The Renovation Rebalance:
How Financial Intermediaries Affect Renter Housing Costs

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Abstract

Residential quality improvements explain a large fraction of recent rent growth, and financial
intermediaries have contributed to this effect by reallocating financing to improvement projects
from other types of residential investment. I perform a quality-adjustment exercise to show
that improvements account for 70% of post-crisis real rent growth. Then I isolate two shocks to
financial intermediaries which can explain 40% of improvement activity and 30% of rent growth.
The first shock is a change in bank regulatory capital requirements, and it channels bank credit
from construction projects to improvements. The second shock is due to institutional treatment
of pension funding gaps, and it channels private equity financing from buy-and-hold projects to
improvements. My results illustrate how financial intermediary portfolio reallocation can affect
the types of real projects that are financed.

Keywords: Financial Intermediaries, Housing Quality, Housing Rents, Financial Regulation

JEL Classification: G21, G23, G28, R30, R31

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1 Introduction

How do financial intermediaries affect the real economy? The traditional view is that intermediaries affect the level of real investment through the supply of financing.\(^1\) Far less is known about whether intermediary portfolio choice affects the types of real investments that are made. I pursue this question in the context of the $11 trillion U.S. rental housing market.\(^2\) This setting is empirically convenient, since professional real estate investment is classified according to distinct project types (e.g. construction, improvements). It is also timely: the rent-to-income ratio set a record high in 2015, while the percent of rental housing units improved each year doubled its pre-crisis peak, shown in Figure 1. These trends have ignited policy debate about rental housing affordability, since improvements constrict the supply of cheap homes by transferring them to the expensive end of the quality ladder (Donovan 2014). Yet improvement projects, like any other type of residential investment, must be financed: could greater supply of financing for this specific type of investment be leading to better quality and, thus, higher rent growth?

I find that greater improvement activity accounts for a majority of recent rent growth, and two shocks to financial intermediaries have contributed to this effect by channeling financing toward improvements and away from other types of residential investment. First, a 2015 change in regulatory capital requirements incentivizes banks to reallocate credit from risky construction projects to improvement projects: a relative “risk-down” shock. Second, declining safe yields coupled with institutional treatment of actuarial funding gaps incentivizes public pensions to reallocate money from private equity funds which perform safe buy-and-hold projects to funds which perform improvements: a relative “risk-up” shock. These findings illustrate how shocks to intermediaries’ portfolio allocation can lead to shifts in the composition of real investment projects. In this setting, separate shocks to desired portfolio risk conspired to channel financing toward improvements and away from other types of residential investment.

My first result — which motivates the rest of the paper — is that quality improvements account for a majority of real rent growth (i.e. excess of non-housing inflation) since the 2008 financial crisis. By contrast, improvements account for a very small share of rent growth in prior periods. Thus post-crisis rent growth is in line with historical norms on a quality-adjusted basis. I arrive at this conclusion through a series of measurement exercises which compute the wedge between observed rent and a measure of quality-adjusted rent. My first approach is a traditional hedonic

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\(^1\) Common references include Bernanke (1983), Gertler and Gilchrist (1994), or Peek and Rosengren (2000).

\(^2\) According to Zillow, the housing stock was worth $29.6 trillion in 2016, of which 37% was in the rental sector.
adjustment, which consists of regressing a home’s rent on a set of observable features and adjusting rent for changes in these features. Compared to statistical agencies, I adjust for relatively granular improvements (e.g. dishwasher installation), which collectively account for 65% of post-crisis real rent growth. Complementing the hedonic approach, I introduce a preference structure to compute a welfare-relevant rent index, which I later use to study how shocks to financial intermediaries affect household surplus. Relative to the hedonic method, this approach can infer growth in both the quantity and quality of improvements (e.g. speed of a newly installed dishwasher). The structural approach implies that quality improvements account for 86% of post-crisis real rent growth.

However, there are striking spatial patterns in where quality improvements have been made. Improvements have been concentrated in above-median income zip codes, and below-median income zip codes have seen little improvement in quality. This gap further widens when measured in terms of household surplus, since the estimated willingness-to-pay for quality is greater in high income markets. To interpret, real estate investors appear to make improvements in areas where the equilibrium price of quality is highest. Consistent with this interpretation, I later find that shocks to the supply of financing disproportionately increase improvement activity in high income areas.

My second — and principal — result is that financial intermediaries have significantly increased real improvement activity by channeling financing toward improvement projects and away from other types of residential investment. I begin with a credit supply shock for multifamily improvements generated by High Volatility Commercial Real Estate (HVCRE) bank capital requirements. These requirements were introduced in 2015 as part of the Dodd-Frank Act, and they assigned a more favorable regulatory risk weight to loans secured by improvements on income-producing properties relative to loans for new construction. This policy introduced a wedge in the cost of capital for different loan types, incentivizing banks to transfer credit to improvement projects from construction. Using a triple difference-in-difference strategy which compares banks (i.e. treated lenders) and specialty nonbank lenders in the multifamily mortgage market, I find that HVCRE capital requirements increase banks’ supply of credit for improvements.

These lender-level results partly reflect shifts in bank market share, and thus they do not necessarily imply an aggregate increase in improvements. To estimate such an aggregate effect, I conduct a county-level analysis. Here I use the observations that real estate lending relationships are sticky and that historical episodes, such as bank failures in the 1980s, appear to dictate where nonbanks

3Explicitly, this approach computes the area to the left of the Hicksian market demand curve across quality segments. I infer time variation in quality using a similar insight as Redding and Weinstein (2018).
have more market share. Using a difference-in-difference specification in which treated counties are those where banks had a higher initial market share than nonbanks, I find that HVCRE regulation significantly increases a county’s share of improved housing units. Then, I consider a counterfactual in which regulatory capital requirements treat all residential investment projects equally. Under this counterfactual, there would have been 44% fewer apartment improvements over 2015-2016, in partial equilibrium. The counterfactual improvements can account for 32% of observed rent growth and, based on the structural rent index, they increased household surplus 41%.

Complementing the county-level analysis, I estimate a property-level specification that exploits both idiosyncratic variation in the timing of debt maturities à la Almeida et al. (2012) and institutional features of multifamily mortgages. Most multifamily mortgages are balloon loans without the option to refinance, which generates a spike in improvement activity on the due date as borrowers use loan renewal to obtain credit for improvements. Having a maturing loan does not correlate with borrower credit risk and thus functions like an idiosyncratic credit demand shock. Interacting this demand shock with the lender’s HVCRE supply shock, I can tightly identify the effect of HVCRE capital requirements on property-level improvement activity. I find that a policy-induced increase in credit supply raises the annual probability of an improvement by 46% (1.2 pps).

Finally, I study a shock which increases the supply of equity financing for private equity real estate funds. This is the first paper to study how changes in the supply of equity, in contrast to debt, affect housing markets. These funds — which comprise half of aggregate investment in rental markets — typically take an equity stake in residential investment projects. They raise money in discrete rounds and are reliant on large institutional limited partners, of which public pensions are 30%. Public pensions are known to take greater risk the more underfunded they are, and this behavior is especially strong when safe yields are low (e.g. Andonov, Bauer and Cremers 2017; Mohan and Zhang 2014; Novy-Marx and Rauh 2011). Both patterns stem from government accounting rules which allow pensions to discount actuarial liabilities using the expected return on their assets. Applying these insights to real estate, I show how more underfunded pensions respond to declining safe yields by reallocating money from safe private equity funds, which pursue buy-and-hold strategies, to riskier funds, which perform improvements.

Thus, a “risk-up” shock channels equity financing toward improvements from buy-and-hold projects, just as the prior “risk-down” shock channels bank credit toward improvements from construction. To measure the real effects of this “risk-up” shock, I use the fact that fundraising relationships are sticky. I find that real estate fund managers who were historically more reliant on
underfunded public pensions increase their real investment in improvements over 2010-2016 relative to other managers. A back-of-envelope calculation suggests that private equity investment in improvements would have been 56% less had all public pensions been fully funded in 2008. Taken alongside the credit supply results, this finding shows how intermediary portfolio reallocation has had significant effects on housing quality through the distribution of resources across different types of residential investment.

This paper contributes to literatures on both financial intermediation and housing markets. First, viewing construction, improvements, and buy-and-hold projects as separate technologies that firms (i.e. property investors) use to produce housing services, I show that intermediaries affect the allocation of inputs across types of production. This finding most directly complements an empirical literature on how intermediary-provided financing affects the overall level of firm inputs, such as labor or investment (e.g. Chodorow-Reich 2014; Greenstone, Mas and Nguyen 2015; Gan 2007). In addition, recent papers have found that house prices affect the distribution of credit across sectors (e.g. Chakraborty, Goldstein and MacKinlay 2018; Martin, Moral-Benito and Schmitz 2018), to which this paper contributes by showing how intermediary-specific shocks affect the distribution of real activity. Methodologically, this paper is among a set of recent papers using capital requirements to obtain identification (e.g. Blattner, Farinha and Rebelo 2018; Kojen and Yogo 2015), and it is among the first to study firm-level effects of regulations associated with Dodd-Frank. In particular, I use the fact that capital requirements shifted bank versus nonbank market shares across loans for different purposes, contributing to a literature on nonbank lenders (e.g. Kim et al. 2018; Buchak et al. 2018; Fuster et al. 2018; Irani et al. 2018; Gete and Reher 2018b).  

On the housing side, a large literature has studied the effect of capital markets on housing markets in the owner-occupied sector, and this paper is among a few to study that effect in the rental market. It is also among the first to explicitly study the supply of housing quality, complementing research on households’ demand for living in different quality segments (e.g. Landvoigt, Piazzesi and Schneider 2015; Piazzesi, Schneider and Stroebel 2017) or improving their own home (Benmelech, Guren and Melzer 2017). In particular, I relate to the gentrification literature by showing how renovation is

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4 At a conceptual level, the idea that firms select technologies of different risk levels supports a common assumption in production based asset pricing (e.g. Cochrane 1993; Belo 2010; Jermann 2013). In addition, the results stem from shocks to desired portfolio risk, which suggests that intermediary risk appetite can affect quantities in real asset markets, complementing a literature studying price effects in liquid asset markets (e.g. Adrian, Etula and Muir 2014; He, Kelley and Manela 2017; Pflueger, Siriwardane and Sunderam 2018).

5 Gete and Reher (2018a) is an exception which studies how mortgage supply affects rent growth. A short list of examples focused on the owner-occupied sector includes Fote, Loewenstein and Willen (2016), Adelino, Schoar and Severino (2016), Mian and Sufi (2009), and Glaeser, Gottlieb and Gyourko (2013).
financed (e.g. Guerrieri, Hartley and Hurst 2013; Couture et al. 2018). Finally, a number of recent papers have studied how urban policies, such as tax credits or rent control, affect rental markets (e.g. Diamond and McQuade 2018; Diamond, McQuade and Qian 2018), and this paper shows how the rental market is also affected by financial regulation.

The plan for the remainder of the paper is as follows. Section 2 begins with some motivating background facts. Section 3 computes the wedge between observed and quality-adjusted rent, discussing implications for inequality, asset pricing, and official rent indices. Section 4 studies the effect of regulatory capital requirements on the supply of bank credit for improvements, and Section 5 studies the analogous effect of pension risk taking on the supply of private equity financing. Section 6 concludes.

