such threshold-based rules in planning systems, there is little causal evidence on how they affect development behavior in built-out urban environments.

Methodologically, the paper also relates to a growing literature that exploits spatial discontinuities to identify the effects of urban policy (Autor et al. (2014); Diamond and McQuade (2019)). By comparing locations on either side of a stable policy boundary, these designs isolate policy-induced discontinuities while controlling for broader spatial gradients. The empirical strategy in this paper follows this logic by exploiting the fixed perimeter of the Paris Region's regulatory regime and comparing municipalities just inside and just outside the boundary before and after the 2019 reform.

This paper contributes to these literatures in three ways. First, it provides causal evidence on threshold-based land-use regulation, a class of policies that has received much less attention than continuous zoning or density restrictions. Second, it shows that in mature, land-constrained metropolitan markets, the main incidence of regulation need not operate through aggregate construction, but through redevelopment reorganization within the existing built environment. Third, it demonstrates that combining a spatial difference-in-discontinuities design with permit-level data makes it possible to identify these margins of adjustment directly, including demolition, transformation, extension, and cross-use reallocation, which are largely invisible in standard aggregate measures.

3.1. Data sources

The analysis combines two administrative datasets capturing complementary margins of adjustment: geocoded building permits, which measure development quantities and project composition, and property transactions from the DV3F database, which allow us to study price responses.

Building permits. I use geocoded building-permit microdata recording, for each authorized project, the year of authorization and detailed information on floor area created, demolished, and transformed by use, including offices and other tertiary uses, housing, and accommodation. The permit records also contain project descriptors such as declared destination and nature of works.

In the administrative files, permits are classified according to the main declared objective of the project. Projects whose primary destination is non-residential are recorded in the non-residential permit file, whereas projects whose primary destination is housing are recorded in the residential permit file. This classification reflects the main intended use of the project even when permits include mixed-use components.

The empirical analysis starts from permit-level observations. Depending on the outcome of interest, I either estimate permit-level specifications or aggregate permits to border-pair-by-side × year cells. For count outcomes, I construct balanced panels covering the full study period and explicitly reintroduce zero values for pair-side-year cells in which no permit is recorded.[1]

Transactions (DV3F). To study price responses, I use transaction-level data from the DV3F database for the Paris Region. The data include transaction value, built area, transaction year, and detailed structural characteristics. I study two market segments: (i) non-residential transactions classified as tertiary or professional mixed-use and (ii) residential transactions. To reduce the influence of measurement error and miscoding, extreme observations in price per square meter are trimmed.

Both datasets are organized around the same regulatory boundary, allowing quantity and price responses to be estimated within a unified spatial framework.

3.2. Spatial design

The empirical strategy exploits the fixed perimeter of the *procédure d'agrément* in the Paris region. The 2019 reform tightened regulatory constraints on large office and tertiary developments located inside this perimeter, while municipalities just outside remained subject to the pre-existing regime.

The design combines a spatial discontinuity with a temporal policy shock. Identification comes from the change in the cross-boundary discontinuity after 2019. Intuitively, the approach compares how the discontinuity at the regulatory boundary evolves before and after the reform, rather than relying on level differences across space.

To operationalize this comparison, I construct cross-boundary municipality pairs. Each pair p matches a municipality inside the regulated perimeter to its closest neighboring municipality outside the boundary. I define a pair-by-side identifier ps, which distinguishes the treated and untreated side within each border pair. These fixed effects absorb all time-invariant determinants of development specific to each side of a given border segment, including persistent differences in accessibility, land availability, urban form, and baseline development intensity.

Let $d_u \geq 0$ denote the distance of unit u to the regulatory boundary. I define signed distance as

$$D_u = \begin{cases} +d_u & \text{if } T_u = 1, \\ -d_u & \text{if } T_u = 0, \end{cases} \tag{1}$$

where $T_u = 1$ indicates that unit u lies inside the regulated perimeter. The boundary itself is normalized to $D_u = 0$.

[1]Municipal geospatial data are obtained from `https://geo.api.gouv.fr`.

Estimation is conducted within local windows satisfying $|D_u| \leq h$, with $h \in \{1000, 2000, 3000\}$ meters. Narrow bandwidths provide highly local comparisons, while wider windows assess robustness to alternative spatial comparison sets.

Importantly, municipalities on the two sides of the boundary differ systematically in baseline levels of development, reflecting the economic geography of the Paris metropolitan area. The design does not require equality of levels across the boundary. Instead, identification relies on the assumption that, absent the reform, the cross-boundary discontinuity would have evolved smoothly over time within local neighborhoods.

This border-based difference-in-discontinuities design therefore isolates the causal effect of the reform as a shift in the discontinuity at the regulatory boundary.

3.3. Outcomes

I examine three sets of outcomes corresponding to distinct margins of adjustment.

Development activity and threshold responses. I construct indicators for projects with authorized tertiary floor area in the intervals [9,000; 11,000] m^2 and [18,000; 22,000] m^2, corresponding to the regulatory thresholds. I also analyze the number of permits and total authorized floor area.

Redevelopment and cross-use adjustment. To capture adjustments within the existing built environment, I examine redevelopment margins and cross-use reallocation.

On the office side, I measure whether office permits involve housing removal and the amount of residential floor area removed.

On the residential side, I analyze indicators for demolition, works on existing structures, and extensions. I also measure whether residential permits include transformed tertiary space and the associated transformed surface. These outcomes capture the reorganization of uses within redevelopment projects.

Finally, I construct the ratio of tertiary to housing floor area within residential permits to capture compositional changes within projects.

Prices. I estimate effects on office and residential transaction prices to assess whether project-level adjustments translate into equilibrium price responses.

Continuous outcomes are transformed using $\log(1 + x)$.

3.4. Baseline specification

For permit-level outcomes, I estimate:

$$Y_{ut} = \beta(T_u \times Post_t) + \theta_1 D_u + \theta_2 (D_u \times T_u) + \alpha_{ps(u)} + \lambda_{d(u)\times t} + \varepsilon_{ut}, \tag{2}$$

where $Post_t$ equals one from 2019 onward, $\alpha_{ps(u)}$ are pair-by-side fixed effects, and $\lambda_{d(u)\times t}$ are department-by-year fixed effects.

The coefficient of interest, β, captures the post-reform change in the cross-boundary discontinuity. It measures how outcomes evolve after 2019 on the treated side of the boundary relative to the untreated side, net of permanent spatial differences across border segments and flexible subregional time shocks.

Pair-by-side fixed effects absorb all time-invariant heterogeneity specific to each side of each local border segment. Department-by-year fixed effects capture differential shocks across subregions of the Paris area, such as localized demand conditions, planning constraints, or economic fluctuations.

The inclusion of signed distance and its interaction with treatment allows for flexible spatial gradients that may differ on either side of the boundary. Identification therefore does not rely on identical spatial slopes across treated and control areas.

Standard errors are clustered at the pair-by-side level.

Count outcomes. For permit counts, the unit of observation is the border-pair-by-side $\times$ year cell. I estimate analogous specifications on a balanced panel covering all pair-side-year cells from 2014 to 2024, with zero counts explicitly reintroduced when no permit is observed.

Retaining zero-permit cells is essential to identify extensive-margin responses, as the reform may affect both the number of permits and the probability of no development occurring in a given local area-year.

3.5. Dynamic effects

To assess pre-trends and characterize adjustment dynamics, I estimate event-study specifications:

$$Y_{ut} = \sum_{k \neq -1} \beta_k \left(T_u \times \mathbb{1}\{t - 2019 = k\} \right) + \theta_1 D_u + \theta_2 (D_u \times T_u) + \alpha_{ps(u)} + \lambda_{d(u)\times t} + \varepsilon_{ut}. \tag{3}$$

The coefficients β_k trace the evolution of the cross-boundary discontinuity relative to the year immediately preceding the reform.

This specification provides a direct test of the identifying assumption. The absence of differential pre-trends supports the interpretation that treated and control locations would have followed similar trajectories in the absence of the reform.

3.6. Donut specifications

A potential concern in boundary designs is that estimated effects reflect very local relocation across the frontier rather than genuine changes in development behavior.

To address this, I estimate donut specifications that exclude observations closest to the boundary. For a given bandwidth h, the estimation sample satisfies:

$$500 < |D_u| \leq h. \tag{4}$$

If estimated effects persist outside the 0–500 meter band, they are unlikely to be driven solely by boundary-hugging relocation and instead reflect broader adjustments in project organization and redevelopment behavior.

3.7. Identification assumptions

The identifying assumption is that, absent the reform, outcomes would have evolved smoothly across the regulatory boundary within local neighborhoods.

I assess this assumption using four complementary approaches. First, I test for bunching and sorting around regulatory thresholds and the boundary. Second, I estimate event-study specifications to detect differential pre-trends. Third, I conduct placebo reforms in pre-2019 years. Fourth, I assess robustness across bandwidths and donut specifications.

Taken together, these exercises support the interpretation that the observed post-2019 change in the cross-boundary discontinuity reflects the causal effect of the regulatory shock, rather than pre-existing differential dynamics or purely local spatial displacement.

4. Results

This section evaluates the four empirical implications of the framework. The results reject a canonical quantity-based account of threshold regulation in this built-out setting. I find no evidence of sorting or bunching around the boundary and no broad quantity response in either residential construction or office permitting. Instead, the clearest effects appear on redevelopment and compositional margins: residential permits become more redevelopment-oriented and exhibit greater cross-use reallocation within existing structures, while office permits shift away from pure office expansion toward mixed and housing-generating forms. Price effects point in the same direction: office prices rise after the reform, while residential prices do not.

4.1. No evidence of sorting or bunching around the boundary

The first implication of the framework is that threshold regulation need not generate canonical bunching responses in a built-out environment. In standard notch settings, agents can often adjust project size smoothly in order to remain just below the threshold. Here, by contrast, avoiding the regulatory burden requires redesign within an inherited structure, which is costly. The relevant question is therefore whether the reform induced any discontinuous change in the spatial density of projects around the approval boundary.

