

The Impact of an Increasing Housing Supply on Housing Prices

The Case of the District of Columbia, 2000 -2018

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Introduction

Following a decades long period of population decline ending in the late 1990s, Washington, DC's net population increased by over 100,000 people between 2000 and 2018. To accommodate a growing population, the city added a net amount of over 40,000 rental units since 2000. But despite adding over 2,100 rental units annually, the city is still generally seen as having an undersupply of housing of all types, particularly for relatively low-income residents.

This paper seeks to answer two questions concerning housing in the District of Columbia. First, what has been the impact of the substantial increase in the supply of rental housing in recent years on average apartment rents in the city? And second, what is the estimated impact of a substantial increase in the planned number of rental housing units on apartment rents as of 2025? We answer these two questions by way of three economic model estimations. The first determines the effect of the actual increase in the number of apartment units in the city on rents for years 2000 to 2018. The second estimation simulates the average citywide apartment rents in 2018 if the city had not doubled the rate of new additions beginning in 2012. And, the third estimation simulates average rents in 2025 under the Mayor's recent plan to stimulate an exceptionally large number of new rental units beginning in 2020.

This study finds that if the delivery of the markedly large number of new apartment units in recent years had not occurred, average city apartment rents may have been 5.84 percent higher in 2018. That is, the average citywide monthly apartment rent could have been \$3,207 in 2018 rather than the actual average of \$3,030. This study also finds that if the planned increase in new additions (under the Mayor's 2019 Housing Initiative) does not occur, then average city apartment rents are estimated to be 5.53 percent higher in 2025. That is, the average citywide monthly apartment rent is likely to be \$3,261 in 2025 instead of \$3,090 under the Initiative. This situation suggests that even though the city's demand for rental units is growing (as a product of a growing population, a growing number of jobs and growing incomes), the actual increases in supply is helping to mitigate the annual appreciation rates of apartment rents. In essence, the city is likely to continue experiencing modest annual growth rates in rents in the near term and, as a result, lower average levels of rent in the medium and longer terms.

Data

The economic models used in this study use quarterly data spanning the period 2000Q1 to 2018Q4. The CityRent variable is the citywide average per square foot asking monthly rent for apartment units, and the NewUnits variable represents the total number of new apartment units added each quarter of the study period. These data variables are quarterly time series for citywide mid- and high-rise class A and class B apartment buildings with 20 or more units, built as early as 1960. The source of the apartment data is CoStar, a real estate information firm. The model also uses quarterly data for the mean per capita income, population and unemployment variables from the Office of the Chief Financial Officer, Office of Revenue Analysis (ORA). The housing price index is from the U.S. Bureau of Labor Statistics and is for all urban consumers in the Washington metropolitan region.

Monthly per square foot rents rose from \$1.68 psf (per square foot) in 2000 Q1 to \$3.05 in 2018 Q4, an 81.5 percent increase over the period (Table 1). The study period started with a citywide total of 16,476 class A and B units in multifamily buildings and grew 236.3 percent to a total of 55,414 units. A total of 38,938 new units of this type were added, with an average of 2,049 new units added per year. Over the same time, the city’s resident population grew 24.8 percent from 567,136 to 707,647. In total, 140,511 people were added during this time period.

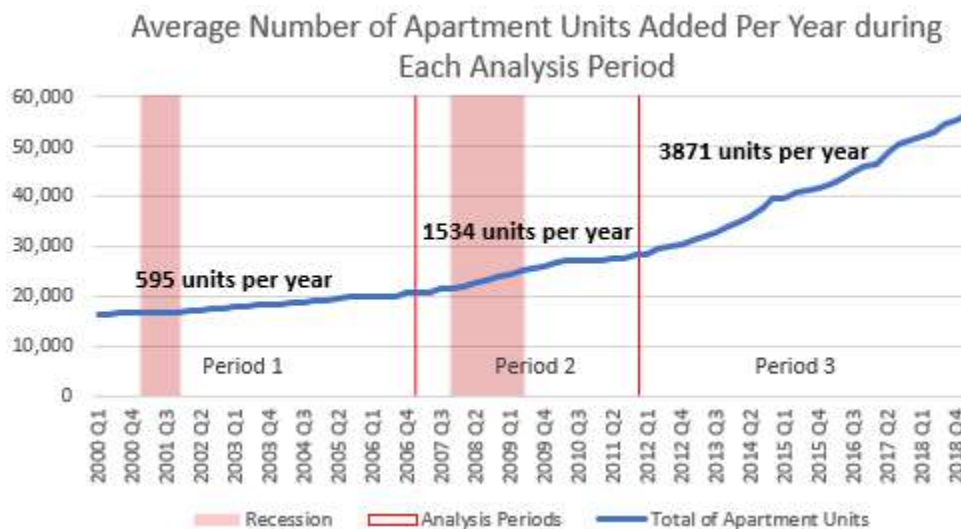
Table 1

Summary Statistics for Dataset				
	Mean	Standard Deviation	Minimum	Maximum
Per SF Rent	\$2.32	\$0.44	\$1.68	\$3.05
New Units	11,896	11,317	0	38,938
All Units	28,372	11,317	16,476	55,414
Population	615,048	48,154	567,136	707,647

Source: CoStar and Office of Revenue Analysis

At the outset, the whole study period from 2000 to 2018 is separated into three subperiods (Figure 1). In period 1 (2000-2006), an average of 595 new units were added per year. In period 2 (2007-2011), an average of 1,534 new units were added per year; and in period 3 (2012-2018), an average of 3,871 new units were added per year. For the same three periods, the annual average growth in rents were 0.62 percent, 1.03 percent and 0.70 percent, and population grew at average annual rates of 0.003 percent, 0.46 percent, and 0.41 percent respectively. With a homeownership rate of 38.4 percent and a rentership rate of 61.6 percent as of 2018, we estimated that 38,028 net new renter households were added to the city in years 2000 to 2018.¹

Figure 1



Source: CoStar

¹ There is not a one-to-one ratio between population and demand for housing and pinning down the exact household number is difficult. However, using the latest population to housing ratio number of 2.276, we estimate that approximately 61,735 households were added.

Methodology: A Housing Model

When analyzing a housing market, it is important to note how housing is different from the general idea of a typical good. Housing is classified as a composite good, meaning that it is a basket of different attributes or features purchased all together. Size, quality, location, neighborhood amenities and distance from the city center are all key attributes of housing that impact its market price. Following are key variables used in our model.

The demand for housing is a function of several factors. Like all other goods, income plays a major part in determining demand. Income level determines the budget each household has, and this budget is allocated between spending on housing, and spending on all other goods. Market rents continually adjust to attain equilibrium where demand and supply of rental housing are equal to each other.² In line with the classic city model, housing units tend to be occupied by households willing and able to afford the highest rent for such units.³ Because permanent changes in household incomes can affect the total amount each household is able to spend, controlling for changes in income is important in uncovering the impact of supply on price.

The level of unemployment in a housing market also impacts demand for housing. Higher levels of unemployment tend to reduce the demand for housing, while lower levels of unemployment tend to increase the demand for housing. And just as when the average income levels of an area decrease, higher levels of area unemployment often spur some shifting of housing demand from more expensive forms of housing to less expensive forms of housing in some cases and outmigration in other cases. A single-family home, townhouse, condominium and apartment rental are housing alternatives. Single-family homeownership tends to be the most expensive form of housing and apartment rentals tend to be the least expensive. Changes in the price of one alternative form of housing often causes the price for the alternative forms to become either relatively cheaper, or relatively more expensive. Hence a change in the price of one form of housing tends to prompt a substitution with another type of housing. In the model, changes in the price of alternative goods is captured in the housing price index (HPI) for the Washington-Arlington-Alexandria area.

The size of the population competing for the available stock of housing is also seen to impact demand. More households mean more demand and thus greater competition for housing. When the total demand for housing increases, we expect the price for housing to rise. Higher housing prices crowds some potential purchasers out of the market and increases the number of units as more suppliers will find it profitable to produce more housing. As such, the citywide number of apartment units in each time period is the independent variable of interest in the model.

We used an Ordinary Least Squares (OLS) regression to predict the dependent variable, CityRent, based on the independent variables and to estimate the effect of each independent variable on the dependent variable for each quarter⁴. The model includes a lagged new units

² DiPasquale and Wheaton, 1996

³ Alonso 1964, Mills 1972, Muth 1969

⁴ Analysis of the data reveals an upward time trend in the key variables of rent, units, and population. The data shows a very strong and significant positive correlation between rent and population, with a Pearson Correlation Coefficient of 0.98. The correlation between rent and inventory units is also very strong, significant and positive,

variable, a dummy variable for time period 2 and another dummy variable for time period 3. For the two dummy variables, period 1 is the reference point. And while the variables CityRent and NewUnits are the most important variables in the model, CityRent is the key variable of interest.

The empirical model is specified as follows:

$$\text{CityRent} = \beta_0 + \beta_1 \text{lag NewUnits} + \beta_2 \text{Income} + \beta_3 \text{HPI} + \beta_4 \text{Population} + \beta_5 \text{Unemployment} + \beta_6 \text{Period2Dummy} + \beta_7 \text{Period3Dummy} + \varepsilon_{it} \quad (1)$$

Where

- CityRent is the model's dependent variable (DV) and represents each quarter's change (differenced year over year) in average city rent amount per square foot for apartment units.
- β_0 is the model constant
- β_1 is the change in the DV per an increase in new apartment units delivered to market in the previous quarter
- β_2 is the change in the DV per an increase in the city's per capita income
- β_3 is the change in the DV per an increase in the area's home sale price index for single family owner-occupied homes
- β_4 is the change in the DV per an increase in the city's population
- β_5 is the change in the DV per an increase in the city's unemployment rate
- β_6 represents the change in the DV for time period 2
- β_7 represents the change in the DV for time period 3, and
- ε_{it} is the error term.

The intent of this study is to explain changes in the city's apartment rents with respect to major demand factors. Theoretically, we aim to distinguish whether the increase in the number of housing units the city has experienced in recent years is due primarily to the increase in demand (with correspondent increases in rent), or due primarily to an increase in supply, which would lower rents or work to slow down the annual rent appreciations that tend to occur each year.

Model Results

Years 2000-2018

By way of the model specified in equation (1), the regression results in Table 2 indicate in general that as the number of new apartment units in the city, home prices and unemployment increase, the average city rents tend to decrease. The table also shows that as per capita income and population increase, the average city rents tend to increase. Additionally, the model results indicate that the growth in average city rents was lower in time period 2 (2007–2011).

with a Pearson Correlation Coefficient of 0.96. The data was therefore first differenced to eliminate the trends in the data and ensure summary statistics of the data are not dependent on time.

Table 2

Citywide Regression Model Results For Years 2000 to 2018		
Variables	Coefficient	Standard Error
Constant	0.078***	(0.011)
New Units (YD)	-0.000016***	(0.00001)
Income (YD)	0.00001***	(0.000003)
HPI (YD)	-0.002***	(0.0004)
Population (YD)	0.008***	(0.002)
Unemployment	-0.015***	(0.006)
Period 2	-0.74**	(0.028)
Period 3	-0.047	(0.037)

Note: Standard errors are shown in parentheses and statistical significance indicated at the 1%(***), 5%(**), and 10%(*)level.

Translating the regression results, we find that after controlling for key factors the average city rent (assuming a 1,000 square foot apartment) increased \$0.08 psf every year in time period 1 and time period 2. For time period 3, city rents increased only by an average of \$0.07 psf per year (Table 3). Table 2 and Table 3 present the core set of relationships between the variables and serve as the basis for the following policy simulations.

Table 3

Estimated Annual Average Change in Rent Per Square Foot	
Time Period	Change in Rent
Period 1 (2000-2006)	\$0.08
Period 2 (2007-2011)	\$0.08
Period 3 (2012-2018)	\$0.07

Policy Simulation #1 (City added 16,000 fewer units in years 2012-2018)

This policy simulation uses model equation (1) with all the same data for the predictor variables except for the NewUnits variable. This policy simulation uses simulated data for the NewUnits variable (i.e. a counterfactual). That is, instead of using actual new units data for years 2012-2018 (an average of 3,871 new units per year), we use simulated data that reflects that the new units trend for years 2007 to 2011 (an average of 1,534 new units per year) continues to 2018. After rerunning the model with the simulated data, we obtain the counterfactual results for years 2012 to 2018 and compare them to the actual results for the same years.

Projecting period 2 changes in new rental units into period 3, the average annual change in rent per square foot (psf) is \$0.10. This suggests that as of 2018 the average annual net difference (savings) was \$0.03 psf (Table 4). The actual rent growth during time period 3 was a

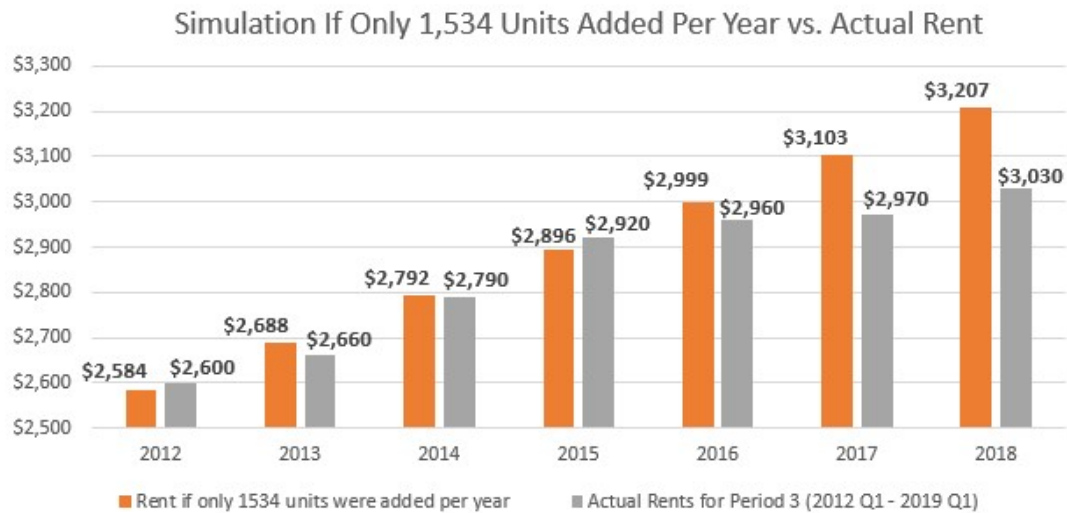
total of 22.2 percent. The model estimates that rents would have grown 29.3 percent absent the actual increase in rental units.

Table 4

Net Savings from Increasing New Units from 1,534 per Year to 3,871 per Year, 2012-2018			
	Average Units per Year	Avg. Ann. \$ Amt Change in Rent psf	% Change in Rent over the period
Period Three (Simulation – Fewer units)	1,534	\$0.10	29.3%
Period Three (Actual)	3,871	\$0.07	22.2%
Net Savings	-	\$0.03	-

If an average of 1,534 new units per year continued throughout period 3, we estimate monthly rents of approximately \$3.21 psf in 2018. This is in comparison to the actual 2018 rent of \$3.03 psf. Stated differently, the model estimates average monthly rent in the city for 2018 would have been \$3,207 per month (5.8 percent higher) if the level of new units had not increased from the period two average of 1,534 units per year to the actual period three average of 3,871 units per year (Figure 2). Therefore, we conclude that the average additional 2,337 units (the difference between 3,871 and 1,534) each year for years 2012-2018 can be explained by an increase in supply. It appears that this finding contributed to the slowdown in average annual rent increases, which afforded renters \$177 in monthly savings or \$2,124 in savings for the entire year of 2018.

