Do Property Tax Incentives for New Construction Spur Gentrification? Evidence from New York City

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Abstract

Recently, many cities have proposed property tax incentives on new construction to counteract rising rents. However, to date, there is little empirical evidence on their local effects. This paper uses a natural experiment in New York City to estimate the local effects of new tax-exempt residential construction. In 2006, the city government decided to make property tax incentives on new construction less generous, but only starting in 2008. Developers rushed to build and claim incentives before the deadline in response. I instrument the number of new units developed within 150 meters from a rental building by the baseline number of vacant parcels available within the same distance. Using a new dataset of rents and investment at the level of a building, I find that the existing rental building's rent increased by 2.3% in response to an additional tax-exempt unit built within a 150 meters radius. I provide evidence consistent with the hypothesis that new residential investment rendered neighborhoods more desirable by attracting affluent households and facilitating the entry of businesses and consumption amenities. Overall, the results indicate that tax-exempt new construction spurred gentrification.

Keywords: Property tax incentives; gentrification; housing supply; amenities

JEL Codes: R31, H23, H71, H30

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Major cities across the United States are grappling with steep increases in housing costs. For instance, more than half of tenants spend more than 30% of their income on rents in New York City. Consequently, economists and policymakers have suggested that a solution to tackle rising rent is to build more units (CEA [2019]; Glaeser et al. [2005]).[1] However, at the ground level, such proposals often face resistance from local residents and politicians who dislike the changes new development brings to a neighborhood (Quigley and Rosenthal [2005]). Understanding these changes is crucial for adequate policy design. My paper asks: What are the effects of new residential construction on rental prices in a neighborhood? Finding credible causal evidence is particularly challenging, because changes in investment and rent growth are strongly correlated. For instance, developers choose *where* to invest and *when* to invest.

In this paper, I make progress on this front by using a rare natural experiment in New York City. The government abolished long-standing property tax benefits on new residential units, but it announced the policy change before implementation. Consequently, developers responded by rushing to apply for the expiring benefits and building more units. I estimate the effect of this short-term increase in new units on existing buildings' rents in the neighborhoods in which these units were constructed. The decline in the expected future return due to the tax reform provides variation in timing that is not correlated with rents. The availability of microdata at the level of a parcel on investment, rent, and property tax data allows me to estimate local effects and control for time-varying factors that determine project location. [2]

I find that an increase in the future property tax as part of the reform significantly increased new rental residential construction in the anticipatory period. However, the units brought to the market by the reform increased the rents in the neighborhoods in which they were built. I propose that one explanation for why new units increase nearby rent prices is that new units increase the average income of the residents in the neighborhood. Higher neighborhood income, in turn, increases local amenities, such as sidewalk cafés, and allows incumbent landlords to charge higher rents. Evidence of changes in demographics and business composition in regions that received investment during this period provides support for this channel.

As a high-rent city with high property taxes, New York City provides an ideal setting.

[1] Other measures employed to increase affordability include rent control (Olsen [1972]; Diamond et al. [2018]); zoning; income tax credits (Diamond and McQuade [2016]; Baum-Snow and Marion [2009]); and public housing (Olsen and Barton [1983]; Chyn [2018]). See Olsen [2003] for a survey of studies on housing programs in the US.

[2] A parcel is a piece of land that contains one or more structures. An average parcel in New York City includes a building.

Rental buildings in New York face the highest property tax in the country (Lincoln [2018]). [3] The tax policy—421a property tax exemption—exempts beneficiaries from any increment in property tax due to new construction. Projects that convert vacant land into residential units qualify for the exemption. The standard model suggests that underlying demand and supply elasticities determine how much of the tax decrease is passed on to tenants. However, the model does not incorporate whether new investment leads to neighborhood change.

By increasing the conditions attached to the property tax exemption, the tax reform effectively raised property tax on new investment in select ”exclusion” regions in the city (blue regions in Figure 1) beginning in July 2008. However, the delay between announcement and implementation created incentives for developers to move investment forward in time.[4] The excess tax-exempt housing starts in exclusion regions in the period between announcement and implementation allows me to estimate the elasticity of current residential (rental) investment to anticipated changes in the future property tax rate.[5] Policymakers selected the exclusion regions based on political and economic reasons.

I find that the excess bunching in the housing starts in the time notch suggests that residential investment is responsive to the property tax. A 1% anticipated increase in the *future* property tax rate increased *current* residential investment by 0.4%. Lutz [2015] estimates a related elasticity of 0 within Boston and 1 in the suburban ring of Boston; however, this estimate combines inframarginal responses with new construction. Overall, the bunching increased the stock of rental housing in the exclusion regions by around 1%.

Next, I estimate the local rent effects of new construction in this period using an instrumental variables approach. In particular, the availability of vacant parcel within a small radius (150 meters in the baseline specification) of an existing rental building acts an an instrument for the magnitude of investment around it in the time notch. A building with a vacant parcel available within 150 meters at the start of the reform received 0.9 more tax-exempt units in the time notch, compared with buildings with no vacant parcels within that distance. The identification relies on the assumption that within a small census tract, which building had a vacant parcel available is uncorrelated with its rent growth in the reform period after controlling for building, year, and census tract-year fixed effects. Several factors not entirely in developers' control determine whether a building had a vacant parcel nearby. For instance, historic development patterns and whether the vacant parcel was up for sale at the time. The

[3] The average property tax rate as a share of assessed valuation is around 3.3% on rental buildings, compared with 0.6-1% on owner-occupied units.

[4] This is referred to as ”grandfathering.”

[5] In other words, the number of projects weighed by the number of units built in each project.

absence of pre-trends in the pre-reform period provides a useful test of these assumptions. Finally, the announced policy reform reduced the return to waiting by an amount exogenous to the rent growth, which provides variation in timing of the investment. A simple model of investment under uncertainty confirms that the investment decision at the deadline depends on the distribution of expected future return, current rents, and the cost distribution, all of which are accounted for in the proposed empirical strategy.

The instrumental variable estimate suggests than an additional tax-exempt unit within a 150 meter radius of an existing rental building in the time notch increased its rent by 2.3 log points. Expressed in elasticity terms, a 1% increase in the rental stock within 150 meters from existing buildings increased their rents by 1.8%. Moreover, in the pre-reform period, trends in rents of buildings with no vacant parcel within 150 meters do not differ significantly from trends in rents of buildings with an available vacant parcel within the same distance. This lends support to the exogeneity assumption. Additionally, price changes in exclusion regions corroborate this finding: Compared with non-exclusion regions, the sales price of owner-occupied homes, land, and commercial rents in exclusion regions increased after the reform. Similarly, differences in spatial regression discontinuity at the boundary of the exclusion region support the finding that rents increased in regions that received new tax-exempt residential investment in this period.

Having established that new tax-exempt construction increased nearby rents, I then show that the increase in business amenities such as restaurants and cafes explain this finding. While a larger rental stock in a neighborhood puts downward pressure on existing buildings' rents, it also makes neighborhood more desirable. This is because new units attract high-income tenants, since at any point of time, new units have depreciated less than older units.[6] These high-income tenants increase demand for local goods and services, which facilitates the entry of businesses. The arrival of these business amenities renders neighborhoods more desirable and increases rents in existing buildings. Note that this business activity channel is in addition to the direct aesthetic effects of new construction. For instance, research has shown that foreclosures have negative spillovers to the sales prices of existing homes (Anenberg and Kung [2014]; Campbell et al. [2011]), and subsidized homeownership programs (Ellen et al. [2001]), eliminating rent control (Autor et al. [2014]), widespread simultaneous reconstruction due to fires (Hornbeck and Keniston [2017]) have been shown to have positive spillover on

[6] Sweeney [1974] develops a commodity hierarchy model in which a consumer faces a choice between quality and quantity, and the population growth leads to the construction of high-quality units for high-income residents. In the long run, new units filter down to low-income users as they depreciate.Rosenthal [2014] provides empirical support for filtering in rental units. However, he does not find evidence of filtering in owner-occupied homes.

the prices of nearby properties.

I find several pieces of evidence consistent with the business amenity hypothesis. First, using the number of vacant parcels at the baseline as an instrument and census data, I find significant demographic changes in census tracts that received new residential investment in the period following the tax reform. An additional tax-exempt residential project in a census tract increased the number of tenants who have a bachelor's degree, are white, and have higher income. With respect to businesses, I find that there is an increase in the number of sidewalk cafes (associated with high-income customers) and a decrease in laundromats (associated with low-income customers) in the exclusion regions in Brooklyn after the reform. This change coincides with a large new residential investment in the exclusion regions in Brooklyn during the reform period. Similarly, using city-wide Zip code-level establishment counts data, I find that larger residential investment in a Zip code increased the number of establishments in industries with high income elasticity. The industry-level income elasticities indicate the propensity of an industry to locate in a high-income zipcode.

In conclusion, my results suggest that while residential investment responds to the property tax exemption, local price effects of the new investment do not necessarily mirror aggregate effects. However, there are a few considerations regarding the external validity of these results. First, it is plausible that the effects would be different if the new investment arrived slowly or in regions with low density. Second, I do not estimate general equilibrium effects, if any. For instance, it is plausible that rents fell in the neighborhoods the tenants of new units came from. Third, the estimated effect is a lower bound to the actual effect, given that a substantial number of apartments in the city are subject to rent stabilization. Fourth, while the results in this paper estimate short run elasticities, they do not shed light on how the mechanisms would differ in the long run for instance, in response to unanticipated permanent property tax increases. Nevertheless, my paper's findings that new construction has differential local welfare effects help explain the puzzle why incumbent residents often oppose new development.

My paper makes three main contributions. First, it provides first empirical evidence that new residential investment leads to gentrification. Causal evidence on the determinants of gentrification is limited. Authors have suggested that endogenous amenities (Diamond [2016]); rising top income inequality (Couture et al. [2019]); aggregate demand (Gyourko et al. [2013]); longer work hours (Edlund et al. [2016]); and pioneer businesses (Behrens et al. [2018]) could potentially be contributors to the rising gentrification in cities. Regarding the consequences of gentrification, Dragan et al. [2019] find no effect of gentrification on low-income mobility, though slight health improvements in children who grow up in gentrifying neighborhoods. Moreover, my paper shows that though new residential investment invigorates neighborhoods and increases the revenue base, it comes at the welfare cost of existing tenants if they do not value such amenities. Back-of-the-envelope calculations reveal that annual welfare loss to local low-income households is $524 per unit, welfare gains to local building owners is $1,050, and the city government gains $157 additional property tax revenue, as a result of spillovers from new housing construction. Low-income housing construction (Nathanson [2019]) and the concentration of richer households in downtown neighborhoods (Couture et al. [2019]) have been shown to have welfare costs for low-income households in the presence of endogenous amenities.[7]

Second, my paper finds that a property tax break for new residential investment stimulates housing supply, which suggests that tax policy is a determinant of housing supply. Authors have identified regulation (Gyourko and Molloy [2014]; Quigley and Raphael [2005]) and buildable land (Saiz [2010]) as key constraints that deter housing supply increases from keeping pace with rising demand in cities. While others have estimated the price/rent elasticity of investment (Case and Shiller [1989]; Topel and Rosen [1988]; Green et al. [2005]; Meese and Wallace [1994]; Glaeser et al. [2008]; Saiz [2010]), my paper estimates this elasticity with respect to the future property tax in a quasi-experimental setting.

Third, in addition to estimating the effect of property taxes on rental prices, my paper provides empirical evidence that changes in amenities, such as the entry of businesses, have consequences for who bears the tax burden. [8] [9] Though a vast literature in public finance investigates the effect of general property taxes on house prices (e.g., Oates [1969]) and renters (e.g., Orr [1968]; Carroll and Yinger [1994]), finding causal estimates is challenging. Some reasons include the lack of experimental variation in tax cuts and unavailability of comparable city-level property tax rates and rents data. Loffler and Siegloch [2018] make progress in this regard and find that in Germany the property tax is entirely shifted to renters in the long run. Additionally, my paper adds to a burgeoning literature in public finance which

[7]A recent paper by Li [2019] uses a similar dataset from New York City. She finds that rents in existing buildings decline after the completion of a new high-rise within 500 feet. My paper does not restrict the sample to high-rise buildings and estimates the rent effects at the *announcement* of new construction. Consistent with her finding, I find that the rents decline post-completion, but this decline does not compensate for the initial rent increase.

[8]Some papers that study the disrtibutional effects of amenity changes include Albouy and Staurt [2016]; Albouy and Zabek [2016]. See England [2016] for a survey of empirical papers that estimate the relationship between property taxes and rents.

[9]Even doing a simple cross-section OLS is not trivial. An extended panel of city-level rents is challenging to obtain, and there is no dataset of effective city-wide tax on rental properties across the US. Surveys such as AHS report property tax for owner-occupied homes but not for renter-occupied units. I circumvent these issues by performing a within-city analysis. All rental properties face the same marginal tax rate. Moreover, assessed valuations proxy net rental income of landlords.

investigates taxes in the housing market in empirical settings using microdata (Kopczuk and Munroe [2015]; Best and Kleven [2018]; Slemrod et al. [2017]; Dachis et al. [2011]; Besley et al. [2014]).

The paper is organized as follows. Section 1 describes the property tax reform and the datasets used in the study. Section 2 estimates the excess housing starts triggered by the tax reform. Section 3 describes and implements the empirical strategies to estimate the local rent effects of new residential investment. Section 4 explores the mechanisms. Section 5 provides back-of-the-envelope calculations of welfare effects. Section 6 concludes.

1 Background and Data

1.1. Property Tax Reform in New York City

The key change in the property tax on new residential investment in New York City was caused by the 2006 reform of a tax policy called "421a" for the section of New York Real Property Tax law, that exempts developers of new construction from property taxes. Generally, new construction increases the assessed valuation, which increases tax payments even when the tax rates are unchanged. In contrast, the 421a property tax exemption provides tax relief for new investment by restoring the assessment to preconstruction value.[10] All new project starts on "underutilized" land with at least three or four proposed units are generally eligible for the exemption [11], which is the single most extensive tax expenditure program in New York City. In 2015, foregone taxes on exempt units cost the city $1.1 billion and represented 15% of all tax expenditures of the city.

The property tax is a significant cost for new residential investment. The statutory property tax rate on a rental building with at least 10 units in New York is around 5.5%. The "rate" here is defined as a fraction of the *annual market valuation*, as opposed to *annual rental income*.[12] This is because the property tax is supposed to be a tax on the *capitalized* income and not actual annual income. I show, in Appendix A-5, that as a share of annual rental income, property tax costs are much larger. For instance, a 5.5% tax on annual market valuation is equivalent to a tax rate of 33% on annual gross rental income. An exemption that reduces the tax bill, therefore, is attractive. Because city governments across the United States use varied

[10] This does not imply that the post-construction tax liability is zero. If the assessed value of the existing structure is nonzero, then the tax liability is most likely positive. 421a Tax Exemption prevents steep increases in property taxes due to large increments in assessed value because of construction.

[11] §6-02 of Title 28 of the Rules of the City of New York [link].

[12] Property owners are also subject to an income tax, either corporate or individual.

approaches to impute market valuation of rental properties, in this paper I report tax elasticities as measured responses to changes in the statutory tax rate on market valuation (5.5%) as opposed to actual rental income (33%).[13]

The Department of Finance's market valuation serves as a good proxy for the income of large rental buildings. This is because annual market valuation for a given fiscal year is based on net operating income minus expenses times a capitalization factor. Information on rental incomes and expenses is collected through the mandatory filing of Real Property Income and Expense (RPIE) reports by large rental building owners each year.[14] For a more detailed description of the administration of property tax in New York City, refer to Section A-5.

The program originated in 1971, when New York City was suffering from steep economic and physical decline. People fled to the suburbs as manufacturing and housing quality declined in the city. In response, the state legislature enacted the 421a Real Property Tax Exemption in 1971 to stimulate new multi-family housing development. Between 1971 and 1984, around 30% of construction claimed 421a exemption (Report [2014]). The program continues to be popular; Figure A-3.2 shows that more than half of the units built each year between 2001 and 2012 claimed the tax exemption.

The property tax exemption stimulates investment by increasing the net of tax return. This occurs through two channels. First, the availability of the tax exemption directly lowers the tax liability for a significant period, which increases the return to new investment. Second, there is evidence that developers often face liquidity constraints.[15] The increased market valuation of the proposed project on receipt of the tax exemption can help developers to raise capital and build more.[16]

There are also several reasons why a developer does not apply for the exemption. First, if the project is located in the "exclusion regions" as discussed next, mandatory provision of affordable units adds significant costs for the developer. Second, even when developers are not required to provide affordable units, non affordable units in the project are subject to rent

[13]The use of the term "annual market valuation" in this paper refers to the value as assessed by the Department of Finance for taxation purposes, as opposed to the actual sale value of the property on the market.

[14]A "Large" rental building contains at least 10 residential units.

[15]Topel and Rosen [1988] document the high volatility of housing starts. Stein [1995] suggests the liquidity constraints faced by developers as a possible explanation. Higher wait times constrain the start of new projects, as developers must sell the current project to obtain cash for the next project, and resort to a "fishing for liquidity" strategy.

[16]Best and Kleven [2018] provide similar evidence for highly leveraged homeowners. A cut in transaction taxes eases borrowing constraints for such homeowners and increases trade of existing housing stock. Zwick and Mahon [2017] show that financially constrained firms respond more to a tax stimulus than financially unconstrained firms.

stabilization and maximal rent (Cohen [2009]). Third, project starts on land that previously contained an income-producing property are not eligible for the exemption. Finally, projects with at least three or four residential units are eligible for the tax break, which effectively limits the tax break to condos and rental buildings.

The 2006 reform expanded the existing exclusion regions i.e., regions with restricted property tax benefits thereby reducing generosity of the tax benefits in key neighborhoods. Table 1 summarizes key changes in the three regions. It discontinued the offsite provision of affordable units in existing exclusion regions (light blue shaded region in Figure 1). Before the reform, developers in these regions were required to provide affordable housing to obtain the exemption.[17] Because these were high-rent regions, developers could save costs by providing affordable housing in offsite low-rent regions. After the reform, developers were required to provide affordable housing onsite. I estimate, in Appendix A-6, that removal of the offsite option increased developers' costs equal to a property tax rate increase of 2.3 pp.[18]

After the reform, in the newly added exclusion regions (unshaded dark blue regions), developers could no longer opt for the short exemption—-a 15-year property tax exemption without any affordability requirement. As I show in Appendix A-6, in low-rent neighborhoods where providing affordable units onsite is inexpensive, the reform largely did not alter tax incentives. In contrast, in high rent neighborhoods within the newly excluded regions, the reform took away the tax benefit by increasing the costs associated with the property tax exemption. This suggests that depending on the project location, either the reform did not affect tax liability or it increased the tax rate, ranging from 0 to 5.5%.[19]

Elsewhere in the city, in general, the reform largely did not affect the tax incentives. This is because low rents in these regions made onsite affordable units inexpensive, and the reform did not affect its benefits significantly. The short 15 year exemption without affordable housing condition continued to exist, but its benefits were capped. Specifically, a citywide cap

[17]Affordable units are restricted to households with 30% to 100% of the area median income (AMI), with a rent limit at 30% of the income of the tenants in the affordable units. AMI is calculated each year by the U.S. Department of Housing and Urban Development (HUD). The 2017 AMI for NYC was $85,900 for a three-person family, constant across all regions within the metropolitan area.

[18]Earlier, the developers could fulfil this option by providing affordable units offsite in low-rent regions. This was achieved through the use of a negotiable certificate program. Under the negotiable certificate program, affordable housing developers receive 4 to 5 certificates for each unit they produce, which then can be sold to market-rate developers.

[19]Even in high-rent regions, it is possible that developer prefers an onsite long exemption. This is the case when the same set of affordable units allows a developer to fulfill affordability requirements for multiple state support programs such as 421a, LIHTC, and Mitchell-Llama. This is referred to as "double dipping." This could also explain why we observe nonzero response in long 25-year exemption in newly excluded regions. In such a case, removal of a short 10-year exemption would have no effect on the effective tax rate.

of $65,000 per unit was imposed on units with a 15-year exemption.[20] Because a developer could obtain a 25-year exemption at practically no cost before and after the reform, the tax change in this region was minimal.

Finally, the reform introduced more changes that were applicable everywhere. First, the minimum number of units in the project to be eligible for the exemption was increased from three to four. Second, conditions attached to affordable units in buildings with the 25-year exemption were made stricter. All affordable units were rent stabilized for 35 years. Also, affordable units were required to have either the number of bedrooms comparable to market-rate units or a specified mix.

1.1.1. The Time Notch

The time notch arose from the delay in implementation of the tax change. In February 2006, discussions to reform the exemption policy began with the creation of a task force that identified additional regions to be included in exclusion regions.[21] The task force submitted the final report to the City Council in October 2006. However, the proposal faced political resistance. Both the city and the state legislature added more regions. Figure A-3.1 shows the initial regions picked by the task force (shown in red) and the regions added by the city council (blue) and state legislature (yellow). The mayor signed the local law on Dec. 28, 2006, and the legislature expanded/amended the local law. The final version of the new 421a law was signed by the governor on February 19, 2008. I refer to the period between December 2006 and June 2008 as the time notch, where the "notch" arises from the fact that investment in the notch period is associated with a strictly higher property tax, compared to investment after the notch period. A developer who obtained a permit, or installed a metal or concrete load-bearing structure before July 2008 was eligible for exemption benefits under the old law. Figure A-3.3 outlines the key steps a developer must go through before obtaining the permit.

1.2. Data

I collected and organized several datasets from city government agencies. Detailed information on the sources of these datasets can be found in Appendix A-4. Here I provide a brief summary of the datasets used in this paper:

[20]The annual tax benefit for a 15-year exemption was capped at [tax rate $\times$ $65,000$]per unit.

[21]To identify such neighborhoods, the task force calculated the return to developers for each neighborhood and building type, using land acquisition cost, hard construction costs, and sale prices. This analysis was carried out with and without the exemption. The task force picked regions where the sales price covered costs without the exemption (see OMB [2008]).

1. **Rents**: MapPLUTO provides geo-coded location and details about the structures on each parcel in the city. Each parcel is identified using a unique identification number called the BBL (Borough, Block, Lot). Along with the building classification, and number of residential and commercial units, this dataset provides information on the actual assessed value (equal to the assessment ratio times the market valuation). As discussed in Section A-5, the market valuation of rental buildings serves as a proxy for their rental income.

2. **Permits data:** An important measure of the flow of residential investment is the issuance of permits for new construction. The Department of Buildings provides data on the universe of permits issued at a monthly level from 2003. The type of permit depends on the nature of alteration sought. Relevant permit types for new construction are DM (demolition), NB (New Building) and A1 (major alteration that changes occupancy of the building). Each permit is linked to a building identification number (BIN). I use the BIN and a crosswalk between the BIN and the BBL provided by the property address directory to merge this dataset with MapPLUTO. See Figure A-4.17 for an explanation of the relationship between the BIN and the BBL.

3. **421a beneficiaries:** Buildings/units that benefit from a 421a tax exemption are identified through their BBL using 421a lists provided by Department of Finance. While these lists are exhaustive, they do not identify whether the beneficiary obtains a 10-year or a 15-year exemption. For this reason, I use 2015 property tax returns of owners residing in these buildings to obtain the type of exemption claimed.

4. **Selling price:** The Department of Finance provides information on sale date, sale price, and characteristics of the property for every arms-length transaction in the city. The sample period covers 2003-2018. This dataset is merged with other datasets using BBL to identify the effects on landowners and developers.

5. **Demographics data:** The Census 2000, and the 2009 and 2017 Americal Community surveys provide information about homeownership, tenure, race, rent, and income at the census tract level. New York City has 2,168 census tracts in total.

6. **Local businesses:** I use two main datasets to estimate the effects on business activity.

    (a) Department of Consumer Affairs license dataset: New York City requires certain businesses to apply for a DCA (Department of Consumer affairs) license. This includes businesses such as sidewalk cafes, laundries, and home improvement contracts. This dataset provides information on license creation date and the geocoded location of the business. Because it covers a fraction of the industries that operate in the city, I complement the analysis with the Zip code-level business counts dataset as described below.

(b) County Business Patterns data: This dataset provides information on the number of establishments by industry-Zip code-year. New York City contains approximately 200 Zip codes. Counts are available at the 6-digit NAICS industry level.

2 The Time Notch and Residential Investment

Because the increase in property taxes on new residential investment in 2008 was known in advance by developers, this created incentives to move their investments ahead in time. In this section, I use a simple framework to show that with the uncertainty and irreversability of investment, an increase in the future property tax stimulates current residential investment by reducing the return to waiting. The forward movement of investment provides exogenous variation in the arrival time of new projects, which is useful for estimating their local price effects. As a by-product, the excess bunching in exclusion regions provides an estimate of the *timing* elasticity of residential investment with respect to the property tax.

Consider the case in which the investment decision for developers is characterized by irreversibility and uncertainty in future return (Dixit and Pindyck [1994]). If things go badly, developers can not destroy the building and fully recover the initial cost of investment. This implies that the decision to invest in any period is equivalent to exercising an option. Each period, developers choose to invest or wait, given the uncertainty in future returns. Waiting is profitable when developers expect future prices to be higher. In contrast, a future increase in the property tax stimulates current investment by decreasing the return to waiting. Consider two periods: today and tomorrow $j \in \{0, 1\}$. Developers decide in period 0 whether to invest today or tomorrow. Assume the period 0 capitalized return is known to be R_0. Next period return is distributed according to function $G(\tilde{R})$. Period 1 returns are discounted at the rate β.

A developer i has perfect knowledge of investment cost C_i.[22] These costs are heterogeneous across developers, distributed according to a cumulative distribution function $F(\cdot)$. [23]

[22]These costs could include the costs of acquiring land and/or the cost of materials and labor.

[23]It is reasonable to assume that land and zoning regulations (Gyourko and Molloy [2014]) and geographical constraints (Saiz [2010]) affect the cost distribution. For instance, we can imagine that zoning regulations make the variance of the probability distribution low. Only a few low-cost developers are able to invest when most developers have high costs.

With no time notch and no property tax, a developer i invests in period 0 if (i) the net present value of investing today is greater than the net present value from investing tomorrow; and (ii) the capitalized current return R_0 is greater than costs C_i. If he invests today, his return is $R_0 - C_i$. If he waits till tomorrow, he can expect a return of $\beta E_G \max(\tilde{R} - C_i, 0)$. Clearly he invests in period 0 when

$$R_0 - C_i > \beta E_G \max(\tilde{R} - C_i, 0) \tag{1}$$

$$R_0 - C_i > 0.$$

For developers with a period 0 return lower that the cost, the period 0 vs 1 investment trade-off is trivial and their period 0 investment does not respond to any changes in the distribution of period 1 return. This is because when $R_0 - C_i < 0$, investment condition requires $\beta E_G \max(\tilde{R} - C_i, 0) < R_0 - C_i$, which is less than 0. For other developers, the decision to invest today is a function of (i) investment cost C_i; (ii) distribution of period 1 returns $G(\cdot)$; and (iii) discount factor β.

When $\beta < 1$, there exists a $\tilde{k}$ that satisfies equation 1 such that when $C_i < \tilde{k}$, developer i invests in period 0.[24] It is easy to see that $\tilde{k}$ is a function of R_0, β and the distribution of period 1 return $G(\cdot)$. A higher return in period 0 makes it worthwhile for developers to invest in period 0. When R_0 increases, $\tilde{k}$ decreases. With a higher β, developers are patient and willing to wait till period 1 to reap any future rewards. When β increases, the right-hand side increases and $\tilde{k}$ falls. Therefore, period 0 investment falls. Finally, a higher mean in period 1 return lowers period 0 investment. For instance, an increase in period 1 mean increases the right-hand side of equation 1 and lowers $\tilde{k}$. This reduces period 0 investment. Similarly, a mean-preserving spread that increases the variance in $\tilde{R}$ reduces period 0 investment by lowering $\tilde{k}$. This is because with a higher variance, larger $\tilde{R}$ values are more likely and waiting is worth more. The total period 0 investment is given by

$$I(0) = \sum_{i=1}^{N} \mathbb{1}(C_i < R_0 \& C_i < \tilde{k})$$

$$= NF(\min(R_0, \tilde{k})).$$

[24] When $\beta < 1$, the slope of the convex function on the right hand side is less than 1, whereas the left hand side is a linear function with slope 1.

The time notch introduced a property tax t on period 1 return, which implies that the developers' net of tax period 1 return falls to $\frac{\tilde{R}}{1+t}$ should they choose to wait. This increases $\tilde{k}$, and therefore the period 0 investment. Investment with a tax is given by

$$I(t) = NF(\min(R_0, \tilde{k_t})),$$

where $\tilde{k_t} > \tilde{k}$, and therefore $I(t) \geq I(0)$. The increase in period 0 investment corresponds to the excess bunching in the time notch.

$$B = I(0) - I(t) = NF(\min(R_0, \tilde{k_t})) - NF(\min(R_0, \tilde{k})). \tag{2}$$

If we normalize quantity 2 with $I(0)$ and the property tax change t, we obtain the *timing* elasticity of investment with respect to the property tax. The reduced form estimate is given by

$$\epsilon^t = \frac{\hat{B}}{I(0)t} \tag{3}$$

where $\hat{B}$ represents the empirical estimate of excess bunching in the notch period.[25] It is important to distinguish two kinds of elasticities: timing elasticity, which is the response of current investment to future increase in property tax, and long-run or contemporaneous elasticity, which is the response of long-run investment to a permanent property tax increase, given by

$$\epsilon^l = \frac{\frac{\Delta I}{I}}{t} = \frac{F(R_0) - F(\frac{R'_0}{1+t})}{\frac{\Delta t}{t} F(R_0)}, \tag{4}$$

where R'_0 is the equilibrium tax-inclusive rent with a nonzero property tax. In general, elasticities 3 and 4 are not equal. Unfortunately, while the reform sheds light on the timing elasticity, it does not inform the long-run rent elasticity. Nevertheless, to the extent that short-term increases in investment can stimulate the local economy, timing elasticity is a crucial parameter for empirical estimation. [26]

[25]Instead, if we normalize by housing *starts*, we obtain the property tax elasticity of housing starts:

$$\epsilon^p = \frac{\hat{B}}{P^0(0)t}$$

where $P(0)$ denotes counterfactual starts in the notch period.

[26]Please refer to Saiz [2010]; Mayer and Somerville [2000]; and Green et al. [2005] for an estimation of the rent

2.1. Anticipated Tax Increase and Short Term Outcomes

The time notch is equivalent to an anticipated permanent property tax increase. This implies that the trajectory of the outcomes in the short term differ when the tax increase is anticipated, compared with the case when not. Here I provide a simple intuition. Refer to Appendix A-7 for a simple framework.

Figure 4c illustrates the time path of rents and investment in two scenarios: (i) Policy 1, in which the permanent tax increase is unanticipated; and (ii) Policy 2, where the same tax increase is anticipated. The long-run decline in investment and net-of-tax rent is equal in the two scenerios. However, the short term outcomes differ starkly. Investement quickly drops to long-run value at the policy implementation in the unanticipated tax increase case (Panel a), whereas it increases in the time notch in the anticipated tax increase case as developers move investment forward. Similarly, net of tax rents fall following the announcement (and implementation) in the unanticipated case, but increase over time as future investment falls and landlords are able to shift some of the tax burden onto tenants. In contrast, in the anticipated case, the bunching of investment in the time notch increases net rents received by landlords when amenities are sufficiently elastic (i.e. $\epsilon_a > 1 + \frac{1}{\epsilon_t}$.). Rents slowly begin to approach the long-run equilibrium once the high tax rate is implemented.

Two points are worth noting. First, while the results in this paper do not inform long-term outcomes, they highlight the role of endogenous amenities in the incidence of the property tax: short-term increase in investment increases local prices. Second, when depreciation is low, the period up to T_0 is long. This implies that there is an extended period in which the net of tax rents are high despite a higher property tax. The government benefits in this period from larger revenues stemming from a larger property tax base and economic activity.

2.2. Estimating Excess Housing Starts in the Time Notch

This section uses a bunching framework to estimate excess tax-exempt housing starts in the time notch. I also provide estimates of timing elasticity of residential investment with respect to the property tax.

