Regulatory Discretion and Housing Supply: Evidence from Chicago's Aldermanic Privilege

Jacob Herbstman[†]

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Abstract

Stringent land use regulations restrict housing supply and are a key driver of the housing affordability crisis in the United States. Most research focuses on formal regulations such as zoning codes, while the role of informal regulatory discretion is less understood. I study how discretionary authority over development affects housing supply using Chicago's "aldermanic privilege"—the informal power of each city council member to unilaterally block or approve development in their ward. Using permit processing times to construct a proxy for alderman-level regulatory stringency, I document large differences in the regulatory environment across wards that are not explained by geography or demographics. A border discontinuity design shows that new multifamily construction is roughly 10% less dense on the more stringent side of ward boundaries, and an event study exploiting ward redistricting finds that blocks reassigned to more stringent aldermen experience a 9% decline in high-discretion permits over the following five years. I also present suggestive evidence that these supply restrictions pass through into higher home prices. The results suggest that informal discretionary authority is a quantitatively important determinant of housing supply, and that reforms targeting the structure of local land use decision-making could meaningfully affect housing affordability.

[†]University of Chicago, Harris School of Public Policy. Email: jherbstman@uchicago.edu.

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1 Introduction

Housing costs in the United States have risen dramatically, and a large body of research points to land use regulation as a key driver. Restrictive zoning, minimum lot sizes, and density caps limit the supply of housing, raise prices, and distort the allocation of labor across cities (Glaeser, Gyourko and Saks, 2005; Saiz, 2010; Turner, Haughwout and Van Der Klaauw, 2014; Hsieh and Moretti, 2019; Duranton and Puga, 2023). But the formal rules on the books are only part of the story. A growing literature emphasizes that the *process* of navigating the regulatory system, including permitting delays, discretionary review, and uncertainty about approval, may matter as much or more than the rules themselves (Mayer and Somerville, 2000*b*; Paciorek, 2013; Molloy, Nathanson and Paciorek, 2022; Soltas and Gruber, 2026). When developers face long and unpredictable timelines, projects shrink, marginal investments become unviable, and housing supply falls (Cunningham, 2006; Bloom, 2009; Oh, Yang and Yoon, 2024). But there is relatively little causal evidence on how the discretionary authority of local actors shapes the development environment and housing supply.

This paper studies regulatory discretion in the context of Chicago's "aldermanic privilege," the informal power of each of Chicago's 50 city council members (aldermen) to unilaterally block or approve development in their ward. Chicago has no city charter, so there is no statutory basis for this power. It operates through deference: other aldermen vote with the local member on land use matters, city agencies follow the local alderman's lead, and developers understand that meaningful projects require the alderman's blessing (Thale, 2005; Einstein, Glick and Palmer, 2019). The result is that the de facto regulatory environment changes discretely at ward boundaries. Two parcels on opposite sides of the same street, in the same neighborhood, with the same zoning, can face very different regulatory regimes depending on which alderman they fall under. This setting provides unusually clean spatial variation in regulatory discretion, which I use to study its effects on the development process.

To measure the regulatory environment that each alderman creates, I construct "aldermanic stringency scores" from permit processing times for high-discretion permits. These are the permit types that tend to draw community opposition and aldermanic intervention: new construction, renovation, demolition, and similar categories that involve meaningful regulatory review. I residualize processing times against ward characteristics and time effects to strip out variation driven by geography and demand, then apply empirical Bayes shrinkage (Kline, Rose and Walters, 2022, 2024) to address noise for aldermen with few permits. The resulting scores show large differences in regulatory stringency across Chicago's 50 wards that are not explained by location or demographics. This approach is motivated by the finding that permitting frictions are a primary determinant of housing supply elasticity (Paciorek, 2013; Gyourko, Hartley and Krimmel, 2021; Soltas and Gruber, 2026), and by a simple time-to-build framework in which the effective delay imposed by the regulatory process acts as a wedge on the present value of development (Kydland and Prescott, 1982; Mayer and Somerville, 2000*a*).

I use these scores, which are proxies for the regulatory environment, in two empirical designs. The first is a border discontinuity design comparing the density of new multifamily construction on opposite sides of ward boundaries, within narrow bands where neighborhood characteristics are nearly identical but the alderman changes. Using border-segment fixed effects to examine new residential construction density within each ward border, I find that a one standard deviation increase in aldermanic stringency is associated with roughly 10% lower multifamily density. This effect is present for both floor-area ratio and dwelling units per acre, is concentrated near ward boundaries, and is robust to a range of specification checks.

The second design is an event study exploiting the 2015 ward redistricting. When ward boundaries are redrawn for population balancing, some census blocks are reassigned to new aldermen while nearby blocks remain under the same representation. Using within-census block variation, I find that blocks moved to more stringent aldermen experience a 9% decline

in high-discretion permit issuance over the following five years. The effect is specific to the types of permits that require aldermanic engagement and is much smaller for the low-discretion permits (excluding signs) used as a placebo comparison group, which do not show a clear post-treatment pattern. This is direct evidence that the development pipeline responds when the regulatory gatekeeper changes.

Finally, I present suggestive evidence that the supply restrictions caused by more stringent aldermen pass through into housing prices. Using the same redistricting variation, I find that home sale prices rise approximately 1% on blocks reassigned to more stringent aldermen relative to nearby blocks that were not reassigned. The estimates are directionally consistent with the supply story, but they are noisier and harder to separate from demand-side explanations, so I treat them cautiously.

This paper contributes most directly to the literature on how local political actors and institutions shape housing supply. Khan (2021) studies the same Chicago setting and finds that aldermen approve smaller rezonings near ward boundaries where benefits spill over to adjacent wards. I study a broader margin: by examining realized new construction and not just statutory changes in allowed density via rezoned parcels, I capture the response of the entire development pipeline to aldermanic frictions, including the projects that proceed by-right at reduced scale precisely to avoid triggering discretionary review.

More broadly, the paper connects to several strands of the urban economics literature. Paciorek (2013) shows that regulation reduces housing supply primarily through its effect on new construction timelines. Mayer and Somerville (2000*b*) and Mayer and Somerville (2000*a*) demonstrate that development lags are a first-order determinant of the supply response to demand shocks. Soltas and Gruber (2026) provides new evidence that permitting costs are large enough to materially affect housing affordability. My paper complements this work by providing causal evidence that variation in the discretionary layer of the regulatory process, not just the formal rules, generates economically meaningful differences in housing supply within a single city.

The paper also connects to the literature on how the fragmentation of decision-making authority leads to undersupply of housing. Fischel (2009) argues that homeowners use local political institutions to restrict development and protect property values. Subsequent work has documented how this plays out across municipalities, with each jurisdiction restricting development to a degree that may be locally rational but is collectively costly (Hilber and Robert-Nicoud, 2013; Ortalo-Magné and Prat, 2014; Kulka, Sood and Chiumenti, 2022; Monarrez and Schönholzer, 2023; Mast, 2024; Bordeu, 2025). My results show that a similar coordination failure operates within a single city when informal authority is fragmented across local representatives, each of whom controls the regulatory environment in their own territory.

The rest of the paper proceeds as follows. Section 2 describes the institutional setting and data. Section 3 presents a short time-to-build framework that organizes the empirical predictions. Section 4 presents the cross-sectional density evidence at ward boundaries. Section 5 presents the permit event study. Section 6 presents the home sales results. Section 7 discusses interpretation and policy implications, and Section 8 concludes.

2 Institutional Background and Data

2.1 Aldermanic Privilege and the Development Process

Chicago's City Council consists of 50 aldermen, each representing a single ward. Unlike most cities, where the mayor or a planning commission controls land use decisions, Chicago's informal norms give each alderman near-total authority over development within their ward boundaries. This practice, known as "aldermanic privilege" or "aldermanic prerogative," is a matter of custom, not an official rule, due to Chicago's lack of a city charter. It operates through a simple convention: on matters affecting a single ward, other aldermen defer to the local representative, and city agencies follow the alderman's lead (Thale, 2005). The convention is strong enough that aldermen have historically been described as "little

mayors" governing their wards as personal fiefdoms (Einhorn, 1991). One alderman described the system candidly: "I often liken the City of Chicago to a feudal system, where the mayor is sort of a de facto king. And each alderman is the lord of their individual fiefdom." (Keefe, 2013)

The formal mechanism. Before Mayor Lightfoot eliminated the practice in July 2019, aldermanic authority over permitting had a formal institutional expression. The Department of Buildings required an "aldermanic acknowledgement letter" for building permits above a threshold value, giving the local alderman a formal 10-day hold on permit issuance—embedding aldermanic review directly into the permitting pipeline as a matter of city procedure, not merely informal custom (City of Chicago, 2019). Ald. Carlos Ramirez-Rosa described the old norm: "Rather than an alderman just picking up the phone and saying, 'stop that permit,' we are going to make it transparent." Ald. James Cappleman was blunter: "Aldermen right now, we have 50 czars." (Schutz, 2019)

Mayor Lightfoot's 2019 executive order eliminated the formal hold (Mayor's Press Office, 2019), but the 60-day review her administration conducted that summer documented how deeply prerogative had penetrated agency practice: department by department, city agencies had organized their workflows around aldermanic deference, and those informal norms proved far more durable than the formal rule (City of Chicago, 2019). As Lightfoot herself put it: "If you've got 50 separate fiefdoms that you have to deal with on issues like affordable housing, or just getting a sign on your building, it makes doing business with the city of Chicago damn near impossible." (Spielman, 2019*b*) The Metropolitan Planning Council reached the same conclusion from the developer side, finding that "before a development even gets to the point of being indefinitely delayed, many never make it past 'Go' because of required aldermanic letters of support." (Metropolitan Planning Council, 2019)

How prerogative shapes the development pipeline. Aldermanic discretion operates at every stage of the development process, not only at the formal permitting step.

Pre-application. Experienced Chicago developers know that any project requiring rezoning, or smaller project modifications, will require obtaining the alderman's support before formal papers are filed. This is not a legal requirement but an operational reality: without the alderman's support, a developer risks investing substantially in design, legal, and engineering work before facing rejection at the political stage. One anonymous Chicago architect described the dynamic: "To get the alderman, you have to get the neighborhood group, and that's where the trouble comes in. It's fraught with risk, even to have this conversation." (Giangreco, 2024)

Rezoning. For projects requiring a change to the zoning code, the alderman's role is effectively dispositive. Although zoning amendments formally require a City Council vote, the council "routinely shows unquestioning deference to local aldermen even in the absence of any articulated local concern" (Chase, 2023). Equally consequential is the alderman's ability to preemptively *downzone* parcels to foreclose future development. Joseph Schwieterman, a DePaul University professor who studies Chicago land use, noted that the practice "creates real uncertainty for real estate developers when downzoning becomes ad hoc or driven by the desire of an alderman to settle scores." (Gallun, 2019) Documented episodes include Ald. Brendan Reilly's proposals to downzone a River North site where a developer had proposed a 60-story tower and a separate site to block a 615-room hotel, and a Pilsen downzoning orchestrated by Ald. Solis that stripped a parcel of its residential designation to kill a planned 465-unit apartment project.

Permit processing. For projects that proceed by-right under existing zoning, the alderman retains substantial informal influence over the Department of Buildings permit queue. The corruption case against Ald. Ed Burke illustrated the mechanism in its most extreme form: federal prosecutors documented how his staff withheld routine permits—including a driveway permit for a fast-food restaurant—until the property owner directed legal business to Burke's law firm (Spielman, 2019*a*). The case made vivid a dynamic that developers had long understood: in Chicago, permit timing is yet another level aldermen can pull to delay and

stifle development projects that they oppose.

A recent example. Developer Fern Hill began community outreach in 2021, proposing a roughly 500-unit mixed-income tower at a transit-accessible site in the 2nd Ward, with approximately 100 affordable units and an estimated $2.5 million in annual property tax revenue. After four years and more than ten community meetings, Ald. Brian Hopkins announced in January 2025 that he would not advance the project, citing opposition from residents of neighboring high-rises. He was explicit about the source of his authority: "Ultimately I am the decision maker on this. I'm not going to hide from that. This is a zoning decision and in Chicago we have something called aldermanic prerogative." (Achong, 2025) Within two weeks the developer resubmitted a scaled-down proposal—fewer floors, fewer units—and Hopkins approved it. The episode captures both margins this paper studies: the approved project is smaller than what the developer proposed (intensive margin), and the four-year ordeal signals to other developers the cost of attempting large projects in wards with active aldermanic gatekeeping (extensive margin).

