A Test for Pricing Power in Urban Housing Markets*

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Abstract

The presence of pricing power in housing markets alters our understand of the housing supply and land-use policies. It biases estimates of housing production functions and supply elasticities and the results of quantitative spatial models. We test for pricing power in the New York City rental market. We make use of a series of tax policy shifts to conduct complementary difference-in-differences and instrumental variable designs. Holding fixed market prices, we find an increase in a single building's idiosyncratic costs results in a similar magnitude increase in its rents, consistent with the existence of pricing power and inconsistent with perfect competition.

Keywords: housing supply, market power, housing demand

JEL Classification: R31, R38, L13

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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 into 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. Using complementary difference-in-differences and IV approaches, we find that lessors pass idiosyncratic cost increases onto rents, which is not possible when housing is provided competitively and each lessor faces perfectly elastic demand for their units.

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: an average cost shift shared by all lessors in the market and an idiosyncratic shift deviating from the average. 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. Through its effect on market rents, the common component shifts lessors' residual demand. The shock's idiosyncratic component does not aggregate, does not alter market rents, and cannot be passed through. By contrast, when demand is downward sloping, both components affect rents. Our empirical specifications seek to control for market-level fluctuations—the effect of the common component—thereby isolating the idiosyncratic shocks' effects.

We generate a data set of rental income and leasing expenses of NYC multifamily rental buildings by collecting data from the NYC Department of Finance's (DOF) publicly-posted communications with individual lessors. To explain how assessed taxes are calculated, the DOF sends annual letters to lessors reporting back to them the income and leasing expenses they provided to DOF on tax forms. We merge this data with information from public sources and supplement the resulting data with an apartment-level panel data set on rents and building characteristics from the New York City Housing and Vacancy Survey (NYCHVS).

Using a series of tax policy changes between 2007 and 2019, we pursue two strategies to isolate exogenous fluctuations in costs. 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 47%, or an average decline of $231 per unit-month. Comparing unit rents before and after this shift for $\{4, 5\}$ versus $\{6, 7, 8, 9\}$ unit buildings, we find that rents fell 12% for the treated group—on average $170 per unit-month (three quarters of the tax decrease)—relative to the controls' rents. Under the assumption that treatment and control face identical market trends, our results identify the pass-through of idiosyncratic costs onto rents, which is inconsistent with perfect competition.

Our second approach employs a synthetic tax IV and saturating fixed effects to identify building-idiosyncratic cost shocks. After 2010, large buildings' net incomes were converted into valuations using capitalization rates derived from an annually changing, nonlinear formula. We use these formula changes in conjunction with buildings' earliest reported (2007) income to predict post-2010 taxes. Because the formula is set city-wide, the effect of predicted tax changes on future rents is orthogonal to lessors' behavioral responses. Conditioning on building and Census tract-year fixed effects, the specification uses only variation approximately idiosyncratic to individual buildings.

We find that a 1% increase in the idiosyncratic component of a buildings' predicted taxes is associated with a 0.03% increase in rents, indicating that lessors pass idiosyncratic cost shocks onto renters, again rejecting the perfect competition null. Further, instrumenting for total costs using reported leasing costs, we find this corresponds to a total pass-through rate of over 100%.[1] The high pass-through rates from both approaches are consistent with lower demand elasticities and substantial curvature, indicating that lessors' pricing behavior is far from negligible.[2]

A key assumption of this second approach is that our instrument and fixed effects jointly account for residual demand shifts. In particular, close competitors in tightly-

[1]Over-shifting is consistent with sufficiently convex demand (Weyl and Fabinger, 2013; Pless and van Benthem, 2019).

[2]We are predominantly focused on rejecting the null. Conditional on doing so, further extrapolating from our results to elasticities requires additional assumptions on market-level shocks' heterogeneous effects.

defined markets may have correlated shocks that are not absorbed by fixed effects. Comparing the value of our instrument to rents of buildings' nearest competitors, we find no evidence that our instrument is correlated with market-level rent shifts in the error term.[3]

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.[4] 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 estimates may need to be revisited.[5] Finally, it is worth noting a major downstream user of such elasticities, structural spatial models, which use them when undertaking counterfactual analyses (Ahlfeldt et al., 2015; Severen, 2023; Brinkman and Lin, 2022). Markups and the behavioral response of owners via changing markups could bias both estimates of spatial reallocations and their welfare effects in counterfactual equilibria.

Two literatures do implicitly assume downward sloping demand for individual build-

[3]Appendix D explores robustness to alternative market definitions, spatially continuous and overlapping markets, and non-geographic markets segmented by building or location observables.

[4]Appendix A.3 shows pricing power reduces density and increases sprawl in a monocentric city.

[5]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.

ings or houses. Arnott (1989); Arnott and Igarashi (2000); Basu and Emerson (2003) examine monopoly power as a theoretical justification for rent control.[6] Second, 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). 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. Relatedly, cost pass-through has been used as evidence of market power in other settings (Brissimis and Kosma, 2007; Fabra and Reguant, 2014; Loy, Weiss, and Glauben, 2016; Duso and Szücs, 2017; Pless and van Benthem, 2019).[7] We propose the prevailing hypothesis in urban economics as our null, and test it using differential predicted behavior.

2 Data

We construct a building-level data set spanning 2007 to 2019 for private, multi (4+) unit buildings in NYC.[8] We combine our collected data on rents and expenses from public letters to landlords with two public data sets 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

[6] Diamond, McQuade, and Qian (2019) consider rent controls' effects on exit, which we discuss in A.1.
[7] The particulars of our setting make these tests unsuitable for our purposes.
[8] We omit Staten Island buildings due to the low number of large rental buildings per tract.

of the lease) in a repeated cross section from 2002 to 2017.[9]

Appendix Tables A1 and A2 present summary statistics for buildings in our sample. For further details on sample construction, see Appendix B. Appendix C discusses the NYC regulatory environment in detail.

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 prices. 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 discusses 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 further 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 f 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:

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

where Δ_{mt} is the average log change in marginal costs across all buildings in the market and ϵ_{fmt} is the residual change of the policy on f in addition to the average change.

In both market settings, rental prices respond in qualitatively similar ways to shifts in Δ_{mt}, which aggregate across firms and shift residual demand. However, rents only

[9]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.1 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.

respond to idiosyncratic shifts ϵ_{fmt} under imperfect competition.[10] Our empirical strategy aims to isolate variation corresponding to idiosyncratic marginal cost changes, ϵ_{fmt}, and test the hypothesis that the elasticity of rents with respect to those changes, $\mathsf{d}\ln(r_{fmt})/\epsilon_{fmt}$ is zero.[11]

Now, with data on rents and marginal cost we could estimate the following regression:

$$\ln(r_{fmt}) = \beta \cdot \ln(mc_{fmt}) + \lambda_f + \lambda_{mt} + U_{fmt}, \tag{2}$$

where λ_f is a building fixed effect, λ_{mt} is a market-time fixed effect, and β is the average building rent-marginal cost elasticity. Although β 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:

$$\begin{aligned}
& \overline{\ln(r_f)} + \mathsf{d}\ln(r_{fmt}) = \beta\left(\overline{\ln(mc_f)} + \mathsf{d}\ln(mc_{fmt})\right) + \lambda_f + \lambda_{mt} + (\overline{u_f} + u_{fmt}) && (3)\\
\implies \quad & \mathsf{d}\ln(r_{fmt}) = \beta\left(\mathsf{d}\ln(mc_{fmt})\right) + \tilde{\lambda}_f + \lambda_{mt} + u_{fmt} && (4)\\
& = \beta\left(\Delta_{mt} + \epsilon_{fmt}\right) + \tilde{\lambda}_f + \lambda_{mt} + u_{fmt} && (5)\\
& = \beta\epsilon_{fmt} + \tilde{\lambda}_f + \tilde{\lambda}_{mt} + u_{fmt}, && (6)
\end{aligned}$$

where we substitute equation 1 into 4. That is, the market-time fixed effects absorb common shocks, so that when observed building-level marginal cost variation is used in conjunction with building and market-time fixed effects, the coefficient β identifies the elasticity of rents with respect to idiosyncratic marginal cost shocks, ϵ_{fmt}. 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 ϵ_{fmt} (i.e., mc_{fmt} controlling for

[10]For a visual representation of this analysis, see figures and discussion in Appendix A.2.

