How Mortgage Financing Costs Affect Rental Housing: Pass-Through & Pricing

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November 2022

Abstract

This paper estimates how changes in landlords' mortgage financing costs affect rental housing. Using a novel dataset in which I link property-level asking rents, mortgages, and financial outcomes, I exploit quasi-exogenous variation in mortgage contract terms coming from (1) the end of interest-only payment periods, and (2) refinances after prepayment lock expiration. I find that landlords significantly raise revenue by 3% after interest-only periods end, passing through half of the rise in mortgage payments to tenants, but also increase revenue by 3-4% at refinance, where mortgage payments decline on average. To explain these seemingly contradictory results, I model the interaction of information costs and financing frictions. Liquidity constrained landlords raise revenues after negative cash flow shocks by updating their information to correct rent mispricing. Landlords engage in similar information updating at refinance in order to extract equity. Aggregated to the city level, I estimate that when 5% of mortgages prepay or mature, a 1 ppt increase in rates causes 0.3 ppt higher rent growth. These results provide new evidence that renters are strongly affected by the financing conditions of their landlords.

Keywords: Rental Housing Markets, Real Estate Economics, Mortgage Finance, Behavioral Pricing.

JEL Classification: R31, G21, D22, D25, D83, E44

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1 Introduction

Do landlords pass along any of the benefits from lower financing costs to their tenants? While many homeowners and landlords benefited from refinancing into mortgages with progressively lower rates over the past two decades, it is unclear whether tenants have shared in those benefits. Over the same time period, median rent-to-income ratios rose from 25% to 30% so that in 2017 half of renter households spent more than 30% of their income on rent (JCHS 2020), generating concerns about a rental housing affordability crisis. Understanding how monetary policy decisions and changing interest rates affect renters—who make up 35% of the U.S. population, have lower average incomes, and are more likely to be from minority groups—is informative about how monetary and macroprudential policy affect the real economy. This paper studies whether and how shifts in debt financing costs on multifamily mortgages affect tenants, landlord behavior, and rental housing markets.

Providing causal evidence on these issues is difficult because financing costs are (1) capitalized into a property's value, (2) endogenously linked to granular geographic changes in neighborhood amenities and labor markets, and (3) likely to simultaneously affect property quality and operations. A chief concern is that interest rates, and financing costs more broadly, are causally linked to or systematically correlated with property values.[1] A related concern is that properties in neighborhoods experiencing a downturn may experience a decline in occupancy, revenues, asking rents, and in the value of the property. Granular shocks to local firms or to nearby amenities could simultaneously lead to changes in market rents and mortgage payments. Finally, quality-related renovation and capital improvements may be related to both changes in mortgage payments and rents. Demand or supply shocks can affect renovations, generating correlated rents and financing decisions.[2] These dynamics make it difficult to estimate how shifts in financing costs affect rental housing markets using cross-sectional or aggregate data.

Using a novel linkage in two proprietary property-level rental housing datasets, I overcome these challenges by analyzing two quasi-experimental settings where within-property variation in mortgage contract terms allows me to identify the effect of financing on landlord behavior. In the first difference-in-differences strategy, I compare landlords experiencing a scheduled change in their payments with those who do not. In the second setting, I examine the expiration of prepayment lock-outs that allow landlords to refinance into a new loan, comparing them to landlords in the middle of their lock-out period. The use of two empirical strategies is necessary to understand the two situations in which landlords are exposed to financing cost shocks: when mortgage payments change because of the terms of their existing contract, or when they refinance into a new mortgage contract. Heterogeneity analyses help identify two reasons for landlords' changing their behavior, leverage and liquidity constraints, and the many margins along which landlords can adjust, including increasing (or decreasing) tenant rents or changing their leasing effort. Finally, the paper shows the aggregate economic consequences of these financing cost shocks, measured with city-level rent indices.

The first empirical analysis studies how landlords respond to changes in cash flows when their mortgage payments increase due to the end of interest-only periods. The end of an interest-only period provides an anticipated shock to the size of mortgage payments (but not to the cost of capital). I compare landlords with interest-only periods for half of their loan term to stacked control groups of landlords with no I-O period. This allows me to measure how much of a change in mortgage payments is passed through to tenants when the mortgage payment change is anticipated in size and timing. If landlords are setting rents competitively

[1] See e.g. Glaeser et al. (2013) on capitalization of interest rates into prices for owner-occupied housing; Gete and Reher (2018) on single-family mortgage credit shocks affecting city-level rents through the renting versus home-owning margin.

[2] See e.g. Almeida, Campello, Laranjeira, et al. (2012)) or Reher (2021) on corporate investment response to credit and financing shocks.

in a frictionless market, then a change in mortgage payments should not affect their behavior.

The end of an interest-only period causes mortgage payments to rise sharply by 20%, while landlords significantly raise revenues by 3%. On average, landlords offset as much as 45 cents out of every dollar increase in debt service payments. Between one third and one half of the increase in revenue net of operating expenses comes from increasing occupancy rates. I find that these changes are concentrated among liquidity-constrained landlords, whose ratio of net operating income to mortgage payments is below 1.5 (i.e. the debt service coverage ratio). The combination of these results provides compelling evidence that financing costs are passed through to renters, likely through a combination of landlords' leasing out more units and a change in rental pricing.[3] While these results are informative about how changes in mortgage payments affect landlords' behavior, they are not enough on their own to understand the effect of a refinance or a change in interest rates.

The second empirical strategy uses variation in financing costs across refinance mortgages for the same property. This strategy exploits contract characteristics called prepayment lock-out periods, which prohibit refinancing for part of the loan's term. These lock-outs generate exogenous variation in refinance timing for ten-year, fixed rate multifamily mortgages with balloon payments.[4] I observe the existing and new mortgage for a subsample of properties. This allows me to construct stacked event study and difference-in-differences regressions to analyze the average change in property-level outcomes as landlords prepay their old mortgage and originate a new refinance mortgage.

On the average refinance, debt service payments fall by 10%, while landlords raise revenues at the same time by 3-4%. Drawing on the richness of the linked datasets, I verify that this seemingly contradictory result is not due to the timing of renovations, or due to selection in properties that are refinanced versus sold to new landlords. I investigate two potential channels that could explain why landlords increase revenues at refinance: leverage and exposure to increasing mortgage rates. When they are at risk of being underwater, landlords are more likely to increase revenues, which can raise the appraised property value. Landlords exposed to rising mortgage rates also increase revenues more, consistent with the salience of higher mortgage payments. However, neither of these channels fully explain the change in financial outcomes for the average refinance.

The two quasi-experimental research designs, each exploring a different type of financing shock, provide surprisingly opposing results: In one, landlords increase revenues in response to an increase in mortgage payments. In the other, financing costs fall, yet landlords nonetheless raise revenues rather than lower them. What can account for such a pattern? I clarify the channels by which landlords respond to financing cost shocks in a stylized model. Landlords have market power and decide whether to pay information costs.[5] Paying information costs allows landlords to re-optimize their pricing and leasing effort, increasing their net income.[6] In the model, changes in financing costs are salient because (a) an increase in mortgage payments

[3]In supplemental results, I study how a smaller sample of landlords respond to a positive cash flow shock when mortgage payments decline due to downward rate resets on hybrid adjustable-rate mortgages (hybrid 5-year ARMs are relatively rare in multifamily mortgage markets). Please find the Online Appendix at my website: https://sites.google.com/view/skhughes/research.

[4]Multifamily mortgages are different than the single-family owner-occupied mortgages (30-year, amortizing fixed rate mortgages). Most multifamily mortgages are shorter duration (5, 7, 10, or 15 years), and only partially amortize over their loan term. 20 or 30 year amortization schedules are common, so landlords have large 'balloon' payments at the end of their mortgage term (sometimes called 'bullet' repayment if they have interest-only payments for the full loan term).

[5]Consistent with this market power channel, Watson and Ziv 2021 argue that vertical differentiation generates significant market power for landlords. Their estimates of negative own-price elasticities of demand are between -2 and -6.

[6]The presence of information costs is consistent with the discussion of distorted beliefs in Giacoletti and Parsons (2022). It matches the real-world presence of large property and revenue management industries focused on information and consulting for landlords.

can raise the risk of negative cash flows or mortgage default (liquidity constraints); and (b) around a refinance, which is the only time landlords cash-out their equity, properties are re-appraised based on their current net income (frictions in equity extraction).

I investigate the information cost channel by showing empirically that landlords correct rent mispricing and update leasing effort in response to each of the financing cost shocks. I create a measure of mispricing and leasing effort for each property in the years before the financing cost shocks. The measures are based on the difference in the property's own asking rent per square foot and vacancy rate compared to every other property within a narrow geographic submarket. Relative to the control group, landlords fix mispricing 60% faster and update leasing 30% faster in the years around a financing cost shock.

Finally, I estimate the aggregate effects of changes in financing costs on rental prices through the "rental financing channel." I develop a measure of city-level financing cost shocks using the interaction between the share of multifamily mortgages prepaying or maturing and the exposure of existing mortgages to changes in mortgage rates. The main regression studies the effect of financing cost shocks on a hedonic city-level rental price index based on Census data on rents and housing characteristics.

I find that over the past 20 years, multifamily mortgage originations and rate exposure has measurably influenced rental prices. The financing cost shocks are positively related to rent growth. Cities more exposed to rate declines have lower rent growth, and cities exposed to higher mortgage rates experience higher rent growth. I find that in cities with 5 percent of mortgages prepaying or maturing, one percentage point higher rate exposure is related to 0.3 percent higher rent growth (and vice versa). I conclude that financing cost conditions have aggregate consequences for renters beyond their role in affecting employment or homeownership decisions.

1.1 Contribution

In interpreting why financing frictions and costs have real effects, I connect the rental housing market to the empirical finance literature on the real effects of financing costs (Modigliani and Miller 1958; Myers 1977; Whited 1992). Firms respond to credit or financing cost shocks in other industries by adjusting corporate investment.[7] This paper's analysis of the role of leverage and cash flow constraints studies an entirely new market with extraordinarily detailed data, and pushes forward the literature on the channels by which landlords update their behavior.[8] Notably, I highlight the role of information costs, which can affect both price-setting and leasing behavior. This paper is consistent with evidence of behavioral pricing in other markets, like reference dependence in the single-family housing market.[9] Landlord decision-making connects to pricing policies in other contexts where firms use discrete pricing policies and exhibit the characteristics of rational inattention (Reis 2006; Ball et al. 2005; Matějka 2016; Stevens 2020), and of similar behavioral wage-setting in labor markets (e.g. Dube, Manning, et al. 2020).

The core empirical results in this paper are the first estimates using within-property variation to identify pass-through of mortgage payments or interest rates in rental housing. Related work has focused on owner-occupied, single-family homeowners changing their borrowing and consumption behavior following changes in mortgage payments through adjustable rate-resets and mortgage refinances (Di Maggio, Kermani, and

[7]See e.g. Almeida, Campello, Laranjeira, et al. 2012; Kahle and Stulz 2013; Danis et al. 2014. There are also related employment effects (Benmelech et al. 2021; Chodorow-Reich 2014), and cash flow effects (Almeida, Campello, and Weisbach 2004; Custodio et al. 2022).

[8]Giacoletti and Parsons (2022) study pricing in the single-family rental market using cross-sectional rent data. Matsa 2011 also investigates related channels in work on retailer product quality and firms' financial leverage.

[9]See e.g. Andersen, Badarinza, et al. 2022; Genesove and Mayer 2001; and more widely among investors: Barberis and Xiong 2012.

Palmer 2020; Di Maggio, Kermani, Keys, et al. 2017; Berger et al. 2021), with some evidence on interest-only periods for homeowners in Denmark (Andersen, Campbell, et al. 2020; Bäckman and Khorunzhina 2020; Bäckman, van Santen, et al. 2022). There has only been limited evidence on the effect of monetary policy on rents and renters. Dias and Duarte (2019) (in the U.S.) and Cloyne et al. (2020) (in the U.K.) study the effect of monetary policy shocks on rental inflation or renter spending in impulse response regressions. This paper extends this literature by focusing on the mechanisms, channels, and sources of heterogeneity in how interest rate changes may affect renters or rental housing. There are similar connections to work on: monetary policy's effect on asset prices (e.g. Krishnamurthy and Vissing-Jorgensen 2011); the extent to which monetary policy is context-dependent (state-dependent in Eichenbaum et al. 2022 or path-dependent in Berger et al. 2021); and how the mortgage market channel of monetary policy affects inequality (Coibion et al. 2017) and may have differential effects by race and income (Gerardi et al. 2021; Cloyne et al. 2020; Kiefer et al. 2021; DeFusco and Mondragon 2020).

Complementing the growing interest in market power in labor and product markets, this paper contributes new estimates of cost-related pass-through to rents, occupancy, and revenues—an essential ingredient to analyzing incidence. This fits into research on landlord behavior and competition including Watson and Ziv (2021) who attempt to quantify mark-ups in New York City, and research empirically quantifying the connection between user costs and rents (e.g. Goeyvaerts and Buyst 2019; Garner and R. Verbrugge 2009) or the incidence of taxes on residential or commercial real estate (Goodman 2006; Rolheiser 2019). Given my focus on the cost of debt financing, an important segment of the prior literature has considered how financing costs affect rental housing, tenants' tenure choice, and landlords' decisions in other contexts like eviction, renovation, or maintenance (Seltzer 2021; Ambrose, An, et al. 2021; Reher 2021; Giacoletti and Parsons 2022; Greenwald and Guren 2021; McCollum and Milcheva 2020). This paper represents an advance in data construction by linking asking rents and net income at the property-level for a large set of apartments, connecting to research on the measurement of rental prices and quality across space and time.[10] The results here indicate that policymakers should use caution when interpreting price series from listings or from net income-based approaches, because in some cases (like responding to cash flow shocks) asking rents and property-level revenues can move in opposite directions.

Section 2 introduces the main datasets and presents stylized facts. Section 3 discusses the I-O empirical strategy and results. Section 4 introduces the refinance empirical analysis and results. Section 5 discusses a conceptual framework for thinking about landlords' decisions around financing cost changes. Section 6 presents results on landlords' updating pricing and leasing behavior. Section 7 examines results on aggregate effects of financing cost shocks. Section 8 discusses implications and concludes.

2 Data & Stylized Facts

2.1 Main Data Sources

This paper features a novel linkage of data from two proprietary data providers, which is necessary to study the effect of changes in financing costs on rental housing. First, to study the property-specific effects of shifts in financing costs, I need to link data on the financial outcomes of landlords' individual properties and details

[10] Several papers measure rents using different approaches, including repeat-rents for the same units (Ambrose, Coulson, et al. 2015) and within-property variation in revenues or net incomes (Ambrose, Coulson, et al. 2022); and more recently, applies those rent measures to study other questions, like labor demand shocks (Howard and Liebersohn 2021) and market power (Watson and Ziv 2021).

about the mortgages connected to those properties over time. I use the universe of securitized multifamily mortgages from Trepp to construct this data, which has national coverage of 64 thousand unique properties from the late 1990s to 2020. Second, to study changes in landlord behavior, I need property-level detail about rents that landlords are posting on the market, concessions landlords offer to new tenants, detailed vacancy or occupancy rates, and quality-related characteristics about properties (e.g. amenities available to tenants). I use a dataset from REIS by Moody's Analytics on the universe of market-rate apartment buildings. This is proprietary data based on surveys of every property owner and manager, which covers 37 thousand unique properties in 48 CBSAs for the fourth quarter of each year from 2005-2019. The average property in Trepp and REIS is 190-200 apartment units. I briefly describe the datasets and the construction of estimation samples in this section. Additional detail is available in Online Appendix A.

To study securitized multifamily mortgages, I construct an annual panel of mortgage characteristics, monthly payments, and outcomes at the property-level going back to the late 1990s. The raw datasets from Trepp contain information about borrowers, multifamily properties, & mortgage characteristics.[11] I observe property values at securitization.[12] Mortgage data includes loan characteristics and eventual mortgage disposition for loans that have matured or prepaid as of 2020.

Landlords who borrow multifamily mortgages report their financial statements to servicers, enabling me to construct a property-by-year panel of financial outcomes and to link property outcomes across mortgages. This panel includes financial outcomes on annual revenue, operating expenses, total debt service payments, and net cash flows.[13] The original Trepp databases are not structured to track individual properties across mortgages. I use the address and location information to build a panel including a subset of properties' financial outcomes across mortgages. This is a necessary step to study the effect of a refinance.[14] I can observe for most loans post-2007 how the initial mortgage is disposed (matures, prepays in open, prepays with penalties, or REO/foreclosure), and the purpose of the newly originated mortgage (refinance, recapitalization, acquisition, construction, etc.).[15]

This paper is among the first to link financial outcomes & market asking rents with the ability to look at these outcomes across mortgages for the same property. I merge address-level data from Trepp with data from REIS (by Moody's Analytics) to study asking rents, occupancy, and concessions for market-rate landlords. Information is collected from surveys of landlords (owners and managers), and the sample used in this analysis is from Q4 of each year. The address merge between Trepp and REIS includes 16 thousand properties. Only 9 thousand of those properties have panel information on financial outcomes and market rents for overlapping years. This provides a range of information about how landlords respond to changes in the cost of financing rental housing, and allows me to verify how changes in average revenue per unit are related to changes in market rents and free rent concessions for new leases (e.g. asking rents and effective rents). To the best of my knowledge this paper is the first to create a property-level panel simultaneously

[11]The panel of mortgage outcomes is at the year-level to match to the property-level financial statement data, which reports full-year financial outcomes. Multifamily mortgages can include multiple properties as collateral. In the relatively infrequent cases of multiple properties backing the same loan, I cluster standard errors at the loan level in regressions since this is the source of financing cost variation. Trepp's existing unique loan identifiers allow me to easily link mortgages to property identifiers within each loan term (but the Trepp-provided identifiers do not link across loans or refinances).

[12]Most Appendix B discusses several institutional details about appraisal and valuation in multifamily real estate, focusing on publicly available GSE guidelines.

[13]Borrowers also report occupancy rates to servicers, though they report occupancy rates less frequently, and without consistent periodicity (e.g. sometimes reporting in Q1, and other times reporting in Q3).

[14]My primary estimates use properties linked with a string matching function, but in robustness checks, I show comparable matching procedures using the batch Census geocoding API.

[15]For the across-mortgage analysis, I define sequential mortgages as cases where one mortgage prepays or matures, and the next mortgage has a recorded origination date within the 3 months around the prior mortgage's prepayment or maturity date.

observing market rents, financing, and financial outcomes.