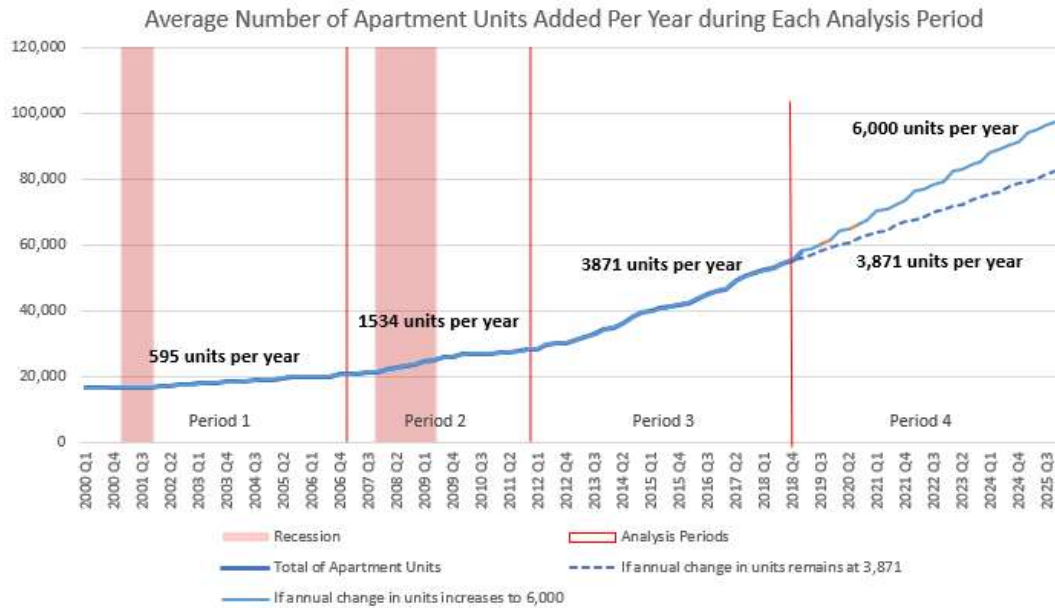
Figure 2



Policy Simulation #2 (City adds 13,000 more units than trend in years 2020–2025)

In May 2019, the city’s Mayor signed an order “directing District agencies to identify new policies, tools, and initiatives to begin fulfilling her bold goal of creating 36,000 new housing units... by 2025.”⁵ The Mayor’s Initiative is to add a total of 36,000 additional units by 2025 or an average of 6,000 units per year beginning in 2020 (Figure 3).

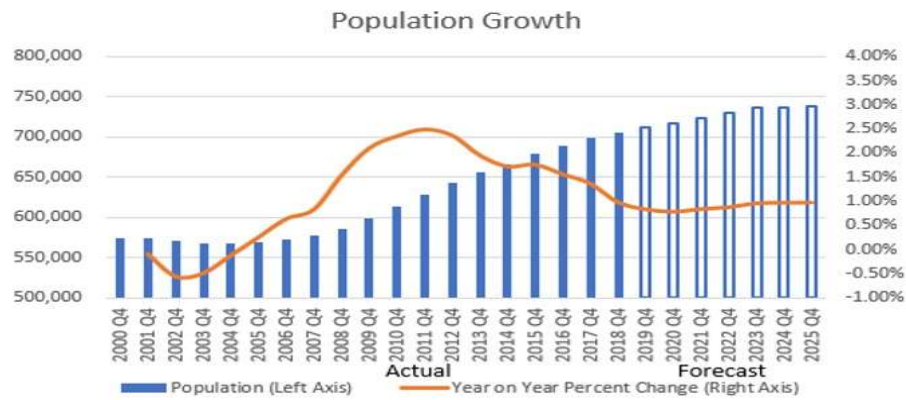
Figure 3



This model simulation uses the actual economic data for years 2000 to 2018 and forecasted (trend) values for the same variables for years 2019 to 2025. However, the population growth variable changes appreciably for years 2019 to 2025 from the prior years. Between 2011 and 2019, the city’s annual population growth rate slowed from 2.6 percent in 2011 to a little less than one percent in 2019. Forecasts for the annual population growth for the 2020 to 2025 period is expected to remain stable around one percent per year (Figure 4). Whereas the city added population by an amount of 10,881 (net) per year during time period 3, the city is expected to add only of 6,860 (net) new residents per year during time period 4. Therefore, Policy Simulation #2 for years 2020 to 2025 will allow for two economic variables to change: NewUnits and population.

⁵ <https://mayor.dc.gov/release/mayor-bowser-signs-order-drive-bold-goal-36000-housing-units-2025>

Figure 4



Source: Office of Revenue Analysis

To allow for changes in two variables while still isolating the effects of each on the dependent variable, Policy Simulation #2 comprises three model estimations. Model Estimation A describes the effect of the independent variables as indicated in time period 3 and continued along trend to year 2025. Model Estimation B assumes only that the population growth rate slows down to about one percent per year beginning in 2019 and all others variables remain the same as in Model Estimation A. Model Estimation C assumes the population growth rate slows down to about one percent per year beginning in 2019, that there is full implementation of the Mayor’s Housing Initiative of 2019, and that all others variables remain the same as in Model Estimation A. This three-step approach allows us to isolate not only the effect of slowing population growth but also the effect of the Mayor’s Initiative on average city rents.

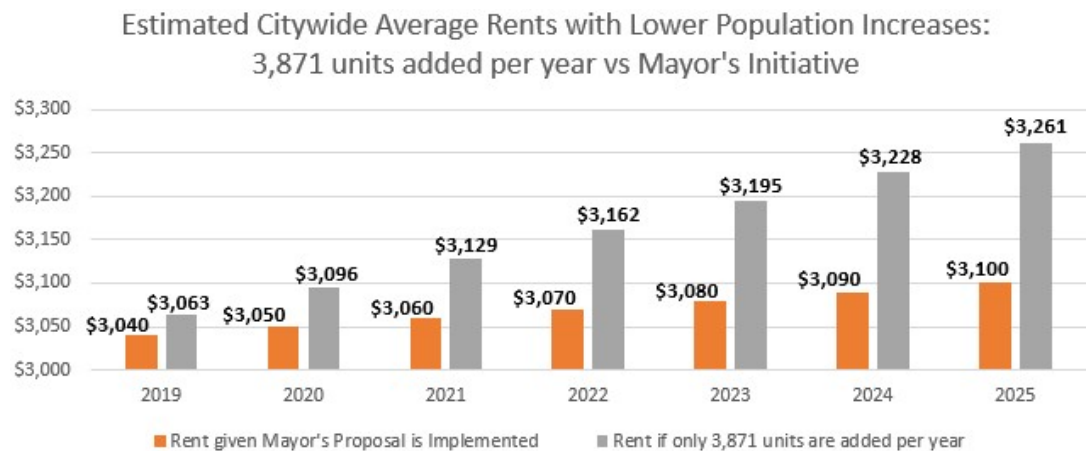
Assuming all economic trends from time period 3 continue into time period 4, Model A estimates an average annual net change in rent per square foot of \$0.07 per square foot in years 2019 to 2025 (Table 5). This would amount to an average rent of \$3,520 per month in 2025. But, since 2010 the city has been experiencing continuous declines in its population growth rate, Model Estimation B assumes the city will gain a net increase of only 6,860 new residents during time period 4 (37 percent less than in period 3). Under this scenario, the model estimates an average annual net change in rent per square foot of \$0.03 per square foot in years 2019 to 2025, and this would amount to an average rent of \$3,261 per month in 2025. Finally, allowing for a lower population increase and the Mayor’s Housing Initiative, Model C estimates an average annual net change in rent per square foot of only \$0.01 per square foot in years 2019 to 2025. This would amount to an average rent of \$3,100 per month in 2025.

Table 5

Net Savings from Increasing New Units from 3,871 per Year to 6,000 per Year For Years 2019 to 2025 (Time Period 4)				
	Average Population Increase per Year	Average Units per Year	Average Annual Change in Rent psf	% Change in Rent over the period
Model Estimation A: Continue Period 3 trends	10,881	3,871	\$0.07	16.2%
Model Estimation B: Period 3 trends except for lower population	6,860	3,871	\$0.03	7.6%
Model Estimation C: Period 3 trends except for lower population & Mayor's Housing Initiative	6,860	6,000	\$0.01	2.3%
Net Savings (Simulation B minus Simulation C)			\$0.02	-

Policy Simulation #2 predicts that the current trend of lower annual population increases, by itself, will help slow down average annual rent increases in the coming years. However, the Mayor's Initiative of adding 13,000 more units than trend will fortuitously help slow down average annual rent increases even further. With full enactment of the Mayor's Initiative, city rents are estimated to amount to approximately \$3,100 (4.9 percent less) per month in 2025 instead of \$3,261. This would amount to monthly savings of \$161 in 2025 alone and \$1,932 for the year (Figure 5).

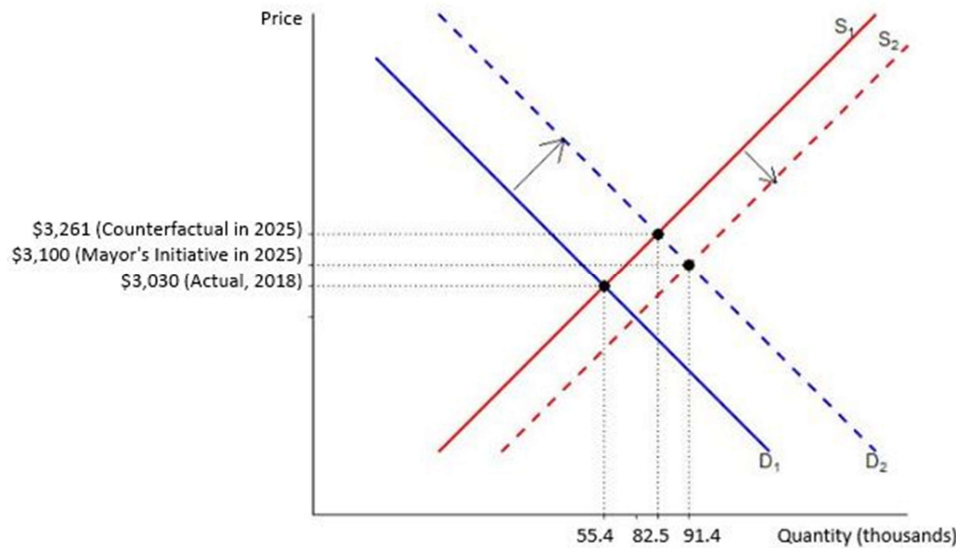
Figure 5



According to the model, the city's growing population, income and number of jobs are increasing the demand for housing in the city. But, the city is continuing to assertively facilitate more housing units (particularly rental housing units). According to economic theory, this predicament can be illustrated in terms of market demand and supply curves as shown in Figure 6. The vertical axis represents the average monthly rent for apartments in the city, and the horizontal axis represents the total number of class A and class B rental units in the city. The figure illustrates that while the city's demand factors are expected to continue to cause the demand curve in 2018 for rental housing units to increase (shift to the right) over time, the city's supply of rental housing units is also increasing (also a shift to the right) in part via a plethora of city policies involving land use, tax, zoning, land reclamation, regulation and housing. Thus, the regression model estimates that citywide average rent in 2025 would be

\$3,261 (7.6 percent higher than in 2018) absent an aggressive increase in new units. But, adding 36,000 new units despite declining population growth rates is expected to cause rents to be only \$3,100 (2.3 percent higher than in 2018).

Figure 6 The District of Columbia’s Rental Housing Market in 2018 and 2025



The figure also shows how the city had 55,400 units in 2018 in mid- and high-rise class A and class B apartment buildings with 20 or more units. For this subset of housing units, the city’s growing economy would naturally (i.e. according to trend) produce 27,100 new units as of 2025 (82,500 total units). But, the Mayor’s Initiative essentially increases the city’s rental units by an additional 8,900 (10.8 percent) as of 2025 (91,400 total units).

Discussion

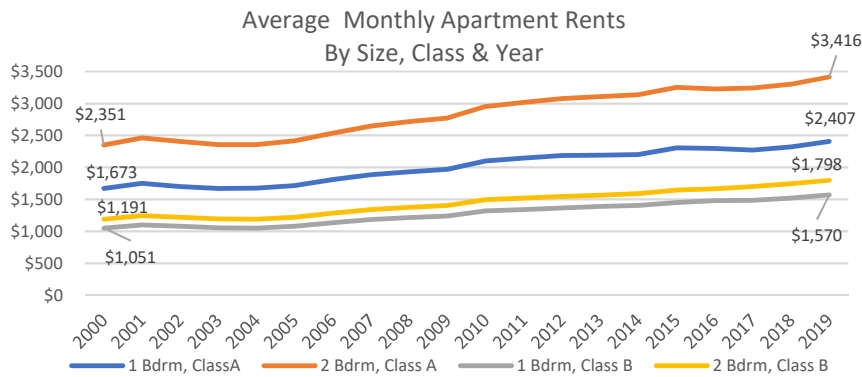
Rental Housing as An Alternative to Homeownership

Even though the levels of apartment rents in the city are relatively high⁶, average apartment rents have grown relatively modestly since 2000 (Figure 7). Over the study period, class A one-bedroom and two-bedroom rents grew at an annual average rate of approximately 1.99 percent. This is in accord with the annual average growth rate in the area’s consumer price index over the same time period, which was 2.2 percent.⁷

⁶ In the District of Columbia, the average monthly rent for a one-bedroom in 2019 was \$2,407 and \$3,416 for a two-bedroom. The rents for two-bedroom Class A apartments tend to be 40 percent higher than one-bedroom Class A apartments. The rents for two-bedroom Class B and C apartments tend to be 15 percent higher than one-bedroom Class B and C apartments. The rents for one-bedroom Class A apartments tend to be about 53 percent higher than one-bedroom Class B & C apartments. And, two-bedroom Class A rents tend to be almost double two-bedroom Class B & C apartments rents.

⁷ CPI

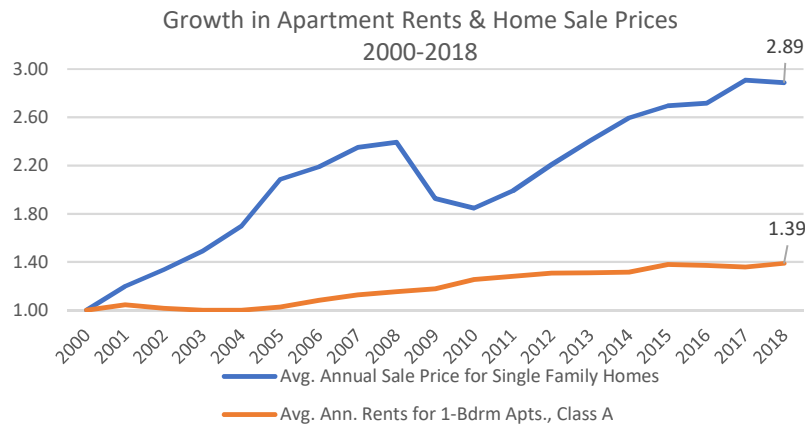
Figure 7



Source: CoStar

In contrast, the average sale price for a single-family home in the city was \$286,100 in 2000 but \$825,898 in 2018. This is almost a tripling in the sale price (188.7 percent) over the time period and an annual average increase of 6.1 percent (Figure 8).⁸

Figure 8



Source: CoStar and MRIS/GCAAR

The District had 103,000 apartment units of all types in 2000, and the supply of units grew to 144,000 in 2018, a 40 percent increase.⁹ On the other hand, property tax records indicate that there were 91,000 single family structures¹⁰ in the city in 2000 but only 101,000 in 2018, an 11 percent net increase.¹¹ In a city with a growing population of primarily 25 to 44 year-olds, it

⁸ In 2000, the median sale price for a single-family home in the city was \$178,250. But in 2018, the median sale price for a single-family home was \$725,000. This is a 306.7 percent increase over the time period and an annual average increase of 8.1 percent. Source: GCAAR

⁹ CoStar

¹⁰ This excludes condominiums and co-operatives.

¹¹ Property tax records also indicate that there were 71,490 homesteads properties in the city in 2000 and 93,826 in 2018. Homestead properties are owner-occupied residential properties that are the principal residence of the owner. Owners applied with the Office of Tax and revenue for an annual deduction from their home's assessment value, which allows for a lower real property tax bill. The deduction also allows for annual increases in the taxable assessment value of the home to be limited to 10 percent beginning in 2006.

appears that the relative lack of growth in the overall supply in single family structures is just one of the considerable contributors to the general unaffordability of single-family home ownership. Conversely, rental housing appears to have been kept more affordable to some extent due to the substantial increase in the supply of rental units.

