The Spatial Incidence of Affordable Housing Policy

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(Job Market Paper)

October 25, 2026

Abstract

How should policymakers design affordable housing policy? Facing challenges in affordability and supply, many cities have turned to inclusionary zoning (IZ) policies, offering subsidies to encourage the integration of income-restricted units into market-rate developments. We study a long-running IZ program in Seattle, showing these policies change where housing is built and affect the neighborhoods they are built in. Although program requirements are uniform, differences in local conditions change developer responses. We document a trade-off: in higher-income neighborhoods, rent discounts for subsidized units are larger, but developer participation is more limited. Using scraped Craigslist rental data and spatial difference-in-differences, we further show that IZ lowers rents in lower-income areas but raises them in higher-income areas, consistent with increased competition in the former and building-driven amenity changes in the latter. To interpret these patterns, we build a quantitative urban model with developer choice and rationed affordable housing, showing how policy design and implementation choices can significantly distort policy effects. Affordable housing programs may expand access to high-quality housing for lower-income residents but may also contribute to gentrification in wealthier areas, resulting in mixed welfare effects.

Keywords: affordable housing; inclusionary zoning; neighborhood spillovers; quantitative urban model; rental housing; public finance; spatial difference-in-differences
JEL Classification: H25, H71, R13, R31, R38

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‡This paper was prepared in part by Yvonne Ng in her personal capacity during her graduate studies. The views expressed in this paper are the authors' own and do not reflect the views of the Postal Regulatory Commission, or the United States government. We are deeply grateful to Kelli Larsen and Laura Hewitt Walker at the Seattle Office of Housing and Kyle Crowder for providing data access. For helpful comments, we thank Arthur Acolin, Ling Ling Ang, Yu-chin Chen, Gregg Colburn, Brian Greaney, Alan Griffith, Rachel Heath, Jason Kerwin, and seminar participants at WEAI, AREUEA, APPAM, and PNW Labor Day. All errors are our own.

1 Introduction

How does spatial heterogeneity affect the incidence of public policy subsidies? Many policies operate through market incentives, but households and firms can often choose whether and where to participate. When local conditions affect these choices, uniform public policy can generate sharply heterogeneous effects across space, with broader general equilibrium consequences.

Affordable housing programs provide a natural setting to study this question, because they typically pursue two, often conflicting, goals of expanding housing supply and maintaining access to affordable housing for low-income residents. Yet their effectiveness depends on both developer participation and households' ability to "vote with their feet" (Tiebout, 1956). Ensuring affordability may distort supply (e.g., rent control), while subsidizing supply may displace low-income residents (e.g., gentrification). And with half of renters in the U.S. considered to be cost-burdened (U.S. Census Bureau, 2024), constraints on housing have the potential to hamper growth (Hsieh & Moretti, 2019) and long-term opportunity (Chetty, Hendren, & Katz, 2016).

Inclusionary zoning (IZ) policies, the most common form of affordable housing policy in the United States[1], promise to address both affordability and supply issues at once. Cities incentivize developers to set aside a portion of units in new market-rate buildings (typically 20-25%) as designated low-income units, through tax credits, expedited permitting, or zoning bonuses.[2] Proponents claim that IZ can provide affordable units with less market distortion than rent control, while also promoting social and economic integration through mixed-income housing. These policies, however, because they are subject to endogenous developer choices in location and density, raise unanswered questions about their efficacy and efficiency in reducing rents and increasing housing supply. As IZ programs have proliferated, there remains scant evidence on how these programs influence where developers build or where residents choose to live, much less their spillover effects on neighborhoods or their overall impact on welfare.

This paper evaluates Seattle's inclusionary zoning program, known as the Multi-Family Tax Exemption (MFTE) program. We first document the program's key tradeoffs between supply and affordability. We then causally estimate effects of new MFTE buildings on neighborhood rents. Using these reduced-form results, we build a quantitative urban model to understand how affordable housing programs can redistribute housing and households across space. Policy parameters affect developer choices, which in turn affect neighborhoods, which in turn reallocate households.

[1] In 2019, there were programs in 734 localities in 31 states (Wang & Balachandran, 2023). In contrast, 32 states *ban* local rent control policies.

[2] IZ policy varies widely across jurisdictions, so the exact definition of "IZ" can be unclear. We consider all programs that result in units set aside as rent-restricted in new construction as "IZ", regardless of funding method.

Descriptively, we find significant variation across neighborhoods in the differentials between MFTE and market-rate rent, with "affordable" rent *exceeding* market rates in some areas. For instance, when compared to surrounding housing, a 551-700 sq. ft., one-bedroom MFTE unit is on average about $500 cheaper in Downtown but about $300 more expensive in Rainier Valley. Although the latter may appear inconsistent with the program's affordability goals, the policy may fulfill its housing supply goals by making participation effectively more generous to developers. Moreover, because MFTE units are generally part of new construction and offer restricted rent growth, tenants may be willing to pay a premium relative to nearby units while still viewing them as attractive affordable options. More broadly, rent differentials increase with neighborhood income, unit size, and number of bedrooms.

We show that developers fully internalize tradeoffs between program costs and benefits. MFTE development only occurs when profitable, and exhibits stronger clustering at the neighborhood level than the general mix of multi-family residential development, suggesting that MFTE program design affects neighborhood choices across developers. Looking at programmatic changes, when Seattle policy tightened affordability of MFTE units in 2019, developer participation significantly decreased and was constrained to lower-cost neighborhoods, where restrictions were least binding. This neighborhood variation suggests that the MFTE program not only changes how much housing is built, but *where* it is built. Developer participation constraints affect stated city goals of providing access to high-opportunity areas for low-income households, with potentially large effects on future outcomes (Chetty, Friedman, Hendren, Jones, & Porter, 2026; Chetty et al., 2016).

We examine the effect of MFTE development on surrounding neighborhoods using a novel, granular dataset of Craigslist rents and a spatial difference-in-differences approach (Asquith, Mast, & Reed, 2023; Diamond & McQuade, 2019). By comparing rents nearer MFTE development sites to those slightly further, we estimate the causal impact of the construction of an MFTE building. While neighborhoods are endogenous, exact location can be treated as exogenous given hyperlocal frictions in the land assembly and development approval process. To account for multiple buildings and building sizes, our preferred specification constructs a continuous treatment intensity using the number of nearby newly-constructed MFTE units. Additionally, as a check against neighborhood clustering, we run an earlier-vs-later design comparing areas exposed to MFTE development earlier in the sample to those exposed later.

MFTE development has a heterogeneous effect on surrounding neighborhoods, with significant rent increases for listings in the highest-income areas, but little to no effect—and in some cases rent decreases—in middle- and lower-income areas. These patterns are robust to alternative specifications of treating each building as a separate event, and appear specific to MFTE buildings. Placebo tests on the set of all new apartment construction and the set of Low-Income Housing Tax Credit (LIHTC) buildings yield mostly null

results, except that LIHTC has a small negative effect in high-income areas.

These results contrast with the existing literature, which has overwhelmingly focused on the Low-Income Housing Tax Credit (LIHTC) (Baum-Snow & Marion, 2009; Diamond & McQuade, 2019; Voith et al., 2022), a federal tax credit program. We interpret this as reflecting three distinguishing aspects of MFTE program design and our data. First, in lower-income neighborhoods, MFTE tends to induce new construction, and the resulting units compete directly with existing rentals for lower-income tenants. Rents may reflect preferences for living near multifamily developments or low-income households differently than home prices do. Second, in higher-income neighborhoods, MFTE adds relatively few net units, but new construction generates positive externalities that increase local desirability (Zahirovich-Herbert & Gibler, 2014). MFTE program standards allow for more variation in property features across neighborhoods compared to LIHTC, and may incentivize developers to build higher amenity buildings in high income neighborhoods and lower-amenity buildings in low income neighborhoods.[3] Finally, LIHTC developments are, in practice, nearly always fully affordable, rather than mixed-income developments containing both market-rate and income-restricted tenants (Cook, Li, & Binder, 2025), which amplifies any amenity effects from low-income tenants.

Based on our reduced-form evidence, we build a quantitative spatial model to understand how affordable housing programs can redistribute housing and households across space. We extend the canonical model (Ahlfeldt, Redding, Sturm, & Wolf, 2015) to explicitly parameterize an affordable housing program with endogenous developers and households types. On the housing supply side, developers choose both the quantity and type (affordable or market-rate) of housing to build. This choice depends on the key policy levers of an affordable housing program, including voluntary and mandatory forms (i.e. subsidy or tax), rent and income limits, and set-aside percentages. On the housing demand side, we segment households into high and low-type, with affordable housing available only to low-type households and subject to rationing.

Our model naturally incorporates both amenity (demand-side) and competition (supply-side) effects in the housing market, which helps rationalize the mixed reduced-form results from the literature. An endogenous neighborhood amenity based on percentage of high-type residents[4] allows affordable development to reduce rents in lower-income neighborhoods by expanding overall housing supply, where market-rate and affordable tenants look similar. While in higher-income neighborhoods, there are relatively few (and lucky) low-type households living in affordable housing, and so the amenity effect of new high-type residents dominates. These features show how affordable housing policy can increase housing supply and expand access to higher-productivity or higher-amenity neighborhoods for low-type households, but face tradeoffs in accomplishing both. Measuring both is essential for capturing a policy's full welfare effects.

[3] Data collection on MFTE building amenities is underway at the Office of Housing.

[4] This is a common feature in the spatial and residential choice literature to generate sorting, e.g. Almagro and Domínguez-Iino (2025); Bayer, Ferreira, and McMillan (2007); Diamond (2016); Tsivanidis (2026)

We estimate our model via simulated method of moments, matching observed developer decisions and population and housing shares. By identifying the universe of all MFTE-eligible new construction from permitting data, we can recover developer choices. We then can quantify the MFTE program's welfare effects relative to counterfactuals where the MFTE program does not exist, is mandatory, or has different programmatic requirements. Aggregate housing supply elasticities with respect to MFTE policy parameters are small, implying limited effects on total construction. Instead, policy generosity primarily changes the composition of development, inducing projects that would have been built as market-rate housing to participate in MFTE. For households, removing MFTE lowers welfare for low-type households through two channels: stronger residential sorting, which reduces their presence in high-amenity neighborhoods, and higher rents across all locations.

We build on a growing urban and public economics literature documenting the effects of inclusionary zoning and tax incentive programs in housing. The vast majority of the literature focuses on LIHTC, a U.S. federal government program that provides tax credits to developers, examining its effects on neighborhood demographic composition, rental prices, and property values (Baum-Snow & Marion, 2009; Davis, Gregory, & Hartley, 2019; Diamond & McQuade, 2019; Eriksen, 2009). These studies have found that LIHTC raises property values in low-income neighborhoods but slightly reduces property values and crowds out private development in high-income or gentrifying neighborhoods. This has led to the critique that LIHTC, as a one-size-fits-all policy, fits poorly across localities and subsidizes housing in the wrong areas (Gyourko & Glaeser, 2008). For households, Cook et al. (2025) evaluates LIHTC for social integration and tenant welfare. Our paper is most similar in spirit to Soltas (2024), which evaluates the cost-effectiveness of New York City's 421-a property tax exemption. We ask the inverse question - given Seattle policy, what does developer behavior look like? Our model builds on a large existing literature on neighborhood sorting (Bayer et al., 2007) and quantitative urban models (Redding, 2024), but there remain few general equilibrium models with affordable housing, and even fewer with features applicable to inclusionary zoning. We are closest to Favilukis, Mabille, and Van Nieuwerburgh (2022), who build a dynamic stochastic spatial equilibrium model that emphasizes the insurance value of affordable housing, and extend this framework to multiple locations.

We see four main contributions of this paper. First, we empirically show how relatively small policy design and implementation choices, voluntary participation, and spatial heterogeneity can create conditions that significantly distort policy effects.[5] Second, our granular rental data let us estimate effects on nearby rents directly, rather than inferring from property values or sale prices. Our contrasting results show how rental and asset prices may respond to subsidized housing through different channels. Third, we make

[5] Gyourko and Glaeser (2008) make this argument at the national level as a critique against LIHTC. Here, we show that heterogeneity matters even within a single locality.

methodological corrections suited to analyzing a dense, staggered policy. Affordable housing clustering reflects policy-shaped incentives rather than zoning or geography, and we must correct for contamination in our spatial difference-in-differences. Finally, we show how housing policy can induce re-sorting of residents in general equilibrium. Policies intended to redistribute across households may inadvertently redistribute economic activity across space.

2 Background and Data

We bring together several rich datasets, including administrative data from the City of Seattle and King County, a novel dataset of scraped rental listings from Craigslist, and rental price data from Co-Star, Inc. Our administrative program data covers all properties since inception, and is linked to King County records for property values and tax exemptions. Our housing micro-data encompasses both the universe of new housing development and existing rental units, including exact geographic coordinates and date. We supplement these housing data with estimates of housing supply elasticity replicated from Baum-Snow and Han (2024), and American Community Survey (ACS) data on population and employment flows to build and estimate the quantitative spatial model.

2.1 Background on Seattle's MFTE Program

First implemented in 1998, Seattle's MFTE program exempts multifamily rental housing developments from property taxes for 12 years, in return for designating 20% or 25% of units as rent and income-restricted. Rent and income restrictions are based on area median income (AMI) and adjusted for household size. While initially designed to encourage residential development in urban centers, since 2007 the program was expanded to all multi-family eligible zones and repurposed to promote affordability (Colburn, G. and Collins, D. and Wang, E., 2024). At the state level, MFTE is codified in Chapter 84.14 RCW, which defines the goals of the program as incentivizing urban housing development, including affordable housing, and encouraging urban development and density.

The MFTE program has undergone significant changes and is currently in its seventh iteration (P7), having been reauthorized with amendments six times since inception. Each "program" (P1, P2, etc.) details exact MFTE tenant income limits and percentage of allocated MFTE units per eligible building, the result of political bargaining between developers, housing officials, and city council. For example, in 2011, city councilmembers raised concerns that tax breaks had been awarded to undeserving developers, and a performance audit spurred greater monitoring and compliance regulation.

In 2019, Washington State's Joint Legislative Audit and Review Committee (JLARC) released a statewide

evaluation of MFTE. The topline conclusion was while developers created housing using MFTE, it is "inconclusive" as to whether this represents a net increase in developments overall. In response, Seattle's Office of Housing refocused its policy on affordability, tightening rent and income limits, increasing regulatory burden, and enforcing strict comparability of income-restricted and market-rate units in MFTE properties.

Overall, 303 rental properties have participated in MFTE over the program's lifetime. These contribute a total of 33,956 total housing units, of which 7,047 are income-restricted. Currently, 286 buildings have an active MFTE contract with the City and receive tax exemptions. While MFTE is the predominant form of affordable housing production in Seattle, there is limited overlap with other affordable housing initiatives, including Mandatory Housing Affordability (MHA)[6] and LIHTC. In our sample of active MFTE properties, 14 have paid into the MHA fund, and 24 received LIHTC funding (representing a quarter of all LIHTC properties in Seattle). Among these properties participating in multiple programs, $\sim 78\%$ report using the same units to satisfy the requirements of multiple programs. Thus, overlapping policies generally do not alter the relative shares of affordable and market-rate units within these buildings.