Federal scrutiny. The consequences of this institution have attracted federal attention. In October 2023, the U.S. Department of Housing and Urban Development issued findings from a five-year investigation stemming from a 2018 complaint by the Chicago Area Fair Housing Alliance and nine other advocacy groups. HUD concluded that aldermanic prerogative functions as an illegal "local veto" over affordable housing proposals: "the city affords each of its 50 wards a local veto over proposals to build affordable housing and that many majority-white wards use the local veto to block, deter or downsize such proposals" (Chase, 2023). The practice has also been accused of accentuating racial segregation and violating civil rights law by outside groups (Shriver Center on Poverty Law, 2019). The question of whether to reform or abolish aldermanic privilege remains a central issue in Chicago politics.[1]

[1]See `https://news.wttw.com/elections/voters-guide/2023/races/city-council`, where each city council candidate was asked specifically about their stance on aldermanic prerogative.

2.2 Building Permits and the Permit Pipeline

The building permits data come from the City of Chicago's open data portal and include all permit applications filed since 2006 with valid geographic coordinates and non-negative processing times. I distinguish between two categories of permits based on how much aldermanic discretion they are likely to face.

High-discretion permits are those for new construction, renovation and alteration, wrecking and demolition, porch construction, and permits reinstated after being previously revoked.[2] These are the permit types most likely to draw community opposition and face delays due to community meetings or direct aldermanic intervention. *Low-discretion permits* are all remaining permit types outside this high-discretion group, excluding signs. These permits serve as a placebo comparison group because they are generally less exposed to discretionary aldermanic intervention than the high-discretion permits.

Distinguishing between these categories is crucial for isolating the alderman-specific component of the regulatory environment. High-discretion permits are the ones that require meaningful interaction with the alderman and are most likely to be affected by their behavior. Low-discretion permits serve as a useful comparison group: if the stringency scores are picking up something specific to the discretionary layer rather than general bureaucratic capacity, effects should concentrate in high-discretion permits and be absent for low-discretion ones.

The data bear this out. High-discretion permits take substantially longer to process and exhibit much more cross-alderman dispersion in mean processing time. At the permit level, longer processing times for high-discretion permits are associated with *lower* ward-month permit volume, while low-discretion processing times are weakly positively correlated with ward-month permit volume, suggesting they reflect administrative capacity rather than regulatory friction. Figure 1 visualizes these patterns; Appendix B.1 reports the corresponding summary statistics and volume correlations.

[2]Porch construction permits have received extra scrutiny since the 2003 balcony collapse.

2.3 Aldermanic Stringency Scores

I construct the stringency score by residualizing permit processing times against ward and permit characteristics, then extracting the alderman-specific component. The first stage regresses log processing time at the permit level on ward-level demographic controls (median household income, racial composition, homeownership rate, population), permit-location geographic controls (distance to CBD, lakefront proximity, CTA rail access), lagged ward-month permit volume, and a rich set of fixed effects for year-month, permit type, and review type:

$$\log(\text{Processing Time}_i) = \mathbf{X}_i\beta + \gamma_{\text{ym}} + \delta_{\text{pt}} + \zeta_{\text{rt}} + \epsilon_i \tag{1}$$

where γ_{ym} are year-month fixed effects, δ_{pt} are permit-type fixed effects, and ζ_{rt} are review-type fixed effects. Table C.1 in the Appendix reports the coefficients on the stage-one controls.

The second stage aggregates these residuals to the ward-month level and projects the ward-month mean residual onto alderman indicator variables and the same lagged ward-month permit volume control, weighted by the number of permits observed in each ward-month:

$$\bar{\hat{\epsilon}}_{wm} = \mu + \alpha_a + \lambda\text{Volume}_{w,m-1} + \eta_{wm} \tag{2}$$

The raw alderman fixed effects $\hat{\alpha}_a$ can be noisy for aldermen with few permits. I apply empirical Bayes shrinkage (Kline, Rose and Walters, 2022, 2024) to pull imprecise estimates toward the grand mean, then standardize the final scores to have mean zero and unit standard deviation.

Appendix C.2 reports additional validation. Higher stringency predicts fewer high-discretion permits and more evidence of regulatory friction in ward-month data, while the correlation between predecessor and successor aldermen within the same ward is relatively low ($\rho \approx 0.18$), supporting the interpretation that the index captures an alderman-specific component of the development process rather than a fixed ward trait.

Appendix Figure C.1 shows the distribution of the final stringency scores, and Figure 2 maps them across the city. There is score variation in wealthy and poor neighborhoods, on the North Side and South Side, confirming that the scores are not simply proxying for location or local demand conditions.

2.4 New Construction Data

I construct a dataset of all new residential construction in Chicago between 2006 and 2025 using publicly available data from the Cook County Assessor's Office. For buildings with up to six units, I use the residential improvement characteristics data, which provides building-level information on dwelling units, stories, and square footage. For larger multifamily buildings, I draw on the commercial valuation dataset, which covers properties assessed under multifamily model groups and provides unit counts, building area, and land area. I merge both sources with the Assessor's parcel universe for precise location coordinates, assign each parcel to its ward using City of Chicago shapefiles, and define the construction date as June 15 of the recorded year built. I keep parcels with positive lot area, positive building area, and at least one dwelling unit.

For each parcel I compute two standard density measures: Floor Area Ratio (FAR), defined as total building square footage divided by lot area, and Dwelling Units Per Acre (DUPAC), defined as dwelling units divided by lot acreage. Distance to the nearest ward boundary is computed in feet using the Illinois East State Plane coordinate system.

Table 1 presents summary statistics. The full sample contains 14,014 new residential buildings. The 2,611 multifamily buildings (2+ units) are the focus of most of the density analysis. While I focus on multifamily buildings where aldermanic discretion has the most scope to matter, I also show results including all new construction and find similar patterns.

2.5 Home Sales

Home sales data come from the Cook County Assessor's Office and cover all residential transactions from 1999 to 2025. I restrict the sample to arm's-length transactions of single-family homes and small multifamily buildings (2–6 units), excluding nominal transfers, land-only sales, and intrafamily conveyances. Sale prices are first deflated to 2022 dollars using monthly Chicago CPI excluding shelter and then winsorized at the 1st and 99th percentiles. Each transaction is geocoded using the county parcel universe and assigned to the relevant ward based on the boundaries in effect at the sale date. Property-level hedonic controls (building square footage, land square footage, building age, bedrooms, bathrooms, garage) are drawn from a rolling join to the most recent assessor record as of the sale year.

3 Conceptual Framework

3.1 Developer Problem

Consider a developer choosing the scale $q \geq 0$ (units of housing) of a project on a given site. Construction costs are $C(q) = F + cq + \frac{\gamma}{2}q^2$ with $\gamma > 0$, so marginal costs are increasing in project size. Let p denote the local price level, taken as given at the project level.

The local regulatory environment s maps into an *effective time-to-build* $T(s)$ with $T'(s) > 0$. I interpret $T(s)$ broadly as a sufficient statistic for any combination of entitlement delay, redesign requirements, negotiation costs, financing frictions, and regulatory uncertainty that pushes expected revenue further into the future. If revenue arrives upon project completion, the developer's expected discounted revenue per unit is $e^{-rT(s)}p$, where r is the relevant discount or financing rate. The developer solves

$$\max_{q \geq 0} \; e^{-rT(s)} p\, q - C(q).$$

The first-order condition gives

$$q^*(T(s), p) = \frac{e^{-rT(s)}p - c}{\gamma}$$

when $e^{-rT(s)}p > c$, and $q^* = 0$ otherwise. The key comparative static is $\partial q^*/\partial T(s) < 0$: longer or more burdensome regulatory processes reduce project scale and can flip marginal projects from "build" to "don't build."

3.2 From Developer Choices to Empirical Predictions

Aggregating across sites, total housing supply is $Q = \sum_i q_i^*$. Higher $T(s)$ reduces Q; with downward-sloping demand $p'(Q) < 0$, equilibrium prices rise. The framework therefore predicts that sites in more stringent regulatory environments should have lower completed density and more projects that fall below the viability threshold (Section 4), that the flow of permits requiring meaningful regulatory review should fall when stringency increases (Section 5), and that equilibrium housing costs should rise as supply contracts (Section 6). These three predictions correspond to the three empirical sections of the paper.

3.3 Interpretation of the Stringency Score

The regulatory environment in this setup affects outcomes through the single sufficient index $rT(s)$, which acts as a multiplicative wedge $e^{-rT(s)}$ on the present value of revenue. This wedge governs both the developer's intensive-margin density choice and the extensive-margin decision to build or not.

The aldermanic stringency score I construct in Section 2.3 is an empirical proxy for $T(s)$, and I interpret it as a "sufficient statistic" for capturing the various dimensions of regulatory friction imposed by the alderman. It measures the effective delay each alderman imposes on the development process, after removing variation attributable to ward characteristics. Other dimensions of regulatory friction, such as impact fees, design mandates, or community

benefit requirements, would enter as additional costs in $C(\cdot)$ and reinforce the same qualitative predictions. The core logic is that longer observed processing times for high-discretion permits are a noisy, but still informative, signal of the regulatory wedge that lowers density, reduces aggregate supply, and in equilibrium raises prices.

4 Density at Ward Boundaries

4.1 Visual Evidence

If aldermanic discretion affects the development process, its effects should be visible in what actually gets built near ward boundaries, where neighborhood characteristics are nearly identical but the alderman changes. I begin with border discontinuity plots that show this relationship directly. I stack all ward boundaries together, placing the more lenient alderman's side at negative distances and the more stringent side at positive distances. The sample is restricted to new multifamily construction, and I residualize density outcomes against zoning-group fixed effects[3], boundary-segment fixed effects, construction-year fixed effects, and own-side demographic controls before plotting. This removes variation from broad zoning differences, location along the boundary, and citywide trends.

Figure 3 presents the results using a 500-foot bandwidth. Both log(FAR) and log(DUPAC) show a clear downward discontinuity at the boundary when moving from the lenient to the stringent side.[4]

These magnitudes are large, and they should be. The border discontinuity captures the total equilibrium difference in density, which reflects both the direct effect of aldermanic behavior and developer self-selection into more lenient wards. Developers can choose where

[3]I group Chicago zoning codes into the following categories: RS-* single-family residential; RT-* and RM-* multi-family residential; B-* neighborhood mixed-use; C-* commercial; M-* industrial; DX-*, DR-*, DS-*, and DC-* downtown; PD planned development; all remaining codes Other.

[4]The jump estimate is from an unweighted linear regression of the residualized outcome on a boundary-side indicator, running distance, and their interaction, with own-side demographic controls and standard errors clustered at the ward-pair level. Exact point estimates and standard errors appear in the figure subtitles.

to build, and they clearly prefer the lenient side when all else is equal. The plots establish that something changes sharply at ward boundaries. The regression analysis that follows isolates how much of that change is attributable to aldermanic stringency.

4.2 Border Fixed Effects Regressions

To move from visual evidence to a formal estimate, I compare density within narrow boundary segments controlling for zoning and time effects:

$$\ln(Y_{iwst}) = \beta \cdot \text{Stringency}_w + X_i'\theta + \alpha_{\text{zone group}} + \alpha_{\text{segment}} + \gamma_t + \varepsilon_{iwst} \tag{3}$$

where Y_{iwst} is a density outcome for parcel i in ward w on boundary segment s, built in year t, and X_i collects own-side demographic controls. The fixed effects enter additively: zoning-group fixed effects absorb differences across broad zoning environments, segment fixed effects absorb differences across boundary segments, and year fixed effects absorb citywide trends. The sample is restricted to new multifamily construction within 500 feet of a ward boundary. Standard errors are clustered at the ward-pair level. Appendix Figure D.1 maps the boundary segments that enter the analysis.

The segment fixed effects are the key design element. Boundary segments are short geographic units, much shorter than full ward-pair borders that can stretch for miles through different neighborhoods, so comparisons within segments are spatially precise. Zoning-group fixed effects ensure that parcels are compared within broad zoning environments rather than across fundamentally different parts of the zoning code.

Table 2 presents the results for the two main density outcomes, DUPAC and FAR. A one standard deviation increase in aldermanic stringency is associated with about 10% lower FAR and DUPAC.

4.3 Robustness

The density results are robust to a range of specification checks, which I summarize here with full results in Appendix D.

The most informative diagnostic is the median-gap split. The border discontinuity plots pool all ward boundaries together and treat the ward switch as a binary treatment variable, not a continuous one as in the regressions. This raises the concern that marginal ward pairs with small stringency gaps, that may not be robust to specification changes, are driving the results. Figure 4 splits the sample into two groups based on whether the stringency gap between the two aldermen is above or below the median stringency gap. For FAR, the plotted break is visually small in the below-median sample and larger in the above-median sample. For DUPAC, both subsamples show downward jumps, with a larger plotted break in the above-median group. These patterns are consistent with larger stringency gaps being associated with larger density differences at the boundary.