As a brief description of the property financial data, Figure 1a shows the average values of revenue, operating expenses, and debt service per unit (divided by 12 to approximate the amount per month) for all observations in the Trepp database. The average property in 2020 earned \$1,290 in monthly revenue per unit, while spending \$620 in operating expenses (47% of revenue) and \$400 in debt service payments (31% of revenue).[16] On average, securitized large apartment buildings' have 22% operating profit margins before capital expenditures (or \$280 in net cash flow after mortgage payments).

I show the size of the sample of properties in securitized mortgages in Figure 1b, and note that the size and composition of securitized mortgage origination changed over time. New originations grow from 2,500 per year in the late 1990s to 15,000 per year in the late 2010s. Growth in securitization accelerated as the GSEs expanded their role in the multifamily market during and after the Great Recession.[17] According to the National Multifamily Housing Council, there are 21 million apartment rental units in the U.S., making up around 1 in 7 housing units in the country. Based on back-of-the-envelope calculations, about half of those apartment units (13 million units) are covered by the Trepp dataset at some point.[18]

To study the aggregate effects of financing cost shocks, I construct a city-level rent index using Census public-use micro-data (American Community Survey from IPUMS Ruggles et al. 2022). I use core-based statistical areas as a measure of city housing markets. I develop a hedonic rent index by recovering year-by-CBSA coefficients from a regression of log gross rents on several housing characteristics (number of bedrooms, type of structure, etc.) and a year-by-CBSA fixed effect.[19]

Appendix A discusses each of these datasets in greater length with additional details about merging and validating the proprietary data. I use several other supplementary public data sources including house price indices from the Federal Housing Finance Agency (Bogin et al. 2019) to proxy for changes in current LTVs, and inflation and labor market data from the Bureau of Labor Statistics to adjust for price changes and control for labor market conditions.

2.2 Stylized Facts

I present two new stylized facts about multifamily rental housing and financing costs. First, the debt service share of revenue has declined over the past 20 years. Second, operating margins for the average landlord have risen since 2003.

Figure 2 shows that between 2003 and 2020 the debt service share of revenue fell from 37% to 30% (a 19% decline; solid line in maroon x's). These figures use within-property variation to account for changes in sample composition. To do this, I regress operating margin and debt share on year fixed effects and property-specific fixed effects, and plot the constant plus the year fixed effect coefficients.

Over the same period, landlords' operating margins have risen (dashed line in green diamonds). Operating

[16] I benchmark this average amount relative to Census data. In Census' American Community Survey, the average gross rent in 2019 was \$1,164, so the average revenue per unit from Trepp securitized properties may represent disproportionately higher quality buildings, neighborhoods and cities than the national average. This is consistent with Black et al. (2020) observation that securitization disproportionately funds safer collateral in commercial real estate. The per unit revenue figure includes parking and other amenity fees. It does not represent exact average rents since there is no adjustment for vacancy losses or credit losses.

[17] I include Ginnie Mae in Figure 1b for completeness, but note that (a) very few FHA loans report complete mortgage characteristics (LTV and DSCR at securitization); (b) almost none report ongoing property-level financials; and (c) Ginnie Mae multifamily loans are for affordable and subsidized apartments, so they cannot be matched to the market-rate multifamily rent data from REIS. Thus, Ginnie Mae properties are excluded from the main analyses.

[18] The REIS dataset covers about 40-50% of total rental units in the 48 largest cities.

[19] The methodology and procedure for this rent index is similar to that used by Albouy et al. (2016) to study changes in housing demand and cost of living.

margins grew from 16% to 22% (a 37% increase). I benchmark to 2003 because it is just after the 2001 recession and several years before the Great Recession.[20] Operating margins are calculated as revenue minus operating expenses and debt service payments divided by revenues. This rise in operating margins is significant because it indicates that the share of rental income that landlords earn as profits has increased. The timing of the rise in operating margins suggests that declining debt service payments played an important role.

Interpreting these time series relationships is fraught because of the sources of endogeneity discussed in the introduction. Those issues include endogeneity due to (1) the relationship between interest rates and the property value, (2) changes in property quality and operations around financing cost shifts, and (3) granular geographic changes in neighborhood amenities and labor markets that affect both rents and financing costs.[21] These facts help motivate a causal identification strategy to study how within-property changes in landlords' financing costs affect tenants.

3 Pass-Through of Financing Costs at the End of Interest-Only Periods

To identify how landlords respond to a change in financing costs, I study the end of interest-only periods. As interest-only periods end, landlords debt service payments rise significantly. If landlords do respond to changes in financing costs, then this I-O setting should provide a useful natural experiment. This section proceeds in three parts: (1) discussing the empirical strategy and setting; (2) showing parallel trends for the treated versus control group then discussing event study and difference-in-difference results; and (3) using heterogeneity analyses to explore the role of cash flow constraints as I-O periods end.

3.1 Empirical Framework for I-O Mortgages

This section studies anticipated shifts in mortgage payments for properties with interest-only periods for only part of their mortgage term. This strategy exploits variation in mortgage payments provided by the rigidity of contracts in the years after origination. At origination, landlords can choose mortgage contracts where they only pay interest on the loan for a portion of the loan term. The most common I-O periods are for one year, five years, or the full loan term (usually 10 years). For identification, my treatment group includes mortgages with I-O periods for the first five years of a 10-year loan term. This empirical setting is particularly interesting because, in recent years, a growing share of multifamily mortgage originations use interest-only periods. As shown in Figure 3, 64% of multifamily mortgages originated in 2020 had interest-only payment periods, compared with 38% in 2005 and less than 5% in the mid-1990s.

The unique and useful part of analyzing variation in the size of mortgage payments around the end of I-O periods is that landlords' mortgage payment changes sharply, but there is no direct change in the valuation or appraisal of their building. These changes are known in advance, and the increase in mortgage payments goes toward paying down the principal balance on the mortgage. To the extent that landlords are planning ahead or smoothing their responses to changes in financing costs or mortgage payments, they should not change their behavior around the end of the I-O period. This empirical strategy tests how much landlords

[20] There was a decline in expenses as a share of revenue around the 2001 recession—I do not investigate that in this paper.

[21] Appendix A2a shows the binscatters of the cross-sectional and panel relationship between property-level asking rents and debt service payments.

are smoothing (or planning ahead for) changes in rents or operations, and whether changes in mortgage payments have real effects on their behavior.

To study the effect of an I-O period ending, I use a difference-in-differences research design comparing I-O properties to a set of non-I-O properties. I construct a control group of non-I-O mortgages that were originated in the same year with the same original term (primarily 10-year amortizing balloon loans). The control group is included to capture economic shocks to landlords or rental housing markets that may occur at the same time I-O periods end. The core difference-in-differences assumption is that without a shift in financing costs in the treated group, landlords' financial outcomes would evolve in parallel with the control group. I visually test the parallel trends assumption after discussing the estimation strategy. I use I-O periods that last 5 years, allowing me to compare 4 years prior and 2 years past the end of the I-O term. Event-time is defined as years relative to the end of the I-O term when payments reset. The relative year -2 is the omitted time period so the coefficients are interpretable as changes relative to the baseline year before the I-O end date.

To analyze the effect an I-O period ending, I run event study regressions of the form

$$y_{p(c)t} = \alpha_{rt(c)} + \alpha_{p(c)} + \sum_{r=-4}^{7} \beta_r \times D_p^c \times \mathbb{1}_{pt}(t = r) + \epsilon_{pt} \tag{1}$$

The regression takes property (p) by year (t) level data for I-O payment resets around cohort c (where I define cohorts by origination year and payment reset year). I define event-time r as years relative to the end of the I-O period. The binary variable D_p^c identifies properties backed by interest-only mortgages, and the indicator function $\mathbb{1}_{pt}(t = r)$ takes the value of 1 when the current-year observation occurs r years before or after the end of the I-O period. The main regression specification includes calendar year by event-time fixed effects indexed by cohort ($\alpha_{rt(c)}$), and property fixed effects ($\alpha_{p(c)}$). The coefficients of interest report the average change in outcomes around the end of the I-O payment reset year. I also run straightforward difference-in-difference regressions of the form

$$y_{p(c)t} = \alpha_{rt(c)} + \alpha_{p(c)} + \beta^{DD} \times D_p^c \times \mathbb{1}_{pt}(r > 0) + \epsilon_{pt} \tag{2}$$

The regression specification takes the average of the two post-periods, $r = 1, 2$ relative to the pre-periods $r = -2, -3$. This has the benefit of measuring the changes taking into account the average change in debt service payments over the course of the full year (whereas event time $r = 0$ only reflects the change for part of the year).

3.2 Event Study & Difference-in-Difference Evidence on Interest Only Periods

This section begins by showing several descriptive graphs testing the parallel trends assumption in the treated versus control group. I then show event study coefficients based on Equation 1, and difference-in-differences regression results. The reported results include six financial outcomes from the Trepp data: debt service payments, revenues, operating expenses, occupied units, net income (revenue minus expenses), and net cash flow (revenue minus expenses and debt service).

Figure 4 displays graphical evidence of the trends in property financials around the year of I-O payment reset. To make the outcomes more easily interpretable, I report financials in real monthly values (indexed to 2020 by CPI-U-RS). The x-axis shows years relative to the year of I-O payment resets—year 0 is the year in which the I-O mortgages begin paying principal.

The average values in the pre-period are informative about pre-existing differences in landlords who use I-O terms versus those who do not. Landlords with I-O terms pay \$8 per unit less in debt service monthly (Figure 4a). Average monthly revenue per unit grows over this time period from just over \$1,100 (4b). Properties with I-O periods earn \$8 per unit more in revenue. Before the payment reset, properties with I-O loans have slightly higher revenue per unit and lower expenses per unit, along with higher vacancy rates, on average.

In Figure 4a, we observe that mortgage payments increase from time -1 to 0. Landlords increase revenues at the same time (Figure 4b). Debt service payments gradually increase from time -1 to 1 because these graphs are measured as annual total debt service payments. Mortgage payments are paid monthly, so in the annual outcomes the principal payments are only paid for part of the year 0. This descriptive analysis suggests that landlords offset more than half of the increase in payments by increasing their revenues. Figure 4c provides limited evidence of a relative decline in operating expenses, while Figure 4d suggests there is a notable increase in occupancy starting in the year before the I-O period ends. This is consistent with landlords decreasing their asking rents, increasing their leasing efforts, or decreasing their standards for new tenants in order to increase cash flows as mortgage payments rise. Figure 4e shows that income net of expenses increases. Even though net cash flows decline due to the increase in mortgage payments, that decline is partly offset by the strong increase in revenues (4f).

Figure 5 shows the results of the stacked event study regressions comparing properties in I-O loans to non-IO loans. The event studies include 95% confidence intervals and point estimates. Total debt service payments increase by 21% (\$90) over the two years following the principal payment reset, while revenues increase by a statistically significant 3.5% (\$40). Over the same period, expenses fall by 1% in Figure 5c, though this is not significantly different from zero at the 95% confidence level. It appears that the increase in revenue is partly due to an increase in occupancy. The outcome variable in Figure 5d is log occupied units.[22] The change in occupied units in the year prior to the I-O period end is 1.2%, which is equivalent to 2-3 apartment units in the average building (see Appendix Figure C4). The combination of these effects results in an increase in net operating income of around 12% and a decline in net cash flows of 41% (accounting for the change in mortgage payments).

Table 1 presents the results of difference-in-difference regressions. The stacked data is balanced in event time, and includes clean cohorts of comparison groups to minimize issues with negative weighting when including fixed effects. I drop years -1 and 0 to maintain a clean 'pre' and 'post' reset period (since the mortgage payment changes during part of the year in period 0 and the descriptive graphs suggest a change in occupancy beginning in year $t = -1$).

Table 1 confirms that the \$90 increase in debt service payments & \$40 increase in revenues is statistically significant. Panel A reports the results in natural logs and inverse hyperbolic sine transformations, which can be interpreted as percent changes in each outcome. Panel B reports the same outcomes in real dollar terms (and in number of units for occupancy). Occupancy grows by 0.9 percentage points, or 2.9 units. Operating expenses decrease by \$10 per unit, leading to a change in net operating income before debt service payments of \$50 per unit. Net cash flows including mortgage payments fall by around \$40 per unit.

In terms of net income, these results provide evidence that at the end of interest-only periods landlords

[22]This variable transformation mitigates a problem with using vacancy rates which are frequently reported as zero (presenting a problem for log transformed regressions), or occupancy rates that can often be 100%.

pass through 55 cents of every dollar increase in debt service payments. Much of this increase is due to an increase in revenue as occupancy rises. The increases in revenue are sizable. The average property in the estimation sample earns $2.9 million annually in revenue, so a 3% increase represents an increase of $90 thousand in revenue per year. Landlords' ability to price discriminate between new and existing tenants makes it difficult to break down how much of the revenue change comes from more tenants occupying units versus tenants paying higher rents.

Because Table 1, which focuses on the Trepp dataset, does not include asking rents, I use a subsample of properties merged to data from REIS to study asking rents and concessions. Figure 6 shows that the I-O period end corresponds to a 1-2% decline in asking rents, and a decline in free rent concessions equivalent to about 1 month in free rent.[23] These declines are statistically significantly different from zero at the 95 percent confidence level. The decline in asking rents suggests that landlords decrease rents for new tenants after the I-O cash flow shock. The increase in revenues, which likely cannot be fully explained by the increase in occupancy, suggests that landlords raise revenue from existing tenants as well.[24] They could do this by raising renewal rents, however I cannot directly observe renewal rents in either of these datasets.[25] This effect is related to Matsa 2011, documenting that highly leveraged retailers degrade product quality to service their debt. If landlords were degrading quality, we would expect sharper declines in operating expenses. Instead, it appears that landlords have scope to increase cash flows by renting out more units, and potentially by raising renewal rents.

3.3 The Role of Liquidity Constraints in Landlords' Response to the End of Interest Only Periods

I show in this section that the strongest response to the end of I-O periods is among landlords' whose debt service coverage ratio (DSCR) is close to or below 1 before the I-O period ends. DSCR is an industry term used in multifamily lending. It is calculated as revenues minus operating expenses divided by total debt service payments. A DSCR below one indicates that annual net income is not high enough to pay annual mortgage payments, so landlords with low DSCR are more liquidity constrained. At origination, many lenders also price credit risk partly based on underwritten DSCR. For example, most agency securitization requires minimum DSCRs between 1.25 and 1.4.

I use heterogeneity in DSCR before the I-O period ends because that is exactly when landlords should anticipate a 20% increase in debt service payments. I use a mark-to-market DSCR, which is calculated based on actual revenues, expenses, and debt service payments 2 years before the I-O period ends. If liquidity constraints are salient, landlords should change their behavior when their DSCR is below 1.2.

In Figure 7, I show the change in log revenues from period $t = -2$ through period $t = 2$ for both the treated and control group with a binscatter of the data points for each and a local polynomial to provide an indication of the difference in group averages across the DSCR distribution.[26]

[23]Because these event studies are on a subsample of merged properties, we cannot directly compare the magnitudes to the results in Figure 5. I report the difference-in-differences results on the REIS-Trepp merged subsample in Online Appendix C1. Consistent with REIS dataset exclusively including market-rate buildings that are more likely to be institutionally owned and managed, the average effect of an I-O period end is smaller.

[24]Rising occupancy can explain about half of the $90 thousand increase in revenue. 2.9 units × $1,200 rent *times* 12 =$42 thousand.

[25]No publicly available rental housing datasets track a panel of asking and renewal rents simultaneously for the same units or buildings.

[26]Appendix Figure C5 shows a histogram of DSCRs in the treated and control groups. Prior to the I-O period end, DSCRs are higher for I-O loans, and I-O loans appear to have a thicker right tail, suggesting some higher quality properties or landlords with lower LTVs (and lower mortgage payments) took out partial-term I-O loans.

The binscatter plots illustrate that treated properties raise revenues and net incomes, consistent with the event study results. This revenue effect only appears to be positive for properties with low DSCR prior to the I-O period end. Figure 7 also provides some evidence on the shape of the liquidity constraint-related cost function—landlords with DSCR as high as 1.5 appear to increase net incomes. This suggests that landlords' response to liquidity constraints may not change discontinuously around a DSCR of one.[27] The effects on properties with DSCR above 1 could also be consistent with lenders pricing credit risk or rationing credit by setting minimum DSCR.

This section has focused on a particular type of financing cost shock: a change in cash flows due to the end of an interest-only period. The results provide evidence that changes in cash flows affect landlords' behavior. The I-O cash flow shocks exclusively increase mortgage payments. To complement the I-O analysis, I analyze decreases in mortgage payments due to rate resets on hybrid adjustable rate mortgages in Online Appendix D.1. The results of that section suggest that net incomes and occupancy decline temporarily in the year of an ARM reset. This is suggestive of a symmetric pass-through effect. The downside of the ARM analysis is that there are very few hybrid ARMs with rate resets that allow the construction of clean control groups with long 'pre' and 'post' treatment periods. In the next section, I analyze refinances, which can raise or lower debt service payments depending on changes in mortgage rates and landlords' leverage.

4 Effect of Financing Costs at Refinance

While the previous section focused on a cash flow channel, long-term changes in financing costs affect landlords when they refinance into new mortgage contracts. Refinancing into a new mortgage may be different than an I-O period because mortgage payments change and landlords extract equity from the property (or inject equity, in the case of cash-in refinance). This section proceeds in three parts: (1) discussing the empirical strategy and setting to study average effects of a refinance; (2) showing descriptive parallel trends then event study and difference-in-difference results for refinance and control group properties; (3) exploring heterogeneity in financing cost shocks focusing on property-level mortgage rate exposure & loan-to-value ratios; and (4) generalizing these results to include all refinances and sales observed in the securitized mortgage data.

4.1 Empirical Framework for Refinances

This section introduces an empirical strategy to analyze changes in property-level outcomes around the time a landlord refinances into a new mortgage. Because normally the timing and terms of refinancing are endogenous, I exploit variation in mortgage contract terms, called prepayment lock-out periods, to provide plausibly exogenous variation in refinance timing. Prepayment locks prevent or make it prohibitively costly to refinance during the lock-out period. Lock-out periods are very common, with 60% of loans in 2019-2020 including some lock-out, as shown in Figure 8.[28] The group of treated loans in the main analysis have

[27] For those properties with DSCR below one, the landlords are likely either borrowing subordinate liens to service their existing debt or using outside capital to pay the debt (or defaulting). The use of second liens or outside capital is not observable in these datasets, and I am not aware of any existing dataset that would capture these capital flows at the property-level.

