A Test for Pricing Power in Urban Housing Markets*

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Abstract

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

Keywords: housing supply, market power, housing demand

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 onto rents. We collect data on building rental income and expenses in New York City (NYC) and isolate a series of tax policy shifts that result in building-specific tax burden changes. We use these to construct two complementary quasi-experiments. We find that lessors exhibit cost pass-through behavior that is not possible when housing is provided competitively.

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

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

We first exploit an unannounced change in assessment procedures that lowered taxes on certain buildings by nearly half. Initially, $\{4, 5\}$ unit buildings were assessed differently from slightly larger buildings, resulting in a large differential tax burden. In 2011, without prior public notification and "on the advice of counsel," the DOF harmonized assessment procedures, cutting taxes on $\{4, 5\}$ unit buildings by 45%. Comparing unit rents before and after this shift with those in the NYCHVS's next largest class, $\{6, 7, 8, 9\}$ unit buildings, we find that rents fell 12% for the treated group relative to the controls' rents. As Section 3 shows, this difference-in-difference estimate isolates the response of rents to the idiosyncratic component of the policy's shock so long as both groups share a market. Interrogating this assumption, we look for pre-trends and imbalances in unit and renter characteristics, finding neither.

Although these results deliver confirmation of pass-through in a transparent manner, the potential presence of general equilibrium effects and lack of cost data complicate the interpretation of a first stage—and thus pass-through rate. We complement the difference-in-difference's large, discrete shift with a second approach following an established literature testing for imperfect competition by isolating smaller, firm-level shocks (Amiti, Itskhoki, and Konings, 2019; Paciello, Pozzi, and Trachter, 2019; Muehlegger and Sweeney, 2022; Garin and Silvério, 2023). Beyond establishing internal validity by using different buildings, variation, and identifying assumptions, leasing cost information for this sample allows us to construct a true first stage and pass-through rate in this setting. Further, this approach allows us to explore underlying heterogeneity, leveraging NYC's substantial cross-neighborhood diversity to probe how market structure mediates our findings.

Our second quasi-experiment leverages annual changes to large buildings' taxes. After 2010, these buildings' incomes were converted into valuations using capitalization rates derived from an annually-changing, citywide formula. To identify building-idiosyncratic cost shocks, we use formula changes in conjunction with buildings' earliest reported (2007) income to construct a synthetic tax IV. Comparing buildings within the same Census tract, we find that a 10% increase in the idiosyncratic component of a building's predicted taxes is associated with a 0.3% increase in rents. Instrumenting for total costs, our reduced-form results correspond to a total pass-through *rate* of over 100%. The positive pass-through rates from both approaches indicate that lessors pass idiosyncratic cost shocks onto renters, such that lessors' pricing behavior is far from negligible.

To test that our instrument plausibly identifies idiosyncratic shocks, we conduct several "placebo tests," (following Garin and Silvério, 2023). In our context, the exclusion restriction requires that our instrument is uncorrelated with rents through the error term. In the language of residual demand, idiosyncratic cost shocks should be uncorrelated with residual demand shifters, such as the rents (or determinants thereof) of close competitors. We regress each building's instrument on the building's nth-nearest neighbor's rent, which are the competitors most likely to embody correlated demand or sorting on other unobservables. We find no evidence of correlation between the instrument and these placebo rents. We show this result holds when competitor proximity is measured using the other parcel observable characteristics available to us.

Finally, we segment our sample by proximity of competitor buildings, ownership concentration, density, zoning restrictions, and vacancy rates. First, we find our measured pass-through rate is remarkably stable across market environments. Second, the competitive environment does have the anticipated results on pass-through, with market characteristics consistent with stiffer competition generally correlated to lower pass-through. Third, we find pass-through rates are substantially higher in lower-income and higher minority tracts, suggesting market power may have implications for housing inequality.

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.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).

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

Three literatures implicitly assume downward-sloping demand for housing. Arnott (1989); Arnott and Igarashi (2000); Basu and Emerson (2003) examine monopoly power as a theoretical justification for rent control.[1] An empirical literature examines the effects of housing ownership concentration (Raymond et al., 2016; Cosman and Quintero, 2021; Austin, 2022; Xiao, 2022; Gurun et al., 2023). Finally, in a large empirical literature on bargaining—"double monopoly"—markets are *sometimes* modeled with price posting in combination with sorting-and-matching, which relies on an implicit, stochastic downward-sloping residual demand (e.g., Harding, Rosenthal, and Sirmans, 2003; Ihlanfeldt and Mayock, 2009; Genesove and Han, 2012; Piazzesi, Schneider, and Stroebel, 2015; Glaeser and Nathanson, 2017; Gilbukh and Goldsmith-Pinkham, 2019; Bracke, 2021). Our finding that demand for individual buildings is downward-sloping is an untested but necessary precondition for the implicit theoretical pathways of interest in these literatures.

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

2 Institutional Setting and Data

A unique aspect of NYC's property tax regime is that rental buildings are effectively taxed based on rental income. All properties are assessed based on market values; however,

[1] Diamond, McQuade, and Qian (2019) consider rent controls' effects on exit, discussed in A.1. Rolheiser (2019); Tsoodle and Turner (2008) consider market-level tax pass-through to rents. Bakker and Datta (2024) examine market power in housing markets with middle men.

rental buildings' values are generated using their own reported income and building-specific multipliers.[2] For small buildings, the connection between income and (future) taxes is established using "gross income multipliers" (GIMs). For large buildings, income is divided by building-specific "capitalization rates" (cap rates). Our analysis leverages changes in the cap rate and GIM formulas to isolate marginal cost shifts. Appendix B discusses tax classification, timing, and functional relation to income in detail.

We construct a building-level dataset spanning 2007 to 2019 for private, multi (4+) unit buildings in NYC.[3] We merge two datasets of administrative building-level records: the Primary Land Use Tax Lot Output and the Final Assessment Roll, for data on buildings' location, zoning, assessed market values, age, and years since renovation. Because tax changes are phased in over 5 year periods, we use assessed market values to calculate per unit taxes, as they reflect the taxes assessed on income fully accounting for this phase-in. We combine this with data collected from an online public portal: to explain their tax calculations, the DOF sends annual letters with each building's revenue and expense information reported to DOF through Real Property Income and Expense forms, as well as building-specific capitalization rates or income multipliers (used to calculate market values) back to lessors.

We supplement this data with the triennial NYCHVS, which provides unit-level rents and characteristics (number of building units and floors, location, reported condition, the presence of an elevator, and the resident's tenure and length of lease), over 2002-2017.[4]

Appendix Tables C.1 and C.3 present summary statistics for our samples. Appendices

[2]While the effective tax rate on rental income is not set *ex-ante*, it is highly stable across years. We assume landlords expect persistence in these rates such that landlords use current tax formulas to inform how present-period income will be taxed.

[3]We omit Staten Island due to the low number of rental buildings.

[4]The NYCHVS samples units from the previous census and surveys each unit every three years. Our main specifications use the provided sample weights. Appendix E shows results are unaffected by their exclusion. We use the smallest available NYCHVS geography, sub-borough area (SBA), which is co-terminus Census PUMAs.

C and D discuss details of sample construction and the NYC regulatory environment.[5]

Our environment is characterized by low entry and exit rates, due to a low supply of "green" sites and long permitting and construction lags. This reduces the possibility that our difference-in-differences estimates are contaminated by supply responses within the brief window of our quasi-experiment. Appendix E examines entry and exit surrounding the small-building reform.

3 Conceptual Framework

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

We consider policy changes that affect the marginal cost of leasing building units. For each building j in market m in period t, the log change in the building's marginal cost can

[5]Rent stabilization would attenuate our results, as it is not systematically binding. We include robustness checks that exclude highly stabilized buildings.

be decomposed into a common component and an idiosyncratic component:

$$\mathsf{d}\ln(mc_{jmt}) \coloneqq \underbrace{\Delta_{mt}}_{\text{Common}} + \underbrace{\epsilon_{jmt}}_{\text{Idiosyncratic}}, \tag{1}$$

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

In both market settings, rental prices respond in qualitatively similar ways to shifts in Δ_{mt}, which aggregate across firms and shift residual demand. However, rents only respond to idiosyncratic shifts ϵ_{jmt} under imperfect competition. Our empirical strategy aims to isolate variation corresponding to idiosyncratic marginal cost changes, ϵ_{jmt}, and test the hypothesis that the elasticity of rents with respect to those changes, $\mathsf{d}\ln(r_{jmt})/\epsilon_{jmt}$, is zero.[6]

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

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

where λ_j 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

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

deviations, we obtain:

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

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

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

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

where we substitute equation 1 into 4. Thus, the market-time fixed effects absorb common shocks, and so 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_{jmt}. Intuitively, by "holding fixed" the market-level, the market-time fixed effects account for the common component of marginal cost shifts (and other forces) resulting in residual demand curve shifts.

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

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

$$\mathsf{d}\ln(r^T_{jmt}) - \mathsf{d}\ln(r^C_{jmt}) = \left(\beta \cdot \mathsf{d}\ln(mc^T_{jmt}) + \lambda_{mt} + u^T_{jmt}\right) - \left(\beta \cdot \mathsf{d}\ln(mc^C_{jmt}) + \lambda_{mt} + u^C_{jmt}\right) \quad (7)$$

$$= \left(\beta \cdot \left[\Delta_{mt} + \epsilon^T_{jmt}\right] + \lambda_{mt} + u^T_{jmt}\right) - \left(\beta \cdot \left[\Delta_{mt} + \epsilon^C_{jmt}\right] + \lambda_{mt} + u^C_{jmt}\right) \quad (8)$$

$$= \beta \cdot \epsilon^T_{jmt} + \left(u^T_{jmt} - u^C_{jmt}\right). \quad (9)$$

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

We stress that this is true even when the treatment is large and generates a market-wide shift. The difference-in-differences estimate still identifies the effect of the shock's idiosyncratic component by subtracting the market effect via the control buildings. Put differently, our setting requires an assumption weaker than SUTVA: if the two groups share the same market effect (violating SUTVA), then the difference-in-differences estimate identifies the idiosyncratic effect. However, as discussed in Section 4, SUTVA violations would complicate interpretation of the estimate's magnitude.