I next provide a placebo test by moving the cutoff 500 feet into each ward. If the discontinuity is genuinely driven by the ward boundary where aldermanic jurisdiction changes, it should not appear at arbitrary locations within wards. Figure 5 shifts the cutoff 500 feet into each ward and re-estimates the discontinuity. Relative to the true boundary, the placebo plots are flatter around the shifted cutoffs and do not show the same visible jump.

The appendix reports several additional checks. In donut plots that exclude parcels within 25 or 50 feet of the boundary, the fitted discontinuity remains negative (Figure D.2). Replacing segment fixed effects with coarser ward-pair fixed effects still yields negative estimates, though they are attenuated relative to the segment-fixed-effects baseline (Table D.1), as does dropping ward-pair-era combinations flagged as potentially confounded by simultaneous institutional changes (Table D.2). Narrowing the bandwidth to 250 feet produces comparable estimates with wider confidence intervals (Table D.3), while widening it to 1,000 feet attenuates the effects (Table D.4), consistent with aldermanic influence being concentrated near ward boundaries. Finally, extending the sample to include all new construction,

not just multifamily, produces a smaller plotted discontinuity (Figure D.4), as expected given that single-family construction has less variation in density and is less likely to be the subject of community meetings, unit count negotiations, or other frictions aldermen could impose.

5 Event Study: Permits

5.1 Redistricting Design

While the border discontinuity evidence in Section 4 is robust, there is still a fundamental concern that opposite sides of ward borders could be fundamentally different in ways that border-segment fixed effects do not capture. To address this, I exploit the 2015 ward redistricting as a natural experiment, which reassigned census blocks to new aldermen for reasons unrelated to housing market conditions. This allows me to look within census blocks over time where aldermen are reassigned, which is an even cleaner comparison because the census block is exactly the same before and after redistricting, so any time-invariant differences across locations are fully absorbed by block fixed effects.

Ward boundaries in Chicago are redrawn following each decennial census to equalize populations across wards. The 2015 redistricting (based on the 2010 Census) reassigned many census blocks to new aldermen while leaving nearby blocks under the same representation. This creates a difference-in-differences comparison between blocks that switched aldermen (treated) and nearby blocks that stayed in the same ward (control). The identifying assumption is that, absent the change in aldermanic representation, treated and control blocks would have followed parallel trends in permit activity.

Several features of the design support this assumption. Redistricting is driven by population equalization and bargaining over demographic composition of the wards, not by housing market conditions on specific blocks. To strengthen the counterfactual, I restrict the control group to blocks that remained in the *origin* ward, so treated and control blocks shared the same pre-treatment regulatory environment. I also exclude control blocks in wards where

the alderman changed via election during the sample window, avoiding contamination from electoral turnover. Figure 6 illustrates the design for one ward pair, showing how blocks are classified into treatment and control groups based on the change in alderman stringency. Figure 7 shows the citywide distribution of treated and control blocks.

5.2 Estimating Equation

The outcome is the count of high-discretion permits issued on each census block in each year. Because permits are counts that aggregate naturally to a balanced block-year panel, with every block observed in every year (including zeros), I can include block fixed effects and identify entirely off within-block variation over time. I estimate the event study using Poisson pseudo-maximum likelihood (PPML), with the log conditional mean given by:

$$\log E[Y_{bt}] = \alpha_b + \lambda_{gt} + \sum_{\tau \neq -1} \beta_\tau \left(\Delta\text{Stringency}_b \times \mathbf{1}[t = \tau] \right) \tag{4}$$

where Y_{bt} is the count of high-discretion permits issued on block b in year t, α_b is a block fixed effect, and λ_{gt} is a border-pair-by-year fixed effect. In the compiled paper's 2015-cohort design, the treatment variable $\Delta\text{Stringency}_b$ is the destination alderman's baseline strictness score minus the origin alderman's baseline strictness score, where origin and destination aldermen are assigned using the 2014 pre-redistricting ward-to-alderman mapping.

The block fixed effects absorb all time-invariant characteristics of each block, so identification comes from within-block changes over time: the same physical block, before and after its alderman changes. The border-pair-by-year fixed effects absorb any local shocks that affect all blocks near a given ward boundary in a given year, ensuring that the comparison is between blocks near the same boundary that experienced different changes in aldermanic representation. The sample uses the 2015 redistricting cohort, a 1,000-foot bandwidth around ward boundaries, and uniform weights. Standard errors are clustered at the census block level.

This within-block design is not feasible for the home sales analysis in Section 6. Home sales are individual transactions with property-level hedonic characteristics that vary across properties within the same block, and many blocks have zero or one sale in a given year, making block-year aggregation uninformative and block fixed effects impractical at the transaction level. The permit count outcome, by contrast, aggregates cleanly to a balanced panel where block fixed effects absorb permanent differences in development activity across locations.

5.3 Results

Figure 8 presents the main result. The pre-period coefficients are all statistically insignificant from zero, and the joint pre-trend test does not reject. At redistricting ($t = 0$), there is a sharp drop in high-discretion permit issuance of roughly 15%, and the effect persists in subsequent years, fluctuating between approximately -5% and -15% through the end of the post-period. The pattern is consistent with an immediate disruption to the permitting pipeline when the regulatory gatekeeper changes.

Figure 9 traces the effect separately for blocks that moved to more stringent versus more lenient aldermen. The plotted coefficients are directionally consistent with the baseline specification: blocks assigned to more stringent aldermen tend to show lower permit issuance, while blocks assigned to more lenient aldermen tend to move in the opposite direction. I treat this split figure as descriptive rather than as a standalone formal test.

Table 3 collapses the post-period into a single pooled DID coefficient. A one standard deviation increase in alderman stringency reduces high-discretion permit issuance by approximately 9%. This is a direct measure of how aldermanic discretion affects the development pipeline: more stringent aldermen do not just produce denser or less dense buildings, they reduce the flow of projects that require their engagement.

A natural falsification test is to run the same specification on low-discretion permits, which are less exposed to discretionary aldermanic intervention than the high-discretion

permits used to construct the score. Figure 10 shows the corresponding event study, and the pooled DID estimate is -0.038 (SE $= 0.034$), roughly a third the size of the high-discretion effect and statistically indistinguishable from zero. The event-study plot does not show a clear post-treatment pattern. This placebo strengthens the interpretation that the permit decline on reassigned blocks operates through the discretionary channel.

5.4 Interpretation and Additional Checks

The permit results address the main limitation of the cross-sectional density evidence. Because the specification includes block fixed effects, identification comes from within-block changes over time: the same physical block, before and after its alderman changes. This design rules out confounding from any time-invariant block characteristics, such as geography or demographics, that could still theoretically bias the border discontinuity estimates.

The permits studied here are also not simply inputs to the stringency index. The stringency score is constructed from processing *times* on all high-discretion permits across a ward, while the event study outcome is the *count* of high-discretion permits issued on individual blocks. The score measures how an alderman treats permits in general; the event study measures whether the volume of development-related permitting responds when that alderman gains jurisdiction over new territory.

6 Event Study: Home Sale Prices

6.1 Design

If the supply restrictions documented in the previous two sections are economically meaningful, they should eventually be capitalized into housing prices. To test this, I apply the same redistricting variation from Section 5 to home sale prices.

The specification is estimated at the transaction level:

$$\ln(\text{Price}_{iy}) = \alpha_{gs} + \lambda_{gy} + X'_{iy}\gamma + \sum_{\tau \neq -1} \beta_\tau \left(\Delta\text{Stringency}_i \times \mathbf{1}[\text{event time} = \tau]\right) + \varepsilon_{iy} \qquad (5)$$

where $\ln(\text{Price}_{iy})$ is the log sale price for transaction i in sale year y, α_{gs} is a border-pair-by-origin-side fixed effect, λ_{gy} is a border-pair-by-year fixed effect, and X_{iy} includes hedonic controls for building square footage, land square footage, building age, bedrooms, bathrooms, and garage presence. The sample is restricted to transactions within 1,000 feet of ward boundaries with uniform weights. Standard errors are clustered at the census block level. I focus on the 2015 redistricting cohort because the 2023 redistricting provides too little post-period data for meaningful inference. The sample window runs from 2010 to 2020, providing roughly five years on each side.

The fixed effects structure here differs from the permit specification in Section 5, and the reason is the nature of the outcome data. Permits are counts that aggregate to a balanced block-year panel, making block fixed effects natural: every block is observed in every year, even if the count is zero. Home sales, by contrast, are individual transactions with property-level hedonic characteristics (square footage, age, lot size) that vary across properties within the same block and that matter for price. Many blocks have zero or one sale in a given year, so collapsing to block-year level would discard the hedonic variation that helps isolate the price effect from compositional changes. I therefore use border-pair-by-side fixed effects, which group blocks by their pre-redistricting ward and boundary location, combined with border-pair-by-year fixed effects that absorb local time-varying shocks. This means identification comes from comparing treated and control blocks on the same side of the same boundary in the same year, rather than from within-block variation over time as in the permit specification. The parallel trends assumption is correspondingly stronger: treated and control blocks must be on similar price trajectories absent redistricting. The flat pre-trends in the event study below support this assumption, and the hedonic controls help

absorb compositional differences across blocks.

6.2 Results

Figure 11 presents the event study. Panel A shows the continuous treatment specification. The pre-period coefficients show some variation but are generally centered around zero. Following redistricting, home prices on blocks assigned to more stringent aldermen rise gradually relative to the reference period. The trajectory is consistent with a supply restriction channel operating through reduced new construction: the price response builds slowly as the housing market tightens, rather than jumping immediately at redistricting.

Panel B shows the continuous split, allowing separate slopes for blocks that experienced positive versus negative changes in stringency. The pattern is directionally consistent with theory: positive price effects for blocks moving to more stringent aldermen, slight declines for blocks moving to more lenient ones. But the confidence intervals are wide, so the split results remain suggestive rather than definitive.

Table 4 collapses the post-period into a single coefficient. The point estimate is approximately 1%.

6.3 Interpretation

The price increase could operate through multiple channels, and I do not try to isolate a single one. The most direct mechanism is the supply channel documented in the earlier sections: more stringent aldermen reduce the flow of permits and the density of new construction, which tightens the local housing stock and raises prices. But prices could also respond through an expectations channel, where a more stringent alderman signals less future development, which incumbent homeowners value. Both channels are consistent with the paper's broader finding that aldermanic discretion has real effects on housing markets, and both represent a transfer from prospective buyers to incumbent owners.

The gradual post-redistricting trajectory is consistent with either story. Supply restric-

tions take time to bind as the existing housing stock turns over slowly, and expectations about future neighborhood character would also be capitalized gradually as buyers and sellers learn about the new alderman's stance. What the price result adds to the paper, even without decomposing the channels, is that aldermanic stringency moves housing costs in the predicted direction. Together with the density and permit evidence, this closes the loop from regulatory discretion to housing market outcomes: stringency reduces density at ward boundaries (Section 4), reduces the flow of new permits when the alderman changes (Section 5), and is associated with modestly higher prices.

7 Discussion

7.1 Interpretation of Estimates

The strongest evidence in this paper concerns the supply side of the housing market. More stringent aldermen reduce multifamily density near ward boundaries (Section 4), and when blocks are reassigned to more stringent aldermen through redistricting, the flow of high-discretion permits falls (Section 5). These results come from two different sources of variation and they tell the same story: aldermanic discretion makes the development process slower, less predictable, and less productive for the types of projects that require meaningful review. The price evidence is supportive but less definitive, with home prices moving in the expected direction but estimates that are noisier and harder to separate from demand-side explanations.

Khan (2021) documents the intensive margin of this institution: conditional on a developer seeking a rezoning, more stringent aldermen approve smaller projects. My density estimates capture this margin but also an extensive margin of realized development, where developers in more stringent wards build smaller by-right projects that avoid discretionary review rather than proposing larger projects that would trigger it. What neither my estimates nor Khan's can capture is the deterrence margin: projects that are never proposed because

developers anticipate rejection or choose to invest elsewhere. The Fern Hill episode described in Section 2 illustrates both the observed margins (the approved project was smaller than originally proposed) and the deterrence channel (four years of community meetings and aldermanic negotiation signals to other developers what attempting a large project entails). The time-to-build framework in Section 3 suggests why deterrence may be quantitatively important: even a modest increase in expected regulatory delay can flip marginal projects from viable to unviable, and these projects leave no trace in the data. My estimates are therefore best interpreted as lower bounds on the total supply impact of aldermanic discretion.

7.2 Policy Implications

The results inform ongoing debates about reforming aldermanic privilege. The supply effects I document accumulate through longer processing times, greater uncertainty, and the resulting deterrence of marginal projects, rather than through any single dramatic veto.