The Housing Type

New York City offers a wide array of housing types. These include rental units, which constitute buildings of types C (walk-ups) and D (buildings with elevator), and owner-occupied condos and coops, classified as type R and family homes. However, the 421a property tax ex-

(price) elasticity of investment.

emption only applies to rentals and condos. As Figure A-3.6 illustrates, both types are fairly popular with the exemption. Because we can expect that tenants care about the stock of *rental* housing available at a point in time, the relevant policy parameter to estimate is the rental housing starts/investment elasticity. Therefore, this paper focuses on the rental investment response. [27] Nevertheless, note that the response of condo starts in the notch period was not insignificant. They account for one-third of total starts in the notch period.

The Counterfactual

Standard bunching estimation in public finance deals with notches in income distribution (Saez [2010]; Chetty et al. [2011]) or price distribution (Kopczuk and Munroe [2015]). Bunching estimates are calculated as the difference between the actual distribution and the counterfactual in the notch region (or time in the context of my paper). Similarly, the difference between the actual distribution and the counterfactual to the right of the notch region is the missing mass. A key input in the calculation of excess bunching and missing mass estimates is choice of counterfactual. One choice is a high-degree polynomial fit of the actual distribution that uses data outside the notch region. Estimation using this approach is sensitive to the choice of upper bound and lower bound, in addition to the choice of the degree of the polynomial (Kleven [2016]). An issue with extending this methodology to time notch is that this counterfactual does not account for any global time shocks during the time notch. Therefore, similar to Best and Kleven [2018] and Kopczuk and Munroe [2015], I use an empirical counterfactual. Specifically, I pick a comparison group whose housing starts time series serves as the counterfactual for tax-exempt housing starts. The lower bound is fixed at the announcement date.

I choose *non-tax-exempt* housing starts in exclusion regions as the comparison group, instead of housing starts in the non-exclusion region (either tax-exempt or non-tax-exempt), for two reasons: (i) it ensures that actual and counterfactual distributions represent the same geographic area; and; ii) it helps to alleviate concern that the recession, which started in 2007, had differential effects on housing starts in the exclusion and non-exclusion regions. Aggregates are sensitive to the magnitude of the shock, therefore using housing starts in non-exclusion regions could overstate the bunching observed in the exclusion region, for instance, if the recession had a larger impact on housing starts in the non-exclusion regions. For instance, Figure A-3.12 plots the spatial distribution of foreclosures in the city in 2008. Foreclosures are concentrated in non-exclusion regions. The identification assumption is that in the absence of

[27] In all bunching graphs, I plot the response of investment in rental units, which includes permits for building types C and D. Figure A-3.5 shows bunching results including condos. The estimation is less clean due to a clearly preexisting rising trend in condo starts.

the time notch, trends in aggregate tax-exempt housing starts are parallel to non-tax-exempt starts within the exclusion regions.

2.3. Bunching Results

Figure 5 plots the quarterly number of tax-exempt permits issued in New York City in the raw data, split by exclusion and non-exclusion regions. Dotted green lines indicate the quarter-year when changes to the policy were announced and implemented. The graph uses permits data from Department of Buildings merged with 421a exemption lists to identify completed tax-exempt units. The time stamp in the permits data indicates the date the permit for construction of a new building was issued. A key thing to note is that these do not include actual tax-exemption applications. If some developers intended to apply for the property tax exemption but failed to obtain approval, or if they proved the eligibility of ”commencement of construction” requirement by installing new metal or concrete structure instead, the bunching estimates would underestimate the actual response.

Figure 6 shows the spatial distribution of tax-exempt non-condo projects. The top panel presents the distribution for the period, 2005q2 to 2006q4,which is the same length as the time notch. Each project is weighed by number of the residential units. The top panel shows that the tax-exempt projects in Manhattan tend to be fewer but bigger. However, there is no specific spatial pattern in the pre-notch period. In contrast, the bottom panel shows that more and larger tax-exempt projects are started in exclusion regions in time notch, and Manhattan and Brooklyn in particular. This is consistent with the concentration of residential investment in exclusion regions in the time notch. To see what exemption type developers sought, Figure A-3.4 splits the bunching by exemption type, separately for exclusion and non-exclusion regions. As expected, the largest spike occurs for short exemptions in the exclusion region; this exemption provides a 15-year benefit without a requirement to provide affordable housing. There is also a smaller spike in the long 25-year exemption preceding the 2008q2 spike, which is driven by removal of the offsite long exemption in old-exclusion regions (shaded regions). Figure 7 plots tax-exempt housing starts in each borough in this time period. We see that Manhattan, Brooklyn, and Queens see sharp increases in housing starts that are not completely reversed post-reform, despite the start of the recession in 2008.[28] The magnitude of bunching varies across boroughs. Finally, estimates of bunching are higher by 80% if we include the response of tax-exempt condo starts (Figure A-3.5). Comparing 7 and A-3.5 suggests that a large share of condo starts in the time notch were concentrated in Manhattan and

[28] I exclude Staten Island, because it largely consists of family homes. Correspondingly, housing starts for type C or D buildings (rental) are small and noisy.

Brooklyn.

The magnitude of bunching in the number of tax-exempt units uncovers the elasticity of the *current* residential investment to future increase in the property tax. The empirical analog of equation 3 can be estimated as follows:

$$\hat{\epsilon^t} = \frac{\sum_{2006}^{2008}(f_{tax\_exempt} - f_{cf})}{\frac{\Delta t}{1+t} S^{2007}}, \tag{5}$$

where the numerator, Excess bunching, is calculated as the difference in the quarterly distribution of tax-exempt rental units starts, f_{tax_exempt}, and the counterfactual, f_{cf}, in the notch period. The *excess bunching* estimate is divided by the existing housing stock in 2007 in the exclusion region to obtain percentage change in housing supply due to the time notch. It is then further divided by the percentage change in the (one-plus) property tax rate to obtain the elasticity, which was equivalent to 2.5 percentage points; this is the average property tax rate change caused by the reform. Note that these estimates capture elasticity with respect to the tax rate on the market valuation rather than annual rental income.

I estimate the numerator by running a simple regression on the counts data of the following form:

$$c_t = \beta_0 + \gamma_t + \beta_1 (Tax\_Exempt)_t \times Notch_t + \beta_2 (Tax\_Exempt)_t + \epsilon_t$$

where t denotes the quarter, c_t denotes aggregate quarterly starts in the quarter t, and $(Tax\_Exempt)_t$ indicates the total tax-exempt housing starts in quarter t. The omitted dummy is the indicator for the counterfactual, which in this case includes non-tax-exempt housing starts. β_1 provides an estimate of the average per-quarter excess bunching in the time notch, which is scaled by 7, to estimate the total bunching observed in the time notch. (The time notch includes Dec q4 to June q2, a total of 7 quarters.)

Figure 7 reports the excess bunching estimates for the exclusion regions in New York City, using non-tax-exempt housing starts as the counterfactual. We observe that while the two distributions are fairly parallel in the pre-reform period, there is a large spike at the notch, for both tax-exempt units (the left panel) and tax-exempt buildings (the right panel). Reassuringly, there is no spike in non-tax-exempt starts, which supports the assumption of an absence of regional non-tax-exempt-related shocks in the exclusion regions in this period. Overall, construction of around 10,735 tax-exempt units and 270 buildings began in the exclusion regions in the time notch. Figure 8 reports the distribution of excess bunching estimates across the four boroughs (Manhattan, Brooklyn, Queens, and the Bronx).

Table 5 summarizes excess housing starts in the time notch citywide and by borough. Excess housing starts in the time notch correspond to 1% of the city's rental housing stock in 2007. There is substantial variation in relative housing starts across boroughs. Manhattan had approximately 3,000 excess housing starts, Brooklyn 5,000, Bronx 200, and Queens 2,500. These correspond, respectively, to approximately 0.4%, 2.1%, 0.8% and 6.8% of the existing rental housing stock in the region. While the differential excess housing starts across boroughs could reflect borough-wide differences in tax elasticities, they also partially reflect borough-wide differences in shock intensity.[29] Nevertheless, the estimates suggest that the housing supply is inelastic with respect to the property tax rate. Assuming that the excess housing starts in the time notch are in response to a property tax increase of 2.5%, we obtain an elasticity of $\frac{1}{2.5} = 0.4$. Specifically, a 1% increase in the future property rate for new housing increases current residential investment by about 0.4% in New York City.

The first concern in the excess starts estimation is the possibility that developers substituted from non-tax-exempt to tax-exempt housing in the time notch. In such a case, using non-tax-exempt starts as a counterfactual leads to overestimation of true excess bunching. To alleviate this concern, I check robustness with respect to alternative distributions as counterfactual. Estimates are fairly robust when I use either housing starts in non-exclusion regions (Figure A-3.13), or ineligible and non-housing starts in exclusion regions as the counterfactual (Figure A-3.14).[30] In either case, the excess bunching estimate is close to 10,500 tax-exempt units.

The second concern relates to the time to completion of the proposed projects. While the bunching estimates only include units that were finished by 2015, it is nevertheless possible that the tax-exempt units were not completed in a timely fashion. This is even more relevant because the recession started in 2008. As a matter of fact, a rule change in 2013 extended the "undue delay" period from 36 months to 72 months for projects that were subject to mortgage foreclosure or other lien enforcement litigation before May 14, 2012.[31] To address this concern, Figure A-3.7 uses the PLUTO data to identify completion year of buildings started in the time notch. While a large share of the buildings finished by 2009, a small share of projects took more than three years to finish.

The excess housing starts observed in the time notch had significant effects on the magnitude and composition of housing stock in the exclusion regions. The reform increased the

[29]Recall that the tax reform made it mandatory to provide affordable housing onsite, the cost of which depends on rents that vary across neighborhoods.

[30]These include commercial and family homes.

[31]See §11-245.1 of Title 2, New York City Administrative code.

concentration of new tax-exempt investment. This has two effects. First, it implies that a greater share of housing in the exclusion regions is newer. Second, because most of the new housing stock benefits from the property tax exemption, the effective tax on these new units is lower. For instance, the top right panel in Figure 26 depicts the effect of the time notch in Brooklyn, a region that will be useful for investigating mechanisms later. Dashed lines denote the *stock* of units built after 1990 in the exclusion and non-exclusion regions within Brooklyn. Thin lines denote the stock of tax-exempt units in either region. We see that the stock of new tax-exempt units increased sharply in the exclusion regions after 2006, which coincides with the investment shock in the time notch. Finally, Figure 9 shows that the increases in the total housing starts in the exclusion regions are not immediately reversed after the time-notch.

3 Effect of New Residential Investment on Rents

We are interested in estimating the effect of a tax-exempt project arrival within a small distance from an existing building on its rents. Consider rings of a fixed radius of 150 meters around an existing rental building.[32] A naive estimation is to compare the rent growth of buildings that receive tax-exempt projects within that radius with buildings that do not. A concern with this approach is that the location of new investment is likely endogenous to trends in building rents. On the one hand, a developer is more likely to build near a building where he expects higher future rents (such as gentrifying neighborhoods), leading to a positive bias in the OLS estimation. On the other hand, a high expected rent growth increases the land acquisition cost and therefore reduces the probability that an expensive rental building receives a tax-exempt project close by. This leads to a negative bias in the OLS estimation. In either case, whether a building receives a tax-exempt project nearby is plausibly endogenous to its rent growth.

The property tax reform and the setting provide key sources of exogenous variation in project arrival in both location and time. While the time notch provides exogenous variation in the time of project arrival, the availability of vacant land within a small distance from an existing building provides exogenous variation in project location (an instrument similar to that of Saiz [2010]). The idea is to compare the rent growth of buildings that had a positive number of vacant parcels within a small distance with that of the buildings that had no vacant parcels within the same distance. When the availability of vacant parcels acts as a constraint on investment, buildings with a larger number of vacant parcels receive more tax-exempt projects in the time notch. Figure 10 illustrates the empirical strategy. In this case,

[32] An average block in New York City is between 150 and 250 meters.

the instrument—land vacancy at the baseline—turns on for buildings in Rings 1 and 4 and is turned off for others. The empirical approach implies that the instrumental variables estimate captures the effect of the arrival of a tax-exempt unit within 150 meters from the rental building. The ”reduced-form” captures the difference in the average rent growth of buildings in rings 2 and 3 (which are more affected by the tax reform) and the rent growth of buildings in rings 2 and 3 (which are less affected by the tax reform) before and after the reform.

Identification : The validity of the instrumental variables approach requires two main assumptions: exogeneity and the exclusion restriction. Exogeneity requires that the baseline availability of a vacant parcel within a small distance from a building is uncorrelated to its rent growth. The exclusion restriction requires that the baseline land availability does not directly affect the building's rent growth. A way in which the exogeneity assumption would fail is when exactly those vacant parcels are undeveloped where developers expect lower future returns. This leads to a negative correlation between the baseline vacancy and the expected rent growth. Similarly, the exclusion restriction fails when a positive number of vacant parcels at the baseline signals lower future rent growth in that neighborhood. This would be the case, for instance, when a positive land supply signals low neighborhood quality. [33]

The empirical strategy I propose circumvents these issues in the following ways. First, the comparison of buildings within a small census tract broadly controls for trends in expected rent growth, which are common to all buildings within a census tract. This is achieved by inclusion of the census-tract times year fixed effects. [34] Second, the inclusion of building-fixed effects controls for any fixed unobserved neighborhood differences across buildings within a census tract. Similarly, year fixed effects control for any rent trends common to all buildings within New York City. Third, the main specification is restricted to buildings that had at most one vacant parcel. This helps address the concern that buildings with a larger number of vacant parcels signal low future rent growth. Consequently, within a census tract, which buildings had a vacant parcel nearby is an outcome of historic development patterns and whether a vacant parcel was up for sale, which I expect is outside developers' control.

Fourth, the time notch provides exogenous variation in the timing of project arrivals. As discussed earlier, the decision to invest in a vacant land is a function of the current rent, developers' cost, and the expected return from investment in the parcel. In periods in which

[33] If buildings with positive land nearby differ from buildings with no land only in terms of fixed *unobserved* characteristics, those differences are absorbed by the building fixed effects. The assumptions are violated when unit land buildings differ from no-land buildings in terms of *time-varying* factors correlated with rent growth.

[34] There are on average 48 rental buildings in a census tract in New York City. This number varies across boroughs: 76 in Manhattan, 34 in the Bronx, 21 in Brooklyn, 18 in Queens, and 5 in Staten Island. A Zip code is larger and has on average 332 rental buildings.

there is no notch, time-varying factors affect both the investment decision and expected rent growth. To the extent that the expected and the actual rent growth are correlated, investment and actual rent growth are also correlated. However, the time notch led to a decline in the expected return by an amount exogenous to the actual rent growth. This implies that after controlling for expected rent, current rent, and costs, the decision to invest in the time notch is uncorrelated with the actual rent growth.

I perform two tests to check the validity of assumptions. First, a signaling mechanism predicts that the first stage is negative; that is, buildings with a vacant parcel receive a lower number of tax-exempt units. The data do not support this. Figure 14b shows that if anything, a higher number of vacant parcels is associated with a higher number of tax-exempt projects in the time notch. Second, in the presence of significant correlation between the baseline number of vacant parcels and rent growth, we expect rents in buildings with a unit land supply nearby to grow slower in the pre-reform period, compared with the buildings with no land nearby. An implication is an observable lack of parallel pre-trends in the event study graphs of rent growth on baseline land availability. Before presenting the results, I provide key summary statistics here.

3.1. Summary Statistics

Figure 2 illustrates the distribution of the baseline 2007 total housing stock as reported in the PLUTO data. Each observation in the PLUTO data is a parcel, which refers to a piece of land containing one or more structures. Typically, a parcel in the city contains a building. Exclusion and non-exclusion regions together had around 3.3 million housing units, which is quite close to the official statistics reported in 2018 (Survey [2005]). Rental units form around 90% of the total units in the exclusion regions, but only one-half in non-exclusion regions. Family homes form a significant share of housing in non-exclusion regions.

Table 2 illustrates the average differences between tax-exempt and non-tax-exempt parcels. For each variable, the first and second rows denote statistics for non-tax-exempt and tax-exempt projects, respectively. Because all exempt parcels have been developed in the past 15-25 years, for comparison purpose, I restrict the sample to parcels developed after 1990. The table shows that tax-exempt and non-tax-exempt projects differ significantly along many dimensions. Tax-exempt projects are slightly larger in terms of residential units, and square footage and lot size, and are more expensive, and more likely to be condos, and less likely to be located in a high-income region. In contrast, both are almost equally likely to be located in exclusion regions.

Table 3 provides summary statistics for rental buildings in New York City in the PLUTO data in 2007, a year before the tax reform. Rental buildings are identified as those that have (i) more than 10 units and (ii) are classified C or D across all years in the sample period. Data are truncated to include buildings with at least 10 units, because the market valuation of such buildings reflects their rental income. Exclusion and non-exclusion regions are far from comparable in terms of housing characteristics. First, exclusion regions are denser. An average lot in this region is small—around 11,000 square feet—and has, on average, one building that contains approximately 50 units. Non-exclusion regions, on the other hand, have almost the same number of residential (rental) units but are located on a lot more than two times the size of a lot in exclusion region. Housing stock, as measured by the number of residential units, is similar across the two regions. Second, the rents in exclusion region are high. Compared with an average per unit monthly rent of about $686 (USD 2015) in non-exclusion region, exclusion region rent is more than two times that amount. In either region, an average parcel contains one building.

Table 4 illustrates the average differences in rental buildings in exclusion and non-exclusion regions within Brooklyn, which is useful when exploring the mechanisms in Section 4. Baseline differences in exclusion and non-exclusion regions are less pronounced within Brooklyn than within New York City overall. An average lot in exclusion region in Brooklyn has fewer units (44 compared to 57) and a smaller lot area. Monthly rent differences between exclusion and non-exclusion region in Brooklyn are only about $200, compared to $700 within New York City overall.

Finally, Table A-3.1 illustrates differences in the tax-exempt housing starts in the time-notch and the tax-exempt housing starts outside the time notch. The table suggests that the projects started in the time notch are larger in terms of building area, residential units and lot area, are more likely to locate in the high-income region,and are more expensive in terms of monthly rent. This is consistent with the fact that tax exemption makes it profitable to bring more expensive and larger projects ahead in time to benefit from generous benefits in the time notch.[35]

3.2. Results: Effect of Tax-Exempt Investment on Rents

Figure 13 shows that a non significant share of vacant parcels at the start (i.e. 2006), were converted into new tax-exempt projects in the time notch, suggesting that the baseline vacancy affects development. A key observation is that many tax-exempt project starts in the

[35] Recall that the exemption applies to only new units constructed and excludes land.

time notch occupied previously non-vacant parcels, despite the fact that the tax-exemption is restricted to vacant parcels. Zoning changes and lobbying sometimes allow developers to build on non-vacant parcels.

Figures 14a and 14b show why the choice of a binary instrument—0 vs 1 vacant parcel—as opposed to a continuous variable that admits all values is a good idea. Figures 14a and 14b present the baseline number of vacant parcels within 150 meters from an existing rental building and mean tax-exempt projects in the time notch respectively. Two observations are worth noting. First, a majority of buildings in the city do not have a single vacant parcel available within 150 meters, consistent with the fact that New York City is fairly dense. Additionally, almost all buildings contained fewer than two parcels within 150 meters. Some 3% of buildings contained more than two vacant parcels. Second, Figure 14b reveals that the mean tax-exempt units received within 150 meters in the time notch jump drastically as the number of available vacant parcels increases from 0 to 1. This is equivalent to a supply shock of approximately 1%, given that an average building contained 97 rental units within 150 meters at the beginning of the reform.

Figure 15 restricts the sample to buildings with at most one available vacant parcel within 150 meters. We see that the probability a building with a vacant parcel receives a tax-exempt unit in the time notch within 150 meters is higher by 90% compared with buildings with no vacant parcel within that distance. The main IV specification uses the dummy variable that takes value 0 for buildings with 0 vacant parcels and 1 for buildings with one vacant parcel within 150 meters. This helps address the concern that the investment patterns differ starkly between buildings that had a larger number of vacant parcels nearby and buildings that had none. For example, Figure A-3.9 shows that while the total investment trends in buildings with one vacant parcel nearby are similar to those for buildings with exactly one vacant parcel in the pre-reform period, the investment patterns differ significantly for buildings surrounded by more than one vacant parcel within 150 meters.

3.2.1. Sample Construction

This requires three steps. First, I restrict the sample to rental buildings built prior to the reform period. The PLUTO data provide the building classification and geographic coordinates of each parcel in the city. Using QGIS, I draw rings of 150 meters radius around each rental building in this sample. Second, using PLUTO data, I intersect vacant parcels with the rings to identify rental buildings that contained a vacant parcel within 150 meters in 2005. [36] Finally, I restrict the sample to rings that contained at most one vacant parcel at baseline for reasons discussed above.

To measure rents, I obtain the annual market valuation, number of residential units, geographic coordinates, and other building characteristics from PLUTO for each building–year. Because the Department of Finance assesses all buildings every year, this facilitates analysis of a balanced panel of buildings, which ensures that the composition of parcels in the sample is unchanged over the years in the sample. Figure 12 illustrates the spatial distribution of rental buildings in the final sample. Figure A-3.15 shows that the market valuation of a building serves as a good proxy for rental income: The regression coefficient of (log) market valuation on (log) rental income is not significantly different from 1, and R^2 is 0.94. [37]

Furthermore, to ensure that the estimation captures spillover effects on existing rental buildings and not the mechanical effects of new tax-exempt investment, [38] I restrict the parcels to (i) non-tax-exempt rental units, (ii) rental buildings constructed prior to 2007, and (iii) parcels that are observed each year in the sample. Additionally, I drop buildings if they report residential area less than 10 sq ft at least once in the sample period, which is likely a reporting error. [39] This leads to a sample consisting of balanced panel of 26,300 rental parcels. Finally, I convert market valuations to 2015 USD using CPI tables provided by the Bureau of Labor Statistics.

3.2.2. Reduced-form Results

Figure 16 illustrates the reduced-form effect of the binary instrument—existing rental buildings with exactly one vacant parcel within 150 meters vs buildings with no vacant parcel within the same distance—on log rents. The outcome variable is the logarithm of gross rents divided by total square footage in the parcel (where I proxy the rental income by the market valuation by the Department of Finance for the year). Figure 16a plots trends in the raw data in buildings with no available land and rents in buildings with exactly one vacant parcel. We

[36] In particular, I identify parcels that were classified V (Vacant) in calendar year 2005 and project them into QGIS using parcel coordinates.

[37] This information is obtained for large rental buildings, for which information on both market valuation and rental income is made publicly available. These buildings are used to assess comparable condos and coops.

[38] Including newer units in the sample mechanically increases the rents, because new units are pricier than old units, all else equal.

[39] Rental properties are identified in the PLUTO data according to the following criteria: (i) The structure on the parcel is classified either C or D in all the years in the data. (ii) The structure on the parcel had at least 10 residential units in all the years.

see that while there are no clear pre-trends in differences between rent growth in buildings with land and without land nearby, the trends begin to diverge after 2006, which coincides with the year when the instrument—land availability—becomes active. Figure 16b plots the β_k coefficients from the following event study regression:

$$ln(r_{bt}) = \beta_0 + \sum_{k \in \{2001,2015\}, k \neq 2005} \beta_k \mathbb{1}(Vacant == 1)_p \times \gamma_k + \gamma_b + \gamma_t + \gamma_{rt} + \epsilon_{bt} \tag{6}$$

where b denotes the existing rental building, t the year, and γ_{rt} 2000 census tract–year fixed effects. $\mathbb{1}(Vacant == 1)$ is a dummy that takes value 1 if the existing building had one vacant parcel available within 150 meters at the beginning of the reform. I weight the regression by the total residential units to account for heteroskedasticity in the outcome variable (Solon 2013). A plot of rent residuals against residential units in Figure A-3.11 confirms heteroskedasticity in the rent outcome variable, which is a group average.

The estimates suggest that the rent increased by almost 10% in the time notch in buildings that had a vacant parcel available within 150 meters, compared with the buildings that had none. The lack of pre-trends provides evidence in favor of the exogeneity assumption. The rent growth exhibits dynamics whereby the initial impact is larger than the impact later. I propose two explanations. First, the majority of housing starts in the time notch came to market beginning in 2009 (Figure A-3.7), which put downward pressure on the initial rent impact. Second, the investment in affected buildings increased following the time notch rent increase, as shown in Figure A-3.10. The figure compares the trends in yearly permits issued for alterations and renovations near buildings with positive vacant parcels within 150 meters, with buildings near rental buildings with zero vacant parcels within the same distance. The 2010 spike puts downward pressure on rents by relieving downward pressure. Finally, Figure A-3.8 plots coefficients from a specification similar to 6, but the vacant parcel dummy is replaced with a dummy that takes value if the building received a project in the notch. Figure A-3.8 is the OLS equivalent of Figure 16. The OLS event study suggests that rents are higher by 5% in buildings that received a new tax-exempt project in the time notch, compared to buildings that did not receive any project.

3.2.3. Instrumental Variables (IV) Results

To capture the effect on buildings where land availability increased tax-exempt units, I calculate the IV estimate.[40] Intuitively, the IV estimate is the reduced-form effect normalized by the probability of receiving a tax-exempt unit. I measure the IV estimate using the following two-stage least squares regression:

$$ln(r_{bt}) = \alpha_0 + \alpha_1 (ExemptUnits)_b \times Post_t + \gamma_p + \gamma_t + \gamma_{rt} + \epsilon_{bt} \tag{7}$$

where the first stage is given by

$$(ExemptUnits)_b \times Post_t = \zeta_0 + \zeta_1 \mathbb{1}(Vacant == 1)_b \times Post_t + \gamma_b + \gamma_t + \gamma_{rt} + \omega_{bt}$$

where b denotes the existing rental building, r denotes the region, and t denotes the year. $(ExemptUnits)_b$ denotes the number of tax-exempt units a building receives within 150 meters in the time notch. The point estimate $\hat{\alpha}$ denotes the semi-elasticity—the percentage change in rents in response to an additional tax-exempt unit received within 150 meters from existing buildings in the time-notch. The regression is a panel IV in which inclusion of building fixed effects controls for constant unobserved differences across buildings, and year fixed effects control for time trends common to all buildings. Similar to the reduced-form specification, I weight the regression by the number of residential units in the buildings. There are 24,718 unique rental buildings containing at most one vacant parcel. In the baseline specification, I cluster standard errors at the building level and add census tract-year fixed effects. The latter allows me to compare rent growth of buildings within a same census tract and controls for any time-varying shocks at the census tract level.

The second column of Table 6 reports IV results. We see that the presence of a vacant parcel within 150-meter radius from an existing rental building increases the number of tax-exempt units within 150-meter by approximately 1. The IV estimate in Column 2 suggests that an additional tax-exempt unit in the time notch increased gross rents by around 2.3 log points. The Cragg-Donald F-stat not adjusted for clustering is 982. The Kleibergen-Paap rk Wald F statistic adjusted for clustering is 12. The Stoko-Yogo weak identification critical values for 10% and 15% maximal IV size are 16.38 and 8.96, respectively. This suggests that

[40] In the heterogeneous treatment framework, the buildings around which vacant land availability increases the number of tax-exempt units are referred to as "compliers". The IV estimate captures the local average treatment effect, which is the treatment effect on compliers.

while we reject 15% maximal bias in the IV estimate, we cannot reject 10% maximal bias. The third column reports the OLS estimate at 0.0006—much smaller than the IV estimate—which suggests the presence of a negative omitted variables bias. The last three columns of Table 6 replace the census tract-year fixed effects with Zip code-year fixed effects. We see that, understandably, both the reduced-form and the IV estimate are larger.

A usual concern in spatial estimation is that standard errors are correlated across space and clustering at the building level ignores such correlation. The standard errors clustered at the building level will be biased in the presence of meaningful spatial correlation. The direction of the bias in the standard errors depends on whether this spatial correlation is positive or negative (Conley [2016]). In Table A-3.4, I report estimates with the standard errors adjusted for both spatial correlation a la Conley [2016] and serial correlation at building level.[41] I use the distance cutoff of 1 kilometer and a uniform kernel. This implies that rent-error terms of buildings located within 1 kilometer distance from each other are assumed to be spatially correlated, and rent-error terms of buildings located farther than 1 kilometer are uncorrelated. Table A-3.4 shows that the standard errors for the reduced form effect are unchanged whereas the errors for the IV and OLS estimate are smaller when adjusting for both serial and spatial correlation.[42]

Table A-3.5 presents the elasticity estimates. The elasticity estimates are obtained by replacing $(ExemptUnits)_b \times Post_t$ by $(\frac{ExemptUnits}{H})_b \times Post_t$ in specification 7. H denotes the number of rental units within 150 meter from the building b at the start of the reform. $\hat{\alpha}$ represents the causal estimate of a 1% increase in the rental stock within 150 meters of existing buildings. p-values with standard errors adjusted for spatial (1km) and serial correlation are reported in parantheses. I also report p-values with standard errors that only account for serial correlation at the building level. The point estimate of 1.86 suggests that a 1% increase in the local rental stock in the time-notch increased rents of existing buildings by 1.8% which is significant at 1% level of significance. In contrast, the elasticity is higher at 4.95 in the specification which replaces tract-year fixed effects with Zip code-year fixed effects. The latter is consistent with the idea that treatment effects are larger when buildings in the control group are farther away from the treated group, such as a comparison of buildings within a Zip code as opposed to buildings within a census-tract.

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[41] I am grateful to Yi Jie Gwee for providing a modified version of the code written by Thiemo Fetzer and Solomon Hsiang, which calculates IV estimates adjusted for spatial correlation and admits high-dimensional fixed effects. See Hsiang [2010] for the original code.

[42] This suggests the presence of negative spatial correlation in building–rent error terms. Griffith and Arbia [2010] explain that one reason for negative spatial correlation includes local spatial competition for tenants/customers.

3.3. Robustness Checks

The main empirical strategy discussed above uses a ring of radius 150 meters around an existing rental building. Figure 17 plots IV estimates for other radii. As expected, positive spillover effects are larger when we consider investment at smaller distances from the existing rental building. Figure A-3.16 plots the marginal causal response. We see that the marginal effect decreases as we increase the radius, suggesting that the rent effects of new investment are larger when the investment is closer. Interestingly, the change in the point estimate is insignificant for an increase in the radius beyond 200 meters, suggesting that the amenity effects of a new unit are highly local.

Similarly, while the main specification uses the number of units received within 150 meters as the treatment variable, one might argue that the relevant margin is the number of *projects* as opposed to the number of *units*. Therefore, Table A-3.2 presents IV estimates when we use the alternative treatment—the number of tax exempt projects. We see that the cluster robust first-stage F-stat is larger at around 37. The IV estimate is also higher: 0.65 compared with 0.02 in the main specification. This is consistent with the fact that a project typically contains many units (Table 2 suggests that an average tax-exempt project has 16 units). Similarly, while we used a dichotomous IV with sample restricted to buildings with 0 or 1 vacant parcel within 150 meters in the baseline, it is useful to check robustness to using an IV that allows all possible values. Table A-3.3 reports estimates for a full IV. We see that the instrument is stronger with an F-stat of 15. Additionally, both the reduced-form and IV estimates are higher at 0.045 and 0.048, respectively. Broadly, the full IV supports the hypothesis that an additional tax-exempt unit in the time notch within a small distance from an existing rental building increased its rents by 2%.

Another concern is that the recession confounds the rent effect estimation. This is the case if, for instance, the probability of a foreclosure is correlated with the presence of a vacant parcel nearby. Figure A-3.12 shows the spatial distribution of foreclosures. A large share of foreclosures were concentrated in Queens and Brooklyn. I check whether the results are robust to leaving either of the two boroughs out. Table A-3.6 presents the IV estimates when we drop each borough one by one. The IV estimate is stable but underpowered when we drop Manhattan (Column 1), partly driven by the fact that the sample size is halved when Manhattan is dropped, reflected in the low KP F-stat (7.35). Nevertheless, the estimates are largely stable and significant when any of the other four boroughs is dropped (Columns 2 to 5), which suggests that it is less likely that the recession confounds the estimates. Finally, while the main IV specification includes rental buildings in both exclusion and non-exclusion regions, Table 7 presents IV estimates separately for exclusion and non-exclusion subsamples.