2 Facts and Framework

This section aims to make the paper’s argument as clear as possible and to provide basic background for the main analysis. After a brief description of the data, I document recent trends in quality improvement activity and propose a simple framework to understand them.

To be clear on terminology, I define “quality” as a structural feature of a shelter. I will use “improvement” as the general term for an increase in quality, which will include large-scale projects (i.e. renovations) as well as small-scale ones (i.e. installing an air conditioner). By “housing unit”, I mean the individual home or apartment, which differs from the “property” for the case of multifamily properties. Finally, I will use “effective rent” or “quality-adjusted rent” as synonymous terms for a quality-adjusted rent index, whose precise definition will vary based on the index, and these terms are to be contrasted with “observed rent”.

2.1 Data

I rely on three main datasets and numerous auxiliary ones which are discussed in turn. The full details are in Appendix B. The datasets vary in observational unit and sample period, and I will be clear about which dataset I am using for a given analysis.

The first main dataset is the Census Bureau’s American Housing Survey (AHS). The AHS is a longitudinal dataset covering a representative sample of U.S. housing units every 2 years, and, because
Note: Panel (a) plots the percent of multifamily units renovated each year. Panel (b) plots average real (i.e. excess-CPI) zip code level multifamily rent growth by rent quintile. The plot sorts zip codes into quintiles by rent relative to the MSA-year mean. Data in panels (a) and (b) are from Trepp and Zillow, respectively.

of sample redesigns in 1995 and 2015, my data span 1997-2013. While lacking geographic information, the AHS dataset is attractive because of its panel structure and information about specific structural features. The second dataset comes from Trepp LLC and covers units in multifamily properties over 2010-2016. The underlying data come from multifamily mortgage servicing records for loans which were eventually securitized, and have detailed information about property improvements. The third dataset is from Preqin, and it covers fundraising and investment activity by private equity real estate funds.

### 2.2 Facts

Figure 1 documents two important trends in housing quality: (a) a large increase in renovation activity since the crisis and (b) a negative cross-sectional correlation between housing quality and rent growth. First, panel (a) plots the percent of multifamily housing units renovated each year, obtained from Trepp. This annual probability of renovation vigorously recovered from its 2008 low and surpassed its pre-crisis high by 2014. Appendix Figure A2 replicates this finding using aggregate investment in residential improvements. Relatedly, Appendix Figure A3 documents a reduction in the rate at which rental units drift down the quality ladder, which I measure by income filtering.
(Rosenthal 2014).

Panel (b) studies the cross-section of rent growth across quality segments. Using Zillow’s zip code multifamily rent index, I sort zip codes into quintiles by level of rent relative to the MSA-year average, intended to proxy for quality segment. Next, I plot annualized real rent growth for each segment. While real rent grew at least 1.6% per year for the bottom 4 quintiles, it actually fell at a rate of 0.4% for the top quintile. This pattern is robust to various other measures of quality segment, shown in the appendix.7 These observations are consistent with improvement activity constituting a positive supply shock to high quality units, and conversely for low quality ones. The Online Appendix, found here, documents a richer set of facts which also help inform subsequent identification strategies.

2.3 Framework

I now develop a simple framework to clarify the paper’s core argument. Consider an economy with a mass of renting households deciding where to live. Investors own the housing stock which, for the sake of argument, is in fixed supply and does not depreciate. Housing units vary in their quality, and the initial distribution of housing quality is shown by the blue dashed bell curve \( \text{Distribution}_0 \) in Figure 2. The equilibrium relationship between a unit’s quality and its observed rent (i.e. the rent schedule) is shown by the upward sloping blue line \( \text{Schedule}_0 \). The average housing unit in the initial distribution has quality \( \text{Quality}_0 \), and its rent is \( \text{Rent}_0 \).

Investors have startup funds which they can use to transform low quality units into high quality ones. Now, imagine an increase in startup funds because, say, unmodeled financial intermediaries would like to invest in improvement projects. This shock results in a rightward shift in the distribution of quality to the red bell curve \( \text{Distribution}_1 \). In addition, improvement activity reduces the equilibrium rent on high quality units by increasing their relative supply, and vice versa for low quality units. The result is a flattening of the rent schedule to \( \text{Schedule}_1 \) as the marginal price of quality has fallen, consistent with Figure 1b.

After the shock, the average unit has better quality, \( \text{Quality}_1 \), and correspondingly it commands a higher rent, \( \text{Rent}_1 \). However, from the standpoint of household surplus, a more appropriate object is average rent under the initial distribution of quality. This effective rent, denoted \( \text{Rent}^*_1 \), has fallen because of the quality supply shock. Summarizing, a reallocation of financing toward improvement

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7 Appendix Figure A5 replicates the figure using professional property inspection ratings, which rank a unit’s quality relative to the rest of the market. As a natural consequence, Appendix Figure A4 shows how the cross-sectional distribution of log rent became more compressed over this period.
projects shifts the distribution of quality toward the upper end of the quality ladder, raising observed rent but lowering effective rent.

3 Mapping Improvements to Rent

This section performs a quality adjustment, and its purpose is to motivate the rest of the paper. Specifically, I map improvements into the space of rent growth — a more interpretable metric — and provide a toolkit for studying how the supply shocks studied in Sections 4 and 5 affect household surplus. To be clear, by “effect of quality” I mean the difference between observed and effective rent. I compute effective rent using two methods: a hedonic approach in Section 3.1 and a structural approach in Section 3.2. Section 3.3 briefly discusses cross-sectional implications of improving quality.

3.1 Hedonic Index

Following a tradition in the housing literature summarized by Sheppard (1999), I first construct a hedonic rent index. The logic of this approach is to hold the cross-sectional distribution of housing quality fixed and ask how the average rent in this distribution has grown over time. Thus, the notion of effective rent is the expenditure required to live in a home with the same set of structural features.
The AHS data are ideal for this exercise because of their detail on property features and inhabitant characteristics. The data are also representative of the entire U.S. housing stock, and they allow me to study single family rentals, whereas for the rest of the paper my focus is on the multifamily sector. As mentioned in Section 2.1, my data end in 2013 because of a sample redesign, and so, given my emphasis on the post-crisis period, I construct a hedonic index over 2007-2013.

Since my interest is in quality improvements to a given housing unit, I estimate the following pricing equation in differences

$$\Delta \log (\text{Rent}_{i,t}) = \beta F \Delta F_{i,t} + \alpha_i + \alpha_t + u_{i,t},$$

(1)

where $i$ and $t$ index housing units and years, $\Delta \log (\text{Rent}_{i,t})$ is the change in log rent, and $\Delta F_{i,t}$ is a vector of indicators for the installment of features $f_{i,t} \in F_{i,t}$. Thus, (1) combines elements of repeat-“sale” (i.e. repeat-rent) and hedonic indices, which has several well-known advantages (Meese and Wallace 1997). All changes are over 2 years because the AHS is administered biennially. Finally, the housing unit and year fixed effects $\alpha_i$ and $\alpha_t$ account for the possibility that improvements only occur in some locations or in certain years.

Given the estimates from (1), shown in Appendix Table A1, I compute a unit’s improvement adjusted rent as

$$\text{Rent}_{i,t}^H = \text{Rent}_{i,t_0} \times e^{\sum_{t_0+1}^{t} [\Delta \log (\text{Rent}_{i,\tau}) - \beta F \Delta F_{i,\tau}]}$$

(2)

where $\text{Rent}_{i,t_0}$ is the property’s rent in the base period $t_0$. Then, I define the hedonic index $\pi_t^H$ as the normalized average of $\text{Rent}_{i,t}^H$ across rental units $i \in I$,

$$\pi_t^H = \frac{\sum_{i \in I} \text{Rent}_{i,t}^H}{\sum_{i \in I} \text{Rent}_{i,t_0}}.$$ 

(3)

As described in Appendix B, I drop units that experienced a change in tenure (e.g. “condo conversions”) from my analysis. The aggregation in (3) has the same basic form as that used by the BLS after accounting for the fact that I work at a biennial frequency (Gallin and Verbrugge 2007).

Figure 3a summarizes 2007-2013 annual growth in $\pi_t^H$ and other related indices. The baseline hedonic index, shown in the center of the figure, saw 0.6% real growth. Moving to the left, I perform

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8 The features in $F_{i,t}$ are: a dishwasher, trash compactor, garbage disposal, washing machine, dryer, air conditioning (A/C), central A/C conditional on installing A/C, and log square feet. For the case of square feet, $\Delta f_{i,t}$ is the increase in log square feet and not an indicator.
Figure 3: Summary of Hedonic Rent Index

Note: Panel (a) plots average annual growth in real (i.e. excess-CPI) rent over 2007-2013 for various rent indices. Unadjusted denotes the growth in average rent. Age Adjusted rent growth performs an age adjustment similar to that used by statistical agencies and is described in Appendix C. Baseline denotes growth in the hedonic index from (3). Time-Varying Price denotes baseline growth after allowing the coefficients in (1) to vary by year. Controlling for Income denotes baseline growth after controlling for the change in the inhabitant’s income percentile. Panel (b) plots unadjusted and hedonic real rent growth for various periods allowing the coefficients in (1) to vary by year. Data are from the AHS.

an age adjustment similar to that used by the BLS and described in Appendix C. This gives a real growth rate of 1.8%, slightly higher than the 1.7% growth in unadjusted average rent. The overall level of rent growth is close to what one would expect given growth in the CPI’s rent of primary residence over the period.\(^9\) Quantitatively, the result suggests that quality improvements can account for 65\% (\(0.7 - 0.6\) / 0.7) of real rent growth, relative to a counterfactual of no such improvements. The remaining 0.6 pps (35\%) of effective rent growth reflect, for example, growth in the value of land.

The indices to the right of the baseline in Figure 3a perform two robustness checks. First, I reestimate (1) after allowing the price vector \(\beta F\) to vary by year. This results in a similar growth rate of 0.8\%. The slight increase means that the price of quality improvements is lower when more of them occur, consistent with the existence of supply shocks. Second, because this is a measurement exercise, the primary threat to interpretation is that the improvements \(\Delta F_{i,t}\) correlate with unobserved shocks (e.g. renter demand) unrelated to quality that would have raised rent anyway. Fortunately, I observe

\(^9\)Average annual real growth in the CPI’s rent of primary residence was 0.7\%, which is within a standard range of the growth rates in Figure 3 after accounting for the fact that rent growth in the AHS is on average 0.8 pps higher than CPI rent growth (McCarthy, Peach and Ploenzke 2015).
changes in the inhabitant’s income percentile and, although non-standard in the classic hedonic tradition, I can control for them in the pricing equation. This leaves the growth rate unchanged at 0.6%, which is again consistent with a role for supply.