Centralizing land use authority, by shifting discretion from individual aldermen to city-wide bodies, would allow decision-makers to internalize the full benefits of housing production at the cost of sacrificing local knowledge. Expanding by-right development, so that code-compliant projects proceed automatically without aldermanic review, would eliminate the discretionary layer while preserving community preferences as expressed in the zoning code. But as documented in Section 2, aldermen retain unilateral power to downzone parcels preemptively, which means they can effectively undo by-right protections before a developer applies. The Reilly and Solis downzoning episodes are illustrative: reforms targeting only the permitting process without constraining aldermanic control over the zoning code itself may have limited impact. Effective reform likely needs to address both channels.

8 Conclusion

This paper studies how informal regulatory discretion over development affects housing supply. I focus on Chicago's aldermanic privilege, the informal power of each of the city's 50 aldermen to unilaterally block or approve development in their ward, and show that it has quantifiable effects on development activity in the city.

Using permit processing times for high-discretion permits, I construct aldermanic stringency scores that isolate the alderman-specific component of regulatory delay after removing variation attributable to ward characteristics. A border discontinuity design shows that new multifamily construction is roughly 10% less dense on the more stringent side of ward boundaries, in comparisons that absorb zoning classification, boundary location, and construction year. An event study exploiting ward redistricting shows that when blocks are reassigned to more stringent aldermen, high-discretion permit issuance falls by approximately 9%, while low-discretion permits that bypass aldermanic review show a much smaller and less clearly estimated response. Home sale prices rise modestly on blocks assigned to more stringent aldermen, consistent with supply restrictions being capitalized into housing costs, though this evidence is noisier.

The paper provides causal evidence that variation in informal discretionary authority, not just the formal rules on the books, generates economically meaningful differences in housing supply within a single city. This complements the broader finding that permitting frictions and time-to-build are primary determinants of supply elasticity (Paciorek, 2013; Soltas and Gruber, 2026). The policy implication is that reforms targeting the structure of land use decision-making in Chicago could meaningfully affect housing production, but as the institutional evidence in Section 2 documents, aldermen control both the permitting process and the zoning code, and effective reform likely needs to address both channels.

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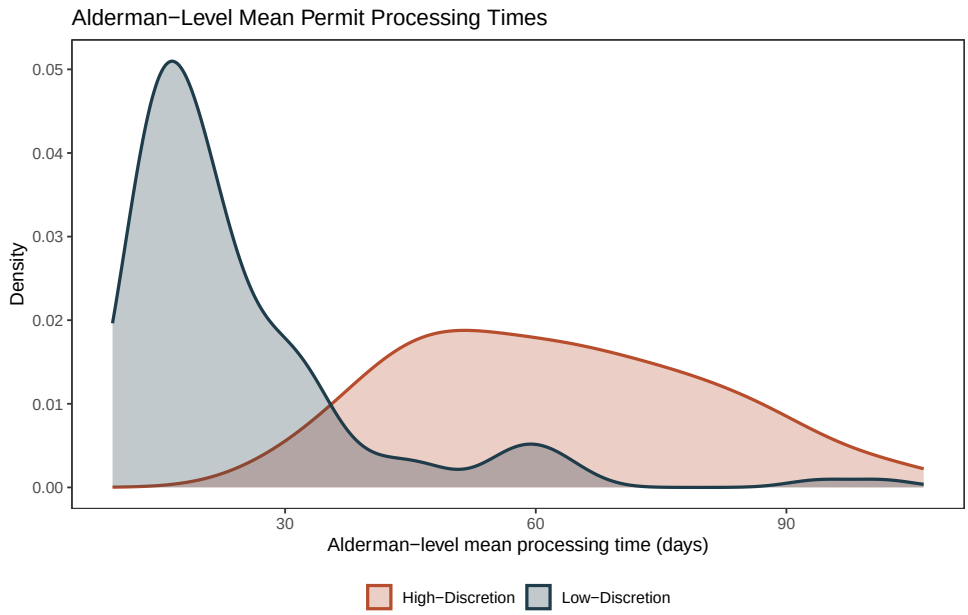


Figure 1: Alderman-Level Mean Processing Time for High- and Low-Discretion Permits

Notes: Figure plots the distribution of alderman-level mean permit processing time separately for high- and low-discretion permit groups. High-discretion permits are new construction, renovation/alteration, wrecking/demolition, porch construction, and reinstated permits. Low-discretion permits are all remaining non-high-discretion permit types, excluding signs. Processing time is days from application to issuance.

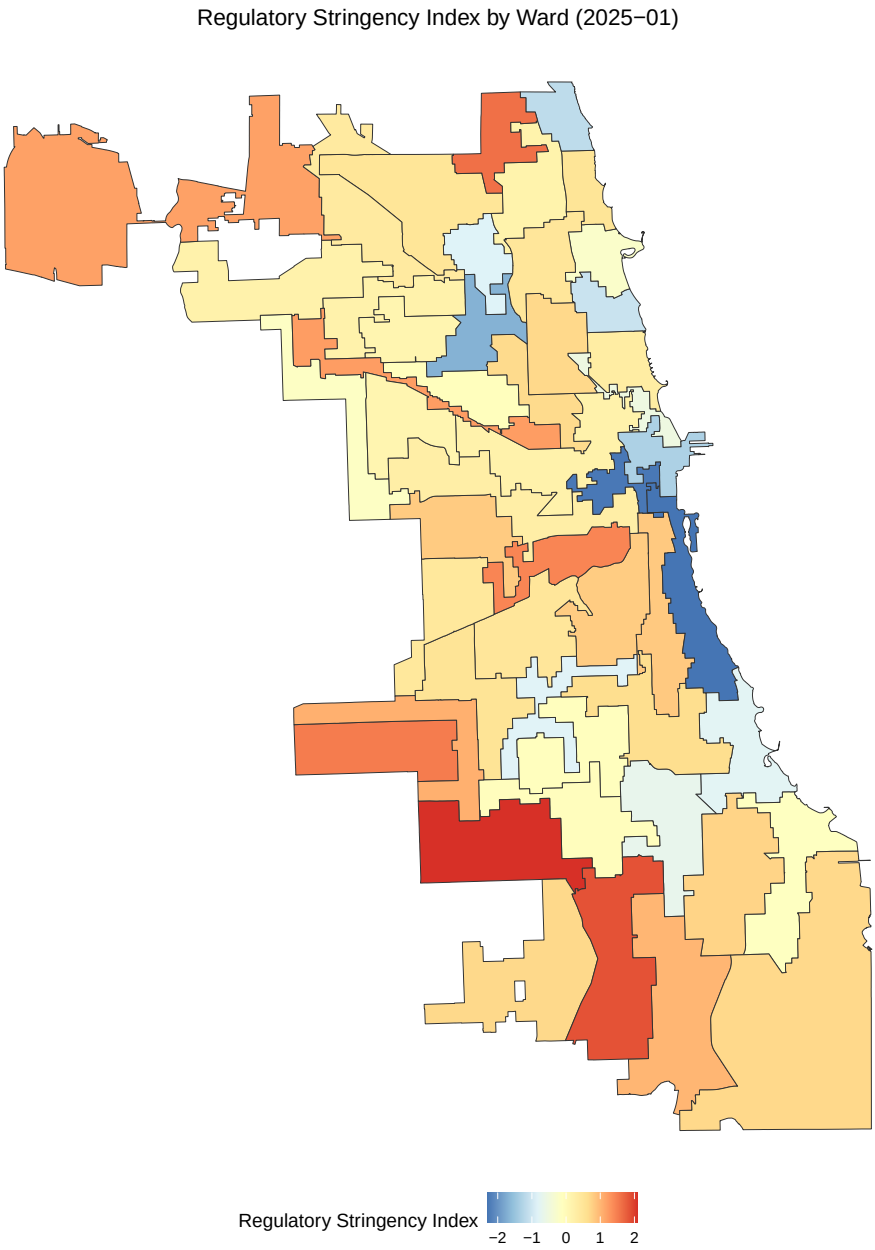


Figure 2: Geographic Distribution of Aldermanic Stringency

Notes: Map shows stringency scores by ward. Darker shades indicate more stringent aldermen.

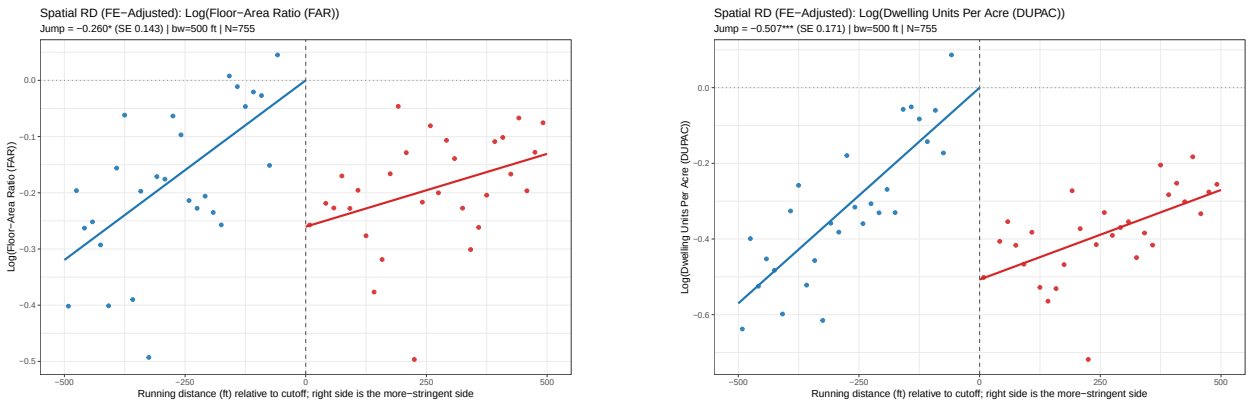


Figure 3: Border Discontinuity in Development Density at Ward Boundaries

Notes: Density measures are residualized against zoning-group, boundary-segment, and construction-year fixed effects plus own-side demographic controls before plotting. Sample restricted to new multifamily construction within 500 feet of ward boundaries. Negative distance indicates the more lenient alderman's side; positive distance indicates the more stringent side. Unweighted linear fits are shown on each side of the boundary. Standard errors clustered at the ward-pair level.

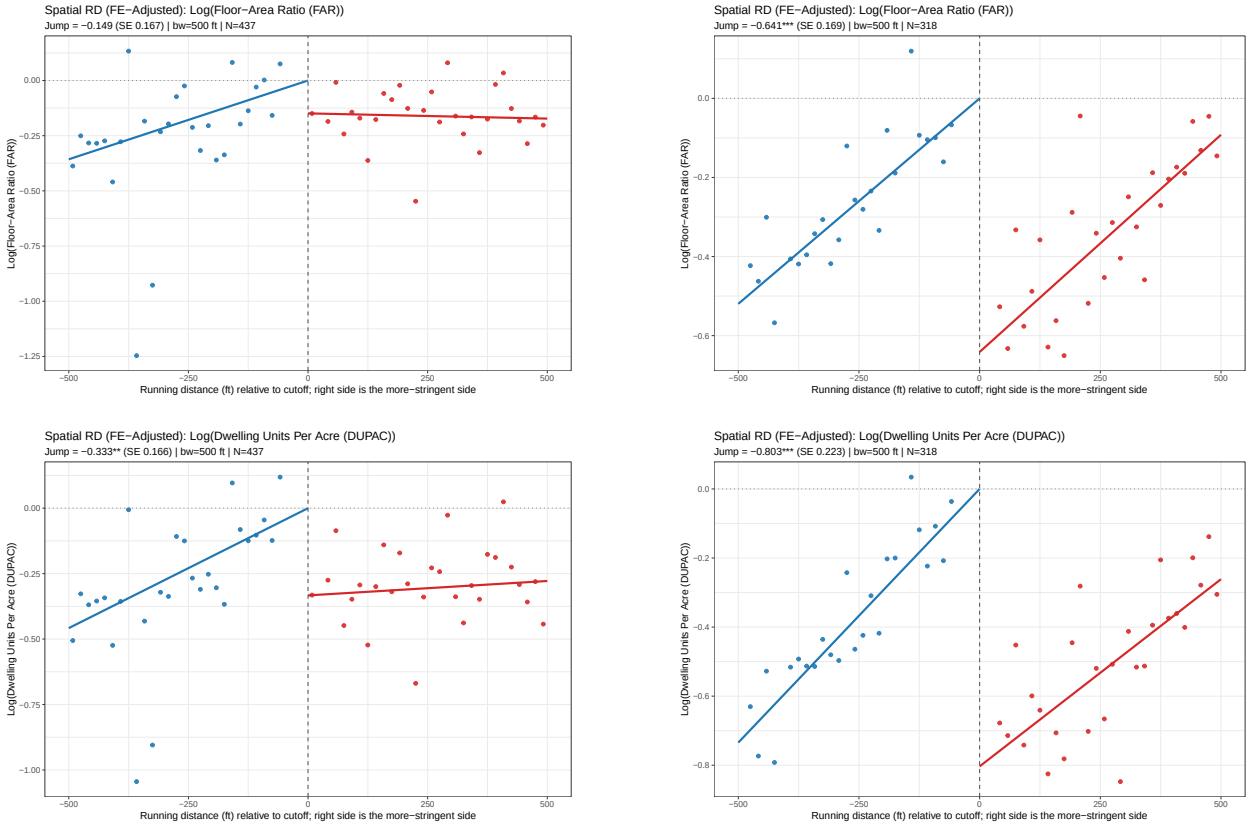


Figure 4: Border Discontinuity: Below vs. Above Median Stringency Gap

Notes: Top row: Log(FAR); bottom row: Log(DUPAC). Left column: below-median stringency gap; right column: above-median gap. Same specification as Figure 3. The plotted discontinuity appears larger for the above-median-gap subsample.