For the latter strategy, we employ an IV approach to estimate equation (2).[7] For this, we need a variable, Z, that is, conditional on building and market-time FEs, correlated with ϵ but uncorrelated with unobserved shifters:

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

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

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

[7]For expositional reasons, we describe a simple first difference; however, in our repeated cross-section setting with > 2 time periods, the building FE yields deviations from the within-building time-average of the variables.

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

4 Evidence from a Tax Regime Shift

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

4.1 Policy and Specification

Prior to 2011, DOF used different methodologies to calculate market values of $\{4, 5\}$ and $6 - 10$ unit buildings, using sale prices of comparable non-income producing buildings in the GIM formula of (only) the former, which resulted in systematically higher GIMs for that group. 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).[8]

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

Because building income data is unavailable for $\{4, 5\}$ unit buildings prior to the policy shift in our scraped data, we rely on the NYCHVS for the rent analysis, but use DOF data to observe the effect of the policy on tax. We use building controls as available, including

[8]See Appendix B for a detailed discussion of the assessment procedures and this change.

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.[9]

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

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

$$\text{ES} \quad Y_{jmt} = \theta_1 1[\{4,5\}]_j + \sum_{s=\underline{t}}^{\bar{t}} \theta_s D^s_{jmt} + \theta_3 X_{jmt} + \theta_4 D_{mt} + \varepsilon_{jmt}, \tag{14}$$

for outcomes (1) log assessed per-unit taxes and (2) log unit rent, where D^s_{jmt} are indicators for each year interacted with treatment (omitting 2008), X_{jmt} are building observables, D_{mt} sub-borough area (SBA)-year fixed effects, and θ_2 and θ_s are empirical counterparts to $\beta\,\mathsf{E}[\epsilon_{jmt}]$ from equation 9. 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.

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 by SBA in Panel B. These clustering levels provide conservative standard errors. From left to right, the specifications add controls but yield similar results. We focus our discussion on our preferred specifications in columns (3) and (6).

Matching the internal Audit estimate of the effect of the 2011 tax year changes on

[9]For locations, we use tracts in the DOF data and SBAs in the NYCHVS data, where tract information is unavailable. Because we cannot link the units over time, covariates may vary. Though they appear not to impact results, building covariates may be 'bad controls' if endogenous to treatment.

Table 1: Difference in Difference Results

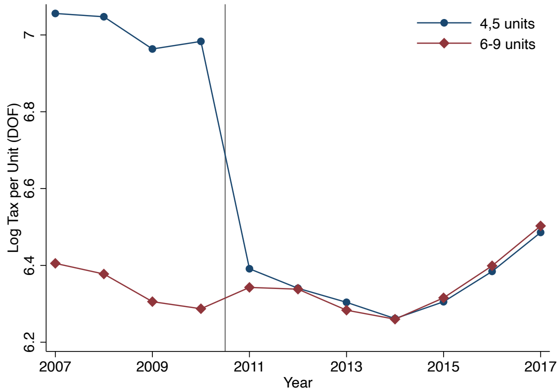
Panel A: Log Assessed Property Tax per Unit	(1)	(2)	(3)
$1[t > 2010] \cdot 1[\{4,5\}]$	-0.66	-0.60	-0.60
	(0.01)	(0.01)	(0.01)
Building Controls	N	N	Y
Tract-year FEs	N	Y	Y
Unique Buildings	54,569	54,445	53,462
Observations	655,853	654,143	653,159
Panel B: Log Unit Rent	(1)	(2)	(3)
$1[t > 2010] \cdot 1[\{4,5\}]$	-0.13	-0.14	-0.12
	(0.04)	(0.03)	(0.02)
Building Controls	N	N	Y
SBA-year FEs	N	Y	Y
Observations	7,895	7,895	7,895

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 calculated using annual 2007-2017 data on market values 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 (condition of building indicators, binned number units in building, binned number of floors, elevator indicator, and lease length and tenure). Standard errors in Panel A are clustered by tract while for Panel B they are clustered by sub-borough-area.

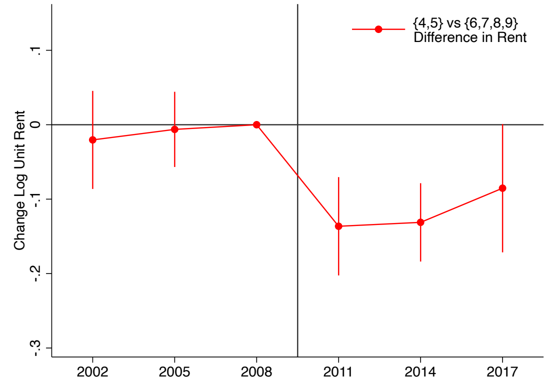
this group's tax burden (New York City Department of Finance, 2012), columns (1-3) report that assessed per-unit property taxes for the small buildings fell by about -0.60 log points.Panel B reports the effects of the policy shift on rents. Columns (1-3) report that the reform reduced unit rents by between 12-14%. Under the assumption that $4-9$ unit buildings face the same aggregate trends, this result is inconsistent with perfect competition.

Figure 1 probes whether the effects are driven by pre-trends. Panel (a) plots log per-unit tax for the two size groups over time. In the pre-period, the two lines move in tandem but with a large spread. Panel (b) presents the dynamic treatment effect regression coefficient estimates corresponding to our preferred specification. We estimate pre-treatment zero

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 on log unit rents using the controls in the specification in column (6) of Table 1 by triennial NYCHVS wave, with treatment effects interacted by year, relative to the 2008 wave as the base (omitted) year. Standard errors are clustered by sub-borough-area.

effects and a clear and relatively persistent decrease in rent relative to the control group and pre-period. Appendix E details several robustness checks, including rerunning our event study with different sets of controls, removing sample weights, dropping all units with rent regulations, and using placebo treatment groups. While we cannot directly test the assumption that the two groups do not constitute distinct markets, Appendix C shows that the pre-period samples are balanced in terms of tenant demographics and building observables (except, mechanically, height).[10]

To generate a pass-through rate from these results, one should proceed with caution. Several empirical and theoretical limitations make such a calculation fraught. Recall, the difference-in-differences is designed to purge the market-wide general equilibrium effects of the policy change. As a result, the effect in panel (a) conflates the idiosyncratic component of the policy with the component generating general equilibrium effects. Further-

[10]An additional important potential threat to identification is differential entry, which could affect the aggregate supply of the treatment and control groups. Appendix E examines this possibility, and shows that in NYC new buildings are a very small fraction of the market.

more, the NYCHVS data lacks the cost information (which we have in the next empirical design) to calculate a true DIDIV estimator, so at best we have a 'split-sample DIDIV' estimate. That said, a dollar comparison of the tax burden shift to the effect on rents implies (see Appendix Table E.3) that for every one dollar in tax reduction, rents were reduced by between 39-47 cents.

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 cap 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.

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

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

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.[11,12]

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

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

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

Market Size and Fixed Effects Implementing equation (6) requires defining a market. If we specify a market size *above* the true market's level of aggregation, we will contaminate our estimate of β with market-level fluctuations. By contrast, specifying a market size

[11]For 2011-2013, these reports are not posted on the DOF website. We back-out these parameters using non-linear least squares using cap rates in our data. Our matching with observed rates yields an R-squared of at least 0.8 each year.

[12]A subtle point is that because income is used twice in taxation, as net income, divided by the cap rate to obtain market values, and as gross to obtain the cap rate, formula changes represent both a shift in marginal cost and a change in slope. By holding fixed building income, our predicted tax omits variation stemming from such slope changes.

below the true size will still fully and correctly absorb all market-time variation. As a baseline, we define markets as tracts, believing they are smaller than the relevant markets, and therefore a conservative definition. Throughout and especially in Appendix F, we buttress our results with a bevy of alternative market definitions and placebo tests. We also examine market definitions that are continuous and overlapping.

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

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

with building fixed-effects D_j and tract-year fixed effects D_{mt}, and changes in observable building characteristics X_{jmt} (age, years since renovation, and an elevator indicator which varies for only a few buildings).

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

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

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

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

5.2 Results

Panel A of Table 2 displays our results. Columns (1,2) present reduced form results, where column (2) adds time-varying controls. We find that a 10% increase in predicted

Table 2: Pass-through of Cost Shocks on Rent

Panel A: Main Specifications				
	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)
First Stage			0.030	0.022
			(0.004)	(0.003)
$H_0 : \beta < 1$ P-val			0.005	0.006
Robust F Stat			69.93	44.23
Robust AR Stat			118.85	86.01
Time-varying controls	N	Y	N	Y
Tract-year FEs	Y	Y	Y	Y
Building FEs	Y	Y	Y	Y
Observations	152,559	152,559	152,559	152,559

Panel B: Alternative Market Specifications Reduced Form				
	Subway Dist	Building Age	Yrs Since Renovation	Avg Unit Size
	(1)	(2)	(3)	(4)
Log Cf Tax	0.027	0.024	0.027	0.025
	(0.003)	(0.003)	(0.003)	(0.003)
Time-varying controls	Y	Y	Y	Y
Building FEs	Y	Y	Y	Y
X-Group-year FEs	Y	Y	Y	Y
Observations	154,254	154,254	154,254	154,254

Note: The table reports multiple regressions using log average unit rent as the dependent variable. In Panel A, columns (1) and (2) report reduced form regressions using our log counterfactual tax instrument directly; columns (3) and (4) report two stage least squares regressions where log total cost is instrumented. 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 (1,3) omit time-varying controls; columns (2,4) include log building age, log years since a renovation, and an elevator indicator. In Panel B, columns (1)-(4) replicate Panel A column (2), adding percentile-year fixed effects for percentiles of distance to subway, building age, years since renovation, and unit size, respectively.

tax payment leads to a roughly 0.3% increase in rent. The positive relationship rejects the null but is qualitative. To interpret these results as a pass-through we must account for the relationship between our instrument and total costs (taxes plus building expenses).