[28] Other common mortgage characteristics also make early prepayment very costly. These include include prepayment penalties or premiums, defeasance (where a prepaid mortgage is swapped with a comparable asset like a Treasury security), and yield maintenance (which is similar to a prepayment penalty structured as a function of the present value of remaining interest payments).

lock-out periods lasting up until the final year of the mortgage term.[29] In the primary results, I focus on the dynamics of property-level financial outcomes around refinance for properties whose ownership and quality do not change, meaning I exclude property sales or renovations.[30]

The treated properties that undergo a refinance are compared using event study and difference-in-difference regressions with a control group of properties that do not refinance within the same event window. A cohort of treated properties, indexed by c, is defined as a group of treated loans with the same quarter of prior origination and quarter of new refinance origination, and with the same seven-year period of calendar years around the origination year of their refinance loan (the 'event window'). Each cohort of properties in the control group were originated five or six years after the treated group's original loans, so control properties are in the middle of their mortgage term when I compare them to the treated group. The control group properties are constructed into stacked comparison groups in calendar time to absorb common economic shocks that would affect property-level revenues, expenses, and occupancy. This construction allows me to use a balanced panel for each cohort of the control group outcomes over the same calendar years as each cohort of the treated group properties. The calendar years are re-centered around the origination year, which is defined in event time as $d = 0$.[31] I index property fixed effects by cohort, $\alpha_{p(c)}$.[32] For the stacked event study regression, cohort-specific event time by calendar year fixed effects are included so that treated properties are compared to control group properties within each time period and cohort. This mitigates the negative weighting problems with comparing already-treated to later-treated units in some difference-in-difference settings.

Difference-in-differences regressions perform best with balanced panels and clean control groups. The stacking approach described above creates a set of clean control groups, but the Trepp data does not always provide a balanced panel of financial outcomes. This is particularly problematic around the year of and year prior to originating a new mortgage. Before landlords begin paying and after they fully pay off a loan, landlords generally do not report financial statements to servicers. This means about half of landlords do not report their property-level financial outcomes for the first or last year of their mortgage, which are $d = -1, 0$ in event time.

To deal with this issue, I use a local projection-style event study to recover average treatment effects of refinancing, following the approach in Dube, Girardi, et al. (2022).

$$\Delta y_{p(c);t+k,t-3} = \alpha_{td(c)} + \sum_{d=-4}^{7} \beta_d \times \mathbb{1}_{p(c)t}(t = d) \times \mathbb{1}_{p(c)}(Orig) + \epsilon_{pt} \tag{3}$$

I regress outcome the change in the outcome variable $\Delta y_{p(c);t+k,t-3}$ relative to three years prior to refinancing on event study indicators around the date of the new mortgage's origination year. The indicator function $\mathbb{1}_{p(c)}(Orig)$ signifies a property is in the treated group which undergoes a refinance at $d = 0$, while $\mathbb{1}_{pt}(t = d)$ takes the value of 1 when the current-year observation occurs d years before or after the

[29] These loans have 10-year terms, and the most common prepayment lock periods are for 108 or 114 months. This means that landlords cannot refinance until the final 12 or 6 months of the 120 month loan term.

[30] In robustness checks, I show results including properties that were sold and renovated, and the relationship between financing costs and sale or renovation.

[31] I draw balanced comparison groups of properties with ten year loan terms & prepayment lock-outs that are in the middle of their term, stacking comparison cohorts that are balanced in property-level financial outcomes from the second to the eighth year of their loan term (or third to ninth year).

[32] In this stacking approach, multiple observations of the same control property can contribute to the different control group cohorts (clustering by property to account for this stacked control approach). It may present a selection problem to compare properties with loans securitized multiple times to those only securitized once in the Trepp data, but without detailed portfolio mortgage data, it is difficult to evaluate whether this is important—Black et al. (2020) suggest securitized mortgages are safer, but do not provide much context for selection for the same property into and out of securitization.

prepayment or maturity of the original mortgage. The main coefficients of interest are the event coefficients β_d, where event time $d = -3$ is the excluded period. The main event study results are measured relative to three years prior to a refinance.

In the difference-in-difference regressions, only balanced panel observations are included. I restrict to the periods before and after refinancing: $d = -4, -3$ and $d = 1, 2$. This simple difference-in-differences on stacked comparison groups allows straightforward interpretation of the event study results.

$$ln(y_{p(c)t}) = \alpha_{p(c)} + \alpha_{td(c)} + \beta_{DD} \times \mathbb{1}_{p(c)t}(d > 0) \times \mathbb{1}_{p(c)}(Orig) + \epsilon_{pt} \tag{4}$$

The coefficient of interest is β_{DD}. It measures the differential change in each outcome for properties originating a new loan, $\mathbb{1}_{p(c)}(Orig)$, in the periods post-refinance, $\mathbb{1}_{p(c)t}(d > 0)$. The exception in panel balance is the occupancy rate reported by landlords to servicers from the Trepp data, which is missing for a portion of properties. In robustness checks, I use reported occupancy to REIS, which is balanced in the merged panel to verify the results.

4.2 Effect of Refinancing on Landlords' Financial Outcomes

Similar to the I-O period setting, prepayment lock-out periods provide exogenous variation in the timing of a change in financing costs, and they are anticipated by landlords. If there were no liquidity constraints or other financing frictions, then landlords should not change their behavior around a refinance. In the presence of liquidity constraints, we might expect a change in behavior at or just before refinance for landlords who are nearly underwater or who are exposed to an increase in mortgage rates. Because of declining interest rates and rising property values over the past 20 years, those situations have been fairly rare.[33] There is another institutional detail that might lead landlords to change their behavior around a refinance. At refinance, the property is re-appraised and landlords can extract equity. Appraisal guidelines require landlords to provide the current rolls of active leases in the property, and financial statements for the three years leading up to appraisal. This limited information set could have an influence on the property valuation.[34] To the extent that landlords can raise revenue, it should be in the two years leading up to a re-appraisal. I examine 4 years prior to refinance to observe anticipatory effects.

In the Figure 9, I graph the main outcome variables from four years prior to a refinance to two years following a refinance for a loan with a prepayment lock during its prior mortgage term. Properties with prepayment locks are lower quality on average. This should not be surprising given that most prepayment lock-out contracts are securitized by Fannie Mae mortgages (which are tilted towards more affordable properties) rather than privately-securitized mortgages (which are more likely to be high quality or market-rate apartments).

The x-axis shows years relative to the year of refinance of the original mortgage—year 0 is the year in which the landlord refinances their mortgage. Debt service payments decline sharply for the refinance group labeled as "Lock→Refi" by \$45 per unit, consistent with declining mortgage rates over this time period. The figure shows that average monthly revenue per unit grows over this time period from \$960 to \$1,080, while revenues are growing in the control group at a slower rate. The graphs provide suggestive evidence of differential revenue in the two years leading up to and year of refinancing. There are similar trends in net income and net cash flows.

[33]See Appendix A3 on mortgage rates, and Appendix H23 on the distribution of loan-to-value ratios just before refinance.
[34]Institutional details about appraisals are included in Online Appendix B. Regressions studying within-property change in appraisal values to test this channel are included in Appendix G.

There is a notable increase in occupancy rates over the 3 years prior to a new refinance. Considering the sample restriction to exclude sold and renovated properties, this seems like clear evidence of a sharp change in property-level outcomes that result from landlord behavior. The changes occur in advance of refinancing into a new mortgage. The scale of these effects suggests that the average property which has 200 units increases occupancy by 2%, meaning an additional 4 units are leased.

In Figure 10, I report the results of the local projection event study regressions. Debt service payments decline by 11 percent in the 2 years after an average refinance over this time period (Figure 10a). The initial year of refinance is slightly lower at -0.19 likely due in part to the missing data issue discussed above. Figure 10b shows that revenues increase by nearly 4% in the years following a new origination. This increase may begin two years prior to the new origination, though the pre-refinance coefficients are not significantly different from zero. The timing of this increase is consistent with landlords' ability to anticipate the end of their prepayment lock period. The increase is particularly sharp in the year before and year after the new refinance.

Figure 10c shows that operating expenses appear to decline after a refinance, though the change is not statistically significant. This suggests that revenues are not primarily increasing due to an increase in the quality of landlords' operations. There is a sharp increase in occupancy beginning two years prior to refinance (Figure 10d). 2-3% additional units are occupied after the refinance, which is 4 to 6 apartment units in the average property.

Corresponding to the increase in occupancy, net incomes increase in the two years prior to refinance, resulting in net incomes that are 10% higher two years after refinance (Figure 10e). Net cash flows increase by the equivalent of 120 log points, though this is slightly more difficult to interpret because I use the inverse hyperbolic sine transformation to deal with net cash flows that are at or below zero in some periods. In the difference-in-differences specification below, I also report these results in dollar terms.

Table 2 shows the stacked difference-in-differences results for landlords with lock-out periods who refinance. Panel A shows the results where outcomes are transformed in natural logs and inverse hyperbolic sines. This is consistent with the event study results—debt service payments decline by 12.5%, revenues rise by 2.9%, and occupancy increases by 2.2%. The increase in net operating income is $22.5 per unit monthly. An alternative way to interpret the IHS results is to take the effects on net incomes and net cash in dollar levels as percentages of the pre-period treated group averages from the descriptive trends above. Net operating income increases by 4%, and net cash flows increase by 40%.[35] If there were no change in landlord behavior around refinancing, the significant $37 decline in debt service payments would correspond to a $37 increase in net cash flows. Instead net cash flows increase by $60 per unit monthly. For a 200 unit apartment property, this means that the landlords' annual net cash flows increase by $140 thousand (instead of the $90 thousand increase due to the change in debt service).

This evidence on refinanced properties suggests that landlords make real changes to their behavior around refinancing. They raise revenues by 3% even though their debt service payments decline by 12%, on average.

There is some evidence of landlords' anticipating their refinance event by increasing occupancy and net income in the two years leading up to a refinance. The timing of this effect matches the standard financial statement reporting to appraisers and underwriters.

[35] 4% = ($22 / $515) × 100. 40% = ($60 / $150) × 100.

4.3 Heterogeneity in Refinancing Effects by Leverage and Rates

In this section, I provide additional evidence that the increase in revenues and occupancy is caused by a change in financing costs. I examine differences in the magnitude of the revenue response at refinancing with respect to two sources of financing cost variation. If landlords are cash-flow sensitive, then landlords' revenue change at refinancing should be larger in response to higher mortgage rates. An increase in mortgage rates raises the landlord's cost of debt and the size of their mortgage payment. Similarly, if landlords are at risk of being underwater and being forced to 'cash-in' at refinance to pay off their balloon payment, then they should be more aggressive in raising revenue. Increasing revenue at refinance may help raise the appraised value of the property. This could have a discontinuous effect for landlords who are at risk of being underwater. Because lenders price property-level credit risk, raising the appraisal value could also lower the cost of debt by lowering the landlord's LTV.

To test for heterogeneity, I interact the two sources of financing cost variation with the difference-in-differences treatment variable in Equation 4. The interaction effects are linear, so I impose the assumption of symmetric effects of higher versus lower financing costs through the rate exposure and LTV variation. In robustness checks, I relax this linearity assumption.

I use two sources of financing cost variation: mortgage rate exposure and mark-to-market leverage before refinance. The first variable is the property-level exposure to changes in mortgage rates since the property's prior origination date. This variable is defined as the difference between mortgage rates between the quarter the original mortgage was originated and the quarter it is prepaid or matures (i.e. the quarter of the new mortgage refinance origination).[36] The second financing cost variable is the mark-to-market loan-to-value ratio two years prior to the new refinance. The mark-to-market LTV is constructed: (1) using the remaining loan balance at the beginning of the year; and (2) by merging a zip code by year-level FHFA house price index (based on the universe of single-family home sales). I project the appraised value of the property from the prior origination year to two years before the year of refinance. Property-level differences in LTV can come from at least two sources. First, landlords' existing loans may have differential rates of amortization (e.g. through interest-only periods or different original loan term). Second, the location of the property may experience changes in value. The use of the FHFA house price index is useful because it provides a meaningful proxy for the value of the location without using any information from the apartment buildings in this data.

The results are reported in Table 3 for the regressions with the outcomes log and inverse hyperbolic sine transformed.[37] Panel A shows results using the specification for the difference-in-differences above. The only difference in the sample from Table 2 is that properties that are not matched to zip codes with house price indices are excluded. The results are quantitatively similar—debt service payments decline by 13% and revenues rise by 3%.

The first column of Panel B shows the effect of mortgage rate exposure and mark-to-market LTV on the change in debt service payments. One percentage point higher mortgage rates (100 basis points) cause 10.1% larger debt service payments. Ten percentage points higher existing LTV (e.g. moving from 70 to 80% LTV) results in 3.4% lower debt service payments on the refinanced mortgage. Higher LTV landlords deleverage at refinance. The debt service payment outcome includes all-in debt service payments. This implies that the financing cost measures can be endogenously related to landlords' borrowing decisions—landlords' exposed

[36] I create a multifamily mortgage rate index, shown in Appendix Figure A3. I construct rate exposure for the control group based on the stacked comparison approach, where each cohort of the control group is drawn by assigning same prior origination year and new origination year corresponding to each cohort of treated loans.

[37] The effects in terms of dollars and levels are reported in Appendix Table C2.

to lower rates may choose longer duration loans, and those with higher leverage may choose smaller loan balances. The fact that each variable significantly affects debt service payments suggests that these measures of financing costs are meaningful proxies for the property-level cost of debt.

The second column shows that the effect of refinancing is heterogeneous with respect to mortgage rate exposure—one percentage point higher mortgage rates results in landlords raising revenues by 2.2% and 10 percentage points higher LTV results in landlords raising revenues by 0.57%. There is little evidence that changes in operating expenses are strongly related to changes in financing costs. Occupancy rates appear to be strongly related to leverage. Properties with higher existing LTVs raise occupancy at refinance. These results are evidence that financing costs are correlated with landlords' response at refinancing.

Are landlords' solely changing their behavior at refinance because of discontinuous changes in financing costs (or liquidity constraints)? I test this in Panel C by relaxing the linearity assumption on the rate exposure and LTV interactions. The difference-in-difference coefficient is interacted with an indicator variable for properties exposed to an increase in mortgage rates (about 10% of properties in this sample period), and an indicator variable for having an LTV greater than 80% in the years prior to refinance (15% of the sample). The cut-off for LTVs is potentially meaningful given that securitized multifamily mortgages are rarely originated with LTVs above 80% and the average LTV at origination is between 65 and 75%. Landlords exposed to higher mortgage rates experience an increase in debt service payments of 45% relative to the average property exposed to lower rates. Landlords with high leverage refinance into loans with 10% lower mortgage payments, suggesting that they endogenously respond to being underwater or having to cash-in by reducing the size of the loan balance in their new refinance mortgage.

Properties exposed to both higher mortgage rates and higher leverage at refinance raise revenues. However, properties with declining rates and lower LTVs also increase revenues, on average. The revenue increase is 7.4% for properties exposed to higher mortgage rates and 2.7% for properties with higher leverage, though only the effect for properties with high leverage is statistically significantly different from zero at the 95 percent confidence level (Column 2, Panel C). The group of properties with lower leverage and declining mortgage rates still significantly increase revenues (2.2%) and occupancy (2.0%).

This last set results in Table 3 are particularly informative because they suggest that discrete liquidity constraints are not the sole reason that landlords change their behavior at refinance.

4.4 Effect of Originating a New Mortgage Comparing Refinances and Sales, Controlling for Renovations

Next, I analyze all matched refinance and sold properties from the Trepp securitization data, including properties without prepayment lock periods. This analysis is useful for two reasons. First, it provides external validity, confirming that the results for locked refinances are not due to selection in what types of landlords' choose prepayment locks. Second, this provides a larger sample of properties to study, increasing the treated group from 425 to just over 858.

Table 4 shows these results controlling for renovations and separately estimating the average effect of originating a new mortgage through refinance versus sale. The average renovation results in higher net income, but controlling for renovations does not change the main effect of refinancing.[38] While renovations may be relevant, the main refinancing effect is distinct. This differs from evidence on corporate investment

[38]The fact that the average refinance results very similar controlling for sales and renovations is reassuring. We may still be interested in how financing cost shifts affect these other margins of landlord behavior. Notably, financing cost shocks could affect the probability of renovation, sale, or default. In Online Appendix C.1, I show that financing cost shocks do act through those channels, but I find no indication that those channels explain the refinancing effect documented in Section 4.

among firms (Almeida, Campello, Laranjeira, et al. 2012), and from owner-occupied refinancing, which can be used for debt repayment (Di Maggio, Kermani, Keys, et al. 2017), home improvement or other spending (see e.g. Melzer 2017 or Greenspan and Kennedy 2008).

The indicator for 'Sale' allows for a separate average effect on properties whose new mortgage is an acquisition. It is striking that the effects on debt service, revenues, occupancy, and cash flows are so similar to the main refinance effect when there is a change in ownership. These results suggest that the main lock-out refinancing results are not due to selection of low quality landlords into prepayment locks. Instead, there appears to be behavioral change, or a change in strategy that leads to similar growth in net income even when the property changes ownership. This common effect on refinances and sales helps motivate my discussion in the next section of information costs and belief updating that lead landlords to update pricing and leasing.[39]

To complement the results on financial outcomes in Trepp, I examine the average effects of refinancing on asking rents and free rent concessions using the subsample of properties merged between Trepp and REIS.[40] Asking rents dip in the year prior to refinancing or sale, then increase by 2.5 percent in the next two years. Free rent concessions (measured as number of months of free rent) rise by about 0.1 months of free rent in the year following a refinance or sale, but the change is not statistically significant after two years. The fact that asking rents and occupancy rates both increase suggests that asking rents (or equivalent posted rents online) might not describe the dynamics of rents paid by existing tenants at renewal. It is possible that landlords increase asking rents and occupancy at the same time through higher leasing effort or through relatively lowering renewal rents.

Contrasting with the focus on corporate investment in studying firm-level financial constraints (e.g. in Almeida, Campello, Laranjeira, et al. 2012), landlords are able to increase revenues with little or no evidence of substantial changes in product quality, investment, or cost of sales (e.g. employment in Benmelech et al. 2021). The change in asking rents and occupancy suggests that there is an important information or belief updating channel, where landlords have some scope to re-optimize their pricing and leasing. The existence of this channel is consistent with the use of stale information or reference dependence in other housing market contexts (e.g. Giacoletti and Parsons 2022 or Genesove and Mayer 2001).

5 Conceptual Framework

The goal of this conceptual framework is to provide intuition about how landlords price apartment units, and to explain the results in both of the empirical strategies. This framework starts with a discussion of how landlords make decisions on rental pricing and leasing effort, which determines their net operating incomes (revenues net of expenses). In the data, I observe this annually. To model landlord decisions, I assume that landlords are each a single entity and they earn profits in two ways: through net cash flows on annual operations, and through equity distributions when they refinance or sell.[41]

First, I introduce landlords' net income, $\pi(R_{ht}, m_{ht})$, and their pricing decision in the case where they have no debt financing. Landlords solve a profit maximization problem, where they earn revenue which

[39] I include heterogeneity results looking at rate exposure and LTVs for all refinances and sales in the Online Appendix. Those results are qualitatively and quantitatively similar to the lock-out refinance sample.