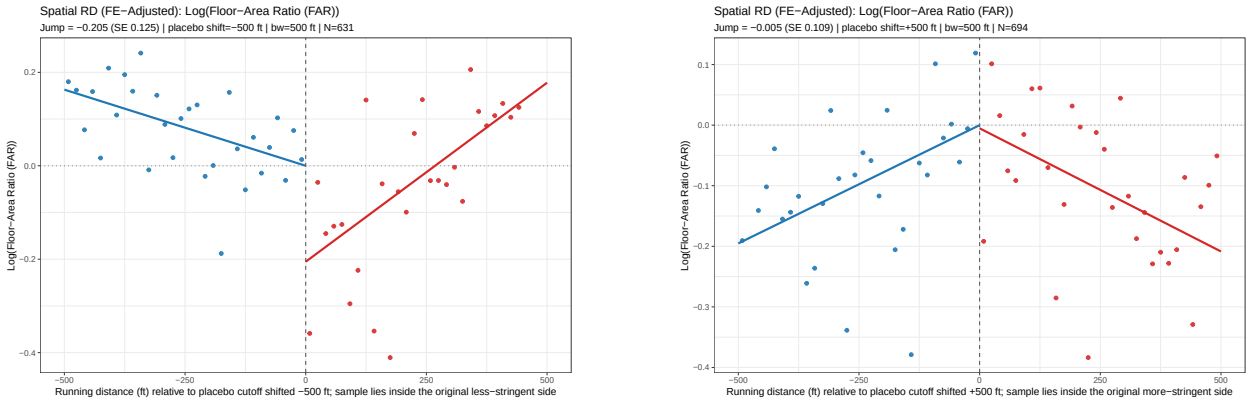


Figure 5: Placebo Boundary Tests: Log(FAR)

Notes: Left: placebo cutoff 500 feet inside the lenient ward. Right: placebo cutoff 500 feet inside the stringent ward. Same specification as Figure 3. The placebo plots are flatter than the main boundary plot. Appendix Figure D.3 shows the corresponding DUPAC results.

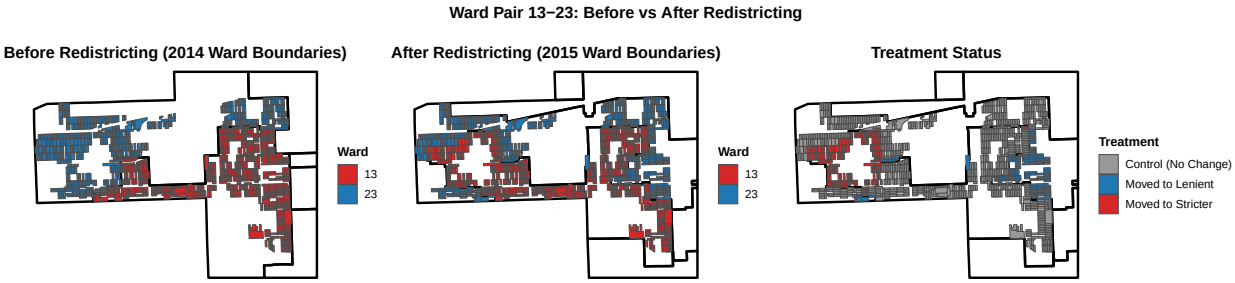


Figure 6: Identification Example: Ward Pair 13–23 (2015 Redistricting)

Notes: Top panel shows ward assignments before redistricting (2014 boundaries), bottom panel shows assignments after redistricting (2015 boundaries). Blocks that switch wards are classified as treated; blocks that remain in the origin ward serve as controls. Sample restricted to blocks within 1,000 feet of ward boundaries.

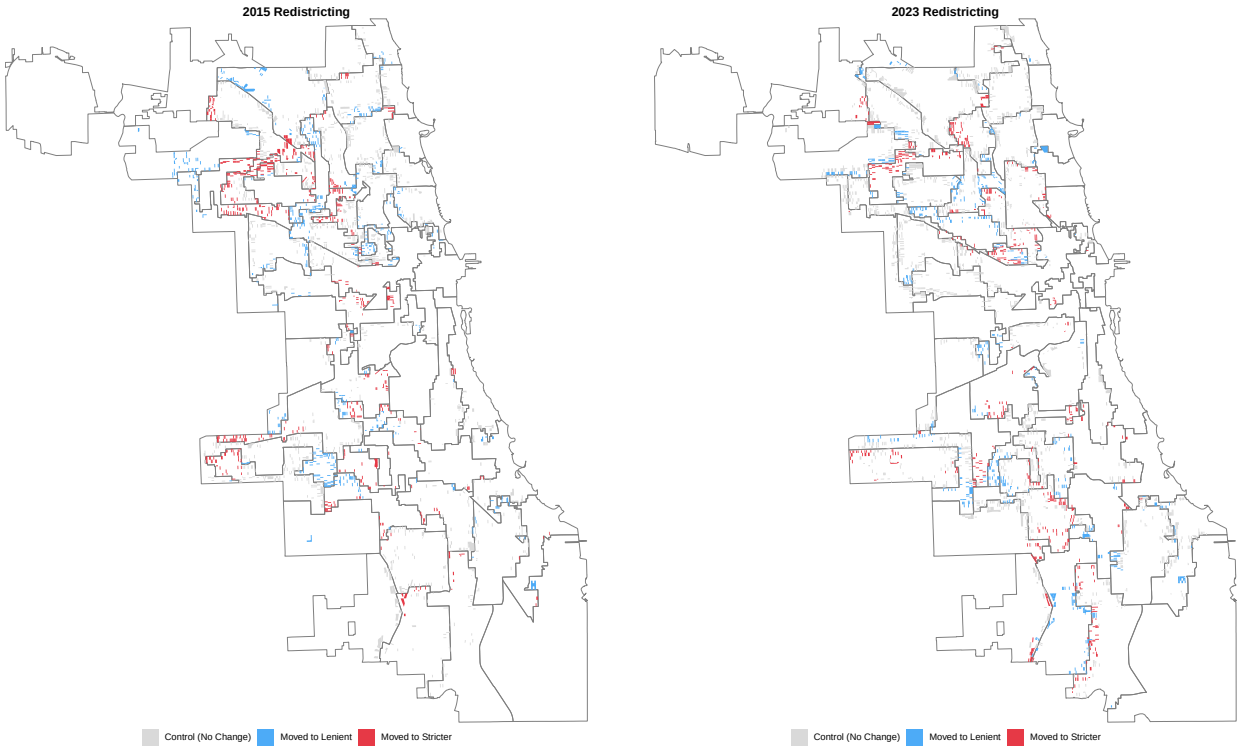


Figure 7: Treatment and Control Blocks: Citywide Distribution

Notes: Geographic distribution of treated and control blocks for the 2015 redistricting (left) and 2023 redistricting (right). Red indicates blocks that moved to a more stringent alderman, blue indicates blocks that moved to a more lenient alderman, and gray indicates control blocks that did not switch wards. Sample restricted to blocks within 1,000 feet of ward boundaries. Controls exclude wards with aldermanic turnover via election.

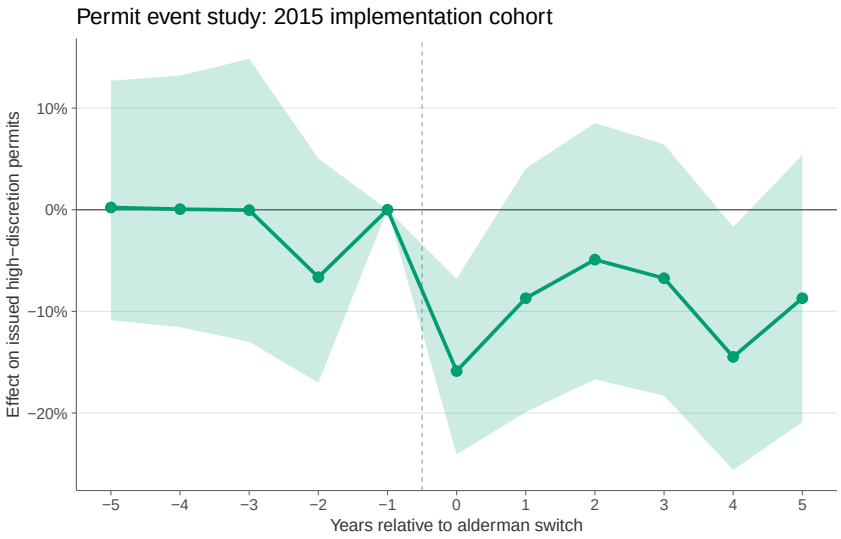


Figure 8: Permit Event Study: Effect of Alderman Stringency on High-Discretion Permits

Notes: Block-year PPML event study. Outcome is the count of high-discretion permits issued per block per year. 2015 redistricting cohort; sample restricted to blocks within 1,000 feet of ward boundaries with uniform weights. Continuous treatment: effect of a 1 SD increase in alderman stringency. Block and border-pair × year fixed effects. Year −1 omitted as reference. 95% confidence intervals based on standard errors clustered at the block level. Pooled post-period estimate: −0.092 (SE = 0.034). Joint pre-trend test $p = 0.729$.

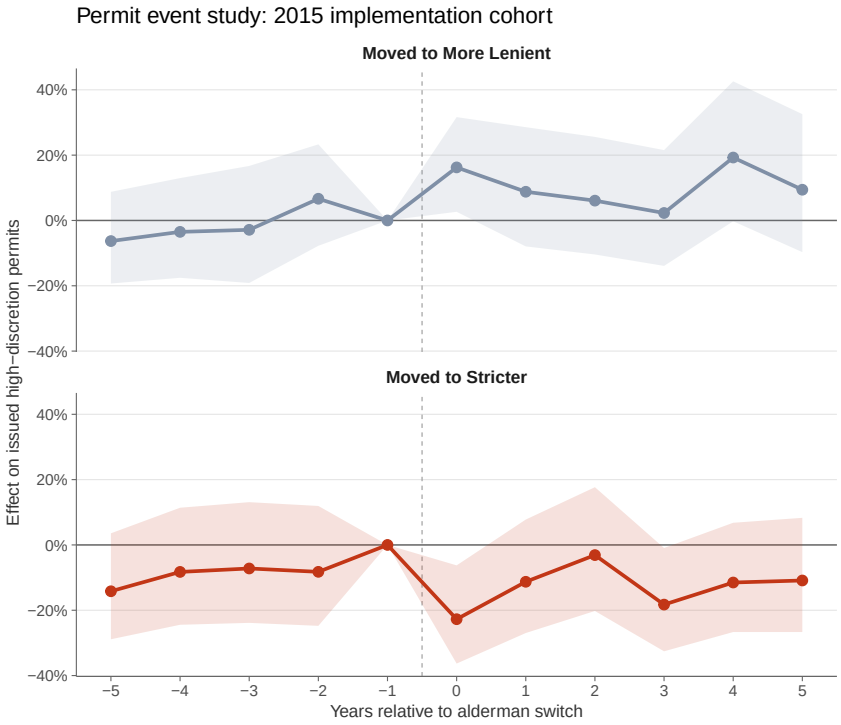


Figure 9: Permit Event Study: Continuous Split by Treatment Direction

Notes: Same specification as Figure 8, estimated separately for blocks with $\Delta\text{Stringency} > 0$ (moved to more stringent) and $\Delta\text{Stringency} < 0$ (moved to more lenient). 95% confidence intervals based on standard errors clustered at the block level.