Because the non-exclusion regions did not receive an investment shock in this period, presence of significant rent effects in these regions casts doubts on the validity of the instrumental variables strategy. Fortunately, Figure 7 shows that while both the reduced-form and IV estimates are insignificant at the 10% significance level in the non-exclusion subsample, they are significant in the exclusion region subsample. This suggests that the rent effects are present only in the exclusion regions which received a large investment shock in this period.

3.4. Heterogeneity in Rent Effects

I perform four heterogeneity tests to better understand the drivers of the rent effect. Figure 18 estimates the IV estimate (Specification 7) separately for two subsamples in the data: below-median income census tracts and above-median income census tracts. I present 95% confidence intervals, in addition to the first-stage cluster robust F-stat with standard errors clustered at the building level. We see that the positive rent effect is driven by below-median income census tracts. In fact, the rent effect is negative in above-median income census tracts. This demonstrates that whether new investment increases or decreases local rents plausibly is a function of neighborhood income. Figure 19 tests whether the rent effect differs by the baseline building rent. No clear pattern emerges, partly because of low first-stage F-stats. Figure 20 tests whether the effect differs by baseline building age. The median age of a building in the sample is 80 years. The point estimate is negative and insignificant for older buildings (below-median age) and positive and underpowered for younger buildings. A large share of older pre-war buildings in New York City is subject to either rent stabilization or rent control which could explain why their rents do not respond significantly to new investment nearby. Note that the first-stage F-stat is low for older buildings, which suggests that they are less likely to receive a new project in their neighborhood. Finally, Figure 21 tests whether point estimates depend on homogeneity in housing quality within the census tract. Interestingly, the rent effect is driven by tracts in which housing quality is less homogeneous, as measured by the mean deviation in building rents in the tract.

3.5. Differences-in-differences Estimate of the Rent Effect

Exclusion regions received a significant investment shock in the time notch, which in principle allows us to estimate price effects by using a simple differences-in-differences (DID) strategy. A concern with this approach is that policymakers plausibly assigned those regions to treatment that were expected to gentrify in any case.[43] A simple comparison of gross rents trends in the exclusion and non-exclusion regions would then lead to overestimation of the rent effect. Nevertheless, it is informative to estimate DID effect for two reasons: (i) it allows us to corroborate the findings from the IV approach above, and (ii) we estimate the effect on other prices, such as the sales price of owner-occupied homes, and land price. [44]

Gross rent The specification for the event analysis is as follows:

$$ln(y_{brt}) = \beta_0 + \gamma_b + \sum_{k \in \{2001,2015\}, k \neq 2005} \beta_k \mathbb{1}(Region = \text{Exclusion}) \times \gamma_k + \gamma_t + \epsilon_{brt} \tag{8}$$

where b and t denote the building and year, respectively. The dummy for non-exclusion is omitted. The outcome variable is the same as in IV specification—the logarithm of building annual market valuation divided by the gross square foot residential area. The sample is similarly restricted to preexisting buildings. I omit the dummy for one year before the reform. Standard errors are clustered at the borough-year level. Figure 22a plots the differences in trends in log rental income per square foot between exclusion and non-exclusion regions, using specification 8. We see that there is a positive and significant increase in the rental income of existing buildings in the exclusion region compared with the non-exclusion region after 2006. The effect is large: about 20% five years after the reform. Pre-treatment coefficients are insignificant at 5% confidence level.

Home prices Owner-occupied homes include one/two/three unit family homes and condos/coops. To estimate the effect on prices of owner-occupied units, I use transactions data from the Department of Finance, which include the universe of property sales in New York City. I estimate the same specification as 8 in which building fixed effects are replaced by *address* fixed effects. This is because a single building has multiple owner-occupied units on its premises. I restrict sales data to properties that consists of family homes and condos/coops.[45] In addition, I restrict the sample to sales of non-tax-exempt properties. The inclusion of address fixed effects in the regression implies that the estimate β_t captures the effect on repeated sales of homes. This allows us to control for fixed unobserved characteristics of the apartment/home/building despite a smaller estimation sample. Figure 22b shows the effect on owner-occupied homes. We see that although the pre-trends are insignificant, the sales price

[43] For instance, as then-Assemblyman Voto Lopez stated, the legislature "picked areas that were being gentrified [to be included in the zone], parts of Williamsburg [for example]" (Cohen [2009]).

[44] Sales data is thin at the ring level which prevents us from implementing a similar IV strategy for sales price.

[45] Specifically, these include properties that belonged to the building classifications A, B, C, D, R, and S at the time of the sale. For buildings of type R, S, C, and D, I restrict to sales of one-unit properties.

increased in exclusion regions by around 20% after 2006.

Land price Landowners and developers, who are responsible for new residential investment, are crucial actors in the real estate industry. Obtaining the exact land price is difficult, because there are very few vacant land sales. To circumvent this issue, I use the same transactions data but restrict to sales that also transfer land.[46] There are around 101,000 sales with nonzero sale of land in the sample period. The specification with the logarithm of sale price per land square foot is as follows:

$$ln(y_{lrt}) = \beta_0 + \sum_{k \in \{2001,2015\}, k \neq 2005} \beta_k \mathbb{1}(Region = \text{Exclusion region}) \times \gamma_k + \gamma_l + \gamma_t + \epsilon_{lrt} \quad (9)$$

where γ_l, γ_t denote land parcel and year fixed-effects, respectively. I cluster standard errors at the borough-year level. Figure 22c shows the event study on the land price. We again find evidence of no significant pre-trends. While the initial effect of the reform is 20%, it increases to more than 40% seven years after the reform.

Commercial rents In addition to the users and suppliers of residential capital, suppliers of commercial capital are also affected. To estimate the effect on commercial rents, I use the PLUTO data but restrict to parcels containing only commercial space. [47] I estimate the same regression as 8, weighted by commercial units on the parcel. Figure 22d shows that commercial rents in exclusion regions increased after the tax reform in 2006. After 9 years, the commercial rents in exclusion regions are higher by about 5%, compared with non-exclusion regions.

3.6. Differences in Regression Discontinuity

One feature of the property tax reform is the presence of a clean boundary between exclusion and non-exclusion regions in Brooklyn—a major borough in the city—which can be used to perform a differences-in-regression discontinuity to provide supplementary evidence for the rent effects of new residential investment. Intuitively, this approach compares the growth in rents on either side of the boundary, where one side of the boundary is more affected by the tax-reform than the other side. An advantage of this approach is that by comparing neighborhoods close to the border but on the other side of the border, we are able to control for local economic shocks. The tax reform led to significant new tax-exempt residential investment in exclusion regions in Brooklyn. Figure 8 shows that the excess starts in Brooklyn comprised

[46] This includes sales of parcels with building classification C, D, R, S, and V at the time of sale.

[47] These include parcels of types K, O, and S.

about half of the total excess starts in the city. The stock of tax-exempt new units increased dramatically in exclusion regions in Brooklyn after the reform (Figure 26). Consistent with results in previous subsections, we expect rents to be higher in the exclusion regions in Brooklyn after the reform.

The sample is constructed as follows. I split the geographic boundary into segments and select those segments that have rental properties on either side of the boundary. I exclude boundary segments that coincide with parks (Prospect Park). Using QGIS, I draw buffers of varying distance from the boundary. For instance, Figure 23 denotes parcels in the sample within 1,300 meters from the boundary. Figure 24 plots the yearly differences in average rents in exclusion and non-exclusion regions within b meters from the boundary in which b ranges from 100 to 1,500 meters in 200 meter increments.

Figure 24 reveals that there are no significant pre-trends in the rent growth differences in the two regions at any bandwidth. Additionally, for neighborhoods within a small distance from the border, the change in rent differences between exclusion and non-exclusion regions is insignificant. This is consistent with amenity changes being uniform within small distances. The rent effects are significant for bandwidths larger than 1,100 meters. Rents are higher by 10% in the exclusion regions within 1,100 meters from the boundary after the reform, compared with non-exclusion regions 1,100 meters outside the boundary. Together, the estimates in this section corroborate the finding that a larger residential investment increased rents in exclusion regions after the reform.

4 A Mechanism: Gentrification

> *As more market-rate housing becomes available on the waterfront, current upland landlords are looking to maximize their profits. This is a situation where supply is actually driving demand. Waves of émigrés from Manhattan.*
>
> — Martin Needelman,
> Project Director of Brooklyn Legal Services Corporation A

The empirical exercise so far has yielded two key results. First, current investment responded to increases in the future property tax. Second, an additional new tax-exempt unit within 150 meters from an existing rental building increased the buildings' rent. In this section, I show that this result is consistent with the hypothesis that new residential investment has positive consumption spillovers, which allows landlords of nearby buildings to impose higher rents.

A key point is that housing is a durable good that depreciates in quality over time. All else equal, richer tenants spend more on housing, which implies that they prefer better quality housing. An increase in the market supply of new housing filters over time from high-income to low-income tenants, as new housing depreciates (Sweeney [1974]; O'Flaherty [1995]; O'Flaherty [1996]). Rosenthal [2014] documents that at any given point in time, high-income tenants tend to occupy newer housing, which provides empirical support for the presence of filtering in tenant-occupied homes. Figure 3 depicts the negative relationship between age and (residualized) rental income per square foot of buildings in 2016. Because several factors determine a building's rental income, I first obtain residuals from a regression of rents on census tract fixed effects, total units, lot area, building area, residential area, number of floors in a sample of buildings developed in the past 100 years. The R^2 is 0.7. Figure 3 plots residualized rents against age. The figure reveals that older units have lower rents, and that this negative relationship is stronger in the first 25 years of development. This is in line with Rosenthal [2014], who uses nationwide data to show that occupant income falls with the age of the rental unit. All else equal, tenants in the newer rental units pay higher rents.

Consider that the introduction of a tax-exempt property in a neighborhood has two effects, both of which move in opposite directions. First, a lower property tax on the new properties creates standard downward pressure on rents of nearby properties and increases gross rents from r_T to $\bar{r}$ (Figure 25). I refer to this as a *supply effect*. Second, the introduction of a new project increases the amenity value of the neighborhood. This has two parts: New development directly improves the amenity value of the neighborhood by increasing the aesthetic appeal, and indirectly it increases consumption amenities by attracting affluent tenants. These two channels together form the *amenity* effect. In contrast to the competition effect, which leads to movement along the supply curve, the amenity effect shifts up the demand curve (see Figure 25), leading to an increase in rents. The net effect on rents depends on which effect dominates.

As a motivation for empirical exploration of the mechanisms, consider the effect on two types of businesses in the Brooklyn following the tax reform. The second panel of Figure 26 shows that the stock of new tax-exempt housing drastically increased in the exclusion regions in Brooklyn following the tax reform in 2006. The second row of Figure 26 plots the flow of new businesses in the exclusion and non-exclusion regions within Brooklyn in this period. We see that while there is a stark increase in the number of new sidewalk cafés (associated with high-income customers) in the exclusion regions after the reform, the number of new laundromats (associated with low-income customers) declined. This is consistent with the hypothesis that new residential investment attracts local businesses that cater to more affluent households. The following sections explore consumption amenities and demographics changes more rigorously.

4.1. Evidence 1: Demographic changes

A key channel by which new residential investment affects amenities is through changes in the average neighborhood income level. In this subsection, I present demographic changes in the neighborhoods that received residential investment in the time-notch. The lowest available geographic level for such data is census tract at 2000 boundaries. The data come from the 2000 Census and 5-year estimates from the 2005-09 and 2013-17 American Community Survey (ACS) and.[48] I obtain information on a host of outcomes such as rent, income, race, education, and age of the household head. Details of data construction can be found in Appendix A-2. The estimating equation is

$$log(Y_{rt}) = \beta_0 + \beta_1 \mathbb{1}(ExemptProjects \geq 1)_r \times Post_t + \gamma_r + \gamma_t + \epsilon_{rt} \tag{10}$$

where $\mathbb{1}(ExemptProjects \geq 1)$ is a dummy that takes value 1 if the number of tax-exempt project starts in the census tract r in the time notch were non-zero; Y_{rt} denotes the outcome variable; γ_r and γ_t denote census-tract and year fixed effects, respectively. The coefficient of interest here is β_1, which I estimate using an IV approach. Specifically, I instrument $\mathbb{1}(ExemptProjects \geq 1) \times Post$ with the $(VacantParcels) \times Post$. $(VacantParcels)$ denotes the total number of vacant parcels available in the census tract at the start of the reform. $Post$ takes value 1 when observation belongs to ACS 2005-09 and ACS 2013-17, and zero for Census 2000. I cluster standard errors at the census-tract level, which is level of the treatment.[49]

Table 8 reports IV estimates. We see that the census tracts that received at least one tax-exempt project in the time-notch have 763 additional occupied units after the tax reform (Column 1), the median rent is higher by 33% (Column 2), and the income is higher by 24% (Column 3). The number of tenants with at least a Bachelor's degree is higher by 20 percentage points (Column 4), the number of white households is higher by 17 percentage points, and the number of young renter households is higher by 7 percentage points (Columns 5 and 6). The heteroskedasticity robust Kleibergen-Paap F-statistic is 20.78, which rejects the weak IV test.

[48] 2005-09 ACS represents an average over the 2005-09 period.

[49] I use the treatment variable —the number of *projects*–– as opposed to the number of *units*, because the instrument is a weak predictor of the number of units at the level of the census tract.

4.2. Evidence 2: Changes in Amenities

This section provides direct evidence on the entry of businesses that cater to the demands of high-income tenants following new residential investment in the time notch. The main dataset I use comes from County Business Patterns (CBP) data. This dataset provides Zip code-level counts of establishments at 6 digit NAICS level.

I estimate how the increase in the log establishments varies by industry-level income elasticity. The income elasticity captures how likely it is that a particular industry caters to high-income demand. To obtain this income elasticity, I regress the log number of establishments at 4-digit industry level in a Zip code in 2001 on mean Zip code renter income in 2000, by each 4-digit NAICS industry. This produces approximately 300 industry income elasticities. A high income elasticity suggests that the industry caters to high-income tenants, which is reflected in its high concentration in high-income Zip codes in 2000. I order industries by income elasticity into deciles. We expect the effect on establishments to be higher for industries with high income elasticity. Figure A-3.8 lists selected industries with high and low income elasticities. Some investment services such as NAICS 5231, 5239, and restaurants/ drinking places have high income elasticity whereas manufacturing such as NAICS 3323, ship and boat building NAICS 3366 have low income elasticity. I estimate the following specification, separately for each decile.

$$log(N_{irt}) = \beta_0 + \beta_1 \mathbb{1}(ExemptProjects \geq 1)_r \times Post_t + \gamma_r + \gamma_i + \gamma_t + \epsilon_{rt} \tag{11}$$

where $\mathbb{1}(ExemptProjects \geq 1)$ is a dummy that takes value 1 if the Zip code r received at least one tax-exempt project in the time notch. The outcome variable is the number of establishments in industry i in region r in year t. I instrument $\mathbb{1}(ExemptProjects \geq 1) \times Post$ with the $(VacantParcels) \times Post$. This specification is similar to the one used for demographic outcomes. $Post$ dummy takes value 1 for all years after 2006. The sepecification includes region, industry and year fixed effects. Standard errors are clustered at Zip code level.

Figure 27 shows that the first stage exists when we consider Zip codes as a unit of geography. The figure plots the average number of tax-exempt projects received by a Zip code in the time notch against the baseline number of vacant parcels. Zipcodes with a higher vacancy at the time of the reform received a larger number of projects in the time notch. Figure 28 plots the trends in the log number of establishments in the high-land Zip codes (Zip codes with a baseline number of vacant parcels above 75th percentile, and hence expect to receive larger investment in the time notch) and low-land zipcodes (Zip codes with a baseline number of vacant parcels below 25th percentile, and hence expect to receive smaller investment). We see that trends for high-land Zip codes begin to diverge, particularly after 2006, for industries such as retail/wholesale and restaurants, which coincides with the time notch.

The IV estimates in Figure 29 confirm that residential investment increased establishments in industries more likely to locate near high-income tenants. The x-axis denotes the industry income elasticity decile. Point estimates are insignificant for industries with below-median income elasticity and significant and positive for above-median income elasticity, consistent with the hypothesis that higher residential investment increased establishments in industries that are more likely to locate near high-income tenants. Finally, the first-stage is stable for each decile subsample.

5 Discussion

This section uses reduced-form estimates to provide back-of-the-envelope calculations for welfare changes for local high- and low-income tenants, building owners, local government, and total efficiency gains as a result of investment in the time notch. Time notch is equivalent to a temporary reduction in property taxes on new units in the time notch in exclusion regions. The estimates provided in this section do not incorporate city-wide aggregate welfare changes.

5.1. Incidence of Tax-exempt Investment in the Time-notch

I use a simple spatial equilibrium model with homogeneous preferences across regions but that is hetereogenous across income types. I make the following assumptions: (i) utilatarian aggregate welfare function; (ii) perfect competition and free entry among developers and providers of amenities; and (iii) the social marginal cost of government funds is equal to 1. Additionally, for simplicity, I assume there are two types of tenants, high-income (h) and low income (l). The mean utility of tenant of income type k in region j is given by

$$V_j^k = w^k + \alpha^k A_j - r_j^k$$

where $j \in e, n$ denotes exclusion/non-exclusion region and $k \in h, l$ denotes high-income/low-income. r_j^k denotes the tax-inclusive rents paid by tenant of income type k in region j. Unless specified, I suppress the subscript j to refer to exclusion regions. The average wage is independent of location choice which is a reasonable assumption in the current setting.[50] The

[50]Tenants can commute to any neighborhood within the city. In contrast to the models in which tenants

relative valuation for amenities differs between high- and low-income tenants. For instance, it is reasonable to assume that $\alpha^h > \alpha^l$. That is, high-income tenants have greater preference for local amenities than low-income tenants. For simplification, I normalize $\alpha^h = 1$, which implies $\alpha^l < 1$.[51]

Tenants: The effect of the time notch on high-income tenants in exclusion regions is given by

$$\Delta V^h = N_t^h(\underbrace{-\Delta r^h}_{\text{Rent change}} + \underbrace{\Delta A}_{\text{Amenities change}})$$

where N_t^h denotes the number of high-income tenants in the exclusion region before the reform. Thus, to the first order, the impact of a tax cut on new investment in region t equals the impact on gross rents and amenities (weighted by the tenant's preference parameter) times the share of high-income tenants residing in region e. The first term captures the change in high-income welfare due to a change in gross rent. The second term captures the change in their aggregate welfare due to amenity changes. When amenities are fixed, the first term is positive while the second term is $\Delta A = 0$ and high-income welfare necessarily increases following a property tax cut.

When there is infinite mobility and homogenous preferences within an income group, any rent increases in the exclusion regions must be compensated by an equivalent amenity increase to make a marginal high-income tenant indifferent between exclusion and non-exclusion regions.[52] In that case, a change in gross rents reflects changes in amenities (i.e., $\Delta r^h = \Delta A$), and thus the net gains to high-income tenants is zero.

Low-income tenants are affected by the changes in local amenities and changes in rents, if any. The effect on low-income tenants' welfare (in exclusion regions) is given by

$$\Delta V^l = N_t^l(\underbrace{-\Delta r^l}_{\text{Rent change}} + \underbrace{\alpha^l \Delta A}_{\text{Amenities change}})$$

choose to locate across cities and city-specific productivity differences are important, this is not a relevant factor in this case, where the analysis is within city.

[51]Note it is possible that $\alpha_l < 0$, which is the case when low-income tenants have a strong dislike for local consumption amenities. Otherwise, a more sensible approach is to assume $\alpha_l \in [0, 1]$. This implies that in general, low-income tenants' valuation of local amenities is a fraction of high-income tenants' valuation with the extreme case of $\alpha_l = 0$, in which they do not care about such amenities at all.

[52]Another interpretation is that the change in rent on high-income units is entirely driven by changes in consumption amenities, when the supply effect on high-income units due to additional new units in the neighborhood is minimal.

where r^l denotes the tax-inclusive rent on the units occupied by low-income tenants. Low-income tenants benefit from amenity changes if and only if they place strictly positive valuation on them. In the extreme case in which $\alpha^l = 0$, which happens when the new amenities include sidewalk cafes and art galleries—which these tenants do not value—low-income tenants are strictly worse off following the tax change, because their out-of-pocket rent cost increases. To provide an upper bound on low-income welfare loss, I assume $\alpha_l = 0$ and that they are completely immobile.[53] The effect of the investment in the time notch on low-income tenants' welfare is given by $\Delta V^l = \tau^l N_t^l \Delta r = 1.15 \times 521,748 \times \Delta r^l = 521,748 \times 1,050 = -\$547mn$. [54] [55]

Owners of rental buildings: There are two kinds of owners. One group owns low-income rental units, and therefore obtains net-of-tax rents r^l. The other group owns high-income rental units, and obtains rents r^h. The total per annum profit accruing to owners is given by

$$V^O = r^h N_h + r^l N_l - C_h - C_l$$

where C_i denotes the fixed cost of maintaining high-/low-income units. Assuming that these costs are unaffected by the time notch, the effect of a tax cut on owners' welfare is given by $\Delta V^O = N_h \Delta r^h + N_l \Delta r^l$. Continue to assume $\Delta r^h = \Delta r^l$, $\bar{H} = N_h + N_l$; then the welfare gains to the owners are $\Delta V^O = \bar{H} \Delta r^h = 1043496 \times 1050 \approx \$1b$.

Local government: Another component of the welfare includes the government. Increased construction activity has two effects on government. On the one hand, government loses property tax revenues on exempt units started in the time notch. On the other hand, the addition of new units to the rental stock has positive spillovers to rents of existing units because of higher rents, which increases the tax base for the government. [56]

[53] For instance, low-income tenants occupy rent-stabilized units and moving implies that they lose that status, or credit constraints prevent low-income tenants from paying fixed costs of moving.

[54] Using a capitalization rate of 6, a property tax rate of 2.5% on the assessed valuation is equal to a tax rate of 15% on the rental income.

[55] Where I have used the higher of the two estimates in Table 6, which allows us to calculate the upper bound of the welfare losses to low-income tenants. The average *monthly* rent in exclusion regions at the start of the reform was $1,462 (Table 3). A 6% increase in rents suggests that the tenants' annual rent payments increased by $1,050 a year. Table 5 suggests that there existed 1,043,496 units in 2005. I assume that half of the total rental stock was occupied by low-income (or below median income) tenants. This is confirmed in Table A-3.7, which shows that the number of low-income tenants was fairly close in exclusion and non-exclusion regions at the start of the reform.

[56] I continue to assume an equal increase in rent of high-income and low-income tenants.

$$\Delta R = \underbrace{-t_h \Delta \bar{H} r^h}_{\text{Revenue lost}} + \underbrace{t_h \Delta r \bar{H}}_{\text{Revenue gains}}$$

The revenue gains to the government, therefore, are $\Delta R = -0.15 \times 17,520 \times 10,735 + 0.15 \times 1,043,496 \times 1,050 = 164mn$, which is around 2.4% of annual city real property tax revenues from rental units. [57]

5.2. Efficiency Effect of the Time-notch

As pointed out by Kleven [2018], for small tax reforms and utilitarian aggregate welfare function, the difference between total and mechanical changes in tax revenues is a sufficient statistic for measuring efficiency loss (or gain in the case of the tax cut), in the absence of externalities. With externalities, as in our case, we add an extra term that corresponds to the benefit accruing to high-income and low-income tenants because of larger amenities due to the tax reform. The aggregate efficiency gains consists of two parts:

$$\Delta W = \underbrace{t_h \tilde{r^h} \Delta \bar{H}}_{\Delta \text{Housing supply}} + \underbrace{(N_t^h + \alpha^l N_t^l) \Delta A}_{\Delta Amenities}$$

The efficiency gains come from two sources: (i) the tax-exemption on new investment stimulates construction and reduces the deadweight loss associated with the property tax; and (ii) new investment increases economic activity and consumption amenities. The increase in aggregate welfare depends on the weight placed on the amenities by low- and high-income tenants. Without knowledge of preference parameters for amenities, we can not calculate total efficiency change. However, when $\alpha^l = 0$, we can easily use the empirical estimates to obtain the efficiency estimates. Note that annual $\tilde{r^h} = \$17,520$ (USD 2015), $\Delta \bar{H} = 10,735$, $\Delta A = \Delta \tilde{r^h}$ we get:

$$\Delta W = 0.15 \times 17,520 \times 10,735 + 521,748 \times 1,050 \approx \$576 million$$

Given that the New York City government collected around \$6.8 billion (USD 2015) in property taxes from an equivalent tax class in 2006, the aggregate annual efficiency gains from the time notch correspond to 0.5 billion dollars, which is equivalent to about 7.3% of total revenues. Note that back-of-the-envelope calculations in this section use local-reduced

[57] The New York City government collected around \$6.85 billion (USD 2015) in property taxes from class 2 properties in 2006; see Furman [2011], page 9. Total revenues in 2006 (\$16 b) are multiplied by 0.36, the share of class 2 properties in total revenues (page 10), which is then further multiplied by 1.19 to convert into 2015 USD.

form estimates and do not incorporate any general equilibrium changes.

6 Conclusion

This paper estimates the local effects of property tax incentives for new construction. I leverage the New York Ciy property tax reform of 2006-08, which reduced tax benefits on new investment. The delay between announcement and implementation led to a time notch in which investment before the deadline was associated with a lower property tax, compared with investment just after the deadline. I exploit the bunching of tax-exempt projects in the notch period in a bunching framework to estimate the elasticity of current residential capital with respect to future property tax. I find that a 1% increase in future property tax increased current residential rental stock by 0.4%.

Importantly, I find that an additional new tax-exempt rental unit within 150 meters in the time notch from an existing rental building increased its gross rental income by 2.3%. I hypothesize that the positive effect on gross rents can be explained by the fact that new tax-exempt investment attracted high-income individuals who increased the average income of the neighborhood, boosted local businesses, and therefore further increased the amenity value of nearby rental buildings. Through event studies and an instrumental variables approach, I document that changes in demographic composition and business composition are consistent with the above hypothesis. Residents are more educated, white, and richer in regions that received larger investment after the reform. Changes in business composition are consistent with the improved amenity hypothesis. The number of sidewalk cafes (associated with high-income consumers) increased after the reform in regions with larger investment shock while laundromats (associated with low-income consumers) declined.

The papers' results have policy implications. They suggest that rent effects at the local level do not necessarily mirror aggregate effects. That said, future work will explore the aggregate effects of new residential investment. A first step in that direction is to estimate the displacement of existing low-income tenants in exclusion regions and neighborhood changes in regions of origin of tenants in new buildings developed in the time notch.

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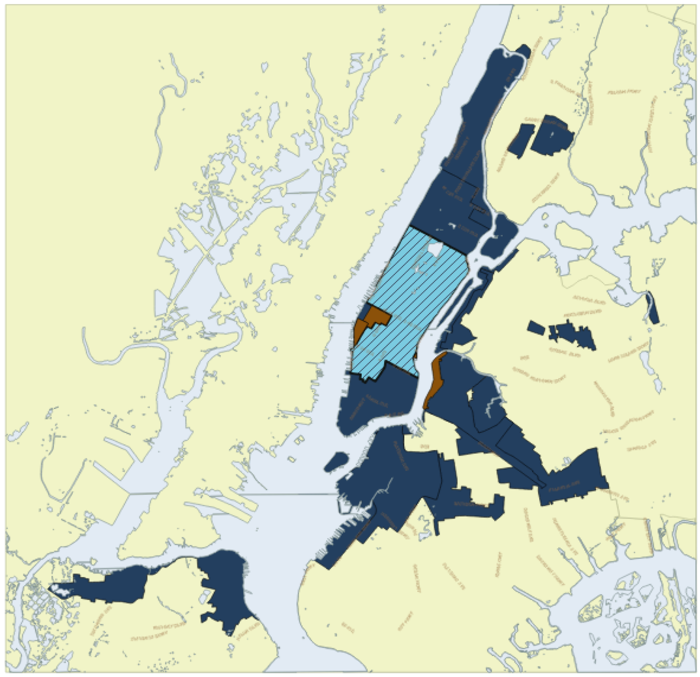
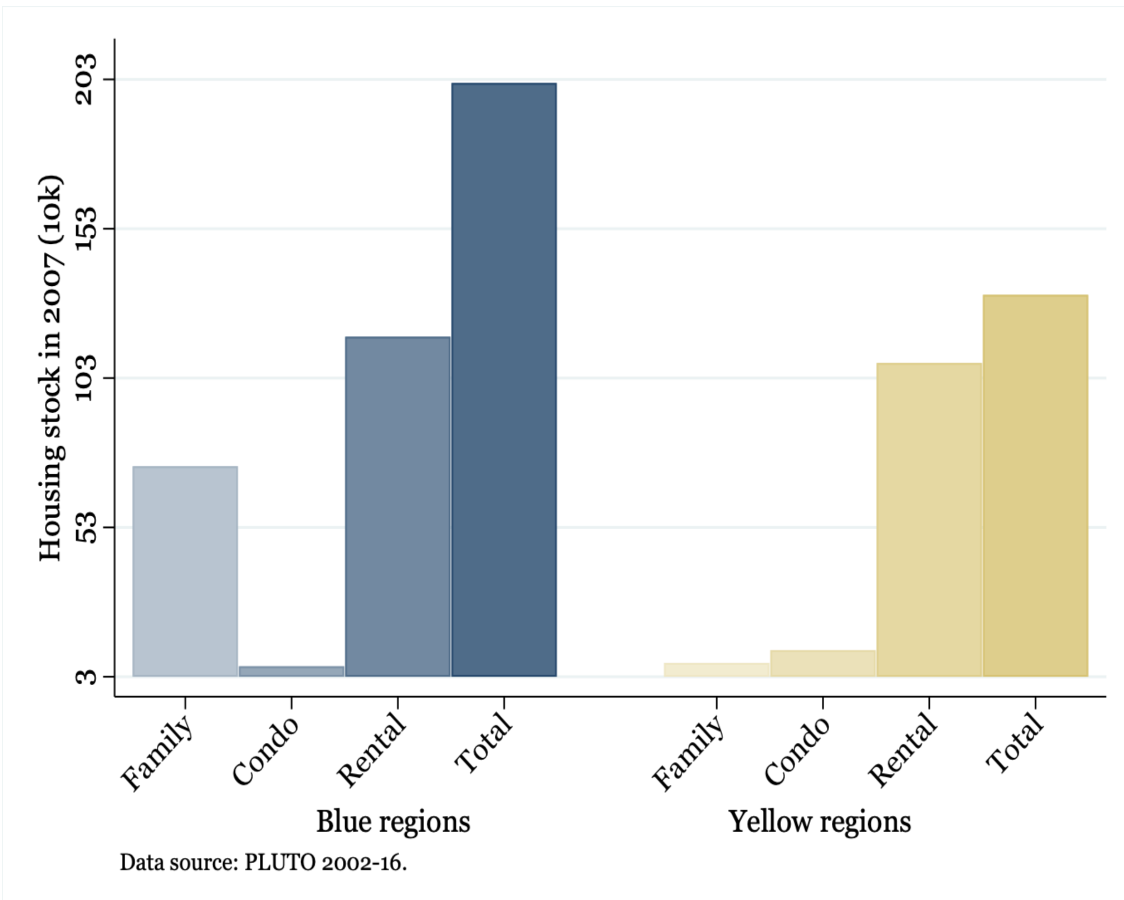
7 Figures and Tables

Figure 1: 421a Property tax reform 2006-08



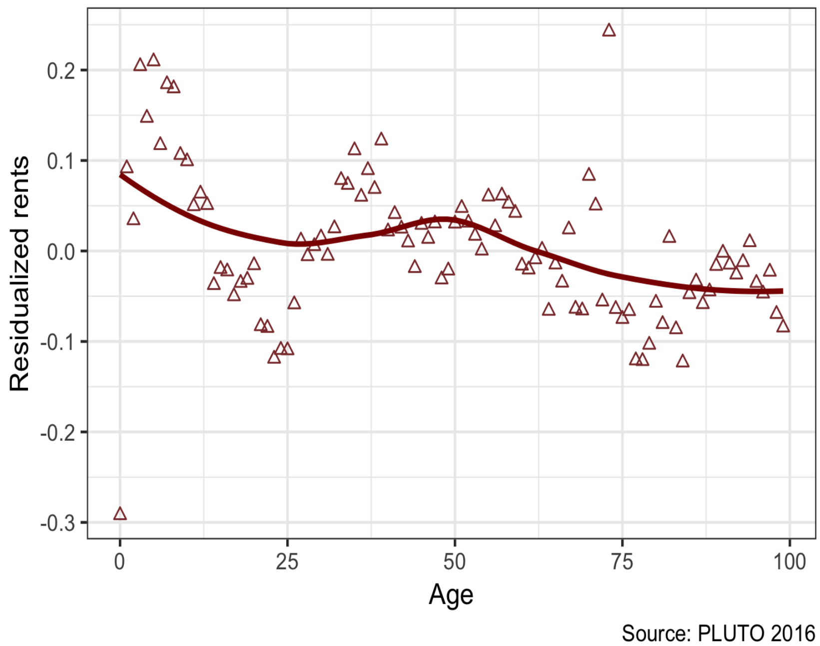
Notes: The figure shows the changes in the "Exclusion region" that were part of the 421a property tax exemption 2006-08 reform. The light blue shaded region indicates the original exclusionary region, where prior to December 2006, developers were required to provide affordable housing, either offsite or onsite to be eligible for 421a benefits. The dark blue unshaded region indicates newly added excluded regions. After 2008, developers in new regions could only obtain long exemption, which requires provision of onsite affordable housing. In addition, 421a developers in the old exclusion region must provide affordable onsite, a more expensive requirement. Together, light and dark blue regions formed the 'exclusion regions' and were to see a loss of generous tax benefits beginning 2008. Note that the brown regions in Manhattan and Brooklyn represent 'Hudson Yards' and 'Williamsburg' region respectively where special provisions applied.