[40] These results are based on a balanced panel of private sector data (unlike the CMBS data from Trepp, which has some missing observations in period $t = -1, 0$. I use the local projection event study approach and report the average financial outcomes for the same merged Trepp-REIS subsample in Appendix Figure C11.

[41] The legal structure of apartment ownership arrangements can vary. Commonly multiple partners may contribute financial capital to purchase a property, and profits on the property may pay out based on varying investment pay-out structures. Importantly for this conceptual framework, one entity is responsible for operating or managing the property.

can be calculated by multiplying rent, R_{ht}, by the number of occupied units n_{ht}. The firm cannot change the number of units in the property and occupancy rates are bounded above by the number of units in the building N_h. The number of occupied units is a function of leasing effort m_{ht} and posted rents R_{ht}, so we can write occupancy as $n(R_{ht}, m_{ht})$. Landlords have some cost of operating the building $C(R_{ht}, m_{ht}, N_h)$. If there were no financing costs, landlords would set rents and leasing effort based on $\pi(R_{ht}, m_{ht})$.[42] I have modeled rental housing as a spot market where rents and occupancy from period t does not carry over to period $t+1$. This simplifying assumption can be relaxed without changing the main economic forces in the empirical results.

$$\max_{R_{ht}, m_{ht}} \pi_{ht}(R_{ht}, m_{ht}) = R_h n(R_{ht}, m_{ht}) - C(R_{ht}, m_{ht}, N_h)$$

$$n(R_{ht}, m_{ht}) \leq N_h$$

To study financing costs, I model landlords as financing their apartment properties with debt, paying some per-period mortgage payment with a schedule maturity date $M_{ht}...M_{ht+k}$. The mortgage payments are determined by interest rates, terms, and loan balances from prior periods. Landlords decide whether to refinance their mortgage in each period, a decision represented by ρ_{ht}. The refinance decision is a difficult real options problem, so for this stylized framework, I consider the case where the timing of refinancing is fixed analogous to mortgages with prepayment locks.[43] Consider the refinance decision as dependent on the discounted value of the change in mortgage payments, the available equity, and the monetary costs of refinancing.[44] If they refinance, landlords extract equity E_{ht}, a function of the value of the property, remaining loan balance and fixed closing and refinancing costs. Assume that the equity available for extraction is based on a loan-to-value ratio which is a fixed portion of the property value. Fixing LTV avoids adding another endogenous variable without changing the main point of the conceptual framework. Finally, this problem is dynamic so that landlords consider their discounted next-period value, a function of mortgage payments and equity entering into next period. This problem can be characterized as

$$\max_{R_{ht}, m_{ht}, \rho} V_{ht} = \begin{cases} \pi_{ht}(R_{ht}, m_{ht}) - M_{ht} + \beta V_{ht+1}(M_{ht}, E_{ht}) & \text{if } \rho = 0 \\ \pi_{ht}(R_{ht}, m_{ht}) - M_{ht}^{\rho} + E_{ht} + \beta V_{ht+1}(M_{ht}^{\rho}, E_{ht}^{\rho}) & \text{if } \rho = 1 \end{cases}$$

$$n(R_{ht}, m_{ht}) \leq N_h$$

For explication, suppose there are two versions of the world, one where net incomes affect financing costs and one where they do not. In both versions, landlords roll over their debt every period. In the first version, rental pricing and leasing effort are separate from financing costs. Interest rates are common for all landlords, so banks do not price in credit risk, and property values are determined by the optimal pricing and leasing based on the market, so each landlords' pricing decision has no bearing on their equity extraction.

$$d\pi_R = dR_h n(R_{ht}, m_{ht}) + R_h dn_R - dC_n dn_R = 0$$

[42] For completeness, there is a participation constraint that landlords' income must cover their fixed costs or they will abandon the property.

[43] Agarwal, Driscoll, et al. (2013) have a closed form solution to an optimal refinancing problem, though commercial mortgages may be even more difficult given the exotic terms available in the mortgage market.

[44] A richer model might provide a larger role for expectations as in Agarwal, Driscoll, et al. (2013), and might also examine behavioral refinancing decisions as in Andersen, Campbell, et al. (2020). Because the refinancing decision itself is not the main subject of the paper, I simplify the decision here.

In this case, mortgages and financing costs do not enter into rental price-setting on the margin.

In contrast, we can consider a world where landlords roll over debt every period, and rents affect mortgage payments and equity because banks price in credit risk and current rents affect extractable equity.[45]

$$d\pi_R - dM_R + dE_R$$

This world should feature rental prices where, depending on the parameterization, mortgage costs could fully pass through to rental prices. This pass-through may not be one-to-one given that any financing cost change depends on the net effect of a financing cost change on mortgage payments and equity extraction. Even in this world, where rents have a marginal effect on financing costs, it is simple to write down a model where financing costs should not affect pricing behavior—for example, if rents enter into mortgages and equity only through their relationship with current net incomes, then $d\pi_R - dM_\pi d\pi_R + dE_\pi d\pi_R = 0 \equiv d\pi_R = 0$. In words, if rents only affect financing costs through their effect on net operating incomes, then optimal rental prices should not differ from a version of the world with no debt financing.

The first test of this paper is whether these simplified models of rental pricing are sufficient approximations of how financing cost shocks affect rental outcomes. The interest-only period strategy provides compelling evidence that shocks to cash flows coming through the mortgage payment affects landlords operations.

5.1 Partial Information and Pricing Policies

In this section, I explain one mechanism by which landlords change their behavior around a discrete change in their financing costs. The above section sketches out a stylized model where landlords have monopolistically competitive pricing power—they know the elasticity of occupancy with respect to rental prices, $\frac{dn}{dR}$. Here, I consider an addition to the model where landlords only observe a signal of this elasticity, and it is costly to update their information about the price elasticity of demand they face in any period.

Suppose there exists a true price elasticity of demand ϕ_{ht}, which allows landlords to optimally price rents and choose leasing effort $R^*_{ht}, m^*_{ht} = argmax\ V_{ht}(R_{ht}, m_{ht}; \phi_{ht})$. In each period, they receive a noisy signal $\nu_{ht} = \nu_{ht-1} + \iota_{ht},\ \iota \sim \mathcal{N}(0, 1)$. The landlord can either optimize based on the noisy signal or pay an information costs δ to observe the true demand responsiveness ϕ_{ht}. This description of landlord signals generates reference dependence, similar to characteristics of cross-sectional rental pricing in Giacoletti and Parsons (2022). There is substantial evidence of reference dependence in the single-family housing market (e.g. Andersen, Badarinza, et al. 2022; Genesove and Mayer 2001; and more widely among investors: Barberis and Xiong 2012).

Landlords will pay information costs to update their pricing & leasing if the value of updating their policies exceeds the cost: $V^*_{ht} - V^\nu_{ht} > \delta$. The existence of these discrete information costs suggests that: (a) there should be sharp changes in rents for individual buildings over time; (b) there should exist rental units that are both over- and under-priced; and (c) in periods where landlords do not pay information costs, they have some scope to increase revenues and net incomes. Similar firm price-setting behavior is consistent with the existence of discrete pricing policies (Stevens 2020; Matějka 2016), or rational inattention in pricing (Reis 2006; Ball et al. 2005).

[45] Online Appendix G includes evidence on the effect of within-property changes in asking rents and revenues on property values, LTVs, and mortgage rates.

5.2 Landlords with Liquidity Constraints

This subsection focuses on the intuition for how information costs and liquidity constraints interact. Consider a distortion in the ability of landlords to smooth profits by borrowing across negative cash flow periods. This friction can represent three real world costs. Borrowing has fixed costs, so going cash flow-negative in one period can result in taking on additional debt which landlords have to pay for when they originate the debt and may have to pay higher interest rates for a subordinate lien or shorter-term debt. Negative cash flows could induce mortgage default which could result in monetary penalties or costs that are reputational or raise future cost of debt. These may be psychic costs or other behavioral frictions that are common in the study of owner-occupied mortgage borrowing in the spirit of Andersen, Campbell, et al. (2020). Negative cash flows could also result in penalties for asset or property managers because of the structure of compensation for managers (i.e. general partners) or payouts to investors (like limited partners).

I model this friction as a function of the property's current debt coverage ratio $\gamma(\frac{\pi_{ht}+\rho_{ht}E_{ht}}{M_{ht}})$, which is declining in its argument. This function is consistent with liquidity costs that are higher as mortgage payments grow larger than net incomes, $\gamma' < 0$. It enters into the landlord value function

$$\underset{R_{ht},m_{ht},\rho}{max} V_{ht} = \pi_{ht}(R_{ht}, m_{ht}) - M_{ht} - \gamma(\frac{\pi_{ht}(R_{ht}, m_{ht}) + \rho_{ht}E_{ht}}{M_{ht}}) + \rho_{ht}E_{ht} + \beta V_{ht+1}$$

In the case where $\rho_{ht} = 0$ under partial information, landlords will pay the cost of updating their information and pricing strategy if $\pi^*_{ht} - \pi^{\nu}_{ht} > \delta + (\gamma(\frac{\pi^*_{ht}(R_{ht},m_{ht})}{M_{ht}}) - \gamma(\frac{\pi^{\nu}_{ht}(R_{ht},m_{ht})}{M_{ht}})$. Because by definition the full information net operating income is larger than the noisy signal income, $\pi^*_{ht} > \pi^{\nu}_{ht}$, the presence of liquidity constraints will lower the threshold for paying the information costs: $\gamma(\frac{\pi^*_{ht}(R_{ht},m_{ht})}{M_{ht}}) - \gamma(\frac{\pi^{\nu}_{ht}(R_{ht},m_{ht})}{M_{ht}} < 0$.

Because the interest-only empirical analysis solely changes the cash flows of landlords, we can use it as a setting to test whether liquidity constraints bind, and how landlords change their behavior. Figure 7 shows that landlords only increase revenue when their debt coverage ratio is low and approaches one. However, the shape of liquidity-related costs, $\gamma(\cdot)$, is an empirical question. My results suggest that it may not be discontinuous at one, as Giacoletti and Parsons (2022) assume in a conceptual model of single family rental pricing. Landlords may respond to the risk of hitting their liquidity constraint, or to lenders pricing credit risk based on recent income history.

5.3 Valuation & Appraisal Frictions

The fact that landlords only realize increases in the valuation of the property at discrete equity events will affect their pricing and leasing behavior under the assumption that there are substantial information costs. This section explores this friction. I mention a related friction before elaborating: lenders may not observe the landlords' full optimization problem, so they rely on imperfect appraisals of property valuation to make lending decisions. Under this conceptual model of valuation, an increase in revenues can increase the property's value at appraisal.

Consider the information cost decision in the time period where landlords are required to refinance, $\rho_{ht} = 1$. The landlord decision now shifts mortgage payments and equity extraction, and locks those changes in through the term of the next loan. To simplify the expression, we can write

$$(\pi^*_{ht} - \pi^{\nu}_{ht}) + (E^*_{ht} - E^{\nu}_{ht}) - (M^*_{ht} - M^{\nu}_{ht}) > \delta + (\gamma(\frac{\pi^*_{ht}(R_{ht}, m_{ht}) + \rho_{ht}E^*_{ht}}{M^*_{ht}}) - \gamma(\frac{\pi^{\nu}_{ht}(R_{ht}, m_{ht}) + \rho_{ht}E^{\nu}_{ht}}{M^{\nu}_{ht}}))$$

The threshold for updating landlords' information and behavior shifts in three ways. The first is unambiguous: an increase in net income should increase the valuation of the property. This allows the landlord to extract more equity today, which raises the probability of paying the information cost. The second term implies that updating the landlords net income should result in a relatively higher mortgage payment if the loan balance is also higher. In a more complex version of the model, we can account for the change in mortgage payments as the present discounted value of the change in mortgage payments and present value of waiting to extract any equity that is left in the property at refinance (continuing to assume the prepayment lock allows us to ignore the option value of refinancing). The third effect is ambiguous in direction. The simultaneous change in net income, equity and mortgage payments affects the liquidity cost function γ. The equity extracted enters into the liquidity cost function to capture the fact that it is easier for landlords to smooth income when they can extract equity. The reverse argument can be made that if landlords have negative equity or are close to being underwater, they may have to pay much higher liquidity costs if they need to cash-in.

The earlier empirical results are consistent with these forces. Around refinance, landlords increase their revenue and occupancy when their LTVs are high (especially above 80%) and they risk being underwater or being forced to put cash back into the property (Table 3). Landlords revenue response is also related to their mortgage rate exposure. High liquidity costs that affect pricing strategy is consistent with van Straelen (2021), which studies internal firm capital markets (focusing on home developers and builders). In the next section, I develop an empirical proxy for mispricing ($R^*_{ht} - R^\nu_{ht}$) and leasing effort ($m^*_{ht} - m^\nu_{ht}$). The measures are based on each building's difference in asking rent and vacancy relative to similar buildings two years before each financing shock.

6 Mechanisms of Landlords' Response to Financing Cost Shocks

The two main empirical strategies show that landlords increase revenues around financing cost shocks. The conceptual framework provides the basis for testing for discrete updates to pricing and leasing.[46] This section uses the financing cost shocks to show a sharp response in landlords price-setting and leasing behavior: they correct mispricing and update leasing effort.

6.1 Pricing and Leasing Response to the End of Interest-Only Periods

Using the proprietary asking rent and vacancy data on the universe of market-rate multifamily properties, I construct a property-level measure of mis-pricing and differential vacancy outcomes compared to other properties in their geographic submarket in each year. The measure comes from regressing log asking rent per square foot and vacancy rate on submarket-by-year fixed effects and 22 property amenities and characteristics, then extracting the residuals from the regression and z-scoring the residuals. As above studying DSCR, I take the mispricing and differential vacancy measure from two years prior to the end of the I-O period for treated and control properties and compare the long difference in outcomes between the two groups.[47]

[46] To corroborate the existence of price dispersion or possibility of behavioral pricing in rental housing markets, Appendix E presents evidence of rental price and vacancy dispersion and evidence of round-number bunching in asking rents.

[47] The distributions of the ex ante mispricing and differential vacancy measures are similar for the treated and control groups (Appendix Figure C6 & C7). The treated mortgages have relatively higher vacancy differentials and appear to be slightly over-priced on average.

Figure 12 shows that treated properties update asking rents relative to the control group. Underpriced treated properties increase asking rents more than underpriced control properties, while overpriced treated properties decrease asking rents relative to control properties.

The final point from Figure 12 is that landlords updating their pricing and leasing strategy should be able to raise net incomes from both underpriced and overpriced properties—and should be able to raise income less from appropriately-priced properties. The figure shows suggestive evidence of a U-shaped pattern in the relative increase in revenues after I-O periods end where revenues appear to increase for the treated relative to control group for both over- and under-priced properties with little change for properties that are appropriately priced based on my mispricing measure.

Properties that have differentially higher vacancy rates prior to the I-O period end are able to raise occupancy rates more, as shown in Figure 13. However, occupancy rates rise across most of the vacancy distribution, suggesting that vacant units may have a higher opportunity cost after a negative cash flow shock leading landlords to increase their leasing effort. These results are compatible with the price reductions to raise cash for home builders exposed to negative house price shocks in van Straelen (2021).

6.2 Pricing and Leasing Response to Refinance or Sale

Figure 14 presents the core piece of evidence on mispricing around refinance and sale. Landlords update asking rents in a way that is strongly related to their mispricing before a refinance or sale.[48]

Refinanced and sold properties correct their pricing at approximately twice the rate of the comparison group. On average, properties in the comparison group that are 1 standard deviation overpriced increase asking rents 2.8% less over the next 3 years. The slope of the relationship for properties undergoing a refinance or sale is 5.6% for refinances and 5.9% for sales.

On average, asking rents are rise faster for both refinances and sales. This increase relative to the control group is in contrast with the results on I-O periods where asking rents fell on average. Appraisers could use asking rents either in choosing comparable properties or calculating potential gross revenues for the income approach to appraisal. I present some evidence for this channel in Appendix G.

Consistent with these pricing corrections leading to higher landlord income, I show the relationship between the mispricing measure and revenue growth over the same time periods. There are two notable insights from this graph. First, the slope of the mispricing-revenue growth relationship is attentuated, suggesting that asking rents do not pass through to revenues one-for-one. The resulting asking rent-to-revenue elasticities are between 0.23 and 0.27 (a 1 percent increase in asking rent leads to a 0.23-0.27 percent increase in revenue). The second observation is that revenues grow across most of the mispricing distribution. Revenue growth is visually higher for overpriced and underpriced properties.

Next, Figure 15 shows the vacancy differential measure, which captures landlord decisions about leasing effort in the two years before a refinance or sale. Figure 15 shows the relationship between the growth in log occupied units and the vacancy differential measure. I plot OLS regressions for each of the three groups (Comparison, Refinance, Sales). The relationships are very similar, but the slopes for refinance and sold properties are steeper (0.036 and 0.42 compared to 0.34). This indicates that landlords more quickly correct poor leasing outcomes around an equity event like refinance or sale. In the second graph, I show revenue growth over these 4 years. In the comparison group, the slope is positive indicating that 1 standard deviation higher vacancy differential leads landlords to increase revenue by 0.8% in the following years. The

[48] Appendix Figure C8 & C9 show histograms of the mispricing and vacancy differential measures for 'treated' properties and control properties.

slope for refinance mortgages is statistically significant and suggests a 4.5% revenue increase from 1 standard deviation higher vacancy differential. This relationship is attenuated for sold properties. This may be due to new landlords making improvements to the property, changing their operations, or being more aggressive about raising asking rents across the vacancy differential distribution.