2.2 MFTE Program Data

For each MFTE project, the Seattle Office of Housing has provided exact geographic locations, counts of housing units by bedroom and bedroom type, and all permitting dates. These dates include initial application, inspection, and certificate of occupancy (CO) dates, which tracks a project from conception to construction to lease-up. The data also contains projects that were withdrawn, denied or otherwise abandoned, however applications are rarely denied and developers treat the program as-of-right. [7] We link our project data to King County assessments to determine the value of the tax exemption to developers.

Additionally, for 2022-2024, we observe unit-level data from the Annual Certification Query, an internal Office of Housing report collected from property managers. For all units in MFTE buildings, we observe exact rents, unit characteristics, and vacancy periods, allowing us to calculate counterfactual rents for MFTE units. Additionally, for actual MFTE units, we observe tenant characteristics, including their gender, race, ethnicity, and income.

We source publicly available data from the City of Seattle on all building permits from 1990 to the present day. These include the exact geographic location of the project, application date, issuance date, net new housing unit counts, estimated project cost, an indicator for whether the permit was for construction or an addition/renovation, and an indicator for whether the permit is for a multi-family or single-family

[6] MHA requires developers in certain areas to provide affordable units or pay into an affordable housing fund, effectively acting as the tax counterpart to MFTE's subsidy. Krimmel and Wang (2023) find limited effects on affordable housing supply as development shifts to unaffected areas.

[7] We observe only seven denials in the 28 year history of the program, or less than 2% of total applications. 84 properties withdrew their application (22 properties after approval and 62 properties before a decision).

residence. These allow us to construct exact timelines for permitting, construction, and lease-up, which will be important for our empirical strategy.

2.3 Neighborhood Rental Listings and Prices

For data on rental listings and prices, we primarily use data on rental listings scraped from Craigslist, a classified ads website. The data runs from February 2017 through November 2023 for the Seattle metro area, totaling around 150,000 listing-month observations. While landlords' usage of Craigslist may vary across regions and months, studies have employed scraped Craiglist data to study the effects of new housing development, such as their impacts on gentrification and renter displacement (Pennington, 2021). Furthermore, Hess et al. (2021) finds that Craigslist listings may offer a more comprehensive data source for rental housing listings than at least other online platforms, finding that "the disproportionately commercial profile of properties advertised on Apartments.com captures a more socioeconomically selective range of neighborhoods than Craigslist, where both commercial and "mom and pop" landlords are likely to post rental vacancy advertisements."

We supplement these with 2023 point-in-time rental estimates from CoStar. These are timed to match the Annual Certification Query data above, and are broken down by neighborhood, bedrooms, and square feet. While this lacks the granularity of the Craigslist data, this does ensure exact comparability of observables between MFTE property units and neighborhood units.

2.4 Household and Neighborhood Characteristics

We compile several data sources as inputs into our quantitative spatial model. First, for population and commuting flows, we use Census Bureau's Longitudinal Employer-Household Dynamics (LEHD) Origin-Destination Employment Statistics (LODES) data for detailed information on where people live and work at the Census tract and block levels. Second, we use Census 5-year ACS estimates for neighborhood demographics and median income. Third, we replicate Baum-Snow and Han (2024) to estimate housing supply elasticites for all Census tracts in Seattle, adjusting their methodology for 2020 Census tracts.

3 MFTE Program Trade-offs

We provide descriptive evidence that illustrate the key policy trade-offs in the city's design of the MFTE program. The Seattle Office of Housing makes a multitude of policy choices which influence developer incentives across neighborhoods. The Office of Housing must set rent limits, income eligibility thresholds, and MFTE

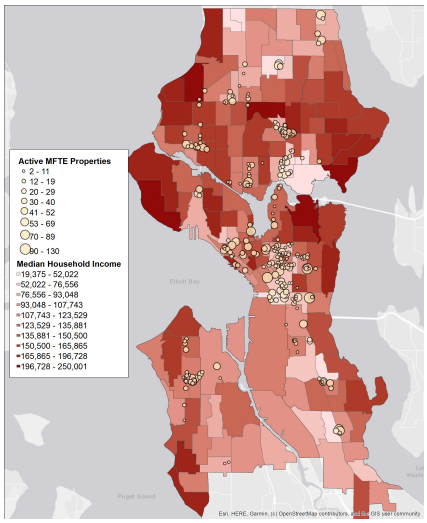
unit comparability with market rate units in terms of features. Developers, in turn, participate only where the amount of exempted taxes outweigh the rent discounts that they are required to provide on MFTE units. We highlight four features: (1) Affordable rent savings and developers' incentives vary by neighborhood; (2) Developers only join MFTE when profitable; (3) Developers are selective about neighborhood, but cannot pick the exact plot; and (4) Developers respond to policy changes.

3.1 Policy Incentives and Developer Participation

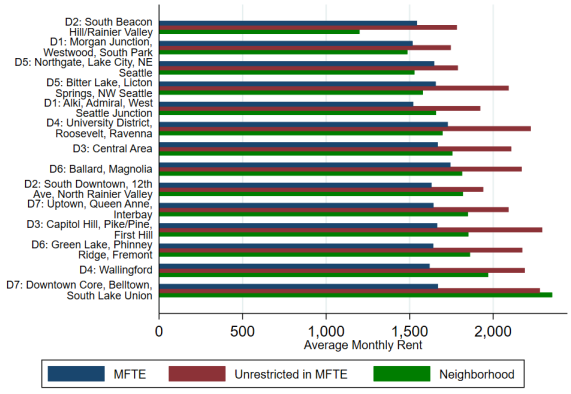
While Figure 1(a) shows that MFTE properties are spread throughout the city, in both high and low income neighborhoods, Figure 1(b) compares rents across market-rate units in MFTE buildings, income-restricted units in MFTE buildings, and neighborhood rents for twelve subdistricts in Seattle.[8] MFTE rent is stable, reflective of MFTE policy, and lower than market-rate units in the same building. On the other hand, MFTE rents can be similar and sometimes even higher than rents in the surrounding neighborhood. For example, in Rainier Valley, a MFTE one-bedroom units (551–700 sq. ft.) rent for about $300 more than comparable market-rate units. Rent differentials increase with neighborhood income, and while MFTE primarily incentivizes studio and one-bedroom apartment program, two-bedroom rent differentials are also higher. These patterns reflect where MFTE is most feasible - not the richest neighborhoods but also not poorest neighborhoods, as MFTE tenants must still meet minimum income requirements, and MFTE units are often higher quality than typical market-rate units. In qualitative surveys conducted by Colburn, G. and Collins, D. and Wang, E. (2024), developers themselves discuss where they are most incentivized to build:

> "this program operates very much within the market. So where buildings are already feasible to build is where you see the most MFTE activity."
>
> "... you're in Rainier Beach, the overall project is going to be very difficult to pencil, because the market rents aren't there ... a very narrow gap between market and affordable rents would make that MFTE incentive very accretive."
>
> "If you compare some of these rents, there's really no difference. So there's no public benefit being provided at all. Zero, zero, zero. So why not do MFTE? It's a no-brainer"

[8] For more comparisons by other margins, such as number of bedrooms and square footage, see Appendix Figure A.2 and Figure A.3.



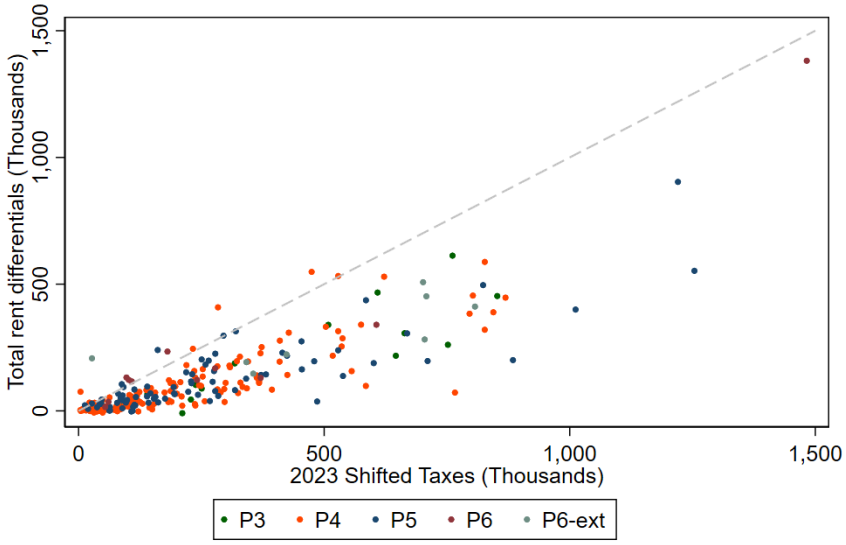
(a) Census tract median income in Seattle



Source: 2023 Annual MILU Certification Query and CoStar, 1520 MFTE units, 551-700 sq.ft.

(b) Comparison of MFTE and market rents for 1-bedroom, 551-700 sq. ft. units

In Figure 2, we compare total rent differentials[9] and the value of the property tax exemption for 2023. Developers have relatively accurate forecast of future rents and are (rationally) placing MFTE buildings only when profitable, as most of the points–each one representing a MFTE building–lie below the 45 degree line. This pattern holds for all programs iterations, as well as for building size (larger buildings with higher property values would have paid higher taxes). This also does not include fixed costs of program compliance, such as finding and income certifying tenants or submitting yearly documentation.



Source: Annual MILU Certification Query.
Each MFTE unit is matched to comparable units based on unit type (dwelling/SEDU/sleeping room), # of bedrooms, bedroom type (open/standard), and within 50 sq ft

Figure 2

[9]These are calculated via matching comparable affordable units to market-rate units in the same building based on bedrooms and square footage. They can be interpreted as benefits to residents or costs to developers

3.2 Policy Variation and Neighborhood Heterogeneity

MFTE buildings exhibit different clustering behavior than other multifamily developments, suggesting that program design changes where developers build, not just whether they participate. We use the universe of Seattle permits and the kernel-density approach of Duranton and Overman (2005) to compare spatial distributions. For each group, we compute all bilateral distances between permits and compare it to the distribution produced by drawing 999 random samples of the same size from a reference population. Our preferred reference population includes all multifamily (MF) starts in MF-eligible zoning along with all non-MF residential permits in MF zones on lots of at least 5,000 sq. ft (n= 7,792).[10] The shaded band in Figure 3 is the 95% global confidence envelope of Myllymäki, Mrkvička, Grabarnik, Seijo, and Hahn (2017), which controls familywise error across the full range of distances. [11]

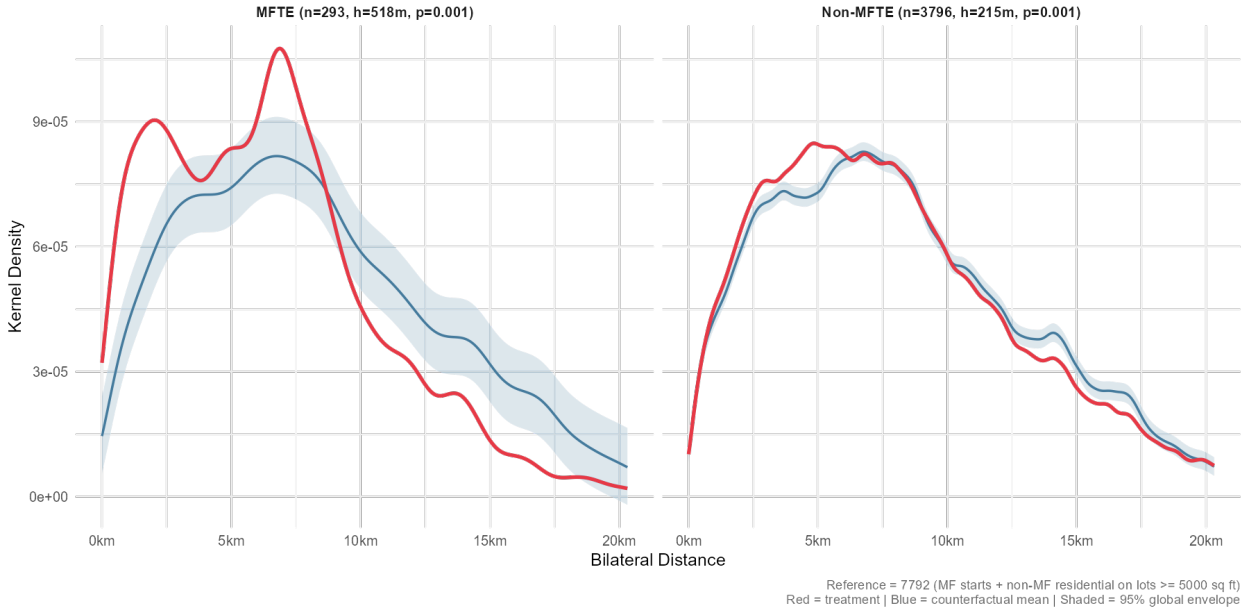


Figure 3: Kernel density of bilateral distances between MF permits, MFTE (left) and non-MFTE (right). Red: observed. Blue: mean across 999 random draws from the feasibility-conditioned reference. Shaded: 95% global envelope.

While both groups reject the null of complete random location, MFTE and Non-MFTE clustering look remarkably different. Non-MFTE produces a single broad mode at roughly 5–6 km of pairwise separation, consistent with the many residential corridors throughout the city. MFTE, on the other hand, clusters more densely and more selectively than Non-MFTE, as indicated by its two peaks at 2 km and 7 km. We interpret this as MFTE buildings concentrating inside dense residential corridors, but only in a small number

[10] This captures all feasible multifamily locations regardless of whether MF was built there and correctly weighs areas with higher permitting density. Changing our criteria to all MF-eligible zones does not change our results.

[11] Bandwidths are set group-by-group via Silverman's rule: $h_{\text{MFTE}} = 518$ m and $h_{\text{non-MFTE}} = 215$ m. The wider MFTE bandwidth reflects its smaller sample ($n = 293$), so fine-grained features of the two curves are not directly comparable; the broad shape difference is.

of discrete "hot zones" rather than a smooth gradient. Beyond 10 km, MFTE density also falls well below non-MFTE density, as MFTE is nearly absent from the city's peripheral residential edges.

Both groups are evaluated against the same feasibility-conditioned reference and thus the shape difference cannot be attributed to zoning, lot size, or the geographic distribution of MF-eligible parcels. Rather, our interpretation is that developer incentives and market rent conditions favor a set of high-rent corridors where the exemption offsets forgone market rent and low-rent areas where market rents and affordable rents equalize.

We also observe policy variation that demonstrates how developers respond to changes in program design. The City has adjusted MFTE rents, income qualification limits, and unit requirements six times since the program's inception. When setting a lower rent for MFTE units, the City is essentially tightening the affordability of MFTE units, as opposed to setting a higher MFTE rent that might be closer to market rents. Tightening affordability leads developers to build only in poorer neighborhoods, whereas relaxing affordability incentivizes developers to build in a wider breadth of neighborhoods in terms of rents and renter income.

Figure 4 shows the differences in the breadth of neighborhoods where developers participated in MFTE between each iteration of the program (see Appendix Figure A.1 for a comparison of MFTE permitting and market-rate). Generally speaking, the differences between P4, P5, and P6 are that the MFTE program enforced tighter AMI income limits and lower MFTE rents received by developers. In addition, in P6, the City greatly increased the regulatory burden of developers; e.g., the City became more specific about what amenities MFTE developers are required to include in the property's units and the income certification that is required. Previously, MFTE could be assigned to units on lower levels, worse views, etc.. However, in reaction to these "poor doors", by P6 the Office of Housing started enforcing stringent comparability requirements, effectively further tightening affordability requirements.