Table 1 reports McCrary-type density discontinuity tests using the signed distance to the boundary as the running variable. Across both office/tertiary and residential permits, the null of density continuity cannot be rejected in either the pre-reform or post-reform period. Figure 1 leads to the same conclusion visually: the number of permit observations evolves smoothly around the boundary before and after the reform.

These results indicate that the reform did not generate sorting or manipulation of permit mass around the spatial cutoff. More broadly, they are consistent with the theoretical argument that in a built-out environment, regulatory incidence need not appear through canonical bunching-type responses.

4.2. No broad quantity response

I next examine whether the reform produced a broad quantity response in either housing or office development. Proposition 2 predicts that when projects are organized within a fixed or quasi-fixed envelope, even a binding regulation need not generate large changes in aggregate quantities.

I begin with residential outcomes. Table 2 reports estimates for three direct measures of residential production: the number of housing permits, residential floor area created, and positive net housing change. Across all three outcomes, the estimated effects are small, negative, and statistically indistinguishable from zero at the baseline 2-km bandwidth. The reform therefore does not increase the number of residential permits, the amount of housing surface created, or net housing gains near the regulated perimeter.

This conclusion is robust. Table 12 shows no positive effect across alternative bandwidths or in the donut specification. If anything, some estimates become more negative. Figure 2 likewise shows no post-reform increase in residential quantities and no evidence of differential pre-trends.

The office-side evidence points in the same direction. Although Table 5 shows some indication of weaker net office expansion after the reform, the extensive-margin office effects are not sufficiently stable across specifications to support a simple suppression story. The panel-level estimates in Table 23 similarly reveal no statistically significant aggregate office-side response.

Taken together, these results reject a broad quantity-based interpretation of the reform. The absence of a residential supply response is especially important: tighter constraints on office expansion did not translate into additional housing production near the boundary.

4.3. Redevelopment reorganization within residential permits

The clearest effects of the reform appear instead in the organization of residential redevelopment. Proposition 3 predicts that when direct quantity adjustment is limited, regulation should operate through project redesign and reallocation within inherited structures. The residential-permit evidence strongly supports this mechanism.

Table 3 shows clear positive effects on redevelopment-related margins. At the 2-km bandwidth, treated-side residential permits become 4.2 percentage points more likely to involve works on existing structures, 3.6 percentage points more likely to involve demolition, 4.3 percentage points more likely to take a redevelopment form, and 1.9 percentage points more likely to include an extension. These are not the signatures of a housing-supply expansion. Rather, they indicate a shift toward intervention on inherited structures and toward projects requiring redesign within the existing built environment.

Figure 2 confirms the timing of these effects. Redevelopment margins remain flat before the reform and rise only afterward, which is consistent with a post-2019 reorganization of project form rather than with pre-existing differential trends. The pattern is also stable across alternative bandwidths and in the donut specification, as shown in Tables 13 and 14.

The residential-permit evidence also reveals how this reorganization occurs. Table 4 shows that, after the reform, treated-side residential permits exhibit both greater housing loss and more office-related transformed area. At the baseline bandwidth, $\log(1 + \text{Housing Loss Area})$ increases by 0.1315, while $\log(1 + \text{Office Area from Transformation})$ increases by 0.0520. By contrast, the effect on the probability that a residential permit is formally mixed is weaker and not robust across specifications.

This distinction is substantive. The strongest residential-side evidence does not come from a broad increase in formal mixed-use permits, but from a more specific pattern of within-project recomposition. Residential permits are increasingly associated with housing loss together with office-related transformation, which is exactly the pattern predicted by a model in which developers reoptimize within constrained sites rather than expand total built volume.

The robustness checks reinforce this interpretation. Tables 16 and 17 show that the effects on housing loss and office-related transformation remain stable across bandwidths and in the donut specification, whereas the mixed-permit outcome is not robust. The residential response is therefore best understood as redevelopment reorganization with cross-use reallocation inside inherited structures.

4.4. Office permits shift away from pure office expansion

The office-permit evidence points to the same mechanism from the other side of the market. Table 5 shows that treated-side office permits become more likely to be mixed and more likely to generate net housing gains after the reform. This indicates a shift away from pure office expansion and toward project forms that incorporate residential components.

This pattern is more robust than a simple decline in office activity. While the coefficient on net office addition is negative in the baseline specification, the robustness tables show that this extensive-margin effect is less stable across bandwidths and specifications than the compositional outcomes. By contrast, the rise in mixed permits and housing-generating forms remains visible in the baseline and donut specifications and fits closely with the mechanism emphasized in the paper.

The office-side evidence should therefore not be read as showing a collapse in office development. Rather, it indicates that office projects become less centered on pure office expansion and more likely to be reorganized into mixed or housing-generating forms. This is precisely the type of response one would expect when the regulatory burden falls disproportionately on large net office additions but leaves alternative redevelopment strategies comparatively less exposed.

Figure 3 supports this interpretation dynamically. The post-reform pattern is not one of abrupt disappearance of office projects, but of compositional reorientation in the way office-side redevelopment is structured.

4.5. *Office prices rise, while residential prices do not*

I next turn to transaction prices. Proposition 4 predicts that a regulation constraining office expansion should raise office prices, while residential price effects remain ambiguous in the absence of additional housing supply.

Table 6 shows a clear increase in office transaction prices per square meter on the treated side after the reform. At the baseline 2-km bandwidth, the estimated effect is positive and statistically significant. Table 20 shows that this result remains positive across alternative bandwidths. Figure 4 further indicates that the office-price response emerges only after the reform and is not preceded by differential pre-trends.

By contrast, there is no comparable evidence for residential prices. The estimated effect on housing prices is close to zero in the baseline specification, and Figure 5 shows no meaningful post-reform divergence. Table 20 likewise shows no robust positive housing-price response.

This contrast is informative. It indicates that the reform tightened constraints on the office side of the market without producing offsetting increases in housing supply. The price evidence therefore reinforces the interpretation that the policy was binding, but that its main incidence operated through redevelopment reorganization rather than net residential expansion.

4.6. *Dynamic evidence and robustness*

The event-study estimates reinforce both the timing and the interpretation of the main findings. For residential quantities, coefficients remain close to zero throughout the post-reform period, consistent with the absence of a housing-supply response. For redevelopment margins, by contrast, the estimates show no differential pre-trends and clear post-2019 increases. The same timing pattern appears for office prices, which rise only after the reform.

The placebo exercises reported in Appendix A.1 support this interpretation. When fake reform dates are assigned in 2015 or 2016, the estimated coefficients are small and statistically insignificant across the main residential and office outcomes. This reduces the concern that the baseline estimates reflect pre-existing changes in the cross-boundary discontinuity.

More generally, the main empirical pattern remains stable across bandwidth choices and in the donut specification excluding the immediate 0–500 meter ring around the boundary. The conclusions of the paper therefore do not depend on a narrow specification choice.

Taken together, the results point to a coherent mechanism. The reform does not generate a housing-supply expansion and does not produce the canonical threshold responses emphasized in standard notch settings. Instead, it changes how projects are organized within a constrained urban environment. On the residential side, permits become more redevelopment-oriented and increasingly involve housing loss together with office-related transformation. On the office side, permits become less centered on pure office expansion and more likely to include residential components. Office prices rise, while residential prices do not.

This combination of findings is difficult to reconcile with a simple quantity-based account of threshold regulation. It is more consistent with a built-out setting in which developers adjust project form and internal use composition within inherited structures. In such environments, the main incidence of regulation lies in redevelopment reorganization rather than in net production.

5. Discussion

The results point to a form of regulatory incidence that differs sharply from the canonical predictions of threshold-based models. The 2019 reform does not generate bunching responses, does not increase residential supply, and does not lead to a broad contraction in office permitting. Yet the policy is clearly binding. The adjustment instead operates through a reorganization of redevelopment within an already-built urban environment.

5.1. Economic incidence versus administrative footprint

A central implication of the findings is the distinction between the administrative footprint of regulation and its economic incidence. While the policy formally targets large office developments, the most robust responses appear in residentially classified permits. Following the reform, residential permits on the treated side become more redevelopment-intensive, more likely to involve demolition and intervention on existing structures, and more frequently associated with housing loss and office-related transformation. On the office side, the response is not a collapse in activity but a shift away from pure office expansion toward mixed and housing-generating configurations.

This divergence is not incidental. In built-out environments, development decisions are made at the level of sites that already embed multiple uses and physical constraints. As a result, a regulation that applies to one use can propagate across margins of adjustment that are only indirectly targeted. The empirical patterns documented here indicate that office-targeted regulation reshapes the composition of redevelopment more broadly, with some of the clearest effects materializing in permits that are not administratively classified as office.

5.2. Why canonical threshold predictions fail in built-out environments

These findings help explain why standard threshold predictions are weak in this setting. In canonical models, agents can adjust project size continuously and avoid regulatory thresholds at relatively low cost, leading to bunching or project suppression. Such margins are less relevant in mature metropolitan markets where land is scarce and development primarily takes the form of redevelopment within inherited structures.

In this environment, reducing the office component of a project is not a marginal adjustment. The existing built envelope constrains feasible redesign, and reallocating space across uses may require demolition, structural modification, or loss of usable surface. The relevant margin of adjustment is therefore not the number of projects or total built volume, but the internal organization of redevelopment.

The empirical results are consistent with this mechanism. The reform does not increase housing supply, but it induces more redevelopment activity within residential permits, greater housing loss, more office-related transformation, compositional shifts in office permits, and higher office prices. These patterns are difficult to reconcile with models in which regulation primarily affects project entry or size, but are natural in a framework where developers reoptimize within constrained sites.