Columns (3,4) are two-stage least-squares (2SLS) results where we instrument for total per-unit leasing costs in order to estimate a pass-through rate. We find a pass-through rate of about 120-130% into rents. This is consistent with variable markups in the presence of several market structures including sufficiently convex demand (so that, as landlords move up the demand curve, they face less elastic consumers and raise mark-ups further) or downward-sloping marginal costs (Weyl and Fabinger, 2013; Pless and van Benthem, 2019).[13]

The first stage is reported in each column below the 2SLS coefficients. In both columns, the first stage magnitude is similar to that of the reduced form. It is less than one because of the disconnect between our predicted tax change and actual changes in total cost, which reflects both imprecision in our predicted values and landlords' endogenous responses.[14] Importantly, as long as the exclusion restriction holds, the pass-through result in the IV is correct.

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

Appendix F details robustness to block-level analysis, dropping mixed-use and highly rent-stabilized buildings, and controlling for model-based, overlapping market structures.[15]

[13] Appendix Figure F.1 uses reported expenses to plot an average cost curve.

[14] Austin (2022) find larger landlords more likely to challenge assessments. This would be an additional source of bias in the RF but not the 2SLS.

[15] Another concern we explore is the threat of variable trends correlated with base-year income. In

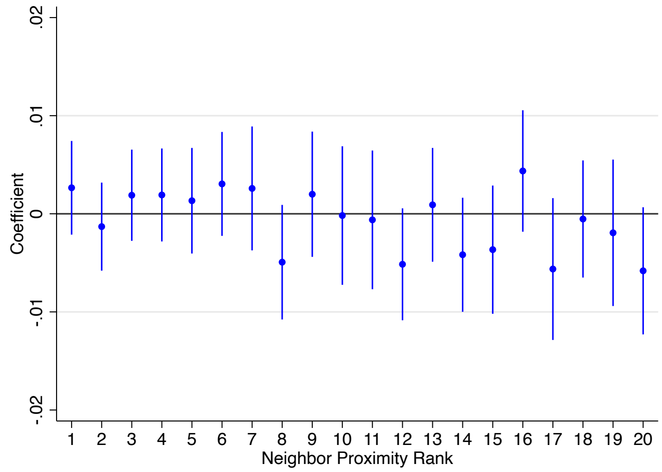
Placebo Test Phrased as an exclusion restriction, our identifying assumption is that our instrument is uncorrelated with unobserved building residual demand shifters. Although we cannot directly test this assumption, we consider a series of placebos where we run our specification using rents of each building's nth nearest neighbor, which are the competitors most likely to embody correlated demand or sorting on other unobservables. If the exclusion restriction holds, then fluctuations in each building's closest competition should be orthogonal to the instrument.

Figure 2 reports the coefficients from a series of regressions of each building's nth closest neighbor's rents on (own) tax instrument. Reassuringly, we find no systematic correlation and coefficients an order of magnitude smaller than our main results. This is robust to a multitude of alternative specifications, including grouping the top 5, 10, or 20 neighbors, and drawing concentric circles around buildings and including all neighbors therein. In Appendix F, we find similar results running placebo tests using proximity in non-geographic observables.

Contrasting Results Our unique setting allows us to test our hypothesis using very different sources of variation. Both tests affirm the existence of pricing power. However, the pass-through rate here is substantially higher than the previous empirical design. This may be due to the caveats discussed in the previous section that cause downward bias, within-SBA sorting, or to price stickiness that partially mutes the large negative cost shock's effect, both of which lead to lower pass-through estimates. Furthermore, LATEs may simply differ for large and small buildings. The subsequent section further investigates how our results are affected by market structure.

unreported results, we find controlling for year trends by initial gross income increases the coefficient's magnitude by about 17%.

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



Note: The figure plots coefficients (blue dots) from a regression of buildings' instrument Z_{jgt} on the average unit rent at their 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.

6 Market Structure and Pass-Through

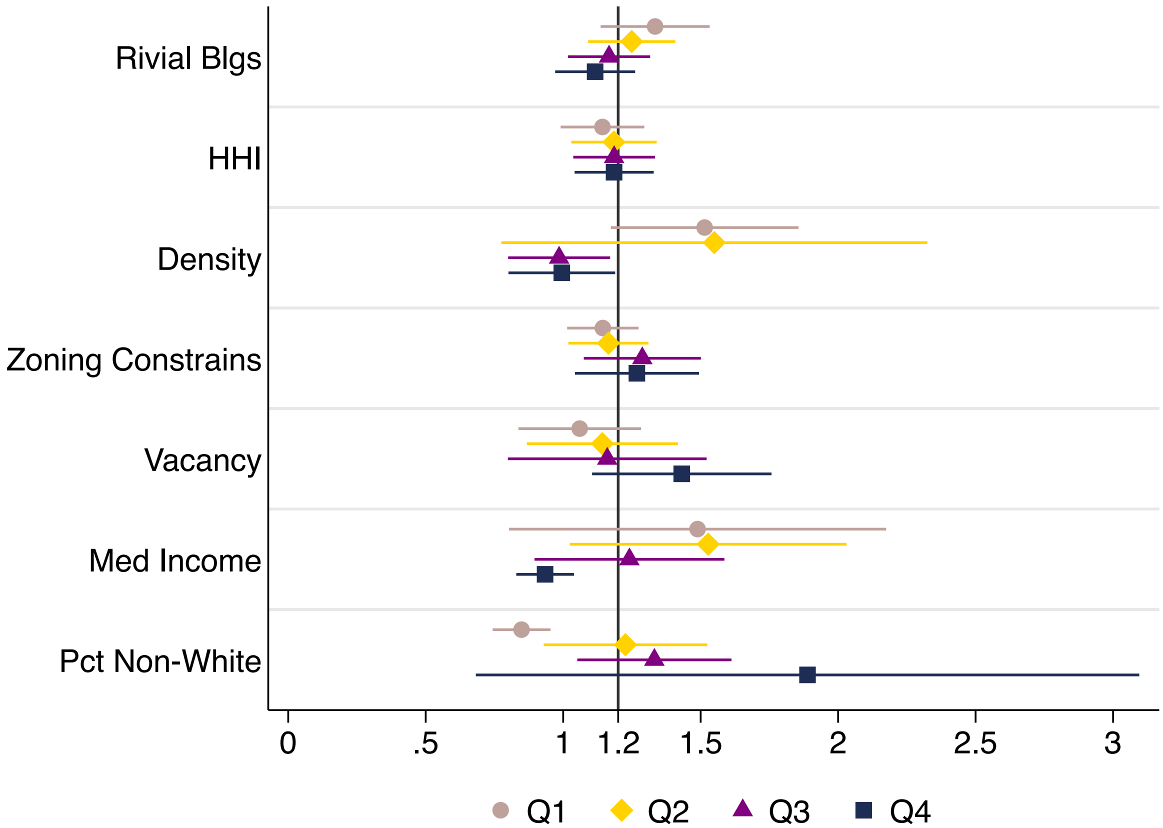
NYC is exceptional in many ways, including its diversity of neighborhood characteristics. Here, we leverage that diversity to investigate how our results vary with market structure and demand characteristics. Figure 3 repeats the specification in column 3 of Table 2 for each quartile of our sample, divided along location characteristics: the number of local competitors, level of ownership concentration, population density, percent of zoning constrained parcels, vacancy rates, and median income and racial composition.[16] These measures are equilibrium outcomes, and differences in pass-through rates could emerge through supply-side differences like costs *or* demand-side factors like sorting.

Despite underlying market structure heterogeneity, nearly all pass-through rates are above 1 and most hover around our main result of about 1.2. These results confirm the robustness of our findings across the various market structures in our data. However, we also find patterns consistent with the hypothesis that competitive market structures

[16]Appendix D discusses our construction of zoning constraints and HHI.

exhibit lower pass-through. The number of competitor buildings in a location reduces the pass-through rate. Local competition could increase renters' ability to substitute across buildings. Although measured population density could also reflect greater local demand, denser tracts exhibit lower pass-through rates. Interestingly, while local ownership concentration appears to increase pass-through as might be expected, the effect is slight and insignificant. Buildings surrounded by a higher share of zoning-constrained buildings also exhibit slightly higher pass-through rates, though these differences are insignificant. This pattern is consistent with zoning either reducing the threat of entry, better enforcing Bertrand competition, or simply making supply tighter relative to demand. This raises an interesting possibility that zoning constraints interact with pricing power. Perhaps surprisingly, tracts with the highest vacancy rates also have buildings with higher pass-through rates (though again differences are insignificant). Vacancy could be taken as an indication of lower demand and greater availability of substitute apartments. On the other hand, vacancy is an equilibrium result of pricing decisions; landlords in tracts with higher vacancy may be responding to market power by raising rents, inducing vacancy. Finally, pass-through results may vary substantially with equilibrium demand characteristics. Tracts with the highest income and lowest share of minorities have the lowest pass-through rates. Here some interquartile differences are significant, though Q1/Q4 measures are particularly noisy. One possible explanation for this result is that higher moving costs for poor households give landlords more pricing power. While we interpret these results with maximal caution, this potentially implies that the impact of pricing power is uneven and may have implications for inequality.