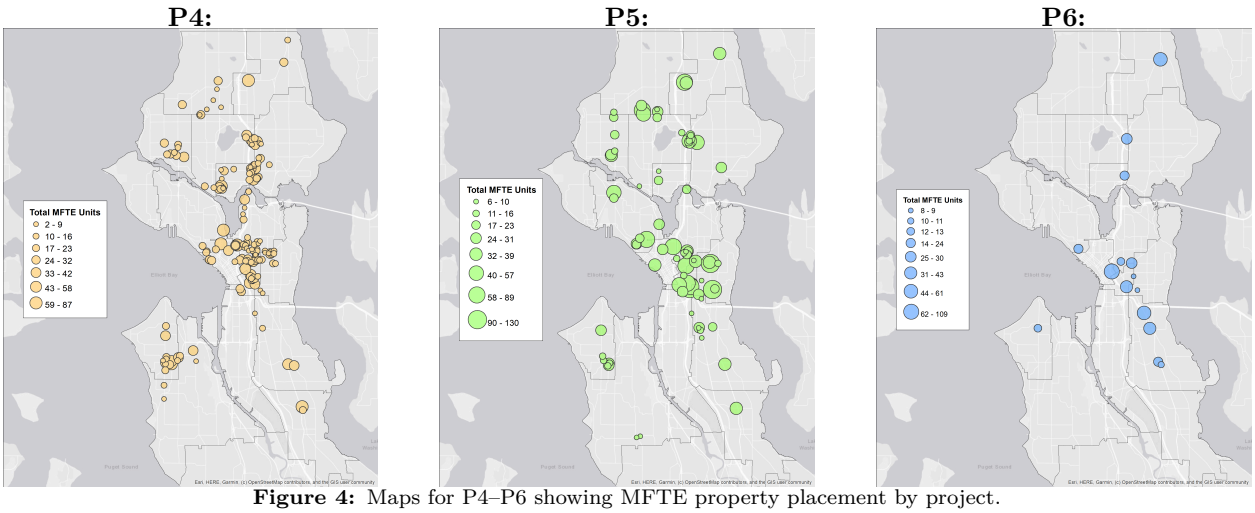


Figure 4: Maps for P4–P6 showing MFTE property placement by project.

Each iteration of the MFTE program represents a "bundle" of policy changes. We summarize the effects of these policies in Table 1. Using data on all MFTE units throughout the history of the program, we show how rents in MFTE buildings decreased during the program's P6 iteration. The first column shows that units in MFTE buildings (both rent-restricted and market rate) in the P6 iteration generally had lower rents. In the second column, we include submarket fixed effects to show that one reason for these lower rents is that developers became more selective about which neighborhoods they placed MFTE properties, choosing ones which had lower market rents. In addition, the third column controls for the size of the unit, and the contrasting sign for the coefficient on the P6 dummy between the second and third columns demonstrate that the lowered rents in MFTE buildings were partially due to smaller units built during P6 as well. In column 4, we regress on square feet directly to show that MFTE units are smaller, but that increased comparability standards seemed to reduce this effect in P5 and P6. Finally in column 5, we present suggestive evidence that vacancy rates for somewhat higher for MFTE units, although this is highly endogenous to market demand.

	Monthly Rent	Monthly Rent	Monthly Rent	Sq. Ft.	2023 Vacancy Rate
MFTE	-602.6***	-579.0***	-521.9***	-31.19***	0.0227**
	(17.33)	(16.27)	(15.41)	(4.744)	(0.00750)
Program=4	-86.77***	-91.36***	51.26***	-79.09***	0.0236***
	(10.44)	(10.01)	(8.512)	(2.941)	(0.00405)
Program=5	10.25	35.38***	123.9***	-48.86***	0.0850***
	(10.92)	(10.34)	(8.454)	(3.120)	(0.00465)
Program=6	-101.4***	-49.76***	25.76*	-39.99***	0.0954***
	(12.76)	(11.88)	(10.63)	(3.870)	(0.00703)
MFTE × Program=4	138.9***	110.9***	115.2***	-1.546	-0.0260**
	(18.30)	(17.42)	(16.43)	(5.261)	(0.00834)
MFTE × Program=5	87.87***	70.70***	47.21**	13.79*	-0.0569***
	(18.94)	(18.00)	(16.72)	(5.491)	(0.00970)
MFTE × Program=6	27.60	-12.88	-34.93	10.52	0.0502***
	(22.02)	(21.22)	(21.21)	(6.855)	(0.0147)
Sq. Ft.			1.825***		
			(0.0203)		
Constant	1677.7***	1671.8***	766.7***	495.9***	0.0674***
	(11.19)	(10.68)	(14.14)	(3.231)	(0.00487)
N	31514	31514	31514	31873	31752
Submarket FE		Y	Y	Y	Y
Bedrooms FE	Y	Y	Y	Y	Y

Table 1: Descriptive regression results showing effects of different program iterations on rental units in MFTE buildings.

4 Neighborhood Spillovers

4.1 Identification and Research Design Overview

We run three different difference-in-differences research designs to estimate the effects of MFTE buildings on nearby rents. First, we estimate the effect of new nearby MFTE buildings using a staggered difference-in-differences with a continuous treatment variable that is the sum of all new MFTE units constructed nearby (Callaway, Goodman-Bacon, & Sant'Anna, 2024). Second, we estimate the effect of a single newly constructed nearby MFTE property using a stacked difference-in-differences (Cengiz, Dube, Lindner, & Zipperer, 2019) that treats each building as a separate treatment event. Third, we compare early treated to later treated areas in a standard DiD set up (i.e. without control and treatment rings). We discuss each of these setups in more detail below, though we first discuss a few definitions and assumptions that are shared by both.

We focus on how the IZ program affects local rental affordability in the first few years after an MFTE property begins leasing. For both research designs, treatment begins in the month the property receives its Certificate of Occupancy (CO), which confirms compliance with Seattle Building Code requirements **?**. To avoid capturing short-run rent declines caused by construction noise or traffic, we exclude control listings posted during each property's construction period. We define this period as running from the later of the

MFTE application date or permit issuance date to the CO date.

To identify treatment effects in both research designs, we exploit how a development site's exact location within a neighborhood often reflects mostly random idiosyncratic factors, such as which parcel was for sale at the time (Diamond & McQuade, 2019). We assume that when the developer applies for a specific MFTE development project, these idiosyncratic factors are uncorrelated with differential changes in rent between our treated and control groups. This is essential for satisfying our identifying assumption of parallel trends in rents between Craigslist listings that are close to a MFTE building (our treatment group in the stacked difference-in-differences design) and listings that are further away (our control group).[12]

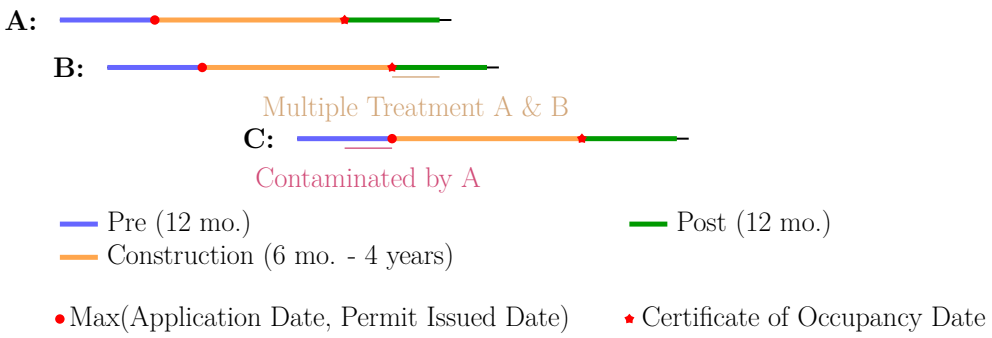
However, one challenge presented in our data is that more than half of MFTE properties together are within a mile of each other. This means that a Craigslist rental listing may be located further away from a MFTE property as a potential control unit, but its rent could be "contaminated" by the effects of another closer MFTE property. For example, in some of our results, we allow for listings that are 1 to 1.2 miles away from a MFTE building to serve as control units and listings that are within 0.6 miles to serve as treated units. In this case, some of these control units may be contaminated if they are within 0.6 miles of another MFTE property. To circumvent this challenge, we exploit the exact locations of the universe available in our dataset to drop any Craigslist rental listings from our control group that were exposed to treatment from another MFTE property.

Overall, our treatment group includes Craigslist rental listings located near a focal MFTE building—either within a circle of specified radii (0 to 0.2, 0 to 0.4, or 0 to 0.6 miles) or within rings (annuli) of defined widths (0.2 to 0.4, 0.4 to 0.6, or 0.6 to 0.8 miles) surrounding the building. Our control group consist of Craigslist rental listings that are within a ring that is further away from the building (between 1 and 1.2 miles away), but that are not contaminated by another nearby MFTE property's treatment effects, either spatially or temporally. As illustrated in Figure 5, an observed listing is contaminated" if and only if it is (1) online during the pre-treatment period of one MFTE building and during the post-treatment period of another MFTE building, and (2) within the treatment zone (a circle of defined radius) of both MFTE buildings.

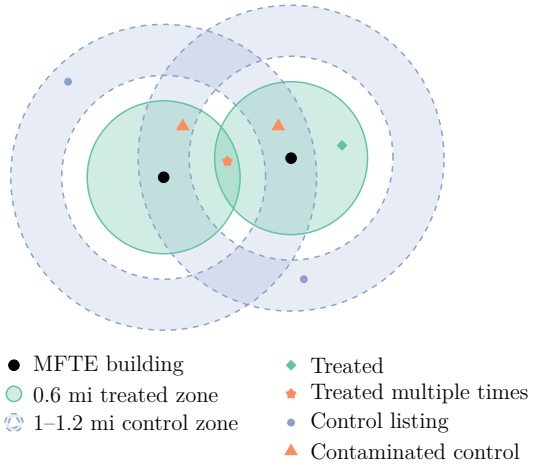
4.1.1 *Continuous* Spatial Difference-in-Differences

For the continuous spatial difference-in-differences setup, we include each Craigslist listing only once in our sample, provided that it is not a contaminated control unit. We quantify treatment dosage for each Craigslist listing (New Units$_i$) as the total units across all MFTE buildings near the listing that began leasing before it was observed online on the Craigslist website. This is a continuous difference-in-difference research design as described in the recent econometrics literature (Callaway et al., 2024). However, our research design is

[12]Our continuous staggered difference-in-differences design also assumes strong parallel trends, which we discuss in 4.3.



(a) Illustration of criteria (1) for which observations of Craigslist listings are treated, control, and potentially contaminated control in the temporal dimension.



(b) Illustration of criteria (2) for which observations of Craigslist listings are treated, control, and potentially contaminated control in the spatial dimension.

Figure 5: Illustration of criteria to avoid sample contamination of control units and to identify multiple treated units among our universe of Craigslist listings.

also staggered because we have Craigslist listings that are exposed to different treatment events that start at different CO dates (Callaway & Sant'Anna, 2021). This is slightly different from a standard continuous difference-in-differences because we consider treatment exposure to begin for a Craigslist listing on the earliest CO date across nearby MFTE properties.[13]

In the specifications (1.1) and (1.2) below, New Units$_i$ is the total housing units at nearby MFTE properties built within two years of a given Craigslist listing. To interpret β_1 as an average elasticity of rents with respect to nearby units in MFTE buildings, we perform a log transformation on New Units$_i$.

$$\log \text{Rent}_{it} = \alpha_c + \delta_t + \beta_0 + \eta_U \log \text{New Units}_{it} + X'_{it}\gamma + \epsilon_{it} \tag{1.1}$$

$$\log \text{Rent}_{it} = \alpha_c + \delta_t + \beta_0 + \eta_U \log \text{New Units}_{it} + \sum_{I=1}^{3} \eta_{U_I} \log \text{New Units}_i \times \text{Income}_I + X'_{it}\gamma + \epsilon_{it} \tag{1.2}$$

As in the stacked difference-in-differences design, we include month fixed effects to control for time-varying factors (δ_t) and tract fixed effects to control for time-invariant effects specific to each neighborhood (α_i). We also control for the number of bedrooms and square footage of listing i. We interpret β_1 as the average elasticity of rents with respect to new units (constructed in the last two years) at nearby MFTE properties conditional on our specified control variables and subject to a "strong parallel trends" assumption detailed in Callaway et al. (2024). In our context, strong parallel trends is the assumption that Craigslist listings that are exposed to a greater number of housing units in nearby MFTE properties would have exhibited the same trends in rents as listings that were exposed to a fewer number of total housing units in nearby MFTE properties.

While we have not yet formally tested the stronger parallel trends assumption, our event study results for the stacked difference-in-differences in Section 4.3 suggest that rent trends between treatment and control groups are parallel once we account for heterogeneous effects. Future work will test this stronger assumption to enable full causal interpretation of the average elasticities estimated in specifications (1.1) and (1.2).

4.1.2 *Stacked* Spatial Difference-in-Differences

For the stacked spatial difference-in-differences design, we construct separate treatment and control groups for each MFTE building based on the spatial and temporal criteria defined above to avoid contamination. Our main parameter of interest is the effect of proximity to a single MFTE property on rental listings' rents (captured by the dummy variable "Near Single") after the property's CO date (indicated by the

[13] As a result of our aggregation of housing units across multiple MFTE buildings for each Craigslist listing, a given listing may have been exposed to treatment to the units from one MFTE building at a different time than the units from another MFTE property. If the treatment effects on local rents vary over time after each MFTE CO date and the CO dates of each nearby MFTE building are relatively far apart, one caveat is that our results may underestimate the magnitude of the average elasticities of rents with respect to nearby units in MFTE properties.

dummy variable “Post"). Listings exposed to multiple MFTE buildings within the treatment zone after their CO dates are included once per treatment event, with a separate indicator “Near Multiple" that is mutually exclusive of “Near Single". Because we define a separate treatment and control group for each MFTE property with each CO date as a separate event date, this design constitutes a stacked difference-in-differences, following recent econometric literature (Callaway & Sant'Anna, 2021; Cengiz et al., 2019; Goodman-Bacon, 2021).

We run two specifications for the stacked difference-in-differences, one for the full sample shown in Equation (2.1) and one separately for each segment of the neighborhood income distribution[14] to capture heterogeneous average treatment effects by tract-level income shown in Equation (2.2).

$$\begin{aligned}\log \text{Rent}_{it} = {} & \alpha_c + \delta_t + \beta_0 + \beta_S \text{Post}_{it} \times \text{Near Single}_{it} + \beta_M \text{Post}_{it} \times \text{Near Multiple}_{it} \\ & + X_{it}'\gamma + \epsilon_{it} \end{aligned} \tag{2.1}$$

$$\begin{aligned}\log \text{Rent}_{it} = {} & \alpha_c + \delta_t + \beta_0 + \beta_S \, \text{Post}_{it} \times \text{Near Single}_{it} + \beta_M \, \text{Post}_{it} \times \text{Near Multiple}_{it} \\ & + \sum_{I=1}^{3} \beta_{S_I} \text{Post}_{it} \times \text{Near Single}_{it} \times \text{Income}_I \\ & + \sum_{I=1}^{3} \beta_{M_I} \, \text{Post}_{it} \times \text{Near Multiple}_{it} \times \text{Income}_I \\ & + X_{it}'\gamma + \epsilon_{it} \end{aligned} \tag{2.2}$$

Conditional on tract fixed effects (α_c), month fixed effects (δ_t), and control variables X_{it} (including the number of bedrooms and nonlinear spline terms for square footage), β_S in Equation (2.1) can be interpreted as the average treatment effect of being located near an MFTE building, under a standard parallel trends assumption. In this context, we assume that in the absence of exposure to a nearby MFTE building, the rent of a Craigslist listing would have evolved similarly to that of comparable listings in the control group not exposed to any MFTE building.