Other Contributing Factors

There are numerous factors contributing to the addition of an average of 2,100 new apartment units per year from 2000 to 2018. These include a growing population, accommodating financial and capital markets and the growing national and local economies. Moreover, the city incentivizes residential development (particularly investment-grade large multifamily projects) through its tax code. The city's 2019 residential property tax rate of \$0.85 (per \$100 of assessed value) was 55 percent less than the commercial property tax rate of \$1.89 (per \$100 of assessed value). And, the city has the lowest real property tax rate for residential property in the metropolitan region. The city's residential property tax rate is 22.3 percent lower than Montgomery County's property tax rate of \$1.0934 and 17.2 percent lower than Arlington County's rate of \$1.026.¹²

A Major Local Determinant

Beyond the financial and economic factors, the District of Columbia government continues to play a major role in facilitating increased residential housing capacity via its assertive use of land, zoning, tax, regulatory and housing policies. The city government has not only allowed more¹³ and higher density residential development¹⁴ throughout the city, but it has also helped to make large swaths of land acreage (in some cases formerly federal and other tax exempt land) available for development.¹⁵ For example, the city's newest neighborhoods of the Capital Riverfront, Southwest Waterfront, NOMA, and Union Market were made possible via explicit government interventions such as special tax incentives or local public subsidies in some cases and efficacious land use, zoning, regulatory and housing policies in other cases. And based on current residential development trends in the city, it is possible that even a few additional new

¹² The District of Columbia is the only jurisdiction in the metropolitan region that has different tax rates for different types of property. The city has residential tax rate of \$0.85 (per \$100 of assessed value) and a commercial tax rate of \$1.89 (per \$100 of assessed value). In the District of Columbia, all multifamily buildings are considered residential property. Arlington County has the lowest tax rate in northern Virginia, and Montgomery County has the lowest property tax rate in suburban Maryland. All counties in the state of Maryland and Virginia apply one tax rate to all properties. Source: Tax Facts, 2019.

¹³ For example, more than 4,000 residential units have been produced out of former office buildings since 2002. But, the more striking trend that has taken hold in the city in recent years has been the conversion of church properties (formerly tax-exempt properties) into residential developments. In some cases, a residential development totally replaced the church, and in other cases the former church structure was replaced with both a new church structure and a large residential development onto the site. Some examples are the 13th Street Sanctuary (3431 13th St, NW), Stanton Tower (609 Maryland Ave., NE), The Vintage (3146 16th St., NW), The Churchill (514 4th St, SE), Scripture Cathedral (810 O St., NW), Riverside Baptist Church (699 Maine Ave., SW) and St. Augustine Episcopal Church (555 Water St., SW).

¹⁴ For example, popups are a form of residential expansion and increased density because they add stories on top of the older buildings which allow for changes in the number of units per structure (e.g., converting a single-family house into multiple apartments, or vice versa). Schuetz, J. (2019). Teardowns, Popups and Renovations: How Does Housing Supply Change? *Journal of Regional Science*, 2019; 1-22.

¹⁵ In 2000, the District devoted one percent of its land area to multifamily development. But, in 2018, multifamily development existed on 8 percent of its land area. Tax Facts, 1999 and 2019.

communities may materialize or mature in the coming years such as the Parks at Walter Reed, Buzzard Point, Capitol Crossing, Reunion Square, and the Skyland Town Center.

Helping to Make Rental Housing More Affordable

The District of Columbia is currently facing an affordable housing crisis, whereby an increasing number of city households are finding it impossible to afford rental housing in the city, much less homeownership. Affordable housing is primarily a function of housing costs relative to household income in a given time period. In the short-term, facilitating affordable housing for relatively low-income households requires a unique set of immediate policy responses.

However, this study maintains that through its assertive use of land use, zoning, regulatory, housing and tax policies the city is having a greater effect helping to prevent rental housing (in contrast to home ownership) from becoming entirely unaffordable for low- and moderate-income households. It appears that this long-term policy of aggressively increasing the city's supply of rental units has, in part, precluded sharp price increases in the medium and short terms.

Conclusions

There are a number of contributing factors why the District of Columbia added an average of over 2,100 new apartment units per year since 2000. But, one of the primary local determinants has been the city government's overall policy to facilitate, induce and even incentivize (in some cases) rapid residential redevelopment throughout the entire city. This overall policy is multifaceted in that it comprises, but is not limited to, zoning, regulatory, land use (including the reclamation of former federal and tax-exempt land), housing and tax policies. This has resulted in over 20 years of allowing both the conversion of voluminous parcels throughout the city to their current highest and best use (residential in many cases) and greater housing density in numerous areas of the city.

In the face of a growing average annual number of new rental units demanded by renters since 2000, there has been a commensurate average annual number of new rental units added to the city's housing market. This appears to be one of the reasons why average annual citywide rent increases has remained on par with the area's inflation rate. This paper suggests this phenomenon has contributed to relatively lower levels of average citywide rents in recent years compared to the counterfactual of a much slower annual increase in the housing supply.

We estimate that the considerable increase in rental units caused the average city renter to save \$2,124 in housing rental costs in 2018. And if this policy continues as planned by city leaders, we estimate that the average renter in the city could save an additional \$1,932 in housing costs in 2025 relative to the counterfactual of a much slower annual increase in the housing supply. It is conceivable that in recent years the city government's overall policy toward residential redevelopment has contributed to slightly less prohibitive rents for at least a few residents resulting in the retention of residents who otherwise would have migrated out of the city.

12-19-2019

Supply Shock Versus Demand Shock: The Local Effects of New Housing in Low-Income Areas

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Supply Shock Versus Demand Shock: The Local Effects of New Housing in Low-Income Areas

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ABSTRACT

We study the local effects of new market-rate housing in low-income areas using microdata on large apartment buildings, rents, and migration. New buildings decrease nearby rents by 5 to 7 percent relative to locations slightly farther away or developed later, and they increase in-migration from low-income areas. Results are driven by a large supply effect—we show that new buildings absorb many high-income households—that overwhelms any offsetting endogenous amenity effect. The latter may be small because most new buildings go into already-changing areas. Contrary to common concerns, new buildings slow local rent increases rather than initiate or accelerate them.

JEL Classification Codes: R21, R23, R31

Key Words: housing supply, housing affordability, gentrification, amenities

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Introduction

Housing costs have risen rapidly relative to incomes over the past 60 years in the United States, particularly in large and economically successful cities (Gyourko et al. 2013; Albouy, Ehrlich, and Liu 2016). This trend has increased rent burdens for low-income households, reduced regional economic convergence, and slowed national economic growth (Ganong and Shoag 2017; Hsieh and Moretti 2019). One potential solution is to increase the supply of housing by allowing more market-rate construction, which both theory and recent empirical results suggest will reduce regional housing costs (Rosenthal 2014; Mast 2019).

However, new housing developments could counterintuitively increase costs in their immediate area, raising questions about the incidence of such policies. This could occur because of amenity or signaling effects—if new units attract high-income households and new amenities that make the area more appealing, it could raise demand by enough to offset the increased supply. Recent literature on the importance and endogeneity of amenities suggests that this mechanism is plausible.¹ Moreover, worries about local rent increases lead many residents and policymakers to strongly oppose new construction, especially in low-income or gentrifying areas where high-end apartments and their occupants represent a large change from the status quo (Hankinson 2018; Been, Ellen, and O’Regan 2019). Although this idea has played a significant role in the housing debate, the very local effects of new market-rate housing have been understudied, likely due to data limitations and the endogenous location of new construction.²

This paper provides new empirical evidence on how the construction of large market-rate rental apartment buildings in low-income, central city neighborhoods affects nearby rents and migration. We assemble granular, address-level microdata on new construction and outcomes for 11 major cities, which allow us to address endogeneity concerns by leveraging extremely local variation in the timing and location of new construction.³ We find that new buildings lower nearby rents by 5 to 7 percent relative to trend and increase in-migration from low-income areas. If there is an endogenous amenity effect, it appears to be overwhelmed by the standard supply effect. Our results suggest that, unlike in other contexts where diffuse policy benefits must be weighed against concentrated costs, there is not a trade-off between

¹Guerrieri, Hartley, and Hurst (2013); Diamond (2016); and Su (2019) demonstrate the importance of endogenous amenities and residential sorting for housing costs and neighborhood composition. Baum-Snow and Marion (2009), Diamond and McQuade (2019), and Davis et al. (2019) show that building Low-Income Housing Tax Credit housing can increase home prices in some cases.

²For example, opposition from tenant groups concerned about rent increases near new development helped defeat California Senate Bill 50, which aimed to broadly increase residential zoning (Brey 2019; Dillon 2019).

³Our sample includes Atlanta, Austin, Chicago, Denver, Los Angeles, New York City, Philadelphia, Portland, San Francisco, Seattle, and Washington, D.C.

the regional and local effects of new housing construction. It benefits both.

Causal identification in this setting is challenging because developers select the locations of new buildings based in part on unobserved local characteristics and trends. In addition, the size and shape of a new building’s amenity or reputation effects is unknown, making it difficult to know where they may shrink or reverse the negative effect of added supply. We attempt to overcome these challenges by leveraging our unique data to construct three related empirical strategies. The first is a difference-in-differences specification that compares the area very close to a new building to the area slightly farther away (our “near-far” specification).⁴ The idea is that frictions in the land assembly and development approval processes lead to random variation in building placement and timing at the hyper-local level, making the outer area a good control for the treated inner area. This specification is well suited to detect one way that new buildings could raise rents—through amenity effects that fade out quickly with distance, such as increased retail options or the replacement of a vacant lot.

The second exercise is a difference-in-differences that compares listings near buildings completed in 2015 and 2016 to listings near buildings completed in 2019, after the conclusion of our sample (our “near-near” specification). The underlying logic is that developers choose sites in both groups for similar reasons, but one building is completed before the other for idiosyncratic reasons, such as the timing of when sites are available for purchase. Because the treatment and control areas are not necessarily in the same neighborhood, this specification can detect price changes driven by broader effects that may cross the near-far boundary of the first exercise. A variant of this specification also allows us to examine how congestion effects near the building could influence our results. Finally, we combine both sources of variation into a triple-difference specification that effectively compares the near-far difference around 2015–2016 buildings to the near-far difference around 2019 buildings.

We first study the effect on rents using listing-level data from ZillowTM that span 2013 to 2018. All three empirical approaches show that new construction in low-income neighborhoods (census tracts) reduces nearby rents by 5 to 7 percent.^{5,6} Event study plots support the parallel trends assumption and suggest that the negative effect begins in the same year as building completion and persists for at least three years after. The most likely source of

⁴Our primary specification uses 250 meters as the treatment group and 250–600 meters as the control group. The former is one or two city blocks, and 600 meters is an 8- to 10-minute walk. Schwartz et al. (2006) provide evidence that the positive effect of replacing blighted lots fades quickly within this range.

⁵We define low-income neighborhoods as census tracts with median household incomes below the metropolitan area median, which is a common definition of “gentrifiable” used in the gentrification literature.

⁶We consider the sample of new buildings in all neighborhoods in an extension and find noisy estimates that are statistically indistinguishable from zero. This may occur because the demand elasticity for established high-income areas is much larger than for gentrifying low-income areas, leading a supply shock to have a smaller effect on prices.

remaining bias—that developers still have some ability to target the best locations and times even at a very local scale—would shift our estimates toward zero.

While our strategies are well-suited to detect rent increases driven by amenity effects that center around the new building, other shapes are possible. For instance, if buildings generate congestion effects that fade out very quickly but relatively broad positive amenity effects, rents could increase in a doughnut shape around the building. We test this story directly and find no supporting evidence. In an extension, we use the empirical derivative method of Diamond and McQuade (2019) to estimate a continuous rent effect of new buildings and find that it shrinks monotonically with distance. While we cannot test every possible spatial pattern in which new buildings could increase prices, our exercises rule out the most likely alternative stories.

In our second set of results, we study the effect on in-migration using individual address histories from Infutor Data Solutions. In-migration speaks directly to the policy debate on neighborhood change, allows us to study cheaper segments of the market that may be under-represented in the Zillow™ data, and is the primary channel through which neighborhoods change (Freeman and Braconi 2004; McKinnish, Walsh, and White 2010; Brummet and Reed 2019).⁷ In our near-near specification, we find that new construction decreases the average origin neighborhood income of in-migrants to the nearby area by about 2 percent. It also increases the share of in-migrants who are from very low-income neighborhoods by about three percentage points, suggesting that new buildings reduce costs in lower segments of the housing market, not just in the high-end units that are the most direct competitors of new buildings. Results are similar in the triple-difference specification and null in the near-far specification.⁸

Our exploration of where new market-rate rental apartments are built provides one explanation for why endogenous amenity or reputation effects may be small: new construction typically occurs after a neighborhood has already begun to change. Although we restrict to neighborhoods that are relatively low-income, those that receive new buildings are relatively high-education and experienced more income and education growth over the previous decade compared to neighborhoods that did not receive any new buildings. This suggests that rather than catalyzing demographic change in previously stable neighborhoods, new market-rate construction in low-income areas tends to follow neighborhood change, or gentrification. It

⁷We discuss out-migration later when presenting descriptive statistics but cannot study it directly due to data limitations described in Section 1.

⁸The migration results are generally noisier than the rent results. One possible explanation is that the Infutor data include both renters and homeowners, who may be differently affected by new rental construction. In addition, there may be a greater lag in the effect on migration outcomes, either because outcomes are actually slower to respond or because Infutor does not pick up a move immediately when it occurs.

may therefore provide little additional impetus for new amenities or signal that a neighborhood is now desirable. Instead, its primary effect appears to be to accommodate preexisting demand, diverting high-income households from nearby units and reducing rents.

These results matter for policy. Approving new housing in low-income areas is often contentious because of worries that new buildings will accelerate rent increases and gentrification. These local concerns also spill over into the regional policy debate and can stall large-scale housing reforms (Brey 2019; Dillon 2019). While these worries may be understandable given the dramatic changes occurring in many neighborhoods, our results suggest they are generally misplaced. Instead, policymakers should recognize that new market-rate housing has both local and regional benefits and should therefore be an important part of strategies to address the growing affordability crisis. In addition, our migration results suggest that strategies that encourage housing construction also foster more economically integrated neighborhoods, which could promote economic mobility for low-income residents (Chetty, Hendren, and Katz 2016; Chetty et al. 2018).

However, there are a few reasons for caution. First, our findings are specific to the large market-rate apartments and strong market cities that we study, and effects could differ for other types of housing or other areas if amenity effects depend on local context. Second, we are only able to follow outcomes for three years after building completion, though we provide evidence that longer-run effects are likely similar to our estimates. Finally, the actual implementation of reforms that increase housing supply requires changing complicated zoning and land-use regulations. Policymakers should keep in mind that the particulars of those changes could affect where housing is built—for example, in vacant lots or through demolition of existing affordable housing.

We contribute to a small but growing literature on the effects of new housing supply. Much of this work focuses on regional effects and uses models rather than quasi-experimental strategies. Favilukis, Mabilie, and Van Nieuwerburgh (2019) and Nathanson (2019) develop calibrated spatial equilibrium models and find that new housing supply promotes regional affordability. Mast (2019) traces the sequence of moves induced by new construction to show that new construction loosens lower-income housing markets throughout a region. By contrast, Anenberg and Kung (2018) develop a neighborhood choice model and find small effects of new supply on prices. We complement these papers by focusing on the very local effects of new housing, which may differ from regional effects and play a large role in the current policy debate.

A few very recent papers also focus on the local effects of increased housing supply. Li (2019) finds that large new buildings in New York City lower nearby buildings' rental income even as they increase the number of nearby restaurants. By contrast, Singh (2019) studies

construction sparked by a 2006 property reform in New York City and finds that each new unit increases nearby buildings' rental income by over 2 percent (relative to a declining trend). We differ from these papers by studying many cities, using listing-level rent data, and examining migration outcomes.⁹

Finally, there is a large literature on land-use regulation, much of which focuses on the effects of regulatory barriers to housing construction.¹⁰ It shows that restrictive regulations lead to higher rent and house price growth (Quigley and Raphael 2004, 2005; Pollakowski and Wachter 1990), less migration into economically successful cities, (Ganong and Shoag 2017), and city- and society-wide welfare losses (Hsieh and Moretti 2017; Bunten 2017; Parkhomenko 2017). Our research adds to this literature by suggesting that market-rate housing construction not only improves regional affordability, but also neighborhood affordability.

