

# A Test for Pricing Power in Urban Housing Markets\*

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## Abstract

The presence of pricing power in housing markets significantly impacts our understanding of the housing supply. It biases estimates of housing production functions, supply elasticities, and the effects of land-use policies as well as the results of quantitative spatial models. We test for the existence of pricing power in the New York City rental market. Using tax policy changes, we conduct complementary difference-in-differences and instrumental variable analyses. An idiosyncratic increase in a single building's costs leads to a proportional rent increase, holding market-level rents constant. Our findings support the existence of pricing power and challenge the prevailing perfect competition framework.

Keywords: housing supply, market power, housing demand

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# 1 Introduction

Housing supply constraints constrict the size of cities and are a major source of economic loss via spatial misallocation (Saiz, 2010; Hsieh and Moretti, 2019). However, our understanding of the housing supply—and therefore a substantial portion of other theoretical and empirical results in urban and spatial economics—rests on the untested assumption of perfect competition. Pricing power’s existence in housing markets as a supply constraint has ramifications for analyses of urban land use policies, estimates of cost elasticities, and counterfactual estimations from quantitative spatial models.

This paper investigates whether individual owners of multi-unit housing have pricing power over their rental units by testing for the pass-through of idiosyncratic cost shocks onto rents. We collect data on building rental income and expenses in New York City (NYC) and isolate a series of tax policy shifts that result in building-specific tax burden changes. We implement two complementary analyses: a difference-in-differences and an instrumental variable approach. We find that lessors exhibit cost pass-through behavior that is not possible when housing is provided competitively.

We rely on a simple difference between price *taking* and *setting*. Any cost shift experienced by a lessor can be mechanically decomposed into two components: a mean cost shift shared by all lessors in the market and an idiosyncratic shift deviating from the mean. When lessors operate in competitive markets, the demand faced by each individual lessor—which we refer to as *residual demand*, contra market-level or aggregate demand—is perfectly elastic. Because the common component of a cost shift is shared market-wide, it affects rents by adjusting aggregate supply. However, the shock’s idiosyncratic component does not aggregate, does not alter market rents, and cannot be passed through. By contrast, when residual demand is downward sloping, both components affect rents. Our complementary empirical strategies isolate idiosyncratic shocks from market-level

fluctuations and examine the response of rents to those shocks in order to evaluate the slope of residual demand.

We generate a dataset of NYC multifamily rental buildings' rental income and leasing expenses. We collect data from the NYC Department of Finance's (DOF) publicly-posted communications with individual lessors which explain how their assessments are calculated. DOF reports back to lessors the income and leasing expenses they reported in tax filings. We merge these communications with information from public sources and supplement the data with an apartment-level panel dataset on rents and building characteristics from the New York City Housing and Vacancy Survey (NYCHVS).

We then use a series of tax policy changes between 2007 and 2019 to test for the pass-through of idiosyncratic shocks. First, we exploit an unannounced change in assessment procedures that lowered taxes on certain buildings by nearly half. Initially, {4, 5} unit buildings were assessed differently from slightly larger buildings, resulting in a large differential tax burden. In 2011, without prior public notification and "on the advice of counsel," the DOF harmonized assessment procedures, cutting taxes on {4, 5} unit buildings by 45%. Comparing unit rents before and after this shift with those in the NYCHVS's next largest class, {6, 7, 8, 9} unit buildings, we find that rents fell 11% for the treated group relative to the controls' rents. As Section 3 shows, this difference-in-difference estimate isolates the response of rents to the idiosyncratic component of the policy's shock so long as both groups share a market. Interrogating this assumption, we look for pre-trends and imbalances in unit and renter characteristics. We find neither, supporting the conclusion that the idiosyncratic component of the policy's effects were passed-through onto rents.

Our second approach follows a large literature testing for imperfect competition by isolating idiosyncratic firm shocks in a number of settings: [Amiti, Itskhoki, and Konings \(2019\)](#); [Paciello, Pozzi, and Trachter \(2019\)](#); [Muehlegger and Sweeney \(2022\)](#), and espe-

cially [Garin and Silvério \(2023\)](#).<sup>1</sup> After 2010, large buildings' net incomes were converted into valuations using capitalization rates derived from an annually-changing, citywide formula. We use formula changes in conjunction with buildings' earliest reported (2007) income to construct a synthetic tax IV identifying building-idiosyncratic cost shocks. Comparing buildings within the same Census tract, we find that a 10% increase in the idiosyncratic component of a buildings' predicted taxes is associated with a 0.3% increase in rents. Instrumenting for total costs using reported leasing costs, this corresponds to a total pass-through *rate* of over 100%.<sup>2</sup> The positive pass-through rates from both approaches indicate that lessors pass idiosyncratic cost shocks onto renters, and are consistent with finite residual demand elasticities and substantial curvature, indicating that lessors' pricing behavior is far from negligible.<sup>3</sup>

To test that our instrument plausibly identifies idiosyncratic shocks, we conduct several "placebo tests," (following [Garin and Silvério, 2023](#)), correlating our instrument with observed outcomes at *other* buildings. In our context, the exclusion restriction requires that our instrument is uncorrelated with rents through the error term. In the language of residual demand, idiosyncratic cost shock should be uncorrelated with residual demand shifters, such as the rents (or determinants thereof) of close competitors. For our test, we regress each building's instrument on the building's  $n$ th-nearest neighbor's rent, which are the competitors most likely to embody correlated demand or sorting on other unobservables. We find no evidence of correlation between the instrument and these placebo rents. We repeat this placebo test using non-spatial proximity, showing competitor instrument values are uncorrelated with price changes when competitor proximity is

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<sup>1</sup>Cost pass-through has been used as evidence of market power in other settings as well ([Brissimis and Kosma, 2007](#); [Fabra and Reguant, 2014](#); [Loy, Weiss, and Glauben, 2016](#); [Duso and Szücs, 2017](#)).

<sup>2</sup>Over-shifting is consistent with sufficiently convex demand ([Weyl and Fabinger, 2013](#); [Pless and van Benthem, 2019](#)).

<sup>3</sup>We focus on rejecting the null. Conditional on doing so, further extrapolating from our results to elasticities requires additional assumptions on market-level shocks' heterogeneous effects.

measured using the other parcel observable characteristics available to us. We further show our results are robust to alternative market definitions, such as spatially continuous and overlapping markets and non-geographic markets segmented by parcel observables, and to controlling for initial rent interacted with a time trend.

Our concluding discussion highlights three possible sources of pricing power (concentration, differentiation, and search and capacity constraints) and discusses the implications of our findings. The existence of pricing power in real estate markets is consequential for the study of spatial misallocation and housing scarcity, the costs and consequences of housing policy, estimates of the production function for housing and measures of housing supply elasticities, and counterfactual analyses in quantitative spatial models.

A new source of supply constraints, pricing power generates housing scarcity and is a contributing factor to spatial misallocation, which is implicated in large-scale economic losses (Hsieh and Moretti, 2019). With pricing power, *laissez-faire* policies would not generate competitive levels of housing supply.<sup>4</sup> Subsequently and second, pricing power forces a reexamination of our understanding of the effect of urban housing policy. Glaeser and Gyourko (2018) use the wedge between costs and rents to measure the impact of zoning. However, pricing power also generates a wedge between rent and marginal cost. Furthermore, in the presence of pricing power, spillovers due to cross-elasticities can complicate explicit or implicit marginal-cost assumptions in carefully designed studies of zoning reforms (Anagol, Ferreira, and Rexer, 2021).

Third, pricing power affects existing estimates of housing supply functions and elasticities, which are important inputs into many other literatures. The conceptual frameworks underpinning these empirical estimates rely on the assumption that supply is provided competitively (Green, Malpezzi, and Mayo, 2005; Baum-Snow and Han, 2023; Combes, Duranton, and Gobillon, 2021). If perfect competition is not a tenable assumption, these

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<sup>4</sup>Appendix A.3 shows pricing power reduces density and increases sprawl in a monocentric city.

estimates may need to be revisited.<sup>5</sup> Our results impact a major downstream user of these elasticities: structural spatial models (Ahlfeldt et al., 2015; Severen, 2023; Brinkman and Lin, 2022). Markups and owner behavioral responses via changing markups could bias both estimates of spatial reallocation and its welfare effects in counterfactual equilibria.

Three literatures do implicitly assume downward sloping demand for housing. Arnott (1989); Arnott and Igarashi (2000); Basu and Emerson (2003) examine monopoly power as a theoretical justification for rent control.<sup>6</sup> An empirical literature examines the effects of housing ownership concentration (Raymond et al., 2016; Cosman and Quintero, 2021; Austin, 2022; Xiao, 2022; Gurun et al., 2023). Finally, in a large empirical literature on bargaining—“double monopoly”—markets are *sometimes* modeled with price posting in combination with sorting-and-matching, which relies on an implicit, stochastic downward-sloping residual demand (e.g., Harding, Rosenthal, and Sirmans, 2003; Ihlanfeldt and Mayock, 2009; Genesove and Han, 2012; Piazzesi, Schneider, and Stroebel, 2015; Glaeser and Nathanson, 2017; Gilbukh and Goldsmith-Pinkham, 2019; Bracke, 2021; Turnbull and van der Vlist, 2022). Our finding that demand for individual buildings is downward sloping is an untested but necessary precondition for the implicit theoretical pathways of interest in these literatures.

We bridge a disconnect between the industrial organization literature on market power, where our null hypothesis may seem obviously false, and urban economics, where perfect competition is both the prevailing and a very strongly held null. We propose the prevailing hypothesis in urban economics as our null, and test it using differential predicted behavior.

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<sup>5</sup>For example, the estimates found in Combes, Duranton, and Gobillon (2021) may be consistent with pricing power up to a fixed proportion under the added assumption of constant demand elasticity.

<sup>6</sup>Diamond, McQuade, and Qian (2019) consider rent controls’ effects on exit, which we discuss in A.1.

## 2 Data

We construct a building-level dataset spanning 2007 to 2019 for private, multi (4+) unit buildings in NYC.<sup>7</sup> While NYC is unique among US cities in many respects, it shares many features of build up cities, including limited entry, and a wide variety of local densities. We combine our collected data on rents and expenses from public letters to landlords with two public datasets of administrative building-level records: the Primary Land Use Tax Lot Output (PLUTO) and the Final Assessment Roll (FAR). In NYC, rental buildings are taxed based on rental income. Every year, lessors report revenue and costs to the DOF on Real Property Income and Expense forms (RPIE) for tax assessment purposes. To explain property tax calculations, the DOF sends letters with this information and building-specific capitalization rates (used to calculate market values) back to lessors. We collect these letters from an online public portal. The PLUTO and FAR, available from 2002 and 2007 respectively, provide data on buildings' location, zoning, assessed values, age, and years since renovation. We use the American Community Survey to allocate rental households to buildings to estimate building vacancies.

We supplement this data with the triennial NYCHVS, which provides unit-level rents and building characteristics (number of units and floors in the building and its location and reported condition, the presence of an elevator, the resident's tenure, and the length of the lease), over 2002-2017.<sup>8</sup>

Appendix Tables [A1](#) and [A3](#) present summary statistics for our samples. Appendices [B](#) and [C](#) discuss details of sample construction and the NYC regulatory environment

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<sup>7</sup>We omit Staten Island buildings due to the low number of large rental buildings per tract.

<sup>8</sup>The survey has a decadal sampling structure where units are followed up with twice in a decade before the sample refreshes using the decennial census. Our main specifications using these data use the provided sample weights. Appendix [D](#) shows results are unaffected by their exclusion. Census tract information is not available in these data. We use the smallest available geography, sub-borough-area (SBA), instead.

(including rent stabilization).<sup>9</sup>

### 3 Conceptual Framework

Our test for pricing power hinges on the differential response of firms to cost shocks in perfectly versus imperfectly competitive environments. Perfect competition implies firms face perfectly elastic residual demand. In response to a cost shock, individual lessors can adjust quantity (or exit). These quantity responses can aggregate to a market-level supply shift, which adjusts market-level rents. This, in turn, is seen by lessors as a shift in their residual demand. However, if a cost shift applies to just a single building, it does not aggregate, and does not affect rent. By contrast, under imperfect competition, where residual demand is downward sloping, a shock to a building's costs impact the building's rent regardless of whether it aggregates into a market-level supply shift. Appendix A.1 shows these distinctions using general equilibrium models. There we discuss the market setting in depth, including our treatment of non-continuous supply, supply constraints, our interpretation of quantity changes in the presence of discrete units, the nature of the marginal leasing costs we observe, and entry and exit. Appendix A.2 elaborates on differential pass-through in perfect versus imperfect competition.

We consider policy changes that affect the marginal cost of leasing building units. For each building  $j$  in market  $m$  in period  $t$ , the log change in the building's marginal cost can be decomposed into a common component and an idiosyncratic component:

$$d \ln(mc_{jmt}) := \underbrace{\Delta_{mt}}_{\text{Common}} + \underbrace{\epsilon_{jmt}}_{\text{Idiosyncratic}}, \quad (1)$$

where  $\Delta_{mt}$  is the average log change in marginal costs across all buildings in the market

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<sup>9</sup>Rent stabilization biases our results towards zero, as it is not systematically binding. We include robustness checks that exclude highly stabilized buildings.



and  $\epsilon_{jmt}$  is the residual change of the policy on  $j$  in addition to the average change.