The interpretation of β_M is less straightforward because it aggregates treatment effects from multiple nearby new MFTE properties. These treatments effects can have different timing since each MFTE property has a different CO date, and each MFTE property might vary in distance from the Craigslist rental listing. For example, a rental listing located 0.3 miles from one MFTE building could also be within 0.2 miles of another newly leased MFTE property. Also, a rental listing that was posted in July 2018 might be affected

[14] While the income data we use is the median income of the Census tract where each Craigslist listing is located, we divided the sample into low, middle, and high income segments based on the income percentiles of Seattle's population as measured using the ACS Public Use Microdata Sample (PUMS) from 2020. The low, middle, and high income segments are defined as the 0 to 40th percentiles ($0 to $65K per year), 40th to 60th percentiles ($65K to $110K per year), and 60th to 100th percentiles (greater than $110K per year), respectively.

by a MFTE property with CO date of February 2018 and another with CO date of May 2018. Therefore, we do not causally interpret our estimates of β_M.

We test our parallel assumption for β_S by conducting event studies in Section 4.3. While we find that parallel trends in rents might not hold well for the stacked difference-in-differences ran on the full sample in Equation (1.1), they appear to be robust when running the difference-in-difference for separate neighborhoods of different income levels. Therefore, the main causal effects that we identify through our stacked difference-in-differences design are $\beta_S + \beta_{S_I}$ in (2.2). Conditional on tract fixed effects, month fixed effects, size of the housing unit (in bedrooms and square footage), these are the average treatment effects on rents of being near a single MFTE property for rental listings in low-, middle-, and high-income Census tracts.

4.1.3 *Early vs. Late* Difference-in-Differences

We implement another difference-in-differences that compares the rental listings near MFTE properties with lease-up dates from 2017 to 2018 to those with lease-up dates from 2019 to 2023. The key assumption of this research design is that developers chose both sets of MFTE sites for similar reasons, but the timing of a lease-up depends on random factors such as when a particular plot of land happened to become available for purchase and idiosyncratic reasons for the timing of construction completion.

This analysis serves as a robustness check for the results of the stacked spatial difference-in-differences. One potential concern with the stacked spatial difference-in-differences is that if a new MFTE building's effect spills beyond the boundary of the treatment ring, then the estimated effects would not fully capture the treatment effects. Meanwhile, an early vs. late difference-in-differences compares rent changes near early-lease-up buildings to rent changes near late-lease-up buildings, holding distance from the MFTE site constant. Because the control group has not yet experienced a nearby lease-up at the time of comparison, spillover effects from a lease-up cannot contaminate it, and the comparison remains unbiased.

Here, the treatment group is listings near buildings with lease-ups before 2019, and the control group is listings near buildings with lease-ups in 2019 or later. However, since treatment is determined by timing of MFTE building lease-up for this design, we include only Craigslist listings that experienced a single MFTE building lease-up nearby. We omit those that experienced more than one such event to ensure that the control group consists of only listings that are treated on or after 2019. We define being "near" a MFTE building in the same way as in the stacked spatial difference-in-differences design. As before, we provide separate results for treatment rings that are 0 to 0.2 miles, 0.2 to 0.4 miles, and 0.4 to 0.6 miles of MFTE sites. The definition of the Post_{it} variable in (3.1) and (3.2) below are also the same as in the stacked design.

We run the early vs. later treated design for both the full sample in specification (3.1) and for the same segments of neighborhood income as in the stacked and continuous spatial difference-in-differences in

specification (3.2).

$$\log \text{Rent}_{it} = \alpha_c + \delta_t + \beta_0 + \beta_E \text{Post}_{it} \times \text{Early Lease-Up}_{it} + X'_{it}\gamma + \epsilon_{it} \tag{3.1}$$

$$\log \text{Rent}_{it} = \alpha_c + \delta_t + \beta_0 + \beta_E \text{Post}_{it} \times \text{Early Lease-Up}_{it} + \sum_{I=1}^{3} \beta_{E_I} \text{Post}_{it} \times \text{Early Lease-Up}_{it} \times \text{Income}_I + X'_{it}\gamma + \epsilon_{it} \tag{3.2}$$

4.2 Results

4.2.1 Average Treatment Effects of Close Proximity to One or More Newly Leased MFTE Properties

Our results from running the stacked difference-in-differences design in the full sample are presented in Table 2. Our estimates for the coefficients on Post × Near Single in the first row suggest that rental listings located within 0.2 miles of a MFTE property that began leasing within the past two years have, on average, 0.037 log points (≈ 3.8% increase) higher rents compared to listings not close to any MFTE property, and this difference is statistically significant at the 5 percent level. However, as shown in the event-study analysis in Section 4.3, our parallel trends assumption does not appear to hold well when we estimate the stacked difference-in-differences on the full sample. Based on further investigation into heterogeneous effects by tract-level income, this likely reflects systematic differences in pre-treatment rent trends between neighborhoods of different income levels that are not fully accounted for by Census tract and time fixed effects (de Chaisemartin & D'Haultfœuille 2020). Consequently, we do not interpret the coefficients in Table 2 as causal effects, and we focus on our results that split the sample by tract-level median income segments in Table A.1.

Nonetheless, it is noteworthy that the direction of the associations in Table 2 is consistent with the mechanism that MFTE developments generate positive local amenities effects on nearby housing rents, which diminish with distance from the development site.[15] This interpretation is supported by our finding that when the treatment zone is defined as the ring between 0.2 and 0.4 miles from an MFTE property (column 2), or the ring farther away (column 3), the estimated effects are not statistically different from zero.

Our estimates for the coefficients on Post × Near Multiple also show a positive average effect of 6.6 log points (≈ 6.8 percent increase) on rents of listings located within 0.2 miles of multiple MFTE buildings. This estimate is even harder to interpret causally, however, because they aggregate treatment effects from

[15] It remains to be seen whether these positive amenities effects stem specifically from MFTE properties or from new housing developments more generally. We plan to explore this further using Seattle permitting data on all multifamily housing development sites.

multiple nearby MFTE properties. For example, a rental listing located 0.2 miles from one MFTE building could also be within 0.3 miles of another newly leased MFTE property. With this caveat in mind, we note that listings located near multiple MFTE properties tend to experience larger post-treatment rent increases than those near only one or none. Because these effects may conflate exposure to multiple developments, we focus our discussion on the estimates for listings located near a single MFTE building, including when we later estimate the stacked difference-in-differences separately by neighborhood income group.

Table 2: Results of running the stacked DiD in (1.1) for the full sample. The dependent variable for all specifications is log(Rent). All specifications include controls for number of bedrooms, three spline terms in square footage, tract fixed effects, and year-month fixed effects. Standard errors are clustered robust by Census tracts of Craigslist listings.

	Treated Ring Width		
	0 to 0.2 Miles	0.2 to 0.4 Miles	0.4 to 0.6 Miles
Variable	(1)	(2)	(3)
Post × Near Single	0.037*	-0.009	-0.002
	(0.019)	(0.031)	(0.022)
Post × Near Multiple	0.066***	0.020	0.002
	(0.023)	(0.031)	(0.012)
Observations	103,622	79,516	94,092
R-squared	0.714	0.682	0.726

Because eligibility for MFTE units depends on household income, it is natural to examine whether the lease-up of new MFTE buildings produces heterogeneous effects on local rents across neighborhoods of different income levels. To do so, we divide our sample into low-, middle-, and high-income segments as defined in our research design and estimate the stacked difference-in-differences separately for each group. The results are presented in Table A.1.

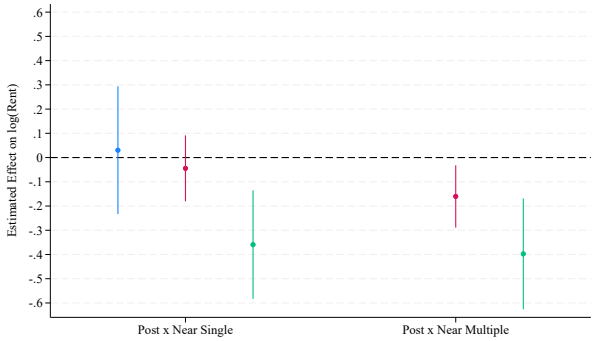
Focusing on the coefficients for Post × Near Single, we find that rental listings in neighborhoods with median income below the 40th percentile experience negative average treatment effects from proximity to a MFTE building. For listings located 0.4–0.6 miles away, rents are approximately 35.9 log points lower on average ($\approx$ 30 percent decrease), corresponding to roughly $450 less for a unit renting at $1,500 per month. These results are illustrated in Figure 6. In contrast, listings in middle- and high-income neighborhoods exhibit zero or positive effects at similar distances. In particular, listings in tracts with median income above the 60th percentile experience an average rent increase of 13.4 log points (roughly 14.3 percent) when within 0.2 to 0.4 miles of a newly constructed MFTE building. Event-study estimates presented in Section 4.3 are broadly consistent with the parallel trends assumption underlying these estimates.

One remaining challenge is that the number of rental listings in the lowest-income subsample is substantially smaller than in the middle- and high-income subsamples across all treatment-ring specifications. Although the negative coefficients for lower-income neighborhoods are directionally consistent with the competitive effects of new housing supply, their magnitudes exceed those typically found in the literature, includ-

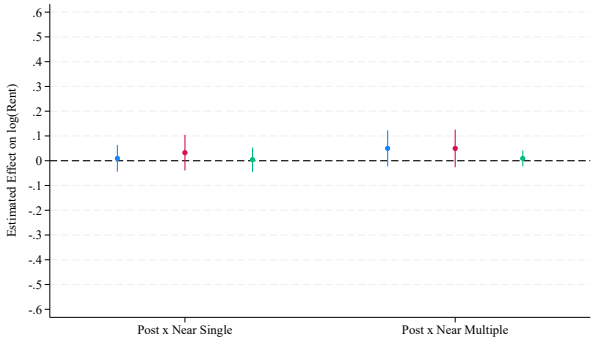
Table 3: Results of running the stacked DiD in Equation (2.2) for each neighborhood income segment. The dependent variable for all specifications is log(Rent). All specifications include controls for number of bedrooms, three spline terms in square footage, tract fixed effects, and year-month fixed effects. Standard errors are clustered robust by Census tracts of Craigslist listings.

	Treated Ring Width		
Income Segment (bolded) and Variables	0 to 0.2 mi	0.2 to 0.4 mi	0.4 to 0.6 mi
Below 40th Percentile: $[\$0, \$65K]$			
Post × Near Single	0.030	-0.044	-0.359***
	(0.061)	(0.049)	(0.092)
Post × Near Multiple	-	-0.160**	-0.397***
		(0.046)	(0.093)
	(0.106)	(0.074)	(0.080)
Observations	766	1,300	4,595
R-squared	0.728	0.676	0.689
40th to 60th Percentile: $(\$65K, \$110K]$			
Post × Near Single	0.010	0.032	0.004
	(0.027)	(0.036)	(0.025)
Post × Near Multiple	0.050	0.050	0.009
	(0.036)	(0.038)	(0.016)
Observations	46,920	41,649	42,227
R-squared	0.713	0.692	0.741
Above 60th Percentile: $> \$110K$			
Post × Near Single	0.033	0.134***	-0.012
	(0.089)	(0.025)	(0.016)
Post × Near Multiple	-0.001	0.057	-0.006
	(0.056)	(0.076)	(0.030)
Observations	6,419	5,520	11,783
R-squared	0.619	0.767	0.741

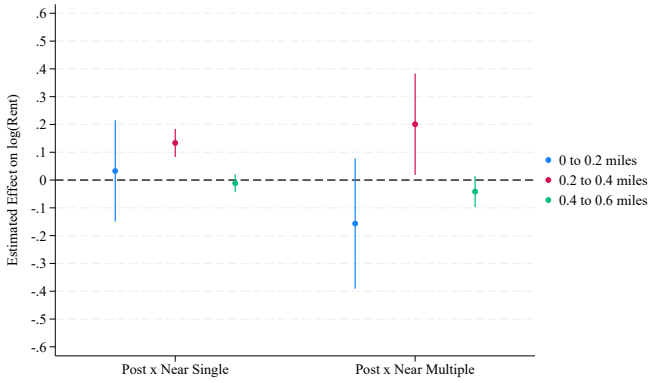
ing Asquith et al. (2023), which studies the effects of new multi-family construction more broadly. This gap may partly reflect reduced precision and limited statistical power arising from sample restrictions designed to avoid contaminated control observations (illustrated in Figure 2) and to exclude listings potentially affected by construction externalities during the MFTE development period. It may also reflect differences in the underlying treatment, as Asquith et al. (2023) focuses on market-rate multifamily development, whereas our specification examines subsidized IZ developments in lower-income neighborhoods below the 40th percentile.



(a) Lowest neighborhood income segment below the 40th percentile ($< \$65K$ per year).



(b) Middle neighborhood income segment between the 40th and 60th percentiles ($65K to $110K per year).



(c) Highest neighborhood income segment above the 60th percentile (>$110K per year).

Figure 6: Panels (a)–(c) show the estimated coefficients and their 95 percent confidence intervals from Table A.1. Each different colored bar reflects different definitions of treatment rings around each MFTE property (0 to 0.2 miles, 0.2 to 0.4 miles, and 0.4 to 0.6 miles).

Furthermore, estimates from the earlier-versus-later difference-in-differences specification, conducted separately by neighborhood income category, corroborate the patterns observed in the stacked spatial difference-in-differences analysis. Nearby MFTE development is associated with rent declines in lower-income neighborhoods, while estimated effects in middle- and higher-income neighborhoods remain neutral or positive. A key distinction in the earlier-versus-later specification is the smaller sample size for the lowest-income subsample, driven in part by the exclusion of listings exposed to multiple nearby MFTE developments. As a result, estimates for lower-income neighborhoods are less precise and somewhat larger in magnitude than findings in prior work focusing on new multi-family construction, including Asquith et al. (2023).

Table 4: Results of running the early vs. late DiD in Equation 2.2 for each neighborhood income segment. The dependent variable for all specifications is log(Rent). All specifications include controls for number of bedrooms, two spline terms in square footage, tract fixed effects, and year-month fixed effects. Standard errors are clustered robust by Census tracts of Craigslist listings.