Figure 3b utilizes the AHS data’s pre-crisis coverage and plots unadjusted and hedonic real rent growth for various periods. Unlike the baseline window, there is little difference between unadjusted and quality-adjusted rent growth over 1997-2007 or the 2001-2005 boom. This finding motivates my focus on the post-crisis period, during which the supply shocks studied in Sections 4 and 5 take place. Lastly, Appendix Figures A6 and A8 study the contributions of particular improvements.\textsuperscript{10}

3.2 Structural Index

There are three advantages to introducing a preference structure. First, a structural methodology can be applied to datasets, such as the Trepp dataset, which lack detailed information on hedonic characteristics but still enable housing units to be ranked by quality (e.g. via property inspection ratings). Second, a structural approach enables inferences about time variation in absolute quality. For example, it can account for how dishwashers installed in 2016 may be better (e.g. less noisy) than those installed in 2010. This is more difficult to do with a hedonic approach because of practical limitations on the set of observables. Third, a structural approach allows rent growth in different quality segments to have a different impact on the resulting index, with weights based on households’ revealed preference. For example, if supply-driven improvements make high quality units less expensive, the index weights top-tier rent growth according to households’ implied willingness to move up the quality ladder.

In terms of underlying logic, a hedonic index holds the distribution of quality fixed, whereas the structural index holds the average household’s utility fixed and asks how she must be compensated for changes in rent across the quality ladder. In particular, the notion of effective rent is the expenditure required to obtain a unit of housing utility (i.e. compensating variation). For reasons of space, I defer details on setup and implementation to Appendix C.2. I compute the structural index using the Trepp data and summarize the results in Appendix Figure C2. Briefly, the baseline structural index saw real growth of 0.2% over 2010-2016, compared to 1.4% growth in observed average rent. This implies that improving quality can account for 86% (\(\frac{1.4-0.2}{1.4}\)) of 2010-2016 real rent growth.\textsuperscript{10}

\textsuperscript{10}Figure A6 decomposes the effect of quality into its constituent improvements for the 5 most important contributors. Figure A8 plots the correlation between the variables used to estimate (1) and their first four principal components, and it suggests that the seemingly mundane improvements used in this exercise capture unobserved, larger scale projects.
3.3 Cross-Sectional Implications of Improving Quality

I conclude this section by briefly discussing cross-sectional implications of improving quality. These extensions provide helpful context for designing the identification strategies of Sections 4 and 5. To keep the paper focused, I defer details to Appendix C, which also discusses implications for official price indices and innovation in property management (“proptech”).

First, Appendix C.3 shows that investors concentrate improvements in high (i.e. above-median) income markets. Based on the structural index, which measures compensating variation and thus reflects changes in household surplus, high income markets experience significantly greater gains from quality. In part, this is because the estimated willingness to pay for quality is greater in high income areas. This finding is consistent with a Q-theory logic whereby investors make improvements where the equilibrium price of quality (i.e. “price of capital”) is highest. This finding also suggests that concerns over a shortage of low quality housing would apply more to high income markets, and specifically to low income households within those markets (JCHS 2017).

Second, Appendix C.4 shows that investors make improvements where valuations are initially lower. The cap rate (i.e. dividend price ratio) forecasts lower future rent growth once one adjusts for quality improvements, consistent with standard asset pricing logic (Campbell and Shiller 1988).

4 Effect of Credit Supply on Quality Improvements

In the remainder of this paper, I study how changes in the supply of financing have affected quality improvement activity and, through it, effective rent. I begin with a shock to the supply of credit for improvement projects that was generated by changes in bank capital requirements. Section 5 studies a separate shock to the supply of equity financing. In both cases, the shock involves a reallocation of financing due to a change in desired portfolio risk. Here, there is an effective “risk-down” shock, as now described.

In January 2015, U.S. bank regulators began to require that High Volatility Commercial Real Estate (HVCRE) loans bear a 150% regulatory capital risk weight, compared to a 100% weight beforehand.\textsuperscript{11} HVCRE loans are for the “development or construction of real property”, which I will simply refer to as “construction”.\textsuperscript{12} By contrast, loans for “improvements to existing income-

\textsuperscript{11}If the regulatory minimum capital ratio is $K$ (e.g. 6%), this means that the bank must reserve $1.50 \times K$ of equity capital for every $1$ of HVCRE credit extended whenever the regulatory minimum is binding.

\textsuperscript{12}The full definition of an HVCRE loan is “a credit facility that, prior to conversion to permanent financing, finances or has financed the acquisition, development, or construction (ADC) of real property”. In addition, the loan must
producing real property” were not subject to this increase and retained the substantially more modest weight of 100%.\textsuperscript{13}

Under some theoretical assumptions, HVCRE regulation should increase the supply of credit for improvement projects. First, and most fundamentally, suppose the Modigliani-Miller theorem does not hold and that equity capital is costly for banks. Then binding HVCRE capital requirements increase banks’ cost of financing construction loans. Next, suppose banks can substitute loanable funds between different project types. Then the resulting cost wedge incentivizes banks to transfer credit to improvements from construction. Of course, the extent to which banks make such a substitution depends on the parameters of their cost and production functions (e.g. Greenwood et al. 2017). Third, banks can, in principle, modify their securitization behavior to reduce the effective capital requirement. For instance, they can securitize loans more quickly, lowering the warehouse period during which the standard risk weight binds.\textsuperscript{14} If banks do not respond entirely along this margin, then HVCRE regulation can lead to a transfer of loanable funds to improvement projects. Anecdotally, many banks indeed shifted resources to loans unaffected by HVCRE regulation and curtailed their lending to developers (Mortgage Bankers Association 2018), consistent with the aggregate behavior shown in Appendix Figure A9.

Viewing HVCRE regulation as a positive credit supply shock to improvement projects, there are three key details which allow me to estimate the real effects of this shock. First, specialty nonbank lenders play an important role in multifamily mortgage markets, and they are not subject to capital requirements.\textsuperscript{15} Second, underwriting loans for improvement or construction projects requires knowledge of local markets (Chandan and Zausner 2015), which leads to sticky borrower-lender relationships, as documented shortly. Third, there was no ex ante adjustment, reflecting

\textsuperscript{13}\footnotesize{There was initially confusion over what constituted an HVCRE loan. The Clarifying Commercial Real Estate Loans Act (H.R. 2148), passed by the House of Representatives in 2017, helped clarify the distinction between loans for construction versus improvements. Note that HVCRE regulations were later modified as part of the Senate’s Economic Growth, Regulatory Relief, and Consumer Protection Act (S. 2155) in May 2018, which made substantial changes to the Dodd-Frank regulatory architecture.}

\textsuperscript{14}\footnotesize{Securitization dilutes, but not eliminate, capital requirements through the risk retention ratio. The risk retention ratio for HVCRE loans is 5%, but, once securitized, the capital risk weight on the retained portion of these loans is no longer the pre-securitization weight (e.g. 150%) and, depending on their assessed risk, can be marked up to 1,250% (Chabanel 2017). See Willen (2014) for more discussion of the lending incentives associated with risk retention ratios.}

\textsuperscript{15}\footnotesize{This market structure is related to the Designated Underwriting Servicers (DUS) program, which allows only certain lenders the privilege of selling multifamily mortgages to the GSEs. Of these lenders, only 40% are banks or direct subsidiaries of bank holding companies. The largest specialty nonbank lenders by origination volume over 2012-2016 were CBRE Capital, Berkadia, Holliday Fenoglio Fowler, Walker & Dunlop, and Berkeley Point Capital. Specialty nonbank lenders accounted for 33% of outstanding balances in 2010 in my data.}
confusion over the precise details of implementation (Mortgage Bankers Association 2018) as well as the grandfathering of pre-2015 loans.\textsuperscript{16}

My goal is to assess the aggregate effect of HVCRE regulation on improvement activity and effective rent. As a necessary first step, I estimate the lender-level effect in Section 4.1, followed by a property-level specification in Section 4.2. Then I turn to the main county-level specification in Section 4.3 and discuss the aggregation procedure in Section 4.4. Unless otherwise stated, data used in this section come from Trepp.

4.1 Lender-Level Effect

I estimate the lender-level effect using two separate strategies. My first approach looks within the same lender and year and asks whether lenders more exposed to HVCRE regulation, namely banks, shift their lending from construction to improvement projects. Separating loans by the type of project they finance allows me to include lender-year fixed effects, and I estimate the following triple difference-in-difference specification over 2011-2016,

\[
y_{k,\ell,t} = \beta (\text{Bank}_{\ell} \times \text{Post}_{t} \times \text{Imp}_{k}) + \gamma (\text{Bank}_{\ell} \times \text{Imp}_{k}) + \alpha_{\ell,t} + \alpha_{k,t} + u_{k,\ell,t},
\]

where $k$, $\ell$, and $t$ index loan purpose, lender, and year.\textsuperscript{17} $\text{Bank}_{\ell}$ indicates if the lender is a bank, $\text{Post}_{t}$ indicates whether the HVCRE requirements are in place (i.e. $t \geq 2015$), and $\alpha_{\ell,t}$ and $\alpha_{k,t}$ are lender-year and purpose-year fixed effects. $\text{Imp}_{k}$ indicates if the purpose is an improvement, where the set of loan purposes are improvement or construction.\textsuperscript{18} The parameter of interest in (4) is $\beta$, which captures the triple difference between treated loan types ($\text{Imp}_{k}$) originated by treated lenders ($\text{Bank}_{\ell}$) during the treatment period ($\text{Post}_{t}$), and the counterfactual purpose-lender-years. For the rest of the paper, I economize on notation by repeatedly using $\beta$ to denote the treatment effect in a regression equation. The outcomes $y_{k,\ell,t}$ are the log number of loans originated or dollar volume for

\textsuperscript{16}HVCRE regulation was announced in 2013 as part of the U.S. implementation of Basel III. It is important to emphasize industry confusion as well as grandfathering since, as discussed in footnote 13, a formal clarification of HVCRE regulation including a full description of grandfathering status did not come until 2017.

\textsuperscript{17}To avoid overweighting idiosyncratic shocks of small lenders, observations in (4) and the following specification (5) are weighted by multifamily mortgage market share over 2011-2016.

\textsuperscript{18}Improvements are not listed as a category of loan purpose, so I classify loans as financing an improvement if they were originated within 1 year of renovation. I classify loans as financing construction if their stated purpose was construction, or if they were originated within 3 years of construction. The latter restriction accounts for the fact that most loans for construction have a construction-to-permanent financing structure, where the lender provides a short term variable rate loan that converts to a long term loan once the project has stabilized, and such loans are more difficult to securitize prior to conversion (Black, Krainer and Nichols 2017).
purpose $k$.$^{19}$

The main drawback to (4) is that the lender-year fixed effects prohibit inference about whether banks actually originated more improvement loans, or whether they simply stopped lending against new construction. This feature was intended to absorb confounding shocks to the overall level of lending, but its restrictiveness motivates a specification that also uses variation across lender-years. I next estimate the difference-in-difference specification

$$Y_{\ell,t} = \beta (\text{Bank}_\ell \times \text{Post}_t) + \alpha_t + \alpha_\ell + \gamma X_{\ell,t} + u_{\ell,t}, \tag{5}$$

where the notation is the same as in (4), although observational units are now lender-years, as opposed to purpose-lender-years. Identification comes from comparing treated lenders (i.e. banks) with nontreated lenders before and after the introduction of HVCRE regulation. The controls in $X_{\ell,t}$ absorb some of the variation that would otherwise be subsumed by $\alpha_\ell$ in (4). Omitted variables related to a lender’s business model may still lead to bias in (5). However, Appendix Figure A10 shows that banks and nonbanks have similar portfolio characteristics in terms of observed loan performance and property features, which suggests the scope for such bias is small.