5.3. External validity and limitations

The scope of the results is shaped by the institutional and spatial context. First, the analysis identifies reduced-form effects near a sharp regulatory boundary. While this design provides credible causal estimates, it does not directly recover the underlying structural parameters governing redevelopment decisions.

Second, the findings are specific to a dense metropolitan environment where redevelopment is the dominant margin of adjustment. In less constrained settings with more available land, threshold-based regulation may still generate the canonical responses emphasized in the literature, including bunching or outward expansion.

Third, the strength of the results varies across outcomes. The absence of residential quantity effects, the increase in redevelopment margins, the rise in cross-use reallocation, and the increase in office prices are robust features of the data. By contrast, some extensive-margin office outcomes are less stable across specifications and should be interpreted with caution.

These limitations clarify rather than weaken the contribution. The evidence shows that in built-out urban markets, the primary incidence of threshold-based regulation lies not in aggregate quantities, but in how redevelopment is structured across uses within constrained sites.

6. Conclusion

This paper studies how threshold-based land-use regulation operates in a built-out metropolitan environment, where development occurs primarily through redevelopment rather than greenfield expansion. Focusing on the 2019 tightening of the Paris Region's approval procedure for large office projects, I exploit a sharp regulatory boundary using a border-based difference-in-discontinuities design and combine detailed permit-level data with transaction-level price information.

The results reject a quantity-based view of threshold regulation. The reform does not generate bunching, does not increase residential supply, and does not lead to a broad contraction in office permitting. Yet the policy is clearly binding.

Instead, the evidence points to a reorganization of redevelopment. Residential permits become more likely to involve demolition, intervention on existing structures, and redevelopment-oriented project forms. At the same time, they increasingly exhibit housing loss together with office-related transformation. On the office side, projects shift away from pure office expansion toward mixed and housing-generating configurations. Consistent with a tightening of office-side constraints, office prices increase following the reform, while residential prices do not.

These findings highlight a disconnect between the administrative target of regulation and its economic incidence. Although the policy directly applies to large office developments, its primary effects emerge through changes in the composition and organization of redevelopment within constrained sites.

More broadly, the paper contributes to the understanding of land-use regulation in mature urban environments. In settings characterized by scarce land and durable capital, the margins emphasized in standard models—project size, entry, or bunching—may be of secondary importance. Instead, regulatory distortions operate through the internal reallocation of space across uses within existing structures.

This perspective has direct implications for policy design. Regulations targeting specific uses may fail to produce intended aggregate outcomes when redevelopment constraints bind. Accounting for the structure of the existing built environment is therefore central to predicting the effects of land-use policy in dense metropolitan markets.

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Tables

Table 1. Density discontinuity tests around the boundary (McCrary-type)

Market	Sample	N	Robust T	Robust p-value
Office / tertiary permits				
	Pre-reform ($t < 2019$)	1,061	0.940	0.347
	Post-reform ($t \geq 2019$)	1,020	-0.703	0.482
Residential permits				
	Pre-reform ($t < 2019$)	1,763	0.571	0.568
	Post-reform ($t \geq 2019$)	1,360	-0.098	0.922

Notes: McCrary-type density discontinuity tests based on local polynomial density estimation of the running variable (signed distance to the boundary). Tests are conducted within the baseline estimation window (±1 km). The null hypothesis is continuity of the density of observations at the cutoff ($D = 0$). Reported statistics correspond to robust jackknife-based inference.

Table 2. Residential Quantity Outcomes

	log(1 + Housing Permits)	log(1 + Housing Area Created)	log(1 + Positive Net Housing Change)
Treated × Post	-0.0148	-0.0512	-0.0807
	(0.0455)	(0.1661)	(0.1668)
Observations		5,137	
R^2	0.713	0.606	0.604
Within R^2	0.00005	0.00003	0.00008
Pair-side FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Dept × Year FE	Yes	Yes	Yes

Notes: Border-based difference-in-discontinuities estimates using a 2-km bandwidth.
Standard errors are clustered at the pair-side level. No statistically significant effects are detected across residential quantity outcomes.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 3. Redevelopment Margins within Residential Permits

	Works on Existing	Demolition	Redevelopment Form	Extension
Treated × Post	0.0420**	0.0364**	0.0430**	0.0193*
	(0.0156)	(0.0127)	(0.0156)	(0.0111)
Observations		19,821		
R^2	0.304	0.220	0.304	0.134
Within R^2	0.00134	0.00060	0.00139	0.00063
Pair-side FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Dept × Year FE	Yes	Yes	Yes	Yes

Notes: Permit-level estimates within 2 km of the boundary. Standard errors are clustered at the pair-side level. The reform increases redevelopment-related activity within residential permits.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 4. Cross-Use Reallocation within Residential Permits

	log(1 + Housing Loss Area)	log(1 + Office Area from Transformation)	Mixed Permit
Treated × Post	0.1315**	0.0520**	0.0162*
	(0.0587)	(0.0208)	(0.0080)
Observations		19,821	
R^2	0.148	0.140	0.163
Within R^2	0.00029	0.00028	0.00084
Pair-side FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Dept × Year FE	Yes	Yes	Yes

Notes: Permit-level estimates within 2 km of the boundary. Standard errors are clustered at the pair-side level. The reform increases housing loss and office-related transformation, while the effect on the probability of a formally mixed permit is weaker.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5. Office Permits: Main Outcomes

	Net Office Addition	log(1 + Positive Net Office Addition)	Mixed Permit	Net Housing Gain
Treated × Post	-0.1200*	-1.0580**	0.1620**	0.1680**
	(0.0698)	(0.5322)	(0.0808)	(0.0789)
Observations		2,252		
R^2	0.212	0.214	0.167	0.153
Within R^2	0.0056	0.0067	0.0050	0.0045
Pair-side FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Dept × Year FE	Yes	Yes	Yes	Yes

Notes: Permit-level difference-in-discontinuities estimates within 2 km of the boundary. The dependent variable *mixed permit* equals one if a permit authorizes both office and residential floor area within the same project, and zero otherwise. Authorized floor area is measured as the sum of newly created surface and surface resulting from transformation. The sample is restricted to permits whose main declared use is office. All regressions include pair-by-side, year, and department-by-year fixed effects. Standard errors are clustered at the pair-side level. The reform does not robustly increase office additions, but shifts office permits toward mixed-use and housing-generating forms.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 6. Transaction Price Effects

	Housing Prices	Office Prices
Treated × Post	-0.0189	0.3551**
	(0.0155)	(0.1565)
Observations	144,545	20,763
R^2	0.712	0.549
Within R^2	0.607	0.239
Pair-side FE	Yes	Yes
Year FE	Yes	Yes
Dept × Year FE	Yes	Yes

Notes: This table reports border-based difference-in-discontinuities estimates of the 2019 reform on transaction prices per square meter using a 2-km bandwidth. The dependent variable is the logarithm of transaction price per square meter. All specifications include pair-side, year, and department-by-year fixed effects. Standard errors clustered at the pair-side level are reported in parentheses.
The reform is associated with a significant increase in office prices, while housing prices do not exhibit a statistically significant response.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 7. Pre-reform differences across the regulatory boundary

	500 m			1000 m			2000 m			3000 m		
	Diff.	SE	p-val	Diff.	SE	p-val	Diff.	SE	p-val	Diff.	SE	p-val
Panel A. Office / tertiary permit environment												
Permits (count)	1.08	0.20	0.000	1.76	0.31	0.000	2.24	0.39	0.000	2.54	0.39	0.000
Permits with tertiary created ($\geq$ 1,000 m^2)	0.085	0.092	0.360	0.386	0.079	0.000	0.507	0.100	0.000	0.637	0.100	0.000
Tertiary floor space created (m^2)	5,924	2,037	0.005	7,219	1,904	0.000	8,842	1,812	0.000	9,865	1,815	0.000
Office floor space created (m^2)	1,544	674	0.025	1,478	596	0.015	1,979	602	0.001	1,967	560	0.001
Share of permits with office $\geq$ 10,000 m^2	0.007	0.003	0.037	0.005	0.002	0.008	0.003	0.001	0.002	0.003	0.001	0.001
Net tertiary floor space (m^2)	4,632	1,831	0.013	5,820	1,754	0.001	7,337	1,578	0.000	8,359	1,620	0.000
Panel B. Housing permit environment												
Housing permits (count)	0.23	0.31	0.459	1.24	0.44	0.005	2.04	0.66	0.002	3.03	0.72	0.000
Housing floor space created (m^2)	1,790	389	0.000	3,109	585	0.000	4,128	677	0.000	4,660	667	0.000
Number of dwellings created	32.8	7.13	0.000	50.1	9.39	0.000	66.8	11.2	0.000	74.4	11.0	0.000
Share of mixed-use among housing permits	0.099	0.025	0.000	0.081	0.021	0.000	0.056	0.011	0.000	0.059	0.010	0.000
Share of collective dwellings	0.271	0.047	0.000	0.214	0.038	0.000	0.233	0.031	0.000	0.267	0.028	0.000
Net housing floor space (m^2)	1,710	381	0.000	2,993	573	0.000	3,980	659	0.000	4,497	648	0.000

Notes: The table reports treated-minus-control differences in pre-reform means for municipalities located within alternative bandwidths around the regulatory boundary. The sample period is 2012–2018. Standard errors correspond to Welch two-sample t-tests.