The rest of this paper is organized as follows. Section 1 describes our building, rents, and mobility data and provides new descriptions of where market-rate housing is built. Section 2 describes our main empirical strategies. Section 3 presents the rent results, and Section 4 presents the migration results. Section 5 concludes.

1 Data and Summary Statistics

1.1 New Buildings Data

Data on large new rental apartment buildings are provided directly by Real Capital Analytics, a real estate market research firm that aims to track the universe of such buildings. Our starting sample includes 1,483 buildings with over 50 units that were completed in 2010–2019 in the following 11 central cities: Atlanta, Austin, Chicago, Denver, Los Angeles, New York City, Philadelphia, Portland, San Francisco, Seattle, and Washington, D.C.¹¹ The data include the name, year completed, number of units, and exact address of each building, which we geocode and match to 2010 census tracts (our definition of a neighborhood).

In order to focus on the types of buildings central to the policy debate and most likely to have large effects on amenities or neighborhood character, we impose several restrictions on our analysis sample.

⁹Other work has studied adjacent questions about new development. Boustan et al. (2019) find that the introduction of condominiums did not increase central city income, education, or share white, which they attribute in part to the fact that most were built in areas that were already attractive to high-income households. Freemark (2019) finds that increasing zoned capacity in Chicago raised the value of affected land but does not study the effect on rents.

¹⁰See Gyourko and Molloy (2015) and Glaeser and Gyourko (2018) for recent reviews.

¹¹We restrict to the borough of Brooklyn when studying New York City because this is the only borough included in our data use agreement with ZillowTM.

1. *Large market-rate rental buildings:* We remove buildings that are income-restricted, senior or assisted living housing, and buildings flagged as student housing or located in tracts that are over 25 percent college students.¹² We also restrict to buildings with 50 units or more, as these are most contentious and represent the largest physical and direct demographic change to a neighborhood. Finally, we drop the small number of large condominium buildings in order to focus on the much more common rental buildings.
2. *Low-income central city neighborhoods:* We restrict to buildings constructed in initially low-income central city neighborhoods, which we define as census tracts that are in the principal city of their metropolitan area (defined as a Core-Based Statistical Area, or CBSA) and which have median household income below the CBSA median in the 2013–2017 American Community Survey.¹³ We focus on low-income areas because they are the primary concern in the policy debate and are likely where amenity and reputation effects are largest.
3. *Sample years:* We restrict to buildings completed in 2015 and 2016 when analyzing effects on rents and 2014 and 2015 when analyzing effects on migration. This ensures that we have a sufficient number of years before and after treatment in our outcome data.
4. *Sufficient nearby listings:* We restrict to buildings with at least one observation (a listing for rental outcomes and a move for migration outcomes) in both the treatment and control areas in every year in the sample.¹⁴ This restriction improves the precision of our estimates and helps to ensure that we are picking up buildings in preexisting neighborhoods where gentrification may be a concern, rather than large brownfield redevelopments.
5. *Isolated:* We include only buildings that had no other new buildings completed within 250 meters (the baseline treatment radius) between 2010 and their date of completion. This identifies relatively stable neighborhoods where a new building may represent a larger shock.

¹²We do so using a field provided with the data, which includes descriptions such as “subsidized” or “affordable.” We do some spot checking to ensure that these flags are accurate.

¹³This is a common definition of “gentrifiable” used in the gentrification literature.

¹⁴The definition of treatment and control areas changes depending on the specification. For the near-far approach, the treatment group is within 250 meters of a building, and the control is those between 250 and 600 meters. For the near-near approach, the treatment area is within 250 meters of early buildings, and the control is the same distance from later buildings.

It is worth emphasizing that we are studying the effects of the buildings captured in this sample, not other sources of new housing such as single family homes, smaller apartment buildings, rehabs and renovations, or affordable housing developments. Moreover, our focus on isolated buildings precludes us from saying anything about the effect of building large clusters of buildings at the same time. However, our data suggests that clustering is limited. Of 1,483 total new buildings, 783 (53 percent) are isolated according to our definition, 336 (23 percent) are in a cluster of 2 total buildings, 178 (12 percent) are in a cluster of 3 total buildings, and the remaining 186 (13 percent) are in a cluster of 4 or more buildings. While large clusters of development such as Nationals Park in Washington, D.C., or Hudson Yards in New York City are interesting and salient, our data show they are relatively rare. Further, because these large clusters typically occur in industrial redevelopments rather than already established residential neighborhoods, they are less relevant to the policy debate. Finally, note that our sample does not drop buildings that successfully spur subsequent nearby construction, since we only require that buildings are isolated at the time of completion.

Table 1 shows building characteristics and location for four different subsamples. The first column, “All Incomes 2010–2019,” restricts to large, market-rate buildings in central cities completed between 2010–2019 (restriction 1 in the above list). It gives an overall sense of the levels and types of construction occurring in our 11 cities. The second column further restricts to buildings built in low-income neighborhoods, which yields 823 buildings. The third column further restricts to those built in the years of our analysis sample (2014, 2015, or 2016), yielding 253. Finally, the last column adds the sufficient listings and isolation restrictions to yield our final rental analysis sample of 92 buildings. While the final sample is much smaller than the starting sample of 1,483, the table shows that most of the loss is simply from studying low-income neighborhoods in the years 2014 to 2016. The remaining decline is partly explained by the isolation restriction, which removes 106 buildings, and partly by the listings restriction, which tends to drop industrial redevelopments that are less relevant to the gentrification debate.

Despite the loss of observations, Table 1 shows that the different samples of low-income buildings are quite similar in both characteristics and location, giving us confidence that our analysis sample is relatively representative. The median number of units in our final sample is 121, though there is considerable variation: the 25th and 75th percentiles are 68 and 256. The nearby rent numbers show statistics for the Zillow™ listings within 600 meters of the new buildings (the baseline control radius).¹⁵ The median nearby rent in our final sample is \$1,807 for all units and \$1,514 for one-bedroom units.

¹⁵For each new building, we take the average of nearby listings. We then report the mean, median, and other percentiles of the distribution of these building-level means.

We next consider what types of neighborhoods new developments are built in. Table 2 describes characteristics of central city neighborhoods (census tracts) that did and did not receive new construction between 2010 and 2019. The first two columns represent characteristics of neighborhoods of all income levels, with the first including only neighborhoods that did not receive construction and the second only those that did (weighted by the number of new buildings in each neighborhood), and columns 3 to 6 represent different groups of low-income neighborhoods. The rows show levels and growth rates for three sets of characteristics—income, education level, and rents—that are often used to characterize socioeconomic status and gentrification.¹⁶

The table reveals several interesting patterns that may help explain our main results. In both the all-income and low-income samples, neighborhoods with construction experienced much faster preperiod growth in college and income share and somewhat faster growth in rents. While the levels of income were similar across samples during the preperiod, the areas that received construction had higher college shares, which is often considered a leading indicator of gentrification. These results suggest that new buildings in low-income areas are typically built in neighborhoods that are already in early stages of gentrification. These areas may be appealing to developers because they are already attractive to the kinds of high-income residents who can pay the higher rents required for new construction.¹⁷ In contrast, building market-rate units in low-income neighborhoods that are not gentrifying may be seen as riskier, as they may not be able to attract high-income residents.

Finally, despite our restrictions, the final analysis sample again appears to be relatively representative of buildings in low-income neighborhoods in general. In contrast, the neighborhoods that never receive construction are quite different, which cautions against generalizing our results to hypothetical buildings in the kinds of low-income, central city neighborhoods that developers currently rarely target.

1.2 Rental Data

We pair the new building data provided by RCA with listing-level data on rental prices provided by ZillowTM, which includes listings from all websites (ZillowTM, Trulia, StreetEasy,

¹⁶The table shows absolute levels and changes, but patterns are nearly identical when de-meaned within each CBSA. This and the similar distribution of building locations from Table 1 suggest that these patterns are unlikely to be driven by changes in sample composition.

¹⁷Another reason why developers target already-gentrifying neighborhoods could be that LIHTC investments can crowd out private development in neighborhoods that are lower income or in less-advanced stages of gentrification (Baum-Snow and Marion 2009; Eriksen and Rosenthal 2010). The Department of Housing and Urban Development (HUD) most heavily subsidizes LIHTC development in census tracts where at least 50 percent of residents are below 60 percent of the area median gross income or have a poverty rate of at least 25 percent. These tracts tend to be poorer than the neighborhoods we study here.

and HotPads) in the Zillow[™] Group during 2013 to 2018. For each listing, we observe price, location, date of listing, and the number of bedrooms and bathrooms.¹⁸ The major restriction is that the sample includes only listings in buildings with 50 units or less. Because there are many such small buildings, this gives excellent geographic coverage, as demonstrated in Appendix Figure A.1, which shows the listings within 800 meters of a new building in Chicago in 2018. This dense coverage is useful for spatial identification strategies.

In total, we have rents for about 740,000 units within 800 meters of one of our new apartment buildings. Appendix Tables A.1 and A.2 show mean rent for all apartments and one-bedroom apartments in each CBSA-year. Rents generally increase over time and are higher in traditionally high-cost cities, but there is some variation in this pattern, possibly because of changes in which units are listed online. In our analysis, we always include CBSA \times year fixed effects (or more granular effects) to absorb these compositional changes.

It is difficult to verify whether this sample of rents is representative, as Census Bureau statistics measure contracted rents, which could be quite different than the average listed rent at a given point in time. However, the mean rent for Zillow[™] listings within 600 meters of a new building in our final sample, shown in Table 1, is \$1,790, whereas the average ACS median rent in the tracts containing those buildings is only \$1,165 (shown in Table 2). This suggests that Zillow[™] rental listings skew toward higher-end units. Given that gentrifying areas often feature listings across a wide range of rents, this is important to bear in mind when interpreting our results. This later motivates us to use the migration data introduced below to directly study cheaper segments of the market. In addition, the differences between tract-level and nearby Zillow[™] rents is consistent with developers targeting new construction to relatively higher-rent areas even within small neighborhoods. Similarly, Appendix Figure A.2 shows that rent (de-measured at the CBSA-level) trends monotonically downward as distance to the nearest new building increases. This is exactly the endogeneity problem that our identification strategy aims to address.

1.3 Individual Migration Data

We use longitudinal individual address histories from Infutor Data Solutions to construct measures of migration. Infutor creates these data using numerous private and public record sources—USPS change of addresses, county assessor records, magazine subscriptions, phone-books, et cetera—and sells the data for use in targeted advertisements. Because addresses are intended to be used in direct mailing, they are high quality and reported at the unit level. Addresses are matched with an estimated arrival date, and there are limited demographics

¹⁸Because a small number of listings have extreme values, we winsorize the variable at the 1st and 99th percentile in all exercises.

(age and gender) for each individual. The data closely match the census over-25 population at the tract level, with about 0.9 observations per census individual, and the coverage is similar across tract characteristics.¹⁹ The Infutor data do miss some moves—the annual migration rate is 5.4 percent, versus 9.8 percent in the 2018 Current Population Survey—but this ratio appears to be uncorrelated with county characteristics.²⁰

We use the address histories to measure the origin tract income of the individuals moving into the new buildings and into the nearby preexisting buildings.²¹ These outcomes are useful for two main reasons. First, they are directly related to the policy debate about whether new buildings accelerate neighborhood demographic change or gentrification.²² Second, they allow us to directly consider lower-income segments of the housing market by focusing on migration from low-income areas, whereas the Zillow™ data may skew toward the upper end of the housing market. A limitation of observing origin tract income rather than actual household income is that we cannot separate low- and high-income individuals within a tract, making it difficult to study low-income out-migration, which features prominently in the policy debate.

We observe about 1.9 million moves to an area within 800 meters of a new building between 2011 and 2017, including about 60,000 to a new building directly. Figure 1 shows trends in in-migration within 250 meters of the 2014–2015 buildings in our low-income analysis sample.²³ The triangle-marked line shows the number of migrants to the new building, the square-marked line shows migrants to the surrounding 250 meters excluding moves into the new building, and the hashed line is the sum of the two. Panel A counts migrants from tracts with above CBSA-median income, while Panel B tracks arrivals from tracts below two-thirds of the CBSA median.²⁴ Both cases show roughly the same story: the total number of arrivals

¹⁹Appendix Figure A.3 plots the ratio of the Infutor and Census populations against tract characteristics. Additional validation is provided in Diamond, McQuade, and Qian (2019), which introduced the data to the academic literature.

²⁰We assess this by computing the average annual migration rate at the county level in the Infutor data and comparing it to census estimates (which are not available below the county level). Appendix Figure A.4 plots the ratio of the two estimates against county characteristics and shows only slight correlations, suggesting that the moves we observe are close to randomly selected. Additionally, Phillips (2019) provides a detailed recent assessment of how well the Infutor data capture mobility among particularly disadvantaged populations, such individuals with very low incomes or initially living in public housing, and finds that they are good at capturing mobility and housing instability after events such as public housing demolitions or flooding from Hurricane Katrina.

²¹Infutor does not provide information on income for individuals themselves. Proxying for individual income with origin neighborhood income is similar to Mast (2019) and Diamond, McQuade, and Qian (2019).

²²While much debate focuses on out-migration from gentrifying areas, the academic literature has reached a strong consensus that neighborhood change primarily occurs through changes in in-migration, making this the relevant outcome to study (Freeman 2004; McKinnish, Walsh, and White 2010; Ellen and O’Regan 2011; Ding, Hwang, and Divringi 2014; Brummet and Reed 2019).

²³We exclude the small number of buildings with addresses that we cannot match to the Infutor data.

²⁴We exclude tracts with over 25 percent college students from the low-income migrants. We choose this low-income definition because it is a common threshold for income-restricted housing and because it provides

of either type increases after building completion, but the change is totally driven by arrivals to the new building. Migration to the surrounding area changes little, though there is a slight decline in arrivals from high-income areas and a small uptick in arrivals from low-income areas. However, the new buildings clearly represent a substantial supply shock to the area: over 20 percent of high-income arrivals to the area in a given year are to the new building.

These results are inconsistent with the new buildings having a major impact on in-migration to the surrounding area, which is the primary way that neighborhoods change. However, because the public debate on gentrification largely concerns displacement of low-income households, we also consider net migration from low-income areas in Figure 2. Net migration does not appear to meaningfully change during the sample period. While this evidence is not causal, it is inconsistent with large displacement effects of new buildings.

2 Rental Empirical Strategy

2.1 Overview

To guide our empirical analysis, we first consider the hypothetical ideal experiment—building a new apartment building in a low-income neighborhood in a randomly assigned set of cities. We could then compare rental prices in the treatment and control group at different distances to the new building. Absent amenity or signaling effects, increased supply would likely cause prices to decrease close to the new building, and the effect would then fade out with distance (assuming that renter choice sets are at least somewhat spatially concentrated). But the shape of these other effects is much less clear, making it difficult to predict where the net effect on rents will be smaller or even positive. The ideal experiment would allow us to simply make comparisons at every distance, but in practice, different quasi-experimental strategies are better suited to detect different spatial patterns. We construct two empirical strategies to test for perhaps the most likely shapes of rent increases and consider some alternatives in extensions.

One possibility is that amenity effects are important very close to the new building and then fade out quickly. This makes sense for amenity changes like increased retail options, increased foot traffic, or aesthetic improvements like the replacement of a vacant lot. If these effects are large, we could see rent increases very close to the new building that quickly fade out with distance. Our first empirical exercise—the “near-far” approach—checks for this pattern by comparing rents very close to a new building to rents slightly farther away. For identification, this approach relies on idiosyncratic variation in the availability of developable

a large enough sample to study.

sites within small areas. Diamond and McQuade (2019) use similar logic to study new low-income housing developments, while Shoag and Veuger (forthcoming), Currie et al. (2013), and Autor et al. (2014) similarly use fine spatial variation in home locations to study the effect of localized shocks on prices.²⁵

Another possibility is that amenity and reputation effects work at a broader level. For instance, a new building could signal that a relatively large neighborhood is on the upswing. This pattern would not be captured in our near-far strategy, as the control group would receive a similar boost as the treatment group. To get around this problem, we leverage variation in the timing of new developments, which varies due to the unpredictable length of the land assembly, permitting, and construction processes. This “near-near” approach compares the area around buildings completed in 2015–2016 to buildings completed in 2019 (after the end of our rent data). Because the control buildings can be in different neighborhoods than the treated, this allows us to detect broader effects of a new building. Finally, in addition to comparing the areas around the treatment and control buildings, we also construct a triple-difference approach that compares the near-far gap around the two sets of buildings.