[11]This approach complements Weyl and Fabinger (2013); Pless and van Benthem (2019); Ritz (2019), who study *market-level* pass-through, which we cannot implement without supply elasticities. We highlight that under pure competition *idiosyncratic* shocks do not affect prices, only quantities. Pless and van Benthem (2019) show that over-shifting is a sign of pricing power in competitive markets. Ritz (2019) show more competitive markets generate lower pass-through of market-wide shocks. Competitive lessors cannot pass-through the *idiosyncratic* shocks we focus on.

building and market-time fixed effects) is uncorrelated with u_{fmt}, and specifically residual demand, for example, through 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 ϵ_{fmt}.

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 ($\epsilon^C = 0$):

$$\begin{aligned} \mathsf{d}\ln(r^T_{fmt}) - \mathsf{d}\ln(r^C_{jmt}) &= \left(\beta \cdot \mathsf{d}\ln(mc^T_{fmt}) + \lambda_{mt} + u^T_{fmt}\right) - \left(\beta \cdot \mathsf{d}\ln(mc^C_{fmt}) + \lambda_{mt} + u^C_{fmt}\right) &(7)\\ &= \left(\beta \cdot \left[\Delta_{mt} + \epsilon^T_{fmt}\right] + \lambda_{mt} + \right) - \left(\beta \cdot \left[\Delta_{mt} + \epsilon^C_{fmt}\right] + \lambda_{mt} + u^C_{fmt}\right) &(8)\\ &= \beta \cdot \epsilon^T_{fmt}. &(9) \end{aligned}$$

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

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

$$\begin{aligned} \text{FS:} \quad \ln(mc_{fmt}) &= \pi \cdot Z_{fmt} + \lambda^{\text{FS}}_f + \lambda^{\text{FS}}_{mt} + v_{fmt} &(10)\\ \text{SS:} \quad \ln(r_{fmt}) &= \beta \cdot \ln(mc_{fmt}) + \lambda^{\text{SS}}_f + \lambda^{\text{SS}}_{mt} + U_{fmt} &(11)\\ &= \beta \cdot \epsilon_{fmt} + \tilde{\lambda}^{\text{SS}}_f + \tilde{\lambda}^{\text{SS}}_{mt} + u_{fmt}, &(12) \end{aligned}$$

with assumptions $\mathsf{Cov}(\epsilon_{fmt}, Z_{fmt} \mid \lambda_f, \lambda_{mt}) > 0$ and $\mathsf{Cov}(u_{fmt}, Z_{fmt} \mid \lambda_f, \lambda_{mt}) = 0$.

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, nearly halved the tax burden on $\{4, 5\}$ unit buildings. Using this sample as group T, we assign 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 will estimate the pass-through of idiosyncratic leasing costs onto

[12] For expositional reasons, we described 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.

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. 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 plots of dynamic treatment effects. In Appendix D, we present robustness checks, including placebo policy groups and differing control variables.

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.[13]

We run the following specifications:

$$\text{DID} \quad Y_{jgt} = \theta_1 1[\{4,5\}] + \theta_2 1[t > 2010] \cdot 1[\{4,5\}] + \theta_3 X_{jgt} + \theta_4 D_{gt} + \varepsilon_{jgt} \tag{13}$$

$$\text{ES} \quad Y_{jgt} = \theta_1 1[\{4,5\}] + \sum_{s=\underline{t}}^{\bar{t}} \theta_s D^s_{jgt} + \theta_2 X_{jgt} + \theta_3 D_{gt} + \varepsilon_{jgt}, \tag{14}$$

for outcomes (1) log assessed taxes per unit and (2) log unit rent, where D^s_{jgt} are indicators for each years interacted with treatment (omitting 2008), X_{jgt} are building observables, and D_{gt} are SBA-year fixed effects. We use NYCHVS provided sample weights. However, Appendix D.1 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.

[13]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: First Stage Dep.var: Log Assessed Property Tax per Unit	(1)	(2)	(3)
$1.[t > 2010] \cdot 1.\{4, 5\}$	-0.71	-0.63	-0.64
	(0.01)	(0.01)	(0.01)
Building Controls	N	N	Y
Tract-year FEs	N	Y	Y
Unique Buildings	49,052	48,905	47,864
Observations	547,314	545,494	544,452

Panel B: Reduced Form Dep.var: Log Unit Rent	(4)	(5)	(6)
$1.[t > 2010] \cdot 1.\{4, 5\}$	-0.05	-0.12	-0.12
	(0.04)	(0.03)	(0.02)
Building Controls	N	N	Y
SBA-year FEs	N	Y	Y
Observations	8,259	8,259	8,259

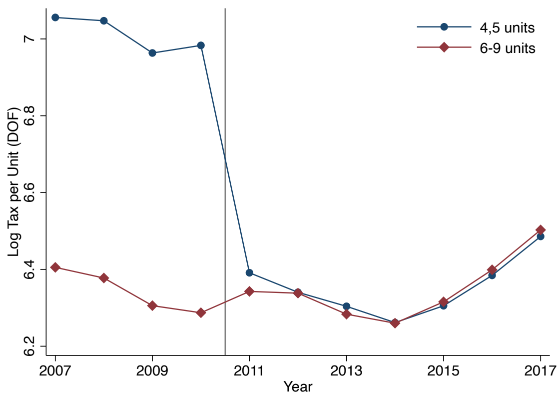
Note: The table reports two sets regressions using tax reform variation that harmonized capitalization rates for small rental buildings in 2011. Panel A reports three DID specification with log assessed property tax per unit as the dependent variable using annual 2007-2017 data from the NYC DOF panel 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

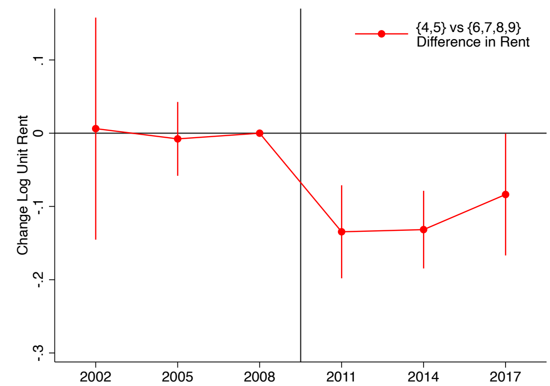
Table 1 reports difference-in-differences coefficient results in two panels. Panel A displays results of the effect of the regime shift on log average unit property taxes from DOF building-level, annual data, while Panel B displays results on log rents from the unit level, triennial NYCHVS data. We cluster 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).

In line with the claim of an internal Audit of the 2011 tax year changes and its affects on multifamily taxes (New York City Department of Finance, 2012), column (3) reports that the reform reduced assessed property taxes per unit for the small buildings by a substantial

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



(a) Tax Regime Shift



(b) Dynamic Treatment Effects

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 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.

-0.64 log points, or 47%. This amounts to an average reduction of $231 per unit per month. Note, this is not the percent change in total leasing costs, which are not reported for small buildings.[14] Panel B reports the the effects of the policy shift on rents. Column (6) reports that the reform reduced unit rents by 12%, or an average reduction of $170 per month. While we lack cost information necessary for a difference-in-differences IV result, when comparing the dollar amount of the tax reform to the rent effect, the implied pass-through rate is roughly three quarters. Under the assumption that $4-9$ unit buildings face the same aggregate trends, this result is inconsistent with perfect competition.

Figure 1 assesses this assumption and 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.1 details several robustness checks, including rerunning our event study with different sets of controls, removing sample weights, and using placebo treatment groups.

[14]Because the DOF assesses small buildings on *gross* income, it does not discuss RPIE expense information in the letters to these lessors.

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.

In particular, gross income per square foot (GIPSF) is mapped to capitalization rates (CAP) using the following formula:

$$\text{CAP}_{j,0,t} = \alpha_t^0 + \text{GIPSF}_{j0}^{\alpha_t^1} + \alpha_t^2 \cdot \ln[\text{GIPSF}_{j0}]. \tag{15}$$

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.[15]

These reports also explain (to a degree) the methodology by which the parameters are determined on an annual basis. Coefficients are *not* building specific, but determined through quantile regressions. While DOF does not publish these specifications, it describes their motivation: the first and third terms jointly generate the predicted median annual growth in building price from a sample of repeat sales, while α_t^1 is the predicted conditional median relationship between income per square foot and sales price divided by rental income from the same sample. If 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

[15] 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.

only affect lessor pricing decisions through their taxes.