Figure 2: Distribution of housing stock type in New York City in 2007



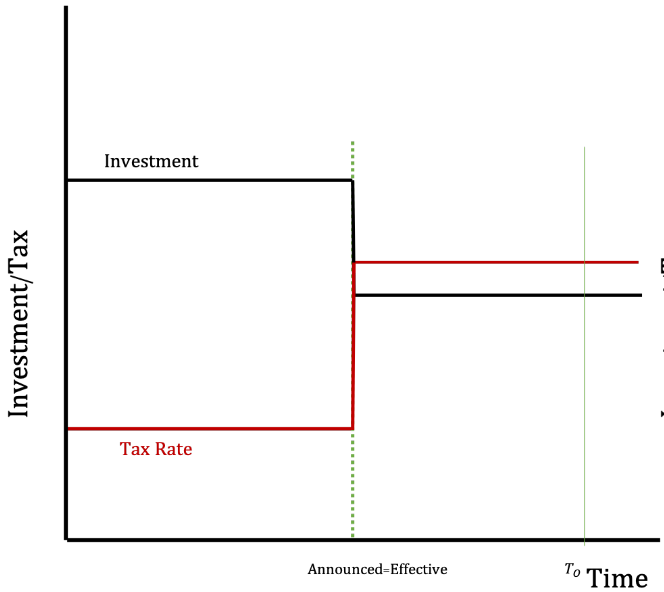
Notes: This figure plots the distribution of the housing stock in exclusion and non-exclusion regions in 2007—a year before the implementation of the tax reform. Data: Primary Land Use Tax Output, 2002-16.

Figure 3: Correlation between age and rent of a housing unit

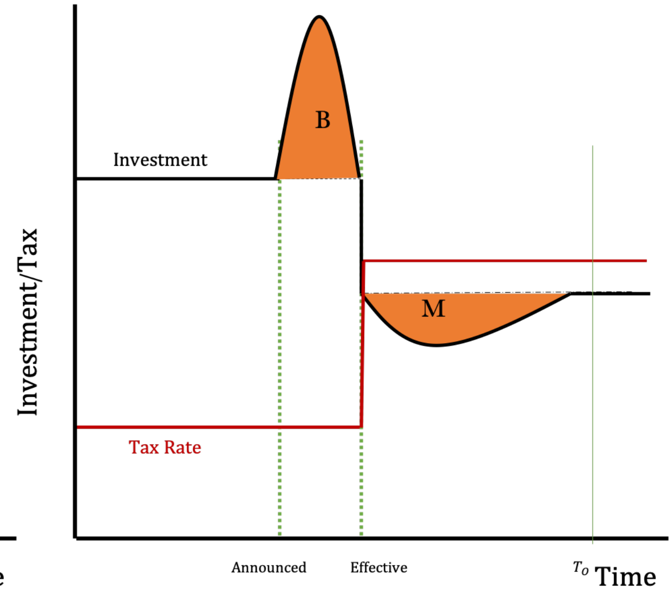


Notes: This figure plots the relationship between the age of a rental unit and its rent price in New York City. Sample includes cross-section of parcels in 2016 and developed in past hundred years. The thick line denotes the lowess fit obtained through a regression of residualized rent on age. Triangles denote average residualized rent for each discrete value of age variable. Residualized rent includes residuals from a regression of rent on total units in the building, census tract fixed effects, lot area, building area, residential area, number of floors and number of buildings on the parcel. Data: Primary Land Use Tax Output 2016.

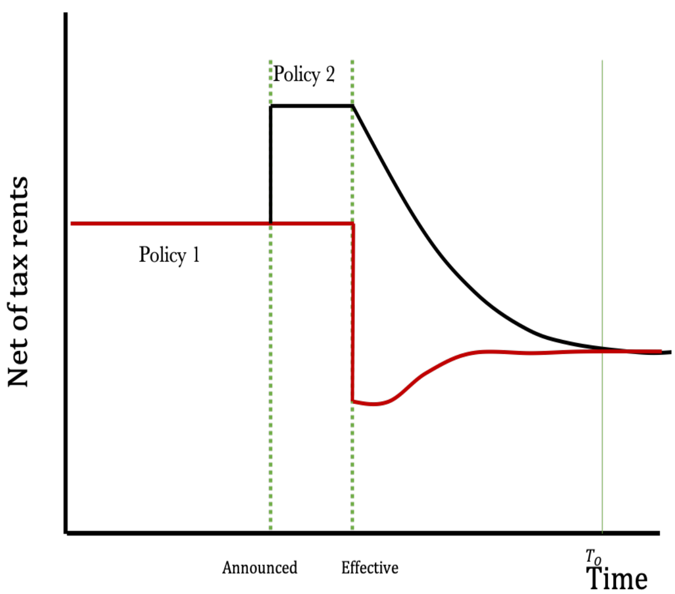
Figure 4: Effect of Time Notch on Short Term and Long Term Outcomes



(a) Policy 1: Unanticipated Tax increase



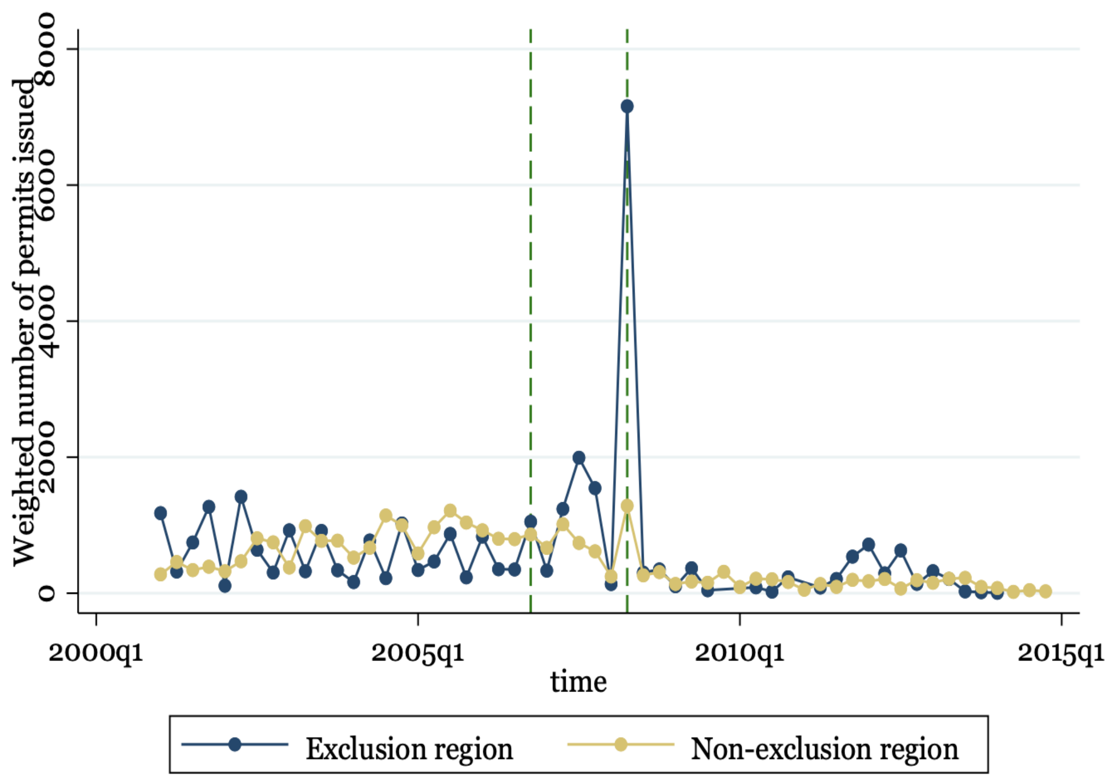
(b) Policy 2: Anticipated Tax increase



(c) Rents path under the two policy scenerios

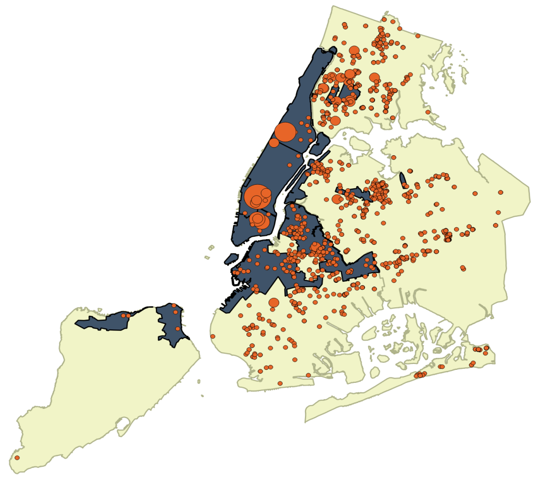
Notes: This figure illustrates how the short term outcomes, as defined by the net-of-tax rents, differ under the two policy scenerios: Unanticipated property tax increase (Policy 1), and anticipated property tax increase (Policy 2). The long term outcome, determined by long run amenity and housing supply elasticity is the same under the two scenerios. The long run elasticities are such that the net rents decline. In Policy 1, the landlord rents decrease but slowly increase to the long-run value as lower investment allows them to shift some burden onto the tenants. In contrast, in Policy 2, the effect on rents is different due to a short term increase in new residential investment following the tax-reform. The figure illustrates the case when the amenities are sufficiently elastic so that the net-of-tax rents increase. These trends are reversed in the long-run where the time it takes to reach the long run depends on how long it takes the housing to depreciate.

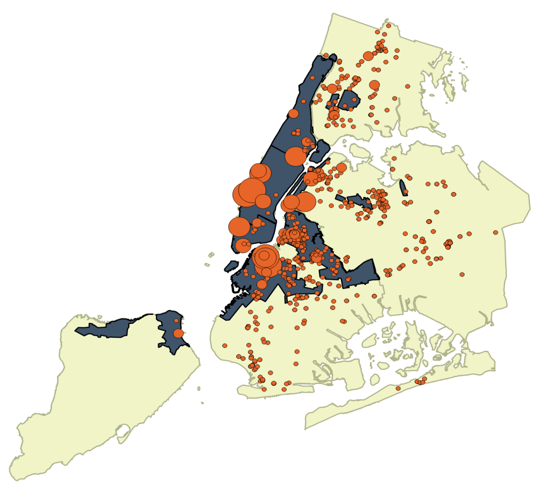
Figure 5: Aggregate quarterly tax-exempt housing starts in New York City, blue and yellow regions



This graph plots number of permits issued for a tax-exempt project (C or D) weighed by residential units in either region. The two green lines denote the quarter-year when the law was signed and became effective respectively. Data includes permits for new building construction (NB).

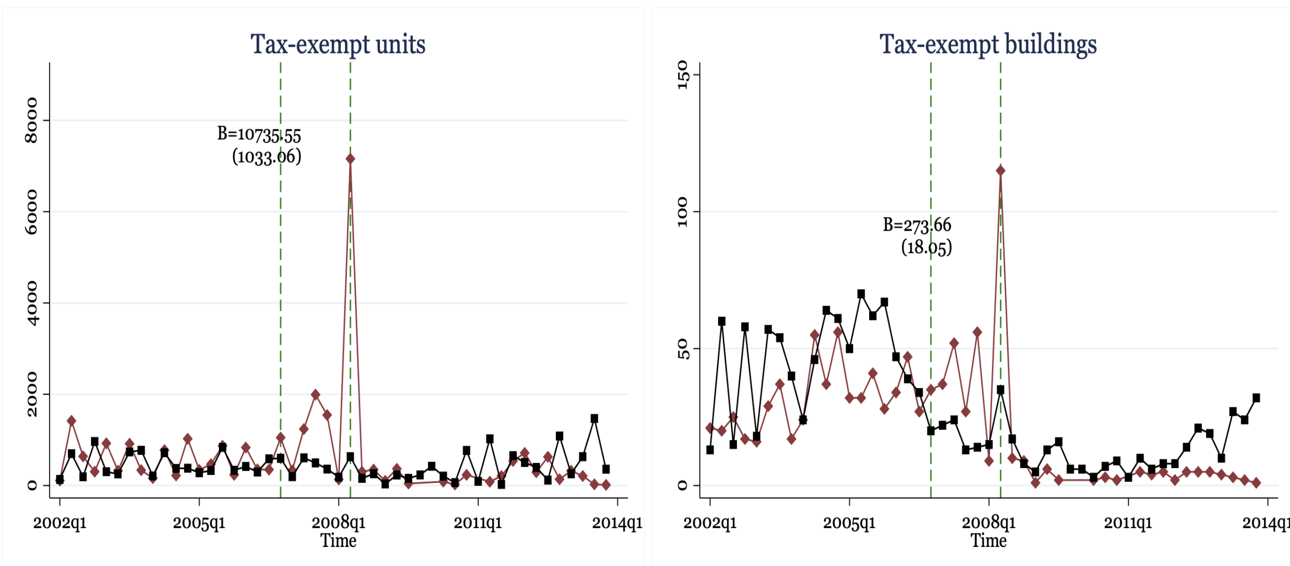
Figure 6: Spatial distribution of Tax-exempt Projects in the Time notch





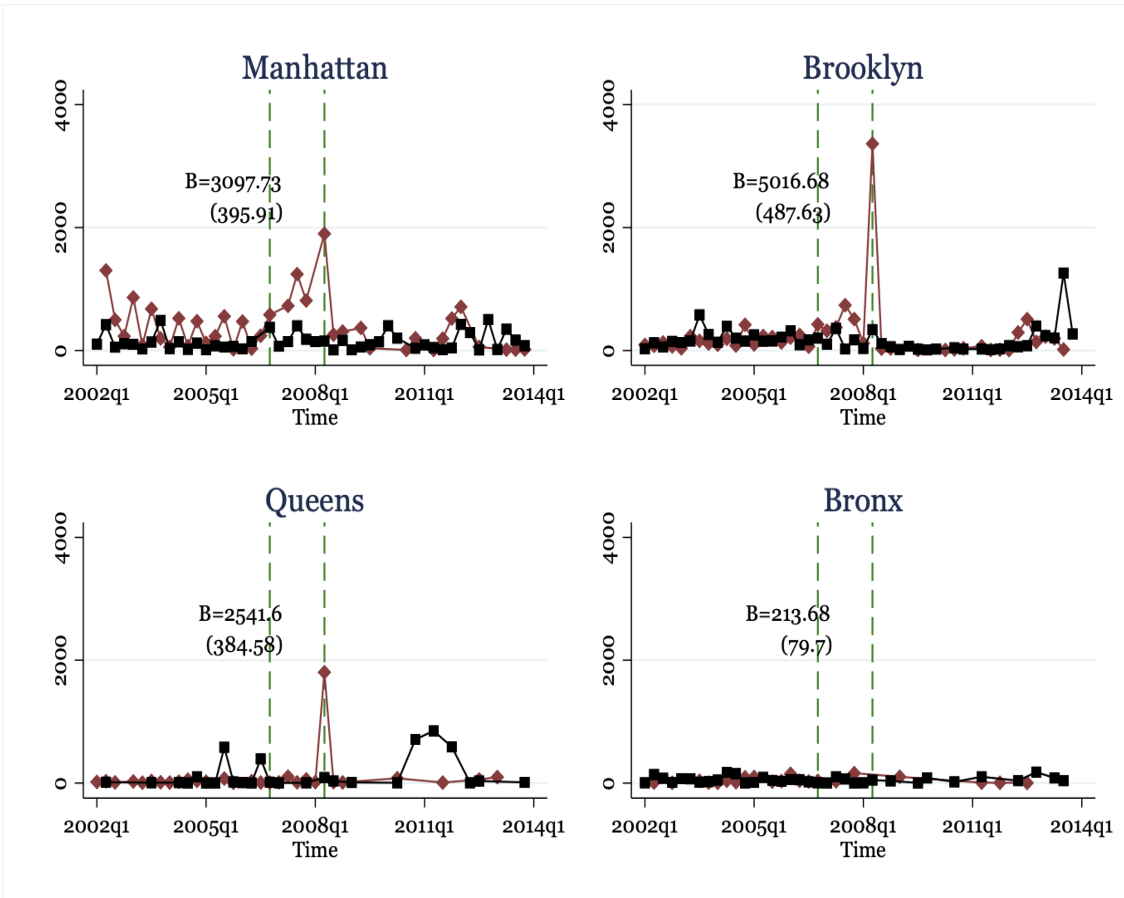
Notes: This figure presents the spatial distribution of tax-exempt project starts before and in the time-notch. Each project is weighed by the total number of residential units built. The top panel includes permits issued during 2005q2 to 2006q4. The bottom panel includes permits issued during 2006q4-2008q2 (the time notch). Data: Department of buldings permits, 2001-15 and Primary Land Use Tax Output 2002-16.

Figure 7: Bunching estimation of tax-exempt housing unit starts



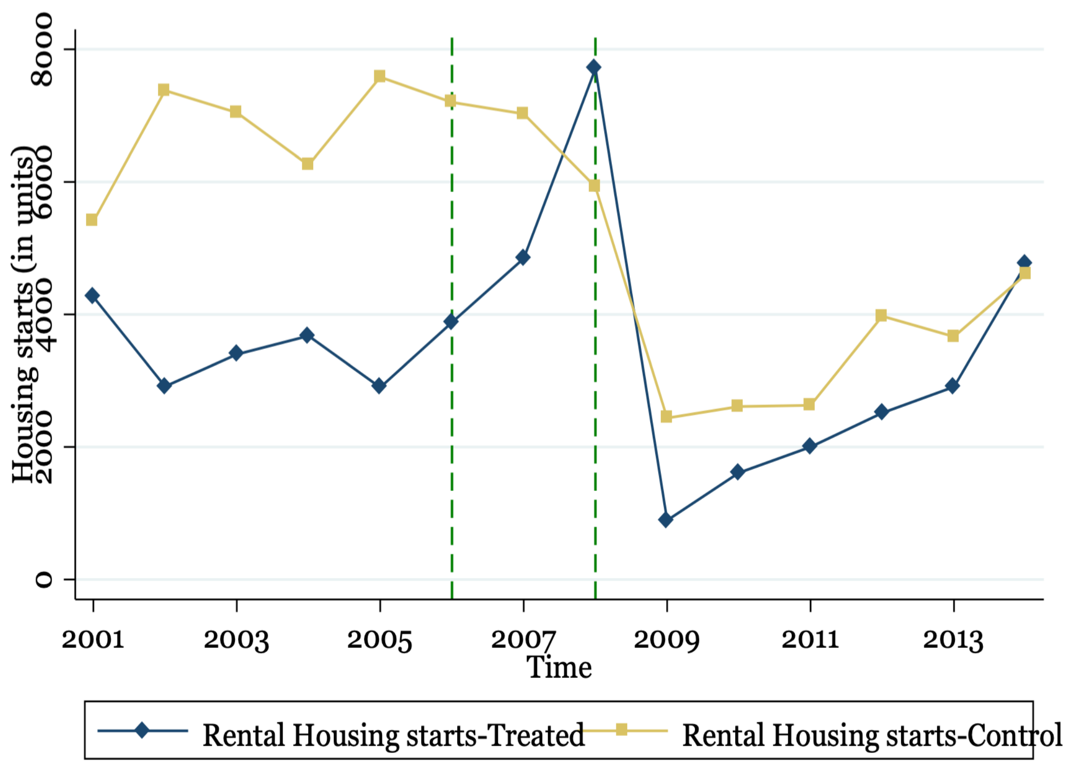
Notes: Diamonds denote the empirical distribution of tax-exempt housing starts. Squares denotes the counterfactual: empirical distribution of non-tax-exempt housing starts in the blue region. Data: Department of Buildings permits, 2001-15.

Figure 8: Bunching of Tax-exempt housing starts in exclusion regions, by boroughs



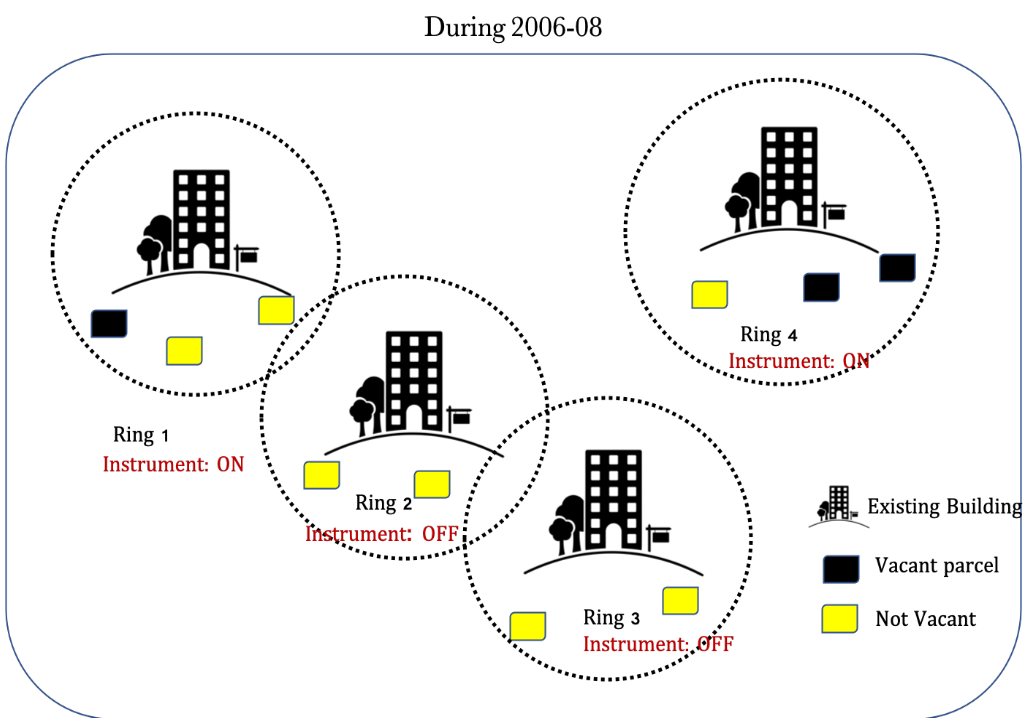
Notes: Diamonds denote the empirical distribution of tax-exempt housing starts. Squares denotes the counterfactual: empirical distribution of non-tax-exempt housing starts in the exclusion region. Data: Department of Buildings permits, 2001-15.

Figure 9: Effect on aggregate housing starts in exclusion vs non-exclusion regions



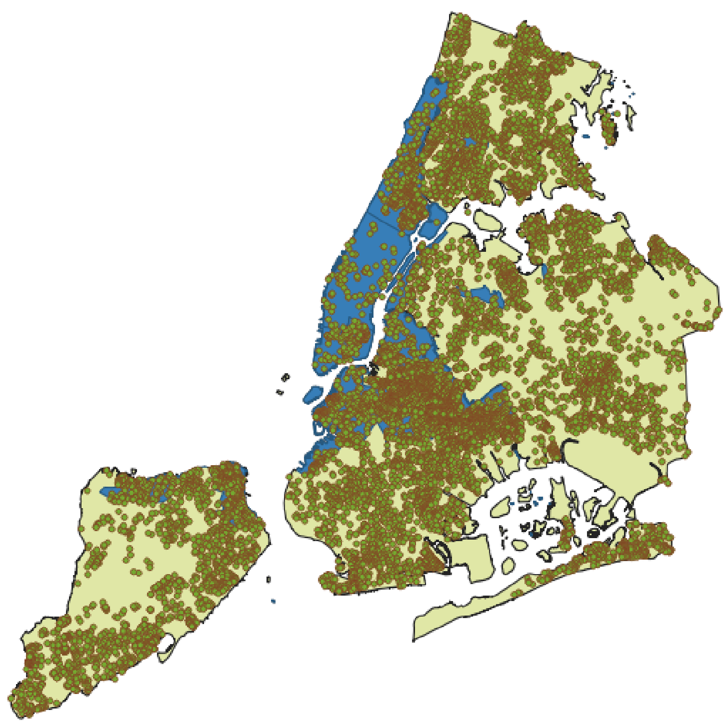
This graph plots yearly rental units built in treated vs control. Data includes permits for new building constructio (NB) for buildings of types C and D.

Figure 10: Empirical design to estimate the effect of the 2006-08 Tax reform on the Rents



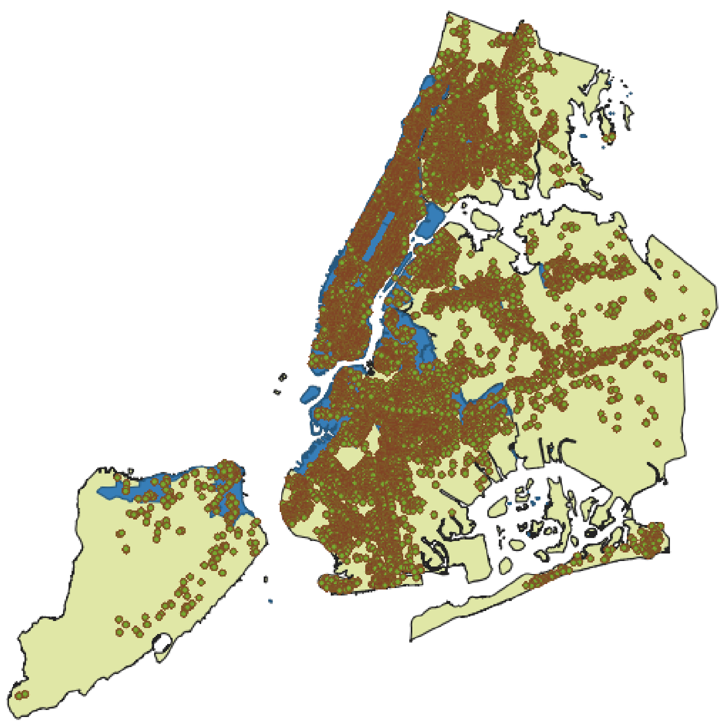
Notes: This picture highlights the main empirical strategy to estimate the effect of new residential investment on existing building rents. The availability of a vacant parcel within 150 meters radius from a building acts as an instrument for the magnitude of investment received by an existing building in the time-notch. Time notch refers to a period between announcement and implementation of property tax increase on new residential projects.

Figure 11: Spatial distribution of Vacant Parcels in 2005



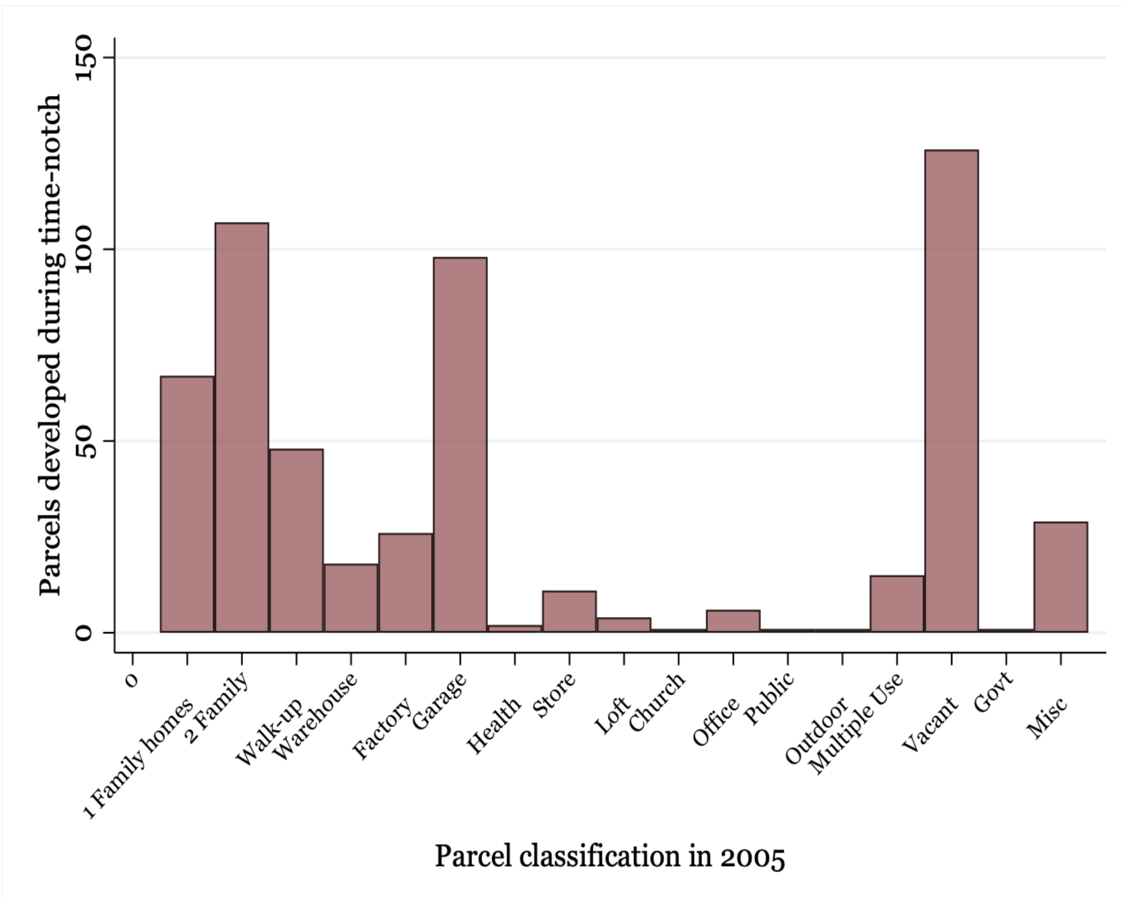
Notes: This figure shows the spatial distribution of vacant parcels across New York City in 2005. A parcel is classified 'vacant' if it was classified as 'V' in 2005. The reform effectively increased property tax on new units in the exclusion (shaded blue) regions beginning July 2008.