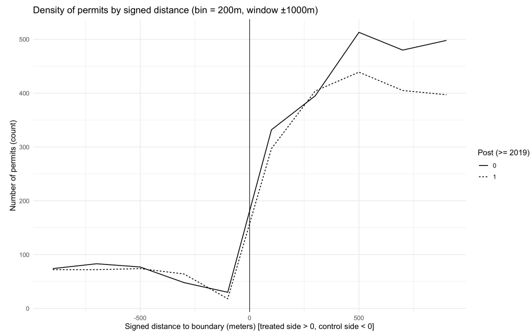
Table 8. Pre-reform differences across the regulatory boundary: transaction environment

	500 m			1000 m			2000 m			3000 m		
	Diff.	SE	p-val	Diff.	SE	p-val	Diff.	SE	p-val	Diff.	SE	p-val
Panel A. Non-residential transactions (DV3F)												
Mean log(price)	-0.235	0.421	0.580	-0.313	0.283	0.273	-0.344	0.270	0.206	-0.283	0.238	0.239
Mean price per m^2	-1541.209	592.878	0.017	-2297.787	385.364	0.000	-1890.021	510.356	0.001	-1656.980	398.405	0.000
Mean built surface (m^2)	1704.979	3024.256	0.578	1778.992	1598.613	0.271	1143.462	1176.395	0.334	1227.602	1010.532	0.228
Mean number of floors	-4.789	1.356	0.003	-3.480	0.480	0.000	-2.711	0.484	0.000	-2.424	0.451	0.000
Share construction period = NR	-0.026	0.071	0.717	-0.012	0.035	0.741	0.013	0.028	0.654	0.022	0.023	0.349
Mean distance to boundary (m)	-141.295	31.787	0.000	-90.792	48.333	0.065	-248.691	74.274	0.001	-153.169	98.210	0.122
Number of transactions (frontier-side)	31.333	30.509	0.314	23.991	24.083	0.323	-77.060	34.603	0.030	-79.310	33.315	0.020
Panel B. Housing transactions (DV3F)												
Mean log(price)	0.009	0.119	0.936	-0.006	0.061	0.922	0.021	0.048	0.668	0.025	0.048	0.600
Mean price per m^2	-1072.893	378.513	0.006	-690.664	194.490	0.000	-530.214	154.209	0.001	-510.769	150.557	0.001
Mean dwelling surface (m^2)	289.419	285.697	0.313	278.187	112.539	0.014	282.471	103.097	0.007	275.674	95.909	0.004
Mean number of floors	-0.899	0.500	0.075	-0.276	0.311	0.375	-0.166	0.234	0.477	-0.126	0.222	0.570
Mean distance to boundary (m)	-19.713	14.706	0.183	-48.005	23.022	0.038	-6.746	41.106	0.870	121.100	57.501	0.036
Number of transactions (frontier-side)	0.878	44.086	0.984	-165.874	88.882	0.063	-416.172	162.156	0.011	-429.429	171.586	0.013

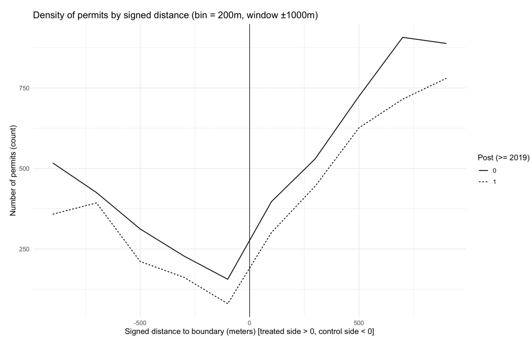
Notes: The table reports treated-minus-control differences in transaction characteristics using DV3F data over the pre-reform period 2012–2018.
Standard errors correspond to Welch two-sample t-tests.

Figures

Figure 1. Density of Permit Observations Around the Approval Boundary



(Panel A) Office / tertiary permits



(Panel B) Residential permits

Notes: The figure plots binned counts of permit observations as a function of signed distance to the approval boundary (treated side > 0, control side < 0), using 200-meter bins within a $\pm 3{,}000$ meter window. Solid lines correspond to the pre-reform period and dashed lines to the post-reform period (reform year = 2019). The outcome shown is the number of observations (permits), not permit characteristics.

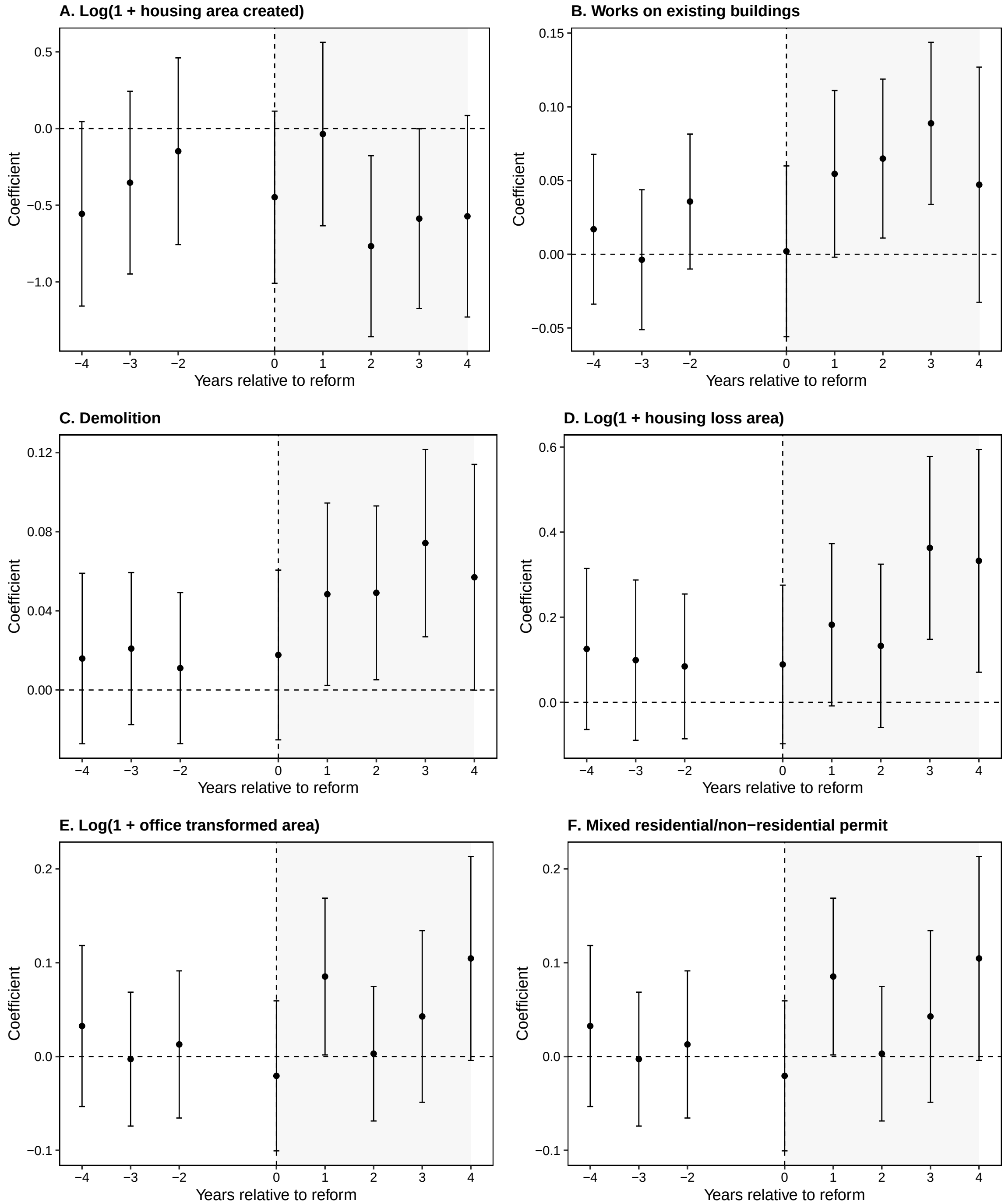
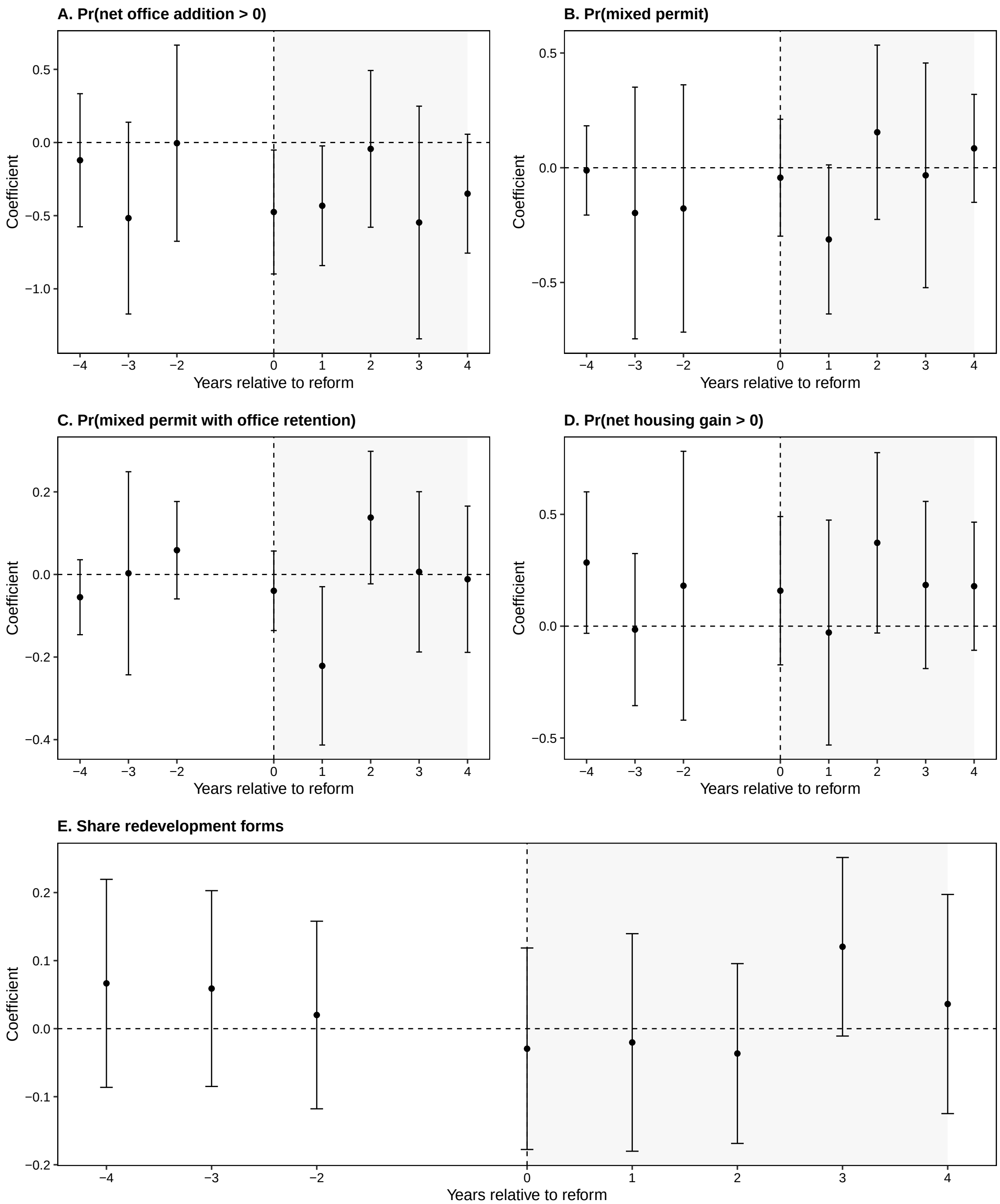