	Treated Ring Width		
Income Segment (bolded) and Variables	0 to 0.2 mi	0.2 to 0.4 mi	0.4 to 0.6 mi
Below 40th Percentile: $[\$0, \$65K]$			
Post × Early Lease-Up	0.041	0.127	-0.362**
	(0.022)	(0.089)	(0.108)
Observations	1,216	2,333	290
R-squared	0.727	0.307	0.846
40th to 60th Percentile: $(\$65K, \$110K]$			
Post × Early Lease-Up	0.034	0.056	-0.030
	(0.020)	(0.062)	(0.067)
Observations	15,090	10,231	5,536
R-squared	0.714	0.623	0.680
Above 60th Percentile: $> \$110K$			
Post × Early Lease-Up	0.127*	0.006	-0.042
	(0.063)	(0.045)	(0.080)
Observations	4,557	8,450	3,846
R-squared	0.710	0.749	0.640

Overall, we find that the lease-up of new MFTE buildings in more affluent Census tracts has zero or slightly positive effects on nearby rents. At first glance, this pattern appears at odds with Diamond and McQuade (2019), who show that LIHTC properties increase house prices by 6.5 percent in low-income neighborhoods while reducing them by 2.5 percent in high-income areas, consistent with spatial re-sorting of residents across neighborhoods. We hypothesis that this is because their analysis focuses on house prices, which capture asset capitalization and longer-run expectations, whereas our estimates reflect rental market responses. In other words, while the price per square foot of land may increase as the area has higher amenities, increased housing supply means the price per square foot of *floor space* may decrease. In our setting, MFTE lease-ups compete directly against the existing stock of rental units. MFTE units place downward pressure on rents, particularly in neighborhoods where these units are closer in price to surrounding rental listings.

After testing for parallel trends in rents between listings near a single MFTE property and those farther away (conditional on our controls, fixed effects, and income-specific pre-trends) in Section 4.3, we discuss how the introduction of MFTE units—lower-priced, high-quality rentals—may reduce nearby rents by intensifying competition among landlords targeting similar tenant segments. In contrast, previous findings in the literature have shown that house prices respond differently, potentially increasing if new mixed-income developments enhance neighborhood amenities or desirability. Consistent with this interpretation, we find positive rent effects only for listings located within 0.4 miles of MFTE buildings, but these effects are statistically significant only in middle- and high-income tracts.

4.2.2 Average Elasticities of Rents with Respect to New MFTE Housing Units

We next examine how local rents respond to MFTE development along a different margin: the average elasticity of rents with respect to the number of new housing units at nearby MFTE properties. As shown in Table 5, the overall average elasticity is close to zero and not statistically significantly different from zero when estimated across the full sample. This is not surprising given our stacked difference-in-differences results, which demonstrated considerable heterogeneous effects by neighborhood income.

Table 5: Results of running the staggered continuous DiD in Equation 3.1 for the full sample. The dependent variable for all specifications is log(Rent). All specifications include controls for number of bedrooms, three spline terms in square footage, tract fixed effects, and year-month fixed effects. Standard errors are clustered robust by Census tracts of Craigslist listings.

	Treated Circle Width		
	0 to 0.2 Miles	0 to 0.4 Miles	0 to 0.6 Miles
Variable	(1)	(2)	(3)
log New Units	-0.002	-0.011	-0.013
	(0.017)	(0.018)	(0.013)
Observations	2,426	2,833	3,671
R-squared	0.714	0.728	0.618

As expected, Table 6 reveals notable heterogeneous effects by neighborhood income. In the lowest-income Census tracts, rents exhibit an average elasticity of negative 2.2 percent with respect to nearby units in MFTE properties within 0.4 miles, whereas in the highest-income tracts, this elasticity is 2.9 percent with respect to units in MFTE buildings within 0.4 miles, respectively. We visually illustrate this finding by plotting the coefficients and the corresponding 95 percent confidence intervals in Figure 7. These patterns are consistent with the stacked difference-in-differences results, which similarly capture how proximity to one or more MFTE buildings affects local rents. [16]

These coefficients can be viewed as reduced-form estimates of how local rents respond to changes in nearby

[16] Note that we define treatment zones differently in the stacked difference-in-differences and the staggered continuous designs. In the former, we isolate how the effect of a single nearby MFTE building on local rents decays with distance by comparing listings in concentric area rings. In contrast, this approach is not feasible for the continuous design, where treatment dosage is defined by the total number of nearby MFTE units, potentially drawn from multiple buildings at varying distances.

Table 6: Results of running the staggered continuous DiD in (3.2) for each neighborhood income segment. The dependent variable for all specifications is log(Rent). All specifications include controls for number of bedrooms, three spline terms in square footage, tract fixed effects, and year-month fixed effects. Standard errors are clustered robust by Census tracts of Craigslist listings.

	Treated Circle Radius		
Income Segment (bolded) and Variables	0 to 0.2 mi	0 to 0.4 mi	0 to 0.6 mi
Below 40th Percentile: $[\$0, \$65K]$			
log New Units	-0.005	-0.022**	-0.016
	(0.016)	(0.010)	(0.011)
Observations	2,449	3,352	4,557
R-squared	0.713	0.722	0.633
40th to 60th Percentile: $(\$65K, \$110K]$			
log New Units	0.016	0.003	0.016**
	(0.014)	(0.012)	(0.008)
Observations	10,416	15,378	12,851
R-squared	0.747	0.702	0.716
Above 60th Percentile: $> \$110K$			
log New Units	0.007	0.029**	0.006
	(0.016)	(0.013)	(0.008)
Observations	1,711	3,350	6,070
R-squared	0.847	0.776	0.684

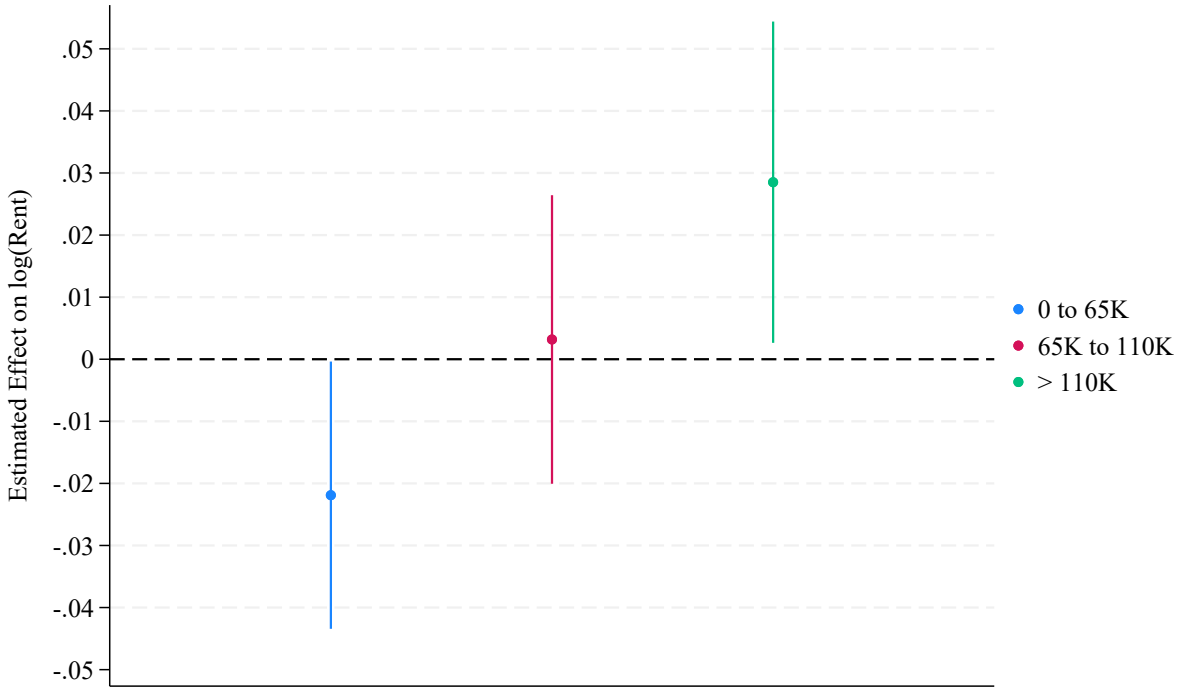


Figure 7: A visualization of the estimated coefficients and their 95 percent confidence intervals from column two of Table A.1, which compares the average elasticities of local rents with respect to new units in MFTE properties within 0.4 miles across Census tracts of different income levels.

housing supply. Under the additional assumption that MFTE construction represents an exogenous local supply shock and that demand conditions are approximately stable after conditioning on fixed effects, these elasticities can be interpreted as empirical analogues of inverse supply elasticities (e.g., Glaeser, Gyourko, and Saks (2005); Saiz (2010); Asquith et al. (2023)). However, in practice they capture the equilibrium net effect of new mixed-income housing on rents, incorporating both an increase in supply-side competition and potential demand-side amenity changes. The former appears to dominate for the lowest income neighborhoods and the latter appears to dominate for the highest income neighborhoods, since the estimates are negative for lowest income tracts and positive for highest income tracts.

4.3 Robustness Checks

4.3.1 Event Studies to Test Parallel Trends for Single Treated and Control Listings

As mentioned previously, we conduct event studies to test the parallel trends assumptions for our stacked difference-in-differences using each MFTE property's CO date as the event date. We perform event studies for the full sample of Craigslist listings, as well as for each income segment, to check the robustness of our results for causal interpretation.

For our event study design, we use the same data and definitions of treatment and control rings as in the spatial stacked difference-in-differences analysis. We include indicator variables for each half-year period after a MFTE property begins leasing, defined relative to the CO date. However, because our research design excludes the construction period, we define pre-treatment ("lead") periods relative to the earliest possible construction start date—specifically, the later of the developer's MFTE application date or the building permit issuance date. All specifications control for the number of bedrooms and square footage (X_{it}), Census tract fixed effects (α_c), and month fixed effects (δ_t).

$$\begin{aligned}\log(\text{Rent}_{ict}) = \alpha_c + \delta_t &+ \sum_{\tau=2}^{4} \beta_{S_\tau} (\text{Lead } \tau)_{it} \cdot \mathbf{1}\{\text{Near Single}_i\} + \sum_{\tau=0}^{3} \gamma_{S_\tau} (\text{Lag } \tau)_{it} \cdot \mathbf{1}\{\text{Near Single}_i\} \\ &+ \sum_{\tau=2}^{4} \beta_{M_\tau} (\text{Lead } \tau)_{it} \cdot \mathbf{1}\{\text{Near Multiple}_i\} + \sum_{\tau=0}^{3} \gamma_{M_\tau} (\text{Lag } \tau)_{it} \cdot \mathbf{1}\{\text{Near Multiple}_i\} \\ &+ X_{it}'\zeta + \epsilon_{ict} \end{aligned} \tag{4}$$

We begin by estimating the event study model in Equation (4) using the full sample, with results presented in Figure 8 below. Since the coefficient on Single Treated × Post was statistically significant and positive in Table 2 for the treatment zones within 0.2 miles of a MFTE, we present the event study point estimates and

their 95 percent confidence intervals that assume a 0.2 mile treatment zone around each MFTE property.[17] These event study results for the full sample suggest that the parallel trends assumption in rents may be

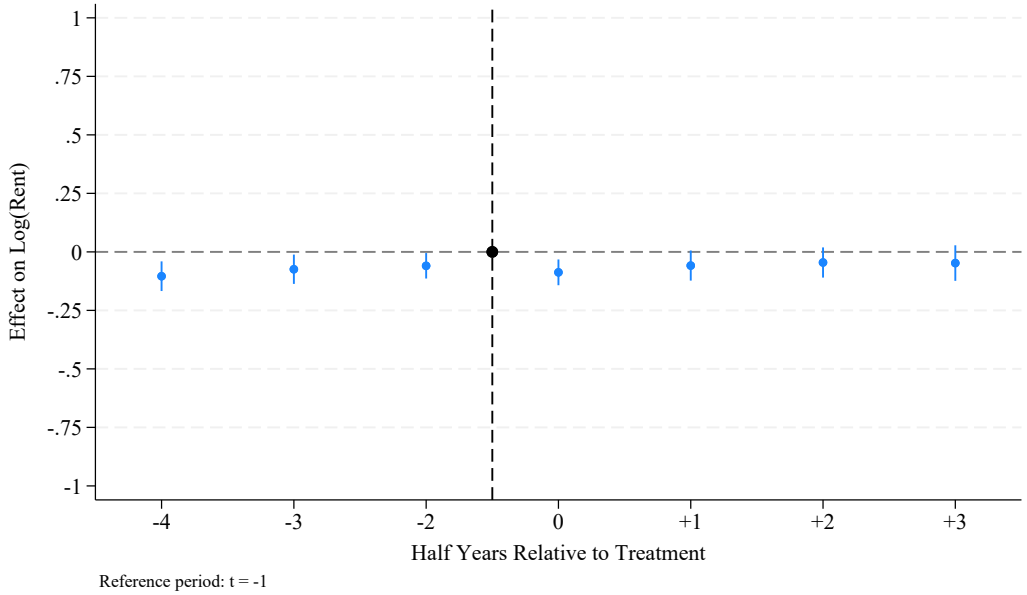


Figure 8: Event study point estimates and their confidence intervals using the full sample of Seattle Craigslist listings, defining the treatment ring as listings within 0.2 miles of each MFTE property.

violated. Although rent growth estimates are slightly closer to zero in the post-treatment period relative to the reference period (half a year before construction), they remain negative both before and after treatment. This pattern indicates possible misspecification when estimating the stacked difference-in-differences model using the full sample of Craigslist listings. One possible explanation is the presence of differential pre-trends across neighborhoods of varying income levels. For example, if rents in high-income areas were already increasing faster than those in low-income areas prior to treatment, fixed effects alone would not account for this divergence. In addition, two-way fixed effects estimators can produce biased estimates when treatment effects vary across groups or over time, as the resulting group-time weights can become non-convex or even negative (Goodman-Bacon, 2021; de Chaisemartin & D'Haultfoeuille, 2020).

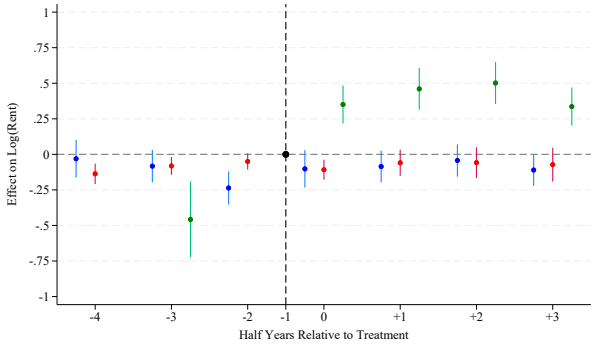
To address these potential threats to validity, we re-estimate the event study model in equation (3) separately by Census tract income, dividing the sample into low-, middle-, and high-income groups as defined earlier. The results are shown in Figure 9.

Although pre-treatment observations are limited for the high-income group, most lead coefficients in panels (a) and (b) of Figure 9 are zero or negative. In the post-treatment period, rents increase substantially relative to the reference period, consistent with the positive coefficients reported in columns (1) and (2) of Table A.1. While the coefficient for lead -3 is positive and significant, it is unclear whether this reflects a

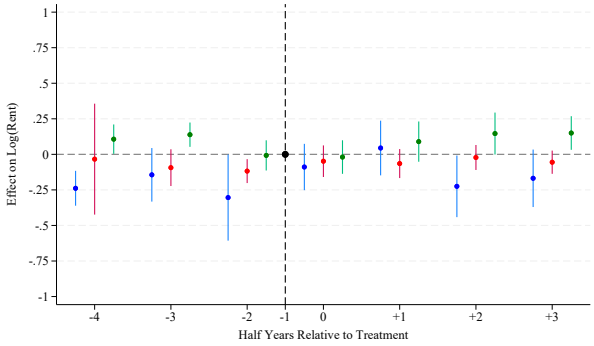
[17] See Appendix figures **??** for event study graphs for other treatment zones.

true violation of parallel trends in high-income Census tracts or noise due to limited pre-treatment data.