In both market settings, rental prices respond in qualitatively similar ways to shifts in  $\Delta_{mt}$ , which aggregate across firms and shift residual demand. However, rents only respond to idiosyncratic shifts  $\epsilon_{jmt}$  under imperfect competition.<sup>10</sup> Our empirical strategy aims to isolate variation corresponding to idiosyncratic marginal cost changes,  $\epsilon_{jmt}$ , and test the hypothesis that the elasticity of rents with respect to those changes,  $d \ln(r_{jmt})/\epsilon_{jmt}$  is zero.<sup>11</sup>

With data on rents and marginal cost we could estimate the following regression:

$$\ln(r_{jmt}) = \beta \cdot \ln(mc_{jmt}) + \lambda_j + \lambda_{mt} + U_{jmt}, \quad (2)$$

where  $\lambda_j$  is a building fixed effect,  $\lambda_{mt}$  is a market-time fixed effect, and  $\beta$  is the average building rent-marginal cost elasticity. Although  $\beta$  appears to identify the elasticity of rents to total marginal cost shifts, the two fixed effects absorb part of the relevant variation. Decomposing equation 2 into building average terms (denoted with upper bars) and deviations, we obtain:

$$\overline{\ln(r_{jmt})} + d \ln(r_{jmt}) = \beta \left( \overline{\ln(mc_{jmt})} + d \ln(mc_{jmt}) \right) + \lambda_j + \lambda_{mt} + (\overline{U_{jmt}} + u_{jmt}) \quad (3)$$

$$\implies d \ln(r_{jmt}) = \beta (d \ln(mc_{jmt})) + \tilde{\lambda}_j + \lambda_{mt} + u_{jmt} \quad (4)$$

$$= \beta (\Delta_{mt} + \epsilon_{jmt}) + \tilde{\lambda}_j + \lambda_{mt} + u_{jmt} \quad (5)$$

$$= \beta \epsilon_{jmt} + \tilde{\lambda}_j + \tilde{\lambda}_{mt} + u_{jmt}, \quad (6)$$

where we substitute equation 1 into 4. Thus, the market-time fixed effects absorb common

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<sup>10</sup>For a visual representation of this analysis, see figures and discussion in Appendix A.2.

<sup>11</sup>Weyl and Fabinger (2013); Pless and van Benthem (2019); Ritz (2019) study *market-level* pass-through, which we cannot implement without supply elasticities. Following Amiti, Itskhoki, and Konings (2019); Paciello, Pozzi, and Trachter (2019); Muehlegger and Sweeney (2022), we highlight that under pure competition *idiosyncratic* shocks do not affect prices, only quantities.

shocks, and so when observed building-level marginal cost variation is used in conjunction with building and market-time fixed effects, the coefficient  $\beta$  identifies the elasticity of rents with respect to idiosyncratic marginal cost shocks,  $\epsilon_{jmt}$ . Intuitively, by “holding fixed” the market-level, the market-time fixed effects account for the common component of marginal cost shifts (and other forces) resulting in residual demand curve shifts.

With exogenous idiosyncratic marginal cost variation and no confounding unobserved variation (e.g.,  $u$ ), we could directly estimate the rent-marginal cost elasticity. However, as typically with observed data, we cannot be assured that the idiosyncratic shocks are uncorrelated with residual demand factors; e.g., sub-tract spillovers on neighboring buildings’ rents. To identify the pass-through elasticity, we therefore employ two strategies: a difference-in-differences specification for buildings in the same market but with differential marginal cost shocks, and an instrumental variable specification that shifts  $\epsilon_{jmt}$ .

To implement the former, we need two sets of buildings,  $\{T, C\}$ , with similar costs and market trends where one group is treated and another not ( $\epsilon^C = 0$ ):

$$d \ln(r_{jmt}^T) - d \ln(r_{jmt}^C) = (\beta \cdot d \ln(mc_{jmt}^T) + \lambda_{mt} + u_{jmt}^T) - (\beta \cdot d \ln(mc_{jmt}^C) + \lambda_{mt} + u_{jmt}^C) \quad (7)$$

$$= (\beta \cdot [\Delta_{mt} + \epsilon_{jmt}^T] + \lambda_{mt}) - (\beta \cdot [\Delta_{mt} + \epsilon_{jmt}^C] + \lambda_{mt} + u_{jmt}^C) \quad (8)$$

$$= \beta \cdot \epsilon_{jmt}^T. \quad (9)$$

The DID estimate yields  $\beta E[\epsilon^T]$ , if the market-time FEs absorb the common marginal cost and demand shocks and there is no differential selection in unobserved demand shifters.

We stress that this is true even when the treatment is large and generates a market-wide shift. The DID estimate still identifies the effect of the shock’s idiosyncratic component by subtracting the market effect via the control buildings. Put differently, our setting requires an assumption weaker than SUTVA: if the two groups share the same market effect (violating SUTVA), then the DID estimate identifies the idiosyncratic effect. However, as

discussed in Section 4, SUTVA violations would complicate interpretation of the estimate.

For the latter strategy, we employ an IV approach to estimate equation (2).<sup>12</sup> For this, we need a variable,  $Z$ , that is, conditional on building and market-time FEs, correlated with  $\epsilon$  but uncorrelated with unobserved shifters:

$$\text{First Stage: } \ln(mc_{jmt}) = \pi \cdot Z_{jmt} + \lambda_j^{\text{FS}} + \lambda_{mt}^{\text{FS}} + v_{jmt} \quad (10)$$

$$\text{Structural Eq: } \ln(r_{jmt}) = \beta \cdot \ln(mc_{jmt}) + \lambda_j^{\text{SE}} + \lambda_{mt}^{\text{SE}} + U_{jmt} \quad (11)$$

$$= \beta \cdot \epsilon_{jmt} + \tilde{\lambda}_j^{\text{SE}} + \tilde{\lambda}_{mt}^{\text{SE}} + u_{jmt}, \quad (12)$$

with assumptions  $\text{Cov}(\epsilon_{jmt}, Z_{jmt} \mid \lambda_j, \lambda_{mt}) > 0$  and  $\text{Cov}(u_{jmt}, Z_{jmt} \mid \lambda_j, \lambda_{mt}) = 0$ .

## 4 Evidence from a Tax Regime Shift

In this section we implement the specification in equation (8) by exploiting an unannounced shift in tax policy for a subset of rental buildings. The shift, implemented for 2011 taxes, nearly halved the tax burden on  $\{4, 5\}$  unit buildings. We assign these as group  $T$  and the next largest size class in the NYCHVS, 6 – 9 unit buildings, as group  $C$ . As shown above, if these groups face similar cost and demand trends before and after the policy change, then we estimate the pass-through of idiosyncratic leasing costs onto rents.

### 4.1 Policy and Specification

In NYC, all properties must be assessed based on their market value; however, the DOF has wide latitude in implementing assessment procedures to assign market values. Prior to 2011, DOF used different methodologies to calculate market values of  $\{4, 5\}$  and 6 – 10 unit buildings, using comparable buildings' sales for the former and income for the latter.

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<sup>12</sup>For expositional reasons, we describe a simple first difference; however, in our panel setting with  $> 2$  time periods, the building FE yields deviations from the within-building time-average of the variables.

In 2011 for taxes due in 2011, “on the advice of counsel” and without prior warning, the DOF harmonized these procedures, imposing the 6 – 10 methodology on {4, 5} unit buildings (New York City Department of Finance, 2012).

We first estimate equation 8 in a difference-in-differences specification and then present the corresponding event study (ES) plots of dynamic treatment effects. In Appendix D, we present robustness checks, including placebo policy groups and differing control variables and building subsamples.

Because building income data is unavailable for {4, 5} unit buildings prior to the policy shift in our scraped data, we rely on the NYCHVS for the rent analysis, but use DOF data to observe the effect of the policy on tax. We use building controls as available, including controls for building and location-year fixed effects, the number of units and floors in the building, the presence of an elevator in and reported condition of the building, and the tenure and length of the lease.<sup>13</sup>

We run the following specifications (for buildings  $j$  in location  $m$  in year  $t$ ):

$$\text{DID} \quad Y_{jmt} = \theta_1 1[\{4, 5\}]_j + \theta_2 1[t > 2010]_t \cdot 1[\{4, 5\}]_j + \theta_3 X_{jmt} + \theta_4 D_{mt} + \varepsilon_{jmt} \quad (13)$$

$$\text{ES} \quad Y_{jmt} = \theta_1 1[\{4, 5\}]_j + \sum_{s=\bar{t}}^{\bar{t}} \theta_s D_{jmt}^s + \theta_2 X_{jmt} + \theta_3 D_{mt} + \varepsilon_{jmt}, \quad (14)$$

for outcomes (1) log assessed taxes per unit and (2) log unit rent, where  $D_{jmt}^s$  are indicators for each years interacted with treatment (omitting 2008),  $X_{jmt}$  are building observables, and  $D_{mt}$  are SBA-year fixed effects. We use NYCHVS provided sample weights. However, Appendix D shows results are unaffected by their exclusion. In addition to absorbing the common component of the shock, the parallel trends assumption requires that other market forces that impact demand are equally distributed between the groups.

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<sup>13</sup>For locations, we use tracts in the DOF data and sub-borough-areas (SBA) in the NYCHVS data, where tract information is unavailable. SBAs are collections of tracts and are the lowest level of publicly disclosed building location in the NYCHVS.

Table 1: Difference in Difference Results

Panel A: Log Assessed Property Tax per Unit			
	(1)	(2)	(3)
$1[t > 2010] \cdot 1[\{4, 5\}]$	-0.66 (0.01)	-0.60 (0.01)	-0.60 (0.01)
Building Controls	N	N	Y
Tract-year FEs	N	Y	Y
Unique Buildings	54,569	54,445	53,462
Observations	655,853	654,143	653,159
Panel B: Log Unit Rent			
	(4)	(5)	(6)
$1[t > 2010] \cdot 1[\{4, 5\}]$	-0.05 (0.04)	-0.12 (0.03)	-0.12 (0.02)
Building Controls	N	N	Y
SBA-year FEs	N	Y	Y
Observations	8,259	8,259	8,259

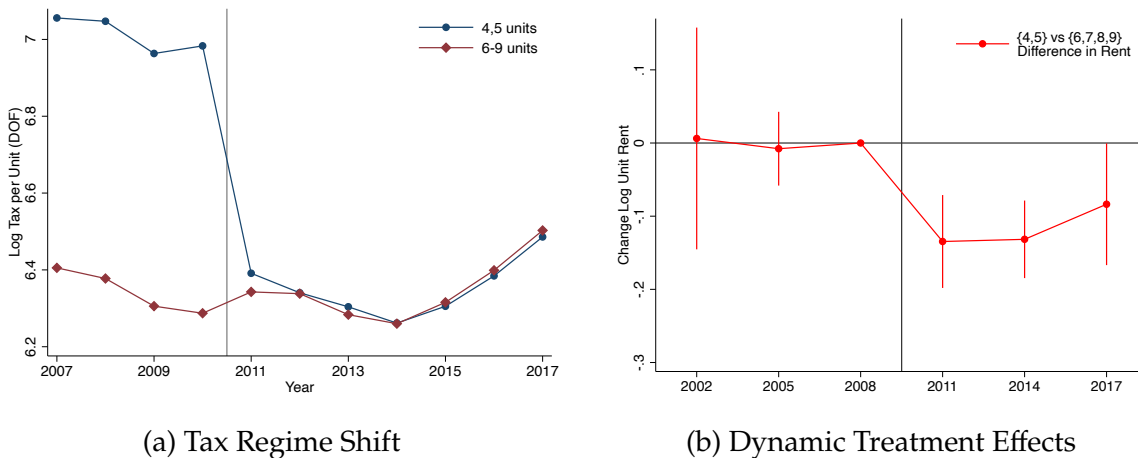
*Note:* The table reports two sets regressions using tax reform variation that harmonized capitalization rates for small rental buildings in 2011. Panel A reports three DID specification with log assessed property tax per unit as the dependent variable using annual 2007-2017 data from the NYC DOF panel of rental buildings. Panel B reports three DID specifications with log unit rent as the dependent variable using triennial 2002-2017 data from the NYCHVS. Columns (1) and (4) omit controls; columns (2) and (5) include tract-by-year FEs and sub-borough-area-by-year FEs (respectively); columns (3) and (6) augment the previous specifications with building FEs and building controls (respectively). Standard errors in Panel A are clustered by tract while for Panel B they are clustered by sub-borough-area.

## 4.2 Results

Table 1 reports DID coefficient results in two panels. Panel A displays results of the effect of the regime shift on log average unit property taxes from DOF building-level, annual data, while Panel B displays results on log rents from the unit level, triennial NYCHVS data. We cluster by tract in Panel A and in Panel B by sub-borough-area, a higher level of geographic aggregation. These clustering levels provide conservative standard errors. From left to right, the specifications add controls but yield similar results. We focus our discussion on our preferred specifications in columns (3) and (6).

Matching the internal Audit estimate of the 2011 tax year changes and its affects on multifamily taxes (New York City Department of Finance, 2012), column (1) reports that

Figure 1: Differential Tax Burden and Pass-Through By Size



Note: Figures 1a plot the unconditional annual time-series averages of buildings' log taxes due per unit by size group: 4 and 5 unit buildings in blue and 6-9 unit buildings in red from NYC DOF data. Standard errors are clustered by tract. Figure 1b plots the estimated dynamic treatment effect coefficients from an event study regression on log unit rents using the controls in the specification in column (6) of Table 1 by triennial NYCHVS wave, with treatment effects interacted by year, relative to the 2008 wave as the base (omitted) year. Standard errors are clustered by sub-borough-area.

assessed property taxes per unit for the small buildings fell by a substantial -0.60 log points, or 45%.<sup>14</sup> Panel B reports the the effects of the policy shift on rents. Column (6) reports that the reform reduced unit rents by 11%, or an average reduction of \$166 per month. Under the assumption that 4 – 9 unit buildings face the same aggregate trends, this result is inconsistent with perfect competition.