Figure 3: Heterogeneity in Pass-Through



Note: The plot reports multiple regressions – reported in Table F.5 – that explore heterogeneity of our pass-through results based on tract level variation in various measures: the annual number of rival buildings in the same tract, annual large rental building HHI, 2007-2011 population density, annual zoning constrained building share, 2007-2011 rental vacancy rate, 2007-2011 median income, 2007-2011 non-white resident share. All regressions use building and tract-year fixed effects, and standard errors are clustered at the tract level. Data come from our NYC Buildings sample (rent, costs, our instrument, rival buildings, HHI, zoning constraints) or the ACS 5YR 2007-2011 tract estimates (density, vacancy, percent non-white, median income).

7 Conclusion

Using two empirical approaches with distinct identifying assumptions and sources of variation, we find increases in buildings' idiosyncratic tax burdens lead to increases in rents. This behavior is inconsistent with market structures featuring perfect competition, implying that rents are set with markups.

The existence of markups has far-reaching consequences for our understanding of the housing supply and housing constraints, responsible for large-scale economic distortions and first-order losses (Hsieh and Moretti, 2019). Pricing power is a supply constraint new to urban theory: developers internalizing pricing power would set oligopolist-optimal quantities below competitive quantities. Moreover, the wedge between marginal costs and rents has been used as a measure of the quantity distortions of regulation (Glaeser, Gyourko, and Saks, 2005; Glaeser and Ward, 2009; Glaeser and Gyourko, 2018). Pricing power is an alternative source of this wedge, and would exist in the absence of regulation, rendering it a source of bias in our understanding of regulations' costs.

Furthermore, the conceptual frameworks used to generate widely-used housing production function and elasticity estimates rely on marginal cost pricing assumptions (Baum-Snow and Han, 2023; Combes, Duranton, and Gobillon, 2021).[17] In the presence of variable markups, specifications used in their estimation are potentially biased, at least requiring additional assumptions.

Finally, the growing quantitative spatial modeling literature uses marginal cost pricing to estimate counterfactual equilibria. The predicted consequences and welfare implications of policy changes such as deregulation, infrastructure improvement, or tax policy shifts—all of which result in localized changes in housing demand—are potentially biased by differential and increasing markups.[18]

Our results call for more work exploring the sources and scope of pricing power, its policy consequences and housing supply effects.

[17]For example, if our results extend to the single-family housing market, then the estimates found in equations (4) and (5) of Combes, Duranton, and Gobillon (2021) can be adjusted by a fixed proportion in the presence of pricing power up to a fixed proportion under the assumption of constant demand elasticity. Watson and Ziv (2021) show that pricing power in leasing impacts development decisions.

[18]An exception is Ospital (2023), who calculates counterfactuals assuming monopolist landlord pricing.

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

C. Luke Watson and Oren Ziv

A Theoretical Appendix

A.1 Theoretical Setting

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

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

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

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

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—administrative costs, maintenance and labor costs, etc—are reported in our data via RPIE forms in addition to average building income. Property taxes, which in our setting are income-based, are also marginal costs we observe.

A.2 Idiosyncratic costs and pass-through

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

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

Pass-through under perfect competition In the former case, by definition, demand for each building is perfectly elastic: any positive deviation from the market rent for their building results in a complete collapse in demand.[19] Here, we explain how shocks to costs only affect market rents through the common component while the idiosyncratic component affects the firm's supply but cannot affect its posted rent.

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

Figure A.1b instead displays a situation where the firm's marginal cost is increasing and the market component is positive: $\mathtt{d}\ln(mc_{ft}) > 0,\ \Delta_t > 0$. 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 an (undrawn) leftward shift in market supply. This supply shift leads to a quantity change along the (undrawn, aggregate) demand that increases the market rent, and, finally, manifests in Figure A.1b as an upward shift in the landlord's residual demand curve as shown in red.

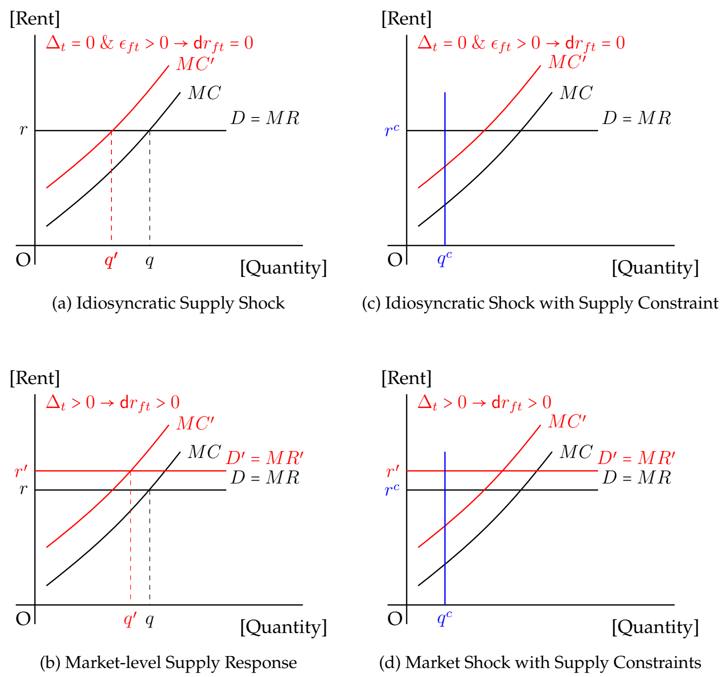
The effects of capacity constraints do not substantially change this analysis. In Figure A.1c we reconsider the case where $\Delta_t = 0$ and $\epsilon_{ft} > 0$ but the landlord faces a binding supply constraint that is represented by the blue vertical line. In this case, the initial equilibrium is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

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

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

analyses apply.

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



(a) Idiosyncratic Supply Shock

(c) Idiosyncratic Shock with Supply Constraint

(b) Market-level Supply Response

(d) Market Shock with Supply Constraints

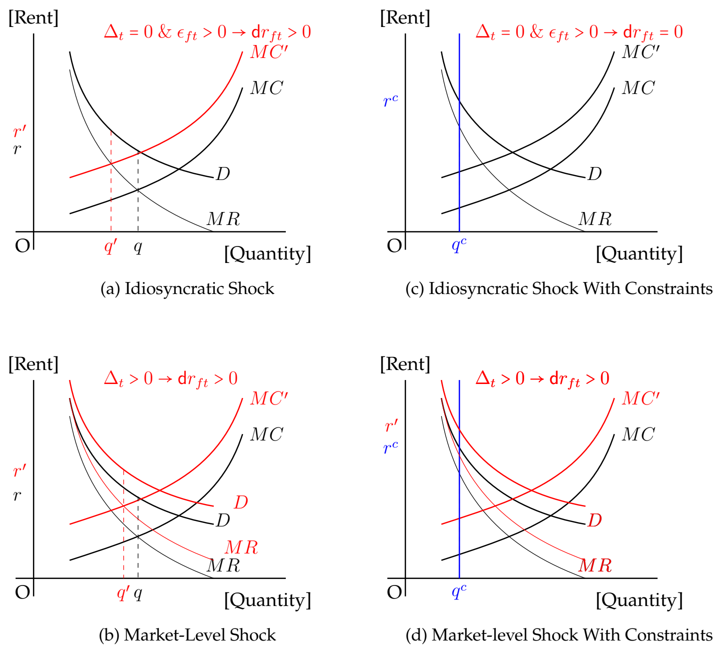
Finally, as with capacity constraints, considerations of entry or exit do not affect qualitatively alter rent responses. In each of the four panels, the area between the residual demand curve and marginal cost is variable profit gross of an entry cost. Entry or exit would occur only in A.1b or A.1d where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

Pass-through of cost shocks with downward sloping demand We now turn to the effect of the same cost shocks under deviations from perfect competition. Figures A.2a through A.2d repeat the above exercise but assuming downward sloping demand. In A.2a the result immediately departs from that of the figure's counterpart, A.1a. In particular, the shift in idiosyncratic marginal cost moves the firm up the demand curve, the new price and quantity are higher and lower, respectively. Note that even if we (somehow) ignore monopoly pricing, and posit instead competitive pricing, the result would hold. The addition of the market-level component in Figure A.2b changes the observable effect quantitatively but not qualitatively.

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

Noting that entry affects the analysis in all cases in the same way as in the perfectly competitive benchmark, we conclude it is only possible for idiosyncratic cost variation to be passed-through into rent when the demand curve slopes down. Informed by this

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



(a) Idiosyncratic Shock

(c) Idiosyncratic Shock With Constraints

(b) Market-Level Shock

(d) Market-level Shock With Constraints

conclusion, our empirical specifications attempt to isolate variation in ϵ_{ft} and test for 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 ($\epsilon^C = 0$):

$$\mathsf{d}\ln(\mathsf{mc}^T_{ft}) - \mathsf{d}\ln(\mathsf{mc}^C_{jt}) = \left(\beta \cdot \left[\Delta_t + \epsilon^T_{ft}\right] + \lambda_t + u^T_{ft}\right) - \left(\beta \cdot \left[\Delta_t + \epsilon^C_{jt}\right] + \lambda_t + u^C_{jt}\right) \tag{A.1}$$

$$= \beta\epsilon^T_{ft} + \left(u^T_{ft} - u^C_{jt}\right). \tag{A.2}$$

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:

$$\text{FS:}\quad \mathsf{d}\ln(\mathsf{mc}_{ft}) = \pi Z_{ft} + \lambda^{\text{FS}}_t + v_{ft} \tag{A.3}$$

$$\text{SS:}\quad \mathsf{d}\ln(r_{ft}) = \beta\mathsf{d}\ln(\mathsf{mc}_{ft}) + \lambda^{\text{SS}}_t + u_{ft} \tag{A.4}$$

$$= \beta\epsilon_{ft} + \tilde{\lambda}^{\text{SS}}_t + u_{ft}, \tag{A.5}$$

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.