2.2 Near-Far Approach

The near-far approach is a relatively standard “ring” difference-in-differences that compares listings within a treatment radius of the new building to listings within a larger control radius. The identification assumption is that in the absence of new construction, rents would have changed in parallel in the treatment and control group. The idea is that within a small area, developers are constrained in the sites that are available and have appropriate zoning, leading to hyper-local variation in the location of new construction that is not related to future price changes.

This identification assumption is more likely to hold with small ring radii that contain a relatively homogeneous area, while larger radii may introduce bias by including a control area that is dissimilar to the treatment area. However, the treatment group should also be large enough that it is substantially more affected by the new building than the control group. We set the baseline treatment radius to 250 meters (roughly one or two city blocks) and the control radius to 600 meters (slightly over a third of a mile, approximately an 8- to

²⁵A review of the land-use literature also supports our identification assumptions. Ease of development can vary city by city, depending on zoning regulations and attitudes by city officials (Behroozi et al. 2001). Parcels may be small or irregularly shaped, and changes to their zoning may need to be made before development can proceed (Steinacker 2003). Developers preferring to circumvent residential opposition by building on formerly industrial or commercial sites may find that vacant land sites may be hard to assemble into sufficiently large parcels and be in poor physical conditions (Bowman and Pagano 2000; Brooks and Lutz 2016).

10-minute walk).²⁶ The control radius is small enough to generally include listings that are part of the same neighborhood and likely on the same price trajectory. The treatment radius is large enough to include the listings in the immediate area of the building. We investigate the validity of our identification assumption by looking for pretrends in the event study graphs and consider alternate radii in robustness checks. Appendix Figure A.5 illustrates the treatment and control groups in Portland and Brooklyn.

To implement this strategy, we associate each listing i with the closest building b that was completed in 2015 or 2016 and define the treatment year t^* as that building’s completion year. We keep listings with a building within 600 meters and index each listing’s treatment status by r , which is equal to 1 if a listing is within 250 meters of its closest building and 0 otherwise. Because our rent data poorly identify when the same unit is listed at multiple points in time, we cannot include unit fixed effects and must instead use fixed effects at the level of the nearest building \times treatment status. We also use the nearest building to define time fixed effects that control for time-varying shocks at a very local level. This yields the following specification for rent in listing i in year t :

$$\log(\text{rent}_{it}) = \alpha_{bt} + \kappa_{br} + \sum_{k=-3}^3 [\beta_k * \mathbb{1}_{it}(t - t^* = k, r = 1)] + \gamma * X_{it} + \epsilon_{it}. \quad (1)$$

X_{it} consists of dummies for the number of bedrooms and bathrooms, which helps to control for changes in the composition of listings over time. We weight each building-year equally to account for different densities of nearby listings and cluster standard errors at the level of the nearest new building. We also estimate a standard difference-in-differences (DiD) to obtain an average effect:

$$\log(\text{rent}_{it}) = \alpha_{bt} + \kappa_{br} + \beta * \mathbb{1}_{it}(t \geq t^*, r = 1) + \gamma * X_{it} + \epsilon_{it}. \quad (2)$$

2.3 Near-Near Approach

The previous approach tests for highly localized spillovers of the new buildings but, because the treatment and control group are very close together, cannot identify more spatially diffuse effects. To allow for this type of spillover, our near-near strategy compares the area around 2015–2016 buildings to a set of similar locations: the area around 2019 buildings. Because the treatment and control group are no longer necessarily in the same neighborhood, even broader

²⁶Time estimates are from Google Maps and vary depending on how directly the road network connects two sites. While the treatment area is small, both amenity and supply effects of new housing have been found to decay quickly with distance. Schwartz et al. (2006) and Rossi-Hansberg et al. (2010) find that positive housing externalities from urban revitalization programs substantially decay within 250 meters. Campbell, Giglio, and Pathak (2011) and Hartley (2014) find that the negative effect of a foreclosure decays even faster.

spillovers from the treatment buildings should not affect the control listings. In addition, this specification buttresses our near-far analysis by using an entirely different source of variation.

The identification assumption is that rents would have changed in parallel near the two sets of buildings in the absence of construction. The rationale is that, because both treatment and control locations received new construction in a four-year period, they share characteristics that make them attractive to developers. Moreover, variation in timing of completion could be largely due to the idiosyncratic nature of the land assembly, building permitting, and construction processes.

Table 3 helps evaluate this assumption by showing differences between neighborhoods near treatment and control buildings. We regress characteristics of each building’s tract on CBSA fixed effects and a treatment dummy. While none of the differences are statistically significant at the 5 percent level, the point estimates suggest that the treatment neighborhoods are somewhat wealthier, whiter, and more expensive. This likely occurs because the earlier buildings went into areas that began to gentrify at least somewhat earlier. However, the control group scores higher on some indicators: it has a higher home ownership rate and a lower vacancy rate. On the whole, the treatment and control neighborhoods appear to be similar, and we further probe the identification assumption by studying pretrends in the event study results.

To implement this approach, we now associate each listing with its nearest 2015, 2016, or 2019 building and keep only those with a building within 250 meters. Because there is no variation in the treatment variable within listings that share the same nearest building, we coarsen our time fixed effects from nearest building \times year to CBSA \times year and similarly coarsen our nearest building \times treatment status fixed effects to the nearest building level.²⁷ We index a listing’s treatment status by c , which is equal to 1 if a listing’s nearest building was completed in 2015 or 2016 and 0 otherwise.

This yields the following event study specification for listing i in CBSA m in year t :

$$\log(\text{rent}_{it}) = \alpha_{mt} + \kappa_b + \sum_{k=-3}^3 [\beta_k * \mathbb{1}_{it}(t - t^* = k, c = 1)] + \gamma * X_{it} + \epsilon_{it}. \quad (3)$$

We weight observations and cluster standard errors as in the near-far specification, and we also estimate the standard DiD:

$$\log(\text{rent}_{it}) = \alpha_{mt} + \kappa_b + \beta * \mathbb{1}_{it}(t \geq t^*, c = 1) + \gamma * X_{it} + \epsilon_{it}. \quad (4)$$

²⁷Because San Francisco and Washington, D.C., do not have 2019 buildings that meet our sample criteria, we drop these CBSAs from the sample for the near-far specification.

2.4 Triple-Difference Approach

Finally, our first two specifications can be combined naturally into a triple-difference approach. This can be interpreted as relaxing the identification assumption in either the near-far or near-near specification. Relative to the near-far, it effectively uses the near-far gap around 2019 buildings to control for time-varying omitted variables that may affect the same gap near 2015–2016 buildings. This changes the identification assumption to: in the absence of construction, the near-far rent differential would have changed in parallel in the 2015–2016 and 2019 areas. This allows for developers to have some discretion in site selection, even within our small radius, as long as that discretion is the same in both 2015–2016 and 2019.

Alternatively, the triple difference can be viewed as improving on the near-near approach by comparing the difference between the outer rings of the treatment and control buildings to the difference between the inner rings of the two sets of buildings. This relaxes the identification assumption to: in the absence of construction, the treatment-control rent differential would have changed in parallel in the inner and outer rings. This allows for differences between the treatment and control inner rings (such as the treatment group gentrifying earlier), so long as the difference is the same in the treatment and control outer rings.

To implement the triple-difference specification, we must first expand the sample to include all listings within 600 meters of either a treatment or control building. Noting that r indexes whether an observation is within 250 meters of its closest building (treated in the near-far) and c indexes whether an observation is near a 2015–2016 building (treated in the near-near), the specification is:

$$\log(\text{rent}_{it}) = \alpha_{bt} + \kappa_{br} + \nu_{rt} + \beta * \mathbb{1}_{it}(t \geq t^*, c = 1, r = 1) + \gamma * X_{it} + \epsilon_{it}, \quad (5)$$

where ν_{rt} is a fixed effect for the inner ring in each year. We use the same weighting and clustering schemes as in the previous specifications.

3 Rent Results

3.1 Main Results

Results for the near-far specification are in Figure 3 and Table 4, Column 1. The event study specification in Figure 3 shows that coefficients (normalized to the year before completion) hover around zero during the preperiod and then sharply drop to roughly -0.06 after building completion. The effect remains very similar for the three subsequent years that

we can track in our sample. Estimating the model using an after-treatment dummy (Table 4, Column 1) shows that new buildings decrease nearby rents by 4.9 percent (S.E.=0.021, $p=0.023$). These results suggest that hyper-local positive spillovers do not cause rents to rise in the immediate area of the building.

Results for the near-near specification, shown in Figure 3 and Table 4, Column 2, also suggest that new buildings decrease rents. The event study coefficients are similar to those from the near-far specification: approximately zero before falling to about -0.07 following building completion. The after-treatment estimate from a standard DiD is -0.062 (S.E.=0.037, $p=0.096$).

Finally, the triple-difference estimate is in Table 4, Column 3. It is again quite similar to the near-far and near-near estimates: -0.071 (S.E.=0.033, $p=0.037$). In dollar terms, these point estimates translate to between a \$100 and \$159 decrease in listing rent, which is about \$1 per unit in the new building on average. We note that if developers are less constrained in their ability to select locations than our specifications assume, this should lead to a positively selected treatment group, likely biasing our results toward 0.

The baseline estimates use a relatively small treatment radius of 250 meters, which we extend to 400 meters in Appendix Table A.3.²⁸ The near-near estimate decreases to a statistically insignificant -2.8 percent (S.E.=0.032, $p=0.39$), and the near-far estimate similarly shrinks. However, the event study coefficients underlying these specifications (Appendix Figures A.6 and A.7) show that there is a strong positive pretrend in the treated area. The pretrend illustrates the hyper-local nature of our identification assumptions: when we push the control group slightly further away from the new building, it no longer provides an ideal comparison. Nonetheless, the evidence from the 400-meter exercises still strongly points to a negative rent effect of the new building. The positive pretrend in the event studies sharply reverses in the treatment year, and our triple-difference specification, which accounts for the positive pretrend, yields a negative effect of 6.8 percent (S.E.=0.041, $p=0.097$).

Before moving to extensions and robustness, we discuss two caveats that we cannot explore with our rent data. First, significant heterogeneity across building and neighborhood types may underlie our average results. Amenity effects likely vary highly depending on the context, and there may be important tipping points or nonlinearities. For example, buildings may have a different effect if they replace a vacant lot versus an existing building, if they offer some particularly attractive amenity, or depending on the current national or local economic climate. Our sample size prevents us from exploring this heterogeneity in detail, and we note that, even given a larger sample, we would not be able to estimate the effect of new buildings in the types of neighborhoods where developers never choose to build. Second, the

²⁸We also extend the isolation restriction on new buildings to 400 meters to match the treatment radius.

data from ZillowTM tend to skew towards the higher end of the rent distribution, which makes it difficult to assess whether the average effects we estimate might be different for the initially lower-rent units that are most relevant to the policy debate. While we have no other source of listing-level rent data to study this directly, our migration data allow us to study the lower end of the housing market by examining migration from low-income areas.

3.2 Congestion Effects and Other Spatial Patterns

Our near-far and near-near strategies aim to detect rent effects that center around new buildings, but these effects could take many shapes, depending on the interaction and size of amenity, disamenity, and supply effects. For example, new buildings could improve consumption amenities within a ten minute walk of the new building, but also generate congestion disamenities by increasing traffic or blocking views on the building’s block. This would yield a “doughnut-shaped” rent effect in which rents decline nearby but increase slightly farther away. To test this idea, we repeat the near-near strategy but compare the 250- to 600-meter bands in the treatment and control groups. Appendix Table A.4, shows statistically insignificant effects of 2.8 percent (S.E. = 0.026, p=0.27) and 1.6 percent (S.E. = 0.021, p=0.46) for the areas between 250–600 meters and 250–800 meters, respectively.²⁹

While congestion likely follows a predictable pattern, cities are complex systems in which a new building could generate other nonlinear or oddly shaped amenity effects. We evaluate these possibilities more generally using Diamond and McQuade’s (2019) empirical derivative approach, which produces continuous estimates of treatment effects on rents at various distances to the new buildings. The primary drawback is that it only accommodates buildings that had no other building within the studied area during the entire sample period, which reduces our sample size and results in a potentially selected set of buildings.³⁰ Appendix Figure A.8 shows the estimated treatment effect at distances within 600 meters of the new building, both before and after its completion. They are analogous to the near-far event study coefficients and show a very similar pattern. Across distances, rents change in parallel prior to the building’s completion. After completion, rents decrease much more sharply closer to the building, especially in the area within 250 meters that represents our treatment radius in the near-far specification.³¹ Beyond 250 meters, the gradient rises toward zero as we approach

²⁹While these point estimates do not allow us to rule out positive effects, we note that point estimates are negative when we do a similar exercise with migration outcomes in the next section.

³⁰We discuss implementation in more detail in the Appendix.

³¹The estimated effect near the construction site is almost –40 percent, which is much higher than the rest of our results. Beyond the selection bias introduced by the more stringent isolation criteria discussed above, it could also be due to the challenges inherent in adopting an algorithm designed to estimate effects on housing transactions to estimating effects on apartment listings. Housing transactions are rarer, and neighboring houses in central cities tend to be more similar than apartment units. The fact that apartment units may be

the 600-meter limit. This simple pattern give us confidence that our ring approach is not netting out any nonlinearities that would undermine our main results.

3.3 Robustness

Our first set of robustness checks explores the sensitivity of the baseline results to alternate assumptions. Appendix Figure A.9 shows the near-far event study under four different restrictions—dropping majority white tracts, reducing the income threshold by \$10,000, requiring that buildings also be in below-median college share neighborhoods, and dropping listings in New York.³² In all cases, the patterns appear similar to the baseline, and the DiD estimates are again between -5 percent and -7 percent (Appendix Table A.5). We repeat these exercises for the near-near specification and find that results are similar to the baseline, including a larger negative effect when we restrict to low-college or highly nonwhite areas (Appendix Figure A.10 and Appendix Table A.6).

Next, we try to examine longer-run effects and longer pretrends, although we cannot do so with our main analysis sample. First, we repeat the near-far specification using 2010–2013 buildings, which allows us to include five postconstruction years (Appendix Figure A.11), and find no evidence of long-run changes in the treatment effect.³³ We then repeat the same specification using only 2019 buildings, which allows us to study pretrends over the six years before completion. We see no evidence of time trends and no discernible effect of project approvals or announcements that precede building completion, although our small number of buildings (23) leads to some noisy estimates (Appendix Figure A.12). We also study longer-run pretrends using the empirical derivative approach (Appendix Figure A.13) and find that this more detailed price surface tells a similar story. While these exercises do not use our main analysis buildings and we cannot study both longer-run pretrends and longer-run effects within the same specification, the results nevertheless provide suggestive evidence that our main estimates would not drastically change if we were able to extend our time window to five years.

Finally, we remove the restriction on neighborhood income and repeat our main specifications. Figure A.14 shows the near-far event studies for both 250- and 400-meter radii, and Figure A.15 shows the same for the near-near specification. The near-far results show no

more heterogeneous within a given area likely makes spatial differencing more difficult.

³²The number of listings and average rent in New York change sharply in 2017, as shown in Appendix Table A.1. Although we include CBSA \times year or nearest building \times year fixed effects in all specifications, dropping these observation provides another check that this change does not drive our results.