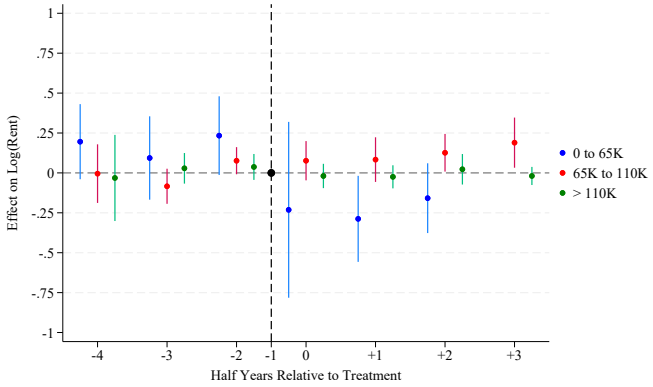
For the lowest-income group, panels (b) and (c) of Figure 9 show no evidence of pre-trends when defining the treatment zone as 0.2–0.4 miles and 0.4–0.6 miles from an MFTE property, respectively. In panel (c), lead coefficients are close to or above zero, while lag coefficients are negative, suggesting that the estimated post-treatment rent decline of 35.9 percent reported in Table A.1 is robust to parallel trends between treatment and control groups.



(a) Event study plot with the treatment zone defined as between 0 and 0.2 miles of each MFTE property.



(b) Event study plot with the treatment zone defined as between 0.2 and 0.4 miles of each MFTE property.



(c) Event study plot with the treatment zone defined as between 0.4 and 0.6 miles of each MFTE property.

Figure 9: Panels (a)–(c) show the estimated event study coefficients β_{S_τ} and γ_{S_τ} from Equation (1.2) and their 95 percent confidence intervals for different definitions of treatment zones. The reference period is lead -1, which is the half year before the construction period. Each different colored bar reflects estimates for Census tracts from a different income segment.

4.3.2 Placebo Test Using Non-MFTE MF Permits

As a placebo test, we conduct an identical stacked spatial DiD with the same specification as in equations (1.1) and (1.2) using estimated lease-up dates of market-rate MF properties as treatment events. From our permitting data, we identify new MF starts within our sample period. To ensure comparability between these properties and MFTE, we include only market-rate MF properties with as many units as the range

of units observed for MFTE properties (i.e. we eliminate duplexes/triplexes and high-rises who generally do not participate in MFTE). This sample provides us with 294 non-MFTE MF properties, about twice as many properties as in our MFTE sample. As seen in Table A.1 in the Appendix, non-MFTE MF properties have little to no statistically significant causal effects on nearby rents. While the coefficients for the Near Multiple terms are significant, we again cannot interpret these coefficients causally for the same reasons as in Section 4.1.1. Instead these are capturing only conditional correlation between rents and being near multiple non-MFTE MF buildings after lease-up, net of fixed effects and controls.

As a caveat, however, our lease-up dates for non-MFTE MF properties are less precise than those for MFTE properties. This is because for about two-thirds of non-MFTE buildings in our sample, we have only the permit issued date. This means that we are not able to drop rental listings that were posted during each building's construction period.

4.4 Potential Mechanisms

Our estimates can be explained as the net effect of two competing theoretical mechanisms. On one hand, MFTE developments increase the overall housing supply and introduce lower-priced, high-quality units intended for lower-income households. These changes are expected to reduce rents by expanding housing availability and increasing competition among the lower segment of the rental market. On the other hand, new development may improve local amenities, increasing local housing demand and exerting upward pressure on rents over time.

In the absence of an analysis of heterogeneous treatment effects across neighborhoods with varying income levels, we find that the estimated average effect is statistically indistinguishable from zero. This suggests that these countervailing forces discussed above may offset one another in the aggregate. After accounting for heterogeneous treatment effects by income, however, the different results suggest a few possibilities that are based on the mechanisms discussed above. More affluent neighborhoods like those in downtown Seattle may have fewer landlords who target the lower-income segment of the market to begin with, so a MFTE project's addition of low-priced, high-quality housing units targeting lower income households may not affect competition substantially. In contrast, the addition of MFTE units in a lower-income neighborhood like Rainier Valley may pressure some landlords targeting lower-income renters to lower their prices.[18] Overall, a new MFTE building might boost housing demand more greatly in higher-income areas, which increases market rents.

[18]While our descriptive statistics show that MFTE units in Rainier Valley are priced at $300 more on average than local market rents, the relative higher quality of MFTE units may still increase competition if there are lower income renters who can afford to pay this margin for quality in that neighborhood.

5 Quantitative Urban Model

The complexity of effects stemming from affordable housing policies motivates us to develop a quantitative urban model (Ahlfeldt et al., 2015; Redding, 2024) with housing developers and households. We aim to understand how affordable housing policy redistributes households, housing development, and prices across space and the welfare effects of this redistribution. We start with a city embedded in a larger economy with I locations, each defined by residential amenities, productivity fundamentals, and a fixed transport network. In addition, we use tract-level housing elasticity supply estimates from Baum-Snow and Han (2024), since our policy primarily affects new construction. Developers decide to build a property and choose to participate in the affordable housing program or not at the time of building. Households choose residence and workplace, receiving utility from housing, wages, and amenities, and disutility from rent and commuting. We abstract from firms and the final good typical of QSMs to focus on residential sorting, because including them does not change any of the qualitative predictions of the model.

Our model includes several novel distinguishing features that capture developer and household responses to affordable housing policy. First, we explicitly model the developer's problem for choosing between different building options (market rate building, affordable housing building, or no building) and their choice of number of units. Local market demand determine both building type and overall supply, and thus the relative supply of affordable units. With endogenous amenities, developers respond on both intensive and extensive margins to incentives, matching our reduced form results of clustering and increased rents in high-income areas.

Second, our model assigns each household a labor type (high- or low-type) and incorporates rationing for affordable units allocated to low-type households. Low-type households incorporate these lottery probabilities into their location choices, which ex-ante moderates demand in high amenity or productivity areas, but ex-post creates welfare differences. This matches our reduced-form estimates of small negative rent effects in low-income neighborhoods, as affordable housing reduces market rents more in lower-income neighborhoods by a full expansion of housing supply.

These features enable us to simulate counterfactual scenarios showing how an affordable housing policy such as the MFTE program can increase housing supply and improve affordability, particularly for low-type households. For these households, the program may expand access to higher-productivity or higher-amenity neighborhoods, better-quality housing, and more affordable market rents in some neighborhoods, outcomes that may not arise without the program. These gains are essential for capturing the policy's full welfare effects. Our model is well equipped to simulate a wide range of affordable housing programs. For example, by shutting down the market-rate housing option and setting subsidy to zero (or negative), we model mandatory

affordable housing programs [19]. By raising set-aside percentage to 100%, we model public housing programs that still are subject to market forces in location choices.

5.1 The Supply-Side (Developer's Problem)

Each developer i has a parcel of land in neighborhood j. They make a one-time decision to build, choosing both the type of building b (affordable, market rate, not build) and number of housing units q to construct. They choose to do this if discounted lifetime profits outweighs construction costs $C(q) = c_b q^\gamma$, a function of units q and a cost shifter dependent on building type. Market-rate rent and existing housing stock (both market-rate housing and affordable housing) are the key equilibrium objects in determining developer decisions, so for market j and time t, we define the state variables as $\mathbf{s}_{jt} = \begin{bmatrix} r^{\text{a}}_{jt} & Q^{\text{m}}_{jt} & Q^{a}_{jt} \end{bmatrix}'$.

For a given market j and time t, developers thus maximize their value function as:

$$V(\mathbf{s}_{jt}, \sigma_{it}) = \max_b \{\Pi(b, s_{jt}) + \sigma_{ijt}(b)\} \tag{5}$$

where $\Pi(b, s_{it})$ is the lifetime profit from each choice. Developers may also choose to wait, receiving $\beta EV(s_{j,t+1}|s_{jt})$. In Equation 5, σ_{ijt} is an unobserved, i.i.d. Type I extreme value error term that represents idiosyncratic characteristics of developers that contribute their choice, and can represent specific expertise in building one type of building over another.

Program parameters affect developer profits for building affordable housing: affordable rent limits (r^a_{jt}), set-aside portion of units (λ), and subsidy per unit ξ.[20] Developer profit and optimal number of units (from the FOC) are given as follows:

$$\pi_{\text{m}}(\mathbf{s}_{jt}) = \sum_{\tau=t}^{\infty} \beta^{\tau-t} E_t[r^{\text{m}}_{j\tau}]q - C_{jt}(q) \tag{6.1}$$

$$\pi_{a}(\mathbf{s}_{jt}) = \sum_{\tau=t}^{\infty} \beta^{\tau-t} (\lambda(r^a_j) + (1-\lambda)(E_t[r^{\text{m}}_{j\tau}]) + \xi)q - C_{jt}(q) \tag{6.2}$$

$$C^{-1}(q^*_{jt,\text{m}}) = \frac{1}{\gamma - 1}\left(\log(r^{\text{m}}_{jt}) - \log\left(\frac{\gamma}{1-\beta}\right)\right) \tag{7.1}$$

$$C^{-1}(q^*_{jt,a}) = \frac{1}{\gamma - 1}\left(\log(\lambda(r^a_{jt}) + (1-\lambda)r^{\text{m}}_{j\tau} + \xi) - \log\left(\frac{\gamma}{1-\beta}\right)\right) \tag{7.2}$$

Thus higher market demand (rents) increases development through both building more housing units (intensive margin) and choosing to construct more market rate buildings over affordable buildings or waiting

[19] Examples include Mandatory Inclusionary Housing (MIH) in New York and Mandatory Housing Affordability (MHA) in Seattle.

[20] These can be considered fixed as program requirements are set at certification, even if policy changes in future years. For example, P5 (2015-2019) buildings could open in 2021 but are still only subject to P5 standards.

(extensive margin).

5.2 The Demand-Side (Household's Problem)

Households of type $e \in \{1, 2\}$ choose where to live j and work k across $J = K$ locations. Each location j has amenity value a_{jt} and housing stock Q_j. Household type (low or high) have different marginal productivity by location of labor w_{ej}. Let H_e denote the total share of households of type e, and let ρ_{ej} denote the location choice probabilities or share of households of type e residing in location j. All households consume one unit of housing of type $b \in \{m, a\}$ and pay r^b_{jt}.

Affordable housing is only available to low-type households and is rationed based on location choices demand and affordable housing supply. For model tractability, we assume low-type households first choose location-workplace j, k, and then enter a location-specific lottery of obtaining affordable housing. The probability of lottery success p^a_{jt} is the supply of affordable units divided by demand:

$$p^a_{jt} \;=\; \min\left(\frac{Q^a_{jt}}{N^a_{jt}},\, 1\right) \;=\; \min\left(\frac{Q^a_{jt}}{\rho_{1jt} H_1},\, 1\right). \tag{8}$$

Low-type households do not have preferences over housing types [21], but are affected by lottery probabilities. Furthermore, demand for market rate housing is affected by lottery losers:

$$N^{\mathrm{m}}_j = H_{2j} + (1 - p^a_{jt}) H_{1j} \tag{9}$$

Household indirect utility for type $e \in \{1, 2\}$ who lives in location j and works in k is given by:

$$U_{1jk} = \left((1 - p^a_{jt})\left(\frac{w_{1k} A_j}{d_{jk} r^{\mathrm{m}}_{jt}}\right) + p^a_{jt}\left(\frac{w_{1k} A_j}{d_{jk} r^{\mathrm{a}}_{jt}}\right)\right)\epsilon_{1jk} \tag{11.1}$$

$$U_{2jk} = \left(\frac{w_{2k} A_{jt}}{d_{jk} r^a_{jt}}\right)\epsilon_{2jk} \tag{11.2}$$

, where utility is derived from wage w_{ej}, amenities A_j, rent $r^b_j t$, commuting cost from j to k d_{jk}, and a Type II Frechet idiosyncratic error σ_{jk}. Low-type households derive utility from a weighted average of market and affordable housing utilities. All households inelastically supply one unit of labor.

We assume an endogenous amenity that improves with the relative proportion of high-type residents. This represents a basic residential sorting, and captures the potential amenity or disamenity from the reduced-form

[21] In reality, MFTE units are in extremely high demand, with waitlists of up to six months common for many buildings. Here we model rent and location, but abstract from quality, although as part of new construction, MFTE buildings feasibly could be of higher quality.

literature. We specify the amenity level in neighborhood j at time t as:

$$A_j = a_j \cdot (Q_{jt})^{\phi} \tag{10}$$

where a_j is an exogenous component of the location (e.g. proximity to parks) and $\phi \geq 0$ is the elasticity of amenities with respect to the housing stock. When $\phi > 0$, this creates a positive feedback loop: new development increases Q_{jt}, which raises a_{jt}, which in turn increases demand for location j.

Given the distributional assumption on the idiosyncratic taste draws of the households, the location choice probabilities for each household type e can be written with the closed form

$$\rho_{ejk} = \Pr\left(U_{ejk} = \max_{j',k'} U_{ej'k'}\right) = \frac{U^{\theta}_{ejk}}{\sum_{j',k'} U^{\theta}_{ej'k'}}. \tag{14}$$

5.3 Equilibrium Conditions

After discussing the demand and supply sides of the model, we discuss the market clearing conditions that connect the supply and the demand sides of the model. Assuming that developers have rational expectations of future market rents, the general equilibrium in this model is a path of market rents $\{r^{\mathrm{m}}_{jt}\}^{\infty}_{t=0}$, housing stocks $\{Q_{jt}, Q^{a}_{jt}\}^{\infty}_{t=0}$, household location choices $\{\rho_{ejt}\}^{\infty}_{t=0}$, and developer decisions such that the conditions detailed in this section hold.

5.4 Clearing in the Market Rate Housing Market

First, market rate housing market should clear and determine the equilibrium market rate r^{m}_{jt} at each time t. We assume that households each consume one unit of housing. Therefore, in equilibrium, the total housing stock in a location j must equal the total mass of households who choose to live there:

$$Q_{jt} = \rho_{1jt} H_1 + \rho_{2jt} H_2 \tag{15}$$

where the total housing supply (stock) is Q_{jt} on the left-hand side and the the total housing demand (mass of households) is on the right-hand side. Since we have two types of households, we break down the left-hand side into the mass of low-type households choosing j ($\rho_{1jt} H_1$), and the mass of high-type households choosing j ($\rho_{2jt} H_2$). This equation must hold for all locations $j \in J$. Because the location choice probabilities (ρ_{1jt} and ρ_{2jt}) depend on the utility U_{ejt}, which in turn depends on the market rent r^{m}_{jt}, this system of equations across all J locations simultaneously solves for the vector of market rents that makes all households' location choices consistent with the available housing stock in each neighborhood.

5.5 Rationing in the Affordable Housing Market

Second, we assume that the affordable housing market does not generally clear through a price mechanisms, since r_j^a is fixed. We define the supply of affordable units as Q_{jt}^a and the demand for affordable units as N^a, but these are not equal. Instead, demand for affordable units equal to the total low-type households that choose to live in location j:

$$N^a = \rho_{1j} H_1 \tag{16}$$

but this could very well be less than the supply of affordable housing units.