The steepness of the slope of the vacancy differential and mispricing measures provides evidence of landlords updating their behavior. To provide evidence on the timing of landlords behavior updating, I show how the slope of the asking rent-mispricing and occupancy-vacancy differential relationships change in the years around the new mortgage origination. To do this, I take differences in log asking rents and occupied units relative to two years before the new origination (the same year I take the mispricing and vacancy differential measures from). I include event by year by cohort fixed effects, leads and lags to capture the effect of renovations, event study treatment coefficients capturing average changes for the refinanced and sold properties, and the average slope in each period of the mispricing measure for all of the treated and comparison properties. The coefficients of interest, $\beta_d^{treatXmis}$, track the differential change in the slope of the mispricing measure with growth in asking rents (and the vacancy differential measure with growth in occupancy).

$$\begin{aligned}\Delta y_{p(c);t+k,t-2} =& \alpha_{td(c)} + \alpha_r^{renov} X_{p(c)t} \\ &+ \sum_{d=-3}^{6} \beta_d^{treat} \times \mathbb{1}_{p(c)t}(t=d) \times \mathbb{1}_{p(c)}(Orig) \\ &+ \sum_{d=-3}^{6} \beta_d^{mis} \times \mathbb{1}_{p(c)t}(t=d) \times mis_{p(c)t} \\ &+ \sum_{d=-3}^{6} \beta_d^{treatXmis} \times \mathbb{1}_{p(c)t}(t=d) \times \times mis_{p(c)t} \mathbb{1}_{p(c)}(Orig) + \epsilon_{pt} \end{aligned} \tag{5}$$

The results are reported in Figure 16. The top panel shows the mispricing correction for refinance and sale properties relative to the comparison group. In the year a new mortgage is originated, landlords on average correct their pricing more sharply—increasing asking rents an additional 1.8% for 1 standard deviation more underpriced properties (and reducing asking rents on overpriced properties). There is no significant slope difference between the treated properties and control group in the years prior to the new mortgage origination. In the two years following the new origination, the relationship differential attenuates, suggesting that properties in the comparison group slowly 'catch-up' by resetting their rental pricing. This is consistent with slow belief-updating even outside of a large financing shock.

The bottom panel of Figure 16 shows the dynamics of landlords' leasing behavior. Occupancy rates grow for under-leased properties in the year just prior to a refinance or sale. This is consistent with landlords correcting their leasing strategy in anticipation of originating a new mortgage and revaluing the property to cash out their equity. The coefficients imply that 1 standard deviation higher ex ante vacancy leads landlords to raise their occupancy by 1 percent in the year before the new origination. There is little evidence that the coefficient attenuates in the years following the new origination. This suggests that landlords' leasing effort is sticky for at least the 3 years post-refinance. It is possible that over the course of a long loan term, landlords are inattentive to changes in local vacancy rates and rents. The cost shock from a refinance (or an I-O period ending) causes landlords to pay attention, re-setting their beliefs, and updating their leasing and pricing strategy.

The combination of these analyses suggests that compared to the control group, landlords fix mispricing 60% faster and update leasing 30% faster in the years around a financing cost shock.

7 City-Level Exposure and Rental Prices

7.1 City-Level Empirical Strategy

Studying within-property variation gives us well-identified evidence on individual landlord behavior. Building on that result, this section quantifies the overall importance of the 'rental financing' channel for interest rates by studying city-level rental price indices.

I aggregate exposure to rental financing cost shocks to the city-level. To study this, I use the simple insight that the effect of changes in interest rates (or financing costs) should be moderated through the amount of the rental housing stock that is prepaying or maturing each year (e.g. context-dependence in Berger et al. 2021). Different cities have different exposure to interest rate shocks in the sense that contemporaneous employment growth in the city is not systematically linked to the share of multifamily mortgages that are prepaying or maturing in a given year.[49]

I develop a financing cost shock variable based on the interaction between the share of prepaying and maturing multifamily mortgages in CBSA c in year t interacted with the interest rate exposure on the stock of multifamily mortgages entering year t. I sum the number of properties and number of units in the Trepp dataset to the CBSA-level, then divide the stock of properties in year $t-1$ by the number of properties whose mortgages prepay or mature in year t—this share prepaying variable is SHP_{ct}. For each property entering year t, I calculate the spread between the current mortgage rate on each property's loan at the beginning of year t and the average mortgage rate on new multifamily origination over that year—this rate exposure variable is REX_{ct}.

As an example of the intuition between these measures, suppose that every existing multifamily mortgage in Kansas City matures in 2012. Here, SHP_{ct} is equal to 1. Now suppose the stock of multifamily mortgages in Kansas City has a current mortgage rate of 5% entering the year 2012, and over the course of the year average mortgage rates on new origination are 4%. In this case, REX_{ct} is equal to -1. Then, by regressing growth in a rental price index on the interaction $SHP_{ct} \times REX_{ct}$, we are estimating the average effect of property owners reducing their financing costs by 1 percentage point (or 100 basis points).

I formalize the regression as follows

$$\Delta r_{c,t-2,t+1} = \alpha_c + \alpha_t + \beta SHP_{ct} \times REX_{ct} + \epsilon_{ct} \tag{6}$$

My main dependent variable of interest is $\Delta r_{c,t-2,t+1}$. This uses a hedonic rental price index, r_{ct}, estimated at the CBSA-level from Census American Community Survey data from 2005-2017. I take differences from year $t-2$ to $t+1$ to study the effect of changes in financing costs in year t on rental price growth within the city (clustering standard errors by CBSA and by year). This choice is informed by the empirical

[49] See Table C4. I show that the relationship between share prepaying/maturing and contemporaneous employment and wage growth is zero after controlling for CBSA and time fixed effects.

results on changes in rents and revenues around refinancing and acquisition, which suggest that differences in rents at the property level change around the time of prepayment or maturity but those changes do not continue to grow over time—in some cases, they change the year prior to prepayment (so I use $t-2$ as the base year), and the dynamics change little after year $t+1$.

7.2 City-Level Results

To understand the market-level effects of changes in financing costs, I modify the empirical strategies from the property-level results sections. I use an aggregate empirical strategy described by Equation (6), analyzing the response in city-wide rents to financing cost shifts.

To illustrate this empirical relationship visually, I show in Figure 17 the relationship between mortgage rate exposure at the beginning of each year t and rent growth in the three years around that year t. I calculate rate exposure as the units-weighted difference between average market mortgage rates for year t subtracted from actual mortgage rates for active loans in the city at the beginning of each year. The figure separates the relationship between rate exposure and rent growth for cities with high shares of existing properties prepaying or maturing in year t compared with cities with low shares paid off. For this exercise, I choose prepayment probability cutoffs at the 25th (2.8%) and 75th (9.3%) percentile of the city-year prepayment share distribution.

The primary lesson of Figure 17 is that interest rate exposure affects rental price growth in cities with high prepayment, and does not affect rents in cities with low or negligible prepayment shares. This provides evidence of differences in the effect of changes in financing costs on rents based on a coarse measure of differences in prepayment behavior. Next, I formalize these differences in a regression with a linear interaction between shares and rate exposure.

In Table 5, I show the results of regressions based on Equation (6). The results are displayed with the first 3 columns in ordinary least squares regressions, and the final 3 columns including fixed effects for year and CBSA. Columns 1 & 4 display bivariate regressions of rent growth on the share of multifamily mortgages prepaying or maturing. Both columns show a negative relationship between prepayment share and rent growth. This is similar to the property-level results on the interaction between rate exposure and refinancing. The coefficient suggests the average refinance has been into a mortgage with lower financing costs, which has, on average, led to lower average rent growth for tenants.

Columns 3 & 6 of Table 5 show the main results. The specification controls for city and year fixed effects, the share of units renovated, and local employment and income growth.[50] This captures the component of changes in interest rates or credit conditions that could be systematically related to changes in local rental market conditions (the results are similar in Columns 2 and 5). We see that the coefficient estimates are statistically significantly different from zero. The positive interaction coefficient indicates that increasing interest rates has a positive effect rents in areas with higher prepayment shares, and, likewise, declining rates decrease rent growth in areas with higher prepayment shares. The point estimates imply that for a 1 ppt increase in rates in city with 5% of loans prepaying or maturing, rents would grow 0.3 percentage points more quickly (or vice versa).

I explore the robustness and dependence of these results to the choice of dataset. I show the dynamics of the effect on rent growth in Appendix Table F13. The financing cost effect is not significantly different from zero before year t, and grows through $t+2$. I provide results from regressions where rent growth is determined

[50] The regressions are weighted by the coverage of the Trepp securitized dataset (taking units covered by Trepp as a share of 2000 CBSA population).

using a repeat-rent index from REIS in Table F14. The results are similar in direction and significance to Table 5. The magnitude of the interaction coefficients is somewhat larger in the REIS regressions than in the Census regressions, which may be due to the fact that asking rents (particularly on higher-quality, market-rate buildings) are more volatile than the distribution of existing rents overall. Notably, the effect of share prepaying and maturing is positive in these results, corroborating the property-level asking rent results at refinance and sale, which are both positive. It appears that an increase in financing events that involve appraisals (refinance or sale) leads to landlords increasing asking rents, even conditional on change in interest rates.

8 Discussion

In this paper, I have shown three main results: (1) landlords increase revenues around the end of interest-only periods; (2) landlords raise revenues at the time that they refinance a new mortgage or around the sale of a property; and (3) there is a positive relationship between rent growth at the city-level and the interaction between new multifamily mortgage origination & existing mortgage rate exposure. I discuss a stylized conceptual framework to interpret these empirical results. At the core of the discussion is that landlords make discrete decisions about updates to their pricing and leasing strategies related to learning new, costly information about their local rental housing markets. Financing costs influence their decisions about updating their pricing and leasing because if they fail to update their behavior, they run the risk of paying liquidity-related costs or they miss out on the opportunity to extract equity from the property.

The average property cashes out $2.9 million at refinance, or about $22 thousand per unit.[51] This is comparable to the average single-family refinance (e.g. $19 thousand in Di Maggio, Kermani, and Palmer 2020). The event study results suggest that landlords anticipate the ability to cash-out and increase rents and revenues leading up to a refinance. Changes in property revenues and rents may affect the appraised value of multifamily properties, likely because appraisers rely on recent leases and financial statements.[52] Landlords ability to 're-optimize' their rental pricing and leasing can be rationalized by the presence of information costs. For the average 200 unit property who is able to increase revenues by 3% around a financing cost shock, the back-of-the-envelope increase in equity is $130,000 at refinance.[53] Scaling the revenue effect to the 15,000 properties annually originating new securitized mortgages implies an increase in rental revenues of $200 million, and an increase in aggregate multifamily values of $2 billion per year due to the information & financing frictions documented in this paper.

Private sector reports from the revenue management industry can benchmark the size of the property-level results. Industry publications report that adoption of pricing and leasing software can generate revenue gains ranging from 1 to 5% of gross revenues. The main results (3% on average) fall right in the middle of this range.[54] Paying revenue management consultants or acquiring new software could represent real-world

[51]See Appendix H.

[52]There is discussion in Online Appendix B and evidence in Appendix G on how appraisers with limited information can over-emphasize the importance of asking rents in valuing multifamily housing. Appraiser bias has been a long topic of study (Geltner 1989; Conklin et al. 2020; Clayton et al. 2001; Calem et al. 2021). Some research on the single-family mortgage market have studied 'de-biasing' appraisers (e.g. Conklin et al. 2020; Agarwal, Ambrose, et al. 2020), there has been less work on multifamily valuation partly due to the difficulty obtaining data linking financial outcomes, rents, and values. Recent papers have proposed regression (Li and Liang 2020) or machine learning algorithms (Kok et al. 2017) to improve appraisal values, often called automated valuation models (AVMs). While the point of this paper is not to provide a new method of appraisal, I provide new evidence on appraisals and point out some issues in multifamily valuation.

[53]Appendix G estimates that the appraisal value-revenue elasticity is between 1 and 1.5.

[54]See e.g. Wendy Broffman, "The Bottom Line on the Revenue Management Industry" Yield Pro, April 1, 2007, https://yieldpro.com/2007/04/the-bottom-line-on-revenue-management/.

examples of the 'information costs' I model above. The adoption and use of algorithmic pricing software is an interesting area for future study. The software could reduce information costs. These pricing and leasing dynamics could involve a costly trade-off for tenants if it effectively creates collusion among landlords in their rent-setting behavior. However, the main results of this paper suggest that tenants and landlords both gain in the two financing cost settings. The number of occupied units rises, suggesting that total welfare increases (even though tenants may pay higher prices on average).

The measurement of the main empirical effects speaks to a higher-level policy question: how do changes in monetary policy and the mortgage market affect inequality (Coibion et al. 2017)? Prior work has clarified the stark disparities by race and income in ability to and speed of refinancing for homeowners (Gerardi et al. 2021; Cloyne et al. 2020; Kiefer et al. 2021; DeFusco and Mondragon 2020). In Figure A3, I highlight that around the Fed's single-family mortgage-backed security asset purchases (QE1) in late-2008 and early-2009, a 1.06 percentage point (106 basis point) wedge opened up between mortgage rates for conforming single-family owner-occupied housing compared with multifamily rental housing. The Fed purchased \$10.5 billion in agency CMBS from March 2020-2021.[55] If monetary policy uses asset purchases in residential mortgage markets, policymakers should account for how the costs and benefits of their policies are shared between homeowners, landlords, and tenants.

The government sponsored enterprises (Fannie Mae, Freddie Mac, and Ginnie Mae) and their overseeing agencies (FHFA and HUD) have heavily increased their presence in multifamily securitization over the past 10 years. The GSEs have substantial scope to affect mortgage terms and characteristics, setting LTV and DSCR limits on multifamily loans that they purchase. Effectively regulating leverage and cash flow constraints can have important effects on tenants. To my knowledge, the effects on pricing and occupancy have not been explored in depth before this paper. During the Covid-19 pandemic, the GSEs' decision to implement multifamily mortgage forbearance and eviction moratoria in likely had a very large impact on both landlords and renters. This paper provides substantial evidence and motivates additional work on how landlord-side mortgage shocks affect real rental market outcomes.

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[55] See Frame and Steiner (2022); New York Fed, Agency Commercial Mortgage-Backed Securities Operation Results, https://www.newyorkfed.org/markets/domestic-market-operations/monetary-policy-implementation/agency-commercial-mortgage-backed-securities/agency-commercial-mortgage-backed-securities-operations.

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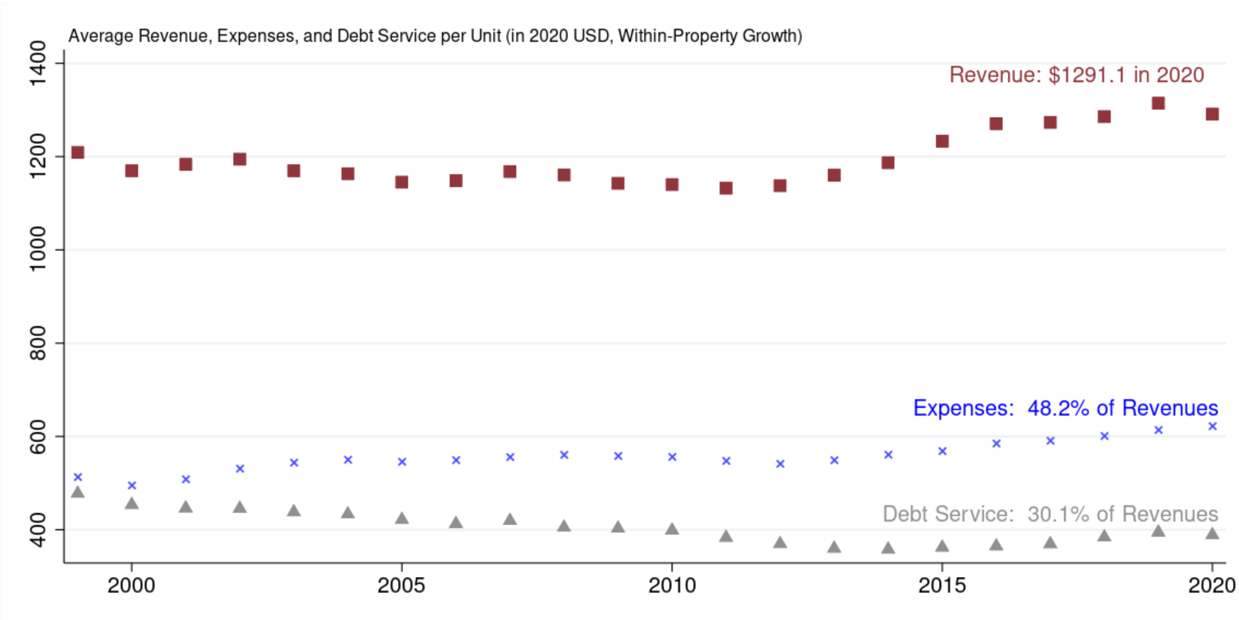
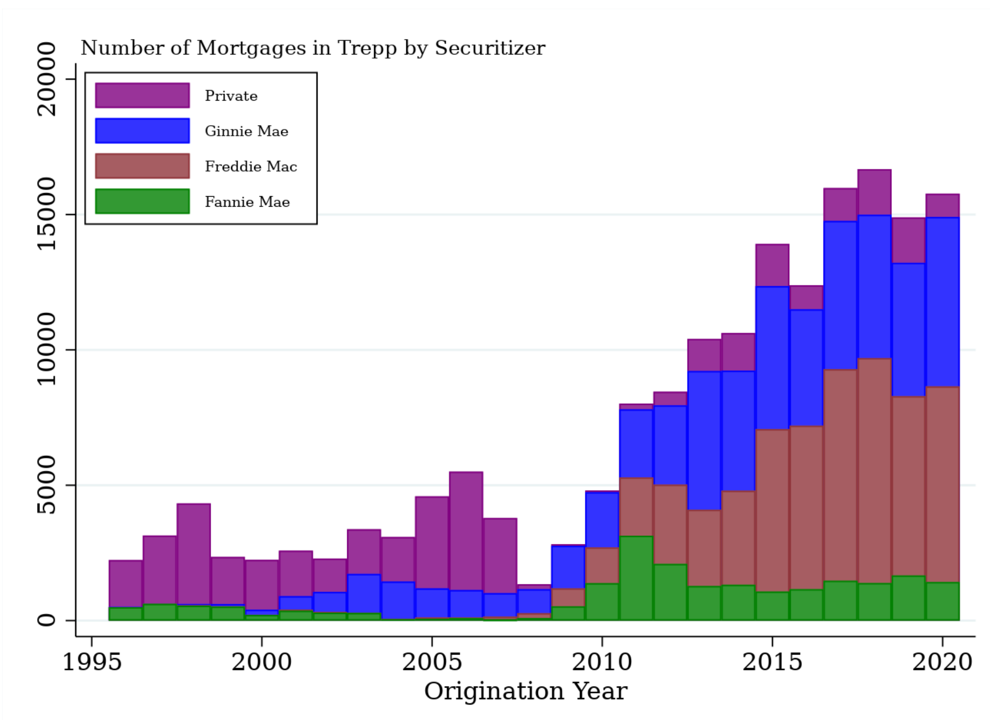
Figure 1: Description of Property-Level Financial Variables & Number of Originations in Trepp Data