This gives us a counterfactual $\widehat{\text{CAP}}_t$ for each year and building. We then use the base net-income (NI), base building area, $\widehat{\text{CAP}}_{jt}$, and the city-wide multiplier ETR_t, provided on the DOF's website, to calculate the (log) counterfactual property tax according to the DOF formula:

$$Z_{jgt} = \ln\left[(\text{NI}_{j0}/(\widehat{\text{CAP}}_{jt} + \text{ETR}_t)) \cdot \text{ETR}_t) \right], \tag{16}$$

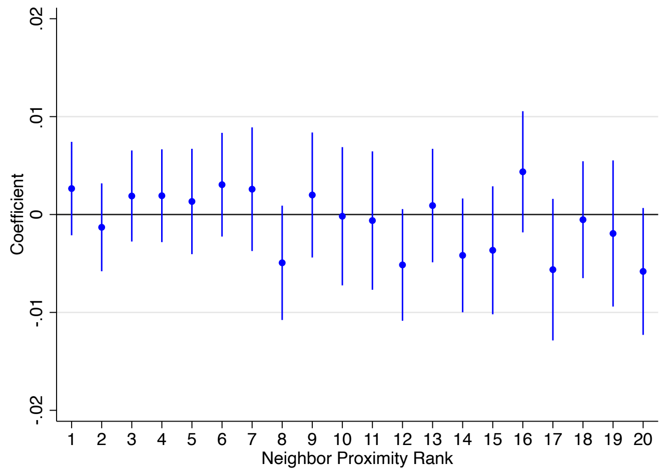
where Z_{jgt}, our instrument, is building j's predicted tax in year t and tract g.

Market Size and Fixed Effects Implementing equation (6) requires us to choose a definition of a market. If we specify a market size *above* the true market's level of aggregation, we will contaminate our estimate of β with market-level fluctuation. By contrast, specifying a market size *below* the true size will still fully and correctly absorb all market-time variation. As a baseline, we use tract-year fixed effects, believing that tracts are smaller than the relevant markets. More generally, within-tract demand shifts can contaminate results. Below, we examine the co-movement of our instrument with aggregate rent fluctuations at even finer geographies. Further, in Appendix D, we more seriously examine the possibility that markets are continuous and overlapping by using neighbors' prices in our specifications as controls in place of (and, separately, alongside) the tract fixed effects in our main specification. There, we also run robustness checks redefining markets at the block level, as segmented on non-geographic observables, and as combinations of the two.

Exclusion Restriction and Balancing Test Rephrasing the assumption that tract-year fixed effects control for market-level variation as an exclusion restriction, our claim is that changes to a building's relative tax burden due to annual changes in the city's building-specific capitalization rate calculation is uncorrelated with changes in the building's demand. Similar to the parallel trends assumption in the difference-in-differences specification, this assumption means our instrument must be orthogonal (conditional on controls) not only to the common component of the shock but to any other demand shift—for example, localized demand spillovers. If, after controlling for tract-year fixed effects, buildings that experience relatively higher tax burden increases also systematically experience increases in demand within tracts, our estimates will be upwardly biased.

To test this assumption, Figure 2 reports correlation coefficients between a building's instrument value and its nth nearest neighbor's rents, conditional on our specification's controls. Reassuringly, we find no systematic correlation between our instrument and neighbors' rents, using the controls in our specification. This is robust to a multitude of alternative specifications, including grouping all the top 10 or top 20 neighbors, and drawing concentric circles around buildings and including all the neighbors therein.

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 price at their nth 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.

In Appendix D, we divide buildings in the first year of our sample in to percentiles based on observables (size, age, unit size) and regress mean rent of each percentile group-year against our instrument and controls. Similar to our geographic results, using those market definitions we can exclude the possibility that rent movements at the market level under those alternative definitions of markets are positively correlated with our instrument.[16]

Specification Implementing equation (6), we first regress building rents on the cost shock using the following reduced form specification:

$$\ln[r_{jgt}] = \gamma_1 Z_{jgt} + \gamma_2 X_{jgt} + \gamma_3 D_j + \gamma_4 D_{gt} + \nu_{jgt}, \tag{17}$$

with building fixed-effects D_j and tract-year fixed effects D_{gt}, and changes in observable building characteristics X_{jgt}.

[16]Another concern we explore but do not include in Appendix D's discussion is the threat of variable trends correlated with base-year income. Controlling for year trends by initial gross income increases the coefficient's magnitude by about 17%.

Table 2: The Pass-through of Costs Shocks onto Rent

	$\ln$[Average Rent]			
	Reduced Form		2SLS	
	(1)	(2)	(3)	(4)
Log Cf Tax	0.036	0.028		
	(0.003)	(0.003)		
Log Total Cost			1.196	1.282
			(0.075)	(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

Note: The table reports multiple regressions using log average unit rent as the dependent variable. 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.

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

$$\text{First Stage:} \quad \ln[TC_{jgt}] = \pi_1 Z_{jgt} + \pi_2 X_{jgt} + \pi_3 D_j + \pi_4 D_{gt} + \varepsilon_{jgt} \tag{18}$$

$$\text{Structural Eq:} \quad \ln[r_{jgt}] = \beta_1 \ln[TC_{jgt}] + \beta_2 X_{jgt} + \beta_3 D_j + \beta_4 D_{gt} + \upsilon_{jgt} \tag{19}$$

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

5.2 Results

Table 2 displays our results. Columns (1) presents reduced form results. Column (2) adds time-varying controls. We find that a 10% increase in tax policy or expected expenses leads to a roughly 0.3% increase in rent. Columns (3) and (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. Appendix D details several robustness checks, including excluding rent stabilized units, using finer geographic controls and fixed effects, non-geographic market fixed effects, and combining geographic and non-geographic market fixed effects.

These results indicate larger pass-through rates than our difference and difference results. There are two explanations we believe reconcile the two. First, our IV results are a local average over different sample (4-5 unit rentals versus buildings with on average more than 50 units). Second, the IV results combine a mix of relative tax increases and decreases, while the difference-in-differences examines an absolute cost decrease. If rents are sticky, we would indeed expect pass-through to be muted in our difference-in-differences. Finally, we note that our difference-in-differences estimates coincide more perfectly with some robustness checks in Appendix D.

6 Discussion and Conclusion

What Causes Pricing Power? We consider three of the possible market structures that might underpin our findings. First, product differentiation in housing may produce downward sloping demand. Renters may have idiosyncratic valuations of building locations or amenities on any number of observable and unobservable dimensions.[17] The extent to which this might generate demand elasticities in line with our pass-through results is beyond the scope of this paper.[18]

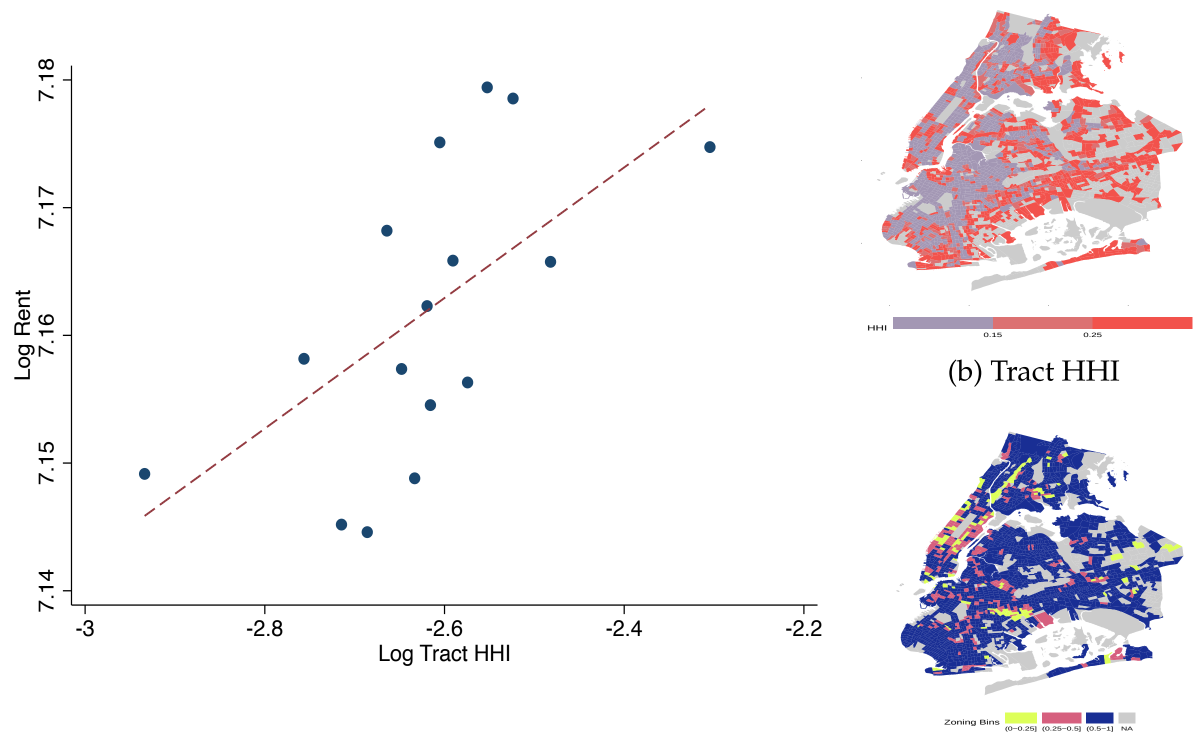
There has been substantial renewed scholarly interest in the effect of concentration on housing market outcomes. While NYC is dense with many buildings competing for renters (implying strong competitive pressure), as discussed above, it's unlikely that the full city is a single integrated market, and determining the correct size of housing markets in NYC is beyond the scope of this paper. As we examine the relationship between concentration and rents below, we continue to use tracts, knowing these likely underestimate market size. Using information from public records on building ownership and contacts, we reconstruct the likely ownership of buildings in each tract and calculate a Herfindahl–Hirschman index (HHI) for each tract.