Figure 12: Spatial distribution of Existing Rental Buildings in 2005



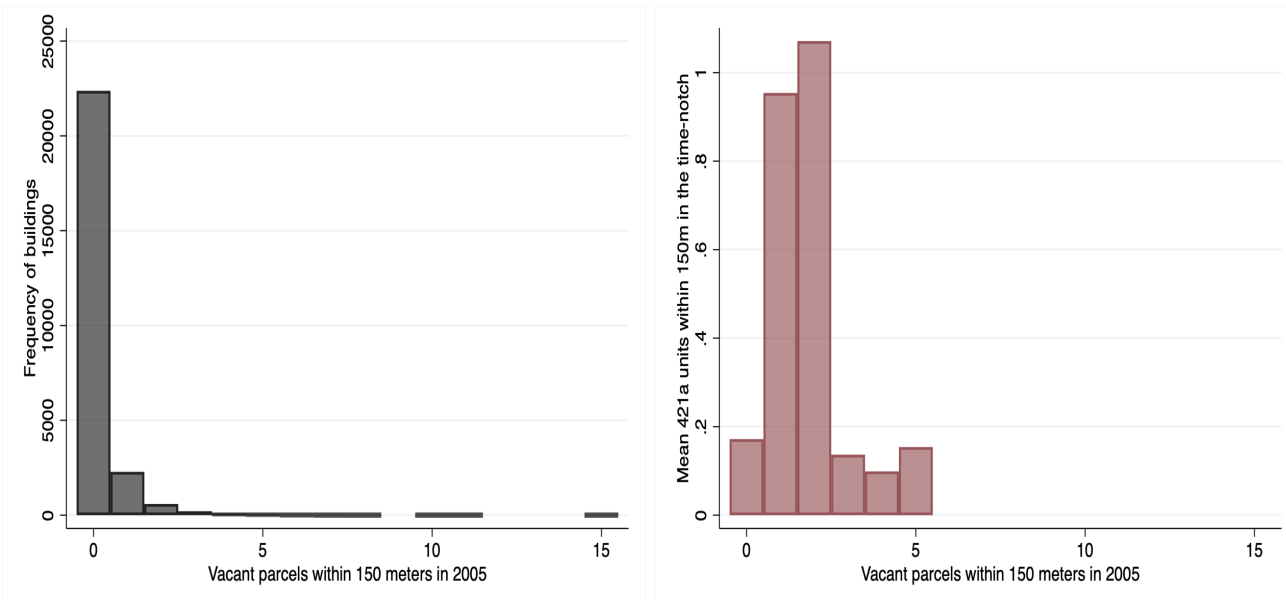
Notes: This figure presents the spatial distribution of existing rental buildings in the sample. The sample includes 25,000 buildings observed for 15 years.

Figure 13: Classification of parcels developed in the time notch



Notes: This figure shows the distribution of the types of parcels that were developed in the time-notch, that is, during 2006-08. Data: Primary Land Use Tax Lot Output, 2002-16 and Department of Finance 421a Exemption list 2015.

Figure 14: First-stage

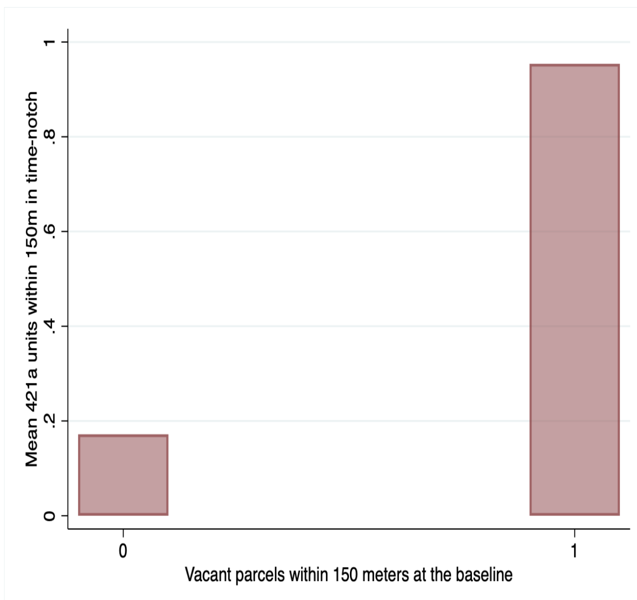


(a) Vacant parcels at baseline

(b) Mean tax-exempt units during the time-notch

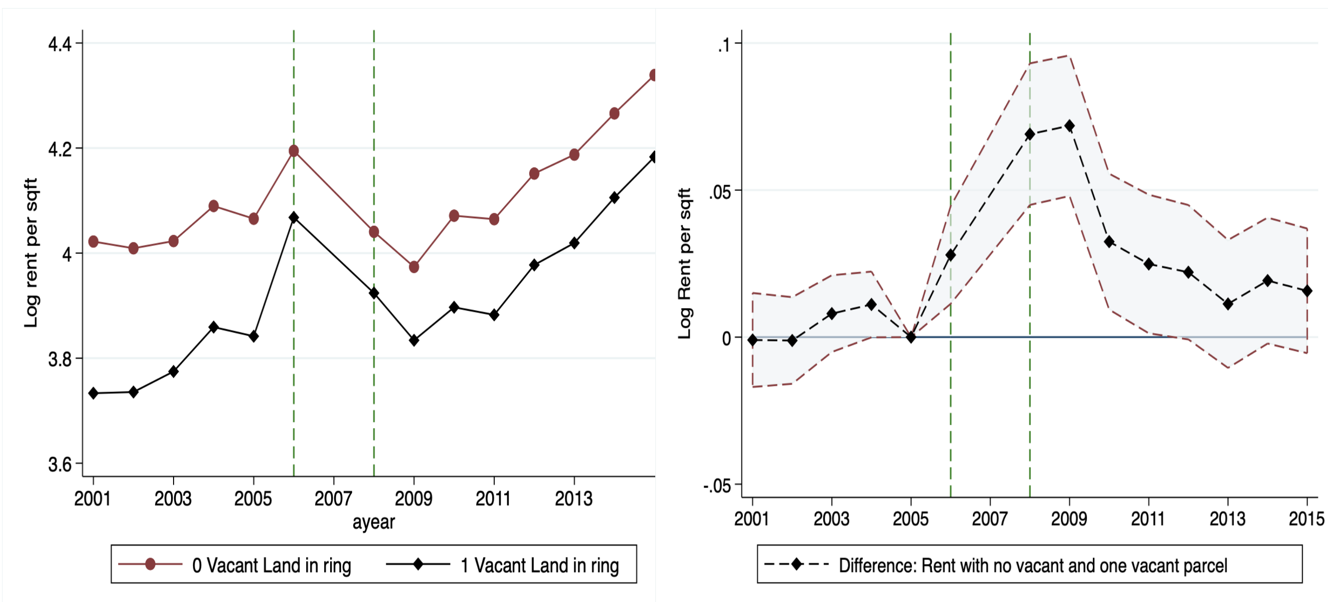
Notes: This figure shows the first stage for the instrument—baseline number of vacant parcels available within a 150 meters radiius from an existing rental building in 2005. Panel 14a shows the distibution of number of vacant parcels within 150 meters and mean tax-exempt units within the same distance in the time-notch. Panel 14b presents the mean tax exempt units received within 150 meters from an existing rental building that had 0 vacant parcels and 1 vacant parcels within that distance in 2005. The latter forms the main dichotomous instrumental variable used for estimating the effect on rents.

Figure 15: Probability that a building receives a Tax-exempt unit within 150 meters in the time notch



Notes: This figure presents the difference in the mean tax exempt units received within 150 meters from an existing rental building with 0 vacant parcel and existing rental buildings with 1 vacant parcel within the same distance.

Figure 16: Rents in buildings with vacant parcel vs Buildings with no Vacant Parcel within 150 meters

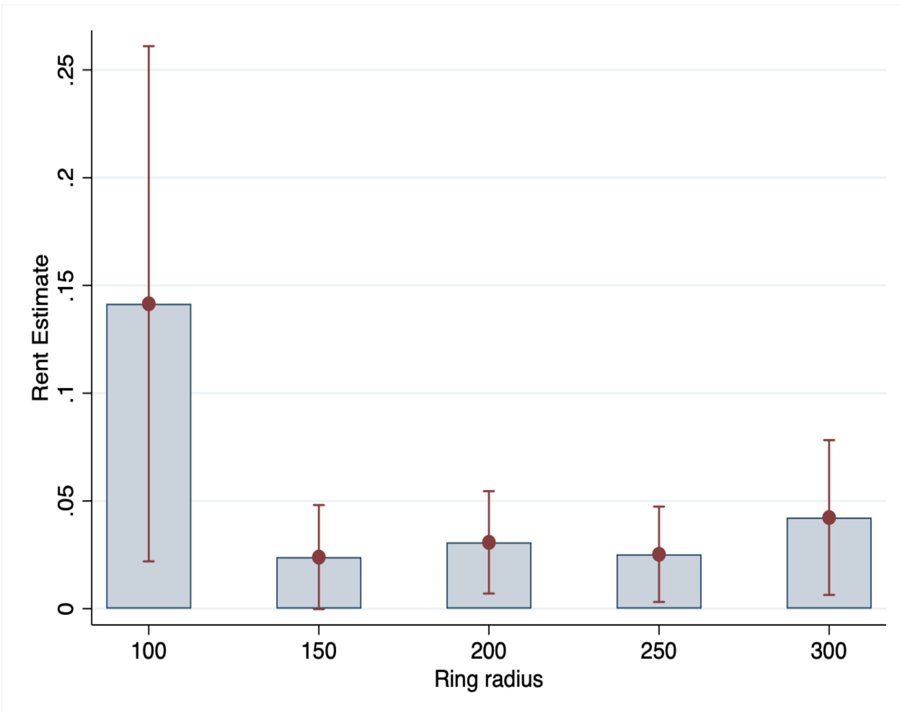


(a) Raw data

(b) Regression estimates

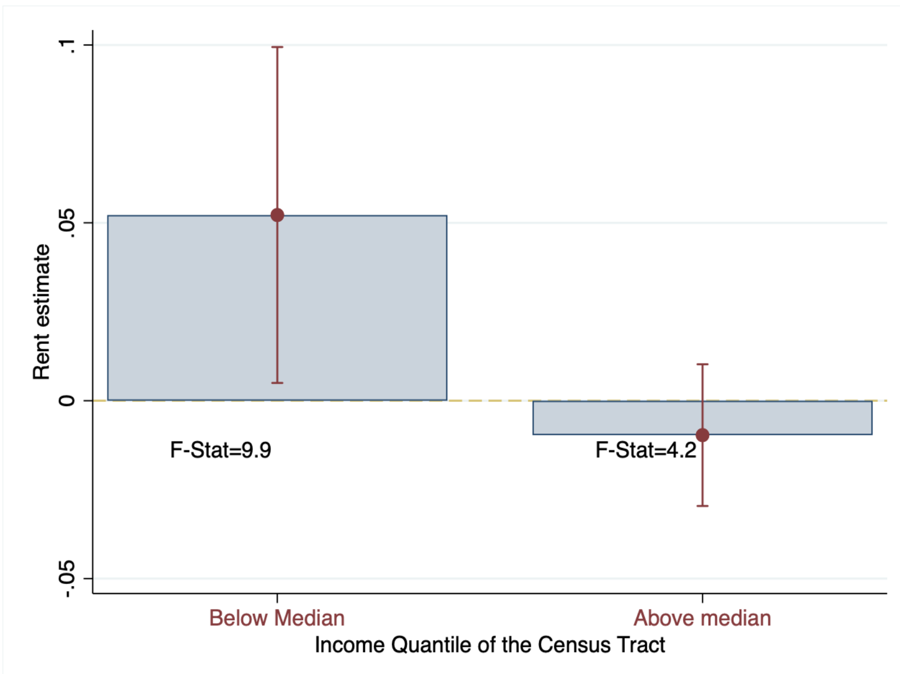
Notes: This figure plots the trends in (log) rents of existing buildings that had a vacant parcel available within 150 meters and buildings that had no vacant parcel available within 150 meters, in the beginning of the notch period. Panel 16a presents trends in the raw data, obtained by taking the weighted average of building–year rent where each building is weighted by the number of residential units. Panel 16b presents the event study coefficients with 5% confidence interval using specification 6. Specifically, this equation estimates the differential trends in buildings with zero and one vacant parcel within 150 metres and controls for building, year and 2000 census tract–year fixed effects.

Figure 17: IV estimate as a function of the Ring Radius



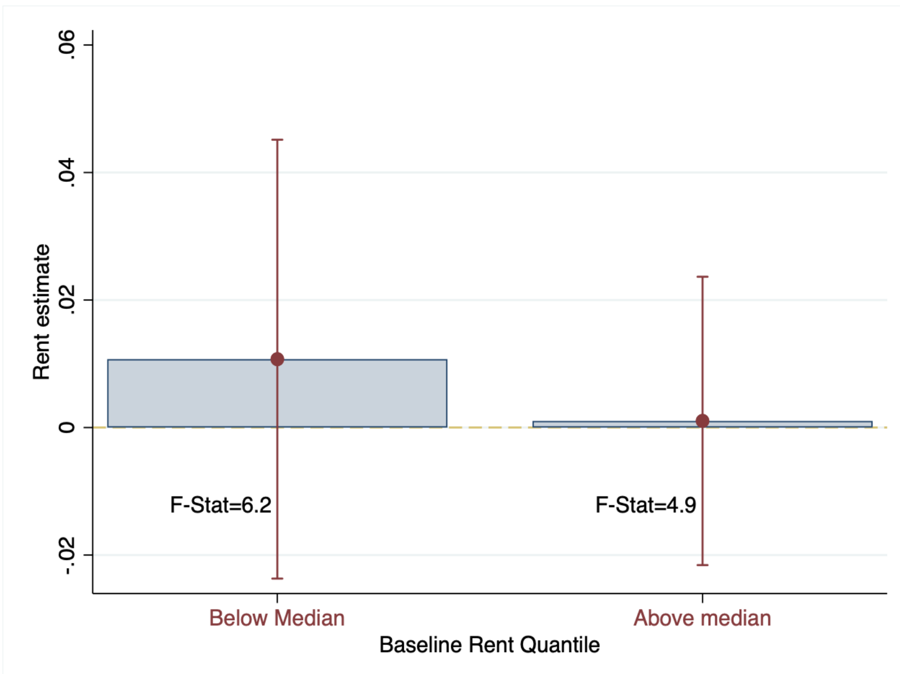
Notes: This figure presents the IV estimate—-the effect of an additional tax-exempt unit in the time-notch within *r* radius from an existing building on its rents—-as a function of ring radius. *r* varies from 50 meters to 300 in increments of 50 meters. The instrumental variable is a dummy that takes value 1 if the building had a single parcel available within 150 meters radius at the start of the reform. All specifications include building, year and census tract-year fixed effects. Bars denote 5% confidence interval with standard errors clustered at the building.

Figure 18: IV Estimate as a function of Census-tract Income



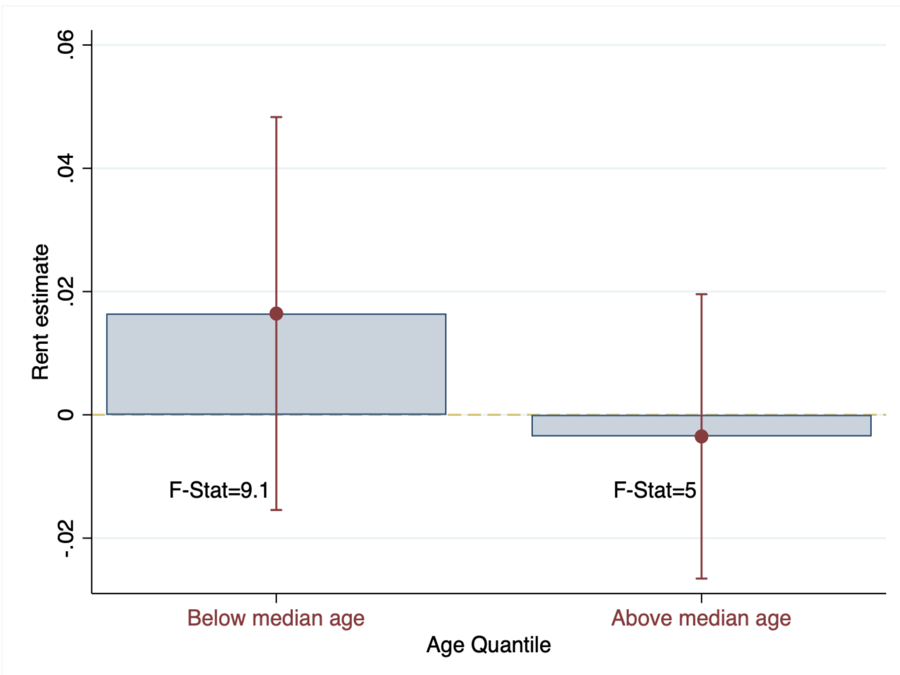
Notes: This figure presents the IV estimate—-the effect of an additional tax-exempt unit in the time-notch within 150 meters radius from an existing building on its rents—as a function of income of the census-tract in 2000. The instrumental variable is a dummy that takes value 1 if the building had a single parcel available within 150 meters radius at the start of the reform. All specifications include building, year and census-year fixed effects. Bars denote 5% confidence interval with standard errors clustered at the building. Cluster-robust (AP) F-Stat are reported.

Figure 19: IV Estimate as a function of baseline building rent



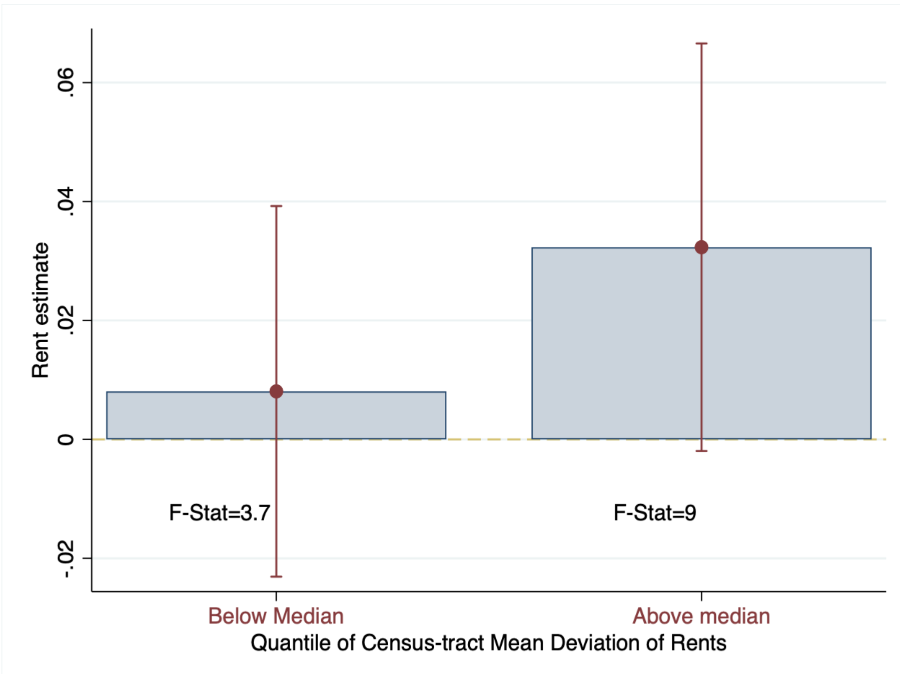
Notes: This figure presents the IV estimate—-the effect of an additional tax-exempt unit in the time-notch within 150 meters radius from an existing building on its rents—as a function of income of building's rent in the baseline. The instrumental variable is a dummy that takes value 1 if the building had a single parcel available within 150 meters radius at the start of the reform. All specifications include building, year and census-year fixed effects. Bars denote 5% confidence interval with standard errors clustered at the building. Cluster-robust (AP) F-Stat are reported.

Figure 20: IV Estimate as a function of Baseline Building Age



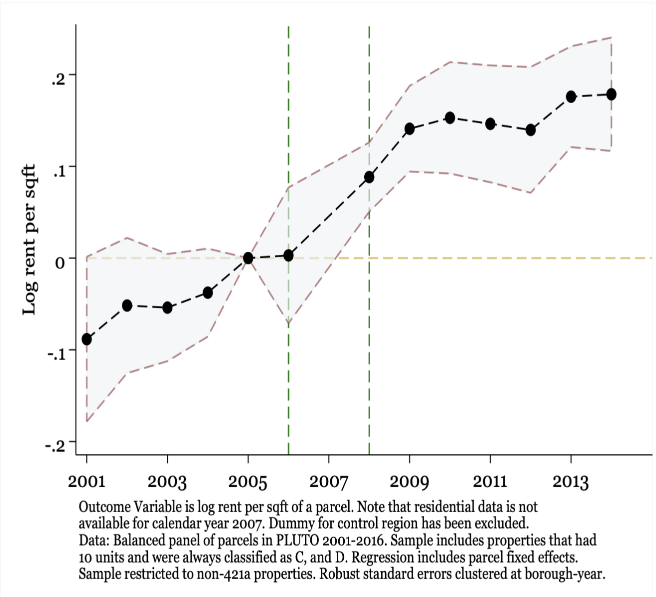
Notes: This figure presents the IV estimate—-the effect of an additional tax-exempt unit in the time-notch within 150 meters radius from an existing building on its rents—as a function of building age at the baseline. The instrumental variable is a dummy that takes value 1 if the building had a single parcel available within 150 meters radius at the start of the reform. All specifications include building, year and census-year fixed effects. Bars denote 5% confidence interval with standard errors clustered at the building. Cluster-robust (AP) F-Stat are reported.

Figure 21: IV Estimate as a function of Census-tract Housing Quality Heterogeneity

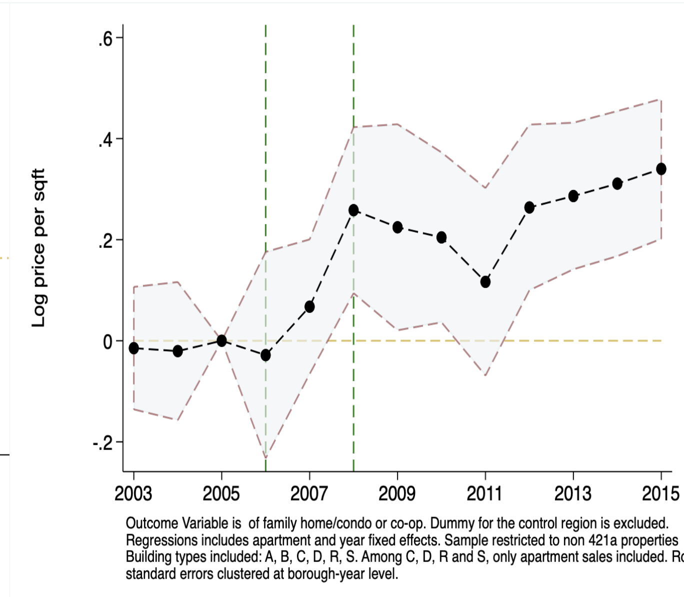


Notes: This figure presents the IV estimate—-the effect of an additional tax-exempt unit in the time-notch within 150 meters radius from an existing building on its rents—as a function of variation in housing quality within the census tract. The latter is measured by mean deviation in rents across buildings within a census tract in 2006. The instrumental variable is a dummy that takes value 1 if the building had a single parcel available within 150 meters radius at the start of the reform. All specifications include building, year and census-year fixed effects. Bars denote 5% confidence interval with standard errors clustered at the building. Cluster-robust F-Stat are reported.

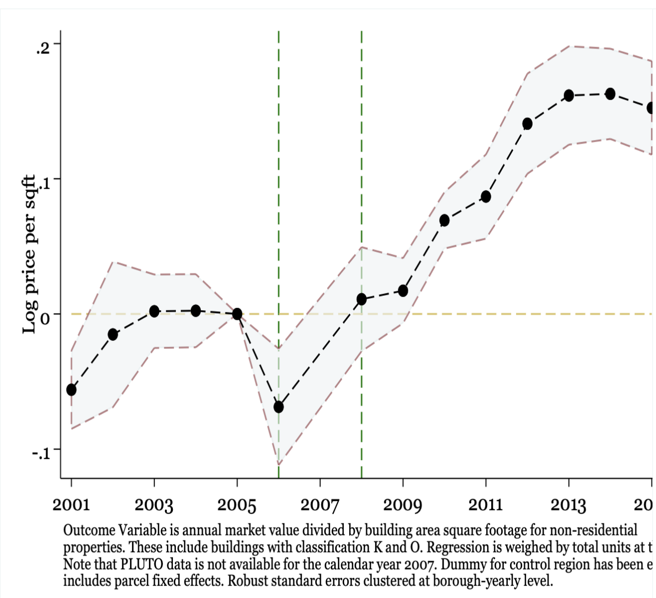
Figure 22: New Residential Investment and Prices: Prices in Exclusion and Non-exclusion regions



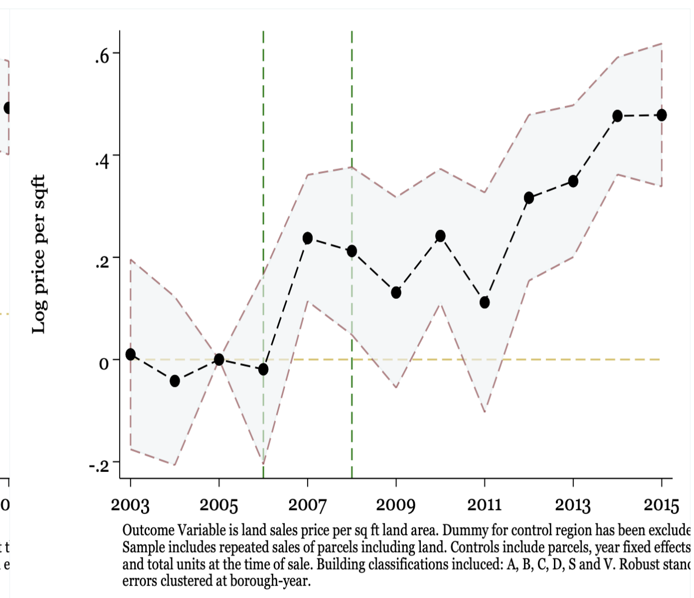
(a) Effect on gross rents



(b) Effect on sale price of homes



(c) Effect on commercial rents



(d) Effect on land prices

Figure 23: Parcels used in Differences-in-Geographic Regression Discontinuity

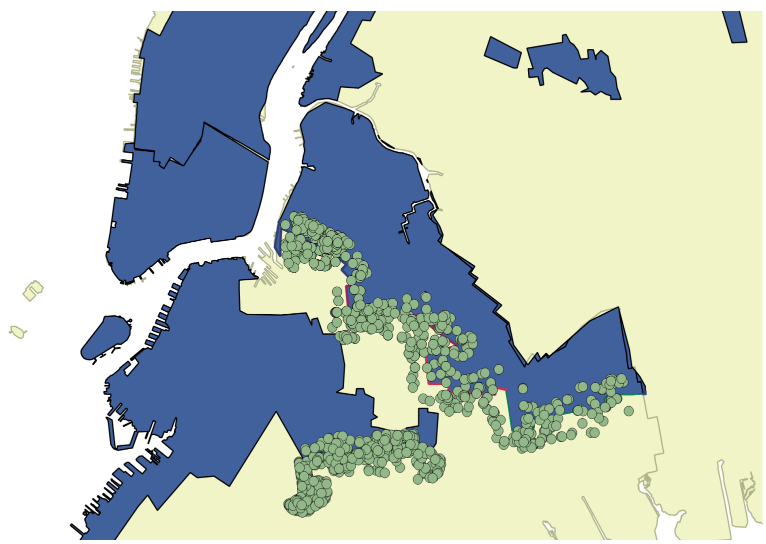
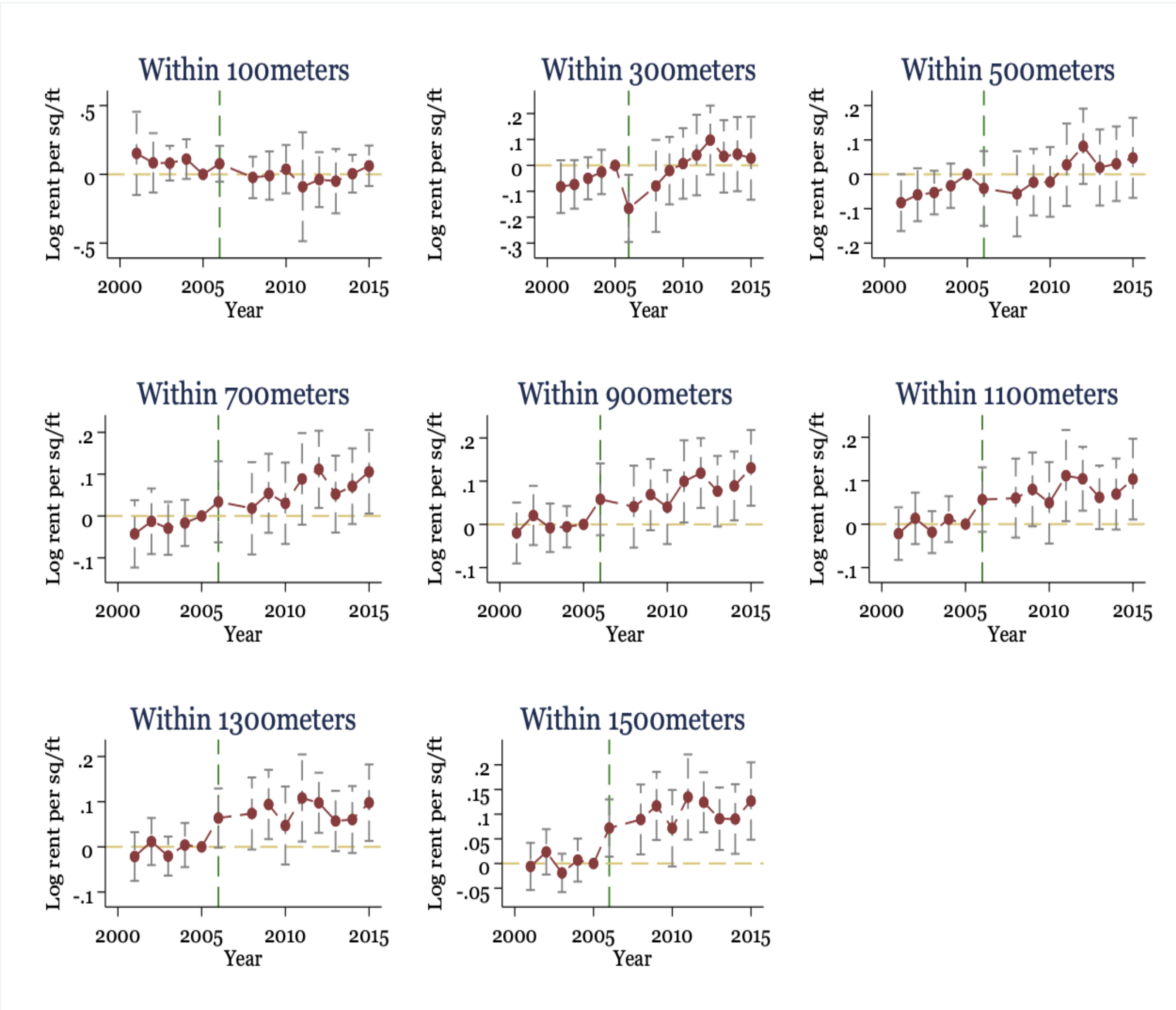
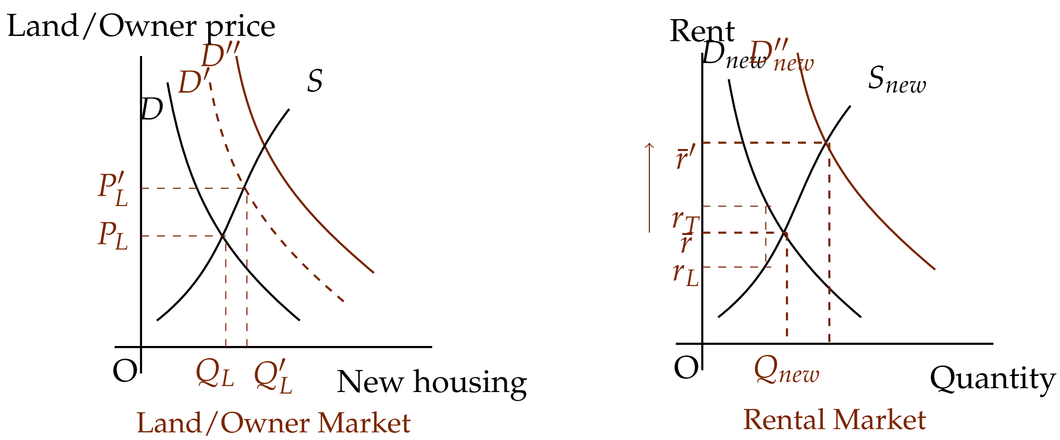


Figure 24: Differences in Regression Discontinuity at varying bandwidths



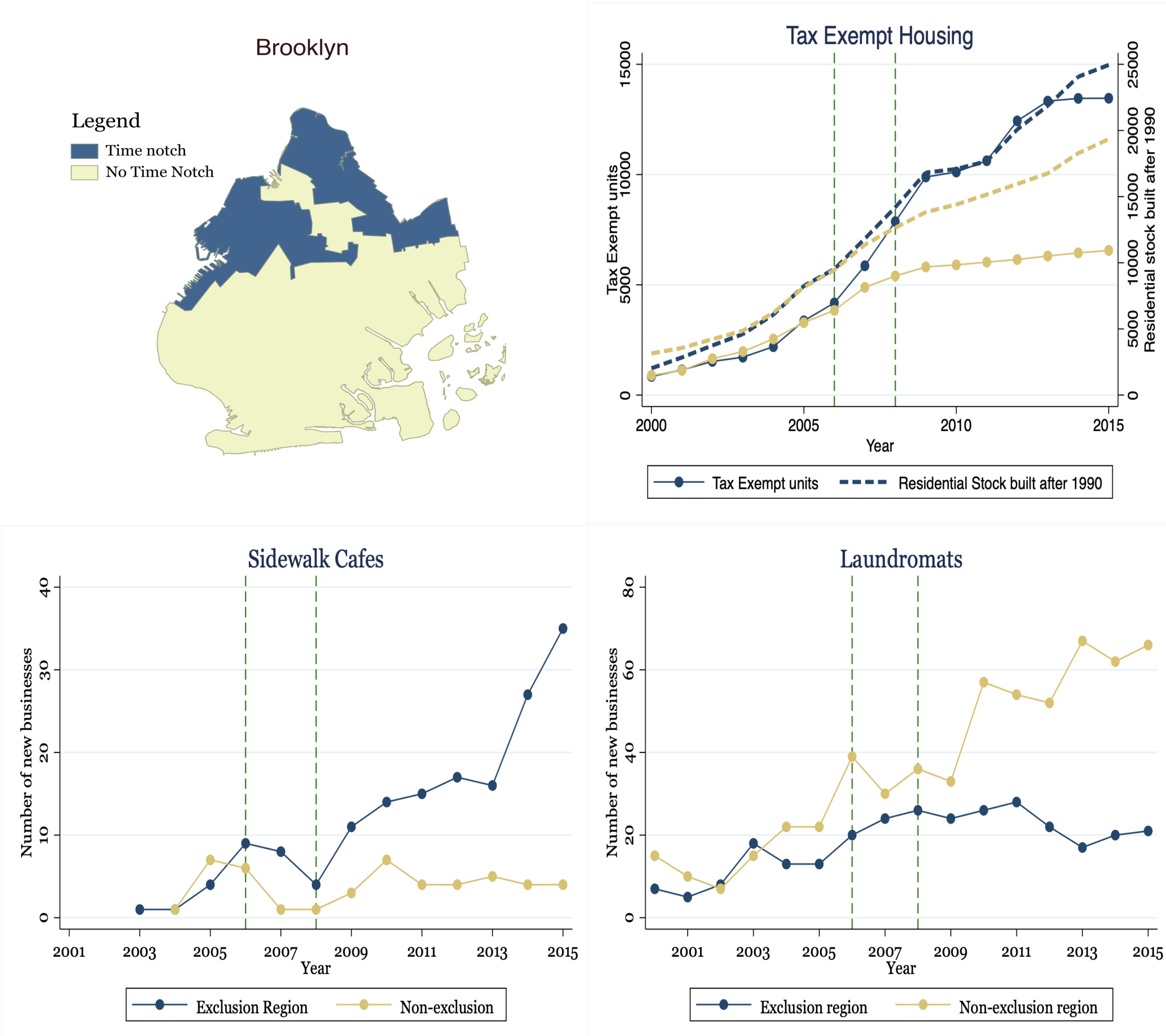
Notes: This figure plots yearly differences in log rent per square feet within and outside the exclusion regions in Brooklyn. *'Within r meters'* includes parcels within *r* meters from the boundary on either side where *r* varies from 100 to 1500.