The results of (4) are in columns 1-2 of Table 1. The point estimate in column 1 suggests that banks increase the ratio of improvement to construction loans by 28% relative to nonbanks after HVCRE regulation is introduced.$^{20}$ Figure 4 illustrates the effect over time by replacing Post$^t$ with a series of year interactions and reestimating column 1. After the regulation is introduced, banks significantly tilt their portfolio toward improvement loans relative to nonbanks, while there is no significant ex-ante adjustment. Returning to Table 1, the magnitude is larger when studying origination volume in column 2. This may reflect economies of scale which incentivize improvements on larger properties, as well as a scaling back of construction lending along the intensive margin.

Columns 3-5 of Table 1 report the results of (5). My outcome of interest is the log number of renovated units financed by new loans. Studying this outcome facilitates continuity with the county-level analysis in Section 4.3, and it contributes to the bank lending literature by directly studying project-level outcomes, on which there is comparatively little research. The estimate in column

---

$^{19}$I follow standard practice and add 1 to the variable before taking the log whenever the variable can equal 0. For example, some lenders do not originate a construction loan every year. The estimates are robust to the choice of normalization.

$^{20}$The point estimate suggests that a 40% ($\log(1.5) = 0.59$) reduction in the relative cost of capital for a particular loan type leads to a 28% increase in relative originations for that loan type. In other words, the elasticity of substitution is 0.7 ($0.28 / 0.40$). Originations are normalized to have unit variance within lender-purposes to account for different business models.
### Table 1: Improvement Financing by HVCRE-Affected Lenders

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Outcome:</td>
<td>log (Loans(_{k,\ell,t}))</td>
<td>log (Volume(_{k,\ell,t}))</td>
</tr>
<tr>
<td>Bank(<em>{\ell} \times Post</em>{t} \times Imp_{k})</td>
<td>0.281** (0.142)</td>
<td>5.329** (2.024)</td>
</tr>
<tr>
<td>Bank(<em>{\ell} \times Post</em>{t})</td>
<td>1.210** (0.528)</td>
<td>1.142** (0.521)</td>
</tr>
<tr>
<td>Bank(<em>{\ell} \times Post</em>{t} \times Sec\ Lag_{\ell})</td>
<td>1.947** (0.841)</td>
<td></td>
</tr>
<tr>
<td>Lender-Year FE</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Purpose-Year FE</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Bank × Imp</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Lender FE</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Year FE</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Lender Controls</td>
<td>No</td>
<td>Yes</td>
</tr>
<tr>
<td>Sec Lag-Year FE</td>
<td>No</td>
<td>No</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.763</td>
<td>0.800</td>
</tr>
<tr>
<td>Number of Observations</td>
<td>966</td>
<td>966</td>
</tr>
</tbody>
</table>

Note: Subscripts \(k, \ell\) and \(t\) denote loan purpose, lender, and year. Columns 1-2 estimate (4) and columns 3-5 estimate (5). Observations in columns 1-2 and columns 3-5 are purpose-lender-years and lender-years, respectively, weighted by the lender’s multifamily mortgage market share over 2011-2016. Bank\(_{\ell}\) denotes if lender \(\ell\) is a bank. Post\(_{t}\) indicates if \(t \geq 2015\). Imp\(_{k}\) indicates if the purpose is an improvement. The set of loan purposes are improvement or construction. Bank × Imp is the interaction between Bank\(_{\ell}\) and Imp\(_{k}\). Loans\(_{k,\ell,t}\) is the number of loans for purpose \(k\) originated by \(\ell\) in \(t\), and Volume\(_{k,\ell,t}\) is corresponding dollar volume. Renovated Housing Units\(_{\ell,t}\) is the number of renovated units financed by a new loan by lender \(\ell\) in \(t\). Lender controls are principal-weighted averages of the following characteristics of existing loans: loan-to-value ratio, debt service coverage ratio, adjustable rate mortgage share, and share of delinquent loans. Sec Lag\(_{\ell}\) is the average number of months between origination and securitization for loans originated by \(\ell\) before 2011, normalized to have zero mean and unit variance. Sec Lag-Year FE are interactions between Sec Lag\(_{\ell}\) and year indicators. The sample period is 2011-2016. Standard errors clustered by lender are in parentheses. Data are from Trepp.

3 suggests that banks finance 120% more improvements relative to nonbanks in the post-HVCRE period, and the results are similar after including lender controls in column 4.

Column 5 tests the theory that banks’ response to the regulation depends on their securitization technology. I interact the treatment variable, Bank\(_{\ell} \times Post_{t}\), with a measure of the lender’s securitization sluggishness in the pre-2011 period, Sec Lag\(_{\ell}\), normalized to have zero mean and unit variance.\(^{21}\) Banks with a higher value of Sec Lag\(_{\ell}\) have a longer typical warehouse period, and, consistent with the theory described above, their estimated response to the regulation is much stronger.

\(^{21}\)Sec Lag\(_{\ell}\) is the average number of months from origination to securitization for loans \(\ell\) originated before 2011.
Figure 4: Improvement Financing and the Timing of HVCRE Regulation

![Graph showing Improvement Loans and HVCRE Regulation](image)

Note: This figure plots the estimated coefficients from a regression similar to that from column 1 of Table 1. The regression is of log originated loans on the interaction between: (a) an indicator for whether the lender is a bank, (b) an indicator for whether the loan’s purpose is an improvement, and (c) a series of year indicators. The rest of the specification is the same as column 1 of Table 1. The gray region indicates the period when HVCRE regulations are in place. Brackets are a 95% confidence interval with standard errors two-way clustered by lender and year.

4.1.1 Evidence from the Syndicated Loan Market

Appendix C.7 performs an analogous exercise in the syndicated loan market to assess the external validity of the results. Secured, syndicated loans are an important source of financing for large-scale residential investment projects, and these loans were also affected by HVCRE regulation (Guggenheim and Seiden 2017). An advantage to this research design is the ability to control for unobserved borrower-lender matching, which is more difficult in the multifamily mortgage market because most borrowers are small. While I do not observe project level outcomes, the results suggest that treated lenders make fewer loans to firms that specialize in construction.

4.2 Property-Level Effect

Lender-level project reallocation is necessary, but not sufficient, for HVCRE regulation to meaningfully affect real improvement activity. To illustrate why, suppose that borrowers can costlessly substitute across lenders. Then a borrower that typically does business with a nonbank and wishes to perform an improvement may now seek more liberal bank credit. This behavior would lead to changes in the market share of different intermediaries, even if the overall increase in real improvement
activity is quite small.

To estimate these real effects, I shift the unit of analysis to the property or county-level. This shift requires cross-sectional variation in exposure to treated lenders. Here, I make the realistic assumption that borrowers have limited ability to substitute across lenders, reflecting, for example, the combination of information asymmetries with screening or monitoring costs (e.g. Diamond 1991; Sharpe 1990). Appendix C.1 investigates this assumption, and it provides evidence of significant relationship stickiness in multifamily mortgage markets. Specifically, the probability a borrower turns to her former lender for her next loan is 52 pps greater than what one would predict based on the lender’s market share. Relationship stickiness forms the basis for the remaining analysis. In fact, such stickiness is not limited to the multifamily mortgage market, but also appears in other areas of real estate finance, like private equity real estate fundraising, and I will use it again in Section 5.

I first study the real effects of HVCRE regulation at the property-level. This exercise is complementary to my main, county-level analysis in Section 4.3. I estimate the following difference-in-difference specification,

$$Y_{i,\ell,t} = \beta (\text{Bank}_\ell \times \text{Post}_t) + \alpha_{c(i),t} + \alpha_{i,\ell} + \gamma X_{z(i),t} + u_{i,\ell,t},$$

where $i$, $\ell$, and $t$ index properties, lenders, and years, and Bank$_\ell$ indicates if the property owner’s lender is a bank. The county-year fixed effect $\alpha_{c(i),t}$ absorbs contemporaneous demand shocks, and the property-lender fixed effect $\alpha_{i,\ell}$ limits variation to the same relationship.$^{22}$ Some specifications also control for zip code dynamics $X_{z(i),t}$. The outcome $Y_{i,\ell,t}$ is a property-level measure of improvement activity.

The intuition for (6) is that borrowers with a treated lender (i.e. bank) should find it easier to obtain credit, in the form of a new loan, to make an improvement. Identifying the treatment effect $\beta$ in (6) requires a “parallel trends” assumption: bank and nonbank-financed properties do not differ in ways that would affect improvement activity after the introduction of HVCRE regulation. Explicitly, my identification assumption is

$$\mathbb{E} [\text{Bank}_\ell \times \text{Post}_t \times u_{i,\ell,t} | \alpha_{c(i),t}, \alpha_{i,\ell}, X_{z(i),t}] = 0,$$

where the conditioning arguments $\alpha_{c(i),t}$ and $\alpha_{i,\ell}$ make clear that identification comes from within the

$^{22}$It is possible that there are multiple borrowers within the same property-lender pair, but based on the 14% of the sample for which I observe the borrower’s identity, this is only the case for less than 1% of such pairs.
Table 2: Property-Level Improvement Activity and HVCRE Regulation

<table>
<thead>
<tr>
<th>Outcome:</th>
<th>Renovation(_{i,\ell,t})</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
</tr>
<tr>
<td></td>
<td>(2)</td>
</tr>
<tr>
<td>Bank(_\ell) \times Post(_t)</td>
<td>0.012**</td>
</tr>
<tr>
<td></td>
<td>(0.004)</td>
</tr>
<tr>
<td>Property-Lender-FE</td>
<td>Yes</td>
</tr>
<tr>
<td>County-Year FE</td>
<td>Yes</td>
</tr>
<tr>
<td>Zip Code Controls</td>
<td>No</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.308</td>
</tr>
<tr>
<td>Number of Observations</td>
<td>30733</td>
</tr>
</tbody>
</table>

Note: Subscripts \(i\), \(\ell\), and \(t\) denote property, lender, and year. This table estimates (6). Bank\(_\ell\) denotes if lender \(\ell\) is a bank. Post\(_t\) indicates if \(t\) is greater than or equal to 2015. The outcome is an indicator for whether a renovation occurs. Zip code controls are log average income and log number of tax returns, from the IRS, and log average rent, from Trepp. Observations are property-years. The sample period is 2011-2016. Standard errors clustered by property are in parentheses. Data are from Trepp.

same county-year bin and lending relationship. This assumption would be violated in the presence of a secular trend if, for example, banks specialize in properties that became more improvement-prone in 2015 or cater to borrowers who became more likely to invest in improvements. However, this is unlikely in light of Appendix Figure A10 and its associated discussion in Section 4.1, which provide evidence that banks and nonbanks have relatively similar portfolio characteristics. If anything, banks have a slight tilt toward smaller properties, which, as discussed below, are less attractive to renovate due to economies of scale.