Figure 1 probes whether the effects are driven by pre-trends. Figure 1a plots log tax per unit for the two size groups over time. In the pre-period, the two lines move in tandem but with a large spread. There is no systematic changes for the 6 – 9 unit buildings but a clear decrease for the smaller group. Figure 1b presents the dynamic treatment effect regression coefficient estimates corresponding to our preferred specification. We estimate pre-treatment zero effects and a clear and relatively persistent decrease in rent relative to the control group and pre-period. Appendix D details several robustness checks, including rerunning our event study with different sets of controls, removing sample

<sup>14</sup>This is not the change in total leasing costs, which are not reported for small buildings because the DOF assesses small buildings on *gross* income.

weights, dropping all units with rent regulations, and using placebo treatment groups. While we cannot directly test the assumption that the two groups do not constitute distinct markets, Appendix B shows that the pre-period samples are balanced in terms of tenant demographics and building observables (except, mechanically, height).

We reiterate our conclusion in Section 3 that our finding of positive pass-through holds even when the treated group is proportionally large.<sup>15</sup> While the large tax shock may have affected on market rents city-wide, the 11% DID estimate is the effect of the idiosyncratic component of that shock on the {4, 5} unit buildings. Because we do not know the relative size of that component of the policy, we cannot use the result to calculate a pass-through rate.<sup>16</sup> For that, we turn to a second source of variation and analysis.

## 5 Evidence from Idiosyncratic Rate Changes

In this section, we develop a complementary instrumental variable approach using a different tax reform on a different set of buildings.

### 5.1 Implementing Equation (6)

**Synthetic Tax Instrument Construction** Since 2011, large (11+ unit) building market values (and tax burdens) are calculated using ‘capitalization rates,’ which the DOF calculated for each building using its reported income in combination with city-wide formulas. We use yearly changes in these formulas to create a synthetic tax instrument by calculating the counterfactual annual property tax rate for each building given that year’s tax formulas holding its income fixed at 2007 levels, our first observed year. This captures the mechanical effect of the assessment changes holding fixed any lessor behavioral responses.

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<sup>15</sup>Violations of STUVA do not bias our estimates as long as general equilibrium shocks affect both types symmetrically; i.e., both types are in the same market.

<sup>16</sup>Further, the NYCHVS data lacks the cost information to calculate a DIDIV estimator.

Our first step is using initial, 2007 gross income per square foot (GIPSF) to calculate counterfactual capitalization rates (CAP) using the following formula:

$$\widehat{\text{CAP}}_{jt} = \alpha_t^0 + (\text{GIPSF}_{j,2007})^{\alpha_t^1} + \alpha_t^2 \cdot \ln[\text{GIPSF}_{j,2007}]. \quad (15)$$

The time-varying parameters  $\{\alpha_t^0, \alpha_t^1, \alpha_t^2\}_{t \in T}$  can be found in annual “Additional Statistical Distributions and Capitalization Rate Methodology” reports on the DOF website.<sup>17</sup>

These reports also explain (to a degree) the methodology by which the  $\alpha$ -parameters are determined on an annual basis. Parameters are *not* building specific, but determined through quantile regressions using administrative building data. While DOF does not publish these specifications, it describes their motivation:  $\{\alpha_t^0, \alpha_t^2\}$  jointly generate the predicted median annual growth in building price from a sample of repeat sales, and  $\alpha_t^1$  is the predicted conditional median relationship between income per square foot and sales price divided by rental income from the same sample. While opaque, the upshot is that coefficients are identified off of city-level variation, the causal determinants of which will be absorbed by our fixed effects. Annual changes in these coefficients create formula changes, which in turn generate idiosyncratic changes to each building’s capitalization rate. Our exclusion restriction requires that the predicted building-specific effects of these coefficient changes only affect lessor pricing decisions through their taxes.

Next, we combine our counterfactual  $\widehat{\text{CAP}}_{jt}$  with each building’s 2007 net-income (NI) and the city-wide effective tax rate (ETR), provided on the DOF’s website, to calculate our instrument, the (log) counterfactual property tax:

$$Z_{jt} = \ln \left[ (\text{NI}_{j,2007} / (\widehat{\text{CAP}}_{jt} + \text{ETR}_t)) \cdot \text{ETR}_t \right]. \quad (16)$$

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<sup>17</sup>For 2011-2013, these reports are not posted on the DOF website, and we back-out these parameters using non-linear least squares using CAP rates reported on DOF communication letters. We obtain capitalization rates matching observed rates with an R-squared of at least 0.8 in each year.



**Market Size and Fixed Effects** Implementing equation (6) requires defining a market. If we specify a market size *above* the true market’s level of aggregation, we will contaminate our estimate of  $\beta$  with market-level fluctuations. By contrast, specifying a market size *below* the true size will still fully and correctly absorb all market-time variation. As a baseline, we define markets as tracts, believing they are smaller than the relevant markets, and therefore a conservative definition. Throughout and especially in Appendix E, we buttress our results with a bevy of alternative market definitions and placebo tests. We also examine markets that are continuous and overlapping.

**Specification** Implementing equation (6), we first regress building rents on the cost shock using the following reduced form specification (for building  $j$  in tract  $m$  in year  $t$ ):

$$\ln[r_{jmt}] = \gamma_1 Z_{jmt} + \gamma_2 X_{jmt} + \gamma_3 D_j + \gamma_4 D_{mt} + \nu_{jmt}, \quad (17)$$

with building fixed-effects  $D_j$  and tract-year fixed effects  $D_{mt}$ , and changes in observable building characteristics  $X_{jmt}$ .

To interpret the results of the above specification, we estimate an additional two-stage least squares specification using reported expenses:

$$\text{First Stage: } \ln[TC_{jmt}] = \pi_1 Z_{jmt} + \pi_2 X_{jmt} + \pi_3 D_j + \pi_4 D_{mt} + \zeta_{jmt} \quad (18)$$

$$\text{Structural Eq: } \ln[r_{jmt}] = \beta_1 \ln[TC_{jmt}] + \beta_2 X_{jmt} + \beta_3 D_j + \beta_4 D_{mt} + \nu_{jmt}, \quad (19)$$

where  $TC_{jmt}$  is the total reported building costs, including taxes and other annual expenses.

Table 2: Pass-through of Cost Shocks on Rent

Panel A: Main Specifications				
	Reduced Form (1)	Reduced Form (2)	2SLS (3)	2SLS (4)
Log Cf Tax	0.036 (0.003)	0.028 (0.003)		
Log Total Cost			1.196 (0.075)	1.282 (0.112)
Robust F Stat			69.93	44.23
Robust AR Stat			118.85	86.01
One-Side Test			0.005	0.006
Time-varying controls	N	Y	N	Y
Tract-year FEs	Y	Y	Y	Y
Building FEs	Y	Y	Y	Y
Observations	152,559	152,559	152,559	152,559
Panel B: Alternative Market Specifications Reduced Form				
	Subway Dist (1)	Building Age (2)	Yrs Since Renovation (3)	Avg Unit Size (4)
Log Cf Tax	0.027 (0.003)	0.024 (0.003)	0.027 (0.003)	0.025 (0.003)
Time-varying controls	Y	Y	Y	Y
Building FEs	Y	Y	Y	Y
X-Group-year FEs	Y	Y	Y	Y
Observations	154,254	154,254	154,254	154,254

*Note:* The table reports multiple regressions using log average unit rent as the dependent variable. In Panel A, columns (1) and (2) are reduced form regressions using the instrumental variable directly; columns (3) and (4) are two stage least squares regressions where the lot total cost is instrumented. We use log counterfactual taxes as the instrument. All regressions are at the building-year level with standard errors clustered at the tract level, and include building and tract-year fixed effects. Columns (1) and (2) omit time-varying controls, and columns (2) and (4) include log building age, log years since a renovation, and an elevator indicator. In Panel B, columns (1)-(3) replicate Panel A column (2), adding percentile-year fixed effects for percentiles of distance to subway, building age, years since renovation, and unit size, respectively.

## 5.2 Results

Panel A of Table 2 displays our results. Columns (1,2) present reduced form results, where column (2) adds time-varying controls. We find that a 10% increase in predicted tax policy leads to a roughly 0.3% increase in rent. The positive relationship rejects the null but is qualitative. To interpret these results as a pass-through we must account for the relationship between our instrument and total costs (taxes plus building expenses).

Columns (3,4) are two stage least squares results where we instrument for total per unit leasing costs in order to estimate a pass-through rate. We find that a pass-through rate of about 120-130% into rents. This over-shifting is consistent with variable markups in the presence of sufficiently convex demand (Pless and van Benthem, 2019).

We reiterate that the RF delivers a qualitative test, and its interpretation is best understood through the IV result. The discrepancy between the RF and 2SLS arises from the variation in taxes not predicted by our instrument.<sup>18</sup> This reduces the RF for the same reason as the FS coefficient is less than 1. As long as the exclusion restriction holds, the pass-through result in the IV is correct.

Panel (B) runs additional specifications adding fixed effects that redefine markets by observable, non-geographic characteristics. We divide the sample into percentiles of distance to subway, building age, date of last renovation, and average unit size, and include percentile-year fixed effects. Across all four columns, we find no evidence that these alternative market definitions affect our results.

Appendix E details several robustness checks, including repeating the analysis at the block level and using different samples including dropping mixed use and highly rent-stabilized buildings, and controlling for model-based, overlapping market structures.<sup>19</sup>

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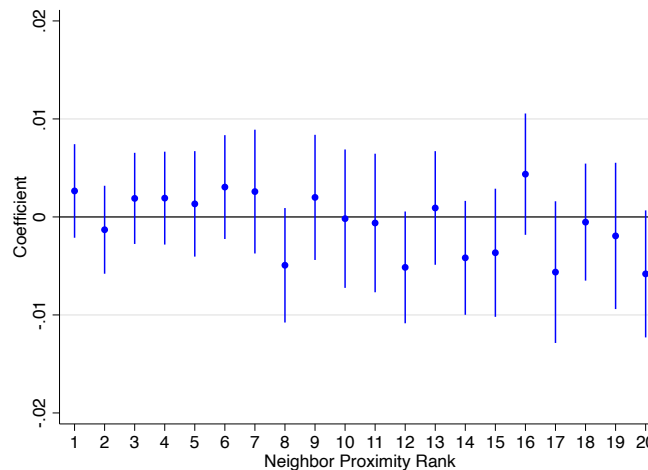
<sup>18</sup>Austin (2022) find larger landlords more likely to challenge assessments. This would be an additional source of bias in the RF but not the 2SLS.

<sup>19</sup>Another concern we explore but do not include in Appendix E's discussion is the threat of variable trends correlated with base-year income. Controlling for year trends by initial gross income increases the

**Placebo Test** Phrased as an exclusion restriction, the assumption that our instrument identifies idiosyncratic cost variation is that our instrument is uncorrelated with (shifters of) the building’s residual demand. To test this assumption, we consider a series of placebos where we run our specification using rents of each building’s  $n$ th nearest neighbor, which are the competitors most likely to embody correlated demand or sorting on other unobservables. If the exclusion restriction holds, then fluctuations in each building’s closest competition should be orthogonal to the instrument.

Figure 2 reports the coefficient replacing in our specification each building’s rent with that of their  $n$ th closest neighbor. Reassuringly, we find no systematic correlation and coefficients an order of magnitude smaller than our main results. This is robust to a multitude of alternative specifications, including grouping all the top 5, 10, or 20 neighbors, and drawing concentric circles around buildings and including all the neighbors therein. In Appendix E, we find similar results running placebo tests using proximity in non-geographic observables.

Figure 2: Correlation Between Instrument and the  $n$ -th Nearest Neighbor’s Rent



Note: The figure plots coefficients (blue dots) from a regression of buildings’ instrument  $Z_{jgt}$  on the average unit rent at their  $n$ th closest neighbor, plotted according to proximity rank. Controls include building and tract-year fixed effects as well as controls for age, years since renovation and an elevator dummy. Blue lines are 95 percent confidence intervals. All regressions cluster by tract.

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coefficient’s magnitude by about 17%.

## 6 Discussion and Conclusion

**What Causes Pricing Power?** Although many frictions may be present in our setting, we consider three market structures that could underpin our findings. First, idiosyncratic valuations of building locations or amenities on any number of observable and unobservable dimensions may produce downward sloping residual demand.<sup>20</sup> Finding markups in the presence of horizontal differentiation requires estimating structural models.<sup>21</sup>

There is substantial renewed interest in the effect of concentration in housing markets. We examine the concentration-rent correlation using our data. Using information from public records on building ownership and contacts, we reconstruct likely shared ownership in each tract and calculate a Herfindahl–Hirschman index (HHI) for each tract.

Figure 3 Panel (a) plots a binned scatter of the twelve year (2007-2019) changes in building rents versus HHI from a panel with building fixed effects. The *non-causal* positive relationship ( $\beta_{\text{HHI}} = 0.058$ ,  $se = 0.02$ ) implies increases in ownership concentration are associated with increasing rents.

Panel (b) maps HHI measures. In red, over 30% of tracts—especially in the periphery and downtown and midtown Manhattan—are above an HHI of 2500, a standard threshold for concentration. Crucially, because true markets are likely defined above tracts, Panel (b) likely overstates concentration levels, but missing concentration in the market beyond the tract adds noise to Panel (a), likely attenuating the correlation.