B NYC Property Taxes

NYC's property tax regime is unique from a number of perspectives. In this appendix we discuss details of the regime pertaining to our analysis. The information in this appendix includes information publicly available on the NYC DOF website, and has been verified verbally with DOF officials.

Tax class definitions NYC divides up properties into four classes. Class 1 consists of residential property of up to three units, including single family homes, buildings with small stores or offices as well as one or two apartments, and small condominium (condo) buildings (three or fewer floors). Class 2 includes all other residential properties, which includes larger condo buildings, cooperatives (coops, which are extremely prevalent in NYC), and multifamily rentals with four or more units. Class 3 includes utilities, and Class 4 includes all other properties including office buildings, retail buildings, and industrial structures. Our dataset consists of Class 2 buildings, omitting Class 2c, which are condominium and cooperative buildings, where most units are typically owner occupied.

A crucial and aspect of the taxation of Class 2 structures, and one which we believe is unique to NYC, is that it is based on prior tax year's reported income, which is reported to the DOF on Rental Property Income and Expense (RPIE) forms. Specifically, NYC links prior income to current taxes due through property valuations. As is the case in most jurisdictions, taxes are assessed on properties' values. However, in NYC, for Class 2 structures, properties are valued based on their income generation. Except for Class 2c, which are typically owner occupied and therefore have no reported income, income generation is the building's own actual income reported to the DOF. This means that the market values for each income-generating property in NYC are directly related to the building's gross and net income through a series of valuation formulas. This makes taxes a function of building income for these buildings.

Deriving market values from income To establish market values, the city uses one of two methods for Class 2 buildings: either multiplying gross income by a gross income multiplier (GIM), or dividing it by a capitalization rate (cap rate). Both cap rates and GIMs are *building-year specific.* They are arrived at using specific formulas. GIMs are used for 4-10 unit buildings (Classes 2a and 2b) and were used for 11+ unit buildings between 2008 and 2010. Capitalization rates were used for 11+ unit buildings prior to 2008 and after 2010.

Each building's GIM is arrived at by identifying a pool of comparable income producing buildings and sold buildings. Crucially, the pool for 4 and 5 unit buildings was not the same as 6-10 unit buildings until 2011. That is

$$GIM_{jt} = \frac{\sum_{p \in SP_j} \text{sale price psf}_p}{\sum_{p \in RP_j} \text{rental income psf}_p}$$

where sale price psf_p is the sale price per square foot of building p, a member of building j's sale comparable pool SP_j, and rental income psf_p is the rental income per square foot for building p, a member of building j's rental comparable pool RP_j.

The policy shift leveraged in Section 4 was the exclusion of properties from SP_j which did not generate income (i.e., owner-occupied). Prior to 2011, such properties were included only for 4 and 5 unit building GIM formulas. Because price per square foot is higher for owner-occupied buildings, their removal led to a nearly 50% reduction in GIMs for 4 and 5 unit buildings, and is the variation we use in our difference in differences specification.[20]

Capitalization rates are also derived in part from comparable sales. However, it is crucial to note that the DOF's definition of capitalization rates differs in important ways from those used colloquially in real estate. While the typical definition of a cap rate is an income-based rate of return derived from a market value and rents, the DOF definition is inverted: multipliers for which the DOF derives market valuations *from* rental income.

To assign building-specific cap rates, the DOF uses a formula whose coefficients are derived from median regressions of buildings' sale prices and income (as discussed in the main text). Conversations with the DOF conveyed to us that the benefits of this approach were to (1) reduce fluctuations in taxes (2) remove the effects of non-income related land appreciation from taxes, and (3) reduce the influence of outlier sales on tax assessment. Buildings' net income per square foot are plugged into the annual formulas to arrive at building-specific capitalization rates.

If net income is an extreme outlier (due to exceptionally high vacancy, for instance), or if RPIE forms are not filed, the DOF does not or cannot use income and expenses to determine market values. In place of income, the DOF constructs market value from potential-income, which is arrived at using reported income from nearby comparable buildings (as is done for condo and coop buildings, discussed below). These buildings do *not* enter into our analysis. Because income (and expenses) are not used to calculate values, Notice of Property Values do not report back these figures to owners, and so we do not (cannot) scrape this information.

Another important caveat to the above is that the Class 2 buildings that we omit from our analysis (2c, condos and coops) are generally not *directly* based on any income produced. This is because they typically are owner occupied. However, because they are Class 2, their market values are assessed based on the potential for income generation. The assessment procedure for those buildings is well documented in Gupta, Van Nieuwerburgh, and Kontokosta (2022). Readers familiar with that paper will note that for such properties nearby comparable properties' incomes are used, so no direct link between rental income (if appropriate) and taxes exists. By contrast, the buildings in our sample are multifamily Class 2 structures and (for those in our sample) it is their own income—and *not* comparables' income—that is used in valuation and for tax calculation, which we discuss below.

Deriving taxes from market values Once market values are established, DOF calculates assessed and transitional assessed values. The link between market and assessed values is straightforward: assessed values are always 45% of market values. However, the link between market value and actual tax assessments (and therefore the link between income and tax liabilities) is mediated by a series of rules that create a potential difference between assessed values, and *transitional* assessed values. Annual changes in assessed values are phased in over a 5 year period, so that transitional assessed values are the previous years' transitional assessed values plus the sum of 20% times the previous five years of changes of assessed values. Furthermore, for Class 2a and 2b structures (small multifamily buildings), state law caps the annual change at an 8% increase relative to the total tax base in the previous year and 30% relative to the tax base five years prior. As a result, transitional assessed values are sometimes far below market values for many years. After transitional assessed values are calculated, a property's exemptions are subtracted, then the resulting value is multiplied by a single, city-wide tax rate that varies annually. Finally, tax abatements are subtracted to arrive at the tax bill. In sum, tax rate changes impact how present income is taxed on future

[20]While 10-unit structures are taxed like 6-9 unit structures, the NYCHVS groups them with larger structures, so they neither appear in our Section 4 analysis using NYCHVS data, nor in Section 5 which focuses on buildings with capitalization rates.

bills. Because of the phase-in rules, taxes on income made in one year are effectively paid over a series of 5 subsequent years.

From taxes to marginal costs Tax formula changes that impact market values act as marginal cost shifters for buildings through their impact on expected future taxation on present-period income. In particular, as with any income tax, taxes on building income are paid *ex-post*. As with any tax on business income, firms will internalize these future taxes in present pricing decisions as they would a marginal cost. That is, future tax bills are factored into present-period effective marginal costs. Whereas current-period taxes due are based on prior income and present-period building income has no effect on those bills, present income will impact future tax bills. Changes in the capitalization rates and GIMs, which effectively change the rate of taxation on income, will therefore function like a shift in marginal cost. However, unlike other income taxes, where the tax rates are typically determined before behavioral decisions are made, the capitalization rate and GIM formulas discussed above are implemented *ex-post*. Buildings must therefore expect effective income tax rate changes to be persistent when considering how their income will be taxed and when factoring those taxes in to present-period pricing decisions. The response of landlords to these tax changes, raising rents more when the last effective tax rate increases on previous income, is in line with the assumption that they expect tax rate changes to persist. Further, these changes generally do persist; the Pearson's correlation coefficient across years in market values (which also includes income changes on top of formula changes) is greater than 0.96.

C Construction of Samples

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.

NYCHVS: Tax Reform DID Sample 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.

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 the 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 use the following controls: condition of building indicators, number units in building (binned), number of floors (binned), elevator indicator, and sub-borough-area-by-year fixed effects. Note, Tables C.1 and C.2 below display more variables than we use in the regression specification in order to fully assess the sample characteristics.

Table C.1 displays summary statistics for the NYCHVS data used in Section 4. The 'Full' columns use all buildings, the $\{4,5\}$ columns use the treated buildings, and the $\{6,7,8,9\}$ columns use the control buildings.

Table C.2 displays balance tests for building and occupant characteristics across treatment and control during the pre-reform period. We report the difference in means and the SBA-cluster-robust t-statistic for each variable. In the main text, we present results with and without covariates and and find consistent results.

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

Table C.1: Summary Statistics: 2002 - 2017 NYCHVS

	Full	$\{4,5\}$	$\{6,7,8,9\}$		Full	$\{4,5\}$	$\{6,7,8,9\}$
Observations	7,895	3,258	4,637	Average Rent	\$ 1413.06	\$ 1468.86	\$ 1372.37
Pct $\{4,5\}$	42%	100%	0%	Median Rent	\$ 1240.64	\$ 1242.31	\$ 1235.18
Pct w/ Elevator	3%	1%	4%	Pct Sound Condition	92%	92%	91%
Pct Built Pre 1947	86%	87%	85%	Pct Less 2 Year Lease	36%	34%	37%
Pct Built 1947-1989	9%	9%	9%	Pct 2+ Year Lease	28%	19%	35%
Pct Built Post 1990	5%	5%	6%	Pct Other Lease	36%	46%	29%
Pct Less 3 Stories	15%	32%	3%	Pct 0 – 3 Year Tenure	34%	37%	32%
Pct 3 – 10 Stories	84%	68%	96%	Pct 3 – 9 Year Tenure	37%	39%	35%
Pct 11+ Stories	0%	0%	0%	Pct 10+ Year Lease	29%	24%	33%

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, subset to all privately owned rental buildings. Full columns include all buildings with 4 – 9 units, $\{4,5\}$ columns include only units in buildings with $\{4,5\}$ units, and $\{6,7,8,9\}$ columns include only units in buildings with 6 – 9 units. Because the NYCHVS reports variables as granular categorical indicators rather than continuous variables, we have summarized the variables to parsimoniously present the data. In our regressions, we use more granular versions of these variables.