Table 2 has the results of (6). The outcome \(Y_{i,\ell,t}\) is an indicator for whether a renovation occurs in \(t\), denoted Renovation\(_{i,\ell,t}\). The point estimates in columns 1 and 2 suggest that properties financed by treated lenders have a 1.2 pps higher probability of renovation after the introduction of HVCRE regulation. This effect is equal to 46% of the unconditional property-level probability of 2.6% over 2010-2016. Since all variation comes from within the same property-lender bin, the effect is identified by new loans on bank-financed properties. To make this channel more clear, Appendix Table A2 estimates an instrumental variables specification, using Bank\(_\ell\) \times Post\(_t\) to instrument for an indicator for whether a new loan was originated.

Stepping back, these property-level results how changes in credit supply can affect the number of completed projects by “firms” (i.e. property investors), complementing analogous results on firm hiring and investment referenced in the introduction.
4.2.1 Robustness: Idiosyncratic Payment Timing and Lender-Year Fixed Effects

Appendix C.8 confirms Table 2 through an identification strategy based on the product of policy-induced movements in the credit supply curve and idiosyncratic movements in the credit demand curve. What makes this approach unique is that both demand and supply shocks are effectively observed, so that identification can come from their product. By contrast, the conventional approach would be to remove demand shocks as a fixed effect (e.g. Khwaja and Mian 2008). These idiosyncratic demand shocks arise because of institutional features of the multifamily mortgage market which incentivize postponing improvements until the time of loan renewal. Moreover, the timing of renewal appears to be effectively exogenous. Thus, I can include lender-year fixed effects to tightly identify the effect of HVCRE regulation. Within lender-year bins, the regulation increases the effect of the idiosyncratic demand shock on the probability of renovation by 170%. Intuitively, because borrower-lender relationships are sticky, borrowers who would like to make an improvement are more likely to do so when their lender also experiences a positive credit supply shock.

4.3 Main Specification: County-Level Effect

While a property-level approach can identify localized effects of HVCRE regulation, it is unsuitable for drawing inferences about the aggregate effect. Thus, my main specification, which will facilitate the subsequent aggregation exercise, is a county-level difference-in-difference specification,

\[ Y_{c,t} = \beta (\text{Bank Share}_c \times \text{Post}_t) + \alpha_c + \alpha_t + \gamma X_{c,t} + u_{c,t}, \]  

(7)

where \(c\) and \(t\) index counties and years, and \(\text{Bank Share}_c\) is the share of multifamily mortgage balances held by banks in 2010. To interpret, treated counties are those where banks had a large market presence in 2010, and the treatment is the introduction of HVCRE regulation in 2015.\(^{23}\) The controls in \(X_{c,t}\) include state-year fixed effects and contemporaneous measures of local demand. The outcome \(Y_{c,t}\) is a measure of improvement activity.

As in any Bartik-style specification, the most important identification assumption is that treated cross-sectional units, here counties where banks have a large share of the multifamily mortgage market, are not predisposed to shocks to the outcome variable that coincide with the introduction of

\(^{23}\) The controls are log multifamily rent, log number of multifamily units, log real income for the surrounding MSA, log winter storms per multifamily unit, and the principal-weighted averages of the lender controls from Table 1. To avoid over weighting idiosyncratic shocks to small counties, I weight observations in (5) by average number of multifamily units over 2011-2016.
Figure 5: Geographic Distribution of Initial Bank Share

Note: This figure plots banks’ share of multifamily mortgage balances in 2010 across states. Data are from Trepp.

the treatment (Goldsmith-Pinkham, Sorkin and Swift 2018). In particular, the assumption is

$$E[\text{Bank Share}_c \times \text{Post}_t \times \alpha_{c,t} | \alpha_c, \alpha_t, X_{c,t}] = 0.$$  

This assumption would be violated if, for example, there is a secular trend in improvement activity and banks locate in high income markets which, per the discussion in Section 3.3, have a higher price of quality and would thus be disproportionately affected by the trend. Measuring Bank Share\textsubscript{c} with bank’s initial share of balances is a step toward addressing this concern since, unlike originations, balances reflect expectations that were formed longer in the past. More substantively, Figure 5 plots the geographic distribution of banks’ initial market share across states. The distribution is fairly uniform, and this uniformity is also borne out when zooming into the county-level within high growth states, shown in Appendix Figure A11.

The importance of borrower-lender relationships suggests that historical episodes may partly determine banks’ market share. To that end, Figure 6 investigates the source of treatment assignment. I divide counties into high and low exposure cohorts according to their initial exposure, Bank Share\textsubscript{c}. Then I perform a series of pairwise tests for a difference in mean in variables of interest, all normalized to have unit variance. Consistent with the geographic uniformity from Figure 5, there are few significant differences between the two cohorts. The most significant difference is in log deposit losses at FDIC insured banks during the 1980s, an era of widespread bank failures and commercial
Figure 6: County Characteristics by Initial Bank Share

Note: This figure plots the difference in mean for the indicated variable between counties with a high and low bank share of multifamily mortgage balances in 2010. High and low are defined according to the median across counties. Variables are normalized to have unit variance and demeaned by state. Bank Losses 80s are log cumulative deposit losses on FDIC insured banks between 1981 and 1991. College Education is the 2010 share of inhabitants with at least a bachelor’s degree, from the U.S. Census. House Price Drop is the percent decline in house prices from 2006-2012 based on Zillow’s Home Value Index. Saiz Elasticity is the Saiz (2010) elasticity of housing supply. The remaining variables are those from Table 3 averaged over 2011-2016. Observations are counties weighted by number of multifamily units. Brackets are a 95% confidence interval with heteroskedasticity robust standard errors. Data are from Trepp and other sources in Appendix B.

real estate speculation. Counties with a high value of Bank Share, experienced less severe crises in the 1980s, supporting the idea that Bank Share, is determined by historical episodes coupled with relationship stickiness.

Table 3 has the results of the baseline specification (7). The outcome in columns 1 through 3 is log number of renovated properties. The estimate in column 1 suggests that counties with a 10 pps higher initial bank share see a 2.3% increase in renovations after the introduction of HVCRE regulation. The estimate is similar after including state-year fixed effects and county controls in columns 2 and 3, respectively, and the standard error falls because these additional terms absorb much of the residual variation. Applying an Oster (2017) correction for omitted variable bias leads to a slightly higher point estimate of 0.298. The baseline result is also borne out when measuring

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24Some of the controls in $X_{c,t}$ are “bad” in the Angrist and Pischke (2009) sense that they are directly determined by the treatment Bank Share, × Post, but they help address some specific concerns with the identification assumption. For example, log average rent depends on improvement activity, but controlling for it accounts for the possibility that unobserved shocks to property values lead to cash-out loan renewals that provide credit for improvements. In any case, the results are similar with or without controls.

25The correction is based on a maximum R-squared of 0.75 and the default selection parameter $\delta = 1$. The corrected
Table 3: County-Level Improvement Activity and HVCRE Regulation

<table>
<thead>
<tr>
<th>Outcome Measure: Log (Renovation Measure)</th>
<th>Properties (1)</th>
<th>Housing Units (3)</th>
<th>Revenue (5)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Bank Share_c × Post_t</td>
<td>0.228**</td>
<td>0.279**</td>
<td>2.991**</td>
</tr>
<tr>
<td></td>
<td>(0.128)</td>
<td>(0.100)</td>
<td>(1.120)</td>
</tr>
<tr>
<td>Year FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>County FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>State-Year FE</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>County Controls</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.565</td>
<td>0.721</td>
<td>0.695</td>
</tr>
<tr>
<td>Number of Observations</td>
<td>3159</td>
<td>3159</td>
<td>3159</td>
</tr>
</tbody>
</table>

Note: Subscripts c and t denote county and year. This table estimates (7). Bank Share_c is banks’ share of multifamily mortgage balances in 2010. Post_t indicates if t is greater than or equal to 2015. The outcome is the log of a measure of renovation activity: columns 1-3 use the number of renovated properties, column 4 uses the number of renovated housing units, and column 5 uses aggregate revenue of renovated properties. County controls are log real income per capita for the surrounding MSA, log number of multifamily units, log multifamily rent, log winter storms per multifamily unit, and the principal-weighted averages of the mortgage controls from Table 1. Observations are county-years weighted by the average number of multifamily units over 2011-2016. The sample period is 2011-2016. Standard errors clustered by county are in parentheses. Data are from Trepp.

exposure using banks’ share of the office commercial mortgage market, shown in Appendix Table A3, and when including heterogeneous time trends for time-invariant county characteristics, shown in Appendix Table A5.

Figure 7 studies the effect over time by replacing Post_t with a series of year interactions and reestimating column 3. There is a slight negative, albeit statistically insignificant, pre-trend, which may reflect a more general phenomenon of nonbanks’ growing role in credit markets (e.g. Buchak et al. 2018). However, once HVCRE regulation is introduced, bank dominated counties see a substantial increase in improvement activity.

Returning to Table 3, column 4 studies the log number of renovated housing units. The result is qualitatively similar to its counterpart in column 3, and the larger point estimate likely reflects the economies of scale discussed in the context of Table 1. Finally, column 5 studies the log aggregate revenue of renovated properties. The larger point estimate suggests that the increase in the quantity of renovations is not offset by a reduction in their quality. This finding is consistent with the evidence of growth in the quality of improvements mentioned in Section 3.2. Appendix Table A4 provides additional intuition by using the treatment variable in Table 3 to instrument for log loan originations.

Next, I study how the treatment effect varies in the cross-section of counties. Based on a simple point estimate is 2.506 when using a maximum R-squared of 0.95.
Figure 7: Improvement Activity and the Timing of HVCRE Regulation

Note: This figure plots the estimated coefficients from a regression similar to column 3 of Table 3. The regression is of log renovated properties in a county on the interaction between: (a) banks’ share of multifamily mortgage balances in 2010 and (b) a series of year indicators. The rest of the specification is the same as column 3 of Table 3. The gray region indicates the period when HVCRE regulations are in place. Brackets are a 95% confidence interval with standard errors two-way clustered by county and year. Data are from Trepp.