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

[17] Alternatively, vertical differentiation in combination with heterogeneous valuations emerging from, e.g., income, would generate sorting and downward sloping demand (Shaked and Sutton, 1983).

[18] Watson and Ziv (2021) estimates a model of idiosyncratic demand.

Figure 3: New York City Housing Market Structure



(b) Tract HHI

(a) The Relationship Between Concentration and Rents

(c) Zoning Constraints

Note: Panel (a) plots the binned scatter (blue dots) overlaid with the linear regression (red line) of 2007 vs 2019 difference in log tract HHI on log rents, estimated as a panel controlling for time-varying buildings characteristics, building FEs, and a 2019 indicator, clustering by tract. The regression coefficient β_{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.

Considering this, we map tract HHI measures in panel (b) . In red, over 30% of tracts—especially those in the periphery and downtown and midtown Manhattan—are above an HHI of 2500, a standard threshold for concentration. It is crucial to note that because true markets are likely defined above the tract level, 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 the existence of capacity constraints. In the short run, the relevant set of competitors for any building in any given year is inherently constrained by the time required to add capacity (National Association of Home Builders, 2022). In the long run, capacity is constrained by zoning. Panel (c) of Figure 3 maps the percent of lots in each tract that we calculate cannot build even one additional unit of minimal legal size without running afoul of one or more regulations.

Tracts where over 50% of parcels are constrained, in blue, dominate the city. Overall, roughly 65% of NYC's housing stock is at a binding zoning capacity constraint according to our definition.

Together with costly, directed search, equilibrium pricing in such markets may not feature Bertrand pricing (Geromichalos, 2014).[19] Intuitively, the rule of "two is enough" to generate competitive pricing relies on the threat of competition to absorb all demand if a lessor raises rents. This threat is no longer sub-game perfect if a lessor's competitors can't credibly commit to servicing additional demand. To our knowledge, urban 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 an elastic extensive margin of supply. It's beyond the scope of this paper to establish in which markets these forces are strongest and weakest.

Concluding discussion and 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 perfectly competitive demand, and thus imply that when pricing, lessors face decreasing marginal revenue schedules and rents are set with markups.

The existence of markups, in turn, 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.[20] Moreover, the wedge between marginal costs and rents has been used as

[19]Market structures such as these are difficult to model in a manner delivering a pure strategy Nash equilibrium (Benassy, 1989, 1991; Wauthy et al., 2014). Under some conditions, competition under constraints yields Cournot pricing, (Kreps and Scheinkman, 1983; Maggi, 1996).

[20]Appendix A.3 presents a monocentric city model and shows that pricing power leads to less dense,

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

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 in the literature. However, in the presence of variable markups, specifications used in their estimation are potentially biased and at least require additional assumptions. For example, 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 with the added assumption of a constant elasticity of demand.

Finally, the growing quantitative spatial modeling literature uses marginal cost pricing to estimate counterfactual equilibria. In these models, the predicted consequences and welfare implications of policy changes such as zoning deregulation, transportation infrastructure improvement, or tax policy shifts—all of which result in localized changes in demand for housing—are biased if the effect of potentially differential and increasing markups are not directly modeled.[21]

Further work A limitation of our reduced-form approach is that we are unable to estimate the demand elasticity; without information on marginal costs, we are unable to infer the slope of the demand curve from the pass-through level alone. Our results call for more empirical work exploring the sources and scope of pricing power and theoretical work on the policy consequences of pricing power in housing markets and the effects of pricing power on housing supply.

wider cities.

[21] A notable exception is Ospital Greslebin (2023) who assumes monopolist landlord pricing when calculating counterfactuals.

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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 prices each period knowing that higher prices 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 price 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.[22] 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

[22] 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.

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' prices, the latter appears unlikely to be affecting our result.

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 price for their building results in a complete collapse in demand.[23] Here, we explain how shocks to

[23]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.

costs only affect market prices through the common component while the idiosyncratic component affects the firm's supply but cannot affect its posted price.

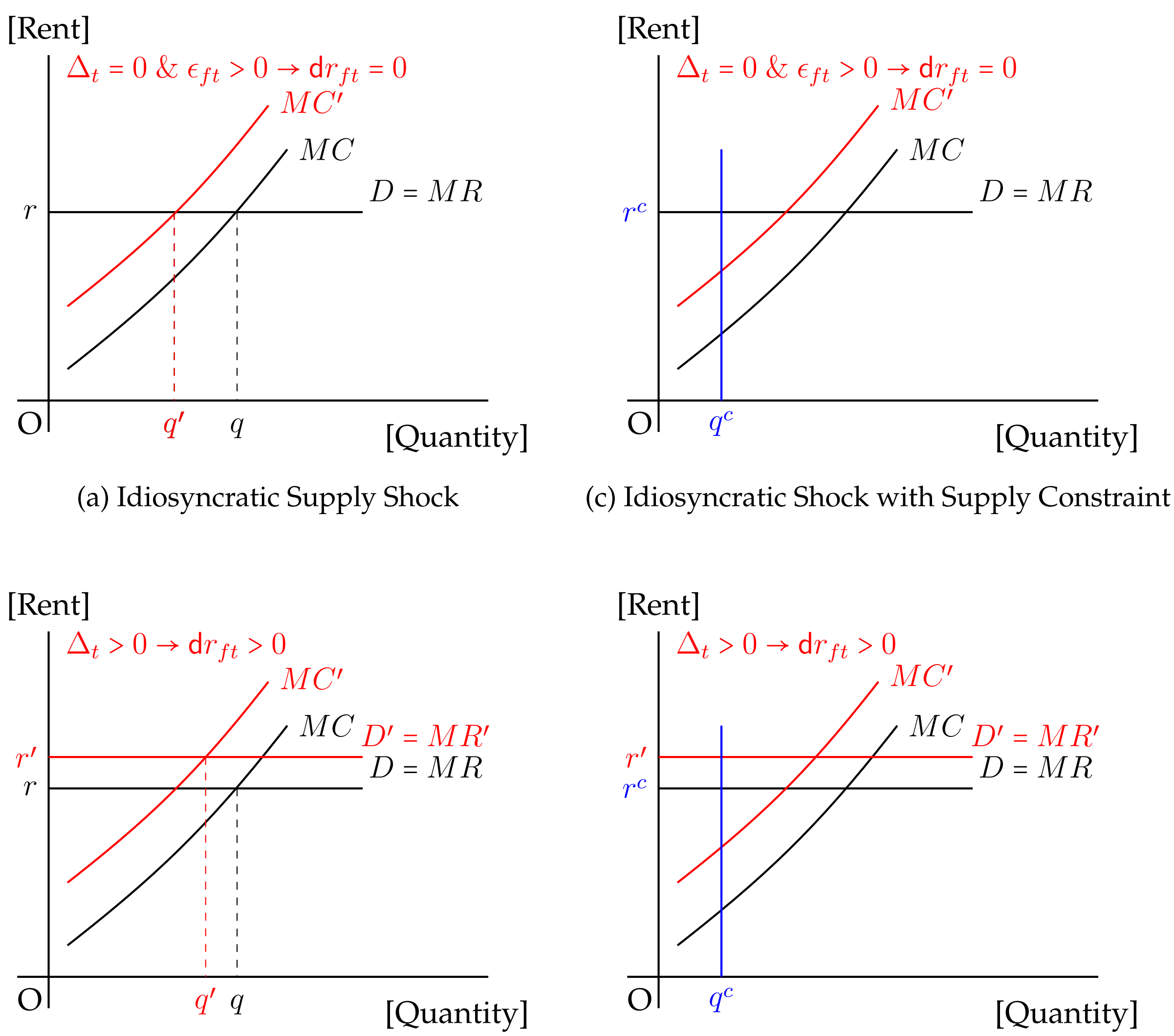
In Figure A.1a we illustrate an increase in marginal cost, where the common component is zero and the idiosyncratic component is positive: $\Delta_t = 0,\ \epsilon_{ft} > 0$. The original equilibrium is where the original increasing marginal leasing cost curve in black intersects the perfectly elastic residual demand determining price and quantity. The idiosyncratic component of the shock, ϵ_{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 price response.