Instead, the rationing condition, which is the definition of the probability that a low-type household in location j successfully obtains an affordable unit, determines how the how much of demand for affordable units is met by supply of affordable units. Then, the low-type households who do not obtain affordable units are matched to market-rate units. For the reader's convenience, we rewrite the rationing condition from Equation (8) here:

$$p_j^a \;=\; \min\!\left(\frac{Q_j^a}{N_j^a},\, 1\right) \;=\; \min\!\left(\frac{Q_j^a}{\rho_{1j} H_1},\, 1\right).$$

5.6 Housing Stock Evolution (Dynamic Condition)

Recall that developers have the outside option of waiting to build each period in anticipation of higher market rents. Therefore, our model must provide a law of motion to connect the flow of new construction (from developer decisions) to the stock of housing (the state variable). This condition ensures that the model is consistent over time.

To be precise, our law of motion states that the total stock $Q_{j,t+1}$ and the affordable stock $Q_{j,t+1}^a$ in the next period are equal to the depreciated stock from today, plus the aggregate new construction from all developers of market rate units $q_{jt,\text{new}}^{\text{m}}$ and $q_{jt,\text{new}}^a$:

$$Q_{j,t+1} = (1-\delta) Q_{jt} + q_{jt,\text{new}}^{\text{m}} + q_{jt,\text{new}}^a \tag{17.1}$$

$$Q_{j,t+1}^a = (1-\delta) Q_{jt}^a + q_{jt,\text{new}}^a \tag{17.2}$$

where δ is the per-period depreciation rate of housing stock.

The aggregate new construction flows, $q_{jt,\text{new}}^{\text{m}}$ and $q_{jt,\text{new}}^a$, are the sum of optimal quantity choices from all developers i in neighborhood j (normalized to a mass M_j) who choose to build that period. Given the developer's problem in Equations (6.1) and (6.2), the aggregate flows are determined by the logit choice

probabilities and the optimal quantities:

$$q^{\text{m}}_{jt,\text{new}} = M_j \cdot P_{jt}(\text{m}) \cdot q^*_{jt,\text{m}} \tag{18.1}$$

$$q^{a}_{jt,\text{new}} = M_j \cdot P_{jt}(a) \cdot q^*_{jt,a} \tag{18.2}$$

where $q^*_{jt,\text{m}}$ and $q^*_{jt,a}$ are the optimal quantities from Equation 7.1 and Equation 7.2. The $P_{jt}(b)$ terms are the probabilities that a developer chooses option $b \in \{\text{m, a, wait}\}$:

$$P_{jt}(b) = \frac{\exp\left(\Pi(b, \mathbf{s}_{jt})\right)}{\sum_{b' \in \{\text{m, a, wait}\}} \exp\left(\Pi(b', \mathbf{s}_{jt})\right)} \tag{19}$$

As before, the profit functions $\Pi(\cdot)$ are defined as $\Pi(\text{m}, \mathbf{s}_{jt}) = \pi_{\text{m}}(\mathbf{s}_{jt})$, $\Pi(a, \mathbf{s}_{jt}) = \pi_a(\mathbf{s}_{jt})$, and $\Pi(\text{wait}, \mathbf{s}_{jt}) = \beta EV(\mathbf{s}_{j,t+1}|\mathbf{s}_{jt})$.

6 Estimation

We estimate the model through simulated method of moments (SMM). For a vector of parameters, we compute model moments and search to match observed data from 2016-2019 with a defined set of policy parameters (as there is no clean pre-policy equilibrium). We then validate our model by changing policy parameters (going from P5 to P6) and testing model fit against observed data from 2019-2024.

Our structural model has only four free parameters: household Fréchet taste dispersion θ, two developer cost shifters $\{c_M, c_A\}$, and the developer dispersion parameter σ. We identify θ from a 2SLS regression of log type-specific renter shares on log indirect utility across Seattle tracts, instrumenting endogenous rents with the Baum-Snow and Han (2024) floor-space supply elasticities γ_i and zoning-permitted density. Cost shifters $\{c_M, c_A\}$ are recovered by maximum likelihood on the universe of market-rate and permit decisions ($n = 6,117$) in Seattle from 2004 onward. Unfortunately the permitting data is not dense enough to use tract-level variation to identify developer σ_{jt}. Instead, we find the take-up gradient across nine major zoning classes (i.e. land use types) across Seattle (from 0.6% in lowrise to 38.5% in master-planned communities).

All other primitives are pinned externally. Policy parameters r^a, λ, and ξ come directly from Office of Housing schedules and levy rates. Tract-level supply curvature $\gamma_{\text{cost},i} = 1 + 1/\gamma_i$ is treated as exogenous from Baum-Snow and Han. Housing depreciation is set at 0.5% (estimated from Baum-Snow and Duranton (2025)). We additionally pull standard values for $\{\beta, \delta, N\}$.

Given parameters (θ, c_M, c_A, σ and fundamentals (residential amenities A_j and type-specific wages w^e_k), we solve for an inner fixed point of rents r^b and lottery rationing probabilities p^a_j. The remaining empirical

moments (rent difference-in-differences coefficients, income-quartile heterogeneity, the Duranton-Overman localization) are withheld from estimation and used as out-of-sample validation.

Our model focuses exclusively on renters, which raises potential data challenges. We construct population/housing supply (ACS) and rents (Craigslist) for renters. However, our commuting probabilites (LODES) and supply elasticities (Baum-Snow & Han, 2024), cover all workers. Since LODES does not distinguish housing tenure, our model may overstate renter commute distances. In Seattle, renters commute roughly 15–20% shorter distances than owners and compose of 0.55% of the population. A back-of-the-envelope calculation implies an upward bias of about 9% in mean renter commute distance. However, since our model only considers neighborhoods with multifamily housing (and MFTE), where renter shares typically exceed 80%, the implied bias falls to about 4%. This is a relatively small standard error for QSM calibrations of κ and we therefore proceed with identifying our commuting elasticity κ from mean commute distance, while we acknowledge tenure composition as a known, bounded source of measurement error.

6.1 Baseline rent fit

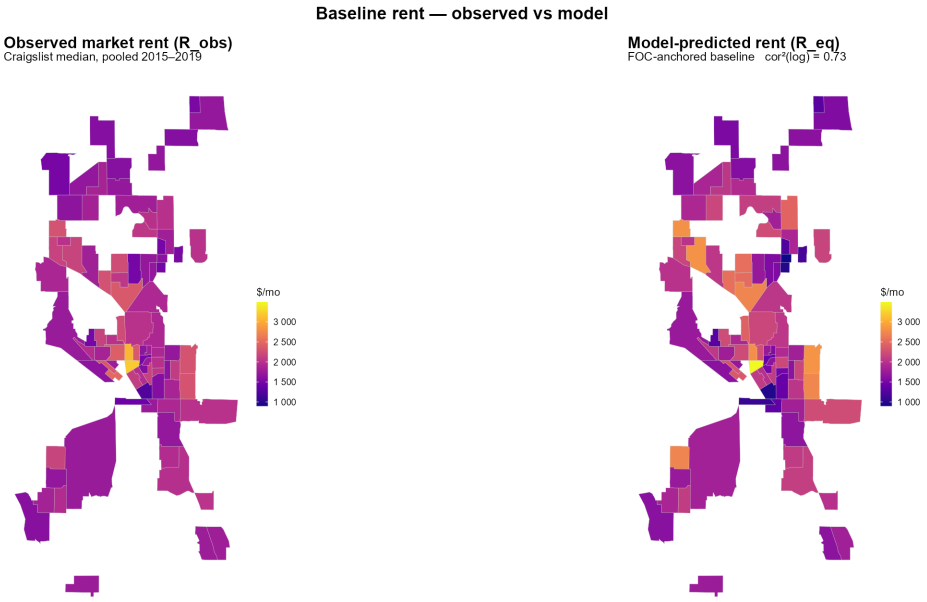


Figure 10: Baseline rent fit. Left: observed median Craigslist rent (2015–2019 pooled, dollars per month). Center: model-predicted equilibrium rent $\hat{R}_i^m$ under the calibrated primitives. Right: residual $\hat{R}_i^m - R_{i,\text{obs}}^m$ centered at zero. The left and center panels use a common *viridis-C* palette so tract shading is directly comparable.

In Figure 10, we compare observed rents to model-predicted rents. Observed market rents (left panel) are highest in the downtown core, as well as some affluent residential neighborhoods such as Ballard, while South, Southeast, and Northwest Seattle are among the lowest. The model surface (right panel) reproduces the same broad gradient but stretches it: model rents range from roughly $1,085 to $4,970 per month against

observed \$1,495–\$2,352. This bias arises directly because of the lack of homeownership in the model, as the richest households do not exist the rental market and buy homes.

7 Counter-factual Simulations

7.1 Intuition for Supply-side Dynamics

Before simulating the entire model, we illustrate developers' building decisions based on market rent and housing stock. Figure 11 simulates some of these processes in partial equilibrium, using a simple inverse housing demand, a fixed cost of construction, and a constant rate of housing stock depreciation. This is in effect an optimal stopping problem for developers, where the value and policy functions show the intuition that choice between affordable and market-rate building ultimately depends on some threshold of market rent.

However, our choice probabilities show an interesting dynamic where waiting is optimal near the threshold. When rents are borderline, waiting till existing housing stock depreciates (and rents rise) is more optimal than immediately building affordable units. This creates a source of dynamic inefficiency compared to the baseline steady-state where new housing exactly replaces depreciated stock.

This trough in affordable housing choices helps explain two of the key results of the model, that we estimate small aggregate elasticities, and a bunching equilibrium where affordable developers choose to build in the same neighborhoods with the appropriate conditions.

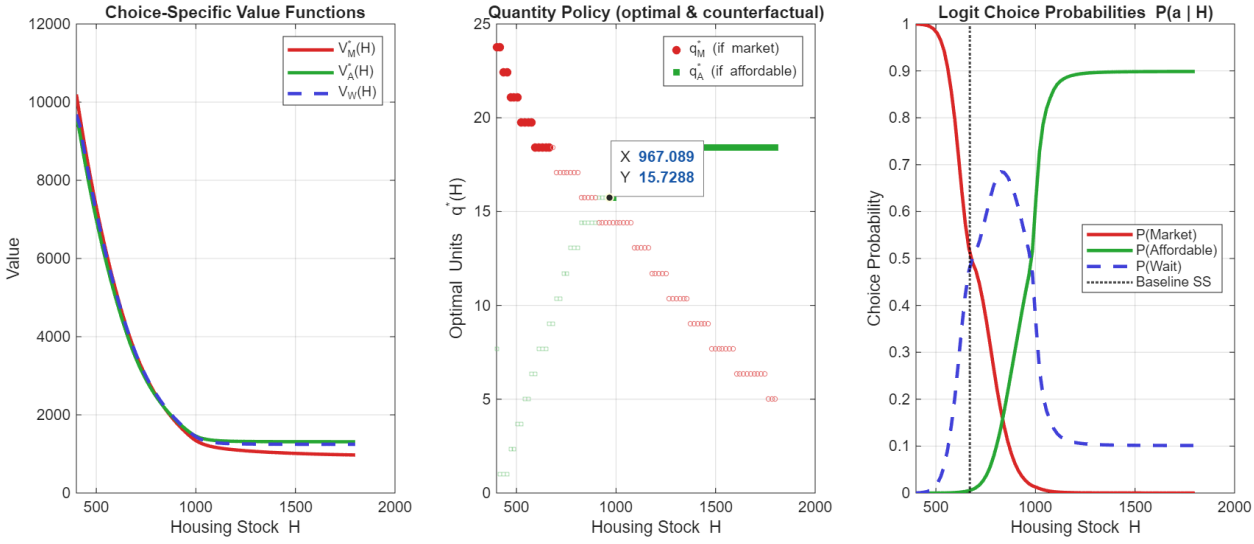


Figure 11: Developer value and policy functions based on market rent and housing stock.

7.2 Counterfactual 1: No MFTE

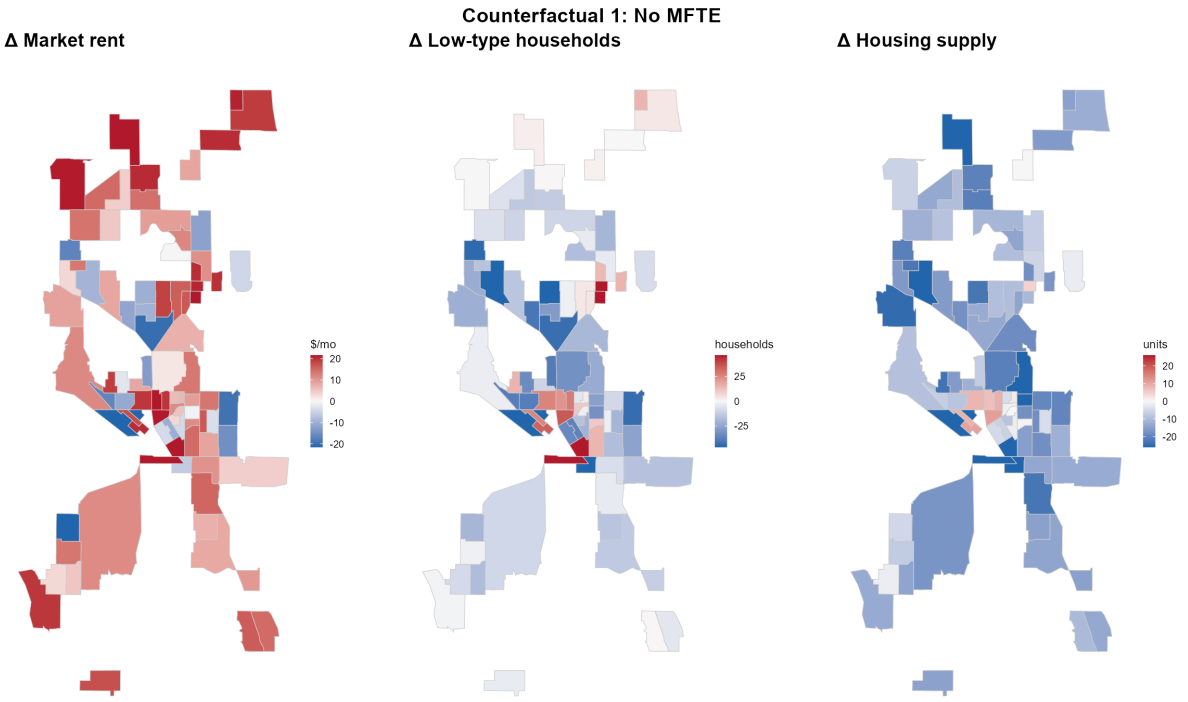


Figure 12: CF1 minus baseline. Panels show the tract-level change in market rent ΔR_i^m, MFTE-restricted supply ΔQ_i^A, low-skill population ΔN_i^L, and market- rate stock ΔQ_i^M. All panels are centered at zero and clipped at the 90th percentile of $|\Delta|$; recall that under the anchored-supply construction, $\Delta Q_i^M + \Delta Q_i^A = 0$ tract by tract, so the Q^M and Q^A panels are exact reflections. A companion map of the lottery win probability Δp_{ai} is not shown here because p_{ai} mechanically collapses to zero everywhere in CF1 (there is no MFTE lottery to win).