³³We include four years of buildings because, in the wake of the Great Recession, there was little construction in the 2010–2012 time period. We do not do this exercise for the near-near specification because the 2019 buildings are a substantially worse control group for 2010 buildings than for 2015 and 2016 buildings.

consistent pattern, and the near-near results show noisy patterns that are, at most, somewhat suggestive of a negative effect of new buildings. Table A.7 shows DiD estimates with the 250-meter radius, which are close to zero and not statistically significant. Table A.8 shows the estimates for the 400-meter radius, which are similar except for the triple difference, which is statistically significant and negative. In some ways, the smaller estimates appear counterintuitive, as new buildings likely represent less of a change for richer neighborhoods, suggesting that the positive amenity effect should be smaller. However, the elasticity of demand for rich neighborhoods may be higher if they are broadly liked by high-income households and gentrifying or low-income neighborhoods appeal to only a smaller subset. This would reduce the price effect of increasing supply. In addition, the near-far strategy in particular may be less valid in high-income areas if well-organized NIMBY opposition is better able to shunt new buildings to specific sites that are unattractive or dissimilar to their surroundings.

4 Migration Results

4.1 Migration Empirical Strategy

Our empirical strategy is extremely similar to the rental analysis described in Section 2, with the following differences. First, instead of a rental listing, an observation is now an in-migrant to an address near a new building. Second, instead of using price as the dependent variable, we use median household income in the in-migrant’s origin tract. Third, because the Infutor sample ends in 2017, we use 2014–2015 buildings as the primary treatment group and 2018 buildings as the control. Finally, we cannot include controls for bedroom and bathroom counts, which we do not observe in the migration data.

With these changes, we estimate event studies and DiDs for the near-far approach (Equations 1 and 2), near-near approach (Equation 3 and 4), and the triple difference (Equation 5). Because our goal is to assess how new buildings affect the surrounding neighborhood, we always exclude from the sample migrants to the new building itself.³⁴

4.2 Migration Results

We begin with results from the near-near specification. Event study coefficients for log origin income are shown in Figure 5, Panel A. There is a parallel trend in the pre-period followed by a sharp decrease of approximately 3–5 percent in the three years following building

³⁴For the small number of buildings with addresses that we cannot match to the Infutor data, we drop all migrants to addresses within 20 meters of the building.

completion. The drop appears to have a 1–2 year delay after building completion, which could occur either because migration is slower to respond to a supply shock than listed rents or because move dates in the Infutor data have a lag, as the address change may not be detected immediately upon moving. Because of this lag, we include the year of building completion as part of the preperiod and drop the year after treatment from the difference-in-differences estimation. As shown in Table 5, Panel A, the average effect of a new building on log origin income is -0.03 (S.E. = 0.017, $p=0.085$).

The result for origin income provides some corroboration of the rent results using an alternative data source, but it does not speak directly to effects on the high- or low-income segments of the housing market. To measure these effects, we repeat the specification using indicators for whether an in-migrant is from a high-income tract (above CBSA median) or low-income tract (below two-thirds of the CBSA median) as the dependent variable. Event study results are shown in Figure 5, Panel B. For low-income arrivals, there is a parallel pretrend that breaks sharply upward one year after treatment. The pattern is nearly the opposite for high-income arrivals, although the pretrend is noisier and slightly upward. The average effects, shown in Panels B and C of Table 5, are 0.034 (S.E. = 0.017, $p=0.059$) for low-income arrivals and -0.023 (S.E. = 0.021, $p=0.295$) for high-income arrivals. For low-income arrivals, this represents an approximately 15 percent increase to the mean in the year before completion. These results suggest that the effect of new buildings on rents is not driven entirely by the high-end listings that are their closest competitors. Instead, the increased migration from low-income areas is consistent with rent decreases among relatively cheaper apartments. Apart from their implications for rent, these results suggest that construction allows more low-income households to move to or remain in the sample neighborhoods, most of which appear to be gentrifying.

By contrast, the near-far results are not very informative. The event study results in Figure 6 show no consistent pattern: across all three outcomes, the years-to-treatment coefficients are small and do not show any strong trend. Similarly, the difference-in-differences estimates in the first column of Table 5 all have t -statistics well below 1. While these null results may be surprising given the statistically significant near-near results, there are a number of reasons that this combination of dependent variable and specification may have the least power. First, new buildings are rentals, but our migration outcomes include both renters and homeowners, whom we cannot separate. If homeowner location choices respond less to shocks to the rental market, this may push our estimates toward zero. Second, the near-far strategy generally has less power because some effects of the new building may affect the control group. Lastly, in the near-far migration specification in particular, variation in the percentage of renters and homeowners at different distances to the new building could

lead to added noise that is not present in the near-far rent specification. This may occur if new rental buildings tend to be constructed in areas that are already zoned for relatively high-density housing.

Finally, the third column of Table 5 shows migration estimates from the triple-difference specification. The magnitudes are quite similar to those of the near-near estimates, with a negative effect on origin income that is significant at the 10 percent level and a positive effect on low-income arrivals that is significant at the 5 percent level.

4.3 Migration Robustness

We repeat a number of the extensions and robustness checks from the rental analysis, focusing on the near-near specification that yielded the most compelling results. First, Appendix Figures A.16 and A.17 show event study results for log origin income and high- and low-income arrivals under a number of restrictions to the sample: reducing the income threshold, restricting to less-white or lower-education neighborhoods, and dropping New York City. All show similar patterns to the baseline results. In addition, we extend the treatment ring to 400 meters. The event studies in Appendix Figure A.18 are loosely similar to the 250-meter radius, but the patterns are less clear.

Next, again motivated by the possibility of nearby congestion effects creating doughnut-shaped net amenity spillovers, we run the near-near analysis using arrivals to the 250–600 and 250–800 meter bands in the treatment and control groups. As shown in Appendix Table A.9, we find small negative point estimates on origin income in both samples, but neither is statistically significant. Like the analogous rent results, this provides no evidence that new buildings increase rents in this alternative pattern.

5 Conclusion

Prior research has shown that new market-rate housing construction improves regional housing affordability, but there is little evidence on how it affects the immediately surrounding neighborhood. This leaves open questions of incidence: who are the winners and losers of new housing construction? The housing policy debate is heavily influenced by the belief that new construction, especially in low-income areas, makes local incumbents worse off by attracting high-income households and high-end amenities, which in turn raises nearby rents and accelerates demographic change (Been, Ellen, and O’Regan 2019; Hankinson 2018). This belief has played a large role in the defeat of major housing reforms such as California’s Senate Bill 50 and is frequently invoked at community meetings that influence the approval

of individual housing development proposals.

We attempt to fill this gap in the literature using quasi-experimental methods and address-level microdata, focusing on the low-income areas that are most relevant to the policy debate. We find that the concerns that motivate opposition to new market-rate housing are mostly unfounded. While there is a strong observed correlation between new construction, rising rents, and demographic change, this is because new buildings are typically constructed in areas that are already changing. When these new buildings are completed, they actually slow rent increases and demographic change in the nearby area. The average new building lowers nearby rents by 5 to 7 percent relative to trend, translating into a savings of \$100–\$159 per month. Results are consistent across a number of specifications, and remaining bias from unobservables driving the selection of building sites likely pushes our estimates toward zero. In addition, we find that new buildings increase low-income in-migration, implying that this improved affordability can foster more integrated, economically diverse neighborhoods that may provide low-income residents with greater economic mobility (Chetty, Hendren, and Katz 2016; Chetty et al. 2018).

The mechanism underlying these results appears to be a simple story of supply and demand. If high-income households like a particular neighborhood, preventing the construction of new housing in those neighborhoods does not prevent them from moving to that neighborhood. Instead, it simply leads them to outbid lower-income households for whatever housing is already available in that neighborhood. This raises rents for everyone and lowers the ability of low-income residents to stay in or move to the area. By contrast, if new housing is built, many high-income households will choose this option instead of a nearby existing unit, reducing rent and out-migration pressures in the area. The new building could theoretically change local amenities or reputation by enough to instead increase demand and raise rents for nearby units, but our findings suggest this is not the case. Our descriptive finding that new buildings go into areas that are already changing could explain why it is not the case: when an area’s amenities and reputation are already changing for other reasons, the marginal effect of a new building on amenities and reputation may be small.

Increasing housing supply should therefore be an important part of any solution to the present affordability crisis. One way to do so is to relax regulations that make it difficult to build in many cities (Gyourko and Molloy 2015; Ganong and Shoag 2017; Glaeser and Gyourko 2018), and another is to directly incentivize localities to increase housing production. However, we note several caveats to our findings. First, we estimate an average effect that may disguise heterogeneity across building and neighborhood types, as amenity and reputation effects are likely highly context-dependent. Second, our sample consists of areas where developers actually chose to build. While this is most relevant to the debate, as these are

likely the neighborhoods that would receive new construction if supply were increased, effects may be different in other types of neighborhoods. For example, developers do not build market-rate units in very low-income areas with high vacancy rates, so our results do not speak to what would happen if they did. Third, relaxing land-use regulation is, in practice, quite complicated. The particulars of a reform could matter both for how much supply is actually added and, depending on the incentives built in (such as encouraging redevelopment of the existing housing stock versus vacant land), the local effect of that new supply. These caveats point to important areas for future research.

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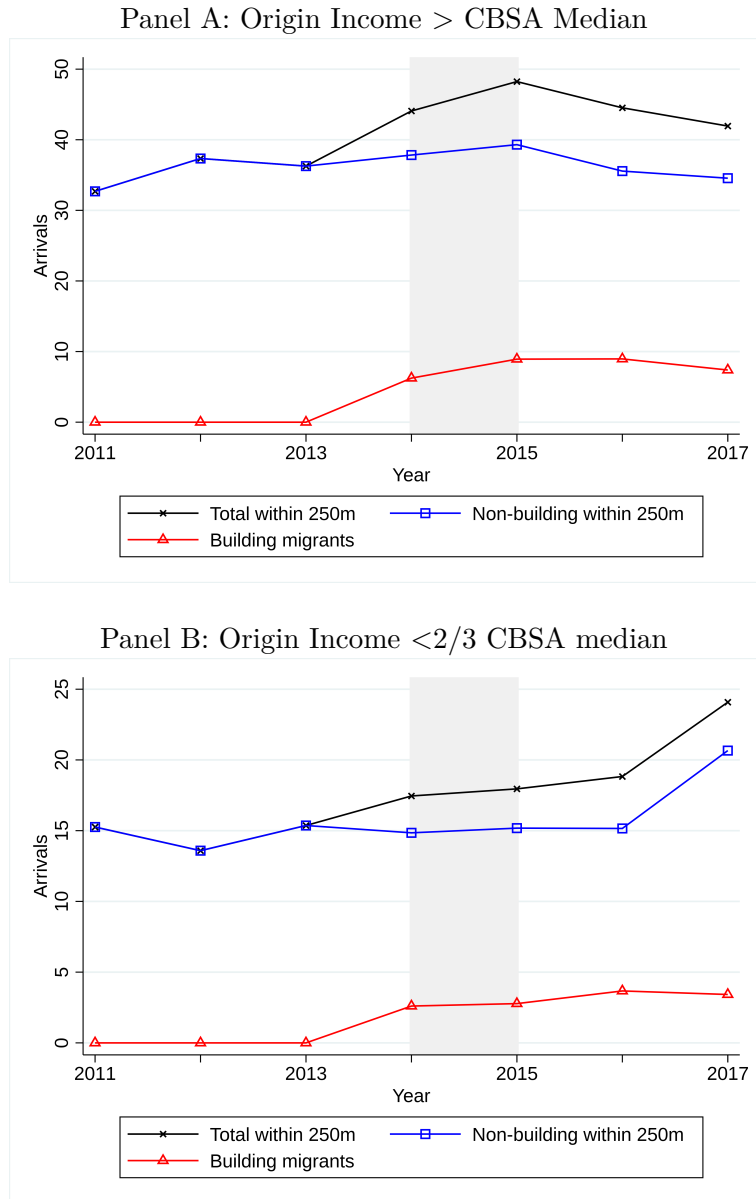
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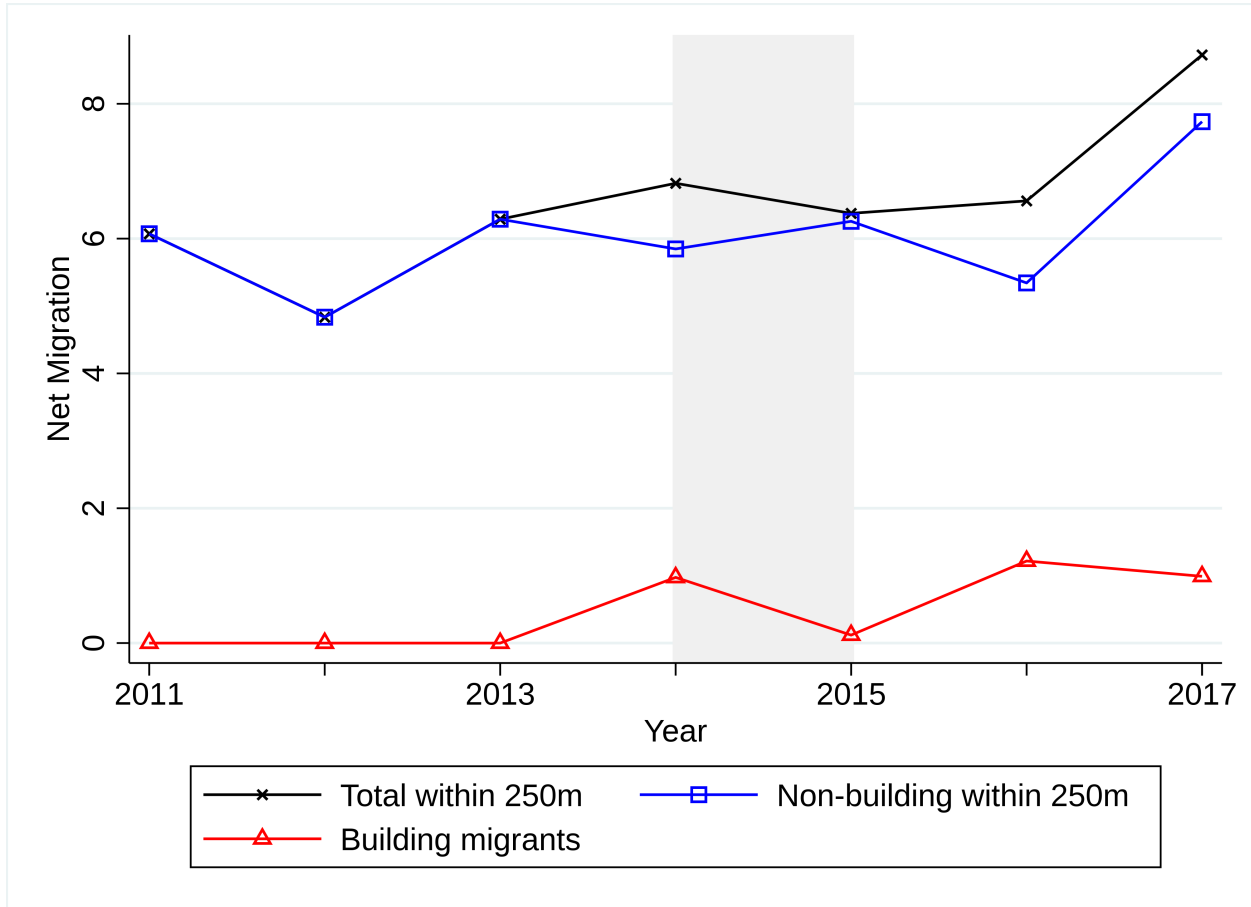
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Figure 1: In-Migration to Areas around New Buildings