Figure 2. Dynamic Effects on Residential Quantity and Redevelopment Margins

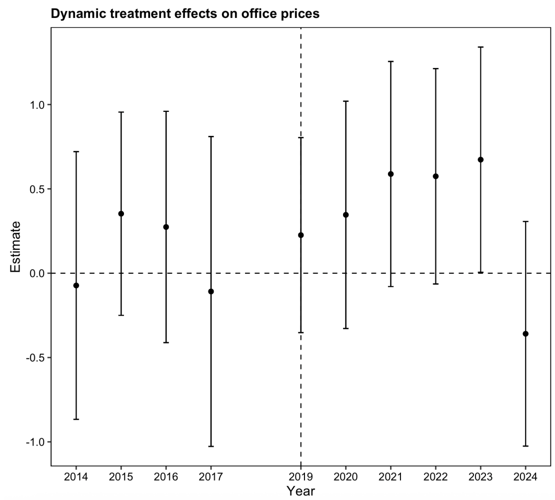
Notes: The figure reports event-study estimates from the border-based difference-in-discontinuities design using the baseline 2-km bandwidth. The omitted year is $t = -1$. All specifications include pair-side fixed effects, year fixed effects, and department-by-year fixed effects. Standard errors are clustered at the pair-side level, and 95% confidence intervals are shown. The estimates indicate no discernible increase in residential area created after the reform, but clear post-reform increases in works on existing buildings, demolition, and housing loss area.

Figure 3. Dynamic Effects on Office Expansion, Mixity, and Redevelopment



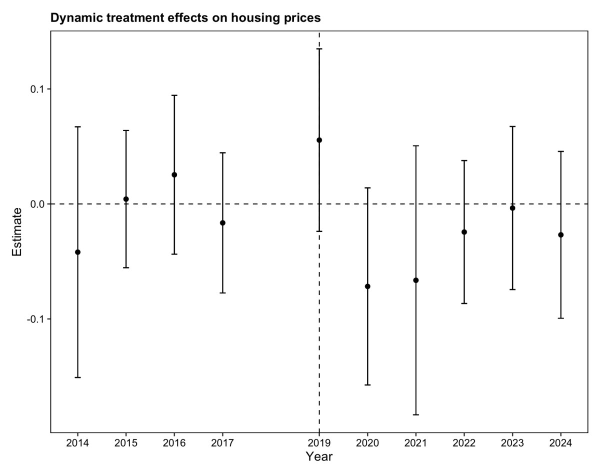
Notes: The figure reports event-study estimates from the border-based difference-in-discontinuities design using the baseline 2-km bandwidth. Coefficients are shown relative to year −1, omitted as the reference period. Vertical bars denote 95% confidence intervals with standard errors clustered at the pair-side level. Panel A examines the extensive margin of net office additions. Panels B and C trace changes in within-permit mixity and office retention. Panel D reports the extensive margin of net housing gains within office permits. Panel E reports the share of redevelopment-type permits at the panel level. The shaded area indicates the post-reform period.

Figure 4. Event-Study Evidence on Office Prices



Notes: The figure reports dynamic border-based difference-in-discontinuities estimates for office transaction prices. The dependent variable is the logarithm of office transaction price per square meter. All specifications include border-pair × side fixed effects and year fixed effects.

Figure 5. No Event-Study Evidence of Housing Price Effects



Notes: The figure reports dynamic border-based difference-in-discontinuities estimates corresponding to Table 6. The dependent variable is the logarithm of residential transaction price per square meter. All specifications include border-pair × side fixed effects and year fixed effects.

A. Identification and Robustness

A.1. Placebo Policy Tests

Table 9. Placebo Treatment Effects: Fake Reform Year 2015

	log(1 + Housing Area Created)	Works on Existing	Demolition	log(1 + Housing Loss Area)	log(1 + Office Area from Transformation)
Treated × Placebo Post	0.2537	0.0068	0.0278	0.0340	0.0354
	(0.2396)	(0.0224)	(0.0218)	(0.0971)	(0.0363)
Observations	5,137	19,821	19,821	19,821	19,821
Pair-side FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Dept × Year FE	Yes	Yes	Yes	Yes	Yes

Notes: Placebo estimates obtained by assigning the reform year to 2015. All regressions include pair-side fixed effects, year fixed effects, and department-by-year fixed effects. Standard errors are clustered at the pair-side level.
None of the placebo coefficients is statistically significant.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 10. Placebo Treatment Effects: Fake Reform Year 2016

	log(1 + Housing Area Created)	Works on Existing	Demolition	log(1 + Housing Loss Area)	log(1 + Office Area from Transformation)
Treated × Placebo Post	0.2627	0.0158	0.0221	0.0326	0.0228
	(0.1871)	(0.0179)	(0.0153)	(0.0695)	(0.0269)
Observations	5,137	19,821	19,821	19,821	19,821
Pair-side FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Dept × Year FE	Yes	Yes	Yes	Yes	Yes

Notes: Placebo estimates obtained by assigning the reform year to 2016. All regressions include pair-side fixed effects, year fixed effects, and department-by-year fixed effects.
Standard errors are clustered at the pair-side level. None of the placebo coefficients is statistically significant.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 11. Placebo Difference-in-Discontinuities Tests for Office-Permit Outcomes

	Placebo Reform: 2015				Placebo Reform: 2016			
	Log office add	Mixed permit	Housing gain	Transform no ext	Log office add	Mixed permit	Housing gain	Transform no ext
Treated × Post (placebo)	-1.257	0.2273	0.2256	0.0399	-0.4345	0.0975	0.0936	0.0681
	(1.261)	(0.3100)	(0.3087)	(0.1539)	(0.9098)	(0.1499)	(0.1488)	(0.0864)
Observations	2,252				2,252			
Pair-side FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dept × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Placebo estimates for office-permit outcomes using fake reform years. The specification mirrors the main model.
Standard errors are clustered at the pair-side level. None of the placebo coefficients is statistically significant.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

A.2. Robustness to Bandwidth and Donut Specifications

Table 12. Residential Quantity Outcomes: Bandwidth and Donut Robustness

	log(1 + Housing Permits)				log(1 + Housing Area Created)				log(1 + Positive Net Housing Change)			
	1 km	2 km	2 km donut	3 km	1 km	2 km	2 km donut	3 km	1 km	2 km	2 km donut	3 km
Treated × Post	0.0618	-0.0148	-0.0755	-0.0321	0.0784	-0.0512	-0.3304*	-0.0228	0.0736	-0.0807	-0.3819**	-0.0489
	(0.0468)	(0.0455)	(0.0467)	(0.0434)	(0.1837)	(0.1661)	(0.1753)	(0.1531)	(0.1846)	(0.1668)	(0.1757)	(0.1528)
Observations	3,982	5,137	4,851	5,445	3,982	5,137	4,851	5,445	3,982	5,137	4,851	5,445
Pair-side FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dept × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robustness checks for residential quantity outcomes across alternative bandwidths and a donut specification excluding the 0–500 meter ring around the boundary.
Standard errors are clustered at the pair-side level. No positive residential quantity response is detected.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 13. Redevelopment Margins: Bandwidth and Donut Robustness

	Works on Existing Structures				Demolition			
	1 km	2 km	2 km donut	3 km	1 km	2 km	2 km donut	3 km
Treated × Post	0.0686***	0.0420**	0.0457**	0.0356**	0.0207**	0.0364**	0.0389**	0.0360**
	(0.0201)	(0.0156)	(0.0170)	(0.0145)	(0.0101)	(0.0127)	(0.0138)	(0.0114)
Observations	9,171	19,821	16,756	25,138	9,171	19,821	16,756	25,138
Pair-side FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dept × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robustness checks for redevelopment-related margins across alternative bandwidths and the donut specification. Standard errors are clustered at the pair-side level.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 14. Redevelopment Margins: Additional Robustness

	Redevelopment Form				Extension			
	1 km	2 km	2 km donut	3 km	1 km	2 km	2 km donut	3 km
Treated × Post	0.0692***	0.0430**	0.0465**	0.0364**	0.0413**	0.0193*	0.0192	0.0174*
	(0.0201)	(0.0156)	(0.0170)	(0.0145)	(0.0156)	(0.0111)	(0.0120)	(0.0098)
Observations	9,171	19,821	16,756	25,138	9,171	19,821	16,756	25,138
Pair-side FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dept × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robustness checks for redevelopment-related margins across alternative bandwidths and the donut specification. Standard errors are clustered at the pair-side level. The positive effects on redevelopment margins remain stable across specifications.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 15. Mixed Housing/Non-Residential Housing Permits: Bandwidth and Donut Robustness