Figure 25: Mechanism



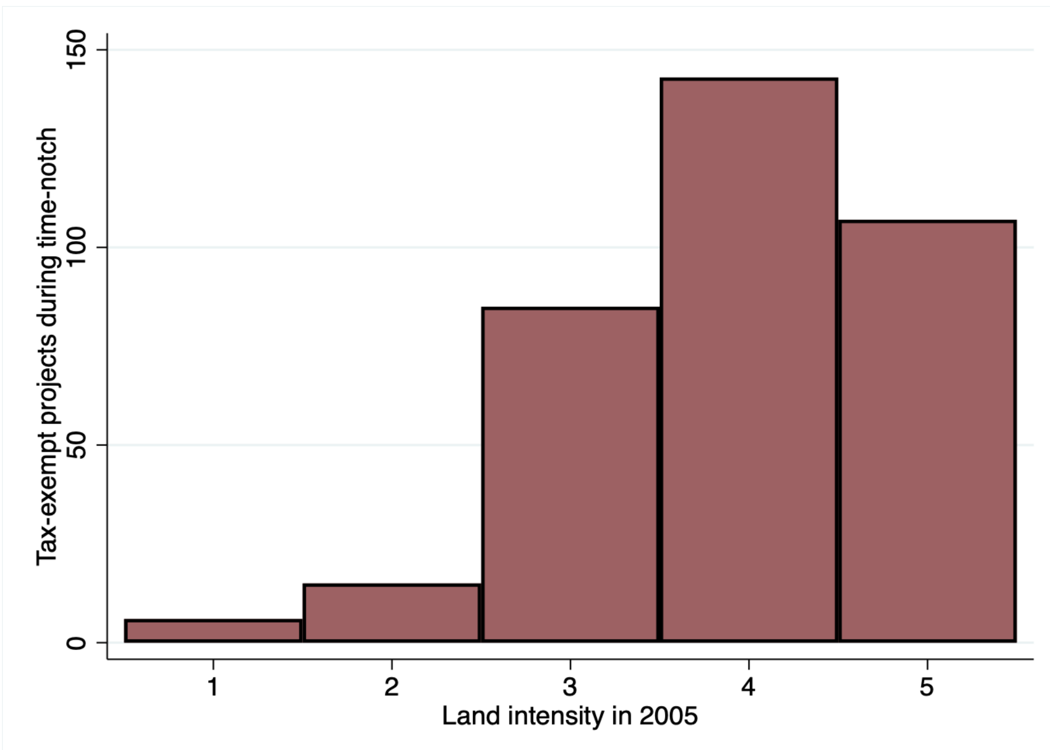
Notes: This figure depicts the effect of increase in tax-exempt residential investment in an open economy with endogenous amentities. The reform increased supply of tax exempt units which was equivalent to reduction in average property tax paid by the tenant. While the reduction in average property tax reduces gross rents (*supply effect* $P'_L \rightarrow P_L$) on incumbent units, there is also an amenity effect, which can be divided into two parts. First, addition of new units directly increases amenity value of the region by introduction of new units in the neighborhood. Second, newer units attract relatively richer households and increases average income of the region. This in turn, increases local demand for local goods and services, further increasing the amenity value of the region. Together, these two channels imply that the amenity value of old units go up. This is what I refer to as *amenity* effect of tax-exempt unit. If *amenity* effect dominates *competition* effect, then local rents rise instead of falling, landlords pocket larger net rents and landowners benefit from even larger land prices.

Figure 26: New tax-exempt residential investment and consumption amenities in Brooklyn



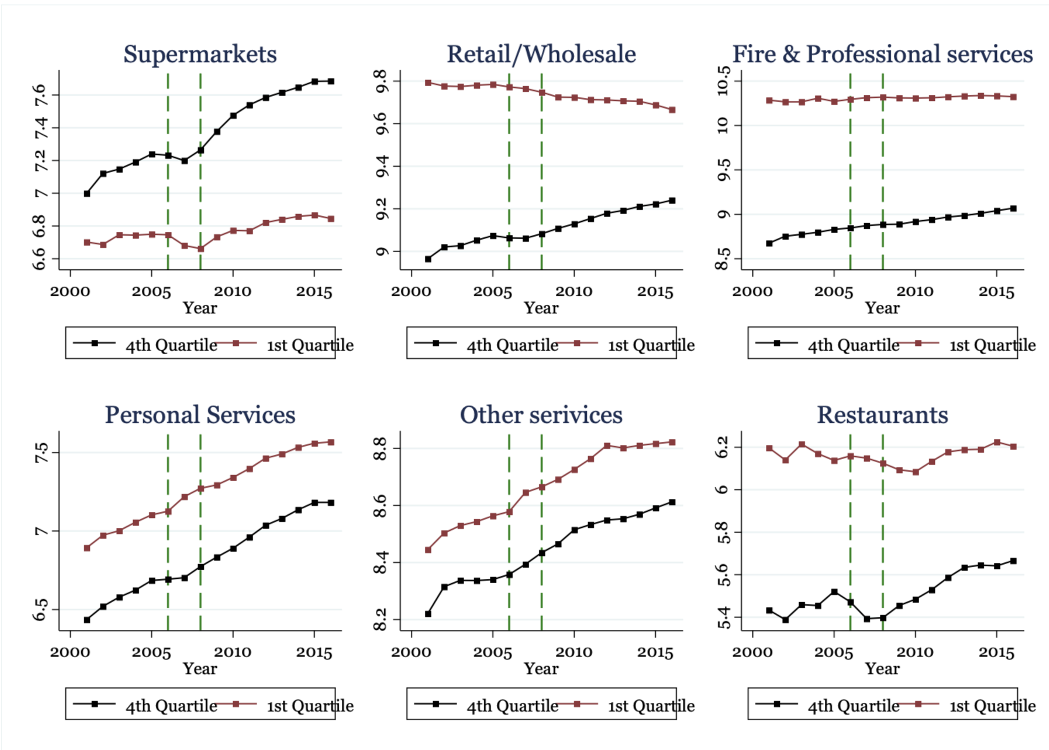
Notes: These figures illustrate the case study of Brooklyn—a major borough in New York City— that witnessed a significant investment shock due to the property tax reform. The top-left panel shows the exclusion regions where the property tax was set to increase beginning 2008. The delay in implementation increased housing starts. Consequently, new tax-exempt housing in exclusion regions increased drastically (top-right). There was also a shift in composition of businesses in the exclusion regions. Sidewalk cafes (associated with high-income tenants) increased while laundromats (associated with low-income tenants) fell, as illustrated by bottom-left and bottom-right panel respectively.

Figure 27: First stage: Tax exempt projects in the time notch against baseline land intensity in the zipcode



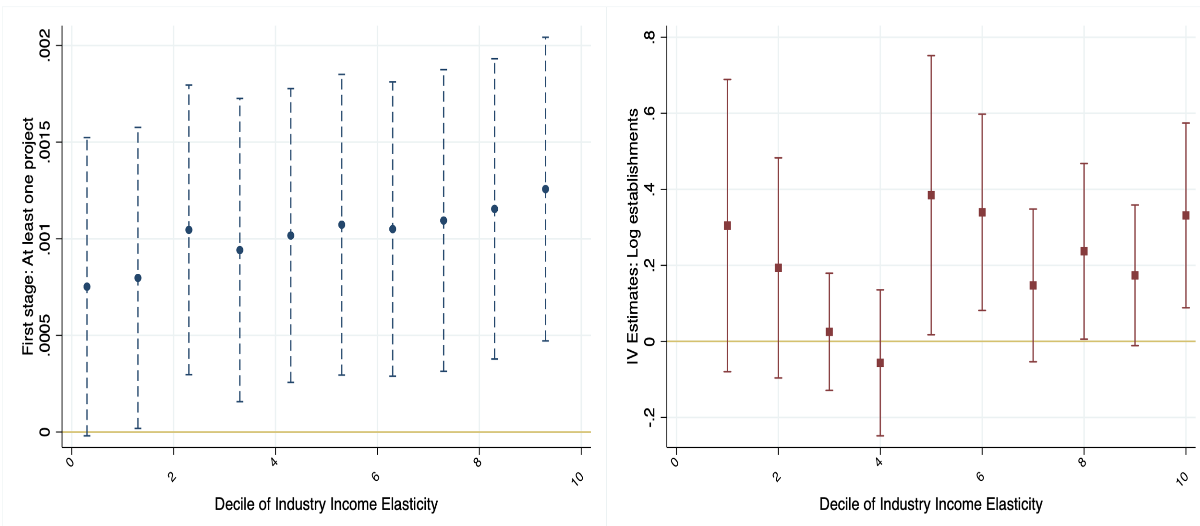
Notes: This figure illustrates the number of projects started in the time notch as a function of baseline number of vacant parcels available in a Zip Code at the start of the notch period.

Figure 28: Log Establishments by High Land and Low Land Availability



Notes: This figure illustrates the raw-trends in the log number of establishments by whether the zipcode belongs to highest quartile (more likely to receive new tax-exempt residential investment in the time-notch) or lowest quartile (less likely to receive new tax-exempt residential investment in the time-notch) in terms of baseline number of vacant parcels at the start of the reform period. This is shown for six broad industry categories.

Figure 29: Business activity: Estimates by Industry Income Elasticity



Notes: This figure provides estimates of the effect of new residential investment on the log number of establishments, as a function of 4-digit industry's income elasticity. The x-axis denotes the industry income elasticity decile. Figure in the left panel shows the first stage—effect of baseline vacant parcels on number of projects in the zipcode in the time notch. Each point represents estimate from a separate regression using industries in the same decile. Industry income elasticity is obtained by regressing log number of establishments in the zipcode in 2001 against log mean renter income in the zipcode in 2000, separately for each 4-digit industry. This produces 300 industry elasticities which are ordered into ten deciles. The figure in the right panel shows the IV estimate, ordered by income elasticity of industries. Data: County Business Patterns 2001-16, and Census 2000.

Table 1: Changes in 2006-08 Property Tax Reform

Period	Light-blue region (Old-exclusion region)	Dark-blue region (New Exclusion)	Yellow (Non-exclusion)
		Offsite Long Exemption	
Before 2006-8	**Avail**	NA	NA
After 2006-8	**NA**	NA	NA
		Short Exemption	
Before 2006-8	NA	**Avail**	Avail
After 2006-8	NA	**NA**	Avail but capped
		Onsite Long Exemption	
Before 2006-8	Avail	**Avail**	**Avail**
After 2006-8	NA	**Avail**	**Avail**

Notes: This table summarizes temporal and spatial variation in the tax incentives created by the 421a property tax-reform. Bold represents most preferred exemption type before the tax reform in the specified region. In newly excluded regions, either the reform did not alter the tax incentives or it effectively removed the tax exemption, depending on local neighborhood rents. The latter is going to be true for high rent-regions within new-exclusion regions. See Appendix A-6 for the underlying calculations.

Table 2: Summary statistics for Non-tax-exempt and Tax-exempt Parcels in 2015

	Mean	Median	SD	N	t-test
Building area	14062.65	3887	53699.92	8613	
	19265.76	4959.5	58284.65	7280	-5.8
Res units	13.50	3	51.82	8613	
	16.98	4	52.88	7280	-4.17
Lot area	4675.71	2450	14430.12	8613	
	5737.86	2502	16690.42	7280	-4.25
No. Buildings	1.12	1	1.39	8613	
	1.29	1	2.51	7280	-5.12
Rent	1316.14	704.15	1766.24	863	
	1927.60	1636.68	1558.80	708	-7.28
If Condo	.08	0	.285	8613	
	.26	0	.43	7280	-28.56
If High-income region	.58	1	.49	8613	
	.45	0	.50	7280	17.25
Age	12.71	11	5.99	8051	
	10.55	10	3.95	7216	26.44
Exclusion region	.30	0	.45	8613	.
	.28	0	.45	7280	2.17
If Manhattan/Brooklyn	.54	1	.49	8613	.
	.48	0	.50	7280	7.94

Notes: This table summarizes key variables for non-tax-exempt (first row) and tax-exempt (second row) buildings. Sample restricted to buildings developed after 1990. *Building area* refers to the gross square feet residential area developed in the building. *High income* is a dummy that takes value one if the parcel is located in above median income census tract. *Imputed Rent* is inferred using Department of Finance's market valuation. *If condo* is a dummy that indicates if parcel also contains condonium units. *Exclusion region* is a dummy indicating if the building is located in exclusion regions which experienced tax increase as part of the tax reform. Data: Primary Land Use Tax Lot Output, 2015 and Department of Finance 421a exemption list 2015.

Table 3: Descriptives: Rental parcels in New York City in 2007

	Mean	SD	Min	p25	p50	p75	Max	N
				Non-exclusion region				
Assessed value land	436195.25	1441824.5	2373.3333	74000	149000	375000	80300000	13134
Residential area	60771.53	176127.41	1173.9131	17400	34330.17	63237	13498063	13134
Residential units	63.69	154.8	11	21	39	65	10914	13134
Lot area	26586.48	103171.12	1208	5300	10000	18000	5048550	13134
No. Buildings	1.82	5.39	1	1	1	1	109	13134
Market value per sqft	57.09	33.23	.56	36.94	48.55	68.11	689.83	13134
Imputed Rent	686.05	392.77	5.75	459.63	594.34	805.13	8604.19	13134
Rental stock	836575	0	836575	836575	836575	836575	836575	13134
				Exclusion region				
Assessed value land	1307870.5	4871917	2562.2222	129000	351000	866000	3.148e+08	16643
Residential area	46875.48	132196.81	100	9860	17281.77	42000	8897465	16643
Residential units	49.860	137.36	11	16	23	43	8756	16643
Lot area	11104.04	54368.63	825	2564	4800	9372	4444613	16643
No. Buildings	1.23	1.28	1	1	1	1	54	16643
Market value per sqft	129.94	137.84	.57	61.86	107.74	178.58	14094.16	16643
Imputed Rent	1462.98	1514.71	3.185	644.05	1037.23	1703.94	31936.91	16643
Rental stock	829826	0	829826	829826	829826	829826	829826	16643

Notes: Summary statistics of rental buildings in 2007. Rental buildings include parcels classified as type C, D or S and having at least 10 units. *Imputed Rent* is obtained by dividing annual full market value by 12 times the number of residential units in the parcel times the average capitalization rate during this period- which is 6%. *Imputed Rent* and *Market Value* are expressed in 2015 US dollars using urban CPI tables provided by BLS. *Rental stock* denotes total rental units stock in buildings with at least ten units. Data: Primary Land Use Tax Lot Output, New York City, 2001-15.

Table 4: Descriptives: Rental parcels in Brooklyn in 2007

	Mean	SD	Min	p25	p50	p75	max	N
				Non-exclusion region				
Assessed value land	415871.81	1141634.1	4400	78000	184000	395000	22600000	4487
Residential area	55626.24	109740.99	1173.91	16640	31480	60000	1788439.3	4487
Residential units	57.77	104.59	11	19	36	60	1760	4487
Lot area	21296.154	67464.586	1500	5300	10000	15717	1410820	4487
No. Buildings	1.40	2.55	1	1	1	1	78	4487
Market value per sqft	54.92	29.20	6.22	37.73	46.474	63.997	689.83	4487
Imputed rent	681.81	389.84	11.57	480.36	595.28	773.97	8604.19	4487
H_S	259246	0	259246	259246	259246	259246	259246	4487
				Exclusion region				
Assessed value land	261069.5	784087.79	3951.1111	48250	80000	195500	14400000	2444
Residential area	40003.16	91482.79	1008.33	11751	18521	35190	1466751	2444
Residential units	43.5	98.56	11	16	22	38	1836	2444
Lot area	14339.81	47532.54	1296	4000	5750	10200	1079791	2444
No. Buildings	1.34	1.99	1	1	1	1	54	2444
Market value per sqft	78.71	52.66	2.34	44.640656	65.1	97.62	550.8	2444
Imputed rent	893.31	707.75	24.36	499.58	725.05	1078.9497	16172.89	2444
H_S	106311	0	106311	106311	106311	106311	106311	2444

*Notes:*Summary statistics of rental parcels in 2007. Sample restricted to parcels in Brooklyn. Rental parcels include parcels classified as type C, D or S and having at least 10 units. *Imputed Rent* is obtained by dividing annual full market value by 12 times the number of residential units in the parcel times the average capitalization rate during this period- which is 6%. *Imputed Rent* and *Market Value* are expressed in 2015 US dollars using urban CPI tables provided by BLS. *Rental stock* denotes the total rental units stock situated in buildings with at least ten units. Data: Primary Land Use Tax Lot Output, New York City, 2002-16.

Table 5: Excess Housing Starts in the Time-notch

Region	B_{total}	H_s^{2007}	$\frac{B_{total}}{H_s^{2007}} \times 100$
New York City	10735	1043496	1%
Manhattan	3097	661940	0.4%
Brooklyn	5016	234866	2.1%
Bronx	213	24380	0.8%
Queens	2541	37336	6.8%

Notes: H_s^{2007} denotes the stock of rental housing (buildings classified types C ,D and S) in 2007 in the exclusion region in the specified borough. B_{total} denotes excess bunching in property tax exempt unit housing starts in the notch period, 2006-2008. Excess bunching is calculated as the difference in bunching mass and the counterfactual.

Table 6: Effect of New Tax-exempt Residential Investment on Rents: IV Estimates

	RF	IV	OLS	RF	IV	OLS
$\mathbb{1}(Land == 1) \times Post$	0.0251			0.0685		
	[0.018]			[0.000]		
$NotchUnits \times Post$		0.0239	0.000692		0.0690	0.00122
		[0.052]	[0.099]		[0.003]	[0.001]
Constant	4.097		4.098	4.092		4.095
K-Paap F Stat		12.58			12.00	
Cragg-Donald F Stat		982.93			982.93	
First stage		1.049			0.993	
Observations	341651	341651	341651	344794	344794	344794
Rings	24718	24718	24718	24718	24718	24718
Region F.E.	Census tract	Census tract	Census tract	Zipcode	Zipcode	Zipcode

Notes: p-values in brackets. Outcome variable is building-year log rent per square feet. The endogenous treatment variable is the number of tax-exempt *units* within 150 meters from an existing rental building at the time-notch. The instrument is a dummy which indicates whether the building had one or zero vacant parels availlable within 150 meters a year before the reform. Standard errors are clustered at the building level. All regressions include building, year and regionXyear fixed effects with the region as specified in the 'Region' row. Regressions are weighed by the number of residential units in the building. Please refer to Section 3.2 for the measurement of rental income and sample construction. Data: Primary Land Use Tax Lot Output, New York City, 2002-16.

Table 7: Effect of New Tax-exempt Residential Investment on Rents: Exclusion vs non-exclusion regions

	Reduced Form		IV Estimate		OLS Estimate	
	Non-exclusion	Exclusion	Non-exclusion	Exclusion	Non-exclusion	Exclusion
$\mathbb{1}(Land == 1) \times Post$	0.0162	0.0305				
	(0.224)	(0.058)				
$NotchUnits \times Post$			0.0660	0.0176	0.000477	0.000881
			(0.358)	(0.102)	(0.490)	(0.068)
Constant	3.743	4.473			3.744	4.475
	(0.000)	(0.000)			(0.000)	(0.000)
Kleibergen-Paap Stat			2.038	11.15		
First stage			0.246	1.733		
Observations	149685	191669	149685	191669	149685	191669
Buildings	10899	13819	10899	13819	10899	13819

Notes: p-values in brackets. Outcome variable is building-year log rent per square feet. The endogenous treatment variable is the number of tax-exempt *units* within 150 meters from an existing rental building at the time-notch. The instrument is a dummy which indicates whether a building had one or zero vacant parels available within 150 meters a year before the reform. Standard errors are clustered at the building level. All regressions include building, year and regionXyear fixed effects. Regressions are weighed by the number of residential units in the building. Please refer to Section 3.2 for the measurement of rental income and sample construction. Data: Primary Land Use Tax Lot Output, New York City, 2002-16.

Table 8: Effect of New Tax-exempt Residential Investment on Demographics: IV Estimates

	Units	Log Median Rent	Log Income	% Educ	% White HH	% Young HH	% New
(Tax-exempt Projects$\geq$ 1) X Post	762.5	0.334	0.242	20.83	17.28	7.496	0.212
	[0.000]	[0.001]	[0.044]	[0.000]	[0.010]	[0.066]	[0.348]
First stage F-stat	20.78	20.78	20.78	20.78	20.78	20.78	20.78
Dep Var. Mean	1459.9	6.938	10.74	27.02	47.40	22.07	0.625
OLS Estimate	109.4	0.0893	0.110	6.417	3.650	3.688	0.126
OLS SE	16.27	0.0104	0.0158	0.717	0.800	0.572	0.0279
Observations	4503	4503	4503	4503	4503	4503	4503

Notes: Units denote total occupied units in the census tract. Variables in Column 2 and 3 denote log of median rent and income respectively. *Educ* denotes % population with at least a Bachelor's degree. *White* denotes % of households where the household head is white. *Young HH* denotes % population with household head age less than 35 years. *New* takes value 1 if the tract median move in year lies in past 10 years. All regressions include census tract and year fixed effects. *p*-values in brackets. Standard errors clustered at the 2000 census tract level. Data source: Census 2000, American Community Survey 2005-09, 2012-17.

Table 9: Effect of New Tax-exempt Residential Investment on Rents: Short-term vs Long term

	RF	IV	RF	IV	RF	IV	RF	IV
				Short-term				
$\mathbb{1}$(Vacant $=1$)x$Year \in (2006, 08)$	0.0466				0.0944			
	(0.002)				(0.000)			
$\mathbb{1}$(NotchUnits)x$Year \in (2006, 08)$		0.0445				0.0952		
		(0.063)				(0.001)		
				Long-term				
$\mathbb{1}$(Vacant$==1$)x$Year \geq 2010$			0.0154				0.0568	
			(0.296)				(0.000)	
$\mathbb{1}$(NotchUnits)x$Year \geq 2010$				0.0146				0.0569
				(0.345)				(0.006)
Kleibergen-Paap Stat		6.533		6.534		11.98		12.03
Region	Tract	Tract	Tract	Tract	Zipcode	Zipcode	Zipcode	Zipcode
Observations	219797	219797	244123	244123	221818	221818	246361	246361

Notes: p-values in brackets. Outcome variable is building-year log rent per square feet. The endogenous treatment variable is the number of tax-exempt *units* within 150 meters from an existing rental building at the time-notch. The instrument is a dummy which indicates whether a building had one or zero vacant parels available within 150 meters a year before the reform. Standard errors are clustered at the building level. All regressions include building, year and regionXyear fixed effects with region specified in the 'region' row. Regressions are weighed by the number of residential units in the building. Please refer to Section 3.2 for the measurement of rental income and sample construction. Data: Primary Land Use Tax Lot Output, New York City, 2002-16.

Appendix

A-1 Marginal Response as a function of distance calculation

Each point in Figure 17 provides the *average* causal response (ACR) of rents to the arrival of a tax-exempt unit, within a small distance s from the building, where s goes from 50 to 300 meters. In practice, we are often interested in the *marginal* change in rent effect of tax-exempt unit as the distance from the site increases by a small amount (MCR). We can infact use ACR to obtain MCR using the standard trapezoid approximation for the intergral. To see this, consider the relationship between $MCR(s)$ and $ACR(s)$ for small changes in distance Δs:

$$MCR(s) = \lim_{\Delta s \to 0} \frac{ACR(s + \Delta s) - ACR(s)}{\Delta s}$$

Each point s in Figure 17 represents one point on the ACR curve. Therefore, the above formula identifies $MCR(s)$ for each $s \in \{100, 150, 200, 250, 300\}$. The standard errors can be obtained through the delta method. [58][59]

Figure A-3.16 shows that the IV estimate decreases the most as we move from 50 to 100 meters. MCR approaches 0 when the radius increases to 250 meters. This is consistent with the fact that the positive spillovers of new 421a construction on a rental building are higher when the construction is not too far away.

A-2 Data construction: Demographic outcomes

The data for demographic outcomes comes from Census 2000, American Community Survey (ACS) 2005-09 and 2013-17. Putting these datasets together faces three challenges. First, while Census 2000 and ACS 2005-09 uses census tract at 2000 boundaries, ACS 2013-17 uses census tract at 2010 boundaries. To make the tracts comparable, I obtain the relationship files from Census Bureau website. Using the percentage of population in 2010 tract contained in 2000 tract, I assign each 2010 to an equivalent population weighted 2000 tract.

The second challenge comes from missing outcome values for some census tracts in certain years. A statistic for a census tract is generally omitted if it falls below 10. This could lead to a biased sample. I circumvent this issue in two steps: i) I drop any census tract which is missing any outcome variable observation in any year; and ii) I restrict to census tracts that are observed each year, leading to a balanced panel of census tracts at 2000 boundaries. These sample restrictions imply that the final sample is representative of relatively larger census tracts.

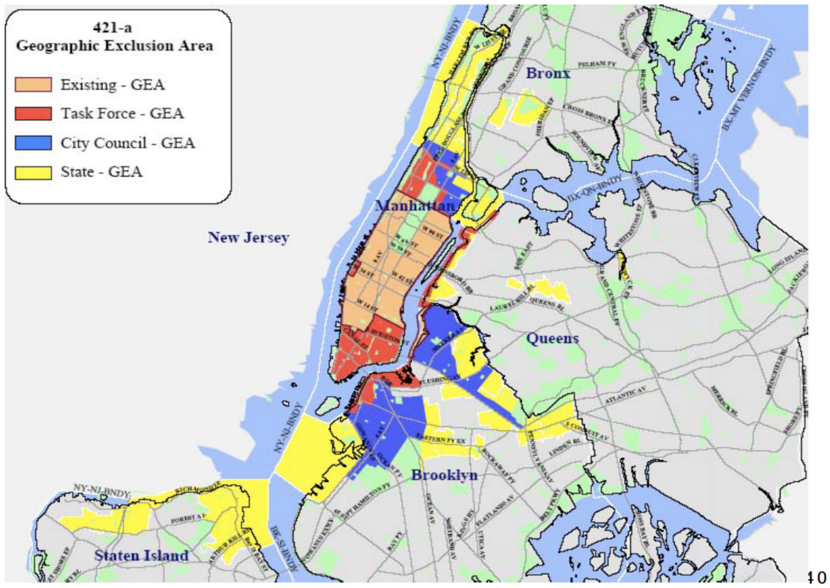
[58]In particular, $V(MCR(s)) = \frac{1}{4}(V(ACR(s)) + V(ACT(s + \Delta s))$, assuming the two ACR estimates are uncorrelated.)

[59]See Diamond and McQuade [2016] for a non-parametric method to measure the effect of LIHTC on prices, as a function of distance.

Finally, the distribution of baseline vacant parcels in a census tract is skewed. 90% of the census tracts have less than 33 vacant parcels. But there are some parcels with number as high as 593. To keep the sample of census tracts comparable, I exclude census tracts with the number higher than the 90th percentile.