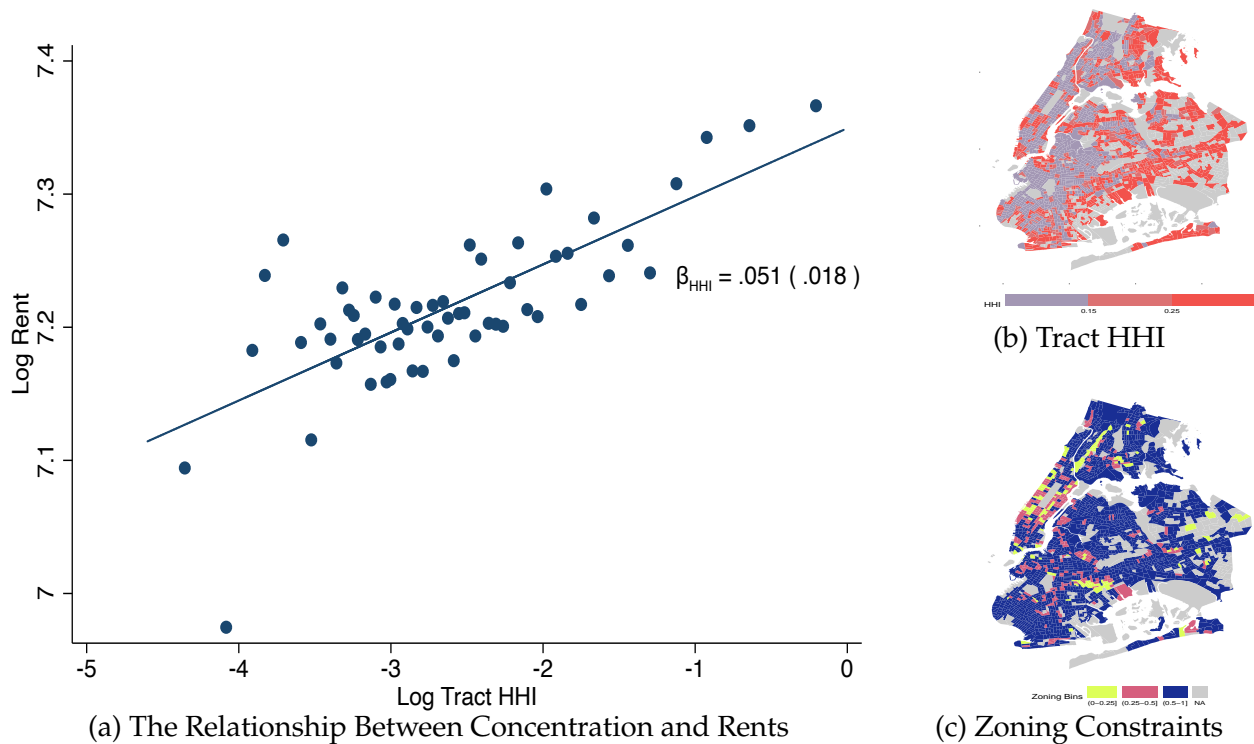
Lastly, a basic characteristic of urban housing markets is constrained capacity. In the short run, capacity is constrained by construction times ([National Association of Home Builders, 2022](#)). In the long run, capacity in built environments is constrained by zoning. Panel (c) of Figure 3 maps the percent of lots in each tract that we calculate cannot build

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<sup>20</sup>As discussed so may search and matching. Bilateral vertical differentiation and sorting also generates downward sloping demand ([Shaked and Sutton, 1983](#)).

<sup>21</sup>[Watson and Ziv \(2021\)](#) estimates a model of idiosyncratic demand.

Figure 3: New York City Housing Market Structure



Note: Panel (a) plots the binned scatter (blue dots) overlaid with the linear regression (red line) of log rents regressed on log tract HHI, estimated as a two-period panel for years 2007 and 2019, controlling for time-varying buildings characteristics, building FEs, and a 2019 indicator, clustering by tract. The regression coefficient  $\beta_{HHI}$  and standard error are printed. Panel (b) maps the pooled, tract-level HHI measures used in panel (A). Panel (c) maps, for each tract, the percent of buildings in our sample that we calculate cannot add a single additional unit of minimal size due to zoning.

even one additional unit of minimal legal size without running afoul of one or more regulations. Blue tracts, where over 50% of parcels are constrained, dominate. Roughly 65% of NYC’s housing stock is at a binding zoning constraint.

Together with costly, directed search, equilibrium pricing in such markets may not feature Bertrand pricing (Geromichalos, 2014).<sup>22</sup> Intuitively, the Bertrand rule of “two is enough” relies on the competitors’ threat to absorb all demand, which is not sub-game perfect if competitors cannot credibly commit to doing so. To our knowledge, urban

<sup>22</sup>Such market structures do not always deliver a pure strategy Nash equilibrium (Benassy, 1989, 1991; Wauthy et al., 2014). Under certain conditions, competition under constraints yields Cournot pricing, (Kreps and Scheinkman, 1983; Maggi, 1996).

theory has so far neglected to integrate this finding.

**External Validity** NYC is the sixth densest city in the country (U.S. Census Bureau, 2011), although not globally. It is not immediately clear if density, with both renters and lessors in closer proximity, would bias results towards or against finding market power. Our sample includes both Manhattan, which is exceptionally dense, and Queens, whose density is comparable to Cambridge, MA. Our results are robust to omitting Manhattan. More generally, the threat of entry likely restores some competition in non-urban settings with elastic extensive supply margins. It is beyond our scope here to establish in which markets these forces are strongest and weakest.

**The consequences of pricing power** Using two empirical approaches with two distinct identifying assumptions and two sources of variation stemming from changes in tax assessment procedures, we find increases in buildings' idiosyncratic tax burdens lead to increases in their rents. This behavior is inconsistent with market structures featuring perfect competition. This implies that when pricing, lessors face decreasing marginal revenue schedules and rents are set with markups.

The existence of markups has far-reaching consequences for our understanding of the housing supply and housing constraints. Supply constraints are responsible for large-scale economic distortions and first-order losses (Hsieh and Moretti, 2019). Pricing power is a supply constraint that is new to urban theory: if developers internalize pricing power, we should expect them to set oligopolist-optimal quantities below competitive quantities.<sup>23</sup> Moreover, the wedge between marginal costs and rents has been used as a measure of the quantity distortions of regulation (Glaeser, Gyourko, and Saks, 2005; Glaeser and Ward, 2009; Glaeser and Gyourko, 2018). Pricing power is an alternative source of this

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<sup>23</sup>Appendix A.3 presents a monocentric city model and shows that pricing power leads to less dense, wider cities.

wedge, and would exist in the absence of regulation. As such, it is a source of bias in our understanding of regulations' costs.

The existence of pricing power also impacts other aspects of our understanding of the housing supply. The conceptual frameworks used to estimate the housing production function rely on marginal cost pricing assumptions (Baum-Snow and Han, 2023; Combes, Duranton, and Gobillon, 2021). These estimates are pivotal and widely used. However, in the presence of variable markups, specifications used in their estimation are potentially biased, at least requiring additional assumptions. E.g., the results in equations (4) and (5) of Combes, Duranton, and Gobillon (2021), can be adjusted by a fixed proportion in the presence of pricing power assuming a constant elasticity of demand.

Finally, the growing quantitative spatial modeling literature uses marginal cost pricing to estimate counterfactual equilibria. The predicted consequences and welfare implications of policy changes such as deregulation, infrastructure improvement, or tax policy shifts—all of which result in localized changes in housing demand—are biased if the effect of potentially differential and increasing markups are not directly modeled.<sup>24</sup>

**Further work** A limitation of our approach is that we are unable to estimate the demand elasticity. Our results call for more work exploring the sources and scope of pricing power, the policy consequences of pricing power in housing markets, and the effects of pricing power on housing supply.

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<sup>24</sup>An exception is Ospital (2023), who calculates counterfactuals assuming monopolist landlord pricing.



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# Online Appendix of A Test of Pricing Power in Urban Rental Markets

C. Luke Watson and Oren Ziv

## A Theoretical Appendix

### A.1 Theoretical Setting

In this appendix, we discuss several aspects of the market for apartments that add realism, but do not have an impact on our basic conceptual framework.

**Non-continuous or unit supply** Our analysis treats quantity as continuous. The treatment of unitary quantities as continuous is consistent with an interpretation of rent setting in a probabilistic demand setting, where lessors post rents each period knowing that higher rents make occupancy less likely. Although it is beyond the scope of this paper to test between specific models of demand, an inverse relationship between posted rent and expected occupancy could be microfounded with a search model where renters pay to search, evaluate fit and rent, and choose to rent or continue searching. Alternatively, capacity constraints—a realistic feature of the short and long-run urban housing equilibrium—can generate downward sloping expected demand in a similar way ([Arnold, 2000](#); [Geromichalos, 2014](#)).

**Supply Constraints** Conditional on entry, each building’s maximum supply is constrained. Increasing supply beyond the physical capacity of the building is extremely difficult, and impossible for the vast majority of buildings who are constrained by zoning.<sup>25</sup> On the other hand, any quantity below the constraint can be chosen—whether this be literally withholding units or in terms of expected occupancy rate, as discussed above— and doing so might be thought of as mothballing units or setting an expected vacancy rate above zero.

**Entry and exit** Entry and exit are margins through which the marginal cost shocks we study will affect total market supply and, therefore, buildings’ residual demand. As such, they are important forces that aggregate in an isomorphic way to intensive-margin supply changes, and must be controlled for. The extensive margin of supply is fairly inelastic. In our sample, there are an average of roughly 120 new buildings, or 0.16% of the total sample, added each year. This is unsurprising since our setting is a fully built-up urban environment with few empty lots. Exit is more difficult to observe. The number of registered building conversions from Class 2 rentals to condominiums per year is roughly 2 dozen. Entry and exit are also likely more appropriate to consider in the “long run.” Nationally, time from authorization to completion of a structure typically takes over a year ([National Association of Home Builders, 2022](#)). Once created, housing is durable and difficult to convert to other uses. Entry and exit could affect super or sub-tract-level demand. The former would be absorbed by tract fixed effects. As our instrument is uncorrelated with neighboring buildings’ rents, the latter appears unlikely to be affecting our result.

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<sup>25</sup>We discuss NYC zoning rules and how we assign zoning constrained status in Appendix C.1; informally, a building zoning constrained if current regulations prevent adding an additional residential unit.

**Leasing costs** Leasing costs may have a fixed and variable component. Fixed costs may involve mortgage costs or other types of costs invariant to the supply decision. Marginal costs of leasing might involve administrative costs, maintenance and labor costs, etc. These are reported in our data via RPIE forms across all buildings and years in our sample, in addition to average building income. These costs include labor, materials, but omit mortgage costs. Property taxes, which in our setting are income-based, are also marginal costs we observe.

## A.2 Idiosyncratic costs and pass through

In this appendix, we describe how different types of cost shocks can be passed through from suppliers onto demand in different market settings, distinguishing between common shocks and idiosyncratic shocks. We explore how different aspects of the housing market affects these conclusions, then connect these intuitions to specific cost shocks in the data, elasticities, and econometric models.

In general, the ability of suppliers to pass through changes to their marginal costs will depend on the market's structure. We consider two broad possibilities: first, that suppliers are price-takers operating in competitive markets, and, second, that they are not.

**Pass-through under perfect competition** In the former case, by definition, demand for each building is perfectly elastic: any positive deviation from the market rent for their building results in a complete collapse in demand.<sup>26</sup> Here, we explain how shocks to costs only affect market rents through the common component while the idiosyncratic component affects the firm's supply but cannot affect its posted rent.

In Figure A.1a we illustrate an increase in marginal cost, where the common component is zero and the idiosyncratic component is positive:  $\Delta_t = 0$ ,  $\epsilon_{ft} > 0$ . The original equilibrium is where the original increasing marginal leasing cost curve in black intersects the perfectly elastic residual demand determining price and quantity. The idiosyncratic component of the shock,  $\epsilon_{ft}$ , shifts up the cost curve, as is shown in red. This generates a quantity response. Because the policy shift creates no market-level change, the landlord experiences no shift in the residual demand curve for units in the building, and so there is no rent response.

Figure A.1b instead displays a situation where the firm's marginal cost is increasing and the market component is positive:  $d \ln(mc_{ft}) > 0$ ,  $\Delta_t > 0$ . Similar to the previous case, there is a left-ward shift in the marginal leasing cost curve in red. Unlike the previous case, a part of the building's cost shock is shared by the market, leading to a correlated decrease in quantity supplied (as in Figure A.1a) that now aggregates, appearing as a(n un-drawn) leftward shift in market supply. This supply shift leads to a quantity change along the (undrawn, aggregate) demand that increases the market rent, and, finally, manifests in Figure A.1b as an upward shift in the landlord's residual demand curve as shown in red.

The effects of capacity constraints do not substantially change this analysis. In Figure A.1c we reconsider the case where  $\Delta_t = 0$  and  $\epsilon_{ft} > 0$  but the landlord faces a binding supply constraint that is represented by the blue vertical line. In this case, the initial equilibrium is  $(q^c, p^c)$ . Again, the landlord's left-ward shift in the marginal cost is represented in red. The new, higher marginal

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<sup>26</sup>In this setting, buildings need not be identical. For example, buildings could have different amenities valued identically by all renters. Equilibrium rent difference between buildings reflect the differences in amenity valuations, such that amenity-adjusted rents are identical.

cost curve reduces the wedge between cost and rent, but as before, does not elicit a rent response. All that's transpired is that the wedge between rent and cost has decreased.

In Figure A.1d, we reconsider the case where  $\Delta_t > 0$  but with the capacity constraint. To the extent that there are some buildings in the market that are not constrained, the market component does elicit a quantity response which aggregates, and, as before, shifts the market supply curve to the left and shifts the residual demand curve. As illustrated, in this case the capacity constrained building *also* experiences a rent change. Note, this analysis does not assume costs are increasing. When marginal leasing costs are constant and below rents, buildings always lease up to capacity as in Figures A.1c and A.1d and those analyses apply.