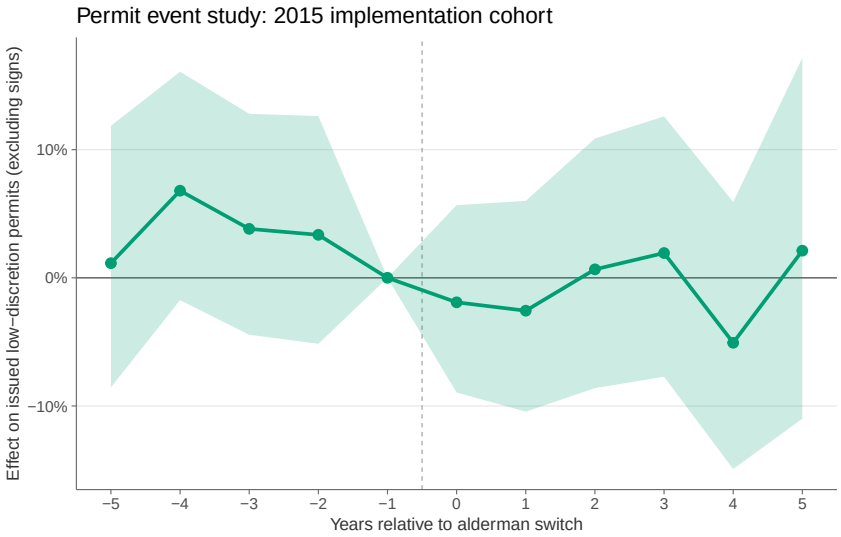
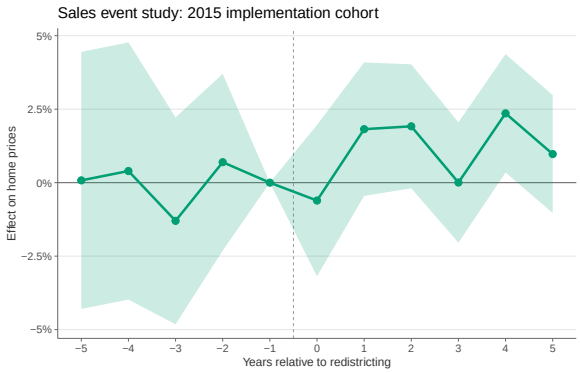
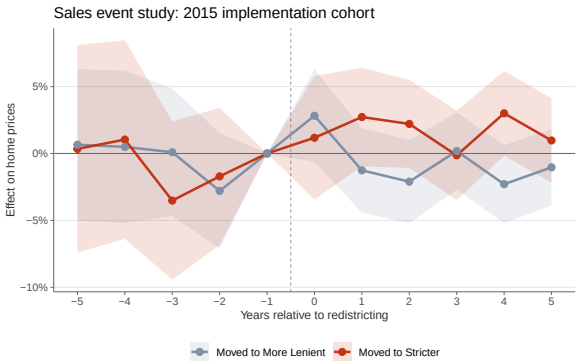


Figure 10: Placebo: Low-Discretion Permits

Notes: Same specification as Figure 8 but using low-discretion permits (excluding signs) as the outcome. The plotted coefficients are small and do not show a clear post-treatment pattern. The absence of a clear placebo response supports the interpretation that the high-discretion result operates through the discretionary channel.



Panel A: Continuous Treatment



Panel B: Continuous Split

Figure 11: Event Study: Effect of Alderman Stringency on Home Sale Prices

Notes: Panel A: continuous treatment (effect of 1 SD increase in stringency). Panel B: continuous split (separate slopes for blocks with positive vs. negative ΔStringency). 2015 redistricting cohort. Border-pair-by-side and border-pair-by-year fixed effects. Uniform weights with 1,000 ft bandwidth. Includes hedonic controls for building square footage, land square footage, building age, bedrooms, bathrooms, and garage. Year −1 omitted as reference. 95% confidence intervals based on standard errors clustered at the census block level. For Panel A, the pooled post-period estimate in the hedonic specification is 0.012 (SE = 0.005), and the joint pre-trend test gives $p = 0.937$.

Table 1: Summary Statistics of New Residential Construction

	(1) All New Construction	(2) Multifamily Only
Average FAR	1.21	2.27
Average Units	6.43	30.17
Average Stories	2.28	2.75
Average DUPAC	28.12	72.79
Median Dist. to Boundary (ft)	941.26	856.90
Average Strictness Score	0.204	0.210
Observations	14,014	2,611

Table 2: Effect of Aldermanic Stringency on Development Density

	ln(FAR) (1)	ln(DUPAC) (2)
Stringency Index	-0.10**	-0.10***
	(0.04)	(0.04)
Zoning Group FE	✓	✓
Segment FE	✓	✓
Year FE	✓	✓
N	755	755
Dep. Var. Mean	2.48	82.61
Ward Pairs	105	105

Notes: Sample includes new multifamily construction within 500 feet of ward boundaries. Regressions include own-side controls for share White, share Black, median household income, bachelor's share, and homeownership rate. Fixed effects are listed in the table. Regressions are unweighted. Stringency is the standardized aldermanic stringency score. Standard errors clustered at the ward-pair level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Effect of Alderman Stringency on High-Discretion Permits

	2015
Post × Strictness Δ	-0.0915***
	(0.0344)
Block FE	✓
Border-Pair × Year FE	✓
N	100,171
Dep. Var. Mean	0.25
Ward Pairs	108

Notes: Block-year PPML regression using the 2015 implementation cohort, restricted to census blocks within 1,000 feet of ward boundaries and relative years −5 through 5. Outcome is the number of issued high-discretion permits. Uniform weighting. Standard errors clustered at the block level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Effect of Alderman Stringency on Home Sale Prices

	(1)	(2)
Post × Strictness Δ	0.011*	0.012**
	(0.006)	(0.005)
Log Building Sqft		0.368***
		(0.012)
Log Land Sqft		0.243***
		(0.013)
Log Building Age		-0.162***
		(0.005)
Log Bedrooms		-0.055***
		(0.012)
Log Bathrooms		0.049***
		(0.009)
Has Garage		0.015**
		(0.006)
N	43,857	43,857
Dep. Var. Mean	$388,214	$388,214
Ward Pairs	134	134
Border-Pair Side FE	✓	✓
Border-Pair × Year FE	✓	✓

Notes: Transaction-level regressions of log sale price using the 2015 implementation cohort, restricted to transactions within 1,000 feet of ward boundaries and relative years −5 through 5. Both columns use the same transaction sample. Column (2) adds hedonic controls for building square footage, land square footage, building age, bedrooms, bathrooms, and garage. Uniform weighting. Standard errors clustered at the block level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A Data and Sample Construction

This appendix documents the data sources and sample construction procedures for the analysis.

A.1 Data Sources

The analysis draws on several administrative and proprietary datasets:

- **Cook County Assessor Data**: Sales records, property assessments, and improvement characteristics for residential parcels in Cook County.
- **Chicago Building Permits**: Permit applications and processing records for high-discretion permit types requiring aldermanic approval.
- **Ward Boundary Shapefiles**: Official ward boundaries for three eras: pre-2015, 2015–2023, and post-2023.
- **Census Data**: American Community Survey block group demographics and 2010/2020 decennial census block boundaries.

A.2 High-Discretion Permits: Sample Construction

The building permits data come from the City of Chicago's open data portal and include all permit applications and processing records.

Sample Restrictions. I restrict the sample to "high-discretion" permit types that are most likely to require aldermanic engagement and face delays due to community meetings or aldermanic intervention. Specifically, I include permits designated as: (1) New Construction; (2) Renovation; (3) Demolition; (4) Porch Construction; and (5) permits that were reinstated after being previously revoked.

All remaining permit types are excluded from the high-discretion sample. In placebo analyses, low-discretion permits are defined as the non-high-discretion complement, excluding signs.

Processing Time. For each permit, I compute processing time as the number of days between the application date and the issue date. Permits with missing dates or negative processing times are excluded.

Ward Assignment. Permits are assigned to wards based on the permit's geographic coordinates and the ward boundaries in effect at the time of application.

A.3 New Construction: Sample Construction

The new construction sample for the density analysis is drawn from the Cook County Assessor's parcel-level data.

Sample Restrictions. I apply the following restrictions: (1) lot area exceeds 1 square foot; (2) building area exceeds 1 square foot; (3) at least one dwelling unit; and (4) construction year is 2006 or later. These restrictions exclude vacant parcels, data errors, and older buildings that predate the sample period.

Density Measures. For each parcel, I compute two density measures: Floor Area Ratio (FAR), defined as total building square footage divided by lot area; and Dwelling Units Per Acre (DUPAC), defined as the number of dwelling units divided by lot acreage.

Ward and Boundary Assignment. Each parcel is assigned to a ward based on its centroid location and the ward boundaries in effect at the time of construction. Distance to the nearest ward boundary is computed in feet using the Illinois East State Plane coordinate system.

A.4 Summary Statistics: New Construction Sample

Table 1 presents summary statistics for the new residential construction sample used in the density analysis. The sample includes all new residential buildings constructed in Chicago

between 2006 and 2025 that meet the restrictions described in Section A.3. Column (1) reports statistics for the full sample, while Column (2) restricts to multifamily buildings with 2 or more units.

The full sample contains 14,014 new residential buildings with an average of 6.43 dwelling units and 2.28 stories. The average Floor Area Ratio (FAR) is 1.21 and the average Dwelling Units Per Acre (DUPAC) is 28.12. Multifamily buildings are substantially denser, with an average of 30.17 units, 2.75 stories, FAR of 2.27, and DUPAC of 72.79. Median distance to the nearest ward boundary is 941 feet in the full sample and 857 feet in the multifamily sample.

A.5 Home Sales: Sample Construction

The home sales data come from the Cook County Assessor's Office and include all residential property transactions in Chicago from 1999–2025.

Sample Restrictions. I apply the following restrictions to construct the analysis sample: (1) residential property classes only, including single-family homes (classes 202–210) and small multifamily buildings with 2–6 units (class 211), excluding mixed-use properties (class 212) and condominiums (classes 299, 399); (2) sale price exceeds $10,000 to exclude nominal transfers and gifts; (3) market transactions only, defined as warranty or trustee deeds; (4) excludes land-only sales; (5) seller and buyer names are valid and non-identical, excluding intrafamily transfers; and (6) single-parcel transactions only.

Price Winsorization. Sale prices are first deflated to 2022 dollars using monthly Chicago CPI excluding shelter and then winsorized at the 1st and 99th percentiles to limit the influence of outliers on mean calculations.

Geocoding. Each sale is geocoded by joining the property identification number (PIN) to the Cook County parcel universe, which contains latitude and longitude coordinates. Sales

that cannot be matched on the full 14-digit PIN are matched on the first 10 digits (PIN10).

Ward Assignment. Sales are assigned to wards based on the ward boundaries in effect at the exact sale date. The ward map changes on May 18, 2015 and May 15, 2023. For the event study analysis, sales before May 18, 2015 use pre-2015 boundaries; sales between May 18, 2015 and May 15, 2023 use 2015 boundaries; and sales after May 15, 2023 use post-2023 boundaries.

Distance to Ward Boundary. Distance calculations follow the same procedure as for new construction (Section A.3), using the Illinois East State Plane coordinate system (EPSG:3435) and computing signed distance based on relative alderman strictness.

A.6 Event Study Panel Construction

For the event study analyses in the compiled paper, I construct a block-level treatment panel around the 2015 redistricting. That treatment panel is used directly for the permit event study and merged into transaction-level home sales data for the price analysis.

Treatment Assignment. Blocks are classified as treated if they were redistricted from one ward to another. In the 2015 cohort used in the compiled paper, I compare ward assignments under the pre-2015 map (2014 boundaries) to post-2015 (2015 boundaries). Census blocks are assigned to wards based on their centroid location.

Predetermined Strictness. Treatment intensity is measured using the difference between the destination and origin aldermen's baseline strictness scores. In the compiled paper's 2015-cohort design, origin and destination aldermen are assigned using the 2014 pre-redistricting ward-to-alderman mapping. The change in strictness for block i is $\Delta\text{Strictness}_i = \text{Strictness}_{\text{dest}} - \text{Strictness}_{\text{origin}}$. For binary treatment specifications, blocks are classified as "moved to stricter" if $\Delta\text{Strictness}_i > 0$ and "moved to more lenient" if $\Delta\text{Strictness}_i < 0$.

Contaminated Control Exclusion. Control blocks may experience treatment through electoral turnover (alderman changed via election rather than redistricting). In the compiled paper's 2015-cohort design, I identify wards with alderman turnover between 2014 and 2015 and exclude non-switching blocks in these wards from the control group.

Analysis Panels. Permit outcomes are aggregated to block-year panels for the event-study analysis. In the compiled paper, the permit event study uses the 2015 implementation cohort with relative years -5 through 5. Home sales are not aggregated in the active compiled analysis: the price event study is estimated on transaction-level data for the 2015 implementation cohort over the same relative-year window, with hedonic controls and border-pair-side plus border-pair-by-year fixed effects.

B Additional Permit Evidence

This appendix collects the permit summary tables that complement the descriptive discussion in Section 2.