(a) Main Financial Variables

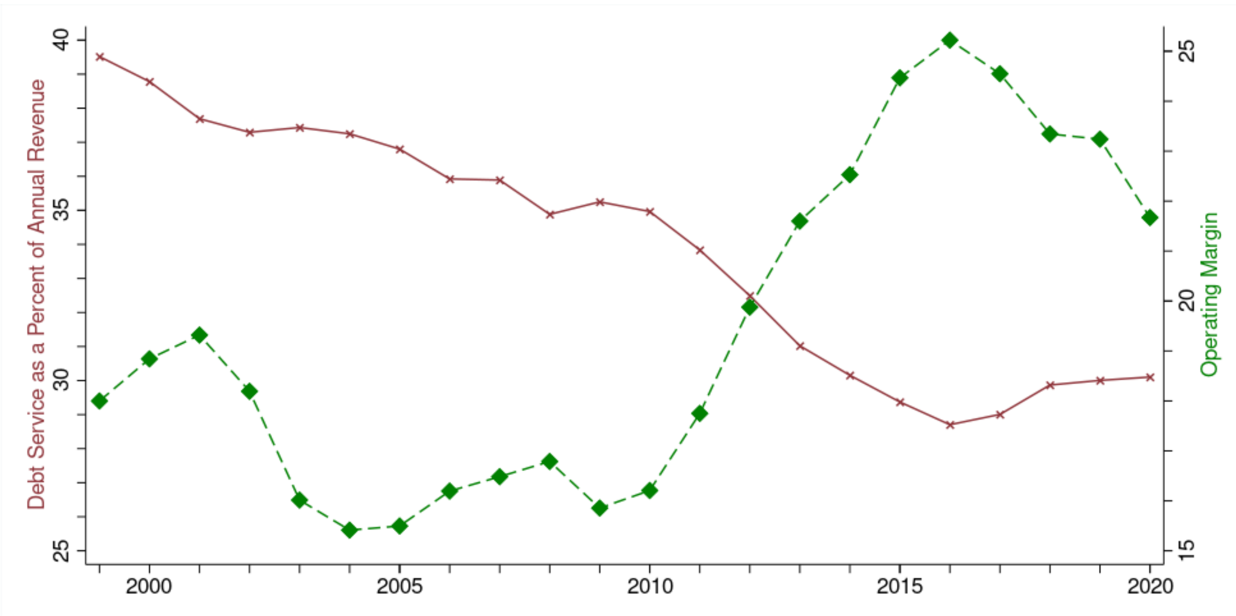


(b) Number of Observations from Private & Agency CMBS



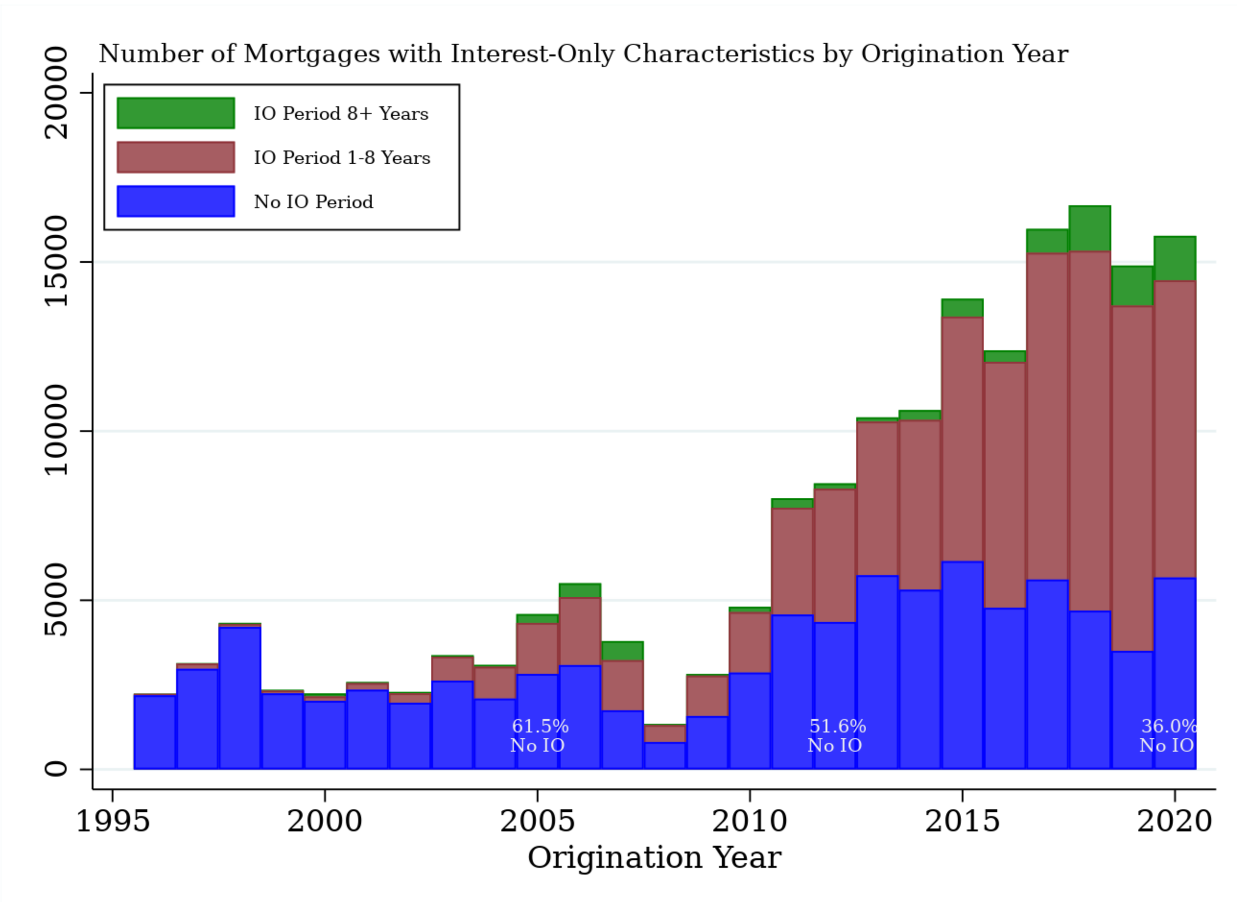
Note: Top panel shows growth in inflation-adjusted average revenues, operating expenses and debt service payments (per unit monthly) using within-property-variation. To do this, I recover constants and year-fixed effects from a regression of each outcome on property fixed effects and year fixed effects. The bottom panel plots the count of newly originated mortgages by origination year in the Trepp CMBS data.

Figure 2: Change in Operating Margin and Debt Service Share of Revenue (using within-property variation)



Note: Figure shows change in annual debt service payments as a percent of revenue and annual operating margin using within-property-variation. Operating margin calculated as revenue minus operating expenses and debt service payments, all divided by revenue. I recover constants and year-fixed effects from a regression of each outcome on property fixed effects and year fixed effects. The left-hand y-axis plots debt service percent in maroon x's, and the right-hand y-axis plots operating margin in green diamonds.

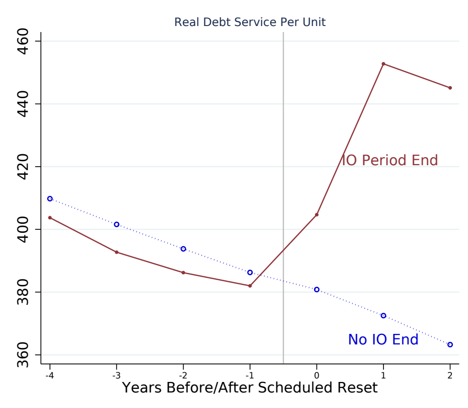
Figure 3: Prevalence of Interest-Only Mortgages



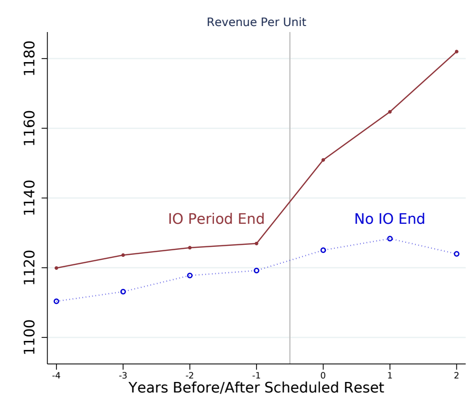
Note: This figure shows the usage and frequency of I-O terms over time for securitized multifamily mortgages. The figure plots the count of newly originated mortgages by origination year in the Trepp CMBS data. I separate three categories: loans with no I-O period, those with an I-O period for 1-8 years, and those with a 8 or more year I-O period.

Figure 4: Trends in Financial Outcomes for I-O Mortgages vs Comparison Group

(a) Debt Service Payments

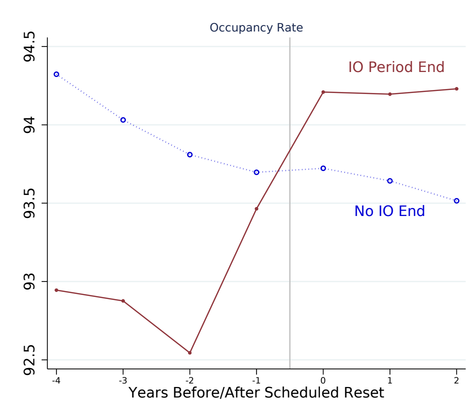


(b) Revenues

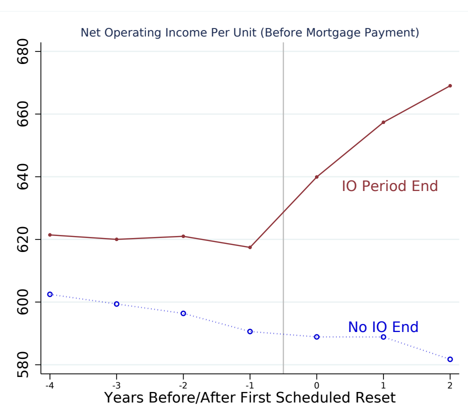


(c) Operating Expenses

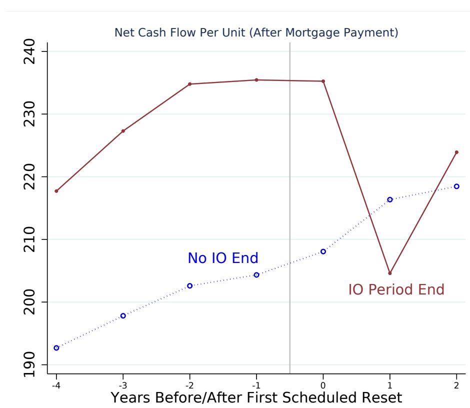
(d) Occupancy Rate



(e) Net Income



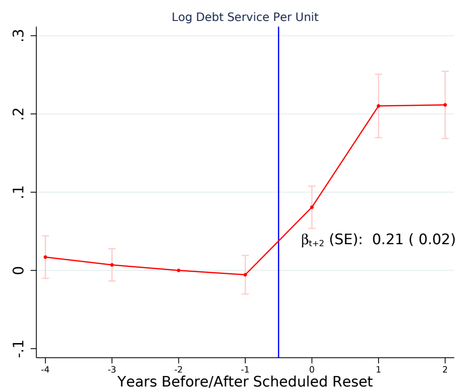
(f) Net Cash Flows



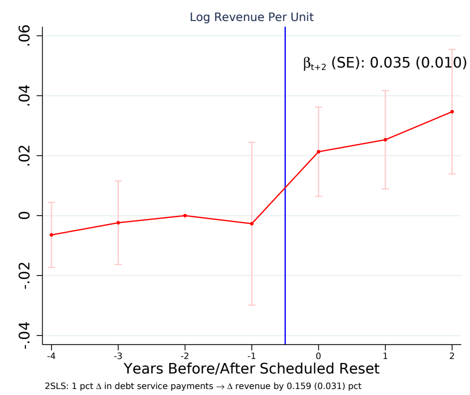
Note: These figures plot average financial outcomes for a panel of properties in the Trepp datasets. The solid maroon lines include properties with 10-year loans whose I-O payment period ends in the 5th year of the mortgage. The blue dotted lines represent properties with 10-year loans in the control group, selected for stacked comparisons with the treated I-O loans. Each group of comparison loans with no I-O were originated and scheduled to mature in the same years as a group of treated I-O loans.

Figure 5: Event Study for I-O Mortgages

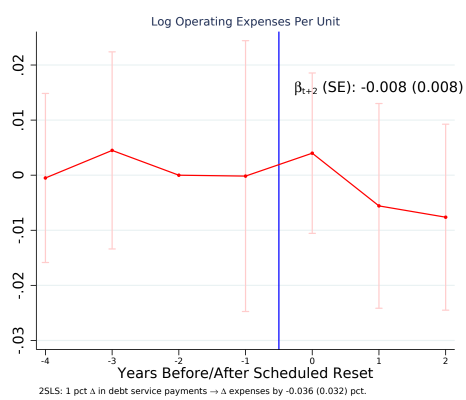
(a) Debt Service Payments



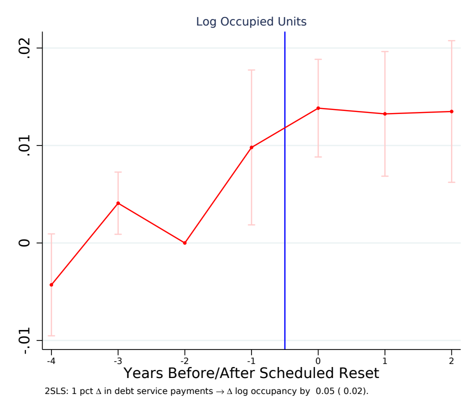
(b) Revenues



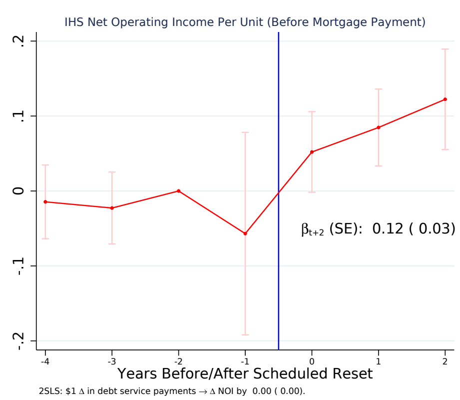
(c) Operating Expenses



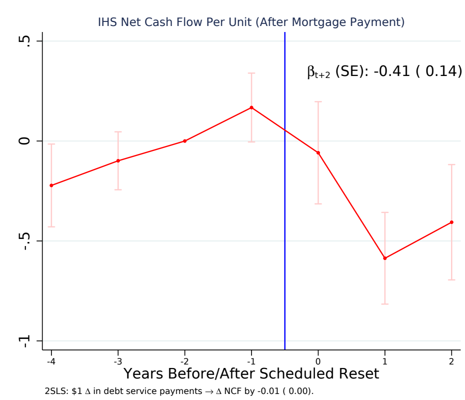
(d) Occupancy Rate



(e) Net Income



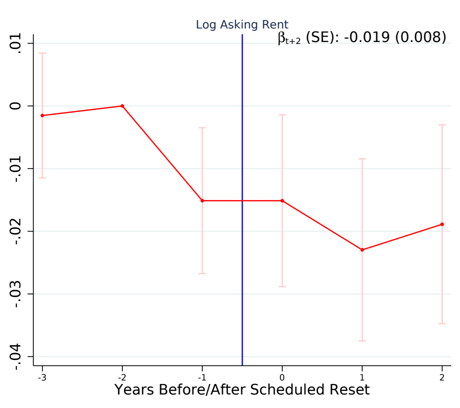
(f) Net Cash Flows



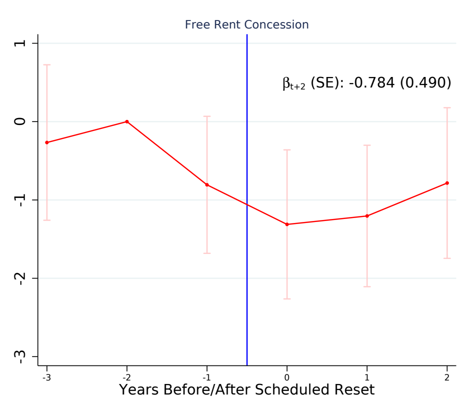
Note: These figures plot event study coefficients for financial outcomes of properties in the Trepp datasets. I compare the treated I-O properties (601 properties) to the control group of non-I-O properties (5,201 properties) using the regression equation 1.

Figure 6: Event Study for I-O Mortgages: Asking Rents & Free Rent Concessions in Trepp-REIS Subsample

(a) Asking Rent



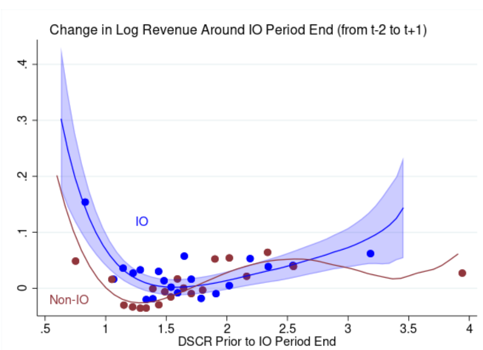
(b) Free Rent Concessions



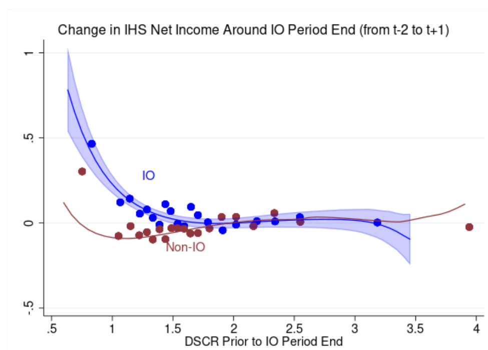
Note: These figures plot event study coefficients for financial outcomes of properties in the merged Trepp-REIS datasets (results for the financial outcomes for this subset are available upon request). I compare the I-O properties (226 properties) to the control group of non-I-O properties (1,076 properties) using the regression equation 1.