Figure A.1b instead displays a situation where the firm's marginal cost is increasing and the market component is positive: $\mathsf{d}\ln(mc_{ft}) > 0,\ \Delta_t > 0$. 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 price, 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 cost curve reduces the wedge between cost and price, but as before, does not elicit a price response. All that's transpired is that the wedge between price 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 up the residual demand curve. As illustrated, in this case the capacity constrained building *also* experiences a price change. Note, this analysis does not assume costs are increasing. When marginal leasing costs are constant and below prices, buildings always lease up to capacity as in Figures A.1c and A.1d and those analyses apply.

Finally, as with capacity constraints, considerations of entry or exit do not affect qual-



(a) Idiosyncratic Supply Shock

(c) Idiosyncratic Shock with Supply Constraint

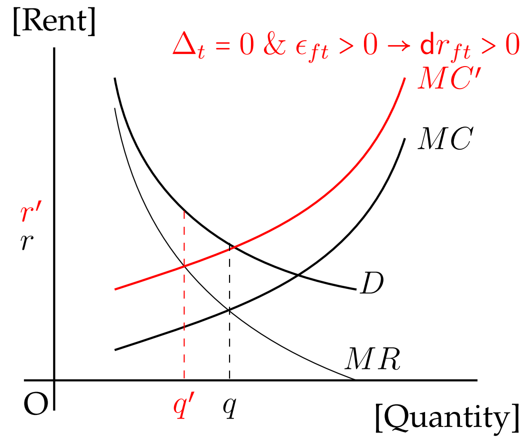
(b) Market-level Supply Response

(d) Market Shock with Supply Constraints

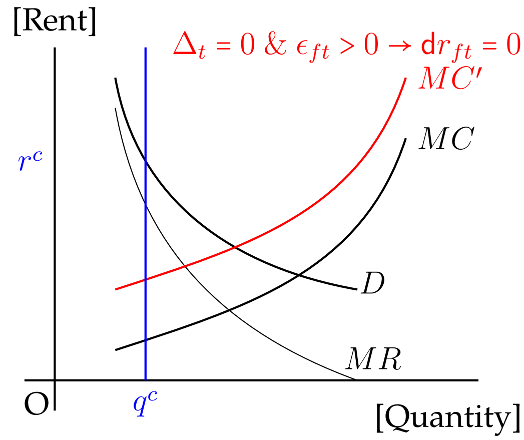
Figure A.1: Idiosyncratic and Market-level Shocks Under Perfectly Elastic Demand

itatively alter price 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.

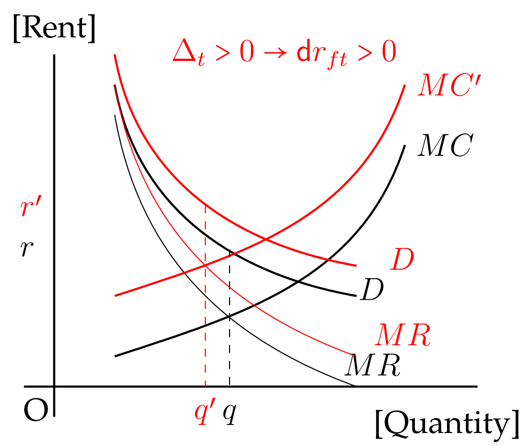
Pass through of cost shocks with downward sloping demand We now turn to the effect of the same cost shocks under deviations from perfect competition. Figures A.2a through A.2d repeat the above exercise but assuming downward sloping demand. In A.2a the result immediately departs from that of the figure's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



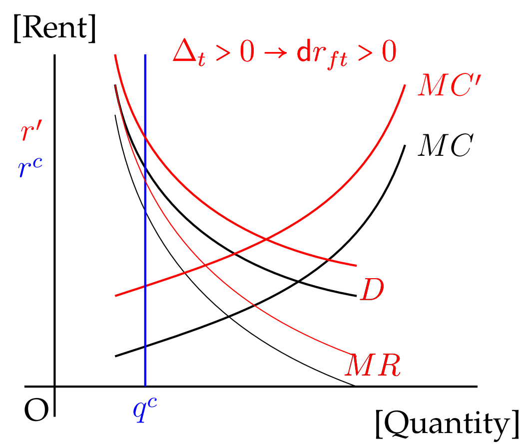
(a) Idiosyncratic Shock



(c) Idiosyncratic Shock With Constraints



(b) Market-Level Shock



(d) Market-level Shock With Constraints

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 price response. Because constrained buildings price 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 price shocks can be passed on to renters through price 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 price when the demand curve slopes down. Informed by this conclusion, our empirical specifications attempt to isolate variation in ϵ_{ft} and test for the the elasticity of rents with respect to ϵ_{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 ($\epsilon^C = 0$):

$$\begin{aligned} \mathsf{d}\ln(\mathsf{mc}^T_{ft}) - \mathsf{d}\ln(\mathsf{mc}^C_{jt}) &= \left(\beta \cdot \left[\Delta_t + \epsilon^T_{ft}\right] + \lambda_t + u^T_{ft}\right) - \left(\beta \cdot \left[\Delta_t + \epsilon^C_{jt}\right] + \lambda_t + u^C_{jt}\right) && (20) \\ &= \beta\epsilon^T_{ft} + \left(u^T_{ft} - u^C_{jt}\right). && (21) \end{aligned}$$

As long as the common marginal cost shifter (Δ) and the time FEs (λ) are the equal and there is no differential selection in unobserved demand shifters (u), then the DID estimate yields $\beta\,\mathsf{E}[\epsilon^T]$. 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 ϵ but uncorrelated with u. The regression specification is:

$$\begin{aligned} \text{FS:} \quad & \mathsf{d}\ln(\mathsf{mc}_{ft}) = \pi Z_{ft} + \lambda^{\text{FS}}_t + v_{ft} && (22) \\ \text{SS:} \quad & \mathsf{d}\ln(r_{ft}) \quad = \beta \mathsf{d}\ln(\mathsf{mc}_{ft}) + \lambda^{\text{SS}}_t + u_{ft} && (23) \\ & \qquad\qquad\quad = \beta\epsilon_{ft} + \tilde{\lambda}^{\text{SS}}_t + u_{ft}, && (24) \end{aligned}$$

with assumptions $\mathsf{Cov}(\epsilon_{ft}, Z_{ft} \mid \lambda_t) > 0$ and $\mathsf{Cov}(u_{ft}, Z_{ft} \mid \lambda_t) = 0$. 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.3.1 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 $\sum_j \left(mc_j^{-1}(x_j - \bar{u})\right) = N$. These equations form a system of $J+1$ unknowns ($\{\vec{r}, \bar{u}\}$), $J+1$ parameters ($\{\vec{x}, N\}$), and $J+1$ equations:

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

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

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.

A.3.2 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, prices in denser areas are higher and the quantity of housing lower than if the supply were competitive (price 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 js (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 $x_{j^{\mathsf{b}}}$.[24] 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, ϵ_i, drawn from some distribution $G(\epsilon)$ where $\epsilon \geq 0$ and $\mathsf{E}[\epsilon] \in \mathcal{R}_+$:

$$v_i(j) = (1 - x_j)\epsilon_i - r_j. \tag{25}$$

Note that this is isomorphic to heterogeneous transportation costs.[25]

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 prices at other locations:

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

Note that for any given option j', the difference between i's valuations of j and j' is

[24] 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 $x_{j^{\mathsf{b}}} \leq x_J$.