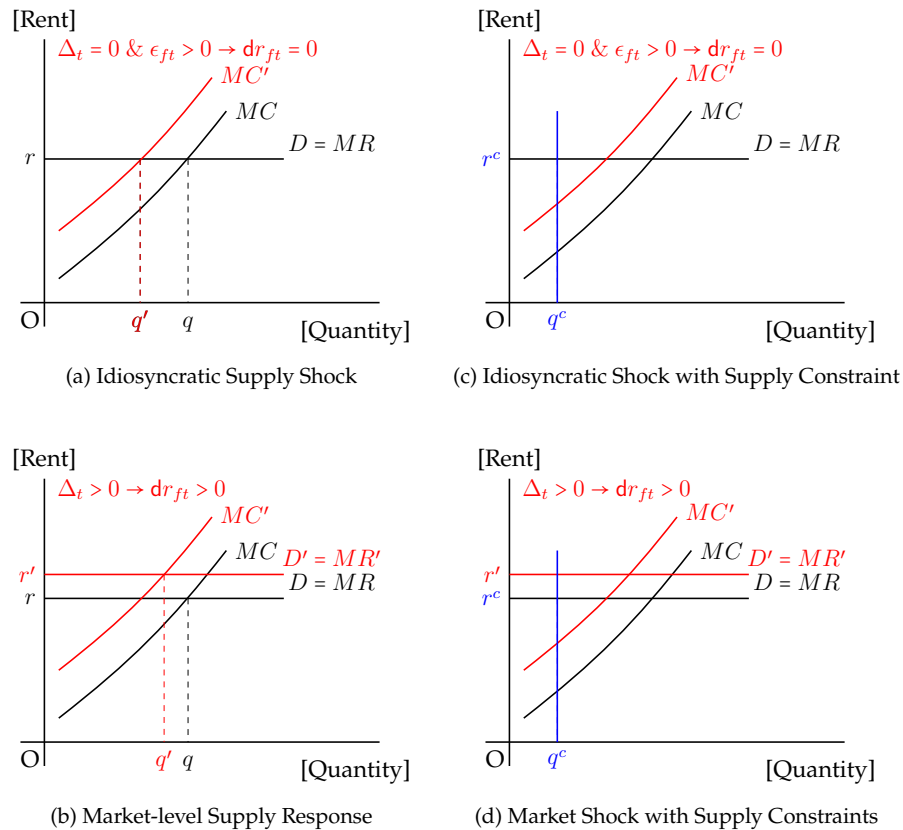


Figure A.1: Idiosyncratic and Market-level Shocks Under Perfectly Elastic Demand

Finally, as with capacity constraints, considerations of entry or exit do not affect qualitatively alter rent responses. In each of the four panels, the area between the residual demand curve and marginal cost is variable profit gross of an entry cost. Entry or exit would occur only in A.1b or A.1d where quantity, and thus profitability, move at the market level. Entry would shift the demand curve back down and exit the reverse.

**Pass through of cost shocks with downward sloping demand** We now turn to the effect of the same cost shocks under deviations from perfect competition. Figures A.2a through A.2d repeat the above exercise but assuming downward sloping demand. In A.2a the result immediately departs from that of the figure's counterpart, A.1a. In particular, the shift in idiosyncratic

marginal cost moves the firm up the demand curve, the new price and quantity are higher and lower, respectively. Note that even if we (somehow) ignore monopoly pricing, and posit instead competitive pricing, the result would hold. The addition of the market-level component in Figure A.2b changes the observable affect quantitatively but not qualitatively.

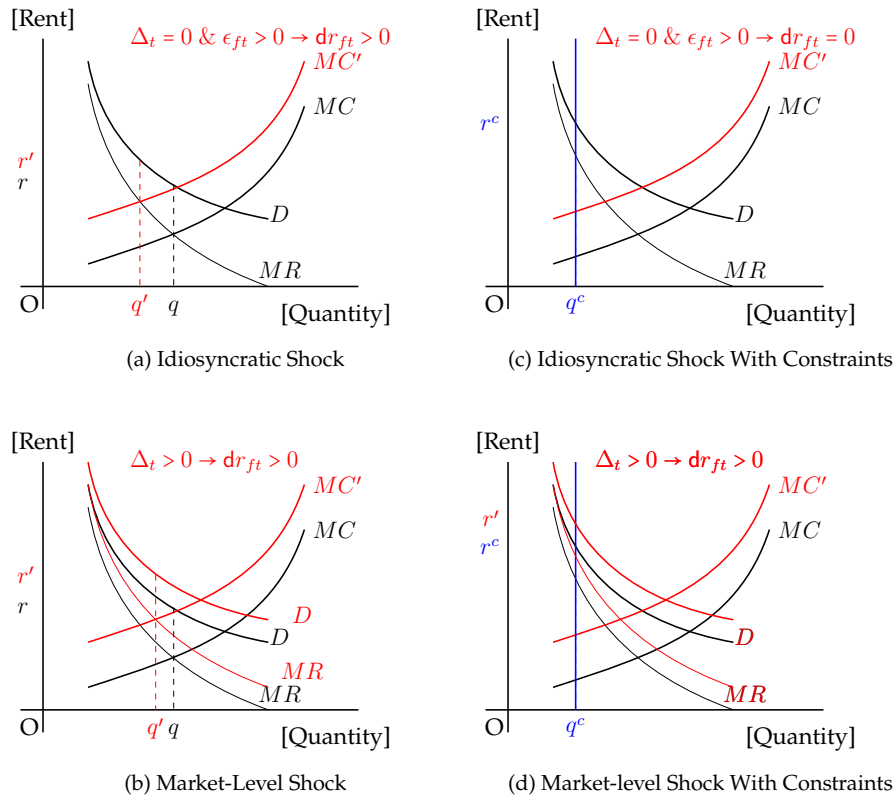


Figure A.2: Idiosyncratic and Market-level Shocks With Downward Sloping Demand

The introduction of quantity constraints yields qualitatively similar analysis as the perfect competition benchmark. In Figure A.2c, as in the corresponding perfect competition counterpart in Figure A.1c, the idiosyncratic cost shift does not elicit a rent response. Because constrained buildings rent at the quantity constraint, the shift in marginal cost affects profits but not price. The analysis in Figure A.2d similarly tracks the competitive case. The expectation that idiosyncratic cost shocks can be passed on to renters is contingent on buildings not being at a binding constraint.

Noting that entry affects the analysis in all cases in the same way as in the perfectly competitive benchmark, we conclude it is only possible for idiosyncratic cost variation to be passed-through into rent when the demand curve slopes down. Informed by this conclusion, our empirical specifications attempt to isolate variation in  $\epsilon_{ft}$  and test for the the elasticity of rents with respect to  $\epsilon_{ft}$ .

For the DID strategy, we need two sets of buildings,  $\{T, C\}$ , with similar marginal cost and



market trends except that one group is treated ( $\epsilon^C = 0$ ):

$$d \ln(\text{mc}_{ft}^T) - d \ln(\text{mc}_{jt}^C) = (\beta \cdot [\Delta_t + \epsilon_{ft}^T] + \lambda_t + u_{ft}^T) - (\beta \cdot [\Delta_t + \epsilon_{jt}^C] + \lambda_t + u_{jt}^C) \quad (20)$$

$$= \beta \epsilon_{ft}^T + (u_{ft}^T - u_{jt}^C). \quad (21)$$

As long as the common marginal cost shifter ( $\Delta$ ) and the time FEs ( $\lambda$ ) are the equal and there is no differential selection in unobserved demand shifters ( $u$ ), then the DID estimate yields  $\beta E[\epsilon^T]$ . As described in the main text, we use buildings with  $\{4, 5\}$  units versus  $\{6, 7, 8, 9\}$  based on a policy change that lowered per unit taxes on smaller buildings.

For the IV strategy, we need a variable,  $Z$ , that is, conditional on time FEs, correlated with  $\epsilon$  but uncorrelated with  $u$ . The regression specification is:

$$\text{FS: } d \ln(\text{mc}_{ft}) = \pi Z_{ft} + \lambda_t^{\text{FS}} + v_{ft} \quad (22)$$

$$\text{SS: } d \ln(r_{ft}) = \beta d \ln(\text{mc}_{ft}) + \lambda_t^{\text{SS}} + u_{ft} \quad (23)$$

$$= \beta \epsilon_{ft} + \tilde{\lambda}_t^{\text{SS}} + u_{ft}, \quad (24)$$

with assumptions  $\text{Cov}(\epsilon_{ft}, Z_{ft} \mid \lambda_t) > 0$  and  $\text{Cov}(u_{ft}, Z_{ft} \mid \lambda_t) = 0$ . As described in the main text, we use a synthetic tax instrument purged of market level marginal cost variation.

### A.3 Perfectly and Imperfectly Competitive Monocentric City Models

In this subsection we briefly outline a perfectly competitive monocentric city model in discrete space and then alter it to generate a model with markups.

**A Perfectly Competitive Monocentric City Model** Here, we develop an urban model where rents are differentiated based on local amenities but lessors still have no pricing power. The primary assumptions are that lessors view themselves as atomistic and that renters' preferences are uniform.

Consider a city with fixed population of renters  $i \in N$  and a set of locations  $j \in J$ . Each location has a local amenity  $x_j$  and rent  $r_j$ , and is owned by a unique, atomistic lessor who is a price-taker, providing housing with a marginal cost of  $mc_j(q)$  for  $q$  residents. Each atomistic renter must choose one unit of housing or an outside option and has preferences over amenities and consumption, and we can describe their utility using the value function  $u_{i,j} = x_j - r_j$ . In the competitive spatial equilibrium, (1) all renters are housed in their utility maximizing location and (2) utility is equal everywhere. These conditions together with competitive supply imply that (1)  $\sum_j q_j = N$ , (2)  $u_{i,j} = \bar{u}$ , and (3)  $mc_j(q_j) = r_j$ .

To find a solution, we note that (2) implies that  $r_j = x_j - \bar{u}$ , that (3) implies  $q_j = mc_j^{-1}(r_j)$ , and together with (1) we have  $\sum_j (mc_j^{-1}(x_j - \bar{u})) = N$ . These equations form a system of  $J + 1$  unknowns ( $\{\bar{r}, \bar{u}\}$ ),  $J + 1$  parameters ( $\{\tilde{x}, N\}$ ), and  $J + 1$  equations:

$$\sum_j (mc_j^{-1}(x_j - \bar{u})) = N$$

$$r_j = x_j - \bar{u}.$$

In Section 3, we show that idiosyncratic cost shocks are passed through to building rents. The above equations make clear that changes in marginal cost at  $j$  do not impact rents  $r_j$ . Furthermore,

were the outside option  $\bar{u}$  to be endogenously determined through  $N$ , say through idiosyncratic tastes for the outside option, the impact of a cost shock at  $j$  on rent  $r_j$  would operate only through the outside option  $\bar{u}$ , i.e. insofar as marginal costs at  $j$  contribute to a market-level cost shock. Thus, an idiosyncratic cost shock will not affect  $\bar{u}$  nor rent.

**Markups in the Monocentric City Model** In this section, we adapt the monocentric city model above to include individual heterogeneity in location preferences for a finite set of locations that induces downward sloping residual demand for locations. This heterogeneity implies that markups exist and that profits are increasing with proximity to the city center. In addition, rents in denser areas are higher and the quantity of housing lower than if the supply were competitive (rent set to marginal cost). Pricing power results in a city that is flatter, demonstrating how pricing power acts as a dispersive force.

We begin with the model which immediately precedes this section, noting that  $x_j$  can denote location  $j$ 's distance to the city center on a line segment, with differences between any two locations  $j$  and  $j + 1$ 's distances  $x_j$  and  $x_{j+1}$  being constant and nonzero for all  $j$ s (such that there are a large but finite set of locations  $J$ ), starting with the CBD itself at  $j = 0$  with distance  $x_0 = 0$ , until an endogenously determined periphery point,  $j^b$  with distance to CBD  $x_{j^b}$ .<sup>27</sup> In addition we assume a mass  $N$  of renters and for simplicity we assume that the city is closed. The assumption of a discrete number of locations follows [Berliant and Fujita \(1992\)](#) and is a departure from standard monocentric city models, where space is continuous for tractability reasons, but similar to Alonso's original model.

We now adjust the above model by positing that indirect utility of each individual  $i$  at location  $j$  is a combination of location  $j$ 's rent,  $r_j$ , and distance to the CBD,  $x_j$ , as well as an idiosyncratic valuation parameter,  $\epsilon_i$ , drawn from some distribution  $G(\epsilon)$  where  $\epsilon \geq 0$  and  $E[\epsilon] \in \mathcal{R}_+$ :

$$v_i(j) = (1 - x_j)\epsilon_i - r_j. \quad (25)$$

Note that this is isomorphic to heterogeneous transportation costs.<sup>28</sup>

Downward-sloping demand for each location is derived from renter heterogeneity and the assertion that for each distance  $x_j$  there is only 1 plot of that distance (with differences  $x_j - x_{j+1} > 0$  and constant). An alternative approach that would sustain markups in the presence of multiple plots with identical values  $x_j$  would be to assume renter-location specific heterogeneity  $\epsilon_{i,j}$  as in a discrete choice model. In such a setting, markups can converge to a positive constant even as the number of choices goes to infinity.