	1 km	2 km	2 km donut	3 km
Treated × Post	0.0229**	0.0163*	0.0216**	0.0127*
	(0.0097)	(0.0085)	(0.0105)	(0.0071)
Observations	15,162	19,821	12,097	25,138
Pair-side FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Dept × Year FE	Yes	Yes	Yes	Yes

Notes: This table reports robustness estimates for the probability that a residential permit includes both residential and non-residential uses. Columns correspond to alternative bandwidths (1 km, 2 km, 3 km) and a donut specification excluding the 0–500 meter ring around the boundary. All regressions include pair-side, year, and department-by-year fixed effects. Standard errors are clustered at the pair-side level.
The positive effect on mixed permits is strongest at shorter distances and remains statistically significant in the donut specification, although it becomes weaker at larger bandwidths.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 16. Cross-Use Reallocation: Housing Loss and Office Transformation

	$\log(1+\text{Housing Loss Area})$				$\log(1+\text{Office Area from Transformation})$			
	1 km	2 km	2 km donut	3 km	1 km	2 km	2 km donut	3 km
Treated × Post	0.0761*	0.1315**	0.1474**	0.1251**	0.0421*	0.0520**	0.0479**	0.0416**
	(0.0402)	(0.0587)	(0.0637)	(0.0521)	(0.0235)	(0.0208)	(0.0230)	(0.0176)
Observations	9,171	19,821	16,756	25,138	9,171	19,821	16,756	25,138
Pair-side FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dept × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robustness checks for cross-use reallocation outcomes across bandwidths and the donut specification. Standard errors are clustered at the pair-side level. The effects on housing loss and office-related transformation remain stable.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 17. Cross-Use Reallocation: Mixed Housing/Non-Residential Permits

	1 km	2 km	2 km donut	3 km
Treated × Post	-0.0022	0.0083	0.0069	0.0065
	(0.0122)	(0.0073)	(0.0077)	(0.0060)
Observations	9,171	19,821	16,756	25,138
Pair-side FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Dept × Year FE	Yes	Yes	Yes	Yes

Notes: Robustness checks for the probability of mixed housing/non-residential permits across bandwidths and the donut specification. Standard errors are clustered at the pair-side level. No statistically significant effect is detected.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 18. Office-Permit Outcomes: Net Office Addition and Mixed Permits

	Net Office Addition				Mixed Permit			
	1 km	2 km	2 km donut	3 km	1 km	2 km	2 km donut	3 km
Treated × Post	0.2971	-0.1200*	-0.0979	-0.1024	0.4830**	0.1620**	0.1873**	0.0423
	(0.2583)	(0.0698)	(0.0733)	(0.0626)	(0.2420)	(0.0808)	(0.0916)	(0.0976)
Observations	1,127	2,252	1,768	3,271	1,127	2,252	1,768	3,271
Pair-side FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dept × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robustness checks for office-permit outcomes across bandwidths and the donut specification. Standard errors are clustered at the pair-side level.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 19. Office-Permit Outcomes: Housing Gains within Office Permits

	1 km	2 km	2 km donut	3 km
Treated × Post	0.4752*	0.1680**	0.1938**	0.0415*
	(0.2432)	(0.0789)	(0.0890)	(0.0255)
Observations	1,127	2,252	1,768	3,271
Pair-side FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Dept × Year FE	Yes	Yes	Yes	Yes

Notes: Robustness checks for net housing gains within office permits across bandwidths and the donut specification. Standard errors are clustered at the pair-side level. The increase is strongest in the baseline and donut specifications.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 20. Transaction Prices: Bandwidth Robustness

	Housing Prices			Office Prices		
	1 km	2 km	3 km	1 km	2 km	3 km
Treated × Post	-0.0132	-0.0189	-0.0270**	0.2115*	0.3551**	0.3020**
	(0.0215)	(0.0155)	(0.0136)	(0.1194)	(0.1565)	(0.1390)
Observations	70,674	144,545	182,535	12,300	20,763	24,384
R^2	0.717	0.712	0.708	0.658	0.549	0.542
Pair-side FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Dept × Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports border-based difference-in-discontinuities estimates of the 2019 reform on transaction prices per square meter across alternative bandwidths. The dependent variable is the logarithm of transaction price per square meter. All specifications include pair-side fixed effects, year fixed effects, and department-by-year fixed effects. Standard errors clustered at the pair-side level are reported in parentheses.
Office prices increase significantly across specifications, while housing prices show no robust positive response and become negative at larger bandwidths.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

A.3. Additional Permit-Level Decompositions

Table 21. Detailed Development Forms within Residential Permits

	1 km	2 km	3 km
Transformation without extension			
Treated × Post	0.0199*	0.0143*	0.0119
	(0.0111)	(0.0083)	(0.0086)
Transformation with extension			
Treated × Post	0.0073	0.0042	0.0012
	(0.0106)	(0.0073)	(0.0061)
Transformation with decrease			
Treated × Post	0.0089*	0.0086***	0.0066**
	(0.0051)	(0.0033)	(0.0029)
Extension without transformation			
Treated × Post	0.0340**	0.0151*	0.0162**
	(0.0140)	(0.0089)	(0.0081)
Decrease without transformation			
Treated × Post	-0.0009	0.0008	0.0005
	(0.0015)	(0.0011)	(0.0009)
Observations	9,171	19,821	25,138
Pair-side FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Dept × Year FE	Yes	Yes	Yes

Notes: Standard errors are clustered at the pair-side level. The residential response is concentrated in transformation-driven and extension margins.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 22. Detailed Development Forms within Office Permits

	Baseline (2 km)	Donut (2 km)
Transformation without extension		
Treated × Post	0.1752**	0.1704**
	(0.0708)	(0.0775)
Transformation with extension		
Treated × Post	0.0508	0.0114
	(0.0900)	(0.1022)
Transformation with decrease		
Treated × Post	-0.0177	-0.0430
	(0.0403)	(0.0601)
Extension without transformation		
Treated × Post	-0.2666	-0.2848
	(0.2127)	(0.2324)
Decrease without transformation		
Treated × Post	0.0313	0.0230
	(0.0277)	(0.0275)
Observations	2,252	1,768
Pair-side FE	Yes	Yes
Year FE	Yes	Yes
Dept × Year FE	Yes	Yes

Notes: Standard errors are clustered at the pair-side level. The office-side response is concentrated in transformation without extension.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

A.4. Aggregate Office Outcomes

Table 23. Panel-Level Office Outcomes

	log(1 + Total net office addition)	Share mixed permit	Share mixed with office retention	log(1 + Total net housing gain)	Share redevelopment form
Treated × Post	-0.0722	0.0192	0.0056	0.0708	-0.0117
	(0.1891)	(0.0124)	(0.0092)	(0.0574)	(0.0291)
Observations			2,079		
R^2	0.529	0.190	0.226	0.407	0.463
Within R^2	0.00006	0.00138	0.00018	0.00018	0.00007
Pair-side FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Dept × Year FE	Yes	Yes	Yes	Yes	Yes

Notes: Pair-side-year panel estimates. Standard errors are clustered at the pair-side level. No statistically significant aggregate office-side effects are detected.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

A.5. Event-Study Robustness

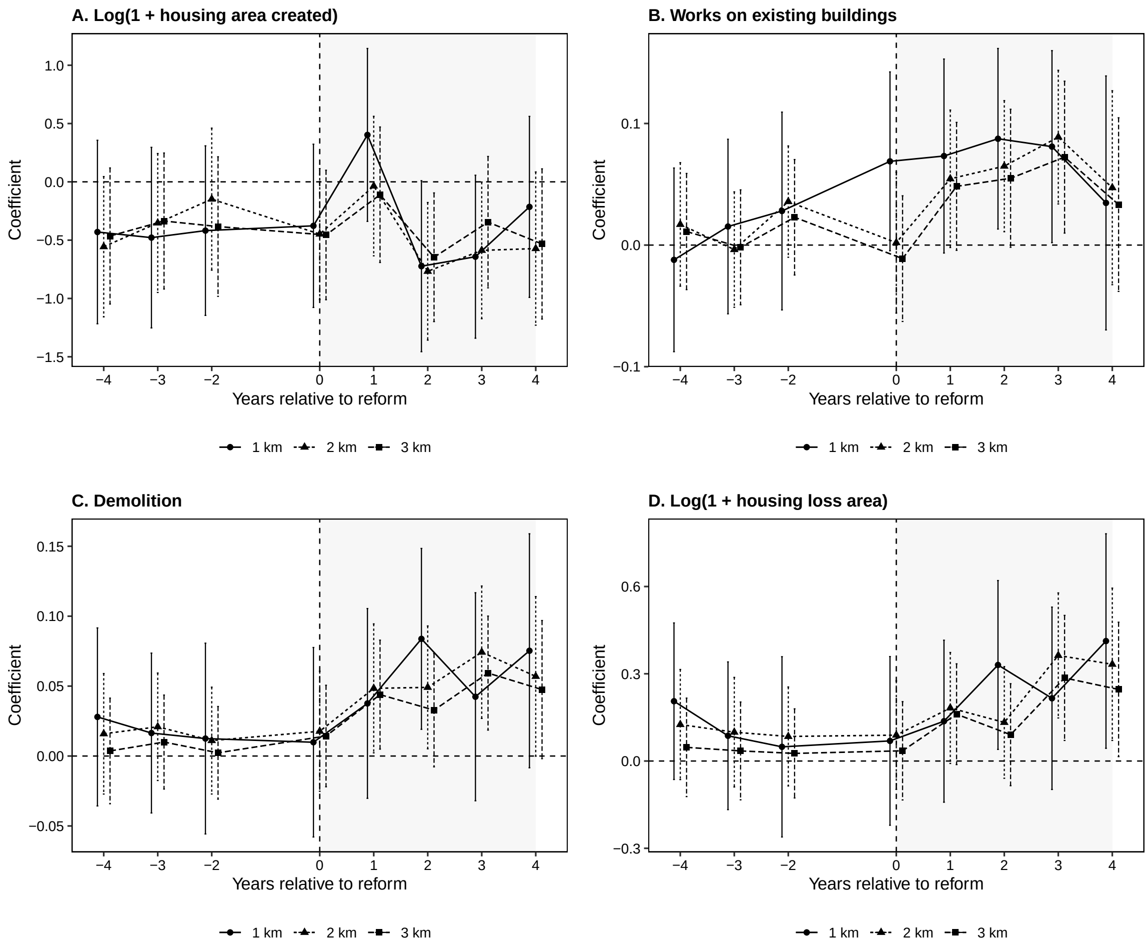


Figure 6. Event-Study Robustness to Alternative Bandwidths

Notes: Event-study estimates for the main residential outcomes using 1-km, 2-km, and 3-km bandwidths. The omitted year is $t = -1$. All specifications include pair-side fixed effects, year fixed effects, and department-by-year fixed effects. Standard errors are clustered at the pair-side level; 95% confidence intervals are shown.