Our first counterfactual, shown in Figure 12, shuts the program off entirely: the property-tax abatement is set to zero for every tract ($\zeta_i = 0 \forall i$), no set-aside is imposed ($\lambda = 0$), and the affordable rent ceiling is pushed to infinity ($\bar{r}_a \to \infty$), which makes the MFTE branch strictly dominated in the developer's static logit. However, the effects of the removal of affordable housing are uneven across the city. In panel 1, we see that while market rent generally rises as predicted from the removal of a subsidy, this effects is not uniform. Rents actually fall in certain areas, as our endogenous amenity value fall in tandem. In panel 2, we see that low-type households become significantly more concentrated in lower-income neighborhoods, disappearing from higher-end residential neighborhoods in northwest and central Seattle. Finally, in panel 3, we see that housing supply generally shrinks across the city as predicted with the removal of a subsidy, although spatial reallocation means a small increase in housing in the downtown core.

We estimate small positive welfare impacts on high-type households but significant negative impacts on low-type households (compensating variation of $\sim$ +\$30/year and $\sim$ -\$403/year respectively). High-type household welfare increases primarily due to increased segregation from low-type households. Low-type households lose from two major channels. First, low-type households must pay higher rents in low-amenity

neighborhoods that would have had more housing. Second, they are more constrained in neighborhood choices and no longer have access to higher-amenity areas. While aggregate rents move very little (mean tract $\Delta R^m \approx +\$0.6$/mo), spatial heterogeneity means that individual effects are larger and affect low-type households more.

7.3 Counterfactual 2: Mandatory Affordable Housing

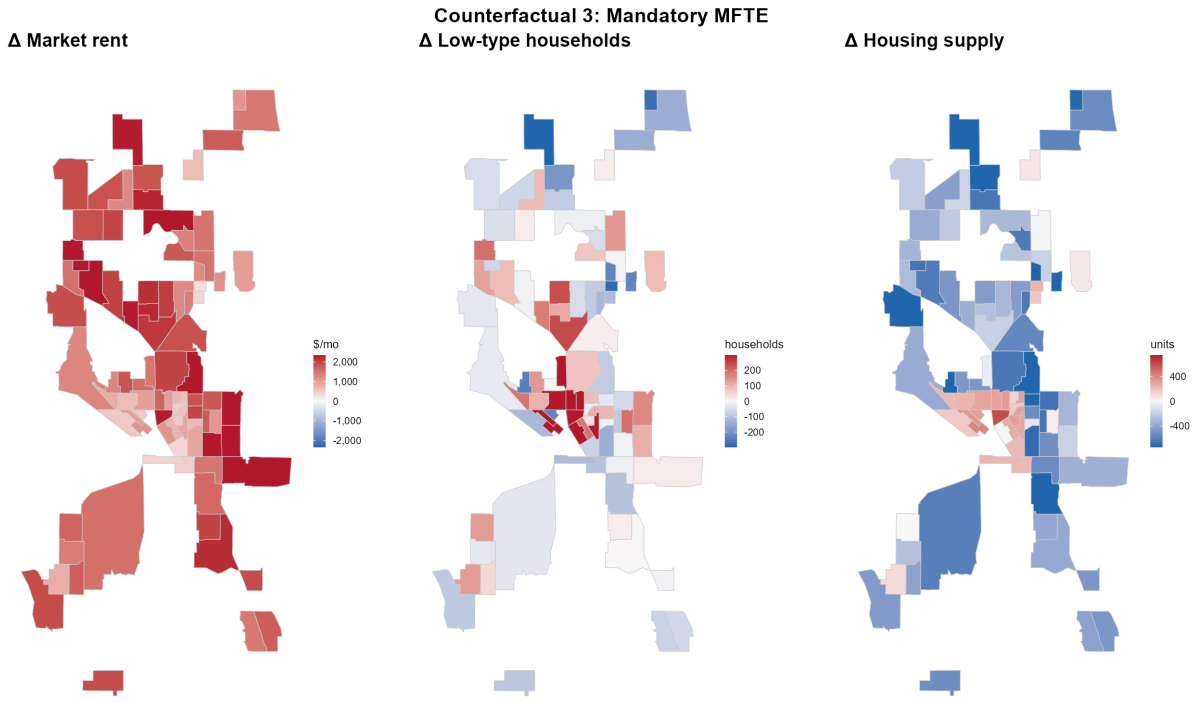


Figure 13: CF3 minus baseline. Same 4-panel layout as Figure 12: ΔR_i^m, ΔQ_i^A, ΔN_i^L, ΔQ_i^M. Every tract now has $Q_i^A = \lambda Q_i$ with $\lambda = 0.2$, so the Q^A and Q^M maps are exact reflections of each other (anchored-supply construction). The R^m level is under-identified in this scenario (see below); the map is normalized so that the tract geo-mean of R^m matches baseline, and reports the spatial reallocation of rents at that anchor.

Our second counterfactual, shown in Figure 13 turns the program mandatory for all developers. While this could function as a potential tax on all developers (with its accompanying market distortions), compensating subsidies imply heterogeneous incidence among developers. Aggregate MFTE-restricted stock jumps dramatically from 1,155 units in the baseline to 24,230 units, but as panel 3 shows, overall housing supply is reduced all areas other than the downtown core. In panel 1, we see that, as expected, developers shift the cost of the tax onto rents, particularly in tracts with lower supply elasticities outside of downtown. Low-type households, as shown in panel 3, are now freely able to choose the highest amenity and productivity locations, now that there are an abundance of affordable housing units.

We estimate large welfare gains to both high and low-skill households, with compensating variation +\$603/yr and \$7,296/yr for high and low-skill households respectively. This aggregates to a total renter-side

welfare change of +$336M/yr. With the abundance of affordable housing, lottery win probabilities rise by roughly a factor of 28.

The primary downside of an inclusionary mandate is fiscal cost, but smaller than one might expect. This is because Property-tax abatement expenditure, computed as $\sum_i \zeta_i \cdot Q_i^A$ with ζ_i held at baseline levels, rises from about $6,000 (baseline notional units) to $127,000.[22] Mandating MFTE at the calibrated ζ multiplies costs of the program by an order of magnitude without any offsetting change in stock composition. Compared to our compensating variation, the tax required to fund the program is an order of magnitude smaller than the welfare gain,. This differs from the usual intuition that inclusionary mandates are expensive per beneficiary because developers seek to minimize tax incidence. Improved access to high-amenity areas more than makes up for the tax costs, and relatively little direct transfers occur because developers choose neighborhoods that minimize the implied tax incidence.

8 Conclusion

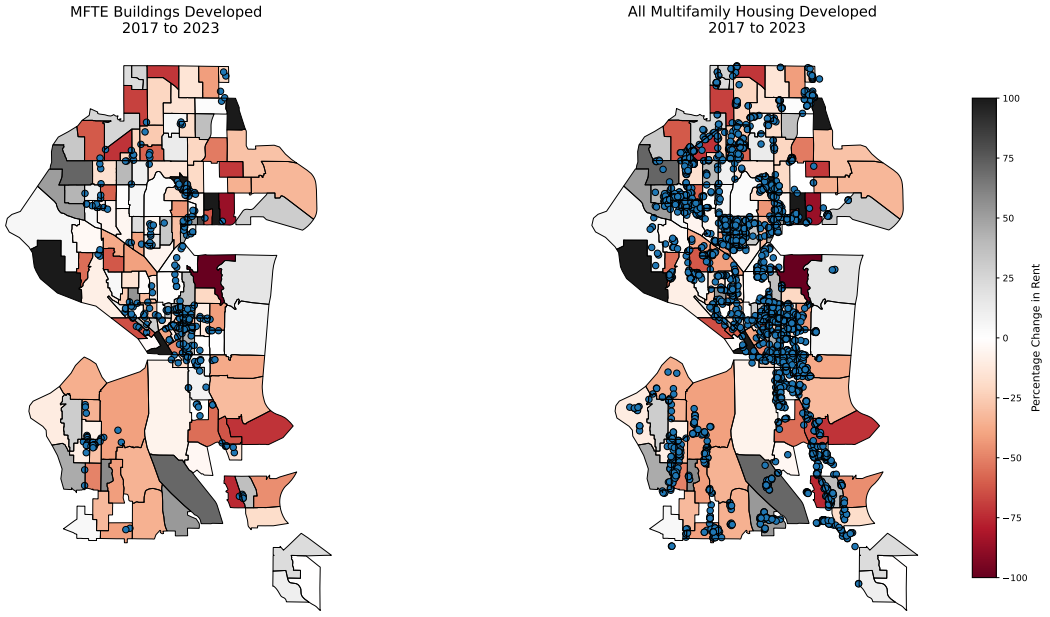
This paper leverages a novel micro-dataset of Craigslist rental data to directly estimate an IZ program's spillover effects on nearby rents. While we borrow methodologies from the literature on LIHTC, such as Diamond and McQuade (2019), we focus on housing rents on our dependent variable, rather than house prices or rents inferred from property values. Our findings from this causal analysis are consistent with a story of countervailing mechanisms where IZ lowers market rents through local competition in lower-income areas in the short-term, but also pushes rent upwards by improving amenities, especially in more affluent areas. This latter effect can possibly incentivize further housing development by boosting housing demand, but it may also contribute to further gentrification in already rich areas. We also provide descriptive evidence that demonstrates how different policy choices (e.g., tightening or relaxing affordable rent restrictions) can result in different spatial distributions of developer program participation and housing development.

We develop a quantitative urban model to clarify the policy implications of the mechanisms underlying the empirical patterns documented above. The model characterizes how an affordable housing program reallocates housing supply, rents, and households across space. Developers choose whether to build on the extensive margin—constructing market-rate housing, affordable housing, or not building—and on the intensive margin, determining the quantity of units supplied. These choices provide a microfoundation for how affordable housing policies can change housing composition and stock. By distinguishing between high- and low-type households and imposing a rationing constraint on affordable units, the model also explains why IZ development can generate larger rent reductions in lower-income neighborhoods, where supply expansions

[22] Note that in the scaled model this is directional rather than dollar-accurate, but the 21× ratio implies large expenditures

disproportionately benefit low-type households. When examining our counterfactual simulations, affordable housing provides access to high-income areas for low-type households and increases supply in low-income areas. Housing policy must account for distributional effects when evaluating the full welfare implications.

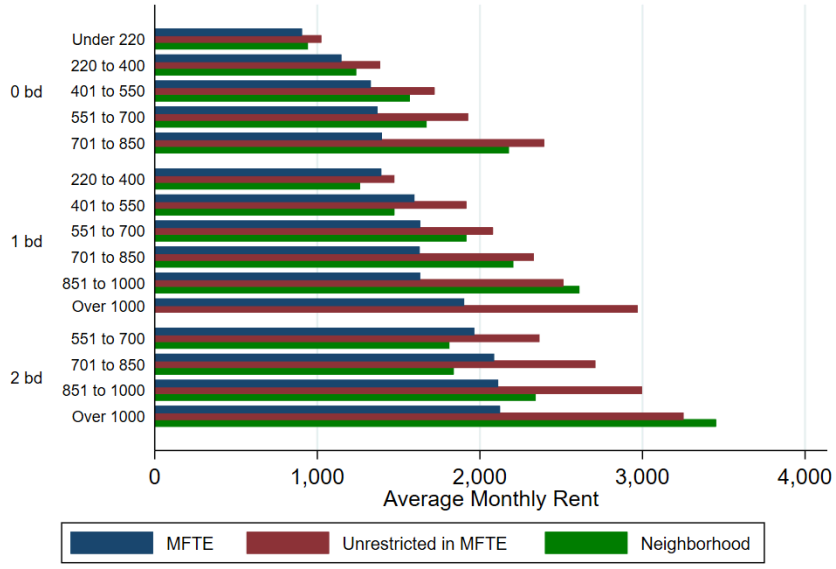
A Appendix



(a) Locations of all MFTE housing buildings constructed from 2017–2023, using permitting data provided by the City.

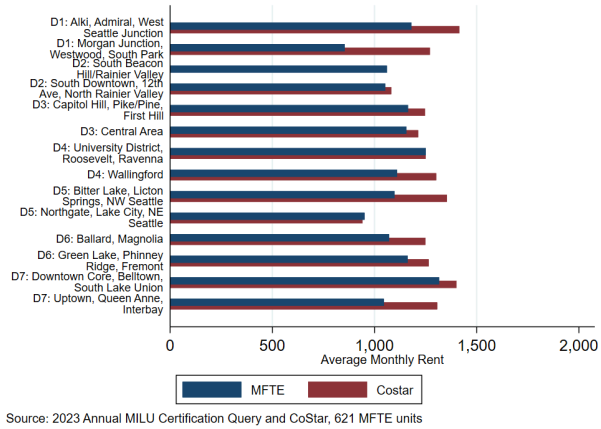
(b) Locations of all multifamily housing buildings constructed from 2017–2023, using permitting data provided by the City.

Figure A.1: Comparison of multi-family and MFTE housing locations (2017–2023).

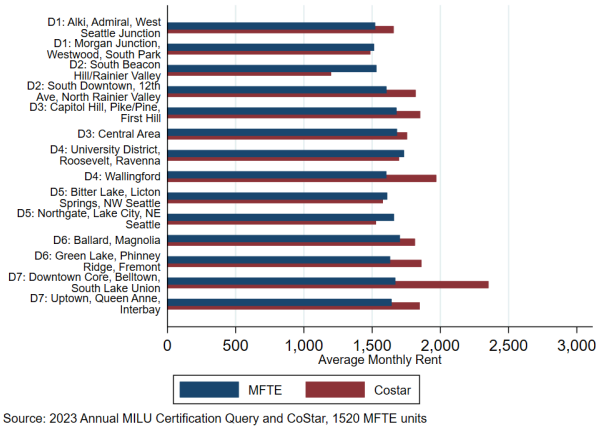


Source: 2023 Annual MILU Certification Query and CoStar, only categories with 50+ units citywide

Figure A.2: MFTE vs. market rents by number of bedrooms and square footage of units



(a) MFTE vs. market rents for studios



(b) MFTE vs. market rents for one bedroom units

Figure A.3: MFTE vs. markets rents by neighborhood

Table A.1: Results from placebo stacked difference-in-differences analyses using non-MFTE buildings, by neighborhood income segment of Craigslist listings. The dependent variable for all specifications is log(Rent). All specifications include controls for number of bedrooms, three spline terms in square footage, tract fixed effects, and year-month fixed effects. Standard errors are clustered robust by Census tracts of Craigslist listings.

	Treated Ring Width		
Income Segment (bolded) and Variables	0 to 0.2 mi	0.2 to 0.4 mi	0.4 to 0.6 mi
Below 40th Percentile: $[\$0, \$65K]$			
Post × Near Single	0.060	-0.059	-0.008
	(0.039)	(0.035)	(0.044)
Post × Near Multiple	0.075***	-0.142***	-0.080*
	(0.015)	(0.028)	(0.042)
Observations	214,172	119,033	97,685
R-squared	0.713	0.668	0.669
40th to 60th Percentile: $(\$65K, \$110K]$			
Post × Near Single	-0.019	0.028	-0.017
	(0.020)	(0.035)	(0.019)
Post × Near Multiple	-0.001	-0.015	0.001
	(0.012)	(0.011)	(0.014)
Observations	46,920	41,649	42,227
R-squared	0.713	0.692	0.741
Above 60th Percentile: $> \$110K$			
Post × Near Single	-0.019	0.028	-0.017
	(0.020)	(0.035)	(0.019)
Post × Near Multiple	-0.019	0.014	-0.107***
	(0.017)	(0.017)	(0.019)
Observations	61,682	37,565	20,106
R-squared	0.698	0.691	0.614

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