[25] In addition, we assume that all units are the constrained to be equal sized.

increasing in ϵ_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 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:

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

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

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

Total demand for j given a price r_j is therefore

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

or

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

It's clear from the above that quantity demanded is declining in price on two margins: as r_j increases, those at the top margin near $\bar{\epsilon}_j$ 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:

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

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 $C^{\mathsf{d}}(0) > 0$.

At each location, taking prices 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 prices 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 price 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, $j^{\mathbf{b}}$ at distance $x_{j^{\mathbf{b}}}$, must be found after which no development occurs. Note that quantity demanded at $j^{\mathbf{b}}$ is given by:

$$q^D_{j^{\mathbf{b}}}(r_{j^{\mathbf{b}}}) = G\left(\frac{r_{j^{\mathbf{b}}-1} - r_{j^{\mathbf{b}}}}{x_{j^{\mathbf{b}}} - x_{j^{\mathbf{b}}-1}}\right). \tag{32}$$

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, $j^{\mathbf{b}}$, 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, $(j^{\mathbf{b}}+1)$, profit upon entry is strictly negative:

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

Despite (near) zero profits, the landlord at $j^{\mathbf{b}}$ still sets rent with a markup over marginal cost. Rather, the fixed cost $C^{\mathsf{d}}(0)$ 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:

$$\mathsf{N} = \sum_{0}^{j^{\mathbf{b}}} q^*_j(r^*_j), \tag{34}$$

where q^*_j and r^*_j are equilibrium, profit-maximizing quantities and prices at plot j.

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

$$q^D_0(r_0) = 1 - G\left(\frac{r_0 - r_1}{x_1}\right). \tag{35}$$

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 $j^{\mathbf{b}}$ and the general equilibrium effects increasing demand at $j^{\mathbf{b}}$ imply that the peripherally developed plot is *futher* 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.[26]

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.

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

[26] Unfortunately, the public data does not include a panel identifier.

Table A1: Summary Statistics:
2002 - 2017 NYCHVS

	Full (1)	$\{4,5\}$ (2)	$\{6,7,8,9\}$ (3)
Average Rent	\$ 1407.78	\$ 1415.48	\$ 1400.26
Median Rent	\$ 1235.18	\$ 1229.23	\$ 1242.97
Pct $\{4,5\}$	49%	0%	100%
Pct w/ Elevator	3%	5%	2%
Pct Sound Condition	92%	92%	92%
Pct Built Pre 1947	86%	85%	86%
Pct Built 1947-1989	9%	9%	9%
Pct Built Post 1990	5%	6%	5%
Pct Less 3 Stories	15%	3%	27%
Pct $3-10$ Stories	85%	96%	73%
Pct $11+$ Stories	0%	0%	0%
Pct Less 2 Year Lease	36%	38%	33%
Pct 2 Year Lease	28%	33%	23%
Pct Other Lease	36%	29%	44%
Pct $0-3$ Year Tenure	34%	33%	35%
Pct $3-9$ Year Tenure	37%	34%	39%
Pct $10+$ Year Lease	29%	33%	25%
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.

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 A2 displays summary statistics for the NYC building panel data used in Section 5.

Table A2: 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.*[27]

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.

C.1.1 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.[28]

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

[27] "What is Zoning?" (`https://www1.nyc.gov/site/planning/zoning/about-zoning.page`).

[28] See the NYC Zoning Glossary for more terms (`https://www1.nyc.gov/site/planning/zoning/glossary.page`).

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.[29]

C.1.2 Zoning Facts in NYC

We consider a building to zoning constrained if the building, given its current building area and zoning policy, cannot add an additional minimum sized unit to the building. If any of the following conditions are met, then we consider a building to be zoning constrained:

1. Average Unit Area is greater than the maximum possible residential area of the building divided by current units plus one: $(B_{area}/U) > (\mathsf{Max}(\mathrm{Res}_{area})/(U+1))$,

2. The denisty factor is greater than the maximum possible residential area of the building divided by current units plus one: $d > (\mathsf{Max}(\mathrm{Res}_{area})/(U+1))$, or

3. Building Area plus 300 sqft is greater than the maximum possible residential area of the building: $B_{area} + 300 > \mathsf{Max}(\mathrm{Res}_{area})$.

In Table A3 we show 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. Pooling all years together, we map the locations of zoning constraints in Figure A.3.

[29]Values are rounded up only if the fractional remainder is greater than $(3/4)$.

Table A3: Zoning Constraints Across Boroughs

	BX	BK	MN	QN	NYC
2007	0.64	0.63	0.54	0.78	0.64
2008	0.64	0.63	0.53	0.79	0.64
2009	0.63	0.63	0.54	0.78	0.63
2010	0.63	0.63	0.53	0.78	0.63
2011	0.63	0.63	0.53	0.78	0.63
2012	0.63	0.62	0.51	0.78	0.62
2013	0.63	0.62	0.51	0.79	0.62
2014	0.64	0.64	0.52	0.80	0.64
2015	0.64	0.63	0.52	0.80	0.64
2016	0.64	0.64	0.52	0.80	0.64
2017	0.64	0.64	0.52	0.80	0.64
2018	0.64	0.65	0.52	0.81	0.65
2019	0.64	0.65	0.52	0.81	0.65
Total	0.64	0.63	0.53	0.79	0.64

Note: 2007-2019 NYC residential buildings with 4+ units. Data from DOF and PLUTO files. Unweighted average across buildings.

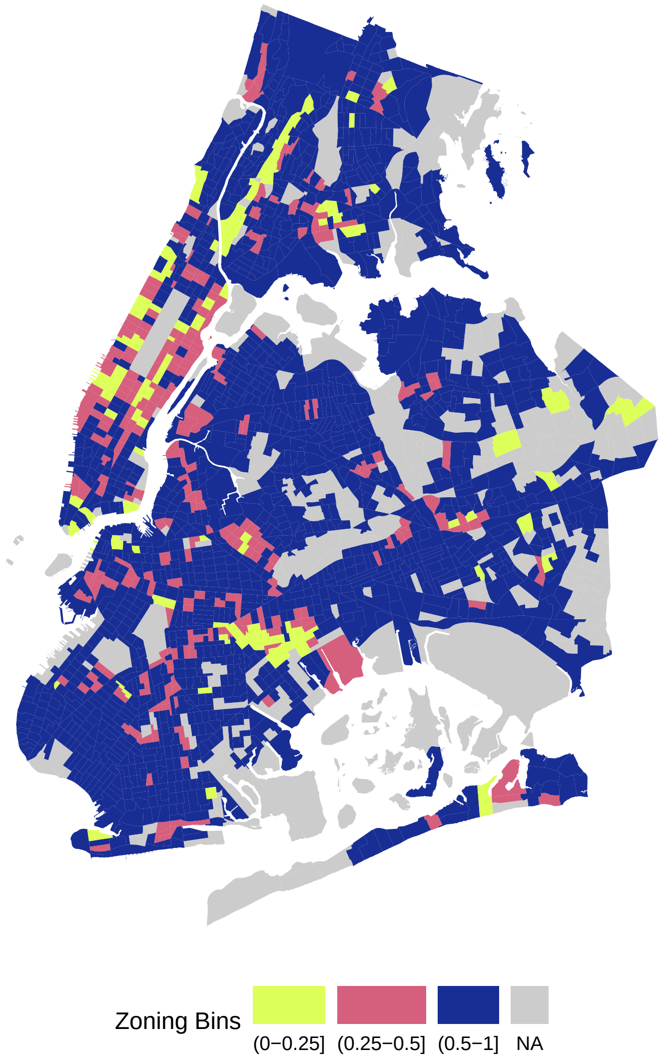
C.2 Price Controls

A full history of NYC's rent regulation is available from the city as "History of the [NYC Rent Guidelines] Board and Rent Regulation."[30] 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

[30] Accessible online at
`rentguidelinesboard.cityofnewyork.us/wp-content/uploads/2020/01/historyoftheboard.pdf`

Figure A.3: Distribution of Zoning Constraints



Note: The figure maps the percent of buildings that are zoning constrained, pooling across years. As described in text, we define a building as zoning constrained if under the current building area and zoning policy the building cannot add an additional unit.

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.[31]

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.

[31]These benefits are for new building construction, conversions, and/or renovations.

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 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.