In the present setting, willingness to pay for each location will be different for each individual. This generates a downward sloping residual demand curve for each location  $j$  that brings about markups. As in the previous model, willingness to pay is also determined by rents at other locations:

$$WTP_i(j) = \min_{j'} \{(1 - x_j)\epsilon_i - (1 - x_{j'})\epsilon_i - r_{j'}\}. \quad (26)$$

Note that for any given option  $j'$ , the difference between  $i$ 's valuations of  $j$  and  $j'$  is increasing in  $\epsilon_i$ . This super-modularity guarantees a sorting equilibrium: each location  $j$  will be populated by a set of types  $[\underline{\epsilon}_j, \bar{\epsilon}_j]$ , with higher locations having higher cutoffs ([Mirrlees, 1971](#)). Locally, the

<sup>27</sup>Note, defining  $x_J$  as the furthest point from the CBD that is feasible given the landscape (possibly due to natural features), we allow for  $x_{j^b} \leq x_J$ .

<sup>28</sup>In addition, we assume that all units are the constrained to be equal sized.

only relevant alternatives for individuals at plot  $j$  are at plots  $j - 1$  and  $j + 1$ . Specifically, at each plot  $j$  there is a threshold,  $\tilde{\epsilon}_j$ , below which the relevant alternative is  $j + 1$ , and above which the relevant alternative is  $j - 1$ : the highest types in each plot will be deciding between  $j$  and moving one space closer to the city center while the lower types will be deciding between  $j$  and moving space further.

Locally, conditional on  $\epsilon_i > \tilde{\epsilon}_j$ , willingness to pay for  $j$  is therefore:

$$WTP_i(j) = (x_{j-1} - x_j)\epsilon_i - r_{j-1}, \quad (27)$$

while below  $\tilde{\epsilon}_j$ , willingness to pay for  $j$  is:

$$WTP_i(j) = (x_{j+1} - x_j)\epsilon_i - r_{j+1}. \quad (28)$$

Total demand for  $j$  given a rent  $r_j$  is therefore

$$q_j(r_j) = P(\epsilon > \tilde{\epsilon}_j) \cdot P\left(\epsilon < \frac{r_{j-1} - r_j}{x_j - x_{j-1}}\right) + P(\epsilon \leq \tilde{\epsilon}_j) \cdot P\left(\epsilon > \frac{r_j - r_{j+1}}{x_{j+1} - x_j}\right) \quad (29)$$

or

$$q_j(r_j) = [1 - G(\tilde{\epsilon}_j)] \cdot G\left(\frac{r_{j-1} - r_j}{x_j - x_{j-1}}\right) + G(\tilde{\epsilon}_j) \cdot \left[1 - G\left(\frac{r_j - r_{j+1}}{x_{j+1} - x_j}\right)\right]. \quad (30)$$

It's clear from the above that quantity demanded is declining in rent on two margins: as  $r_j$  increases, those at the top margin near  $\tilde{\epsilon}_j$  leave for  $j - 1$  and those at the bottom margin  $\underline{\epsilon}_j$  leave for  $j + 1$ . Marginal revenue is therefore also declining. Profits are given by:

$$\pi = \max_{\mathbb{1}_{redev}, q_j} \begin{cases} r_j(q_j) \cdot q_j - C^d(q_j) - C^f(q_j) & \text{if } \mathbb{1}_{redev} = 1 \\ 0 & \text{if } \mathbb{1}_{redev} = 0, \end{cases} \quad (31)$$

where here we assume for simplicity no structure currently exists at any location  $j$ , no transfers, and costs are symmetric. We also now assert a fixed cost of development  $C^d(0) > 0$ .

At each location, taking rents at all other plots  $r_{j'}$  as given, landlords set quantities to maximize profits and will do so where falling marginal revenues are equal to marginal cost. Because rents must be decreasing with distance  $x_j$  in the sorting equilibrium described above,  $r_{j+1}$  and  $r_{j-1}$  are also falling with  $x_j$ . All else equal, demand at any rent is falling in  $x_j$ . It follows that plots closer to the city will have higher quantities and profits. It further follows that the city will be filled in: if a location  $j$  is developed, all locations  $j' < j$  will also be developed.

To close the model, the peripheral location,  $j^b$  at distance  $x_{j^b}$ , must be found after which no development occurs. Note that quantity demanded at  $j^b$  is given by:

$$q_{j^b}^D(r_{j^b}) = G\left(\frac{r_{j^b-1} - r_{j^b}}{x_{j^b} - x_{j^b-1}}\right). \quad (32)$$

A slight complication to note is that, as is present in the above demand expression, at this location (as well as at the most central location) there is only a single margin from which to draw additional individuals, or to which marginal individuals leave.

Because space is discrete, we have an approximation of a zero profit condition in Eq 33. The

peripheral location,  $j^b$ , is defined as the furthest plot from the city center such that its profit is weakly positive (and almost zero) but at the next undeveloped plot,  $(j^b + 1)$ , profit upon entry is strictly negative:

$$0 > r_{(j^b+1)}(q_{(j^b+1)}) \cdot q_{(j^b+1)} - C^d(q_{(j^b+1)}) - C^f(q_{(j^b+1)}). \quad (33)$$

Despite (near) zero profits, the landlord at  $j^b$  still sets rent with a markup over marginal cost. Rather, the fixed cost  $C^d(0)$  almost exactly offsets variable profit from the optimally chosen quantity at the periphery (and profits are increasing towards the city center).

Finally, the total quantity of housing provided must equal the total quantity demanded:

$$N = \sum_0^{j^b} q_j^*(r_j^*), \quad (34)$$

where  $q_j^*$  and  $r_j^*$  are equilibrium, profit-maximizing quantities and rents at plot  $j$ .

To see monopoly power act as a dispersion force, note that at the city center,  $j = 0$ , demand is given by:

$$q_0^D(r_0) = 1 - G\left(\frac{r_0 - r_1}{x_1}\right). \quad (35)$$

Quantity at this location is lower than in a competitive model due to both landlord at  $j = 0$  setting rent above marginal cost *and*, through general equilibrium forces: rent at  $j = 1$  is also set its marginal cost. However, not every location will experience reduced density: the equilibrium effects of reduced density at the center push demand out to the periphery, and, at some point, these effects may dominate the pricing power effect and locations further away may be more built up than in under marginal cost pricing. Furthermore, the increased profitability at  $j^b$  and the general equilibrium effects increasing demand at  $j^b$  imply that the peripherally developed plot is *further* away from the CBD under monopolistic pricing. Taken together, these facts imply the density of the monopolistically priced city first-order stochastically dominates that of the city built to marginal cost pricing (although we do not formally show this).

We reiterate that the markup in this setting is a feature of two items: heterogeneity of individual taste in the form of the (variance of the) distribution  $G(\epsilon)$ , and amenity differences  $x_j - x_{j-1}$ . As either go to zero, markups are eliminated. This is **not** the case if instead heterogeneity took the form of individual-by-location draws,  $\epsilon_{i,j}$ , as in the model in the main text. Under this regime with specific assumptions on the distribution of  $G(\epsilon)$ , markups will converge to a constant as the distance between spaces converges to zero or, as on a disk, multiple buildings with the same value of  $x$  coexist.

## B Construction of Samples

In the following subsections, we discuss the samples' constructions. We primarily use two datasets. First, we present our New York City Housing and Vacancy Survey (NYCHVS) sample, conducted by the US Census Bureau and the NYC Department of Housing Preservation (DHP). Second, we present our sample of NYC rental buildings constructed from NYC government data sources and letters to lessors.

## B.1 NYCHVS

We use the NYCHVS Occupied Unit samples from 2002, 2005, 2008, 2011, 2014, and 2017. After each decennial census, the Census and DHP sample residential structures in NYC and follow these units three and six years later in the same decade.<sup>29</sup>

**Tax Reform DID Sample:** We subset the main analysis to rental buildings with 4 – 9 units that are privately owned. We use log contract rent as our main dependent variable. For treatment indicator we use the reported number of units, which is binned, to create an indicator for {4, 5} units. We consider the years 2011 – 2017 as being in the post period. We also use the following controls: condition of building indicators, number units in building (binned), number of floors (binned), elevator indicator, number of years occupant has been in the unit, length of the lease (binned), and sub-borough-area-by-year fixed effects.

Table A1 displays summary statistics for the NYCHVS data used in Section 4. Column (1) reports for the whole sample (all years), while columns (2) and (3) report for the {4, 5} unit buildings and 6 – 9 unit buildings, respectively.

Table A1: Summary Statistics: 2002 - 2017 NYCHVS

	Full (1)	{4, 5} (2)	{6, 7, 8, 9} (3)
Average Rent	\$ 1,408	\$ 1,415	\$ 1,400
Median Rent	\$ 1,235	\$ 1,229	\$ 1m243
Pct {4, 5}	49%	100%	0%
Pct w/ Elevator	3%	2%	5%
Pct Sound Condition	92%	92%	92%
Pct Built Pre 1947	86%	86%	85%
Pct Built 1947-1989	9%	9%	9%
Pct Built Post 1990	5%	5%	6%
Pct Less 3 Stories	15%	27%	3%
Pct 3 – 10 Stories	85%	73%	96%
Pct Less 2 Year Lease	36%	33%	38%
Pct 2 Year Lease	28%	23%	33%
Pct Other Lease	36%	44%	29%
Pct 0 – 3 Year Tenure	34%	35%	33%
Pct 3 – 9 Year Tenure	37%	39%	34%
Pct 10 Year Lease	29%	25%	33%
Unique Buildings	8,259	4,051	4,208

*Note:* This table reports summary statistics for the sample we use in Section 4. We use the 2002, 2005, 2008, 2011, 2014, 2017 Occupied Units tables from the NYC Housing and Vacancy Survey. We subset the data to all privately owned rental units with 4 – 9 units. Column (1) shows all buildings, (2) shows buildings with {4, 5} units, and (3) shows those with 6 – 9 units. Because the NYCHVS reports variables as granular categorical indicators rather than continuous variables, we have created summarizing indicator variables to more parsimoniously present the data. In the regressions, we use the more granular versions of these variables.

Table A2 displays balance tests for various building and occupant characteristics during the pre-reform period. Column (1) reports the difference in means for each variable, and column (2) reports the SBA-cluster-robust  $t$ -statistic. In the main text, we present results with and without covariates and find consistent results.

<sup>29</sup>Unfortunately, the public data does not include a panel identifier.

Table A2: Balance Tests:2002 - 2008 NYCHVS

	Panel A - Building Controls		Panel B - Respondent Controls	
	Difference	<i>t</i> -stat	Difference	<i>t</i> -stat
Average Rent	\$ 94	1.99	HH Income	\$ -1,011 -0.34
Pct Sound Condition	-1.41%	-1.27	Ln HH Income	0.11 1.09
Pct Built Pre 1947	1.98%	0.74	Age	-1.15 -1.82
Pct Built 1947-1989	-0.85%	-0.36	Pct Male	-0.51% -0.24
Pct Built Post 1990	-1.13%	-1.42	Pct White	-6.73% -2.21
Pct Less 3 Floors	19.90%	5.49	Pct Black	5.62% 1.62
Pct 3 – 10 Floors	-19.81%	-5.47	Pct Other Race	1.11% 0.77
Pct 11+ Floors	-0.09%	-0.46	Pct Hispanic	-0.11% -0.05
Pct w/ Elevator	-3.61%	-4.41		
... controlling for floors	-0.97%	-1.58		

*Note:* This table reports balance tests for the sample we use in Section 4. We subset the data to all privately owned rental units with 4 – 9 units in the pre-reform period. Panel A reports building level covariates; Panel B reports respondent level covariates. Column (1) shows the difference in means for the variable and (2) shows the SBA-cluster-robust *t*-stats. The row ‘Pct w/ Elevator controlling for floors’ also includes controls for number of floors in order assess whether the presence of an elevator indicates other unobservable amenities.

## B.2 NYC Buildings Data

The underlying source data for NYC buildings comes from combining multiple public administrative data sets from the NYC government. We combine the Primary Land Use Tax Lot Output (PLUTO), the Department of Finance Final Assessment Roll (FAR), the Multiple Dwellings Registration and Contacts (MDRC) datasets (with prior years graciously provided to us by the NYU Furman Center), and communications between the DOF and building owners, scraped off the Property Tax Public Access web portal, which we call the Notice of Property Value (NPV) dataset.

The PLUTO and FAR provide location, zoning, market value, and other building characteristics, and the MDRC reveals common ownership across buildings. The NPV includes information mailed to building owners including gross revenue and cost estimates and the number of rent stabilized units.

We collect all available datasets from 2007 to 2019. We only collect data that excludes parcels for 1-3 family buildings (i.e., we exclude NYC Tax Class 1 buildings) due to the fact that these buildings are assessed differently and, as a result, we cannot recover income or expense data for them. In addition, we exclude Staten Island parcels as there are very few large rental buildings in this borough.

The initial dataset features about 860,000 parcels per year, which includes all commercial buildings (specifically, NYC Tax Classes 2-4). We keep parcels with buildings that have a NYC Building Class C1-C9, D0-D9, S3-S5, or S9 at some point in their tenure in the dataset. This returns a dataset of rental buildings with about 87,000 parcels per year.

**Synthetic Tax IV Sample:** For our pass through results using our synthetic tax instrument, we subset the data using only the years 2011 to 2019. We do this because our financial data is most complete for these years and because from 2007 to 2010 there was a systemic change in property tax procedures. We also drop buildings where the average building rent is in the extreme tails of the distribution (0.1% and 99.9%). We then use data from 2011 as a baseline for creating our tax

based instruments and omit this year from the regressions.

Table A3 displays summary statistics for the NYC building panel data used in Section 5.

Table A3: Summary Statistics: 2007 - 2019 NYC Buildings Panel

Median Rent	\$ 1262.87
Median Expenses per Unit	\$ 853.19
Median Market Value per Sqft	\$ 74.04
Median CF Tax per Sqft	\$ 206.62
Residential Units	42.7
Years Since Construction	91.7
Years Since Renovation	61.1
Avg Unit Sqft	808.2
Pct w/ Elevator	33%
Unique Buildings	23,143

*Note:* This table reports summary statistics for the sample we use in Section 5—all privately owned rental buildings with 11+ units from 2007 to 2019. This data is sourced from public records and communications with buildings owners. See Section 2 and Appendix B for more details on data sources.