B.1 Permit Summary Statistics

Table B.1 compares the high- and low-discretion permit groups used throughout the paper. High-discretion permits are much slower on average and display substantially more cross-alderman heterogeneity in mean processing times, consistent with the idea that these permits are most exposed to discretionary review.

B.2 Permit Volume and Processing Time

Table B.2 reports the correlation between individual permit processing time and ward-month permit volume. The negative correlation for high-discretion permits is consistent with political and regulatory friction reducing throughput where discretionary review is most important, while the positive correlation for low-discretion permits suggests routine administrative scale rather than aldermanic discretion.

Table B.1: Summary Statistics for High- and Low-Discretion Permits

Statistic	High-Discretion	Low-Discretion
Total permits in sample	137,983	152,747
Mean processing time (days)	59.8	24.1
Median processing time (days)	32.0	6.0
Alderman-level mean of processing time	62.4	24.5
Alderman-level IQR of mean processing time	28.7	13.2
Number of unique wards	50	50
Number of unique aldermen	127	127

Notes: High-discretion permits are new construction, renovation/alteration, wrecking/demolition, porch construction, and reinstated permits. Low-discretion permits are all remaining permit types excluding signs. The alderman-level interquartile range is computed from the cross-section of alderman-specific mean processing times within each permit group.

Table B.2: Processing Time and Permit Volume Correlation

Permit group	Corr. of processing time and ward-month permit volume
High-Discretion	-0.099
Low-Discretion	0.074
All	-0.018

Notes: Entries report the Pearson correlation between individual permit processing time and ward-month permit volume for each permit group.

C Alderman Fixed Effects and Index Validation

This appendix provides additional detail on the construction and validation of the aldermanic strictness index.

C.1 Construction of the Strictness Index

Alderman strictness is measured using permit processing times from the high-discretion permit sample described in Section A.2, residualized to remove the influence of ward characteristics.

Stage 1: Residualization. I regress permit-level log processing time on ward geographic fundamentals (distance to the CBD, distance to the lake, CTA station density), demographic controls (homeownership rate, population, median household income, racial composition), lagged ward-month permit volume, and fixed effects for month, permit type, and review type.

Stage 2: Alderman Fixed Effects. I aggregate the Stage 1 residuals to ward-month mean residuals and regress those ward-month averages on alderman fixed effects and the same lagged permit-volume control, weighting observations by ward-month permit counts. The alderman fixed effect captures the systematic component of processing time attributable to the alderman.

Empirical Bayes Shrinkage. Raw alderman fixed effects are shrunk toward the grand mean using an empirical Bayes procedure: $\hat{\theta}_j^{\text{EB}} = B_j \cdot \hat{\theta}_j$, where $B_j = \hat{\tau}^2/(\hat{\tau}^2 + \hat{\sigma}_j^2)$ is the shrinkage factor.

Index Standardization. The final strictness index is standardized to have mean zero and unit standard deviation across aldermen. Higher values indicate stricter aldermen.

Table C.1 reports the baseline Stage 1 residualization regression. The dependent variable is permit-level log processing time. The specification controls for ward fundamentals and lag permit volume while partialing out month, permit-type, and review-type fixed effects, since review and permit-type composition may be endogenously determined by the regulatory environment. This stage is meant to remove systematic differences in permit timing attributable to observable ward characteristics so that the resulting strictness score isolates the alderman-specific component.

C.2 Validation of the Strictness Index

The strictness index is intended to capture alderman-specific regulatory frictions rather than purely ward-level conditions. Table C.2 shows that wards with higher stringency scores issue fewer high-discretion permits and exhibit more signs of project modification: a larger share of permits with any unit change and more units reduced per permit. Figure C.2 complements this by comparing predecessor and successor aldermen within the same ward. The weak relationship indicates that the index is not simply reflecting time-invariant ward characteristics.

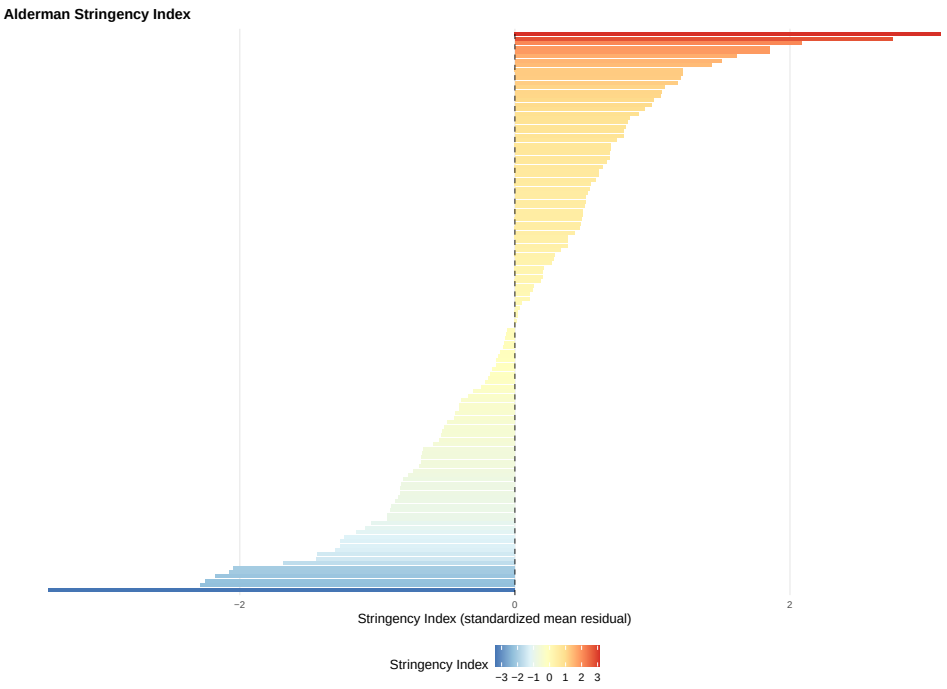


Figure C.1: Distribution of Aldermanic Stringency Scores

Notes: Stringency scores are constructed from residualized permit processing times with empirical Bayes shrinkage. Scores are standardized to mean zero and unit standard deviation. Higher values indicate aldermen associated with longer permit processing times after controlling for ward characteristics and permit-level fixed effects.

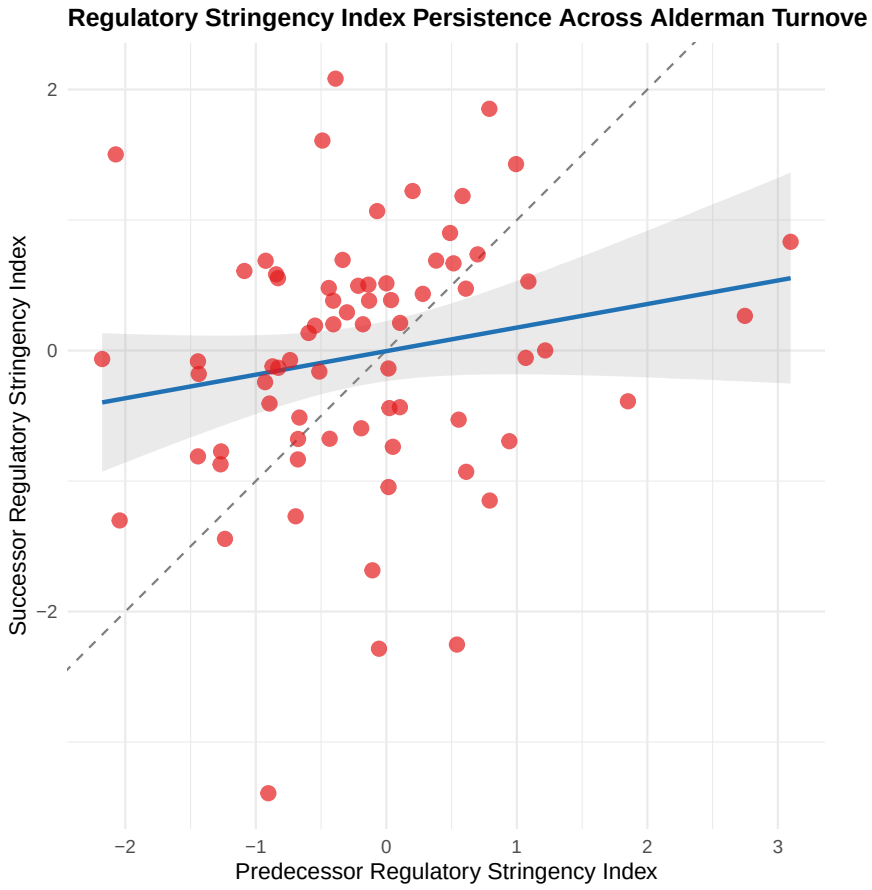


Figure C.2: Predecessor and Successor Alderman Strictness Within the Same Ward

Notes: Each point compares the strictness score of an alderman to that of the predecessor or successor who represented the same ward in an adjacent period. The low correlation indicates that the strictness measure is not simply a persistent ward-level attribute.

Table C.1: Stage 1 Residualization: Permit-Level Log Processing Time on Ward Fundamentals

	Log Processing Time
	(1)
Median HH Income ($10k)	-0.015***
	(0.005)
Share Black	0.057
	(0.077)
Share Hispanic	0.154***
	(0.053)
Share White	0.427***
	(0.084)
Homeownership Rate	0.198***
	(0.067)
Population (10k)	-0.058***
	(0.012)
Dist. to CBD (km)	-0.012***
	(0.002)
Dist. to Lake (km)	0.003
	(0.002)
CTA Stations (800m)	-0.022***
	(0.002)
Lag Permits	-4.34×10^{-5}
	(0.0006)
Year × Month FE	✓
Permit Type FE	✓
Review Type FE	✓
N	137,087

Notes: Permit-level OLS regression from the baseline uncertainty-index specification. Covariates are median household income, racial shares, homeownership, population, distance to the CBD, distance to the lake, CTA station density, and lag permit volume. Fixed effects are listed in the table. Regressions are unweighted. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.2: Validation of the Strictness Index Using Permit Outcomes

Outcome (Ward-Month)	Coef. on Stringency Index
N permits (all high-discretion)	-16.48^{***}
	(0.36)
Share with any unit change	0.0103^{***}
	(0.0008)
Units reduced per permit (text)	0.0088^{***}
	(0.0026)
N	10,753

Notes: Ward-month regressions of permit outcomes on the standardized strictness index, using high-discretion permits and weights equal to ward-month permit counts. Outcomes are the number of high-discretion permits, the share of permits with any unit change, and units reduced per permit based on permit text. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

D Additional Density Results

D.1 Border Segments

Figure D.1 shows the ward-boundary segments that enter the density analysis after applying the pruning procedure described in the text. The retained segments span the city and clarify the local comparisons underlying the spatial discontinuity design.

D.2 Alternative Fixed Effects: Ward-Pair

Table D.1 replaces segment fixed effects with ward-pair fixed effects, which pool across all segments of a given ward pair. This is a coarser comparison—ward-pair borders can stretch for miles through different neighborhoods—but it uses more variation. The estimates remain negative, but they are attenuated relative to the main segment FE estimates in Table 2.

D.3 Pruned Sample

Table D.2 drops ward-pair-era combinations where confounding boundary changes (e.g., simultaneous zoning remaps or major infrastructure projects) may contaminate the comparison. The estimates remain negative and are larger in magnitude than in the full sample.

D.4 Bandwidth Robustness

Tables D.3 and D.4 present the main segment FE specification at alternative bandwidths. At 250 feet the estimates are similar in magnitude but based on fewer observations. At 1,000 feet the estimates attenuate, consistent with the effects being concentrated near the boundary where aldermanic jurisdiction changes.

D.5 Donut RD Robustness

One concern with spatial RD designs is that observations precisely at the boundary may be unusual—for example, if ward boundaries follow major streets that independently affect development. Figure D.2 implements a donut specification (Barreca et al., 2011; **?**) that excludes parcels within 25 or 50 feet of the boundary. In both donut plots, the fitted discontinuity remains negative, suggesting the main pattern is not driven only by parcels right at the cutoff.

D.6 Placebo Boundary Tests: DUPAC

Figure 5 in the main text shows placebo tests for Log(FAR). Figure D.3 presents the same tests for Log(DUPAC). Relative to the true boundary plots, the placebo plots are flatter around the shifted cutoffs and do not show the same visible jump.

D.7 All Construction

The main analysis restricts to multifamily buildings with 2 or more units. Figure D.4 includes all new construction, including single-family homes. The plotted discontinuity appears smaller than in the multifamily-only sample, as expected given that single-family construction has less variation in density and is less subject to aldermanic discretion.