Figure 7: Role of Liquidity Constraints: Heterogeneity in Effect of I-O End by Mark-to-Market DSCR

(a) Growth in Revenue

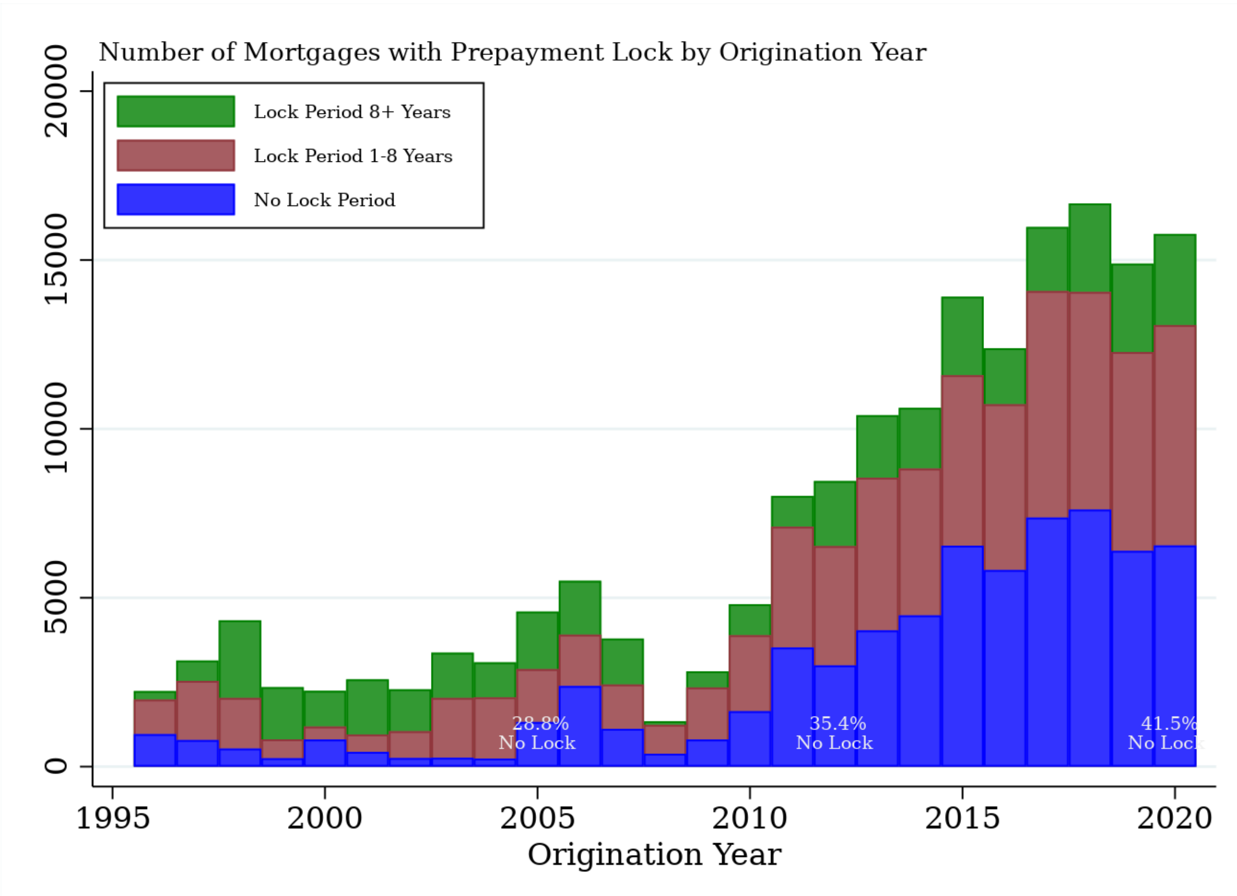


(b) Growth in Net Income



Note: These figures plot long differences for financial outcomes of properties in the Trepp datasets. I compare the treated I-O properties to the control group of non-I-O properties in long differences from two years before the end the I-O period to one year after. The x-axis plots the debt service coverage ratio at the property-level observed 2 years before the cohort's actual or placebo I-O period ends, based on the stacked comparison between actual 'treated' and the cohorts of 'control' group properties. The top panel plots a binscatter of the change in log revenues and the inverse hyperbolic sine of net incomes for the treated and comparison groups. Over the binscatter, I graph a local polynomial of degree three with 95% confidence intervals around treated group's line. Appendix Figure C5 graphs histograms of the distribution of the mark-to-market DSCR 2 years before the I-O period ends for the treated and control groups. DSCR is calculated as net cash flow divided by debt service payments, where net cash flow is revenue minus operating expenses and debt service payments. A DSCR of 1 means net cash flows equal debt service payments.

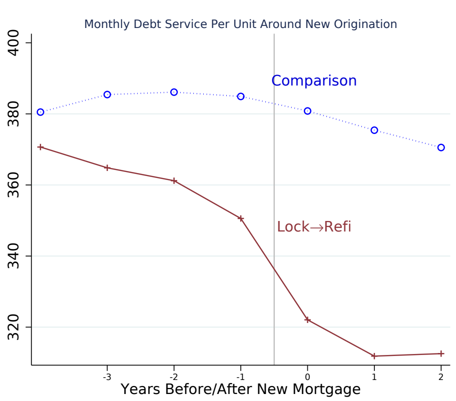
Figure 8: Prevalence of Prepayment Locks for Mortgages



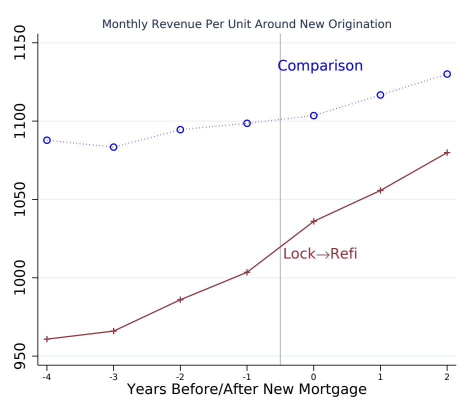
Note: This figure shows the usage and frequency of prepayment lock terms over time for securitized multifamily mortgages. The figure plots the count of newly originated mortgages by origination year in the Trepp CMBS data. I separate three categories: loans with no prepayment lock-out period, those with a lock period of 1-8 years, and those with a lock-out period of 8 or more years. These lock-out periods are bunched at a few numbers of months. Most with 8 or more years locked are for 108 or 114 months out of a total 120 month (10-year) loan term.

Figure 9: Trends in Financial Outcomes for Locked Refinance Mortgages vs Comparison Group

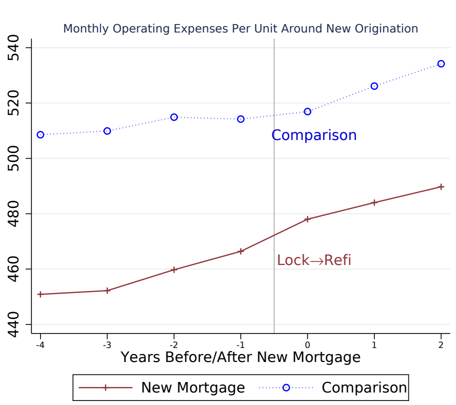
(a) Debt Service Payments



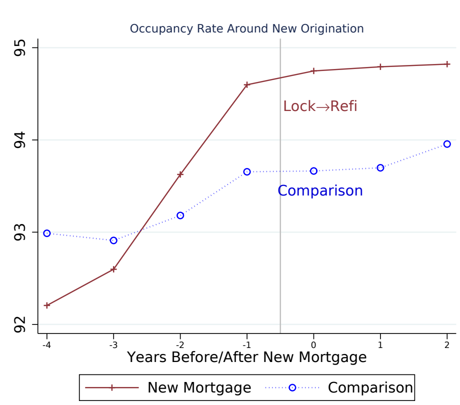
(b) Revenues



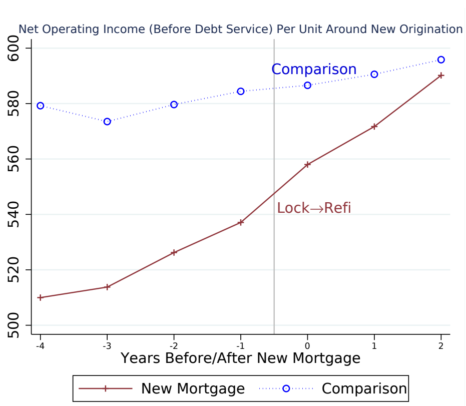
(c) Operating Expenses



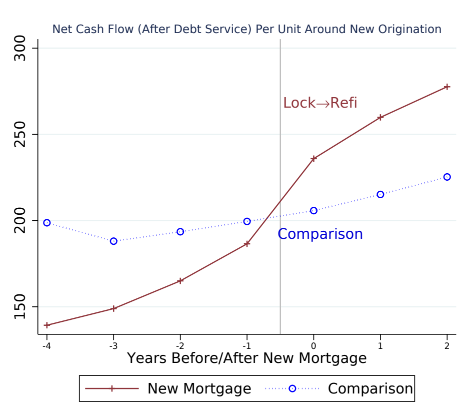
(d) Occupancy Rate



(e) Net Income



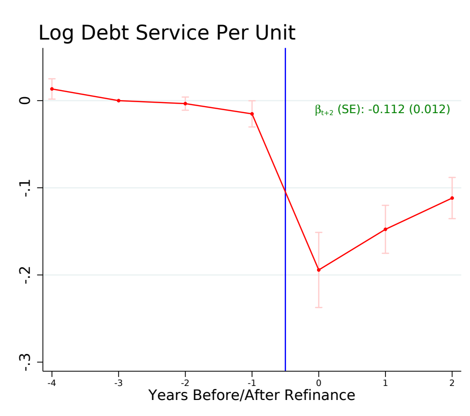
(f) Net Cash Flows



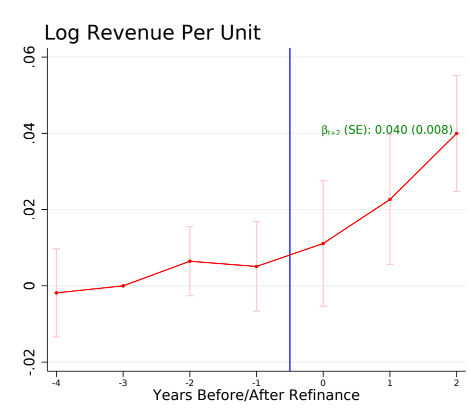
Note: These figures plot average financial outcomes for a panel of properties in the Trepp datasets, where the treated properties are linked across mortgages. The solid maroon lines include properties with prepayment locks on their existing mortgage, where time 0 is the year their existing mortgage prepays or matures. The blue dotted lines represent properties with 10-year loans in the control group, selected for stacked comparisons with the treated lock-refi loans. Each group of control group properties are selected for stacked comparisons with a cohort of treated properties in the lock-refi group. The control group cohorts are balanced in event time in the middle of their current mortgage term (2nd to 8th or 3rd to 9th year), and stacked with a treated group to align in calendar time.

Figure 10: Event Study Results for Locked Refinance Mortgages

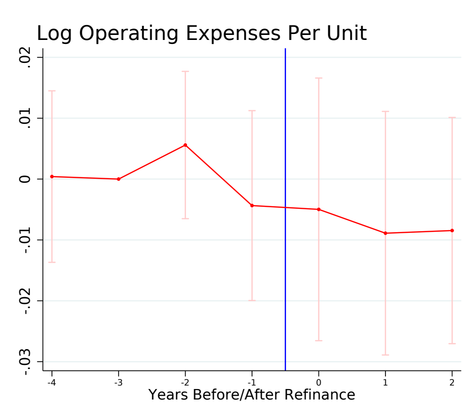
(a) Debt Service Payments



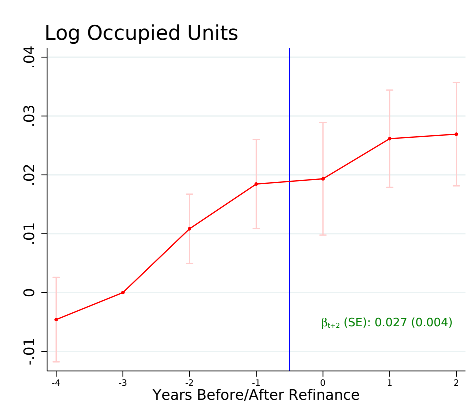
(b) Revenues



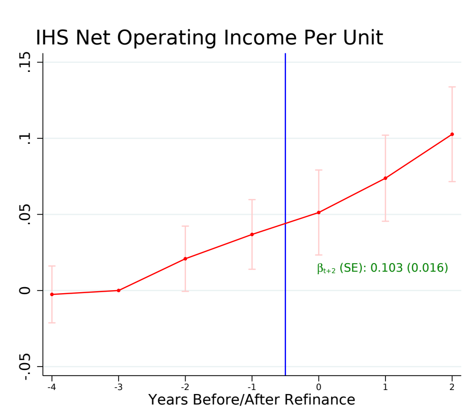
(c) Operating Expenses



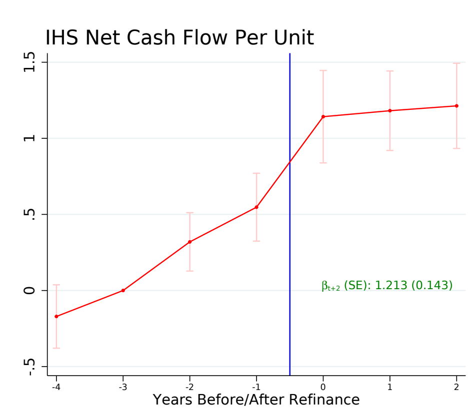
(d) Occupancy Rate



(e) Net Income



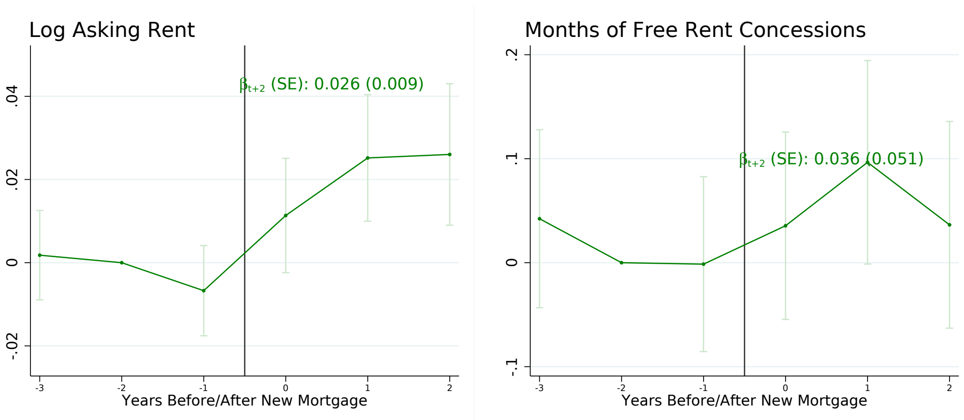
(f) Net Cash Flows



Note: These figures plot event study coefficients for financial outcomes of properties in the Trepp datasets. I compare the treated lock-refi properties to the control group of non-refinanced properties using the regression equation 3.

Figure 11: Landlords Change Asking Rents and Free Rent Concessions at Refinance

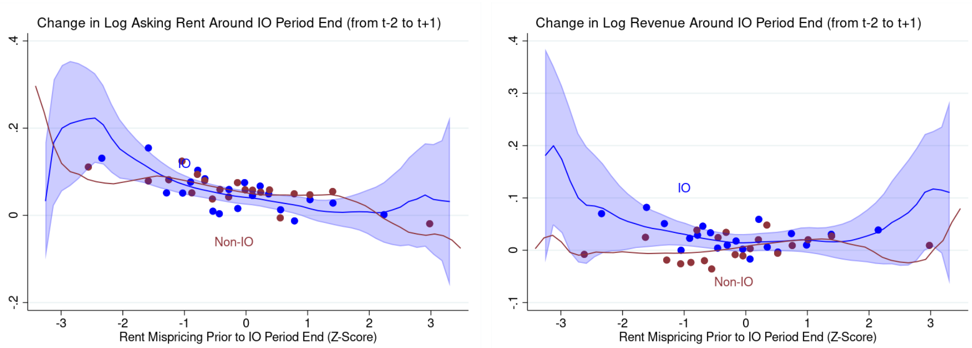
(a) Asking Rent & Free Rent Concessions (REIS)



Note: These figures plot event study coefficients for financial outcomes of properties in the REIS-Trepp datasets. I compare the treated properties undergoing a refinance or sale at time 0 to the control group of non-refinanced properties using the regression equation 3. Appendix Figure C11 includes Trepp financial outcomes to show consistent effects with the Trepp only main effects.

Figure 12: Showing Landlords Update Pricing Using Heterogeneity in I-O End by Ex Ante Mispricing

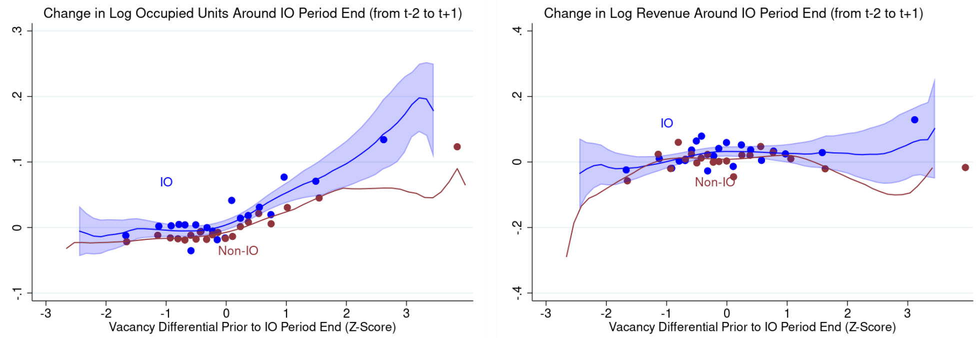
(a) Growth in Asking Rents and Revenues



Note: These figures plot long differences for market rent and financial outcomes of properties in the merged REIS-Trepp datasets. I compare the treated I-O properties to the control group of non-I-O properties in long differences from two years before the end the I-O period to one year after. The x-axis plots a measure of property-level mispricing observed 2 years before the I-O period ends. The top panel plots a binscatter of the change in log asking rents and log revenues for the treated and comparison groups. Over the binscatter, I graph a local polynomial of degree three with 95% confidence intervals around treated group's line. The mispricing measure is the z-score of residuals recovered from a regression of log asking rent per square foot on 23 property amenities and characteristics as well as neighborhood by year fixed effects.