I focus on the economic intuition associated with Bank Share$_c$ $\times$ Post$_t$ $\times$ Interaction$_c$ in Table 4. For example, column 1 shows how the policy’s effect is stronger in high income counties, which is natural given the greater price of quality in such counties discussed in Section 3.3. Column 2 shows how the effect is weaker where the average borrower has more distinct lending relationships, which inversely proxies for constraints on her ability to access credit. While not the focus of this paper, column 3 suggests that the effect is weaker where treated banks have more market power, proxied

26The characteristics are: average real income per capita over 2011-2016; the average borrower’s number of distinct lending relationships in 2010, weighted by principal; the Herfindahl-Hirschman index of multifamily mortgage balances among banks in 2010; and an indicator for whether the county is in a state where rent control or stabilization policies are in place. I normalize interactions to have zero mean and unit variance, except for the rent control indicator. I only observe the borrower’s identity for 14% of properties, and for the remaining 86% I predict the property owner’s number of distinct lending relationships from a linear regression on log property size, log loan balance, loan-to-value ratio, debt service coverage ratio, and indicators for whether the loan is adjustable-rate or 60+ days delinquent.
Table 4: Heterogeneous Effects Across Counties

<table>
<thead>
<tr>
<th>Outcome</th>
<th>log (Renovated Properties$_{c,t}$)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
</tr>
<tr>
<td>Bank Share$_c \times Post_t$</td>
<td>0.291$^{**}$</td>
</tr>
<tr>
<td></td>
<td>(0.110)</td>
</tr>
<tr>
<td>Bank Share$_c \times Post_t \times Interaction_c$</td>
<td>0.181$^*$</td>
</tr>
<tr>
<td></td>
<td>(0.104)</td>
</tr>
</tbody>
</table>

Interaction Variable | Income | Borrower Credit Access | Bank Concentration | Rent Control |
<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Year FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
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<tr>
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<td>Yes</td>
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<tr>
<td>Interaction-Year FE</td>
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<td>Yes</td>
<td>Yes</td>
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<td>0.706</td>
<td>0.708</td>
<td>0.705</td>
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<tr>
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<td>3159</td>
<td>3159</td>
<td>3159</td>
<td>3159</td>
</tr>
</tbody>
</table>

Note: Subscripts c and t denote county and year. The specification is the similar to column 2 of Table 3 with the inclusion of the following interaction terms: Income is real income per capital for the surrounding MSA averaged over 2011-2016; Borrower Credit Access is the average borrower’s number of distinct lending relationships in 2010, weighted by principal; Bank Concentration is the Herfindahl-Hirschman index of multifamily mortgage balances among banks in 2010; Rent Control indicates if the county is in a state where rent control or stabilization policies are in place. Interaction variables are normalized to have zero mean and unit variance, with the exception of the rent control indicator. Interaction-Year FE are a set of interactions between the indicated interaction variable and year indicators. Observations are county-years weighted by the average number of multifamily units over 2011-2016. The sample period is 2011-2016. Standard errors clustered by county are in parentheses. Data are from Trepp.

by a high Herfindahl-Hirschman index. This finding is consistent with a model of monopolistically competitive credit markets (e.g. Drechsler, Savov and Schnabl 2017). Finally, the effect appears weaker where there is rent control, shown in column 4. In Appendix Figure A12, I estimate a cross-sectional version of (7) to investigate nonlinear treatment effects.

4.4 Aggregate Effect

Bridging the gap with the first part of the paper, I map the county-level estimates to an aggregate effect on quality improvements and, through them, effective rent. My counterfactual is a world without HVCRE regulation in which capital requirements treat loans for all residential investment projects equally. As discussed below, the result should be interpreted as a partial equilibrium effect, which I will reweight according to sample representability.

The first question is how many fewer housing units would have been renovated under this counterfactual. Addressing this question requires two additional assumptions. The first assumption,
which relates to general equilibrium, is that the regulation does not affect improvement activity in counties with no initial bank exposure. The second assumption, which relates to sample representability, is that banks respond to the regulation in the same way for loans they eventually do and do not securitize.

The general equilibrium assumption may produce an overstatement of the regulation’s effect on improvement activity, since imperfectly segmented markets would imply a reallocation of improvements from low exposure counties to high exposure ones. In the case of housing markets, improvement activity in high exposure counties raises the supply of high quality units, possibly attracting high income households from low exposure counties (e.g. Diamond 2016). Based on Section 3.3, this migration would disincentivize improvement activity in low exposure counties, since the price of quality there has fallen. In the case of mortgage markets, despite relationship stickiness, regional investors may abandon improvement projects otherwise performed in low exposure counties to take advantage of more accessible credit elsewhere.

The sample representability assumption would most likely produce an understatement, but it is necessary because I only observe properties whose mortgages were eventually securitized. On one hand, if banks originated such mortgages with the intent of securitization, the full capital requirement binds during the warehouse period, which averages 15.7 months in the data, after which it can be diluted through a risk retention ratio. Alternatively, banks may have intended to hold these loans on balance sheet, but they were later purchased by a CMBS conduit. The second scenario is plausible for the 43% of bank loans that were sold at least 5 months after origination, which is at the upper end of the typical warehouse period (Echeverry, Stanton and Wallace 2016). The incentive to substitute toward improvement loans is stronger in the latter case, which implies that the observed effect is a lower bound on the true one. To substantiate this logic, Appendix C.9 performs several tests related to external validity, including use of a novel dataset on bank portfolio loans secured by multifamily properties. These tests suggest that Table 3 indeed provides conservative estimates.

Under these two assumptions, one can compute the aggregate effect of HVCRE regulation by

27Specifically, I reestimate a version of Table 3 using a unique dataset on bank portfolio loans. These data have some limitations that make them inappropriate for the baseline analysis. Most importantly, I cannot observe whether the loan financed an improvement and am constrained by a small sample size. These limitations aside, the results support the interpretation of the baseline estimates in Table 3 as conservative. Appendix C.9 also uses the within lender-year specification (4) to show that banks reduce the rate at which they securitized improvement loans following regulation, suggesting the in-sample estimates are biased toward zero.
reweighting the in-sample effect. Define the in-sample effect as the sum of county-level effects,

\[
\text{Effect}_{\text{Sample}} = \frac{\sum_{c} \sum_{t \geq 2015} \text{Renovated Housing Units}_{c,t} \times \left[ 1 - e^{-\beta_{\text{HVCRE}} \text{Bank Share}_{c}} \right]}{\sum_{c} \sum_{t \geq 2015} \text{Renovated Housing Units}_{c,t}},
\]  

(8)

where \(c\) and \(t\) index counties and years in the sample, \(\text{Renovated Housing Units}_{c,t}\) is the number of renovated units in the sample, and \(\beta_{\text{HVCRE}}\) is the estimate from column 4 of Table 3.\(^{28}\)

The implied in-sample effect equals 63\% of renovated units over 2015-2016. However, the regulation only affects mortgaged properties, which account for 70\% of multifamily renovations over 2010-2015 according to the Rental Housing Finance Survey (RHFS). One then reweights to obtain an aggregate, partial equilibrium effect equal to 44\% (0.63 \times 0.7) of multifamily renovations over 2015-2016. This magnitude is large, and it is consistent with a sharp aggregate reallocation from construction to improvement projects, shown in Appendix Figure A9, and the large increase in renovation probability shown in Figure 1a.

The second question is how much higher effective rent growth would have been in the absence of policy-induced quality improvements. I use the structural rent index from Section 3.2 to address this question, since it has the interpretation of household surplus and can be applied to the Trepp dataset, which lacks detailed hedonic characteristics. The index is effectively a weighted average of observed rent growth across quality segments.\(^{29}\) Thus, the policy-induced improvements can affect effective rent growth in two ways. First, they change the share of housing units in each quality segment, and thus the segment’s weight in the index. Second, they may change the distribution of observed rent across segments. Obtaining the counterfactual share of units is straightforward in light of (8). However, obtaining counterfactual observed rent is more complicated, since HVCRE regulation likely affects rent through channels unrelated to quality improvements, like discouraging construction. Given this tradeoff and my focus on the quality improvement channel, I work under the simplifying assumption that the policy does not affect observed rent. On one hand, this assumption may underestimate how much the regulation reduces effective rent growth, because Figure 2 illustrates how supply-driven improvements lower the rent on high quality units. By the same token, however, improvements increase rent on low quality units, leading to an overestimate.

With this simplifying assumption in mind, HVCRE regulation lowers effective rent growth by

\(^{28}\)I reason on improved housing units, as opposed to properties, because they are the more relevant level of analysis in the context of the quality adjustment exercise from Section 3. They can also be mapped to aggregate data.

\(^{29}\)I partition the market into quality segments using official property inspection ratings conducted by the Mortgage Bankers Association and Commercial Real Estate Finance Council (MBA/CREFC), which are regularly collected as part of the multifamily mortgage servicing protocol. See Appendix C.2 for details.
reallocating housing units to high quality segments, where observed rent growth was weakest, per Figure 1b. Define the county-level analogue of (8) as

$$\text{Effect}_{c}^{\text{Sample}} = \frac{\sum_{t \geq 2015} \text{Renovated Housing Units}_{c,t} \times \left[ 1 - e^{-\beta \text{HVCRE Bank Share}_c} \right]}{\sum_{t \geq 2015} \text{Renovated Housing Units}_{c,t}},$$  \hspace{1cm} (9)

which gives the share of renovated units in county $c$ attributable to HVCRE regulation. Next, I reassign a fraction $\text{Effect}_{c}^{\text{Sample}}$ of renovated units in each county to their initial quality segment. This delivers a counterfactual share of housing units in each segment. Using these shares, I recompute the structural index over 2015-2016, which yields effective rent growth of 1.7%. To place this number in perspective, observed rent growth over this period is 2.2%, and effective rent growth without the counterfactual renovations is 1.0%. Thus, the improvements generated by HVCRE regulation can account for 32% ($\frac{1.7-1.0}{2.2}$) of observed rent growth over 2015-2016. Viewed differently, the counterfactual improvements lowered effective rent growth by 41% ($\frac{1.7-1.0}{1.7}$), as illustrated in Figure 2.

5 Effect of Equity Supply on Quality Improvements

This section turns to the equity financing of improvement projects, and I study a shock to the supply of financing for private equity real estate funds. These funds constitute half of aggregate investment in rental markets, shown in Appendix Figure A13, and they typically take an equity stake in residential investment projects.\textsuperscript{30} This research design lends three insights to the previous credit supply analysis. First, I provide evidence that changes in the supply of equity, versus debt, can affect real outcomes. Second, this shock spans most of the post-crisis period. Third, while HVCRE capital requirements constitute an effective “risk-down” shock, this research design exploits a “risk-up” shock that also channels financing toward improvement projects.

5.1 Institutional Details

There are three key institutional details which enable this research design. First, private equity real estate funds are strictly classified by the type of project they perform, called the fund’s “strategy”.

\textsuperscript{30}These funds are a subset of the private equity market, and, unlike REITs, they are usually organized as closed-end partnerships with limited secondary market liquidity. Whether the fund organizes as closed or open-end depends on the fund’s stated strategy, which in turn depends on the types of projects it performs. Over 97% of funds which specialize in improvements, which are the focus of this paper, are closed-end in my data. Buy-and-hold funds are more likely to be open-end.
Figure 8: Risk and Return in Real Estate

Note: This figure plots the average and standard deviation of realized total returns over 1996-2012 for various assets. Core RE and VA respectively denote core and value added private equity real estate funds, whose returns are time-weighted. Data come from: CRSP value-weighted stock index; Bank of America U.S. bond indices; and NCREIF core (ODCE) and value added (CEVA) indices.

For this paper’s purposes, there are two main project types: buy-and-hold investments are performed by “core” funds, and improvements are performed by “value added” funds. Figure 8 plots the risk-return frontier for these various strategies and other conventional assets. Note that the risk-return profile of value added funds — again, whose economic function is to perform improvements — is similar to that of a high-yield bond.