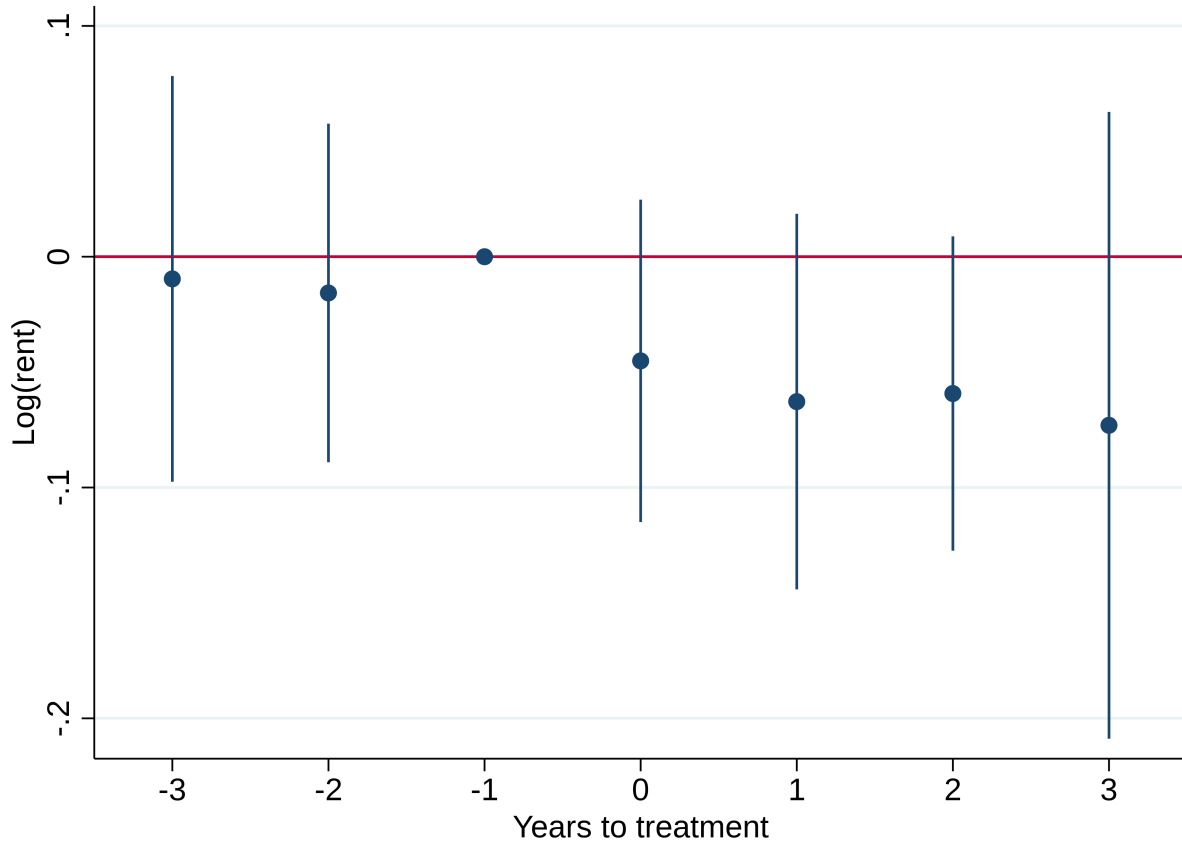
Note: This figure shows trends in the number of in-migrants to the area within 250 meters of new buildings. Panel A counts only in-migrants whose previous address is in a census tract with income above the CBSA median, while Panel B restricts to below two-thirds of the CBSA median income. Nonbuilding migrants are those arriving to the area within 250 meters but not the new building, building migrants are arrivals to the new building itself, and total migrants is the sum. The sample includes 2011–2017 moves to the area within 250 meters of the 2014–2015 buildings in our final analysis sample. Income in the origin tract is defined as median household income in the 2013–2017 ACS, and tracts with over 20 percent college students are excluded from the low-income definition. Migration totals in each year are normalized to account for differences in aggregate Infutor coverage across years, and we drop the small number of new buildings with addresses that we cannot match to the Infutor data.

Figure 2: Net Migration between Low-Income Tracts and New Building Areas



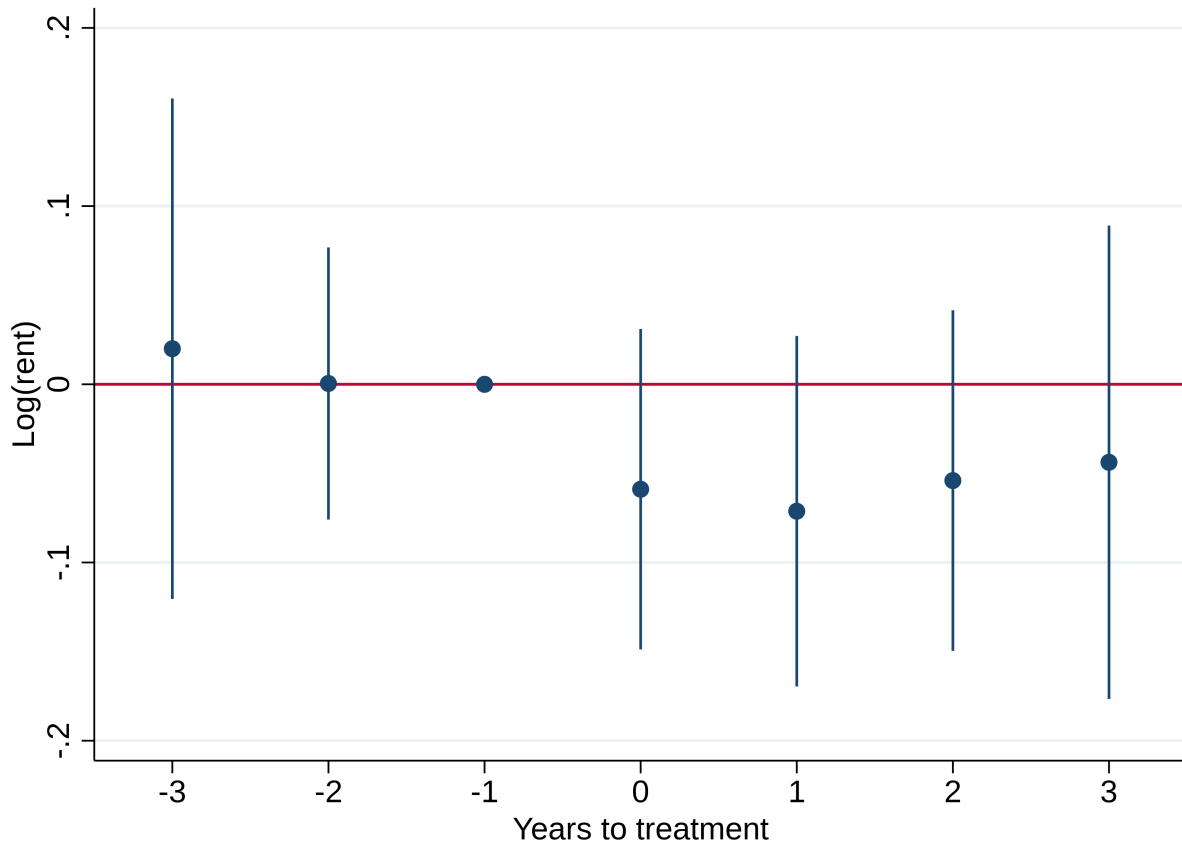
Note: This figure shows trends in net migration (arrivals - departures) between tracts with income below two-thirds of the CBSA median and the area within 250 meters of new buildings. Nonbuilding migrants are those arriving to the area within 250 meters but not the new building, building migrants are arrivals to the new building itself, and total migrants is the sum. The sample includes 2011–2017 moves and the area within 250 meters of the 2014–2015 buildings in our final analysis sample. Income in the origin tract is defined as median household income in the 2013–2017 ACS, and tracts with over 20 percent college students are excluded. Migration totals in each year are normalized to account for differences in aggregate Infutor coverage across years, and we drop the small number of buildings with addresses that we cannot match to the Infutor data.

Figure 3: Near Versus Far Event Study for Rent Outcome



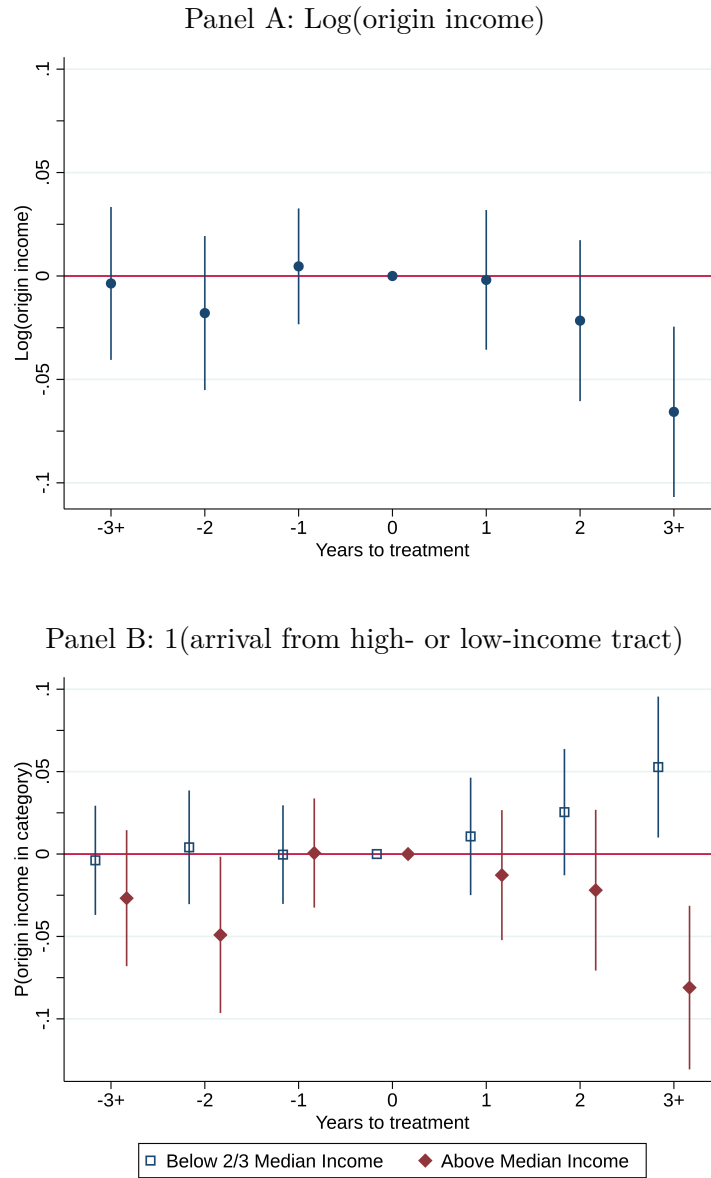
Note: This figure shows the near-far event study of the effect of new buildings on nearby rents. The treatment group is listings within 250m of a new building, and the control group is listings between 250m and 600m of the same buildings. Listings are provided by ZillowTM for years 2013–2018, and we include only the 2015–2016 buildings from our final analysis sample. The specification, shown in Equation 1, includes nearest-building \times year and nearest-building \times treated fixed effects, as well as controls for bedroom and bathroom counts. Rents are winsorized at the 1st and 99th percentiles. Nearest building-years are weighted equally, and standard errors are clustered at the level of the nearest new building.

Figure 4: Near Versus Near Event Study for Rent Outcome



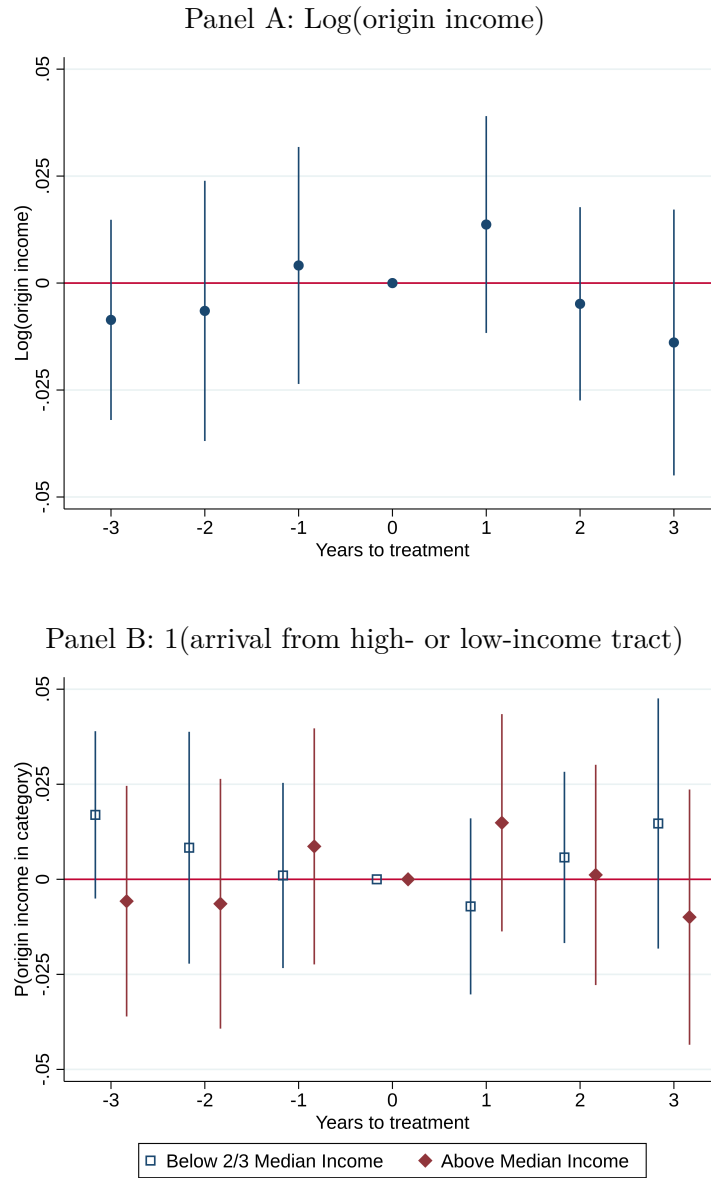
Note: This figure shows the near-near event study of the effect of new buildings on nearby rents. The treatment group is listings within 250m of a building completed in 2015–2016, and the control group is listings within 250m of buildings completed in 2019 (after the sample period). Listings are provided by Zillow™ for years 2013–2018, and we include only the 2015, 2016, and 2019 buildings from our final analysis sample. The specification, shown in Equation 3, include nearest-building and CBSA-year fixed effects, as well as controls for bedroom and bathroom counts. Rents are winsorized at the 1st and 99th percentiles. Nearest building-years are weighted equally, and standard errors are clustered at the level of the nearest new building.

Figure 5: Near Versus Near Event Study for Migration Outcomes



Note: This figure shows the near-near event study of the effect of new buildings on nearby in-migration. The treatment group is arrivals within 250m of a building completed in 2014–2015, and the control group is arrivals within 250m of buildings completed in 2018 (after the sample period). Migration is observed in the Infutor data for years 2011–2017, and we include only the 2014, 2015, and 2018 buildings from our final analysis sample. The specification includes nearest-building and CBSA-year fixed effects. Nearest building-years are weighted equally, and standard errors are clustered at the level of the nearest new building.

Figure 6: Near Versus Far Event Study for Migration Outcomes



Note: This figure shows the near-far event study of the effect of new buildings on nearby in-migration. The treatment group is arrivals within 250m of a building, and the control group is arrivals between 250m and 600m of the same buildings. Migration is observed in the Infutor data for years 2011–2017, and we include only the 2014 and 2015 buildings from our final analysis sample. The specification includes nearest-building \times year and nearest-building \times treated fixed effects. Nearest building-years are weighted equally, and standard errors are clustered at the level of the nearest new building.

Table 1: Building Characteristics and Distribution across Cities

	All Incomes 2010-2019	Low- Income 2010-2019	Low- Income 2014-2016	Final Sample
<u>Building Units</u>				
Units (mean)	198	176	168	166
Units (25th pctile)	85	79	76	68
Units (50th pctile)	162	139	131	121
Units (75th pctile)	287	249	250	256
<u>Nearby Rents</u>				
Rent 1 br (mean)	1,784	1,609	1,568	1,539
Rent 1 br (25th pctile)	1,356	1,290	1,284	1,262
Rent 1 br (50th pctile)	1,633	1,528	1,508	1,514
Rent 1 br (75th pctile)	2,045	1,801	1,739	1,740
Rent (mean)	2,129	1,917	1,875	1,855
Rent (25th pctile)	1,667	1,572	1,597	1,601
Rent (50th pctile)	1,942	1,799	1,784	1,807
Rent (75th pctile)	2,441	2,098	2,037	2,052
<u>Buildings by City (column percentages)</u>				
Atlanta	7	4	5	4
Austin	10	12	14	13
Brooklyn	13	13	11	13
Chicago	8	3	2	4
Denver	9	10	10	15
Los Angeles	11	13	11	15
Philadelphia	3	3	2	1
Portland	9	11	11	10
San Francisco	4	3	2	1
Seattle	17	20	25	20
Washington, D.C.	7	7	6	3
Observations	1,483	823	253	92

Note: Distributions of units in buildings, rents in Zillow™ listings near buildings (within 600 meters), and city locations of buildings for different samples of buildings. Samples of buildings are described in detail in Section 1.1 of the main text. Building locations and unit counts are provided by Real Capital Analytics.