C.2.1 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. In Table A4 we list the portion of buildings that have any rent stabilized units and in Table A5 we list the portion of buildings that have over 50% of units rent stabilized. Manhattan has a lower proportion of rent stabilized units relative to the other boroughs. In Figure A.5 we show the distribution of substantially rent-stabilized buildings around the city.

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.

C.2.2 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

Table A4: Portion of Buildings with Any Stabilized Units

	BX	BK	MN	QN	NYC
2007	0.58	0.41	0.67	0.47	0.51
2008	0.58	0.40	0.67	0.46	0.50
2009	0.55	0.36	0.63	0.43	0.47
2010	0.55	0.36	0.63	0.43	0.46
2011	0.57	0.37	0.64	0.44	0.47
2012	0.58	0.38	0.65	0.45	0.48
2013	0.58	0.38	0.65	0.45	0.48
2014	0.57	0.37	0.63	0.44	0.47
2015	0.56	0.36	0.62	0.43	0.46
2016	0.55	0.36	0.62	0.43	0.46
2017	0.55	0.35	0.61	0.42	0.45
2018	0.54	0.34	0.61	0.42	0.45
2019	0.53	0.34	0.61	0.42	0.44
Total	0.56	0.37	0.63	0.44	0.47

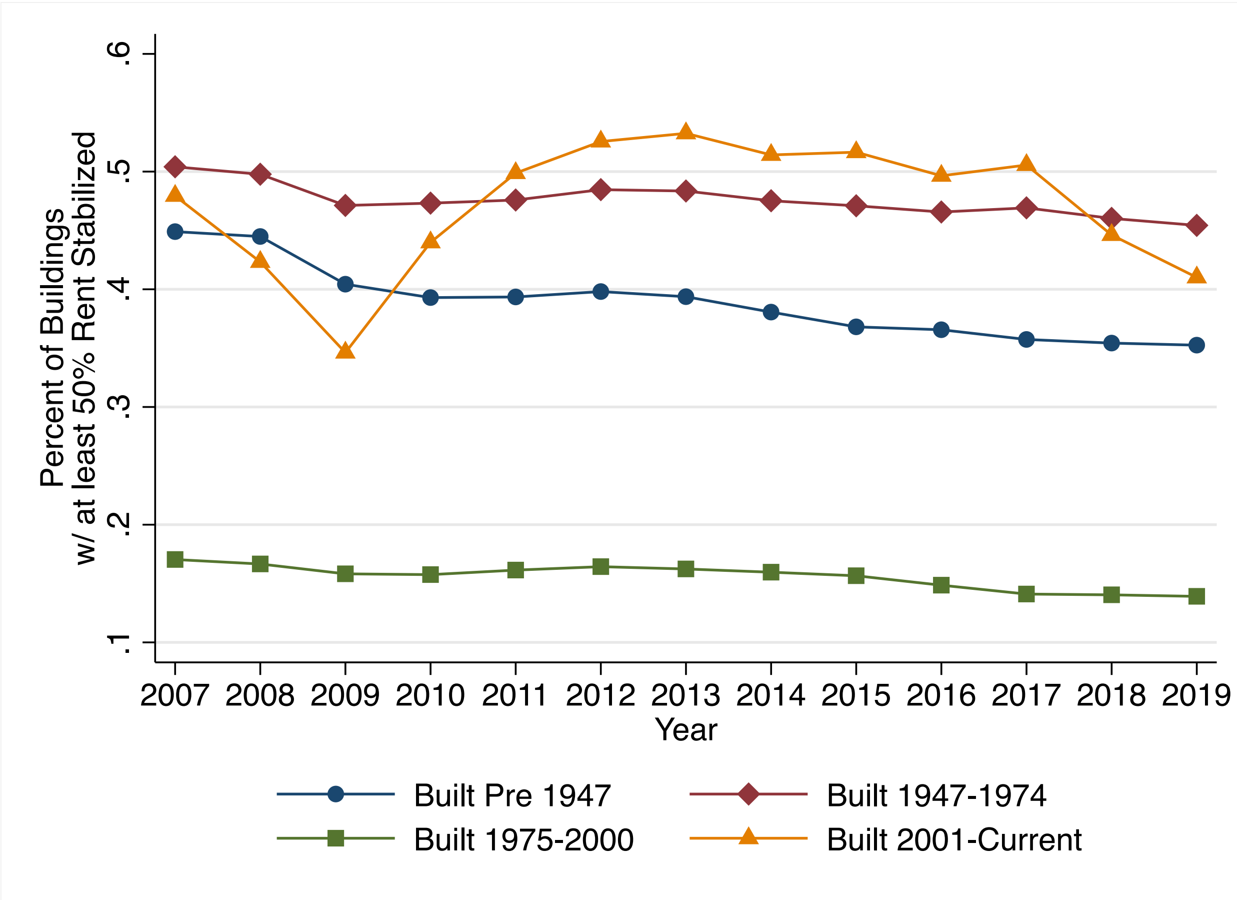
Note: 2007-2019 NYC residential buildings with 4+ units. Data from DOF and PLUTO files. Unweighted average across buildings.

Table A5: Portion of Buildings with Over 50% of Units Stabilized

	BX	BK	MN	QN	NYC
2007	0.57	0.38	0.47	0.45	0.44
2008	0.57	0.38	0.46	0.45	0.44
2009	0.54	0.34	0.41	0.42	0.40
2010	0.54	0.33	0.39	0.41	0.39
2011	0.56	0.34	0.38	0.41	0.39
2012	0.57	0.35	0.37	0.42	0.40
2013	0.57	0.35	0.36	0.42	0.39
2014	0.56	0.33	0.35	0.41	0.38
2015	0.55	0.32	0.33	0.40	0.37
2016	0.55	0.32	0.33	0.40	0.37
2017	0.54	0.31	0.32	0.39	0.36
2018	0.53	0.31	0.32	0.39	0.35
2019	0.53	0.30	0.32	0.39	0.35
Total	0.55	0.34	0.37	0.41	0.39

Note: 2007-2019 NYC residential buildings with 4+ units. Data from DOF and PLUTO files. Unweighted average across buildings.

Figure A.4: Distribution of Rent Stabilization

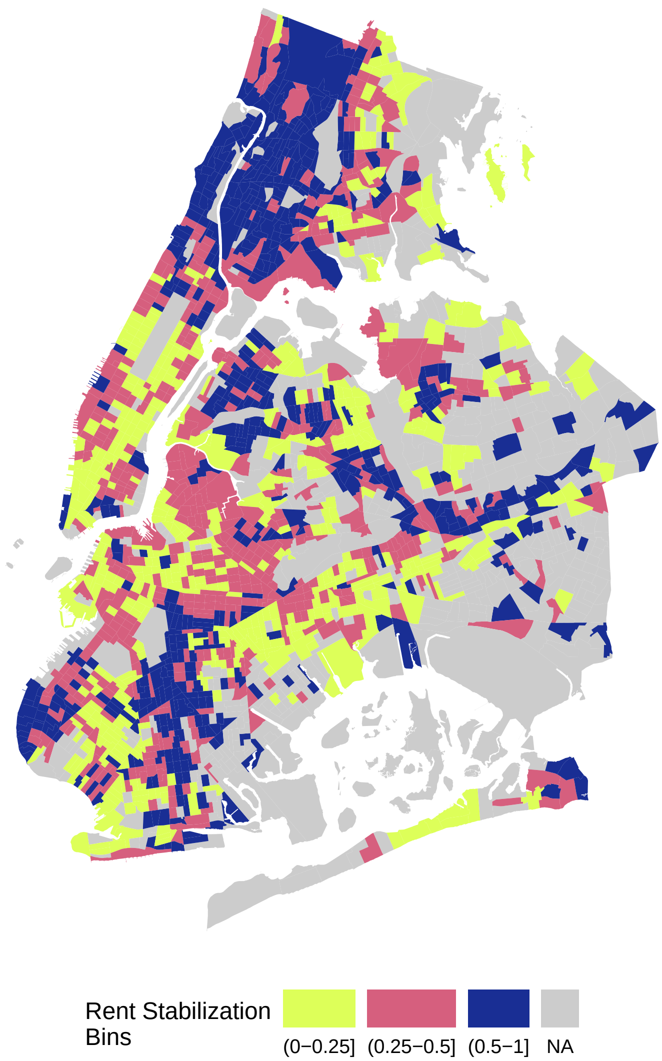


Note: The figure plots the percent of buildings that have more than 50% of units rent stabilized over 2010 to 2019 by building age groups. Buildings built after 1974 are not *de facto* rent stabilized. Buildings that take advantage of tax benefits for new construction, renovation, or conversion are rent stabilized for a fixed amount of time.