Second, it is well-documented that public pensions take more risk the more underfunded they are, which is largely due to Governmental Accounting Standards Board (GASB) accounting rules (e.g. Andonov, Bauer and Cremers 2017; Mohan and Zhang 2014; Novy-Marx and Rauh 2011). Specifically, public pensions have an incentive to report low accounting values of their actuarial funding gaps. Doing so lowers the likelihood that regulators require higher contribution payments or reduce future benefits. However, GASB rules allow public pensions to discount actuarial liabilities using the expected return on their assets. Thus, investing in assets with greater compensated risk, and thus a higher expected return, lowers the accounting value of a pension’s funding gap. Andonov,

31There is a third major fund type, called “opportunistic” funds, which perform construction. Opportunistic funds have a historic average net return of 13.5% with a standard deviation of 19.2% (Pagliari 2017). The mapping from fund type to economic function is a best approximation, and there are some exceptions. For example, transactions in niche property sectors (e.g. student housing) and extreme rehabilitations may be done by opportunistic funds. Value added funds may also improve property management in addition to structural quality.
Note: This figure plots the relationship between a pension’s: (i) change in the share of private equity real estate portfolio allocated toward improvement-oriented (“value added”) funds from the 2009-2012 period to the 2014-2016 period, and (ii) the percent difference between the pension’s actuarial liabilities and assets in 2008. Each observation is a public pension. Larger dots correspond to larger pensions by total assets. Data are from Preqin.

Bauer and Cremers (2017) further show that more underfunded pensions take even greater risk when safe yields are low. The intuition is that a decline in safe yields lowers expected asset returns and thus raises the funding gap’s accounting value. To offset this effect, pensions can invest in riskier assets which justify a higher expected return and thus, given GASB accounting rules, reduce the gap.

Applying these features to real estate, one would expect more underfunded pensions to tilt their portfolio toward riskier improvement-oriented (“value added”) real estate funds and away from safer buy-and-hold (“core”) funds during a period of declining safe yields. Figure 9 provides preliminary evidence in favor of this hypothesis. It shows that pensions with a larger 2008 funding gap disproportionately increase their portfolio allocation to improvement-oriented funds over 2009-2016, during which safe yields fell on average.\(^\text{32}\) This reallocation can potentially have meaningful effects, since public pensions are dominant financiers of private equity real estate funds, comprising between 30% and 50% of limited partners as shown in Appendix Figure A14.

The third institutional detail is fundraising stickiness between private equity real estate fund managers and their limited partners (e.g. public pensions). For example, Appendix C.1 shows how

\(^{32}\)The allocation is within the pension’s private equity real estate portfolio. Valuing private equity portfolios is a well-known challenge, and it is further complicated by the fact that I have limited information on the size of a limited partner’s commitment. Thus, I approximate the portfolio share allocated to improvement-oriented funds as the fraction of active funds in the pension’s portfolio that are improvement-oriented.
the probability a fund manager turns to an existing limited partner in her next fundraising round is 22 pps higher than what one would predict based on the limited partner’s market share. Like in the credit supply analysis, relationship stickiness is what enables me to identify the real effects of pension portfolio reallocation. In light of Figure 9, one might therefore expect fund managers historically reliant on underfunded public pensions to set up more improvement-oriented funds and, through them, to perform more improvements. This hypothesis is the focus of my empirical specification.

Drawing an analogy to the credit supply research design, “pensions” will play the role of “lenders” in that they supply financing. Likewise, “fund managers” will function like “counties” in that they are the economic unit at which improvement activity occurs. The critical distinction between the two research designs — apart from that of debt versus equity financing — is that the alternative to improvement projects is a safe buy-and-hold investment, whereas before it was new construction. In particular, the shock in this setting will be “risk-up”, whereas the credit supply analysis studied an effective “risk-down” shock. Moreover, as discussed below, the “shock” here will not coincide with a discrete event like HVCRE regulation. Rather, borrowing from the public pension literature, I show how more general patterns of risk taking by underfunded public pensions have manifested in real estate. Thus, I cannot identify the effect of intermediary portfolio reallocation as tightly as in the bank lending research design, partly reflecting more general data constraints faced by the literatures on private equity and alternative asset classes (Kaplan and Lerner 2016).

5.2 Pension Risk Taking and Investment in Improvements

My first question is whether underfunded pensions are more likely to invest in improvement-oriented funds — which, again, resemble a “high yield bond” — when risk taking incentives are stronger. I address this question through a panel specification, which provides additional variation and allows me to include pension fixed effects. Specifically, I use the previously-discussed feature that underfunded pensions take greater risk when safe yields are low. My pension-level specification is similar to Andonov, Bauer and Cremers (2017), and I estimate

\[ Y_{p,t} = \beta (\text{Funding Gap}_p \times \text{Yield Gap}_t) + \alpha_t + \alpha_p + \gamma X_{p,t} + u_{p,t}, \]  

\[ (10) \]

There is a sense in which very risky (“opportunistic”) private equity funds that perform construction are also an attractive investment for underfunded public pensions wishing to take more compensated risk. However, Appendix C.10 shows how pensions’ substitution into these very risky funds, while positive, was of weaker magnitude relative to the more moderate improvement-oriented (“value added”) funds. Studying value added funds is also conceptually cleaner, since opportunistic funds occasionally perform extreme rehabilitations, per footnote 31.
where \( p \) and \( t \) denote pension and year, Funding Gap\(_p\) is the pension’s funding gap in 2008, and Yield Gap\(_t\) is the spread between the safe yield in 2008 and in \( t \). To interpret, Funding Gap\(_p\) is the cross-sectional measure of risk-taking incentive, and Yield Gap\(_t\) captures when this incentive is strongest. The controls in \( X_{p,t} \) include state-year fixed effects, which account for public pensions’ local investment bias (Hochberg and Rauh 2013), as well as a vector of contemporaneous pension characteristics. I estimate (10) over 2009-2016 using the Preqin dataset merged with public pension data from Boston College’s Center for Retirement Research (CRR).

My second question is whether managers more reliant on underfunded public pensions for fundraising are more likely to form an improvement-oriented fund and, through it, to invest more in improvements. Mirroring (10), I next estimate

\[
Y_{m,t} = \beta (\text{Funding Gap}_m \times \text{Yield Gap}_t) + \alpha_t + \alpha_m + u_{m,t},
\]

(11)

where \( m \) and \( t \) index private equity real estate fund manager and year, and Funding Gap\(_m\) is the average of its analogue from (10) across \( m \)'s limited partners. To interpret, treated units in (11) are managers with a longstanding relationship with underfunded public pensions (i.e. Funding Gap\(_m\)), and the treatment is these pensions’ incentive to take risk (i.e. Yield Gap\(_t\)).

The outcome \( Y_{m,t} \) is a measure of the manager’s formation of or investment through improvement-oriented funds. Correspondingly, the main identification assumption in (11) is that shocks which affect such activity and covary with safe yields do not disproportionately affect managers with a high average funding gap. Explicitly, the assumption is

\[
\mathbb{E} [\text{Funding Gap}_m \times \text{Yield Gap}_t \times u_{m,t} | \alpha_m, \alpha_t] = 0.
\]

Appendix Figure C9 and its associated discussion support this assumption, providing evidence that managers with high and low exposures to underfunded pensions are similar on observable characteristics.

Columns 1-2 of Table 5 contain the results of the pension-level specification (10). My outcome

\[34\]I measure the safe yield using the yield on a 10-year TIPS bond. Appendix C.10 assesses robustness to the choice of measure. Note that (10) is computationally equivalent to replacing Yield Gap\(_t\) with just the 10-year TIPS yield, since the effect of the initial yield is subsumed by the fixed effect \( \alpha_p \). I weight observations by the pension’s average assets over 2009-2016 to avoid overweighting idiosyncratic shocks to small pensions.

\[35\]The largest 5 managers are Angelo, Gordon & Co, Wereldhave, CBRE Global Investors, Crow Holdings Capital, and Beacon Capital Partners. To avoid overweighting idiosyncratic shocks to relatively small managers, I weight observations in (11) by the manager’s total real estate capital raised over 2009-2016.
Table 5: Value Added Investment and Public Pension Risk Taking

<table>
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<th>Pension-Level</th>
<th>Manager-Level</th>
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<tr>
<td>Outcome:</td>
<td>Prob of Commitment&lt;sub&gt;VA&lt;/sub&gt;&lt;sup&gt;p,t&lt;/sup&gt;</td>
<td>Fund Formed&lt;sub&gt;VA&lt;/sub&gt;&lt;sup&gt;m,t&lt;/sup&gt;</td>
</tr>
<tr>
<td></td>
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<td>(2)</td>
</tr>
<tr>
<td>Funding Gap&lt;sub&gt;p&lt;/sub&gt; × Yield Gap&lt;sub&gt;t&lt;/sub&gt;</td>
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<td>0.367**</td>
</tr>
<tr>
<td></td>
<td>(0.140)</td>
<td>(0.145)</td>
</tr>
<tr>
<td>Funding Gap&lt;sub&gt;m&lt;/sub&gt; × Yield Gap&lt;sub&gt;t&lt;/sub&gt;</td>
<td></td>
<td>0.065**</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.020)</td>
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<td>State-Year FE</td>
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<tr>
<td>Pension Controls</td>
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<td>Yes</td>
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<tr>
<td>Manager FE</td>
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<td>Yes</td>
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<td>Year FE</td>
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<td>R-squared</td>
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Note: Subscripts <sub>p</sub>, <sub>m</sub>, and <sub>t</sub> denote pension, private equity real estate fund manager, and year. Columns 1-2 estimate (10) and columns 3-4 estimate (11). Observations in columns 1-2 are public pension-years weighted by average assets over 2009-2016, and observations in columns 3-4 are private equity real estate fund manager-years weighted by the manager’s real estate capital raised over 2009-2016. Funding Gap<sub>p</sub> is the percent difference between the pension’s actuarial liabilities and assets in 2008. Yield Gap<sub>t</sub> is the difference between the yield on a 10-year TIPS bond in 2008 and in <sub>t</sub>. Prob of Commitment<sub>VA</sub><sup>p,t</sup> indicates an investment in an improvement-oriented (“value added”) fund. Funding Gap<sub>m</sub> is the average percent difference between actuarial liabilities and assets in 2008 across manager <sub>m</sub>’s limited partners. Fund Formed<sub>VA</sub><sup>m,t</sup> indicates whether <sub>m</sub> formed an improvement-oriented fund for U.S. residential real estate with vintage <sub>t</sub>. Investment<sub>VA</sub><sup>m,t</sup> is the annualized investment of such funds between their vintage year <sub>t</sub> and 2016. Pension controls are: log actuarial assets, and allocations to cash, bonds, equity, and alternative assets. The sample period is 2009-2016. Standard errors clustered by pension in columns 1-2 and by manager in columns 3-4 are in parentheses. Data are from Preqin and the CRR.

The point estimate in column 1 suggests that a 1 standard deviation increase in the treatment corresponds to a 40 pps, or 0.8 standard deviation, higher annual probability of investing in an improvement-oriented fund.