Table C.2: Balance Tests: 2002 - 2008 NYCHVS

Building Characteristics			Respondent Characteristics		
	Difference	t-stat		Difference	t-stat
Average Rent	\$ 192	3.03	HH Income	\$ 4,243	1.07
Pct Sound Condition	1.51%	1.17	Ln HH Income	0.17	1.43
Pct Built Pre 1947	2.05%	0.51	Age	-2.08	-2.51
Pct Built 1947-1989	-1.02%	-0.27	Pct Male	2.95%	1.23
Pct Built Post 1990	-1.03%	-0.99	Pct White	-8.34%	-2.39
Pct Less 3 Floors	28.08%	5.82	Pct Black	5.62%	1.40
Pct 3 – 10 Floors	-28.13%	-5.82	Pct Other Race	2.72%	1.38
Pct 11+ Floors	0.05%	0.19	Pct Hispanic	-2.65%	-0.98
Pct w/ Elevator	-2.73%	-4.37			
... controlling for floors	-0.71%	-1.15			

Note: This table reports balance tests for the sample we use in Section 4. We report the difference in means and the SBA-cluster-robust t-statistic for each variable across treatment and control during the pre-reform period. The row 'Pct w/ Elevator controlling for floors' also includes controls for number of floors in order to assess whether the presence of an elevator indicates other unobservable amenities.

Primary Land Use Tax Lot Output (PLUTO), the Department of Finance Final Assessment Roll (FAR), the Multiple Dwellings Registration and Contacts (MDRC) datasets (with prior years graciously provided to us by the NYU Furman Center), and communications between the DOF and building owners, scraped off the Property Tax Public Access web portal, which we call the Notice of Property Value (NPV) dataset.

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

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

The initial dataset features about 860,000 parcels per year, including all commercial buildings (specifically, NYC Tax Classes 2-4). We keep parcels with buildings that have a

NYC Building Class C1-C5, C7, C9, D0-D3, D5-D9, RR, S3-S5, or S9 at some point in their tenure in the dataset. This yields about 87,000 rental building 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 C.3 displays summary statistics for the NYC building panel data used in Section 5.

Table C.3: 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.

Our instrument's mean is 7.8 log points and the standard deviation is 0.72 log points. This implies a coefficient of variation of 9.1%. Because our specification includes tract-year fixed effects, it is important to discuss the conditional variation as well, which will naturally be lower. The across-tract average of within-tract average value of the instrument is 7.8 log points with median 7.7 log points. The across-tract average of within-tract standard deviation is 0.47 log points and the median is 0.45. The across-tract standard deviation of the within-tract standard deviation is 0.25 log points, implying an across-tract coefficient of variation of 53%. The across-tract average of the within-tract coefficient of variation is 6.0% and the median is 5.9%. This implies that while we lose substantial variation, as is expected, by including tract-year fixed effects, our instrument maintains substantial within-tract variation, which is consistent with its strength in the first stage.

C.1 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. NPV mailings confirm NYC DOF did receive income and/or expense information from owners and includes the amounts for each. RPIE forms collect data on the following cost categories: fuel, light and power, cleaning contracts, wages and payroll, repairs and maintenance, management and administration, insurance (annual), water & sewer, advertising, interior painting and decorating, amortized leasing costs (annualized, pro-rated cost), amortized tenant improvement costs (annualized, pro-rated cost), and miscellaneous expenses (not all deducted by Finance during valuation). If expenses are not reported to the NYC DOF, then we cannot scrape this information. 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.

D 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.

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.[21]

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

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

Density factors are "approximations of average unit size plus allowances for any common areas (NYC Zoning Glossary)," and when combined with floor area ratios result in the maximum number of dwelling units in a building. For example, if d is the density factor, then B_{area}/d is the maximum number of units allowed in the building.[22]

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

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

Zoning Constraint Definition We consider a building to zoning constrained if the building, given its current size and zoning policy, cannot add an additional minimum sized unit. If any of the following conditions are met, then we say a building is zoning constrained: (1) Average Unit Area is greater than the maximum possible residential area of the building divided by current units plus one: $(B_{area}/U) > (\mathsf{Max}(\text{Res}_{area})/(U+1))$, (2) The density factor is greater than the maximum possible residential area of the building divided by current units plus one: $d > (\mathsf{Max}(\text{Res}_{area})/(U+1))$, or (3) Building Area plus 300 sqft is greater than the maximum possible residential area of the building: $B_{area} + 300 > \mathsf{Max}(\text{Res}_{area})$.

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

A full history of NYC's rent regulation is available from the city as "History of the [NYC Rent Guidelines] Board and Rent Regulation."[23] 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 since 1951.

While rent control still exists for long-time incumbent renters, rent stabilization was introduced in 1969 and is the dominant form of rent regulation today. Both rent control and rent stabilization create the legal right to renew a lease, but the difference between the two is that rent control regulates the level of rents while rent stabilization regulates the growth in rents. Rent control applied to buildings built before 1947 while rent stabilization applied to buildings built between 1947-1974 (with six or more units), formerly rent controlled units, and units that accept J-51, 421-a, or 421-g tax benefits.[24]

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

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

In our sample period, units in rent stabilized buildings can become unregulated ("destabilized") if upon being vacated the landlord lists the unit above a predetermined rent. 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 threshold.

Rent Stabilization in NYC 44% of units in NYC are under rent stabilization. However, at any one time, about one third of these fall below the binding constraint Podkul (2017). These units are leased at "preferential rent," which is defined as any rent lower than the

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

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

maximum allowable under stabilization. In discussions, former high-ranking DHPD officials suggested these are especially common in outer boroughs and northern Manhattan. We parse communications from the NYC Department of Finance to building owners that lists the number of regulated units in a building.

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

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

To deal with this, an important robustness check we perform is to exclude buildings with many rent-stabilized units. These robustness checks can be found in Table F.1. Overall, we still find pass-through results inconsistent with perfect competition. While the reduced form in these regressions is higher, the pass-through rate is lower. When accounting for the selection in rent control buildings, these results are consistent with the heterogeneity results in the main text. In particular, rent-stabilized units are concentrated further from the center, in poorer and higher-minority neighborhoods. Exactly these neighborhoods exhibit higher pass-through rates according to the analysis in Section 6.

E Additional Tax Regime Reform DID Results

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

Table E.1: Event Study Regression Table

Dep.var: Log Unit Rent				
1.2002 × 1.Small	1.2005 × 1.Small	1.2011 × 1.Small	1.2014 × 1.Small	1.2017 × 1.Small
-0.02	-0.01	-0.14	-0.14	-0.09
(0.03)	(0.03)	(0.03)	(0.03)	(0.04)
Building Controls: Y		SBA-year FEs : Y		Observations : 7,895

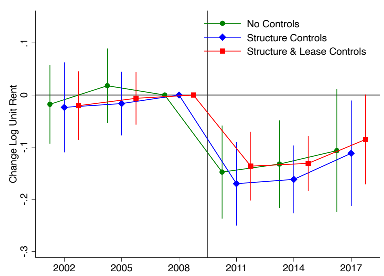
Note: The table displays the regression results for Figure 1b based on equation 14. Data is from the NYCHVS 2002 to 2017 samples. The regression controls for sub-borough-by-year fixed effects, building age fixed effects, condition of building indicators, number of floors groups, units in building, passenger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

Additional Event Study Results We present additional event study results, similar to Figure 1b. Figure E.1a presents three sets of event study results: without controls, with only structure controls, and, our baseline from the main text, structure controls with lease controls. Figure E.1b replicates the previous figure sans 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.

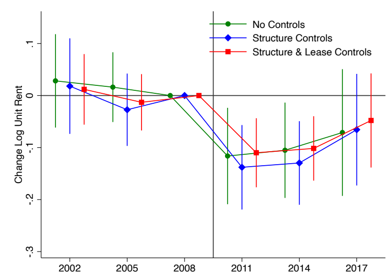
Figure E.1c compares our main specification with a subsample that drops all units that are rent regulated. The NYCHVS uses administrative data to code the regulation status of units, and using this coding we drop all rent *stabilized* and *controlled* units. As expected the subsample widens the confidence intervals, but does not qualitatively change our main findings that the policy reduced rents for the buildings.

In Figure E.1d, 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.

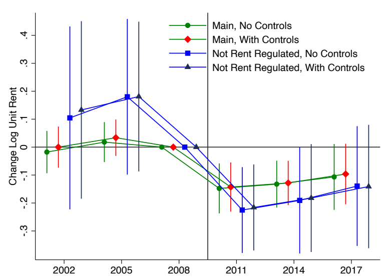
Figure E.1: Additional Event Study Results



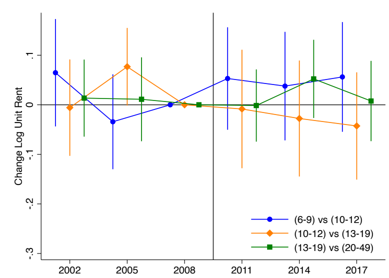
(a) Different Controls, Weighted



(b) Different Controls, Unweighted



(c) No Rent Regulation



(d) Placebo Groups

Entry and Exit Surrounding the Policy Change A specific concern regarding the policy shift we exploit in our difference in differences approach is that it may have differentially altered entry and exit among buildings of different sizes; for example, if lower taxes induce entry among smaller buildings. Such a positive supply shock could threaten our identification. Here, we examine both gross and net entry into each of the two groups, 4-5 unit buildings (treatment) and 6-9 unit buildings (control) in each year.