### B.3 Sourcing of Average Building Rent

Recovering building average unit rents is a key feature of this analysis that relies on three facts. By law, the NYC DOF assesses rental buildings based on their income generation. This is called income-based assessment. For single-use, residential rental buildings, this corresponds to the rent paid to landlords. For mixed-use rental buildings, we cannot separate the source of income between commercial and residential tenants. This leads us to restrict our sample to single-use residential buildings in all regressions. For all such buildings, NYC DOF income information comes from income and expenses reported on Real Property Income and Expense (RPIE) statements filed by owners with DOF. All income generating property owners are required to file these annually and face financial penalties for not doing so. NYC DOF uses these forms to generate tax assessments and reports these values to building owners in mailings called Notices of Property Values (NPVs), sent annually and also posted publicly online. NPVs confirm NYC DOF have received income and or expense information from owners and the amounts for each. We download these statements where they are publicly available for all buildings in our sample.

If an owner does not file, the DOF has the right to assign a market value based on its best judgement. NPVs note whether actual information was provided and we only record income and expenses derived from RPIE statements. The DOF also adjusts extreme outliers. Without access to the RPIE statements, it is not possible to determine which properties have been adjusted. However, owners have a financial stake in ensuring the information is correct.

## C The NYC Housing Policy Environment

Here we briefly describe the major policy constraints in NYC—zoning restrictions on quantity and rent stabilization on prices, their prevalence in the data, and our approach to their interaction with our empirical specifications.

## C.1 Zoning

*Zoning is a law that organizes how land may be used. It establishes an orderly pattern of development across neighborhoods and the city by identifying what may be built on a piece of property.*<sup>30</sup>

In 1916, New York was the first major city in the United States to adopt citywide zoning, and the zoning ordinance has been amended many times since. The city has four zoning district types: Residential, Commercial, Manufacturing, and Special Purpose. The Special Purpose districts modify an area within a district. Some residential districts (or areas within) may have a commercial overlay that allows for commercial space in a ground floor (and possibly a second floor) with residential units above.

The zoning law establishes the classes of use and the physical dimensions of a building on a given parcel of land. The zoning law often changes and new construction is subject to the current zoning regulations. This can imply that within a given block buildings can vary in several dimensions based on construction time.

**Zoning Concepts** There are numerous concepts involved in establishing the physical shape and dimensions of a building, and a full discussion is far beyond the scope for this appendix. However, some useful concepts for the physical dimension rules are: setbacks, building envelope, floor area ratio, open space ratio, and density factor.<sup>31</sup>

Setbacks are regulations about how far *back* a building must be *set* from some reference point. Street setbacks dictate how close the street-facing wall of a building must be from the street; building setbacks dictate how far back a portion of a building must be from its edge as height increases. The building envelope is the three dimensional shape that represents the maximum regulatory dimensions of a building; i.e., the true building must fit within the building envelope.

The floor area ratio (FAR) is the factor by which a parcel's lot area permits building area. For example, suppose a lot has area  $L_{area} = L_{width} \cdot L_{depth}$  and a FAR of  $f$ , then the allowable floor area of the building is  $B_{area} = f \cdot L_{area}$ . The open space ratio (OSR) is the percent of a lot that must have open space; i.e., that cannot be covered by the building. For example, given  $\{B_{area}, L_{area}\}$  and OSR of  $o$ , then the footprint of the building must be contained within  $L_{area} - o \cdot B_{area}$ . One use of these two tools is 'height factor buildings' where the zoning regulations can promote tall, skinny buildings. Specifically, to maximize floor area available, the number of stories of the building must be  $f/(1 - o \cdot f)$ . If  $f = (5/2)$  and  $o = (1/3)$ , then this results in a  $(5/2)/(1/6) = 15$  story building.

Density factors are "approximations of average unit size plus allowances for any common areas (NYC Zoning Glossary)," and when combined with floor area ratios result in the maximum number of dwelling units in a building. For example, if  $d$  is the density factor, then  $B_{area}/d$  is the maximum number of units allowed in the building.<sup>32</sup>

**Zoning Constraint Definition** We consider a building to zoning constrained if the building, given its current size and zoning policy, cannot add an additional minimum sized unit. If any of the following conditions are met, then we say a building is zoning constrained:

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<sup>30</sup>"What is Zoning?" (<https://www1.nyc.gov/site/planning/zoning/about-zoning.page>).

<sup>31</sup>See the NYC Zoning Glossary for more terms (<https://www1.nyc.gov/site/planning/zoning/glossary.page>).

<sup>32</sup>Values are rounded up only if the fractional remainder is greater than  $(3/4)$ .



1. Average Unit Area is greater than the maximum possible residential area of the building divided by current units plus one:  $(B_{area}/U) > (\text{Max}(\text{Res}_{area})/(U + 1))$ ,
2. The density factor is greater than the maximum possible residential area of the building divided by current units plus one:  $d > (\text{Max}(\text{Res}_{area})/(U + 1))$ , or
3. Building Area plus 300 sqft is greater than the maximum possible residential area of the building:  $B_{area} + 300 > \text{Max}(\text{Res}_{area})$ .

We find that zoning constraints affect about 60% of NYC with variation in levels across boroughs but little variation over time. Interestingly, Manhattan has the least zoning constrained buildings while Queens has the most.

## C.2 Price Controls

A full history of NYC's rent regulation is available from the city as "History of the [NYC Rent Guidelines] Board and Rent Regulation."<sup>33</sup> Rent regulations came to NYC from a 1920 state law allowing rent controls due scarcity in housing induced by the war-effort for World War One. Because the problem was of housing scarcity, the law (1) exempted all properties building after September 1920 from the law and (2) exempted all buildings built between 1920-1924 from property tax until 1932. The 1920 law expired at the end of 1929. Rent control returned in 1943 due to World War Two price controls. Rent control legislation was controlled at various times by the state and federal government, with the state assuming control after in 1951.

While rent control still exists for long-time incumbent renters, rent stabilization was introduced in 1969 and is the dominant form of rent regulation today. Both rent control and rent stabilization create the legal right to renew a lease, but the difference between the two is that rent control regulates the level of rents while rent stabilization regulates the growth in rents. Rent control applied to buildings built before 1947 while rent stabilization applied to buildings built between 1947-1974 (with six or more units), formerly rent controlled units, and units that accept J-51, 421-a, or 421-g tax benefits.<sup>34</sup>

While we speak of rent regulated buildings, regulations actually apply to specific units in buildings. That is, a building may have only zero, one, or many regulated units. Individuals can contact the Rent Guidelines Board to inquire about specific units; however, the best method to observe regulated units at the building level is through parsing tax communications with the Department of Finance, which we have done.

Rent stabilization in NYC is managed by the Rent Guidelines Board. The board oversees these issues and establishes rent rate increases. Broadly, the rate increase per year is the minimum of (1) 7.5% or (2) the average rent increase of the last five years. Individual landlords may request exemptions or special consideration based on hardships, agreements with the tenant, or major renovations.

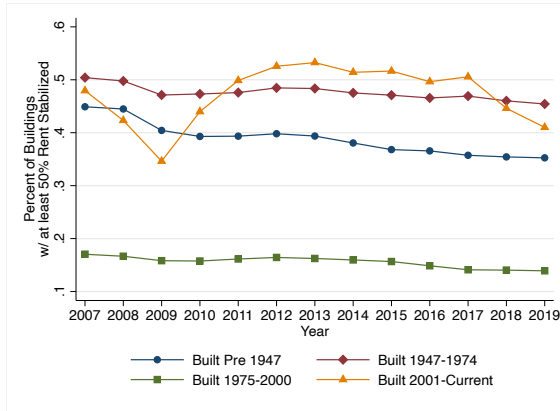
In our sample period, units in rent stabilized buildings could become unregulated ("destabilized") if upon being vacated the landlord could rent the unit above a certain amount. One method

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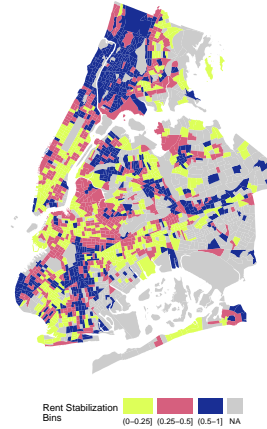
<sup>33</sup> Accessible online at [rentguidelinesboard.cityofnewyork.us/wp-content/uploads/2020/01/historyoftheboard.pdf](http://rentguidelinesboard.cityofnewyork.us/wp-content/uploads/2020/01/historyoftheboard.pdf)

<sup>34</sup> These benefits are for new building construction, conversions, and/or renovations.

Figure A.3: Distribution of Rent Stabilization



(a) Different Controls, Weighted



(b) Different Controls, Unweighted

Note: Panel (a) plots the percent of buildings that have more than 50% of units rent stabilized over 2010 to 2019 by building age groups. Buildings built after 1974 are not *de facto* rent stabilized. Buildings that take advantage of tax benefits for new construction, renovation, or conversion are rent stabilized for a fixed amount of time. Panel (b) maps the percent of buildings that have more than 50% of units rent stabilized, pooling across years, at the tract level.

of doing this was a renovation that allowed the landlord pass some cost of renovation to the rent by an amount enough to push the rent above a predetermined threshold.

**Rent Stabilization in NYC** 44% of units in NYC are under rent stabilization. However, at any one time, about one third of these fall below the binding constraint [Podkul \(2017\)](#). These units are leased at “preferential rent,” which is defined as any rent lower than the maximum allowable under stabilization. In discussions, former high-ranking DHPD officials suggested these are especially common in outer boroughs and northern Manhattan. We parse communications from the NYC Department of Finance to building owners that lists the number of regulated units in a building.

[Chen, Jiang, and Quintero \(2022\)](#) note that rent stabilized units are more likely to be cheaper, and that the longer a unit remains rent-stabilized the cheaper it will be. These facts are consistent with selection out of stabilization using the above characteristics, as well as with a dynamic pricing model where stabilized units experience less dynamic price increases but larger jumps at vacancies. A separate possibility is that rent-stabilization selects on or causes declines in unobservable unit amenities, which is a prediction of the large literature on price controls.

**Impact on Empirical Specifications** Despite not being akin to a true price control, rent stabilization can manifest as a bias in our empirical estimates. Our main reduced form specifications consider within-building changes in rents over time. To the extent to which some units are both regulated and regulations bind on those units over the long time period we study, we expect stabilized units to show lower rent responses. We expect this to bias our estimates downward.

To deal with this, an important robustness check we perform is to exclude buildings with many rent-stabilized units from the analysis. These robustness checks can be found in [Table A6](#). Overall, we still find pass-through results inconsistent with perfect competition.

## D Additional Tax Regime Reform DID Results

Here we present additional results for Section 4. Specifically, we present the regression results underlying Figure 1b, an event study figure without sampling weights, an event study figures with different controls, and a placebo event study figures using different groupings of buildings by number of units.

**Regression Results for Figure 1b** Table A4 displays the regression parameters underlying Figure 1b using the specification from equation 14. The regression includes sub-borough-by-year fixed effects, building controls, and lease controls, whose coefficient estimates are not presented. Standard errors are clustered by sub-borough-area.

Table A4: Event Study Regression Table

Dep.var: Log Unit Rent	
1.2002 · 1.{4,5}	0.01 (0.08)
1.2005 · 1.{4,5}	-0.01 (0.03)
1.2011 · 1.{4,5}	-0.13 (0.03)
1.2014 · 1.{4,5}	-0.13 (0.03)
1.2017 · 1.{4,5}	-0.08 (0.04)
Building Controls	Y
SBA-year FEs	Y
Observations	8,259

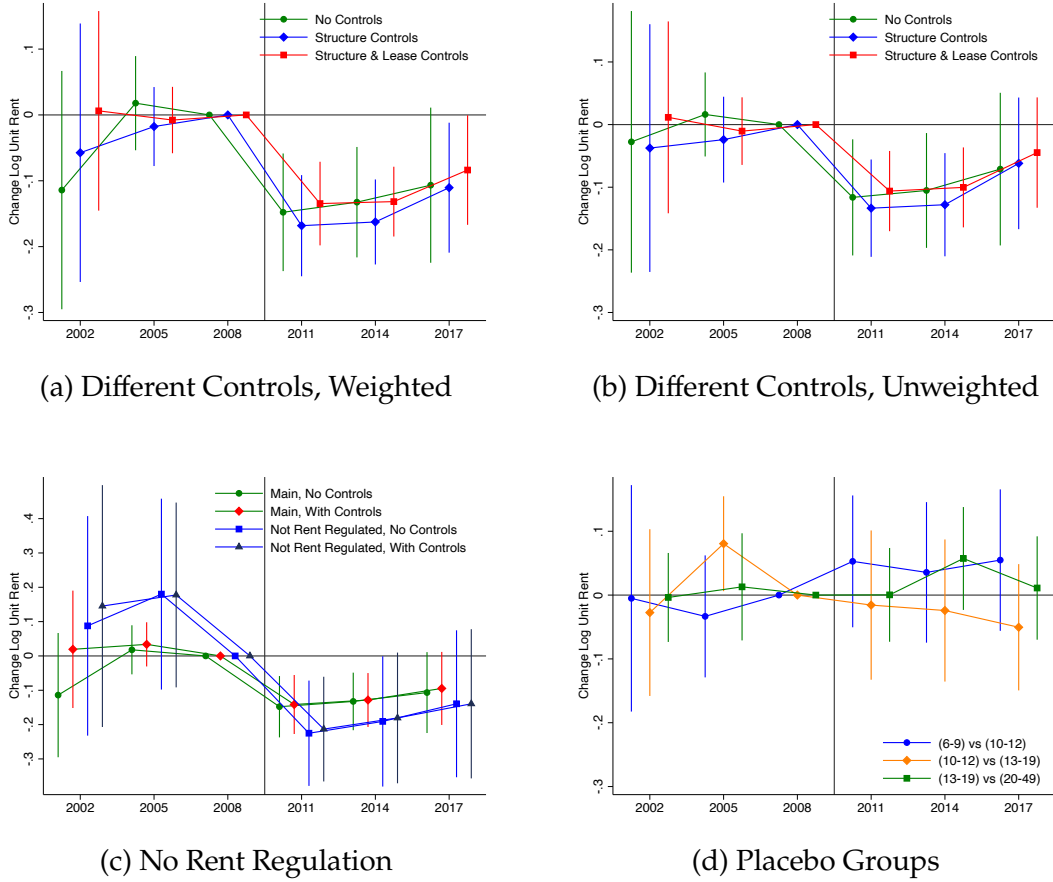
*Note:* The table displays the regression results for Figure 1b based on equation 14. Data is from the NYCHVS 2002 to 2017 samples. The regression controls for sub-borough-by-year fixed effects, building age fixed effects, condition of building indicators, number of floors groups, units in building, passenger elevator indicator, years occupant in unit, and length of lease. Standard errors are clustered by sub-borough-area.