Table 2: Building Neighborhood Characteristics

	All Incomes		Low-Income			
	No Building	Some Building	No Building	Some Building	2014-6 Building	Final Sample
<u>Household Income</u>						
2000 (\$)	57,879	55,240	46,575	43,376	44,278	42,732
2010 (\$)	58,328	61,513	45,116	45,201	45,722	44,949
2017 (\$)	60,330	76,332	44,724	53,369	53,631	52,544
2000 to 2010 (pct)	-1	11	-4	5	4	5
2010 to 2017 (pct)	2	22	-1	17	16	16
<u>College Degree</u>						
2000 (pct)	25	39	17	30	32	27
2010 (pct)	30	50	22	40	42	37
2017 (pct)	34	60	26	49	51	48
2000 to 2010 (pp)	5	12	4	10	10	10
2010 to 2017 (pp)	4	9	4	9	9	10
<u>Rent</u>						
2000 (\$)	1,001	992	884	872	884	845
2010 (\$)	1,168	1,199	1,045	1,002	995	954
2017 (\$)	1,271	1,506	1,111	1,238	1,227	1,175
2000 to 2010 (pct)	16	19	17	14	12	12
2010 to 2017 (pct)	7	22	6	21	21	20
Observations	3,218	1,483	2,375	823	253	92

Note: Means of the characteristics of the neighborhoods (census tracts) where our new buildings were built. The left two columns include all central city tracts in our sample cities, while the right four only include those with median household income below the CBSA median. “No Building” denotes neighborhoods that did not receive any of our sample buildings between 2010 and 2019. “Some building” includes neighborhoods with at least one building, “2014-6 Building” is neighborhoods with a building completed between 2014 and 2016, and “Final Sample” is tracts that contain a building in the final analysis sample described in Section 1.1. For columns with some building, the column means are weighted by the number of buildings in each neighborhood. Information on building locations is provided by Real Capital Analytics, and demographic information comes from the Census 2000 Long Form (“2000”), American Community Survey 2006-2010 5-Year Estimates (“2010”), and American Community Survey 2013-2017 5-Year Estimates (“2017”).

Table 3: Treatment versus Control Buildings in Rent Near-Near Analysis

Dependent Variable	β	t -stat
Median household income	6025	1.3
2BR Rent	169	1.6
Pct. White	0.06	1
Pct. Black	-0.029	0.6
Pct. College	0.09	1.8
Pct. Poverty	-0.05	1.6
Pct. 200k+	0.005	0.5
Pct. Owner-Occupied	-0.03	-0.6
Pct. Vacant	0.012	1
Population	-184	0.3

Note: This table shows differences between the neighborhoods around the treatment (2015–2016 completion) and control (2019) buildings in the near-near rental analysis. Each row shows results from a regression of the dependent variable shown in the first column on a treatment indicator and a set of CBSA fixed effects. β is the coefficient on the treatment indicator, and t is the t-statistic on that coefficient

Table 4: Baseline Difference-in-Differences Results for Rent Outcomes

	Near versus far	Near versus near	Triple-difference
After*within 250 (S.E.)	-0.049 (0.021)		
After*treated building (S.E.)		-0.062 (0.037)	
After*within 250*treated building (S.E.)			-0.071 (0.033)
Treated buildings	46	44	42
Control buildings		20	19
Listing observations	56,000	20,400	56,800

Note: This table shows baseline difference-in-differences results for rents. The first column shows the near-far specification shown in Equation 2, where the treatment group is listings within 250m of a building completed in 2015–2016, and the control group is listings between 250m and 600m of the same buildings. The second column shows the near-near specification, shown in Equation 4, in which the treatment group is listings within 250m of a building completed in 2015–2016, and the control group is listings within 250m of buildings completed in 2019 (after the sample period). The third column shows the triple-difference specification from Equation 5, which compares the near-far gap in the 2015–2016 and 2019 buildings. Listings are provided by Zillow™ for years 2013–2018, and we include only new buildings that meet the restrictions of our final analysis sample. All specifications include controls for bedroom and bathroom counts, and rents are windsorized at the 1st and 99th percentiles. Building-years are weighted equally, and standard errors are clustered at the level of the nearest new building.

Table 5: Baseline Difference-in-Differences Results for Migration Outcomes

	Near versus far	Near versus near	Triple-difference
<i>Panel A: Log(origin income)</i>			
After*within 250 (S.E.)	-0.004 (0.01)		
After*treated building (S.E.)		-0.03 (0.017)	
After*within 250*treated building (S.E.)			-0.027 (0.014)
<i>Panel B: 1(above median origin)</i>			
After*within 250 (S.E.)	-0.0003 (0.01)		
After*treated building (S.E.)		-0.023 (0.021)	
After*within 250*treated building (S.E.)			-0.019 (0.017)
<i>Panel C: 1(<2/3 median origin)</i>			
After*within 250 (S.E.)	0.002 (0.008)		
After*treated building (S.E.)		0.034 (0.017)	
After*within 250*treated building (S.E.)			0.027 (0.013)
Treated buildings	66	66	66
Control buildings		72	72
Arrival observations	119,400	57,400	186,700

Note: This table shows baseline difference-in-differences results for migration. Each panel uses the dependent variable indicated in the heading. The first column shows the near-far specification, where the treatment group is arrivals within 250m of a building completed in 2014-2015, and the control group is arrivals between 250m and 600m of the same buildings. The second column shows the near-near specification, in which the treatment group is arrivals within 250m of a building completed in 2014-2015, and the control group is arrivals within 250m of buildings completed in 2018 (after the sample period). The third column shows the triple-difference specification, which compares the near-far gap in the 2014-2015 and 2018 buildings. Migration data for 2011-2017 is from Infutor, and we include only new buildings that meet the restrictions of our final analysis sample. Building-years are weighted equally, and standard errors are clustered at the level of the nearest new building.

A Appendix

A.1 Empirical Derivative Estimation Details

In this section, we describe our approach to estimate a continuous treatment effect of new buildings, which follows Diamond and McQuade (2019). While full details on the methodology are available in that paper, we provide a summary here. The basic idea is to calculate how rents changed with distance from the development site, holding constant rental listing timing and proximity as closely as possible. The equation of interest is:

$$\log(\text{rent}_{jt}) = \tilde{m}_{\mathbf{Y}}(r_j, \tau_j) + \phi_l(r_j, \theta_j) + \psi_l(\theta_j, t) + \epsilon_{jt} \quad (6)$$

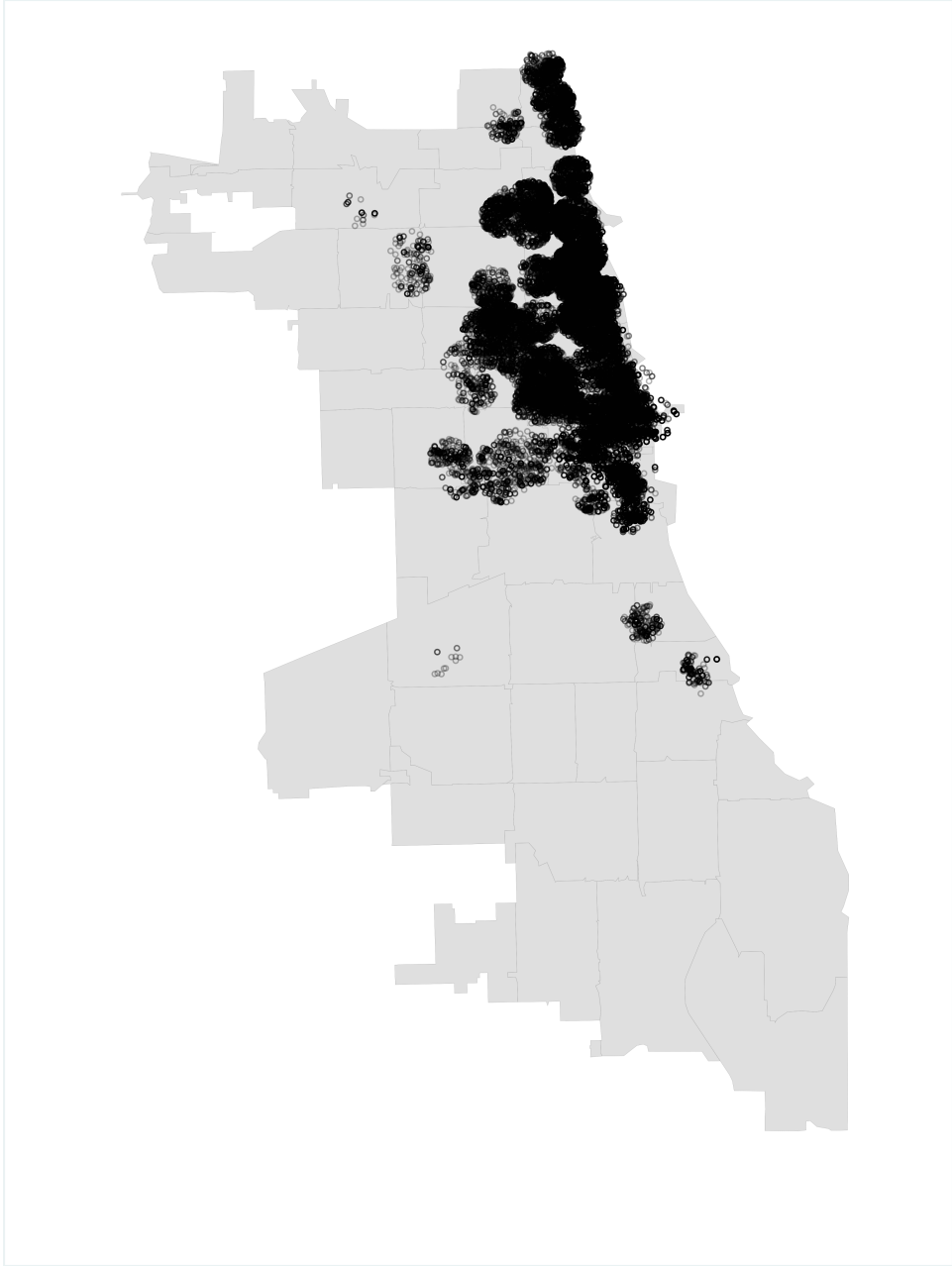
where rent_{jt} is the listed rent of apartment unit j in year t , unit locations are defined in polar coordinates (r_j, θ_j) , and τ represents years to building completion. $\tilde{m}_{\mathbf{Y}}(r_j, \tau_j)$ is the semiparametric function of interest, which describes the relationship between rents and distance at different years to the building's completion. Our main results for the exercise are graphs of this function. $\phi_l(r_j, \theta_j)$ represent "neighborhood-specific" fixed effects, and $\psi_l(\theta_j, t)$ are distinct time trends for neighborhood l (which could vary by θ).

To estimate the equation, we calculate the change in prices over r by differencing over pairwise transactions that vary in r but are as close in t and θ as possible. We then average over these pairwise calculations, and smooth the estimates with a Nadaraya-Watson kernel estimator. This yields the estimated derivative of the $\tilde{m}_{\mathbf{Y}}(r_j, \tau_j)$ function that we plot in our results.

We depart from Diamond and McQuade chiefly by first demeaning rents according to bedroom and bathroom counts, which is necessary because of the large variation in rental units within a short distance. In addition, since we study a shorter time span than Diamond and McQuade and use a smaller outer radius, we choose a smaller Nadaraya-Watson bandwidth. We set $h_{t,n} = 3$ instead of $h_{t,n} = 5$ as our time bandwidth, and $h_{r,n} = 0.2$ instead of $h_{r,n} = 0.3$ as our distance bandwidth. Additionally, since listings are relatively more common than housing price transactions, we use 12 pairwise transactions for each calculation of the derivative.

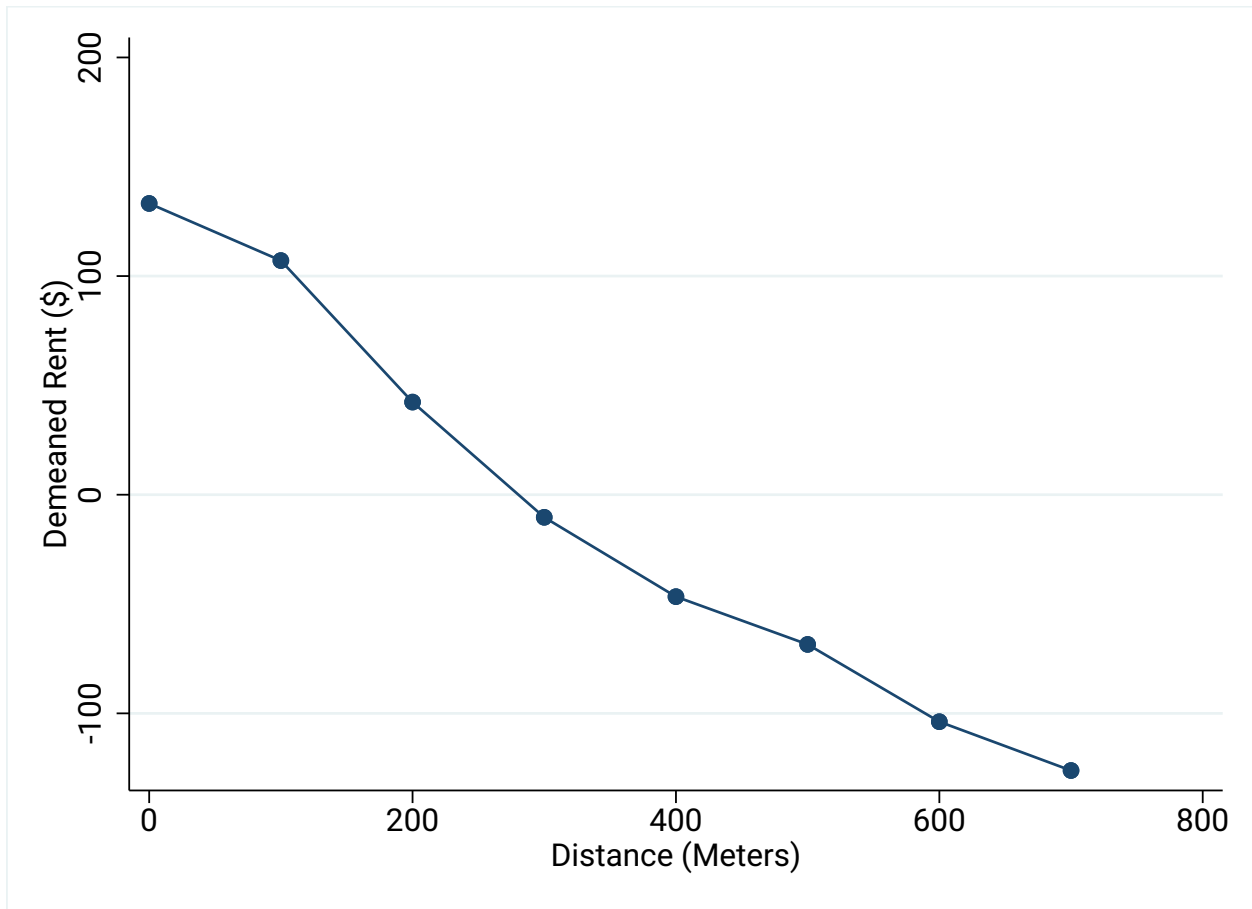
A.2 Additional Figures and Tables

Figure A.1: 2018 Rental Listings Near Chicago Buildings



Note: This figure shows the location of 2018 Zillow™ listings in Chicago that are within 800 meters of a sample building completed between 2010 and 2019.

Figure A.2: Rent vs. Distance to Nearest Building



Note: This figure shows rent versus distance to the nearest new building for units in the ZillowTM sample. Rent is de-meaned at the CBSA-year level.