We use our DOF tax data to calculate gross and net entry. Entry is calculated as the percent of new buildings in a sample reporting a built age of the current year. Exit is more difficult to correctly capture. We calculate each building's final year in the sample. If a building's final year in the sample is the current year, we designate that as an exit. This assumes the final year in the sample is the building's exit year from the market (as opposed to, for instance, data limitations). In each year, we calculate the total number of such entries and calculate net entries as entries minus exits. We divide by the total number of buildings in each group and multiply by 100 to obtain a percentage.

Table E.2: Percent Gross and Net Entry by Size and Year

	Gross (%)		Net (%)	
Year	4-5 Units	6-9 Units	4-5 Units	6-9 Units
2007	0.14	0.27	0.14	0.27
2008	0.13	0.18	0.13	0.17
2009	0.12	0.32	-0.07	-0.22
2010	0.10	0.19	-0.23	-0.08
2011	0.06	0.09	-0.91	-0.45
2012	0.08	0.07	-0.55	-0.41
2013	0.09	0.11	-0.50	-0.30
2014	0.10	0.14	-0.50	-0.35
2015	0.08	0.22	-0.57	-0.28
2016	0.12	0.25	-0.66	-0.17
2017	0.06	0.23	-0.45	-0.17
2018	0.05	0.21	-0.40	-0.10

Note: The table reports the gross and net entry into each group of buildings in our sample by year and building size in percentage points. Gross entry is the percent of all buildings in the sample group reporting a year built equivalent to the current year. Net entry is the number of buildings in the sample group built in the current year minus the buildings whose last year in the sample is the current year, divided by the size of each sample group.

Table E.2 reports the percent of each sample entering and the net entry of the sample in each year. Entry rates (both gross and net) are quite low. Our setting is a built-up urban environment and in any given year, entries into either group are only on the order of two or three dozen. Typically a larger but still small number of buildings exit, so that in no year does any group experience a more than 1% change in supply. Second, and more importantly, we do not see differential entry into the 4-5 unit market around the time of the policy shift, nor is there differential attrition in the control. If anything, gross and net entry are more negative for treatment post-2011, although the difference is altogether slight.

Table E.3: Small Building Reform in Levels

	Real Prop. Tax per Unit-Month			Real Unit Monthly Rents		
	(1)	(2)	(3)	(4)	(5)	(6)
$1.[t > 2010] \cdot 1.\{4, 5\}$	-442.64	-423.43	-456.03	-172.32	-198.30	-193.53
	(10.72)	(8.83)	(8.15)	(73.34)	(57.03)	(50.12)
Building FEs	N	N	Y	–	–	–
Building Controls	–	–	–	N	N	Y
Tract-year FEs	N	Y	Y	–	–	–
SBA-year FEs	–	–	–	N	Y	Y
Observations	644,684	642,982	641,878	7,895	7,895	7,895

Note: The table replicates Table 1 with dependent variables in levels, using the same controls. Tax per unit-month is trimmed at the 1% and 99% level. Columns (1-3) use the NYC buildings data, and results are clustered at the census tract level. Columns (4-6) use the NYCHVS data, and are clustered at the sub-borough-area level.

Level Regressions This table replicates Table 1 except the dependent variables are in levels rather than logs. Panel A and B use real assessed property taxes per unit-month, trimmed at the 1% and 99% levels and real unit rent, respectively.

Occupancy Results In this subsection, we complement our analysis in Section 4 with an investigation into how the small-building reform affected occupancy rates.

To do so, we rely on ACS data on block-group level vacancy rates. The ACS reports 5-year overlapping averages does not provide breakdowns by building size. We exploit differences in the percent of local buildings that are in the "treated" (4-5 unit) vs "control" (6-9 unit) group. We run our DiD specification interacting this treatment intensity measure with with a "post reform" indicator ($t > 2010$), observing the differential impact of the 2011 policy change on the more intensely treated block groups.

Because of the 5-year overlapping ACS samples, we omit samples from 2009-2012, as they span both the pre- and post-periods. Table E.4 reports results. Panel (a) focuses just on the subset of block groups with both treated and control groups. Column 1 uses just two waves of the ACS (2008 and 2014). We find an appreciable occupancy rate effect of 3.9 percentage points. Column 2 includes all overlapping ACS samples and finds a similar effect size. Columns 3 and 4 include tract-year fixed effects. The effect size here increases to around 4.5 to 5 percentage points, although our precision decreases (effects are still significant at the 5% level). Panel b repeats the exercise using all block-groups. Here our effect sizes shrink by around 2/3, although without tract-year fixed effects they maintain significant. Finally, adding tract-year fixed effects (columns 7 and 8) again removes a great deal of variation, making our estimates less precise. While our estimates here decline to zero, we cannot rule out the effect sizes found in the previous columns.

Table E.4: Block-Group Occupancy Response

	Panel A: Block-Groups w/ All Size Groups				Panel B: All Block-Groups			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\text{Post}_t \times \text{Sh}_{\ell,t_0}$	0.039	0.033	0.053	0.044	0.015	0.012	-0.001	-0.005
	(0.012)	(0.010)	(0.026)	(0.021)	(0.006)	(0.005)	(0.011)	(0.010)
Pre/Post	'08/'14	'07-'09/'13-'15	'08/'14	'07-'09/'13-'15	'08/'14	'07-'09/'13-'15	'08/'14	'07-'09/'13-'15'
Block-Group FEs	Y	Y	Y	Y	Y	Y	Y	Y
Year FEs	Y	Y	N	N	Y	Y	N	N
Tract-year FEs	N	N	Y	Y	N	N	Y	Y
Observations	4,356	13,401	3,386	10,497	10,078	30,322	9,420	28,363

Note: The table reports multiple regressions adapting our Small Building Reform strategy from our main analysis to rental vacancy rates. We use 5-Year ACS block-group aggregate rental occupancy estimates for the dependent variable. Sh_{ℓ,t_0} is the share of 4-5 unit buildings among all 4+ unit buildings in the block group in the 2007-2009 period. Columns 1-4 use only block-groups with a positive share of small and large buildings, while columns 5-8 use all block-groups. Columns (1,3,5,7) use only 2008 (pre) and 2014 (post) to match the NYCHVS; columns (2,4,6,7) use additional years: 2007-2009 (pre) and 2013-2015 (post). Columns (1,2,5,6) use year FEs; columns (3,4,7,8) use tract-year FEs. All columns use block-group FEs and are clustered at the tract level.

F Additional Synthetic Tax IV Results

OLS results The first two columns of Table F.1 report OLS results of log average rent on log total cost per unit. These results show a lower cost-price elasticity than the results in our main analysis. An obvious reason for this difference is endogeneity concerns about unobserved correlated determinate of cost and demand. Another possibility is that the IV results identify a specific LATE due to idiosyncratic tax-cost shocks.

Alternate Samples Next, 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. Table F.1 reports our results for both subsamples, which are largely similar to our main specification with one exception: the latter sample has smaller-magnitude pass-though estimates.

Table F.1: Additional Pass-Through Results

	OLS		Residential Only				<50% Rent Stabilized				Census Block FEs			
			Reduced Form		2SLS		Reduced Form		2SLS		Reduced Form		2SLS	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Log Total Cost	0.688	0.677			1.161	1.217			0.886	0.829			1.096	1.161
	(0.006)	(0.006)			(0.077)	(0.107)			(0.068)	(0.117)			(0.078)	(0.124)
Log Cf Tax			0.038	0.031			0.045	0.024			0.039	0.028		
			(0.004)	(0.004)			(0.008)	(0.006)			(0.005)	(0.004)		
Robust F Stat					53.22	37.68			44.54	26.68			50.11	27.00
Robust AR Stat					78.90	63.31			35.83	18.73			70.08	42.10
One-Side Test					0.019	0.021			0.953	0.928			0.110	0.097
Time-varying controls	N	Y	N	Y	N	Y	N	Y	N	Y	N	Y	N	Y
Building FEs	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Tract-year FEs	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	N	N	N	N
Block-year FEs	N	N	N	N	N	N	N	N	N	N	Y	Y	Y	Y
Observations	152,559	152,559	112,823	112,823	112,809	112,809	52,354	52,354	52,342	52,342	131,979	131,979	131,966	131,966

Note: The table displays robustness results for Table 2. Columns (1-2) report the OLS results for our main sample, columns (3-6) report IV results for fully-residential buildings, columns (7-10) report IV results for only buildings with less than 50% rent stabilized units, and columns (11-14) report IV results for our main sample but replaces tract-year FEs with block-year FEs. Standard errors and diagnostic statistics are clustered by Census tract.

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 placebo test sought to ensure neighbors' rents were orthogonal to our instrument, here we adopt this hypothesis wholesale and use neighbors' rents as controls meant to absorb demand shifters in place of or in combination with tract-level fixed effects.

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

Block-Year Fixed Effects The last four columns of F.1 display 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, 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.