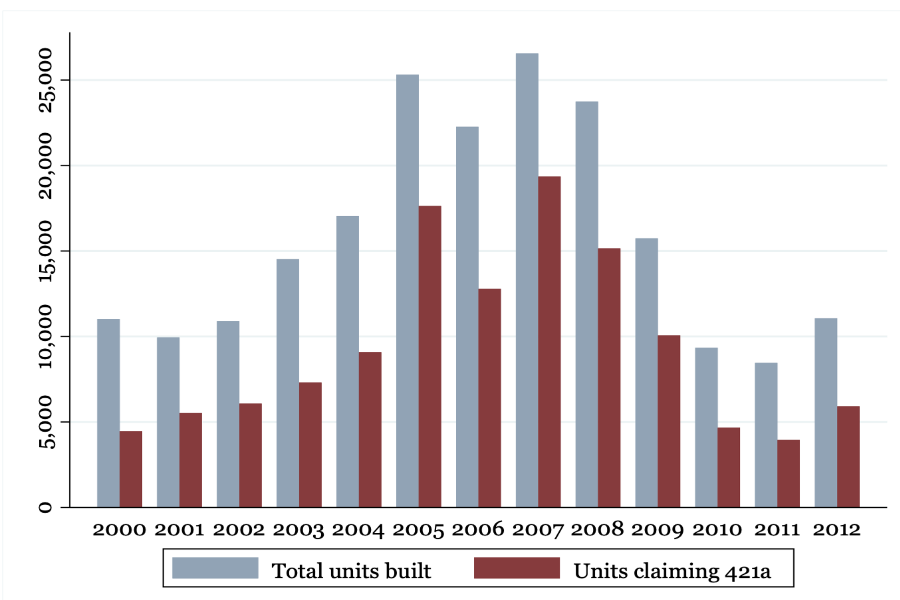
A-3 Supportive tables and figures

Figure A-3.1: Political process in the selection of the exclusion regions



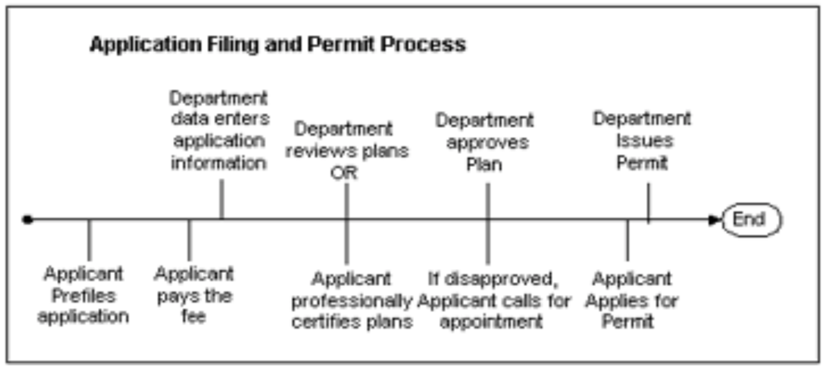
Notes: This figure presents the political process by which *new* exclusion regions (or dark exclusion regions in Figure 1) were decided. Data source: New York City Independent Budget Office.

Figure A-3.2: 421a property tax exemption use over time



Notes: Sample restricted to Class 2 residential buildings (defined 3 units and above)— only those that are eligible for the 421a property tax exemption.

Figure A-3.3: Steps Required in Obtaining a Work Permit in New York City



Notes: This figure outlines the key steps in obtaining a work permit in New York City. Source: Department of Buildings

Figure A-3.4: Quarterly tax-exempt housing starts by exemption type

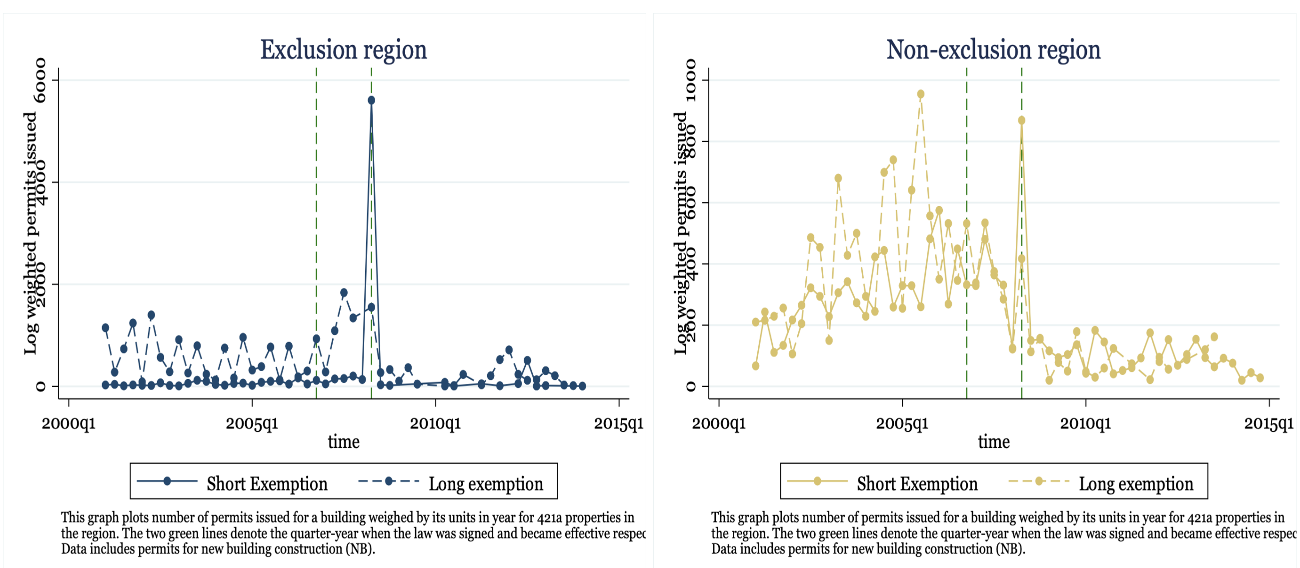
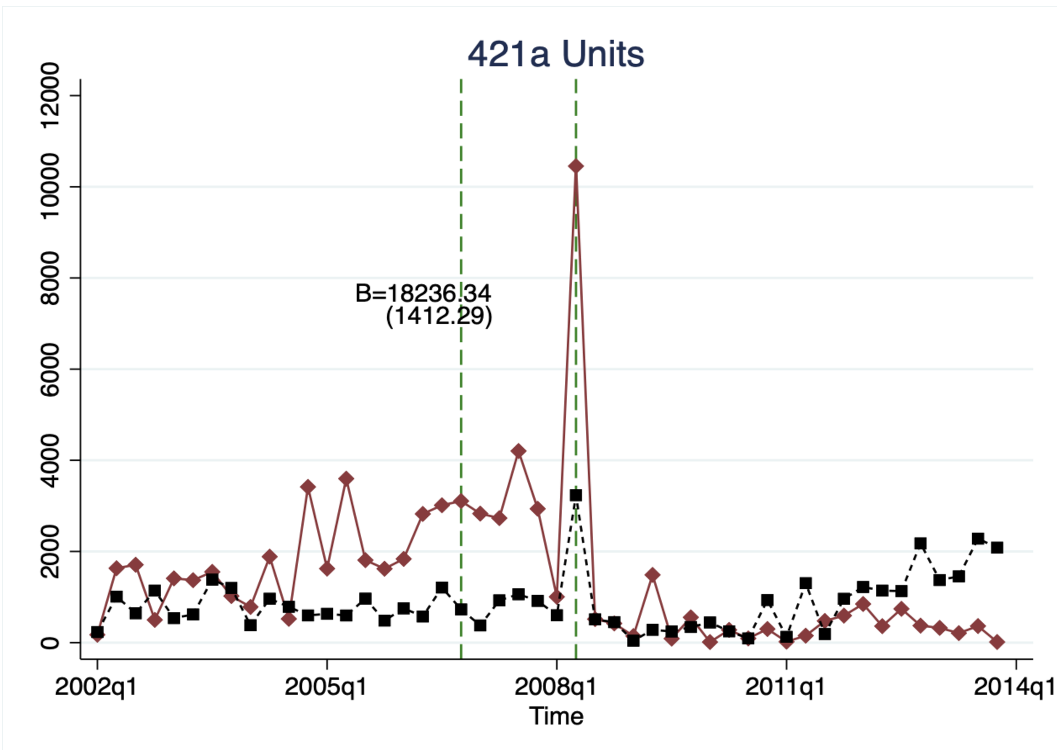
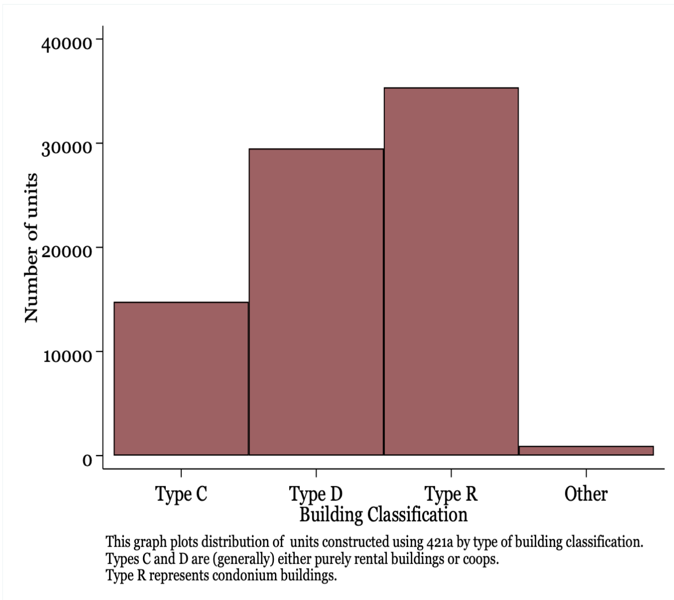


Figure A-3.5: Bunching estimates including CONDOS

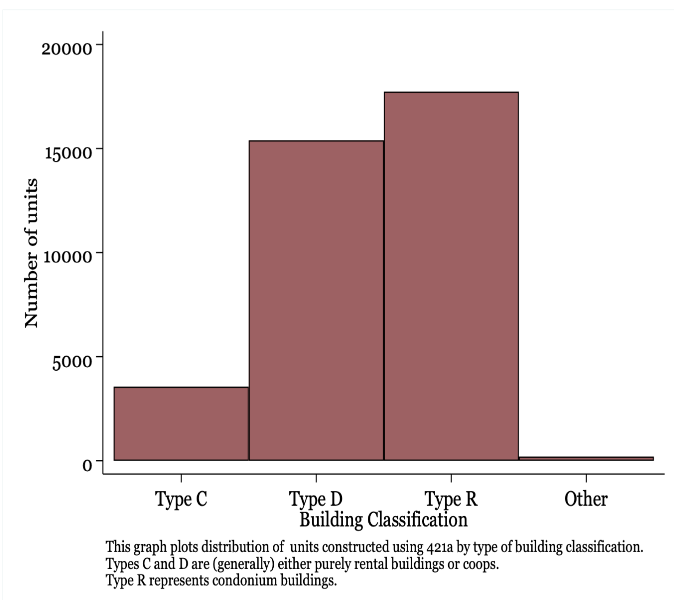


Notes: Diamonds denote the empirical distribution of tax-exempt housing starts. Squares denotes the counterfactual: empirical distribution of non-tax-exempt housing starts in the exclusion region. Data: Department of Buildings permits, 2001-15.

Figure A-3.6: Distribution of tax-exempt projects across building types

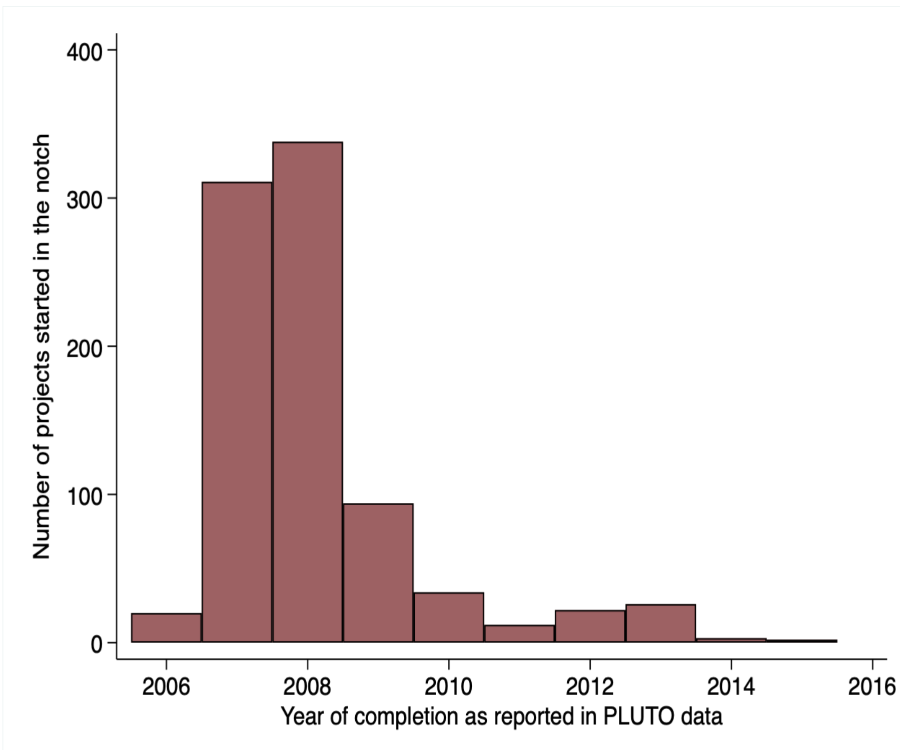


(a) Before the time-notch



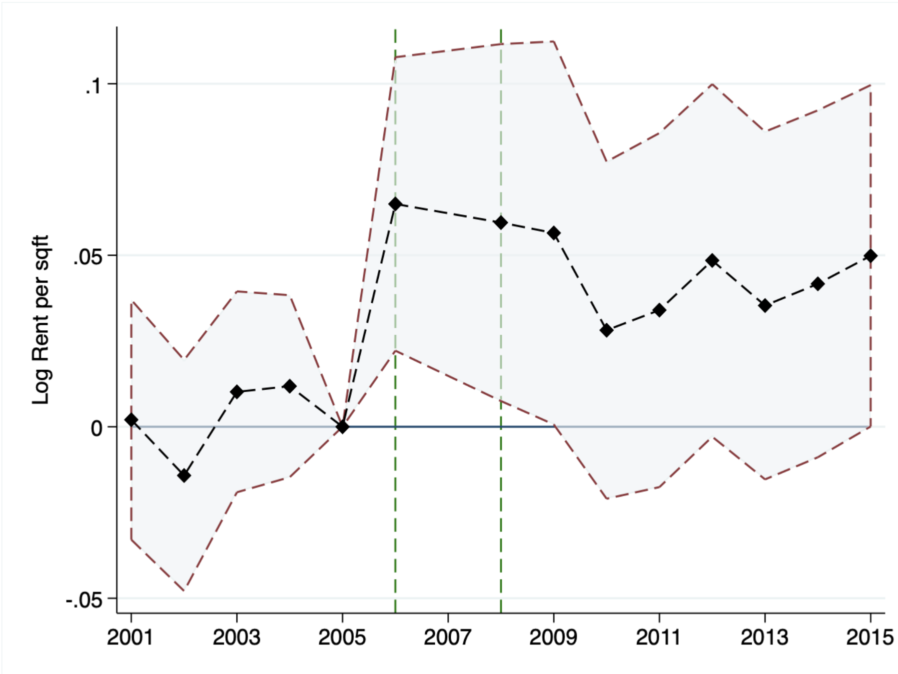
(b) In the time-notch

Figure A-3.7: Completion Year of Notch Projects



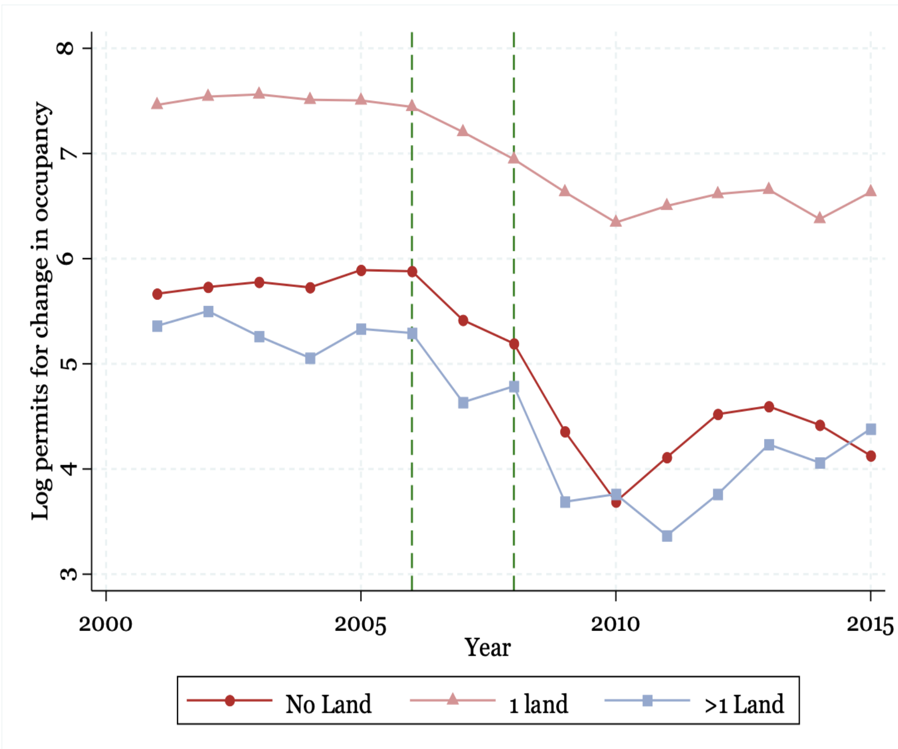
Notes: The graph plots the distribution of the year of completion of tax-exempt projects that started in the time-notch. Data: Permits data matched with Primary Land Use and Tax Output 2002-16.

Figure A-3.8: Effect of New Residential Investment on Rents: OLS Estimates



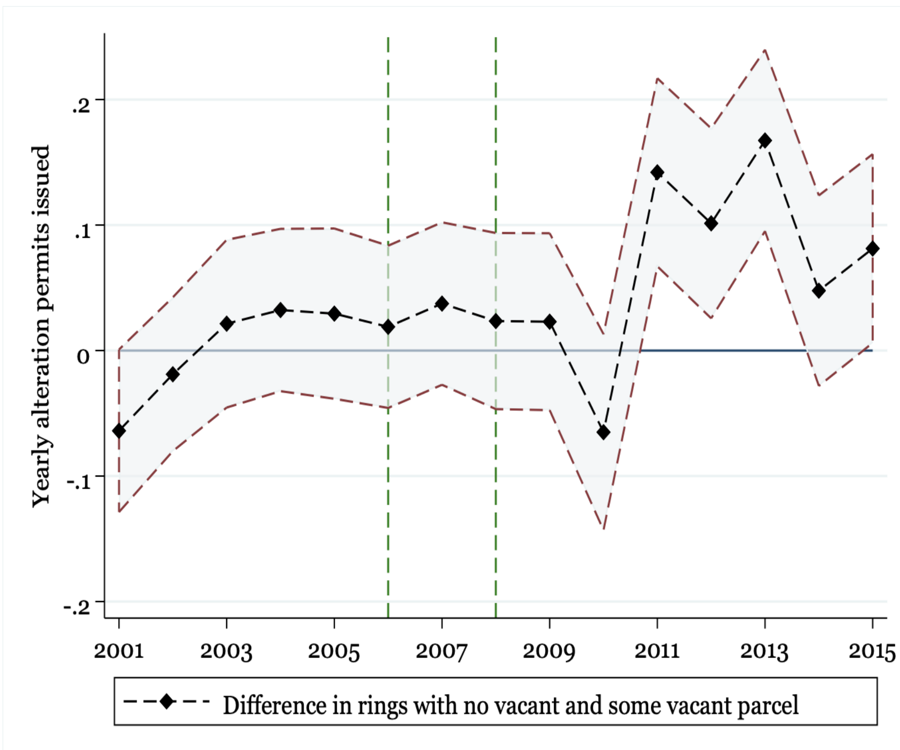
Notes: The graph plots the event study of the effect of a new tax-exempt project in the notch within 150 meters radius from an existing rental building on its rents. The regression includes building, year, and zipcodeXyear fixed effects. It is weighted by the number of residential units in the building. 5% confidence interval clustered at the building level are plotted on the graph. Please refer to Section 3.2 for the measurement of rental income and sample construction. Data: Primary Land Use Tax Output, 2002-16 and Department of Buildings permits, 2001-15

Figure A-3.9: Total investment in within 150 meters from the building



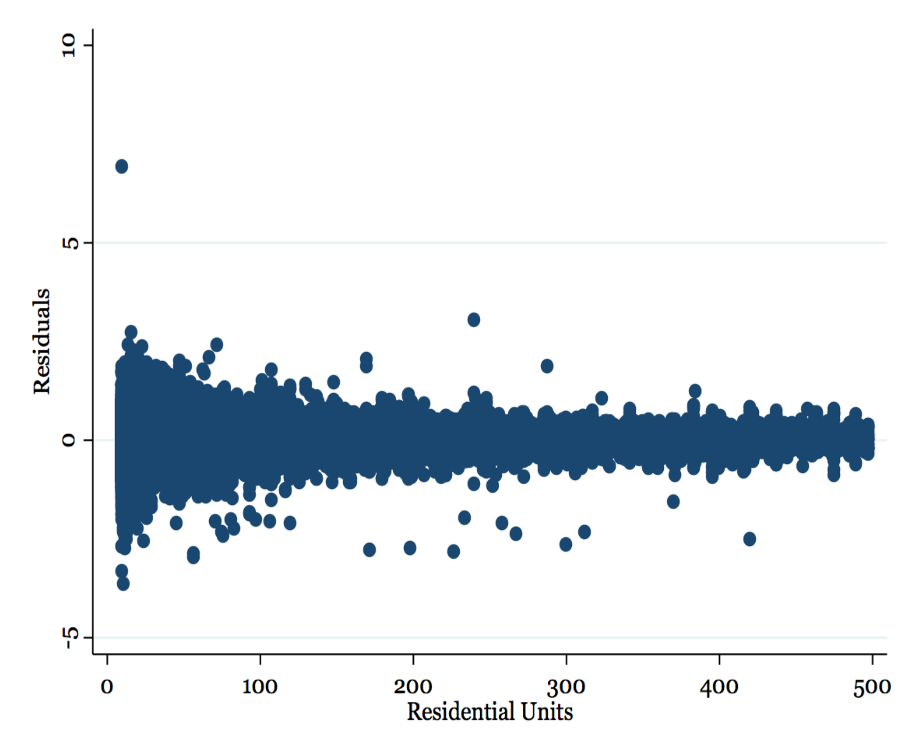
Notes: This figure plots the *total* investment within 150 meters radius from an existing rental building. The buildings sample is divided into three subsamples i) rental buildings that did not have a single vacant parcel within 150 meters distance; ii) buildings that had exactly one vacant parcel within the same distance; and finally, iii) buildings that had more than one vacant parcel available within the same distance. The y-axis denotes the average yearly logarithm of permits issued for any building/parcel. The permits include application for New Building (NB), Demolition (DM) or a major renovation that changes occupancy (A1). Data: Primary Land Use Tax Output, 2002-16 and Department of Buildings permits, 2001-15.

Figure A-3.10: Alterations and renovations



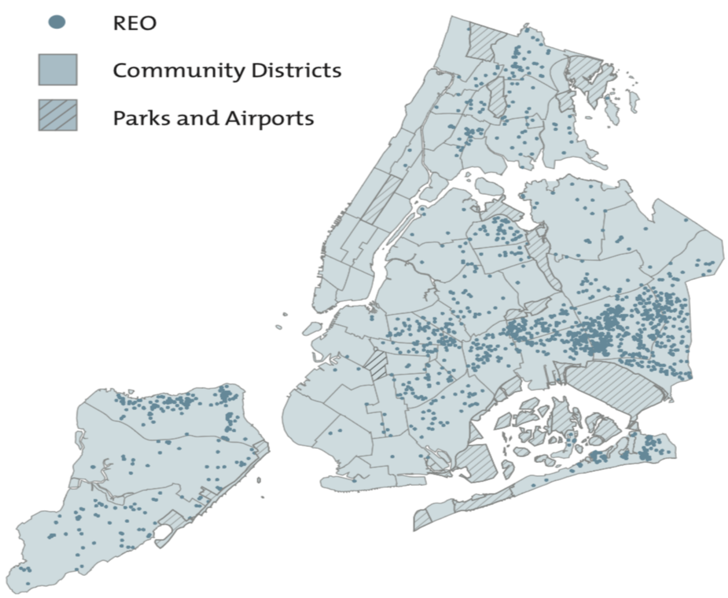
Notes: This figure plots the difference in permits for minor alterations and renovations (designated as 'A2') issued for buildings located near rental buildings with zero vacant parcels within 150 meters and buildings near existing buildings with one or more vacant parcels within 150 meters. Data: Primary Land Use Tax Output, 2002-16 and Department of Buildings permits, 2001-15.

Figure A-3.11: Heterogeneity test



Notes: Data truncated to less than 500 units.

Figure A-3.12: Foreclosures in 2008 in New York



Notes: Figure from NYU Furman center report 2008

Figure A-3.13: Alternative counterfactual: Commercial and family homes starts

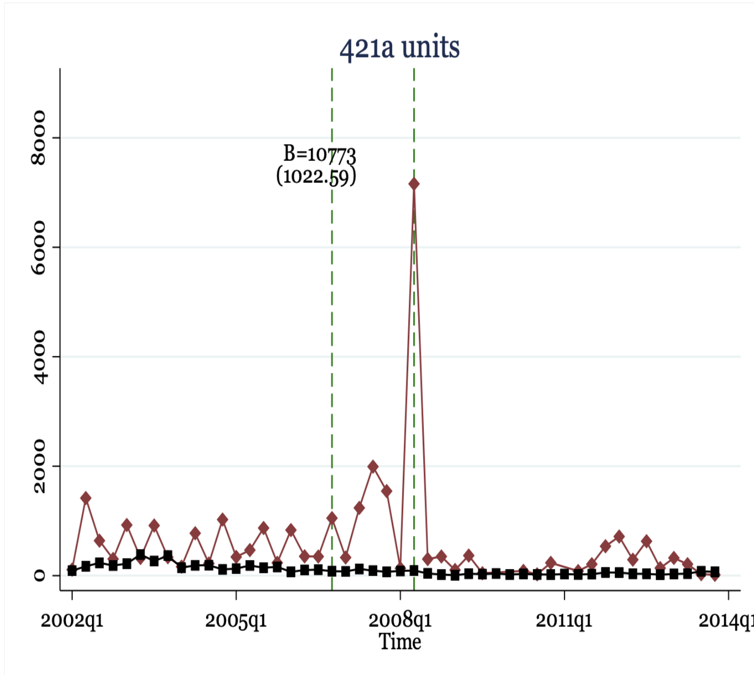


Figure A-3.14: Alternative counterfactual: Non-tax-exempt starts in the Non-exclusion regions

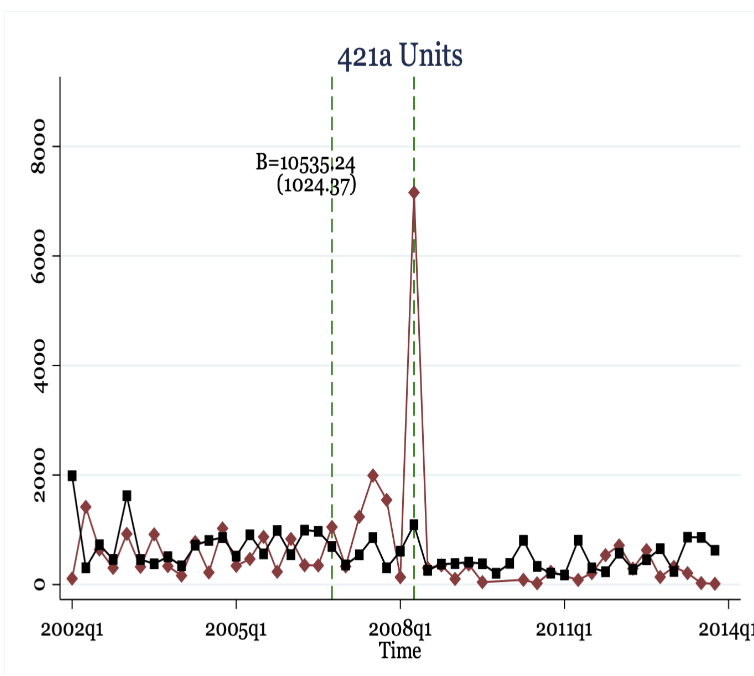
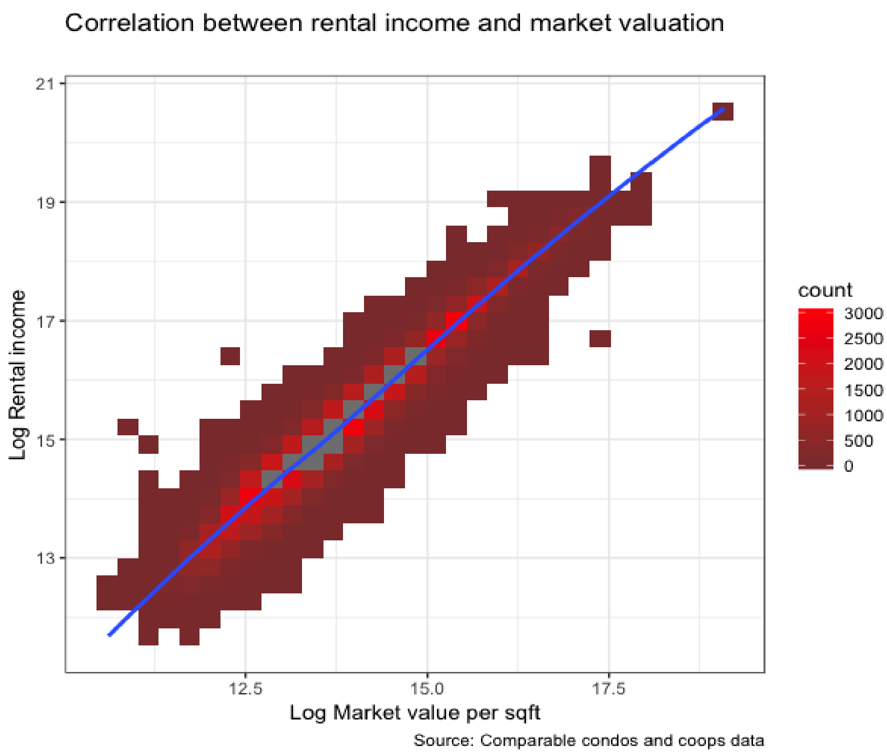


Figure A-3.15: Relationship between market valuation and rental income of the building



Notes: This figure plots the relationship between (log) rental income per square feet from a building and (log) market value per square feet. The latter assessed by the Department of Finance is used to determine annual property tax liability of a building. $R^2 = 0.94$. Data: Comparable condos and coops, Department of Finance 2009-15.