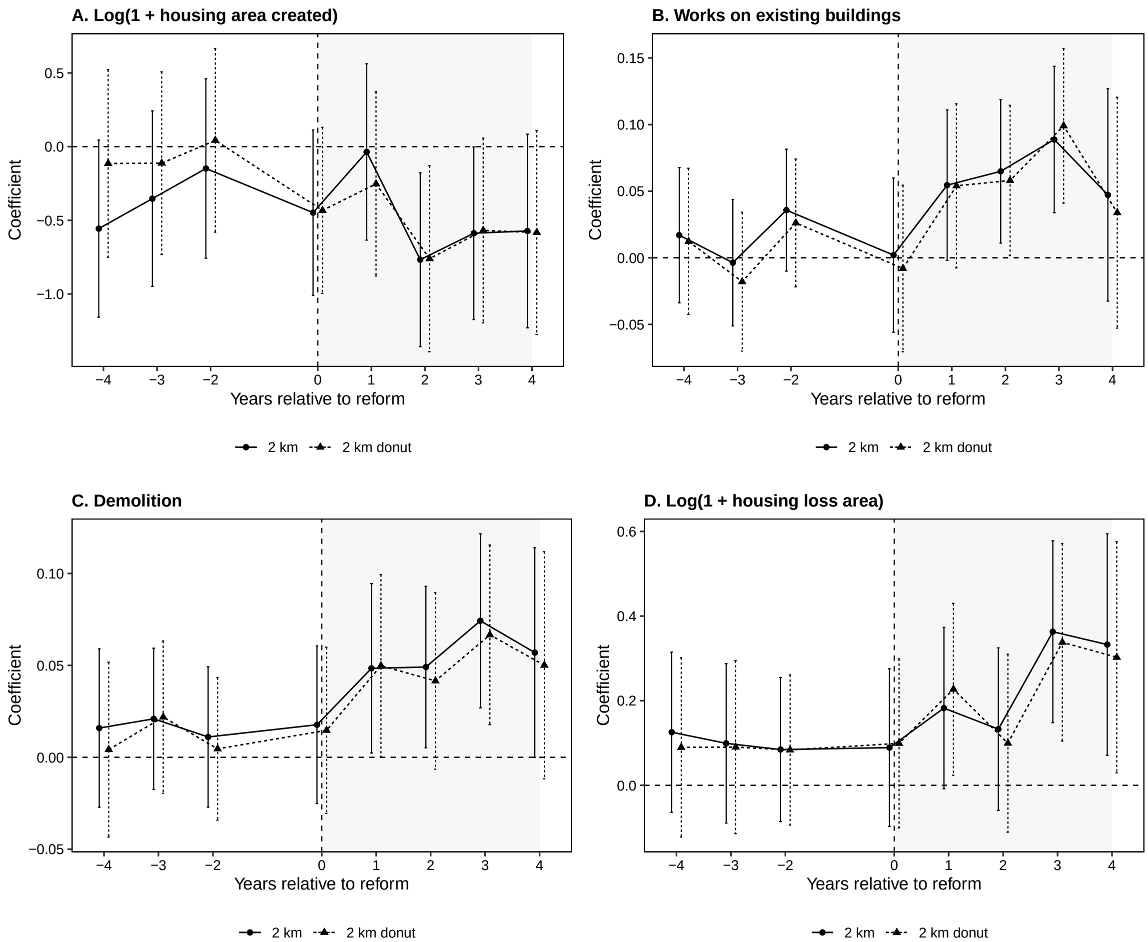


Figure 7. Event-Study Robustness to the Donut Specification

Notes: Comparison of baseline 2-km event-study estimates with a donut specification excluding permits within 500 meters of the boundary. The omitted year is $t = -1$. All specifications include pair-side fixed effects, year fixed effects, and department-by-year fixed effects. Standard errors are clustered at the pair-side level; 95% confidence intervals are shown.

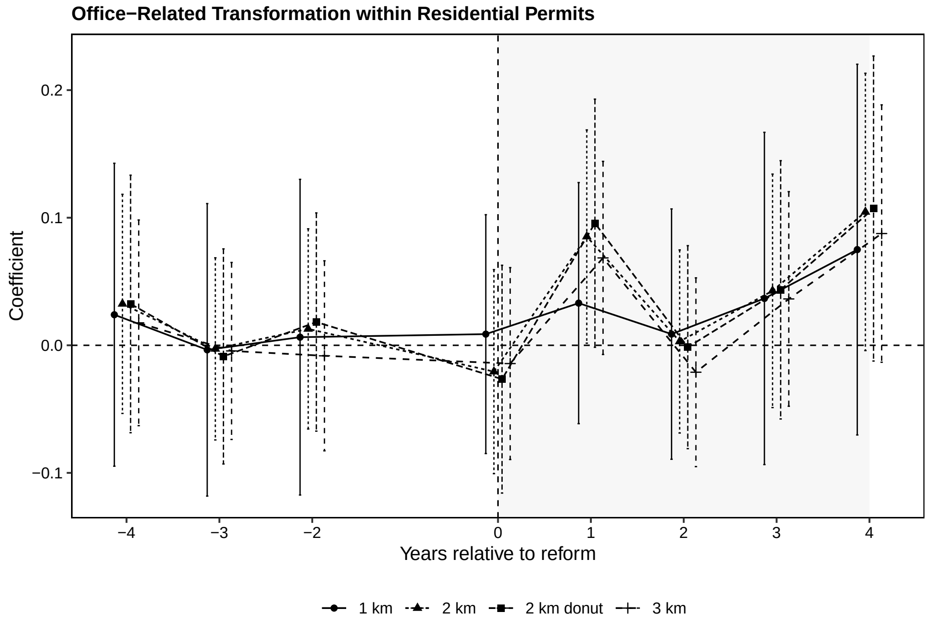


Figure 8. Dynamic Effects on Office-Related Transformation within Residential Permits
Notes: Event-study estimates for office-related transformed area within residential permits across alternative bandwidths and the donut specification. The omitted year is $t = -1$. All specifications include pair-side fixed effects, year fixed effects, and department-by-year fixed effects. Standard errors are clustered at the pair-side level; 95% confidence intervals are shown.

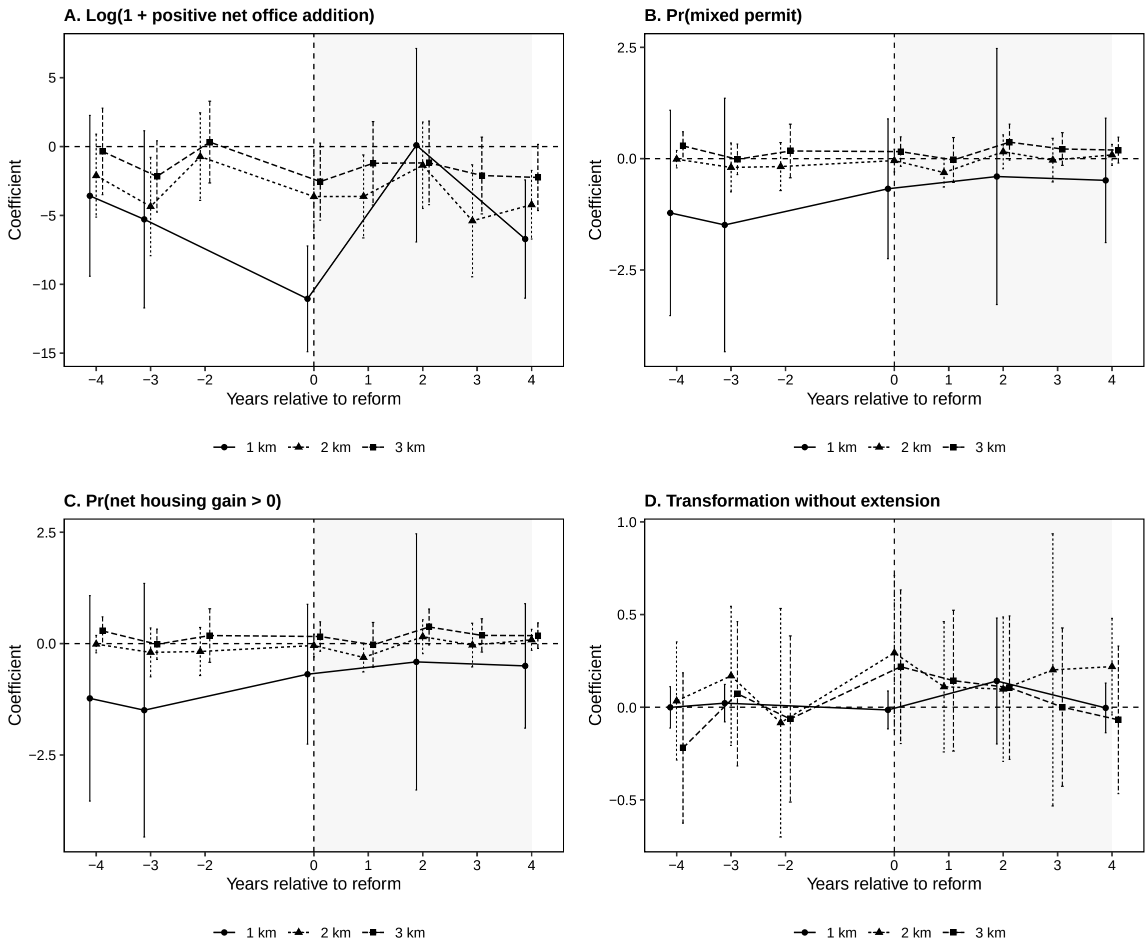


Figure 9. Event-Study Robustness for Office-Permit Outcomes: Alternative Bandwidths

Notes: Event-study estimates for the main office-permit outcomes using 1-km, 2-km, and 3-km bandwidths. The omitted year is $t = -1$. All specifications include pair-side fixed effects, year fixed effects, and department-by-year fixed effects. Standard errors are clustered at the pair-side level; 95% confidence intervals are shown.