Nearest Neighbors If residential search markets are continuous, then each building is subject to a unique set of competitors which overlap. While a full theoretical and structural model using this assumption is beyond the scope of this paper, we note that if renters search geographically, the overlapping set of renters between two competitors is decreasing in their distance. The extent to which a competitor's rents affect competitive environment at any location is also therefore a function of distance, and the market rent at each location will be a weighted averages of competitors rents.[25] To flexibly capture this alternative market definition, for each building, we include the buildings' neighbor's rent according to the neighbor's distance rank.

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

$$\ln[r_{jgt}] = \gamma_1 Z_{jgt} + \gamma_2 \mathsf{F}\left(\{\ln[r_{hg_h t}]\}_{\mathsf{j}_t(20)}\right) + \gamma_3 X_{jgt} + \gamma_4 D_j + \gamma_5 D_{gt} + \nu_{jgt} \tag{F.1}$$

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.[26] Under the assumption that neighbors' rent is 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 F.2 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.[27] Across specifications we find that the reduced form results are essentially the same as in Table 2 column (2).

An important point to reiterate is that we would expect spatial spillovers to be present in this context, and our results shouldn't be taken as implying that they do not exist here. Rather, the spillovers that operate from our instrument on local prices at large are sufficiently captured, along with any other such general equilibrium effects working through (from the landlords' perspective) residual demand, by our controls, which importantly include tract-year fixed effects. Nearly 97% of nearest neighbors are in the same tract, and over 64% of the 20th-nearest neighbors are in the same tract. We find that rank-one neighbors in the same have a median distance of 13 meters, while those in different tracts are 74 meters away; for rank 20, these are 107 and 183 meters, respectively. Overall neighbors in the same tract have a median distance of 67 meters, and those in different tracts are 145 meters.

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

[25] If modeled, the distance parameter in renters' search would be the same governing a "market access" measure at each location, isomorphic to a location rent index, as in gravity trade models.

[26] 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.

[27] As we include tract-year fixed effects, many neighbors are in the same tract as a given building. We find that 97% of closest (rank 1) neighbors are in the same tract, at rank 10 it is 77%, and at rank 20 it is 64%. Overall, about 78% of neighbors are in the same tract across all building-neighbor pairs.

Table F.2: Pass-Through 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 a continuous measure of market definition. Columns (1,2) use a rolling five-neighbor average while (3,4) use a linear interpolation of missing values to estimate eq F.1. Columns (1,3) use year and building FEs. Columns (2,4) use tract-year and building FEs. All columns include building-level controls. Standard errors are clustered by census tract.

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

Table F.3 creates characteristic specific percentile group leave-one-out averages of rent, then regresses that average on our instrument using our main specifications' fixed effects:

$$\frac{1}{N_{H_j^{x^c}}} \sum_{h \in H_j^{x^c}} \ln[r_{hg_h t}] = \zeta_1 Z_{jgt} + \zeta_2 X_{jgt} + \zeta_3 D_j + \zeta_4 D_{gt} + u_{jgt}, \tag{F.2}$$

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

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

Positive and Negative Split Table F.4 replicates the analysis from Table 2 testing for asymmetric effects of the IV around zero. We residualize the tax IV on building and tract-year FEs and segment it around zero. Column (2) adds time-varying covariates. We include both segments in the reduced form regressions. We test for equality of the coefficients using a cluster robust F stat. We report the coefficents, the F stat, and the F stat's p-value.

Heterogeneity Analysis Table F.5 report heterogeneity analysis of our pass-through results along seven dimensions: annual number of rival buildings in tract, annual tract HHI, tract vacancy, annual percent of zoning constrained buildings, tract median income, tract percent non-white population, and tract density. We assign each buildings into quartiles of the variables, from the lowest values in quartile 1 to highest in quartile 4. We calculate rival buildings, HHI, and zoning constraints from our NYC data; we calculate density, vacancy, median income, and percent non-white from the ACS 2007-2011 tract estimates. All

Table F.3: Non-spatial Placebo Tests: X-Group Instrument Correlations

	Dependent Variable: X-Group Neighbors' Monthly Rent				
	Subway Dist (1)	Building Age (2)	Yrs Since Renovation (3)	Avg Unit Size (4)	Zipcode AGI (5)
ζ_1	-0.000	-0.002	-0.009	0.000	-0.001
se	(0.002)	(0.002)	(0.004)	(0.002)	(0.001)
t	0.000	0.934	2.212	0.109	0.936
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 F.2. 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.

Table F.4: Pos-Neg: Pass-through of Cost Shocks on Rent

	(1)	(2)
max(0, Resid Log Cf Tax)	0.033	0.017
	(0.007)	(0.007)
min(0, Resid Log Cf Tax)	0.039	0.038
	(0.006)	(0.006)
F	0.229	3.967
pval	0.633	0.047
Time-varying controls	N	Y
Building FEs	Y	Y
Tract-year FEs	Y	Y
Observations	152,559	152,559

Note: The table reports tests for asymmetric effects of our instrument based on the sign of the cost shock. The simulated tax IV from Table 2 is residualized then segmented at zero. Column (2) adds building-level time-varying controls. All regressions are at the building-year level with standard errors clustered at the tract level, and include building and tract-year fixed effects.

models include both building and tract-year fixed effects and use standard errors clustered at the tract level. We report the effective F statistic of instrument strength from Lewis and Mertens (2022), which accounts for multiple endogenous variables and instruments, and the Anderson-Rubin robust F statistic, which is a weak instrument robust test for the joint significance of the endogenous variables. While our point estimates are all in-line with our baseline estimate of 1.2, the diagnostic tests reveal that some models of heterogeneity appear 'stronger' than others: columns (1,2,3,4) appear reasonably strong while columns (5,6,7) appear weaker at conventional statistical levels.

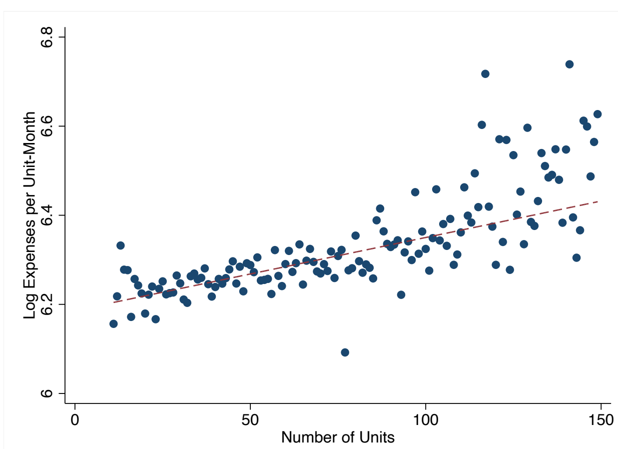
Average Costs Figure F.1 plots a binned scatter plot of the log of buildings' average monthly expenses (total cost divided by number of units divided by twelve) by building size. Recall expense data is sourced from DOF communications for 10+ unit buildings. Although there's some suggestion that average cost is falling between for the very smallest buildings, the curve displays a clear upward trend. These reported costs come with heavy caveats. This cross-sectional relationship does not account for differences in amenities across building types. Larger buildings may have more amenities. Still, the figure is consistent with a cost structure involving some fixed costs and increasing marginal costs.

Table F.5: Heterogeneity of Pass-through of Cost Shocks on Rent

	Rival Bldgs (1)	HHI (2)	Density (3)	Vacancy (4)	Zoning Const. (5)	Med. Income (6)	Pct. Non-White (7)
Q1	1.321	1.141	1.515	1.144	1.060	1.489	0.849
	(0.100)	(0.077)	(0.174)	(0.066)	(0.114)	(0.350)	(0.054)
Q2	1.227	1.184	1.550	1.165	1.143	1.527	1.227
	(0.079)	(0.079)	(0.395)	(0.074)	(0.140)	(0.257)	(0.152)
Q3	1.164	1.187	0.985	1.288	1.160	1.241	1.332
	(0.075)	(0.076)	(0.095)	(0.109)	(0.184)	(0.176)	(0.143)
Q4	1.120	1.186	0.995	1.268	1.431	0.934	1.889
	(0.075)	(0.074)	(0.099)	(0.115)	(0.167)	(0.054)	(0.616)
LM Ef.F	11.50	12.67	6.78	8.66	13.57	6.85	4.04
AR F	37.94	34.67	33.84	31.08	36.35	34.68	37.52
P-Value Q1 vs Q4	0.013	0.236	0.010	0.151	0.066	0.117	0.092
Tract-year FEs	Y	Y	Y	Y	Y	Y	Y
Building FEs	Y	Y	Y	Y	Y	Y	Y
Observations	152,559	152,559	152,559	152,559	152,559	152,559	152,559

Note: The table reports multiple regressions to explore heterogeneity of our pass-through results based on tract level variation in various measures. Across columns 1 to 7, we consider the annual number of rival buildings, annual large rental building HHI, 2007-2011 population density, 2007-2011 rental vacancies, annual zoning constrained building share, 2007-2011 median income, 2007-2011 non-white resident share. For each variable, we group the buildings into quartiles. We interact these quartile indicators with the instrument and endogenous cost to estimate 2SLS results. All regressions use building and tract-year FEs, and standard errors and diagnostic statistics are clustered at the tract level. Data come from our NYC Buildings sample (rent, costs, our instrument, rival buildings, HHI, zoning constraints) or the ACS 5YR 2007-2011 tract estimates (density, vacancy, percent non-white, median income).

Figure F.1: Average Costs, by Building Size



Note: The figure plots a binned scatter of buildings' total units against the natural log of their reported monthly nominal expenses divided by the number of units, for all buildings with between 11 and 150 units, for all years of data between 2007-2019. Each bin the average reported monthly per unit expenses across all buildings with the same total number of units.