Figure A-3.16: Marginal Rent Effect as a function of distance

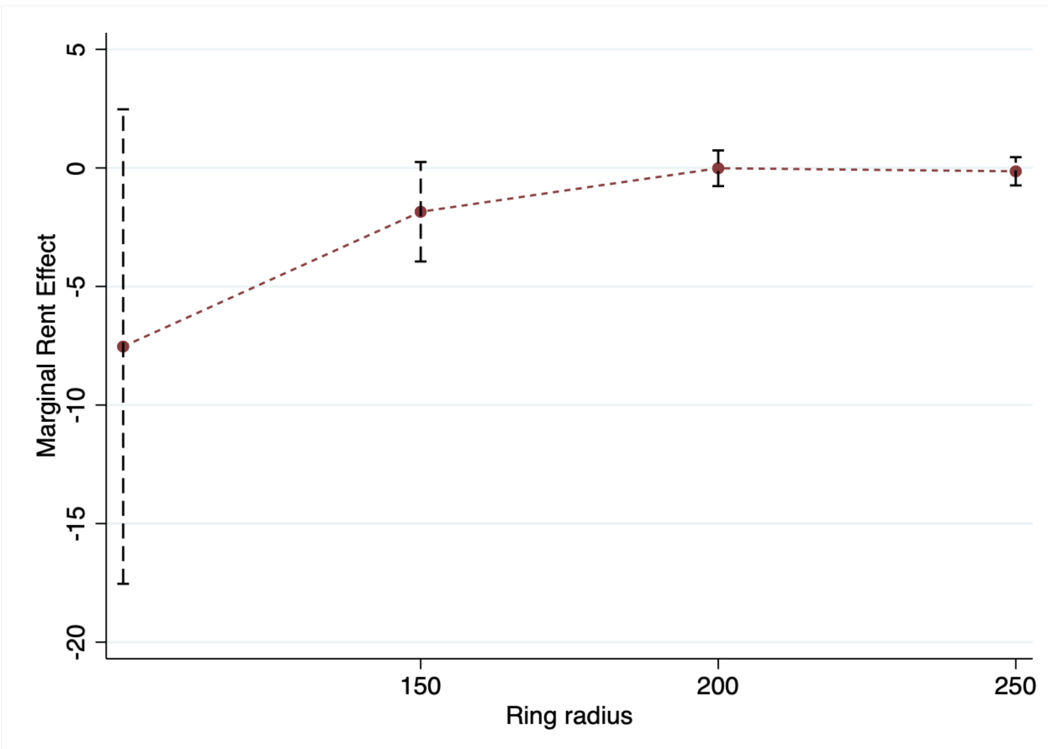


Table A-3.1: Summary statistics for non-notch and notch properties in 2015

	Mean	Median	SD	N	T-test
Building area	13152.702	4146	48192.697	4644	
	21344.83	5118	64792.689	745	-3.3073834
Res units	12.128984	3	45.001232	4644	
	20.296644	4	63.895911	745	-3.3579692
Lot area	3912.1051	2437	6996.7107	4644	
	4806.9987	2500	7289.0423	745	-3.1278333
No. Buildings	1.1302756	1	.65041666	4644	
	1.0939597	1	.33081607	745	2.3540461
Imputed Rent	1819.5576	1477.0795	1485.3728	556	
	2322.8212	2032.8167	1751.3924	152	-3.2385606
If High-income region	.2459087	0	.43067105	4644	
	.35436242	0	.478641	745	-5.8183106
Age	11.300195	11	4.1481946	4607	
	7.0175676	7	1.3459565	740	54.46332
Exclusion region	.22200689	0	.41564051	4644	
	.38389262	0	.486659	745	-8.5907678
If Manhattan/Brooklyn	.32988803	0	.47022285	4644	
	.52751678	1	.49957765	745	-10.103428

Notes: This table summarizes key differences in parcels that started outside the time-notch (first row) and those that started during the time-notch (second row) as of 2015. Sample restricted to parcels that contain non-condo buildings and that were built after 1990. *Building area* refers to the gross square feet residential area developed on the parcel. *High income* is a dummy that takes value one if the parcel is located in above median income census tract. *Imputed Rent* is inferred using Department of Finance's market valuation. *Exclusion region* is a dummy indicating if the parcel is located in exclusion regions which experienced tax increase as part of the tax reform. Data: Primary Land Use Tax Lot Output, 2015 and Department of Finance 421a exemption list 2015.

Table A-3.2: Effect of new tax-exempt residential projects on rents: IV Estimates

	Reduced Form	IV Estimate	OLS Estimate
$\mathbb{1}(Land == 1) \times Post$	0.0251		
	(0.018)		
$NotchProjects \times Post$		0.659	0.0285
		(0.029)	(0.060)
Constant	4.097		4.098
	(0.000)		(0.000)
Kleibergen-Paap Stat		37.20	
First stage		0.0381	
Observations	341651	341651	341651
Rings	24718	24718	24718

Notes: p-values in brackets. Outcome variable is building-year log rent per square feet. The endogenous treatment variable is the number of tax-exempt *units* received by an existing rental building within 150 meters in the time-notch. The instrument is a dummy which indicates whether a building had one or more than one vacant parels within 150 meters a year before the reform. Standard errors are clustered at the building level. All regressions include buiding, year and regionXyear fixed effects. Regressions are weighed by the number of residential units in the building. Data: Primary Land Use Tax Lot Output, New York City, 2002-16.

Table A-3.3: Alternative instrument 0 and ≥ 1 vacant parcels: IV Estimates

	RF	IV	OLS	RF	IV	OLS
$\mathbb{1}(Land >= 1) \times Post$	0.0454			0.0942		
	(0.000)			(0.000)		
$NotchUnits \times Post$		0.0488	0.000700		0.107	0.00128
		(0.003)	(0.088)		(0.000)	(0.001)
Constant	4.092		4.094	4.088		4.093
	(0.000)		(0.000)	(0.000)		(0.000)
Kleibergen-Paap Stat		15.04			15.36	
First stage		0.930			0.882	
Observations	355146	355146	355146	358218	358218	358218
Rings	25681	25681	25681	25681	25681	25681
Region F.E.	Census tract	Census tract	Census tract	Zipcode	Zipcode	Zipcode

Notes: p-values in brackets. Outcome variable is the building-year log rent per square feet. The endogenous treatment variable is the number of tax-exempt *units* received by an existing rental building within 150m in the time-notch. The instrument is a dummy which indicates whether a building had one or more than one vacant parels within 150 meters a year before the reform. Standard errors are clustered at the building level. All regressions include the buiding, year and regionXyear fixed effects. Regressions are weighed by the number of residential units in the building. Data: Primary Land Use Tax Lot Output, New York City, 2002-16.

Table A-3.4: Adjusting for spatial correlation: IV Estimates

	Reduced Form	IV Estimate	OLS Estimate
$\mathbb{1}(Land == 1) \times Post$	0.02		
	(0.02)		
	[0.02]		
$NotchUnits \times Post$		0.023	0.0006
		(0.01)	(0.07)
		[0.052]	[0.09]
Observations	341069	341069	341069

Notes: p-values adjusted for spatial correlation in parantheses. p values with standard errors clustered at the building level are reported in the square-brackets. Standard errors are adjusted according to Conley [2016]. Distance cutoff (for spatial correlation) = $1km$, Lag cutoff (for serial correlation) = 15. Yi Jie Gwee provided the code to adjust IV estimates. The code builds upon Conley [2016], Hsiang [2010], and Fetzer [link]. Observations are at building-year. Outcome variable is log rent per square feet. The endogenous treatment variable is the number of tax-exempt *units* within 150 meters from an existing rental building at the time-notch. The instrument is a dummy which indicates whether the building had one or zero vacant parels available within 150 meters a year before the reform. All regressions include building, year and 2000 census-tractXyear fixed effects. Regressions are weighed by the number of residential units in the building. Please refer to Section 3.2 for the measurement of rental income and sample construction. Data: Primary Land Use Tax Lot Output, New York City, 2002-16.

Table A-3.5: Effect of new residential investment on rents: Elasticity estimates

	RF	IV	OLS	RF	IV	OLS
$\mathbb{1}(Land == 1) \times Post$	0.02			0.05		
	(0.02)			(0.00)		
	[0.02]			[0.00]		
$\frac{NotchUnits}{H} \times Post$		1.86	0.033		4.95	0.05
		(0.01)	(0.01)		(0.00)	(0.00)
		[0.075]	[0.01]		[0.008]	[0.00]
Constant	4.096		4.097	4.092		4.093
	(0.000)		(0.000)	(0.000)		(0.000)
First stage		0.0129			0.0135	
Observations	341069	341056	341056	344212	357623	357623
Buildings	25639	25639	25639	25639	25639	25639
RegionXYear F.E.	Census tract	Census tract	Census tract	Zipcode	Zipcode	Zipcode

Notes: p-values with standard errors adjusted for spatial (1km) and serial correlation (15 periods) are reported in parantheses. p-values with standard errors clustered at building level in square brackets. Outcome variable is building-year log rent per square feet. The endogenous treatment variable is the number of tax-exempt *units* within 150 meters from an existing rental building at the time-notch, expressed as percentage of existing rental stock. The instrument is a dummy which indicates whether a building had one or zero vacant parels available within 150 meters a year before the reform. All regressions include building, year and regionXyear fixed effects. Regressions are weighed by the number of residential units in the building.

Table A-3.6: Leave a borough out: IV Estimate

	(1)	(2)	(3)	(4)	(5)
$\mathbb{1}(Land == 1) \times Post$	0.0207	0.0263	0.0227	0.0246	0.0241
	(0.289)	(0.086)	(0.067)	(0.050)	(0.051)
Kleibergen-Paap Stat	7.352	8.249	10.66	12.23	12.55
First stage	0.687	1.099	1.273	1.072	1.053
Observations	190521	270077	262145	304590	339271
Rings	24718	24718	24718	24718	24718
Borough removed	Manhattan	Bronx	Brooklyn	Queens	Staten Island

Notes: p-values in brackets. Outcome variable is log rent per square feet. Observations are building-year. The endogenous treatment variable is the number of tax-exempt *units* received within 150 meters from an existing rental building in the time-notch. The instrument is a dummy which indicates whether a building had one or zero vacant parels available within 150 meters a year before the reform. Standard errors are clustered at the building level. All regressions include building, year and 2000 census-tractXyear fixed effects and are weighed by the number of residential units. Please refer to Section 3.2 for the measurement of rental income and sample construction. Data: Primary Land Use Tax Lot Output, New York City, 2002-16.

Table A-3.7: Demographics Statistics: Exclusion and Non-exclusion regions

	Non-Exclusion region		*Exclusion region*	
	Mean	SD	Mean	SD
Monthly rent	991.49	430.08	1415.50	1063.67
Total Income	44783.87	43024.10	75165.60	118052.94
Non-white household	.49	.50	.36	.48
Above median income	.43	.49	.55	.49
Persons in household	2.49	1.54	2.07	1.31
Tenure	.47	.49	.48	.50

Notes: This table reports key demographics statistics in exclusion and non-exclusion regions in 2005. Sample restricted to include Manhattan, Brooklyn and Bronx because they allow better identification of sub-boroughs that were affected by the property tax reform. *Monthly rent* denotes the contractual rent reported in the survey. *Total income* refers to the sum of wage and non-wage household income. *Non-white household* is a dummy that takes value 1 if the household head is non-white. *Above median income* is a dummy that takes value 1 if the total household income is greater than the city median income. *Tenure* takes value 1 if the household moved into the current residence within past five years. Both *monthly rent* and *Total income* variables are expressed in 2015 US dollars. Data: New York Housing and Vacancy Survey, Occupied Units, 2005.

Table A-3.8: Selected High and Low Income Elasticity Industries

4-Digit NAICS Code	Description	Industry Income Elasticity
	Industries with High Income Elasticity	
6223	Speciality Hospitals (except Psychiatric and Substance Abuse)	2.4
5239	Other Financial Activities	2.2
5231	Securities and Commodity	2.18
5122	Sound Recording Industries	1.83
7114	Agents and Managers for Artists, Sports, Entertainers and Other Public Figures	1.58
7139	Other Amusement and Recreation Industries Industries	1.02
7224	Restaurants and Drinking Places	0.92
	Industries with Low Income Elasticity	
3323	Architectural and Structural Metals Manufacturing	-0.46
6241	Individual and Family Services	-0.51
3366	Ship and Boat Building	-0.56
4859	Other Transit and Ground Passenger Transportation	-0.63
5622	Waste Treatment and Disposal	-0.63
4451	Grocery Stores	-0.69
8111	Automative Repair and Maintenance	-0.93

Notes: This table presents income elasticities for 4-Digit industries. Income elasticity for an industry reflects the strength of association between zipcode renter income and the number of industry establishments in the zipcode. Intuitively, establishments belonging to an industry with high income elasticity are more likely to be located in a high-income zipcode. Each industry's elasticity is obtained as a coeffcient on log renter income in a regression of log number of establishments in an industry on the zipcode's mean log renter income in 2001. There are approximately 300 industries observed in New York City sample in 2001. Data: County Business Patterns 2001 and Census 2000.

A-4 Data

Tax-Pluto Data

PLUTO and MapPLUTO provide a detailed record of every piece of land in New York City, referred to as a *tax-lot*. An average tax plot in NYC consists of 1.26 buildings. PLUTO (short for "Property Land Use Tax [Lot] Output") lists properties at borough-block-tax lot level and provides useful information regarding the property such as address, number of residential and total units, residential and commercial square footage, year built, lot and building area, owner name and number of floors. MapPLUTO provides shape files containing precise geographic location of properties on the map.

Note that under NYC real property law, condos are assessed separately from the traditional tax lot (or a parcel) and are represented by a *condonium tax-lot* that is different from a *traditional* tax-lot. A traditional parcel may contain serveral buildings, each comprising of several condos. PLUTO data lists information at the *traditional tax-lot* level and not *condonium unit* level. Each parcel in NYC is uniquely identified by its borough-block-tax lot (referred to as a BBL). BBL consists of three numbers: the borough, which is one digit (coded alphabetically 1-5); the block number, which is upto five digits; and the lot number, which is upto four digits. Each condo, on the other hand is identified by borough-block- condo tax lot (referred to as *condonium* BBL). To identify all condos that share the same parcel, PLUTO data can be merged with another dataset called Property Address Directory (PAD) which contains the directory of condos and their billing BBLs. The Pluto and Map-pluto data was accessed from NYC's website in March 2018 and provides data updated as of October 2016. The link is as follows:

https://www1.nyc.gov/site/planning/data-maps/open-data/dwn-pluto-mappluto.page

Geographic Exclusion Boundaries

I obtained the map of exclusion regions from NYC's website. I digitized the map using QGIS to create a shape file which I then merged with PLUTO data to identify whether a parcel (identified by its BBL) is located in exclusion or non-exclusion regions. The original map can be found at the following address:

https://www1.nyc.gov/assets/hpd/downloads/pdf/GEA_Map.pdf

Property Address Directory

While PLUTO contains rich information at the *billing-lot* or building level and not at *tax-lot* level. While *tax-lot* and *billing-lot* are essentially same for all rental properties and coops, they differ for condos which are individualy owned and assessed separately from the building. To identify condos within a building, I use PAD (Property Address Directory) which provides a crosswalk between *billing* BBLs and *tax* BBLs. This directory also provides corresponding BIN (Building Identification Number) for each BBL which is useful in merging permits data with PLUTO and 421a dataset. The 2018 version of this dataset was downloaded from the following website in May 2018.

https://www1.nyc.gov/site/planning/data-maps/open-data.page

Investment or Permits Dataset

While PLUTO provides characteristics of properties in stock, more important to my analysis is daily permit issuance data, maintained by the Department of Buildings, which provides information on permits issued at the parcel-month level. This dataset contains universe of permits issued for any parcel in New York City from 2003 to 2014. A parcel undergoing any major alteration or construction must first obtain a permit from the Department of Buildings. These include permits issued for demolition (permit type denoted as 'DM'), for new building construction (permit type denoted as 'NB') and major alteration (permit type denoted as A1) that changes occupancy structure of the building. I restrict my analysis to DM, NB and A1 type permits only. Also, I focus on 'initial' issuance of permits and ignore 'renewal' permits in all my analysis.

See: https://data.cityofnewyork.us/Housing-Development/Historical-DOB-Permit-Issuance/bty7-2jhb/data

Properties with 421a Exemption or '421a dataset'

I downloaded the NYC Department of Finance's report on all properties receiving the 421a partial tax exemption listed by borough, block, tax-lot, neighborhood, building class, tax class and address. This dataset lists all condos and buildings that received the 421a Tax Exemption in fiscal year 2015-16. The combination of borough, block and tax-lot forms the unique property identifier (*tax-BBL*) which I use to merge this dataset with others.

See: https://www1.nyc.gov/site/finance/benefits/benefits-421a.page

Sales Data

The department of finance makes available the date on every transaction of a property located in NYC. This dataset is hosted on their website and covers the period 2003 to today. Each property, whether a condo, co-op or a building is identified by its unique tax BBL in this dataset.

See: https://www1.nyc.gov/site/finance/taxes/property-annualized-sales-update.page

Data on establishments and local businesses

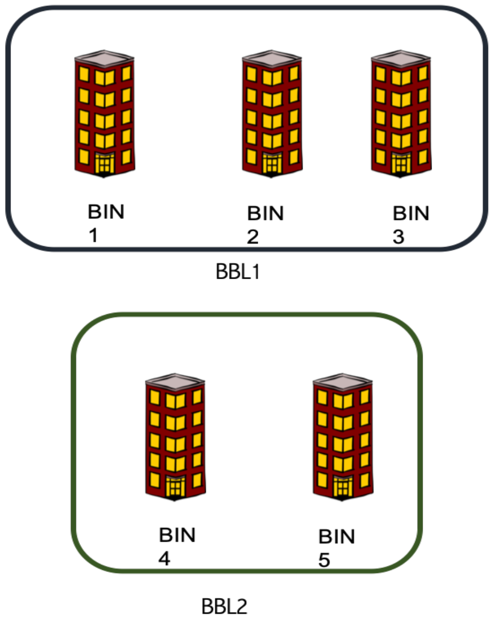
An important part of the exercise is to determine how local businesses react to the new residential investment brought by the reform. For this purpose, I use two main datasets. New York City requires certain businesses to apply for DCA (Department of Consumer Affairs) license before beginning operations. These include businesses such as sidewalk cafes, laundries, home-improvement contractors The dataset provides information on license creation date and the geocoded location of the business. This allows me to measure changes in licenses granted over time and space. A disadvantage of this dataset is that it includes selected businesses and major businesses such as retail and restaurants are excluded because they do not require a Department of Consumer Affairs license. Therefore, I supplement the analysis by using County Business Patterns 2001-15, which lists the total number of establishments in each 6-digit NAICS (North American Industry Classification System) industry located in a zipcode-year.

New York City Housing and Vacancy Survey

New York City's Housing and Vacancy survey (NYCHVS), sponsored by the New York

City Department of Housing Preservation and Development (HPD), is conducted every three years to comply with New York state and New York City's rent regulation laws. The survey uses most recent census data to survey households living in the city, either as tenants or homeowners. For each household, it provides information such as tenure, current and previous place of residence, income and race. The lowest geographic unit is sub-borough, which is a group of census tracts summing to at least 100,000 residents, determined by NYC HPD. The boundaries of sub-borough areas often approximate community districts.

Figure A-4.17: Geographies in New York City Datasets



Notes: This figure shows relevant identifiers in . BBL denotes a parcel (billing-tax-lot) which may contain one or more buildings. Each building is associated with a unique identification number called BIN. Investment (permits) data is at the building level (identified by unique BIN) whereas rents/market valuation dataset is at the parcel level (BBL). Further, sales dataset is either at the apartment (identified by the 'tax' BBL) or building level (identified by 'billing' BBL). A parcel contains approximately a building. The unit of analysis in the paper is a parcel (identified by a billing BBL).

A-5 Property taxes in New York City

Property tax for all classes, is determined according to the following formula:

$$\text{Annual tax} = \text{tax rate} \times [\underbrace{\text{assessment rate} \times \text{Annual Market Value}}_{AssessedValue} - \text{Exemption}] - \text{Abatement} \tag{12}$$

There are several components that determine a landlord or homeowner's annual property tax bill. First, assessment ratio determines the fraction of market valuation subject to property tax. For instance, this ratio is 6% for one-family homes and 45% for rental buildings. [60] Second, exemptions reduce the assessed value. Some of the available exemptions include STAR, Enhanced STAR, Senior Citizen and Disabled Homeowners' exemption. 421a property tax exemption, available for larger residential buildings with at least 3-4 units reduces assessed value to the pre-contruction assessed for a significant period.. Finally, abatements, similar to a tax credit, reduce the tax bill dollar for a dollar. Condo-coop abatement is a major abatement available to owners of condos and coops who use it for their primary residence. [61]

Tax rate, assessement ratio, market valuation determination and the availability of various abatements and exemption depend on the tax class a property belongs to. Properties in New York City are divided into four tax classes. Class 1 properties include one-to-three family homes. Class 2 properties include large rental buildings, condos and coops, properties that are focus of the analysis in this paper. Class 3 properties include utilities. Class 4 properties include commercial buildings, offices, factories and warehouses. Though both owner occupied units and apartments buildings are residential, they are subject to different tax rates and assessment ratios. While Class 1 properties are assessed at 6% of their full market value, class 2 properties are assessed at 45%.

The process of development in the city is quite similar to elsewhere. Developers buy vacant land and raise money; obtain permit from Department of Buildings to begin construction. They can chose to build family homes, purely rental buildings, or mixed condo buildings. [62]

A-5.0.1. Property taxes on Rental Properties in New York City

Property taxes form a significant share of operation costs of rental developments in New York. Tax rates, assessment formula and assessment rate all contribute to the high property tax. Rental buildings are assessed on the basis of their net rental income, where net rental income is defined as the difference between gross income and operating expenses [63] For this reason, all landlords of rental buildings greater than 10 units are required to submit Real Property

[60] This implies that at the tax rate of say 20%, a household living in a one-family home valued at $100,000 will pay an annual tax of $1,200.

[61] See `https://www1.nyc.gov/site/finance/taxes/property-assessments.page` for more details.

[62] There are almost no new co-ops built in recent decades. A co-op is an LLC where the owner of each unit owns shares equivalent to the size of his unit. The co-op board is responsible for the maintainence of the common spaces.

[63] In contrast, family homes are assessed on the basis of sales price of similar properties nearby.

Income and Expense form annually where they declare their annual revenues and expenses. The Department of Finance adjusts the net rental income for risk, liquidity and capitalization to obtain their annual market valuation. In general,

$$\text{Annual market value} = [\text{Rental Income - Expenses}] \times \frac{1}{\text{Capitalization rate}} \tag{13}$$

Capitalization rate is determined annually on the basis of risk, liquidity and management costs. It is equal to 16% on average which suggests a capitalization factor of around 6. Using equations 12 and 13, tax rate of 12% and capitalization factor of 6, we can determine the net tax burden on rental income (net of expenses) generated by the property tax. The effective tax rate on net rental income is given by:

$$ETR = \frac{12 \times 0.45 \times 6 \times NOI}{NOI} = 32.4\%$$

This suggests that upto 33% of the landlord's net rental income is spent towards the payment of the property tax, quite high.This suggests that the 421a property tax exemption that reduces the tax on new investment is quite valuable to a developer of rental property. Figure A-3.6 shows that a large share of buildings that benefit from 421a tax exemption are classified as C (rental walk-ups), D (rental buildings with elevator) or R (condos). Finally, Figure A-3.15 shows that the correlation between log rental income and log market valuation of rental properties is strong and close to 1. [64] This implies that annual market valuation serves as a good proxy for the rental income buildings.

A-6 Effect of the Tax Reform on Property Tax Across Regions

In this section, I illustrate that 2006-08 tax reform of 421a property tax exemption had differential impact on the three regions in Figure 1: Light shaded region (old-exclusion region, blue colored), dark unshaded region (new-exclusion region, dark blue colored), and light unshaded region (non-exclusion region, yellow colored). In particular, I show that in the light shaded region, the reform effectively removed the tax exemption by making onsite provision of affordable units mandatory. In dark unshaded region, the reform either did not affect tax incentives or it completely removed them, depending on the cost of providing affordable housing onsite. Finally, in the light unshaded regions, the reform did not alter exemption benefits significantly.

Light shaded regions (old exclusion region)

Prior to the reform, the developers were required to provide affordable housing but they could choose to do so offsite. By allowing the developer to build offsite in relatively low-rent regions, the offiste option provided significant cost savings to the developer. In fact, I show here that taking away the option to provide affordable housing offsite imposed compliance

[64] This uses 'comparable rental properties' data provided by DoF. This dataset provides comparable rental properties used to determine market valuation of nearby condos and coops. Condos and coops in NYC are assessed on the basis of their 'imputed rental income' instead of recent sales that family homes are assessed on.

costs under the property tax exemption equivalent to an increase of property tax between 4 and 5 percentage points. Given the statutory property tax rate of around 5.5% faced by a developer with no exemption, the tax-reform effectively made the property exemption in light shaded regions regions unattractive.

To see why this is the case, consider that an average rental building in light shaded region has 56 units and can expect to get at least \$34,820 (2015) USD per year from a single market rent unit. In contrast, a developer can expect to get at most \$14,013 from an affordable unit in the building. This is because for a 4 person family, the 50% Area Median Income (AMI) is $\$38,400$ and therefore, the maximum rent the developer can charge is 30% of this income limit (See HUD source for a list of AMI for different years.) [65]. Note that the maximum rent the developer can charge in an affordable unit is fixed throughout the city, irrespective of variation in local rents across neighborhoods within the city.

With onsite affordability requirement, the developer must keep 11 out of 56 units affordable with a discount of $\$34820 - \$14013 = \$20807$ on each affordable unit. Total cost imposed by onsite affordability requirement is therefore, \$228,877, which suggests an effective tax rate of $\frac{228877}{5*56*34820} \times 100 = 2.3\%$. If we use a higher capitalization factor (such 6), we get an effective tax rate of up to \$1.9%. Previously, with the offsite affordability option, this tax rate was effectively zero. Therefore, the 2006-08 reform removed the tax break and therefore, increased the effective property tax rate on future projects in light-shaded regions.

Dark unshaded regions (new exclusion region)

The major change brought by the reform in dark unshaded regions included the elimination of short exemption which provided exemption from property taxes for 15 years without affordable housing requirement. Whether or not the reform effectively altered the tax benefits in these regions depends on whether the second best option—long exemption with onsite affordable housing requirement—–is a worse option than obtaining no exemption at all. This in turn depends on how local rents compare with the affordable rent. In low-rent regions, the long exemption would still be a better deal because of low discount on affordable rents. On the other hand, in high-rent regions, long exemption with affordable housing requirement is less lucrative because of high discount on affordable units. This is because the rent a developer can expect from an affordable unit is fixed throughout the city, irrespective of variation in rents across neighborhoods within the city.

To see why location of a neighborhood within the dark region matters in determininng whethere long exemption is profitable, consider the following case. According to the raw data, an average rental building [66]has an annual market value of \$ 3,088,948 in 2015 US dollars. This implies an average annual property tax payment=\$166,803.

Net present value of this sum is given by:

$$NPV = \frac{166803(1-(1+i)^{-T})}{i}$$

[65]Because AMI varies from year to year, all calculations are based on 2008 AMI which is converted to 2015 USD. Additionally all calculations are based on income limit for a 4 person family, earning 50% of area median income. After 2012, all affordable units are advertised through online portal called *nycconnect*.

[66]i.e. a building classified as type C, D or S.

For $T = 25$ and $i = 15\%$, and no costs, the net present value of long tax exemption is $\$1,078,239$.[67] However, a developer must keep 20% of the units affordable to households with income between 30 and 100% of area median income, which adds to the cost under long exemption. The average annual income from a market rate rental unit in the dark regions in 2008 was about \$12,187 and a building had on average 46 units. For an affordable unit, the developer can expect annual rental income of atmost $\$14,013$. This is actually higher than the expected income from the market-rate unit, which suggests on average the costs under long exemption are minimal.

However, the average masks substantial heterogeneity in rents within the dark regions. For instance, 95th percentile average annual rental income is about $\$34,423$, which is much higher than the affordable rent limit. Annual cost of imposed by an affordable unit is then $= 34,423 - 14,013. = 20,410$ and total number of affordable units to be provided $= 9$. Using the net present value formula, we can see that the annual cost of long exemption for developer is then$=$ $\$183,690$, higher than the property tax savings under exemption.

Therefore, we can expect the reform to drastically cut benefits in high rent regions with no significant change in low rent region within the dark unshaded region. This further suggests that the future marginal tax in dark regions either did not change or increased by 5.5% (the statutory tax rate) after the reform.

Unshaded light regions (non exclusion region)

The only change brought by the reform in unshaded light regions was the introduction of a cap on benefits under the short exemption. Because these regions are low-rent, the discount on an affordable unit is minimal and the long exemption is the most preferred option which was effectively unaffected by the reform. For instance, an average rental building in light unshaded region has an annual market valuation of about 2,755,881 US 2015 dollars and has around 65 units. This suggests that the rental income of the average building is about $\$459,313$ or around 7000 per rental unit, much lower than the average income from an affordable unit. Therefore, long exemption imposes non-zero costs and the exemption lasts for a longer duration implying that long exemption is a clear winner in these regions. This further suggests that the change in effective property tax here as a result of the reform was insignificant.

[67] I use $i = 15$ because this interest rate is consistent with the average capitalization rate used by Department of Finance in calculating market valuation of apartment buildings. DoF uses the capitalization rate to calculate the present discounted value of the annual rental income, which serves as the tax base for the property tax calculations for that year. See here, for example. In general, with capitalization rate of 15%, market valuation of a property with rental income R is equal to $\frac{R}{0.15} = 6.6 \times R$. Capitalization rate changes over the years, but is largely in the range of $\{13, 17\}$.

A-7 Short Term Outcomes in the Time Notch

Let us suppose there are three periods $j \in \{0, 1, 2\}$. The world ends in period 2 and outcomes in period 2 refect the long-run. While there is uncertainty in periods 0 and 1, it disappears in period 2 (the long-run). Consider a region where the demand for housing in each period is increasing in the level of amenities and decreasing in the net-of-tax rent R_j:

$$D_j = \frac{A_j}{R_j(1+t)}$$

The region starts with the baseline stock S_0. Assume a depreciation rate of δ. Housing stock in any period is given by: $S_j = I_{j-1} + (1-\delta)S_{j-1}$. The timing and long-run investment elasticities (with respect to the property tax) are given by ϵ_t and ϵ_l respectively. Given a property tax t, the equilibrium in period j is given by R_j such that $D_j = S_j$. An important piece is that the amenities in region j, A_j respond to the current investment. New residential investment attracts more businesses to the region which makes it more attractive for investment in the future. Similar to Diamond [2016], I assume that amenities respond to the stock of residents at a point in time with a constant elasticity ϵ_a,

$$A_j = (S_j)^{\epsilon_a}$$

Policy 1: Unanticipated property tax increase

First, consider the when the tax increase is announced and implemented in period 1. This is illustrated in Figure 4a. Period 0 investment is unchanged because the developers are not aware of the policy change in period 0. Consequently, the decline in the net-of-tax rents in period 1 is equal to the tax increase.

$$\Delta log R_1 = -\Delta log(1+t) < 0 \tag{14}$$

In the short-run, landlords bear the entire burden of the property tax increase as the supply does not adjust quickly. However, in the long-run, developers shift some of the incidence onto the tenants through changes in investment. In fact, the lower net-of tax returns decreases period 1 investment by $\epsilon^l \Delta log R_1$, where ϵ^l is the rent elasticity of investment as discussed in the previous section. Lower period 1 investment affects period 2 rents through two channels: lower housing stock (the standard supply channel) and amenities (the indirect demand channel). In particular, the rent paid by the tenants in the long-run is given by:

$$\Delta log R_2^g = \Delta log A_2 - \Delta log S_2 = (\epsilon_a - 1)\epsilon_l \Delta log(1+t)$$

$$= (\epsilon_a - 1)\epsilon_l \Delta log(1+t) \tag{15}$$

The rent paid by the tenants in the long-run is higher because a higher property tax reduces the long-run housing stock (a standard public finance result). However, this result holds with endogenous amenities only when the amenities in the region do not decline drastically in response to a lower long-run housing stock, that is $\epsilon_a < 1$. In fact, the rents paid by the tenants decrease in response to a property tax increase when a drop in the amenities makes the region less desirable. Tenants in that case must be compensated in the form of lower rents. In the end, which effects dominate (amenity effect which decreases the rent paid vs the supply effect which increases the rents paid) depends on the relative elasticities.

Policy 2: Anticipated property tax increase

Now suppose that in period 0 the government announces that the property tax will increase, beginning period 1. In the long run, any short-term investment timing responses are reversed and the long-run housing stock falls by $\epsilon_l \Delta log(1+t)$ and,

$$\Delta log R_2^g = (\epsilon_a - 1)\epsilon_l \Delta log(1+t) \tag{16}$$

While the long term outcomes 15 and 16 are the same, the medium term outcomes, characterized by rents in period 1 differ. Because the developers are aware of the future tax increase, they have incentives to move the projects from period 1 to period 0 as illustrated in Figure 4b. High investment in period 0 increases the period 1 housing stock given by, $\Delta log S_1 = \epsilon^t \Delta log(1+t)$, where $\epsilon_t > 0$ denotes the timing property tax elasticity of residential investment, as discussed in the previous section. The period 1 *net-of tax rent* is given by:

$$\Delta log R_1 = (\epsilon_a - 1)\epsilon^t \Delta log(1+t) - \Delta log(1+t)$$

The above equation is positive when: $\epsilon_a > 1 + \frac{1}{\epsilon_t}$.