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 price 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 A8. Overall, we still find pass-through results inconsistent with perfect competition.

Figure A.5: Distribution of Rent Stabilization



Note: The figure maps the percent of buildings that have more than 50% of units rent stabilized, pooling across years, at the tract level.

D Additional Estimation Results

D.1 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.

D.1.1 Regression Results for Figure 1b

Here we present the regression table of 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 A6: Additional Pass Through Estimation Results
Event Study Regression Table

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

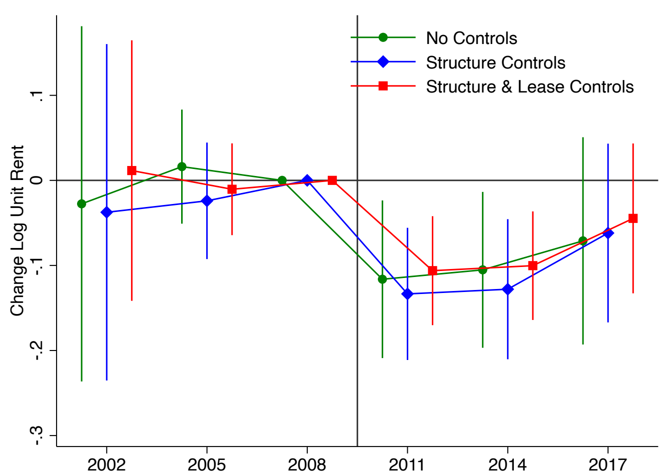
Note: The table displays the regression results for Figure 1b based on equation 14. Data is from the NYCHVS 2002 to 2017 samples. The regression controls for sub-borough-by-year fixed effects, building age fixed effects, condition of building indicators, number of floors groups, units in building, passenger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

D.1.2 Additional Event Study Results

Here we present additional the event study results, similar to Figure 1b. Specifically, in Figure A.6 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.7, 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.

Figure A.6: Additional Event Study Results: Different Controls

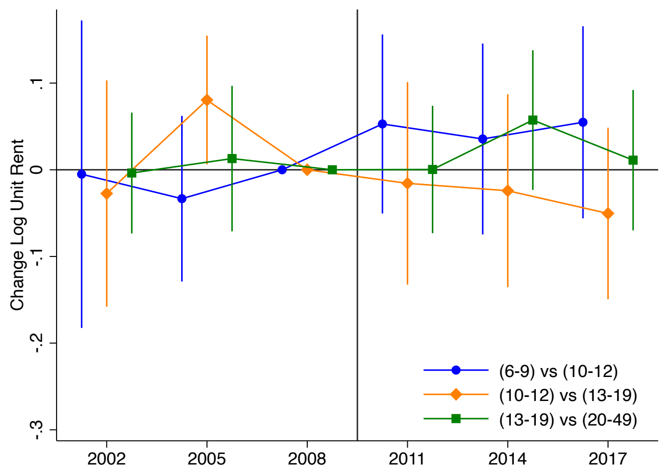
Figure A.7: Additional Event Study Results: Different Controls, Unweighted



D.1.3 Placebo Event Study Results

In Figure A.8, 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 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.

Figure A.8: Additional Event Study Results: Placebo Groups



D.2 Additional Synthetic Tax IV Results

Here we present additional empirical results from Section 5.

D.2.1 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 A7 and A8 report our results for both subsamples, which largely similar to our main specification with one exception: the latter sample has smaller-magnitude pass-though estimates.

Table A7: Additional Pass Through Estimation Results
Residential Only Subsample

Dependent Variable: Log Rental Income				
	Reduced Form		2SLS	
	(1)	(2)	(3)	(4)
Log Cf Tax	0.038	0.031		
	(0.004)	(0.004)		
Log Total Cost			1.161	1.217
			(0.077)	(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 A8: Additional Pass Through Estimation Results
$\leq 50\%$ Rent Stabilized Subsample

Dependent Variable: Log Rental Income				
	Reduced Form		2SLS	
	(1)	(2)	(3)	(4)
Log Cf Tax	0.045	0.024		
	(0.008)	(0.006)		
Log Total Cost			0.886	0.829
			(0.068)	(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.

D.3 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' prices were orthogonal to our instrument, here we adopt this hypothesis wholesale and use neighbors' prices 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.

D.3.1 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 A9 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.

D.3.2 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

Table A9: Additional Pass Through Estimation Results
Census block-year Fixed Effects

Dependent Variable: Log Rental Income				
	Reduced Form		2SLS	
	(1)	(2)	(3)	(4)
Log Cf Tax	0.039	0.028		
	(0.005)	(0.004)		
Log Total Cost			1.096	1.161
			(0.078)	(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.

extent to which a competitor's prices affect competitive environment at any location is also therefore a function of distance, and the market price at each location will be a weighted averages of competitors prices.[32] To flexibly capture this alternative market definition, for each building, we include the buildings' neighbor's price 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:

$$\ln[r_{jgt}] = \gamma_1 Z_{jgt} + \gamma_2 \mathsf{F}\left(\{\ln[r_{hg_h t}\}_{\mathsf{j}_t(20)}\right) + \gamma_3 X_{jgt} + \gamma_4 D_j + \gamma_5 D_{gt} + \nu_{jgt} \tag{36}$$

where $\mathsf{j}_t(20)$ is the set of the 20 nearest neighbors of building j (which we call the focal building) in year t and $\mathsf{F}(\cdot)$ 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 nth neighbors' rent where we interpolate missing data on neighbors' rent.[33] 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 A10 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).

D.3.3 Non-spatial Markets

Up until now, we have assumed that residential markets are spatial in nature. Here, we consider non-spatial markets. We divide our sample into markets according to their 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

[32] 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 price index, as in gravity trade models.

[33] 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.

Table A10: Additional Pass Through Estimation Results
Nearest Neighbors Regression

Dependent Variable: Log Monthly Rent				
	Rolling Average		Interpolated	
	(1)	(2)	(3)	(4)
Log Cf Tax	0.022	0.022	0.025	0.025
	(0.003)	(0.004)	(0.003)	(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.

of the building's zipcode. For each of these characteristics, we assign each building into its characteristic specific percentile group.[34]

In Tables A11 and A12, we report regressions to test whether such market segmentation biases or changes our results substantially. For Table A11, we first create characteristic specific percentile group leave-one-out averages of rent and then we regress that average on the our instrument with tract-year and building fixed effects:

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

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. We view this as a non-spatial balance test, similar to Figure 2.

As shown, we observe no systematic correlation between the instrument and the neighbor-average, and so we are confident that our instrument is picking up idiosyncratic variation and is valid even under these alternative market assumptions.

[34]Specifically, we use each building's 2007 values of characteristics to avoid changes in groupings that could be due to the tax changes.

Table A11: Non-spatial Markets:
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)
ζ_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.

Nevertheless, for Table A12, we create characteristic specific percentile groups, and then replicate 2 using characteristic-group-year FEs as a control rather than tract-year FEs:

$$\ln[r_{jg^C t}] = \gamma_1 Z_{jg^C t} + \gamma_2 X_{jg^C t} + \gamma_3 D_j + \gamma_4 D_{g^C t} + u_{jg^C t}, \tag{38}$$

where g^C indexes the characteristic specific percentile group. As shown, the estimated reduced form coefficient is roughly the same as the main text, and so we take this as evidence that our instrument is still informative even under alternative market assumptions.

Finally, 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 A12: Non-spatial Markets:
X-Group Pass Through Regressions

	Dependent Variable: Log Monthly Rent				
	Subway Dist	Building Age	Yrs Since Renovation	Avg Unit Size	Zipcode AGI
	(1)	(2)	(3)	(4)	(5)
γ_1	0.027	0.024	0.027	0.025	0.027
se	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
t	8.694	7.565	8.646	8.097	8.823
CI	(0.021,0.034)	(0.018,0.030)	(0.021,0.033)	(0.019,0.032)	(0.021,0.033)
Time-varying controls	Y	Y	Y	Y	Y
Building FEs	Y	Y	Y	Y	Y
X-Group-year FEs	Y	Y	Y	Y	Y
Observations	154,254	154,254	154,254	154,254	152,302

Note: The table displays robustness results for Table 2 using alternative market definitions. As denoted by the column title, each column uses a characteristic percentile group indicator interacted with year as a market level variation control. 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 robust to arbitrary heteroskedasticity but not clustered to compare across columns easily.