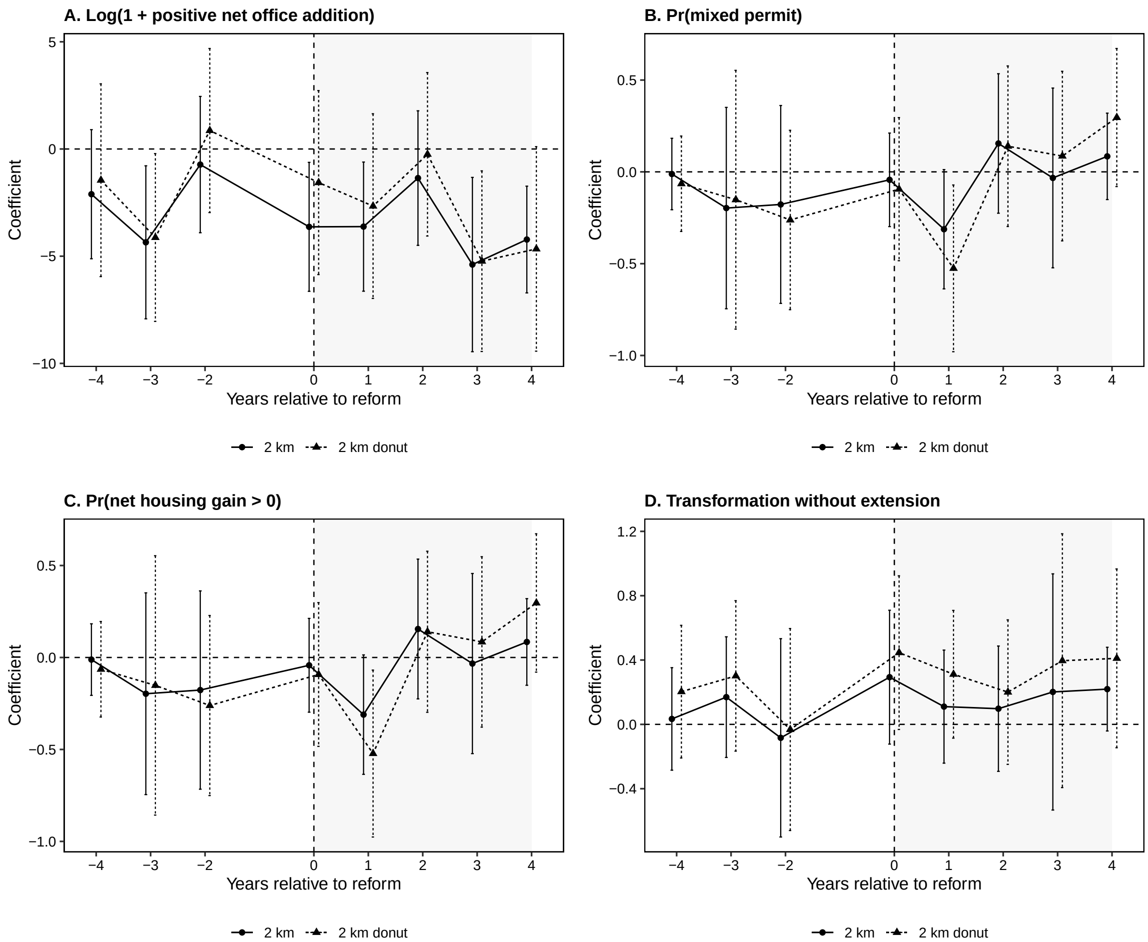
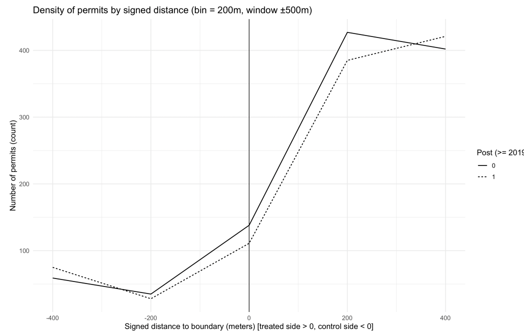


Figure 10. Event-Study Robustness for Office-Permit Outcomes: Donut Specification

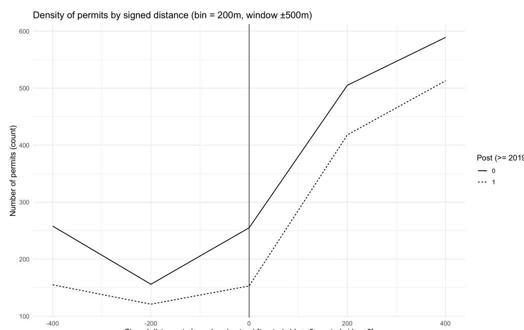
Notes: Comparison of baseline 2-km office event-study estimates with a donut specification excluding permits within 500 meters of the boundary. The omitted year is $t = -1$. All specifications include pair-side fixed effects, year fixed effects, and department-by-year fixed effects. Standard errors are clustered at the pair-side level; 95% confidence intervals are shown.

B. Density and Sorting Checks

Figure 11. Density of Permit Observations Around the Approval Boundary



Panel A. Office permits



Panel B. Residential permits

Notes: The figure plots binned counts of permit observations as a function of signed distance to the approval boundary, where the treated side is positive and the control side is negative. Counts are computed using 200-meter bins within a ±3,000 meter window. Solid lines correspond to the pre-reform period and dashed lines to the post-reform period. The outcome shown is the number of permit observations rather than permit characteristics.

C. Institutional Details on the *Procédure d'Agrément*

This appendix provides additional details on the regulatory framework governing large office developments in the Paris Region and clarifies the mapping between institutional rules and the empirical design.

C.1. Regulatory scope and thresholds

The *procédure d'agrément* applies to office and tertiary developments located within a predefined perimeter covering the most employment-intensive areas of the Paris Region. For sufficiently large projects, accreditation must be obtained prior to the issuance of a building permit.

The 2018 reform reinforced regulatory requirements for projects involving net additions to office floor space. Let Δb denote the increase in office surface relative to the pre-existing structure. The regulatory burden depends on the magnitude of Δb and becomes substantially more stringent once project size exceeds specific thresholds.

Table 24. Simplified Mapping of Regulatory Thresholds and Requirements

Project type	Net office increase (Δb)	Threshold range	Regulatory requirement
Small projects	$\Delta b < 1{,}000$ m^2	Below threshold	No or minimal constraint
Intermediate projects	$\Delta b \geq 1{,}000$ m^2	Moderate	Case-by-case assessment
Large projects	$\Delta b \geq 10{,}000$ m^2	Binding	Residential compensation required
Very large projects	$\Delta b \geq 20{,}000$ m^2	Strongly binding	Compensation up to multiples of office surface

For large and very large projects, residential compensation is expressed in floor space and may include minimum shares of social housing. In some cases, compensation requirements can reach several times the additional office surface.

C.2. Asymmetry between expansion and redevelopment

A central feature of the regulatory framework is that constraints are tied to *net additions* to office space rather than to the level of office surface.

Projects that increase office capacity through new construction or large extensions are subject to the strongest requirements. By contrast, operations that do not increase total office floor area are generally exempt from these reinforced obligations or subject only to qualitative expectations of functional mixity.

In particular, the following cases are treated more flexibly:

- rehabilitation of existing buildings without net expansion,
- demolition-reconstruction that does not increase total office surface,
- mixed-use projects combining office and residential uses on-site.

These provisions imply that redevelopment within the existing built envelope is comparatively less constrained than expansion-oriented projects.

C.3. Spatial implementation and compliance

Residential compensation is typically expected within the same municipality as the office project, although in some cases it may be provided elsewhere within the Paris Region or associated territories. In addition, housing delivery is generally expected to be linked to the realization of the office project.

The institutional guidance also allows for some forms of compliance through the removal of existing office space when this facilitates residential production. This further reinforces the fact that the regulation operates on the organization of development within the existing urban fabric.

C.4. Timing and implementation lag

The reform formally entered into force on November 1, 2018. However, accreditation approval precedes permit issuance, and the processing of applications typically requires several weeks to months.

As a result, permits issued in November and December 2018 are unlikely to reflect the new regulatory constraints. In the empirical analysis, the post-reform period is therefore defined as starting on January 1, 2019. This timing minimizes the risk of misclassifying projects initiated under the pre-reform regime.

C.5. Implications for identification

The regulatory framework generates a discontinuous increase in the cost of office expansion above specific thresholds within a fixed geographic perimeter. At the same time, redevelopment within the existing built stock remains comparatively less constrained.

This structure underlies the empirical design. The analysis exploits the stability of the geographic boundary and the threshold-based nature of the reform to identify the effect of the policy on development outcomes near the perimeter.