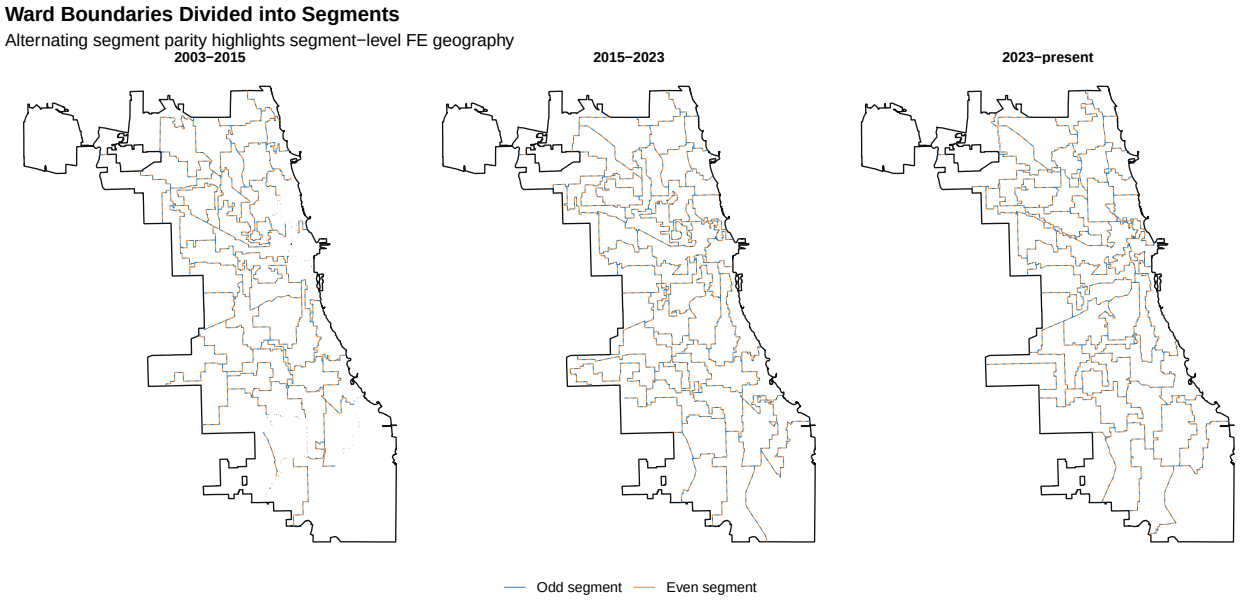


Figure D.1: Ward-Boundary Segments Used in the Density Analysis

Notes: Map shows the ward-boundary segments used in the density regressions, by era. The pruning step removes boundary segments flagged as unsuitable for clean comparison because of confounding institutional or geographic features.

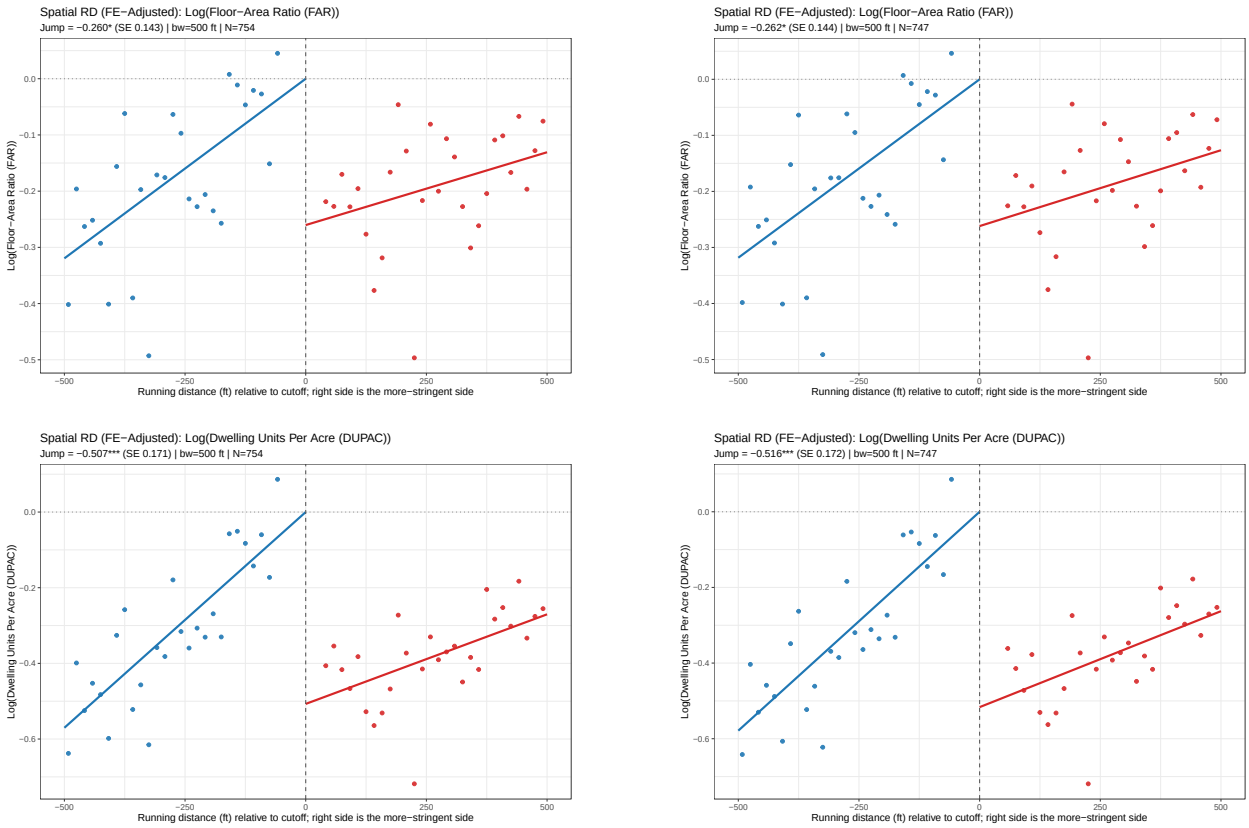


Figure D.2: FE-Adjusted Donut RD Robustness

Notes: Top row: Log(FAR); bottom row: Log(DUPAC). Left column excludes parcels within 25 feet of boundary; right column excludes parcels within 50 feet. Same FE-adjusted specification as Figure 3. The plotted fits remain negative after excluding parcels nearest the boundary.

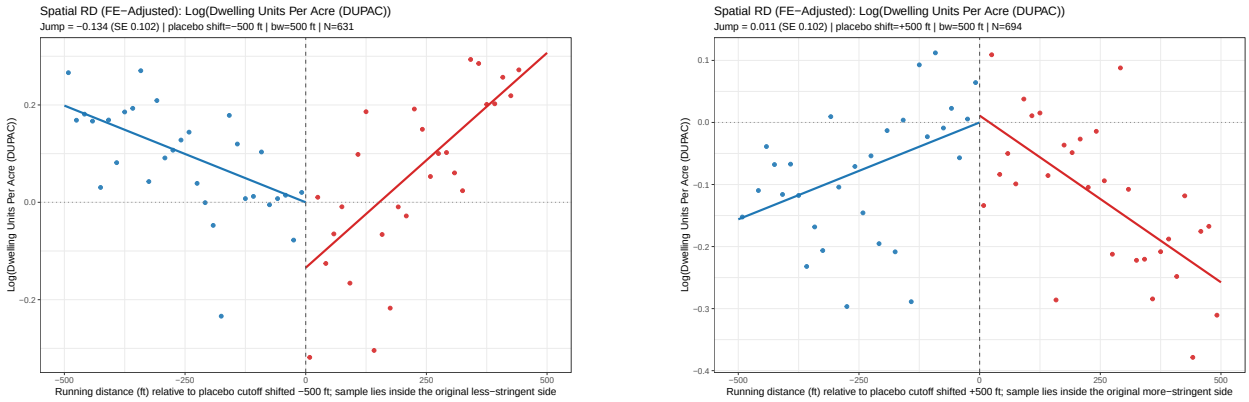


Figure D.3: Placebo Boundary Tests: Log(DUPAC)

Notes: Left: placebo cutoff 500 feet inside the lenient ward. Right: placebo cutoff 500 feet inside the strict ward. Same FE-adjusted specification as Figure 3. The placebo plots are flatter than the main boundary plot.

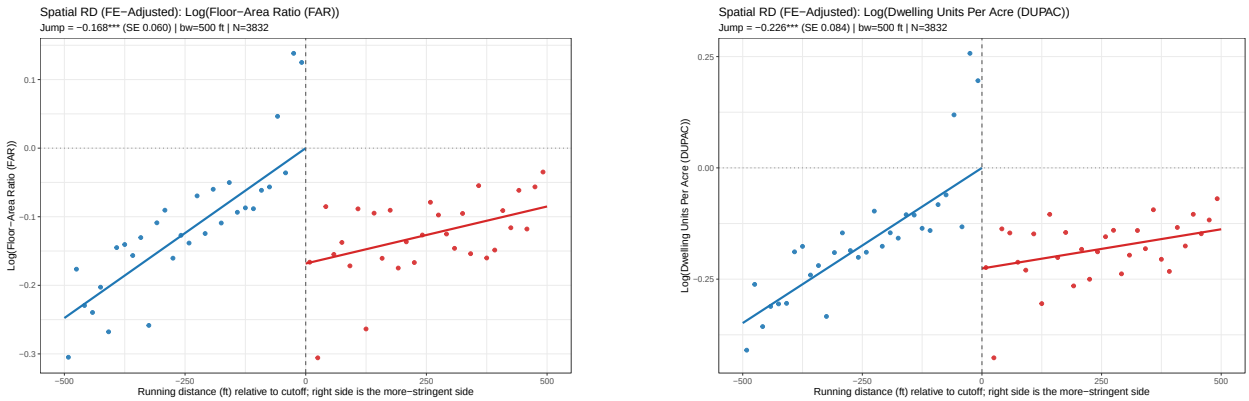


Figure D.4: FE-Adjusted Spatial RD: All New Construction

Notes: Same specification as Figure 3 but including all new construction (not just multifamily). Left: Log(FAR); right: Log(DUPAC). The plotted discontinuity appears smaller than in the multifamily-only sample.

Table D.1: Effect of Aldermanic Strictness on Development Density (Ward-Pair FE)

	ln(FAR)	ln(DUPAC)
	(1)	(2)
Stringency Index	-0.03	-0.05*
	(0.02)	(0.03)
Zoning Group FE	✓	✓
Ward-Pair FE	✓	✓
Year FE	✓	✓
N	755	755
Dep. Var. Mean	2.48	82.56
Ward Pairs	105	105

Notes: Sample includes new multifamily construction within 500 feet of ward boundaries. Regressions include own-side controls for share White, share Black, median household income, bachelor's share, and homeownership rate. Fixed effects are listed in the table. Regressions are unweighted. Stringency is the standardized aldermanic strictness score. Standard errors clustered at the ward-pair level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.2: Effect of Aldermanic Strictness on Development Density (Pruned Sample)

	ln(FAR)	ln(DUPAC)
	(1)	(2)
Stringency Index	-0.18***	-0.17**
	(0.07)	(0.07)
Zoning Group FE	✓	✓
Segment FE	✓	✓
Year FE	✓	✓
N	622	622
Dep. Var. Mean	2.23	76.07
Ward Pairs	83	83

Notes: Same specification and controls as Table 2, but dropping ward-pair-era combinations flagged as confounded in the pruning file. Regressions are unweighted. Standard errors clustered at the ward-pair level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.3: Effect of Aldermanic Strictness on Development Density (250 ft Bandwidth)

	ln(FAR)	ln(DUPAC)
	(1)	(2)
Stringency Index	-0.22*	-0.22
	(0.13)	(0.15)
Zoning Group FE	✓	✓
Segment FE	✓	✓
Year FE	✓	✓
N	440	440
Dep. Var. Mean	2.69	89.63
Ward Pairs	92	92

Notes: Sample includes new multifamily construction within 250 feet of ward boundaries. Regressions include own-side controls for share White, share Black, median household income, bachelor's share, and homeownership rate. Fixed effects are listed in the table. Regressions are unweighted. Stringency is the standardized aldermanic strictness score. Standard errors clustered at the ward-pair level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.4: Effect of Aldermanic Strictness on Development Density (1,000 ft Bandwidth)

	ln(FAR)	ln(DUPAC)
	(1)	(2)
Stringency Index	-0.07**	-0.06**
	(0.03)	(0.02)
Zoning Group FE	✓	✓
Segment FE	✓	✓
Year FE	✓	✓
N	1,325	1,325
Dep. Var. Mean	2.36	76.95
Ward Pairs	115	115

Notes: Sample includes new multifamily construction within 1,000 feet of ward boundaries. Regressions include own-side controls for share White, share Black, median household income, bachelor's share, and homeownership rate. Fixed effects are listed in the table. Regressions are unweighted. Stringency is the standardized aldermanic strictness score. Standard errors clustered at the ward-pair level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.