**Additional Event Study Results** Here we present additional the event study results, similar to Figure 1b. In Figure A.4a, we present three sets of event study results: without controls, with only structure controls, and, our baseline from the main text, structure controls with lease controls. In Figure A.4b, we replicate the previous figure but without sample weights. While we prefer the specification with structure and lease controls and sample weights, these choices do not qualitatively change our findings of a decrease in rent following the tax reform.

In Figure A.4c, we compare our main specification with a subsample that drops all units that are rent regulated. The NYCHVS uses administrative data to code the regulation status of units, and using this coding we drop all rent *stabilized* and *controlled* units. As expected the subsample widens the confidence intervals, but does not qualitatively change our main findings that the policy reduced rents for the buildings.

In Figure A.4d, we present placebo results that compare the rent dynamics for different groups of buildings by number of units. There are arbitrary changes to the tax regime that idiosyncratically

Figure A.4: Additional Event Study Results



affect buildings, which is variation we use in Section 5. The change from gross-income-multiplier-based to capitalization-rate-based taxation for 11+ unit buildings in 2011 created differential tax effects for units with higher or lower net income to gross income ratios, but did not substantially affect average tax rates for this group [New York City Department of Finance \(2012\)](#). As can be seen, all the placebo groupings have systematically statistically and economically insignificant estimated dynamic treatment effects.

## E Additional Synthetic Tax IV Results

Here we present additional empirical results from Section 5.

**Alternate Samples** First, we probe robustness to our results in Table 2 using two alternative samples. We use a sample of only residential buildings and then a sample of buildings with less than 50% of units rent stabilized. Tables A5 and A6 report our results for both subsamples, which largely similar to our main specification with one exception: the latter sample has smaller-magnitude pass-through estimates.

Table A5: Pass Through Results: Residential Only Subsample

Dependent Variable: Log Rental Income				
	Reduced Form		2SLS	
	(1)	(2)	(3)	(4)
Log Cf Tax	0.038 (0.004)	0.031 (0.004)		
Log Total Cost			1.161 (0.077)	1.217 (0.107)
Robust F Stat			53.22	37.68
Robust AR Stat			78.90	63.31
One-Side Test			0.019	0.021
Time-varying controls	N	Y	N	Y
Tract-year FEs	Y	Y	Y	Y
Building FEs	Y	Y	Y	Y
Observations	112,823	112,823	112,809	112,809

Note: The table displays robustness results for Table 2 using a residential only subsample. Standard errors are clustered by Census tract and the first stage F statistics and the Anderson-Rubin F statistic for the estimated coefficients are cluster robust as well.

Table A6: Pass Through Results:  $\leq 50\%$  Rent Stabilized Subsample

Dependent Variable: Log Rental Income				
	Reduced Form		2SLS	
	(1)	(2)	(3)	(4)
Log Cf Tax	0.045 (0.008)	0.024 (0.006)		
Log Total Cost			0.886 (0.068)	0.829 (0.117)
Robust F Stat			44.54	26.68
Robust AR Stat			35.83	18.73
One-Side Test			0.953	0.928
Time-varying controls	N	Y	N	Y
Tract-year FEs	Y	Y	Y	Y
Building FEs	Y	Y	Y	Y
Observations	52,354	52,354	52,342	52,342

Note: The table displays robustness results for Table 2 using only buildings with less than 50% rent stabilized units. Standard errors are clustered by Census tract and the first stage F statistics and the Anderson-Rubin F statistic for the estimated coefficients are cluster robust as well.

**Alternative Markets** Next, we turn to the assumption of spatial markets. Again we emphasize that as long as spatial markets exist at a geography larger than Census tracts, fixed effects in our main specifications absorb market-level demand shifts. However, if markets are at a geography *lower* than tracts or *non-spatial* and demand shifts are correlated with our instrument, then our main specification may be mis-specified.

Below we show three alternative approaches. First, we switch to Census block-year fixed effects rather than Census tract-year. Second, in the main text we discuss the possibility that markets are extremely local and/or are actually heterogeneous and overlapping for each building based

on a spatial decay rate (i.e. “continuous”). While the main text’s balancing test sought to ensure neighbors’ rents were orthogonal to our instrument, here we adopt this hypothesis wholesale and use neighbors’ rents as controls meant to absorb demand shifters in place of or in combination with tract-level fixed effects.

Finally, under the possibility that markets are non-spatial and are instead segmented by buildings’ characteristics, we (1) perform balancing tests of our IV and (2) use the characteristic specific percentile group indicator interacted with year indicators to control for markets in a reduced form specification. In nearly all cases, our main results are quantitatively similar as our main specification.

**Block-Year Fixed Effects** First, we present results using Census block-year fixed effects rather than Census tract-year fixed effects. While we believe that spatial rental markets are likely at a *higher* geography than Census tracts, we show below in Table A7 that our results are robust to using Census block-year fixed effects as controls. This is consistent with Figure 2 that shows our instrument is uncorrelated with nearest neighbors’ rent.

Across specifications, our results are very similar to those in our main specification. The reduced form results are very close to those in Table 2 while the 2SLS results are slightly attenuated.

Table A7: Pass Through Results: Census block-year Fixed Effects

Dependent Variable: Log Rental Income				
	Reduced Form		2SLS	
	(1)	(2)	(3)	(4)
Log Cf Tax	0.039 (0.005)	0.028 (0.004)		
Log Total Cost			1.096 (0.078)	1.161 (0.124)
Robust F Stat			50.11	27.00
Robust AR Stat			70.08	42.10
One-Side Test			0.110	0.097
Time-varying controls	N	Y	N	Y
Block-year FEs	Y	Y	Y	Y
Building FEs	Y	Y	Y	Y
Observations	131,979	131,979	131,966	131,966

*Note:* The table displays robustness results for Table 2 using Census block-year FEs rather than Census tract-year FEs. We estimate the (1,2) the reduced form regressions of the Tax IV on log rental income and (3,4) a 2SLS regression where we instrument log total building costs using the Tax IV. All models include building fixed effects, along with controls for log distance to nearest subway station, log age, log years since renovation, log average unit square-feet, and an indicator for having an elevator. Standard errors are clustered by Census tract and the first stage F statistics and the Anderson-Rubin F statistic for the estimated coefficients are cluster robust as well.

**Nearest Neighbors** If residential search markets are continuous, then each building is subject to a unique set of competitors which overlap. While a full theoretical and structural model using this assumption is beyond the scope of this paper, we note that if renters search geographically, the overlapping set of renters between two competitors is decreasing in their distance. The extent to which a competitor’s rents affect competitive environment at any location is also therefore a function

of distance, and the market rent at each location will be a weighted averages of competitors rents.<sup>35</sup> To flexibly capture this alternative market definition, for each building, we include the buildings’ neighbor’s rent according to the neighbor’s distance rank.

We run the following regressions to show that our use of our synthetic tax instrument is robust to the inclusion of local competitors:

$$\ln[r_{jgt}] = \gamma_1 Z_{jgt} + \gamma_2 F(\{\ln[r_{hgn,t}\}_{j_t(20)}\}) + \gamma_3 X_{jgt} + \gamma_4 D_j + \gamma_5 D_{gt} + \nu_{jgt} \quad (36)$$

where  $j_t(20)$  is the set of the 20 nearest neighbors of building  $j$  (which we call the focal building) in year  $t$  and  $F(\cdot)$  is a function of those neighbors’ rent. To deal with missings, we use two different functions: (1) a five neighbor rolling average; (2) linear controls for  $n$ th neighbors’ rent where we interpolate missing data on neighbors’ rent.<sup>36</sup> Under the assumption that neighbors’ rent in missing at random, then our approach is equivalent to if we observed all neighbors’ rents.

As long as  $\gamma_1 > 0$  under either specification, our results presented in the main text are robust to spatial market definitions that are smaller than the Census tract level or are continuous in nature. Table A8 displays the results of the above nearest neighbor specification. Columns (1) and (2) use a five neighbor rolling average approach, and columns (3) and (4) use the building level interpolation approach. In addition, we consider different sets of fixed effect controls: (1) and (3) use year and building FEs; (2) and (4) use tract-year and building FEs. Across specifications we find that the reduced form results are essentially the same as in Table 2 column (2).

Table A8: Pass Through Results: Nearest Neighbors Regression

Dependent Variable: Log Monthly Rent				
	Rolling Average		Interpolated	
	(1)	(2)	(3)	(4)
Log Cf Tax	0.022 (0.003)	0.022 (0.004)	0.025 (0.003)	0.025 (0.003)
Time-varying controls	Y	Y	Y	Y
Building FEs	Y	Y	Y	Y
Year FEs	Y	N	Y	N
Tract-year FEs	N	Y	N	Y
Observations	145,533	144,151	153,509	151,967

Note: The table displays robustness results for Table 2 using an alternative market definition assumption. Columns (1,2) use a rolling five year average while (3,4) use a linear interpolation of missing values to estimate eq 36. Columns (1,3) use year FEs and building FEs while columns (2,4) use tract-year and building FEs. All columns include controls for log distance to nearest subway station, log age, log years since renovation, log average unit square-feet, and an indicator for having an elevator. Standard errors are clustered by census tract.

**Non-spatial Placebo Tests** In the main text we considers whether nearest competitors’ rents adjust in response to a building’s instrument value. We can repeat this test checking for a positive

<sup>35</sup>If modelled, the result would be that the distance parameter in renters’ search would be the same governing a “market access” measure at each location, isomorphic to a location rent index, as in gravity trade models.

<sup>36</sup>Because we are including twenty neighbors, interpolation or rolling averages are necessary to maintain a reasonable sample size. Allowing for missing data reduces our sample size since, if any one of the twenty neighbors is missing rent information, then we lose the focal building observation.

correlation between a building’s instrument and close competitors in any dimension. We divide buildings into percentiles using each observed characteristics: (1) distance to a subway station, (2) building age, (3) years since major renovation, (4) average unit size, (5) number of units, and (6) adjusted gross income of the building’s zipcode. For each of these characteristics, we assign each building into its characteristic specific percentile group. We use each building’s 2007 values of characteristics to avoid changes in groupings that could be due to the tax changes.

Table A9 first creates characteristic specific percentile group leave-one-out averages of rent and then regresses that average on the our instrument using our main specifications’ fixed effects:

$$\frac{1}{N_{G^C(j)}} \sum_{h \in G^C(j)} \ln[r_{h,g,t}] = \zeta_1 Z_{jgt} + \zeta_2 X_{jgt} + \zeta_3 D_j + \zeta_4 D_{gt} + u_{jgt}, \quad (37)$$

where  $G^C(j)$  is the set of buildings in the same characteristic specific percentile group as building  $j$  but without building  $j$ , the dependent variable is the leave-one-out average value of log monthly rent for that group, and all other notation is the same as equation 17.

As shown, we observe no systematic correlation between the instrument and the neighbor-average, and can reject a positive correlation for renovation date and area AGI. These results support the claim that our instrument uses idiosyncratic variation and identifies the effect of an idiosyncratic shocks. Interactions between non-spatial and spatial market segmentation also do not substantially change our results. These tables are omitted for brevity but available upon request.

Table A9: Non-spatial Placebo Tests: X-Group Instrument Correlations

Dependent Variable: X-Group Neighbors’ Monthly Rent					
	Subway Dist (1)	Building Age (2)	Yrs Since Renovation (3)	Avg Unit Size (4)	Zipcode AGI (5)
$\zeta_1$	-0.000	-0.002	-0.009	0.000	-0.002
se	(0.002)	(0.002)	(0.004)	(0.002)	(0.001)
t	0.000	0.934	2.212	0.109	1.919
CI	(-0.005,0.005)	(-0.007,0.002)	(-0.017,-0.001)	(-0.004,0.005)	(-0.003,0.000)
Time-varying controls	Y	Y	Y	Y	Y
Building FEs	Y	Y	Y	Y	Y
Tract-year FEs	Y	Y	Y	Y	Y
Observations	152,638	152,638	152,638	152,638	152,638

Note: The table displays the correlation of our instrument with characteristic specific percentile group leave-one-out averages, specified in the column names, estimated in equation 37. All columns include building and tract-year FEs as well as controls for log distance to nearest subway station, log age, log years since renovation, log average unit square-feet, and an indicator for having an elevator. Standard errors are clustered by Census tract.