Figure 13: Showing Landlords Update Leasing Using Heterogeneity in I-O End by Ex Ante Vacancy Differential

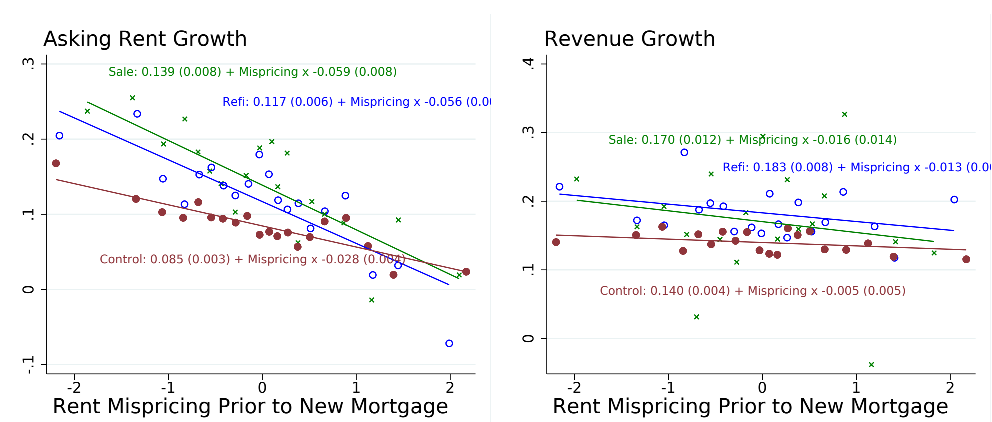
(a) Growth in Occupancy Rates and Revenues



Note: These figures plot long differences for occupancy and financial outcomes of properties in the merged REIS-Trepp datasets. I compare the treated I-O properties to the control group of non-I-O properties in long differences from two years before the end the I-O period to one year after. The x-axis plots a measure of property-level vacancy differential observed 2 years before the I-O period ends. The top panel plots a binscatter of the change in log revenues and log occupied units for the treated and comparison groups. Over the binscatter, I graph a local polynomial of degree three with 95% confidence intervals around treated group's line. The ex ante vacancy differential measure, which proxies for leasing effort, is the z-score of residuals recovered from a regression of vacancy rates on 23 property amenities and characteristics as well as neighborhood by year fixed effects.

Figure 14: Heterogeneity in Asking Rents by Ex Ante Mispricing from 2 Years Before to 1 Year After Refinance or Sale

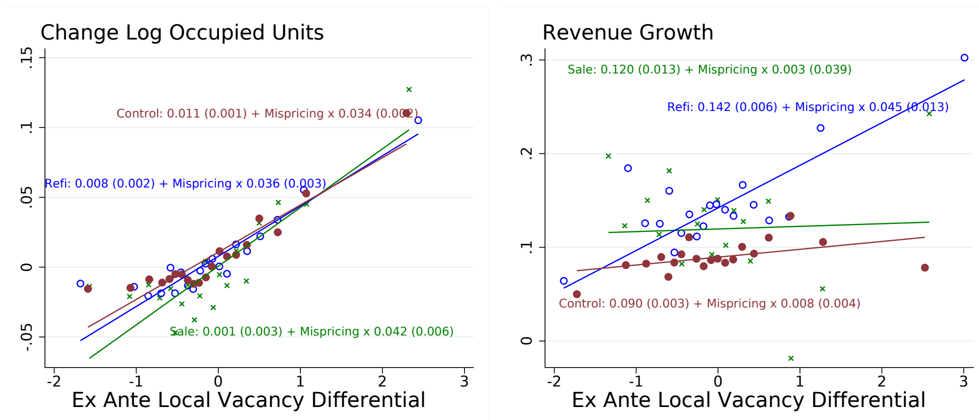
(a) Growth in Asking Rents and Revenue



Note: These figures plot long differences for market rent and financial outcomes of properties in the merged REIS-Trepp datasets. I compare the treated properties undergoing a refinance or sale at time 0 to the control group of non-refinanced properties in long differences from two years before the end the new mortgage is originated to one year after. The x-axis plots a measure of property-level mispricing observed 2 years before the origination. The top panel plots a binscatter of the change in log revenues and log asking rents for the treated and comparison groups. Over the binscatter, I graph a linear regression and report the bivariate regression coefficients for the refinance, sale, and comparison groups. The mispricing measure is the z-score of residuals recovered from a regression of log asking rent per square foot on 23 property amenities and characteristics as well as neighborhood by year fixed effects.

Figure 15: Heterogeneity in Occupancy by Ex Ante Vacancy Differential from 2 Years Before to 1 Year After Refinance or Sale

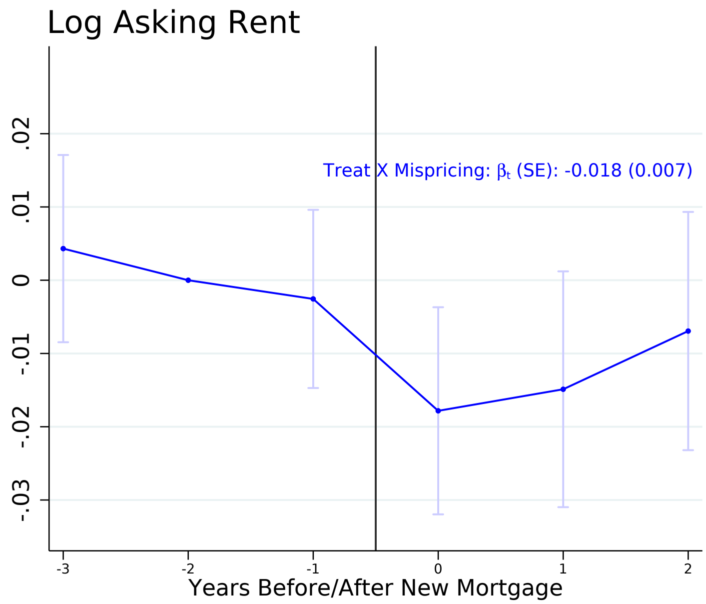
(a) Growth in Occupancy and Revenue



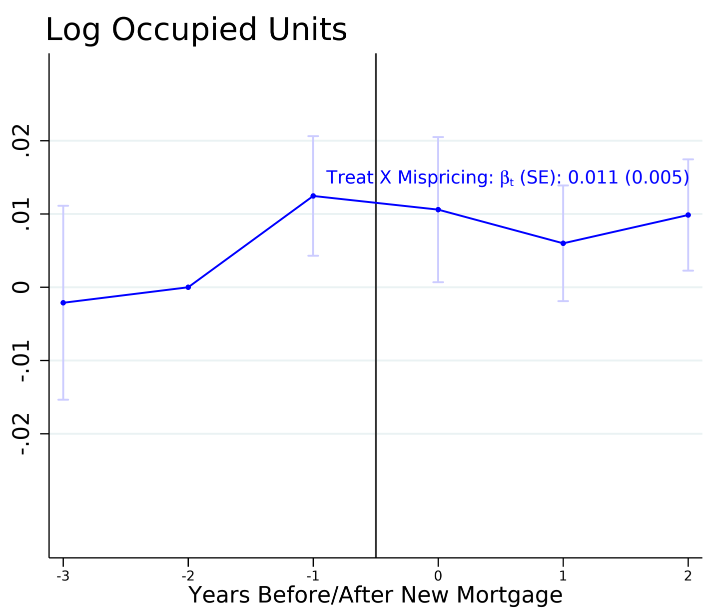
Note: These figures plot long differences for market rent and financial outcomes of properties in the merged REIS-Trepp datasets. I compare the treated properties undergoing a refinance or sale at time 0 to the control group of non-refinanced properties in long differences from two years before the end the new mortgage is originated to one year after. The x-axis plots a measure of property-level mispricing observed 2 years before the origination. The top panel plots a binscatter of the change in log revenues and log asking rents for the treated and comparison groups. Over the binscatter, I graph a linear regression and report the bivariate regression coefficients for the refinance, sale, and comparison groups. The ex ante vacancy differential measure, which proxies for leasing effort, is the z-score of residuals recovered from a regression of vacancy rates on 23 property amenities and characteristics as well as neighborhood by year fixed effects.

Figure 16: Timing Dynamics of Pricing and Occupancy Correction

(a) Change in the Asking Rent-Mispricing Relationship Around a New Mortgage Origination

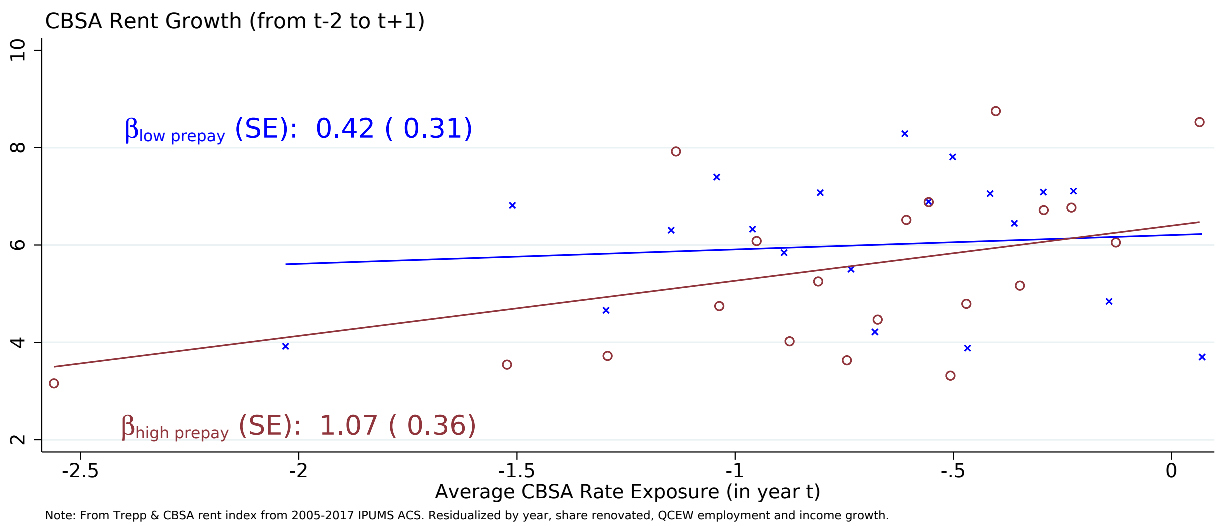


(b) Change in the Occupancy-Vacancy Differential Relationship Around a New Mortgage Origination



Note: These figures plot coefficients $\beta_d^{treatXmis}$ based on Equation (5) using the merged REIS-Trepp datasets. This plots the change for the treated group in the slope of the relationship between the mispricing measure (& vacancy differential measure) and asking rent growth (or occupancy growth). It suggests that treated landlords who undergo a refinance or sale fixed mispricing and underperformance in leasing more quickly relative to the control group who were not refinancing or selling. The ex ante mispricing and vacancy differential measure, which proxies for leasing effort, is the z-score of residuals recovered from a regression of asking rent per square foot (or vacancy rates) on 23 property amenities and characteristics as well as neighborhood by year fixed effects.

Figure 17: Effect of Interest Rate Exposure on Cities with High Versus Low Prepayment



Note: This plots shows the differential change in rent growth for CBSAs in the top versus bottom quartile of originations (measured by the share of existing mortgages prepaying or maturing). The share variable is defined in year t, while rent growth is measured in the years around that, from $t-2$ to $t+1$. Controls include employment and income growth by CBSA over the same period as rent growth (from QCEW), and the share of units renovated in year$t$. Slope coefficients clustered by CBSA.

Table 1: End of I-O Period: Difference-in-Difference Specification

	Debt Service	Revenue	Expenses	Occupied Units	NOI	NCF
Panel A: Log and IHS Specifications						
Post X IO Loan	0.216***	0.030***	-0.013	0.009***	0.088***	-0.478***
	(0.019)	(0.008)	(0.008)	(0.002)	(0.014)	(0.122)
Obs.	22428	22428	22428	22428	22428	22428
Panel B: Dollar and Level Specifications						
Post X IO Loan	88.900***	38.894***	-10.044**	2.884***	48.938***	-39.962***
	(11.515)	(12.650)	(4.510)	(0.630)	(11.264)	(9.412)
Obs.	22428	22428	22428	22428	22428	22428
Year-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Property FE	Yes	Yes	Yes	Yes	Yes	Yes
Event-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.10$. Standard errors are clustered by loan and year. Includes 4 years in event time; 2 years prior to reset and 2 years post reset. I compare the treated I-O properties (601 properties) to the control group of non-I-O properties (5,201 properties) using the difference-in-differences regression based on equation 2. Relative to I-O period end year t, specification includes years: t-3, t-2, t+1, and t+2.

Table 2: Locked Refinance: Difference-in-Difference Specification

	Debt Service	Revenue	Expenses	Occupied Units	NOI	NCF
Panel A: Log and IHS Specifications						
Post X Refi	-0.125***	0.029***	-0.010	0.022***	0.078***	1.018***
	(0.012)	(0.007)	(0.008)	(0.003)	(0.013)	(0.113)
Obs.	1420444	1420444	1420444	1340250	1420444	1420444
Panel B: Dollar and Level Specifications						
Post X Refi	-37.048***	13.215*	-9.310**	4.162***	22.526***	59.574***
	(4.770)	(7.071)	(3.879)	(0.701)	(6.619)	(7.689)
Obs.	1420444	1420444	1420444	1340250	1420444	1420444
Event-Year-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Property FE	Yes	Yes	Yes	Yes	Yes	Yes

Note: *** p<0.01, ** p<0.05, * p<0.10. Standard errors are clustered by address. Includes 4 years in event time; 2 years prior to refinance and 2 years post refinance. Relative to refinance year t, specification includes years: t-4, t-3, t+1, and t+2. Treated sample includes 515 properties whose original mortgage had a prepayment lock for more than 80% of the mortgage term, who refinanced and did not renovate the property. The control group sample includes 6,222 properties (some of which are sampled multiple times) in a balanced panel of properties in the middle of their mortgage term who also have a prepayment lock.

Table 3: Heterogeneity in Effect of Locked Refinance: Difference-in-Difference Specification

	Debt Service	Revenue	Expenses	Occupied Units	NOI	NCF
Panel A: Diff-in-Diff Log and IHS Specifications						
Post X Refi	-0.133***	0.031***	-0.009	0.028***	0.087***	1.252***
	(0.013)	(0.008)	(0.009)	(0.004)	(0.014)	(0.134)
Obs.	1357300	1357300	1357300	1280805	1357300	1357300
Panel B: Heterogeneity by LTV and Rate Exposure (Continuous)						
Post X Refi	0.251***	0.022	0.018	-0.026*	0.061	-1.564**
	(0.055)	(0.027)	(0.033)	(0.015)	(0.052)	(0.612)
Post X Refi X Rate Exposure	0.101***	0.022*	0.010	0.007	0.037*	-0.171
	(0.027)	(0.013)	(0.016)	(0.004)	(0.022)	(0.184)
Post X Refi X Existing LTV	-0.339***	0.057*	-0.019	0.089***	0.110*	3.618***
	(0.060)	(0.032)	(0.039)	(0.022)	(0.066)	(0.867)
Obs.	1357300	1357300	1357300	1280805	1357300	1357300
Panel C: Heterogeneity by LTV and Rate Exposure (Discrete)						
Post X Refi	-0.118***	0.022**	-0.011	0.020***	0.071***	0.931***
	(0.015)	(0.009)	(0.011)	(0.004)	(0.017)	(0.134)
Post X Refi X Rate Increase	0.453***	0.074	0.055	0.003	0.134*	0.147
	(0.111)	(0.050)	(0.065)	(0.013)	(0.073)	(0.752)
Post X Refi X High LTV	-0.100***	0.027**	0.000	0.031***	0.047*	1.201***
	(0.026)	(0.013)	(0.015)	(0.008)	(0.025)	(0.329)
Obs.	1357300	1357300	1357300	1280805	1357300	1357300
Event-Year-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Property FE	Yes	Yes	Yes	Yes	Yes	Yes

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.10$. Standard errors are clustered by address. Includes 4 years in event time; 2 years prior to refinance and 2 years post refinance. Relative to refinance year t, specification includes years: t-4, t-3, t+1, and t+2. Treated sample includes 425 properties whose original mortgage had a prepayment lock for more than 80% of the mortgage term, who refinanced and did not renovate the property. The control group sample includes 6,115 properties (some of which are sampled multiple times) in a balanced panel of properties in the middle of their mortgage term who also have a prepayment lock. Rate exposure is calculated as the change in multifamily mortgage rates between the origination quarter of the prior loan and prepayment or maturity quarter. Rate increase is an indicator variable equal to 1 if rate exposure is positive. Existing LTV is the loan balance two years prior divided by a measure of the current property value (the securitized property value on the prior mortgage times the increase in a zip code-level FHFA house price index since the origination year). High LTV is an indicator for existing LTV greater than 80%.

Table 4: Effect of Refinance and Sale, Controlling for Renovations: Difference-in-Difference Specification

	Debt Service	Revenue	Expenses	Occupied Units	NOI	NCF
Panel A: Diff-in-Diff Log and IHS Specifications						
Post X Refi	-0.121***	0.034***	-0.003	0.024***	0.090***	1.096***
	(0.013)	(0.007)	(0.008)	(0.003)	(0.014)	(0.120)
Post X Sale	-0.117***	0.027***	-0.006	0.015***	0.076**	1.367***
	(0.026)	(0.010)	(0.012)	(0.005)	(0.030)	(0.224)
Renovation	0.067***	0.044***	0.007	0.001	0.101**	-0.050
	(0.026)	(0.012)	(0.012)	(0.007)	(0.039)	(0.193)
Obs.	1195372	1195372	1195372	1117788	1195372	1195372
Event-Year-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Property FE	Yes	Yes	Yes	Yes	Yes	Yes

Note: *** p<0.01, ** p<0.05, * p<0.10. Standard errors are clustered by address. Includes 4 years in event time; 2 years prior to refinance and 2 years post refinance. Relative to refinance year t, specification includes years: t-4, t-3, t+1, and t+2. Treated sample includes 858 properties. The control group sample includes 5,520 properties (some of which are sampled multiple times) in a balanced panel of properties in the middle of their mortgage term. This sample only includes loans refinanced or sold after 2007 because loan purposes (sale vs refinance) were not reported in prior years.

Table 5: CBSA Rent Growth by Exposure to Interest Rates and Prepayment

	OLS			TWFE		
Share of Mortgages Prepay/Matured	-7.514**	1.342	-0.411	-3.275	4.677	1.580
	(3.665)	(4.528)	(4.473)	(5.153)	(4.752)	(5.285)
Share X Rate Exposure (Mkt Rate - Act Rates)		13.174***	10.501***		11.501***	6.179***
		(3.663)	(3.567)		(2.925)	(1.021)
Share Renovated			1.549***			1.015*
			(0.502)			(0.473)
Employment Growth			0.143***			0.274**
			(0.038)			(0.088)
Income Growth			0.419***			0.253**
			(0.084)			(0.103)
Obs.	1550	1550	1550	1550	1550	1550
Year FE	No	No	No	Yes	Yes	Yes
CBSA FE	No	No	No	Yes	Yes	Yes

Note: *** p<0.01, ** p<0.05, * p<0.10. Standard errors are clustered by CBSA and year, regression is weighted by coverage of units in Trepp data as a share of CBSA population in 2000. The share of mortgages variable is defined in year t by the share of units in existing mortgages prepaying or maturing, while rent growth is measured in the years around that, from $t-2$ to $t+1$. Rate exposure is the spread between mortgage rates available during year t and actual rates on existing mortgages entering the year. Controls for share renovated in year t, and employment and income growth over the same period as rent growth (QCEW). Share renovated is the share of units with renovation flag in Trepp data in year t.