Columns 3-4 report the estimates of the manager-level specification (11). The outcome in column 3 is the annual probability of forming an improvement-oriented fund, denoted Fund Formed<sub>VA</sub><sup>m,t</sup>. Interpreting the point estimate, managers with a 1 standard deviation higher pension investment shock, Funding Gap<sub>m</sub> × Yield Gap<sub>t</sub>, have a 6.5 pps, or 0.23 standard deviation, higher probability of forming such a fund. Column 4 studies log annualized investment by improvement-oriented funds formed by <sub>m</sub> between the fund’s vintage year, <sub>t</sub>, and 2016. This variable is an approximation to total improvement activity created by the fund which <sub>m</sub> formed in <sub>t</sub>. Based on the estimated coefficient,
managers with a 1 standard deviation higher pension investment shock in $t$ invest 39% more per year in improvements through funds formed in $t$. This last result suggests that pension risk taking has a significant effect on real improvement activity through the supply of private equity financing.

Appendix C.10 performs several extensions to assess the robustness of this research design. These include: verifying that, when safe yields fall, underfunded pensions invest less in the safer core real estate funds and more in very risky opportunistic funds which perform construction; using real yields, which may matter if pensions vary in cost of living adjustments; studying private debt as another alternative asset class subject to pension risk taking; discussing time variation in the yield gap; placebo analysis in a period with rising safe yields and similar stage in the real estate cycle; addressing GASB accounting rule changes in 2012; estimating a separate specification with manager-year fixed effects; studying matching between public pensions and real estate fund managers; and discussing the concern that real estate fund managers are too large for relationships to matter.

### 5.3 Magnitude of Effect

The last part of this section relates the estimates to overall improvement activity. Unlike with the credit supply research design, I cannot trace the shock all the way through to aggregate improvement projects and effective rent growth. Instead, I focus on the in-sample effect of pension funding gaps on investment by improvement-oriented private equity real estate funds.

I consider a counterfactual in which all public pensions were fully funded in 2008, equal to the 92nd percentile of funding status that year. Appendix C.11 describes the calculation procedure, which implies that investment by improvement-oriented funds would have been 56% less over 2010-2016 under this counterfactual. While difficult to map this effect to aggregate improvement activity, one can obtain a rough sense of magnitude by noting that improvement-oriented funds can account for 28% of aggregate investment in existing rental housing units over 2010-2016. By extension, portfolio reallocation by underfunded public pensions may account for around 16% ($0.28 \times 0.56$) of aggregate investment in existing rental units over that period.

---

^36Investment by private equity real estate funds from Preqin accounts for 44% of aggregate investment in existing rental units from the Fixed Assets Accounts over 2010-2016, of which improvement-oriented funds comprise 64% (i.e. $28\% = 0.44 \times 0.64$).
6 Conclusion

I found that quality improvements account for the majority of post-crisis real rent growth, and a significant share of improvement activity can be attributed to a reallocation of financing across different types of residential investment. Beginning with a measurement exercise, I showed that accounting for quality improvements results in effective rent growth that is between 65% and 86% less than observed growth. Then, based on a bank lending shock due to changes in capital requirements, an increase in credit supply raises improvement activity 44% over 2015-2016 and accounts for 32% of observed rent growth. Similarly, risk taking by underfunded public pensions raises improvement activity over 2010-2016 through an increase in the supply of private equity financing for improvement projects.

This paper shows how intermediary portfolio reallocation affects the types of real projects that are financed. In policy terms, financial regulations – or more broadly policies that affect intermediaries’ portfolio choice – can have real effects through such project reallocation. An open question is how such policies affect the composition of intermediaries within each project type. In addition, given that improvements appear to be targeted toward high income markets, these policies likely have distributional effects, which is another avenue for future study.

References


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A Additional Figures and Tables

Figure A1: Rent-to-Income Ratio and Real Rent

Note: Panel (a) plots the ratio of median rent to median household income. Panel (b) plots average real rent across the top quartile of MSAs sorted by 2008-2015 rent growth. Data are from Zillow and the Census Bureau.

Figure A2: Aggregate Spending on Improvements

Note: This figure plots real aggregate investment in residential improvements from the Fixed Assets Accounts. Data are from the BEA.
Figure A3: Rate of Income Filtering

Note: This figure plots the average rental unit’s change in its inhabitant’s overall income percentile. Data are from the AHS.

Figure A4: Cross-Sectional Distribution of Log Rent

Note: This figure plots the cross-sectional empirical density of zip code level multifamily log rent in 2011 and 2016. Log rent is demeaned by MSA and year. The density is constructed using a Gaussian kernel. The plot excludes observations more than 3 standard deviations from the mean. Data are from Zillow.
Figure A5: Quantity and Rent Growth of Top Tier Units

(a) Top Quality Segment

(b) Real Rent Growth by Market Segment

Note: Panel (a) plots the percent of multifamily units in the top quality segment, based on MBA/CREFC rating. Panel (b) plots average real rent growth for properties in the top segment, above and below average segments, and bottom segment, based on the MBA/CREFC rating. Data are from Trepp.

Table A1: Rent Growth and New Features

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<th>Installment of:</th>
<th>$\Delta \log (\text{Rent}_{i,t})$</th>
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<tbody>
<tr>
<td>Dishwasher$_{i,t}$</td>
<td>0.118** (0.022)</td>
</tr>
<tr>
<td>Washing Machine$_{i,t}$</td>
<td>0.097** (0.026)</td>
</tr>
<tr>
<td>Disposal$_{i,t}$</td>
<td>0.031 (0.020)</td>
</tr>
<tr>
<td>Trash Compactor$_{i,t}$</td>
<td>0.013 (0.040)</td>
</tr>
<tr>
<td>Central A/C$_{i,t}$</td>
<td>0.023 (0.021)</td>
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<tr>
<td>A/C$_{i,t}$</td>
<td>0.063** (0.015)</td>
</tr>
<tr>
<td>Dryer$_{i,t}$</td>
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<td>log (Square Feet$_{i,t}$)</td>
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<td>Year FE</td>
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</tbody>
</table>

Note: This table estimates equation (1). Subscripts $i$ and $t$ denote housing unit and year. The outcome $\Delta \log (\text{Rent}_{i,t})$ is the change in log rent. The vector of regressors, denoted $\Delta F_{i,t}$ in the text, are indicators for the installment of the given feature, except for log (Square Feet$_{i,t}$) where, instead of an indicator, the variable is the increase in log square feet. A/C denotes air conditioning. Central A/C is conditional on having any air conditioning. All changes are over 2 year intervals. Observations are rental housing unit-years. The sample period is 1997-2013. Standard errors are in parentheses. Data are from the AHS.
Figure A6: Contribution to Hedonic Index by Feature

Note: This figure plots the contribution of each feature to the hedonic index, defined as the feature’s average price effect from (1) across properties and years, divided by the sum of price effects across features. The contribution of feature $f$ is $\frac{\sum_i \sum_t \beta_f \Delta f_{i,t}}{\sum_f \sum_i \sum_t \beta_f \Delta f_{i,t}}$, for $t \in \{2009, 2011, 2013\}$, $i \in I$, and $f \in F$. The plot is restricted to the top 5 features sorted by price coefficient $\beta^F$ from (1), and so the area underneath the bars sums to 100. Data are from the AHS.

Figure A7: Allocation of Credit for Improvements

Note: This figure plots the percent of multifamily mortgages originated within 1 year of renovation for the indicated time periods. Data are from Trepp.
Note: This figure plots the correlation coefficients between the variables in the hedonic pricing equation (1) and their first four principal components. Log Rent is the change in log rent. Householder Income is the change in the household’s U.S. income percentile. The remaining variables are defined in Table A1. Data are from the AHS.
Figure A9: Improvements as a Share of Projects

Note: This figure plots the number of renovated multifamily units divided by the number of renovated multifamily units plus the number of newly built multifamily units. The gray region indicates the period when HVCRE regulations are in place. Data are from Trepp.

Figure A10: Portfolio Characteristics by Type of Lender

Note: This figure plots the difference in mean for the indicated variable between bank and nonbank lenders. Variables are normalized to have zero mean and unit variance and aggregated to the lender-level by averaging across loans in the lender’s portfolio over 2011-2016, weighting by loan principal. Default Rate and Loans Due are, respectively, the share of loans 60+ days delinquent and the share of loans coming due in a given year. LTV is the current loan-to-value ratio. Occupancy is the property’s occupancy rate. Property Size is in number of units. Brackets are a 95% confidence interval with heteroskedasticity robust standard errors. Data are from Trepp.
Table A2: Property-Level Instrumental Variables Specification

| Outcome: Renovation \(_{i,\ell,t}\) | New Loan \(_{i,\ell,t}\) | 0.167** (0.065) |
| Estimator | 2SLS |
| Property-Lender-FE | Yes |
| County-Year FE | Yes |
| Zip Code Controls | Yes |
| F Statistic | 82.766 |
| Number of Observations | 30733 |

Note: Subscripts \(i\), \(\ell\), and \(t\) denote property, lender, and year. This table estimates a version of (6). New Loan \(_{i,t}\) indicates whether a loan was originated. The outcome is an indicator for whether a renovation occurs. The estimator 2SLS, and New Loan \(_{i,t}\) is instrumented for using the interaction between an indicator for whether lender \(\ell\) is a bank and an indicator for whether \(t\) is greater than or equal to 2015. Zip code controls are log average income and log number of tax returns, from the IRS, and log average rent, from Trepp. Observations are property-years. The sample period is 2011-2016. Standard errors clustered by property are in parentheses. Data are from Trepp.

Table A3: Measuring HVCRE Exposure with the Office Sector

| Outcome: \(\log (\text{Renovated Properties}_{c,t})\) | Bank Exposure \(_c\) \(\times\) Post \(_t\) | (1) | (2) |
| | 0.152* (0.081) | 0.335** (0.167) |
| Exposure Sector | Office | Office |
| Base Period | 2010 | 2001-2009 |
| Year FE | Yes | Yes |
| County FE | Yes | Yes |
| State-Year FE | No | No |
| County Controls | No | No |
| R-squared | 0.601 | 0.610 |
| Number of Observations | 3236 | 3236 |

Note: Subscripts \(c\) and \(t\) denote county and year. The specification is the similar to column 1 of Table 3 with different measures of exposure to bank lenders, denoted Bank Exposure \(_c\). Column 1 measures exposure using banks’ share of office commercial mortgage balances in 2010. Column 2 measures exposure using banks’ share of office commercial mortgage originations over 2001-2009. Observations are county-years weighted by the average number of multifamily units over 2011-2016. The sample period is 2011-2016. Standard errors clustered by county are in parentheses. Data are from Trepp.
Figure A11: Distribution of Initial Bank Share in High Growth Areas

Note: This figure plots banks’ share of multifamily mortgage balances in 2010 across counties in high growth metro areas. Panels (a)-(d) plot this share across counties in northern California, northeast states, the Chicagoland area, and southern California, respectively. The plot is analogous to the state-level map in Figure 5. Warmer colors indicate a higher share.
Table A4: County-Level Instrumental Variables Specification

<table>
<thead>
<tr>
<th>Outcome:</th>
<th>( \log(\text{Loans}_{c,t}) )</th>
</tr>
</thead>
<tbody>
<tr>
<td>( \log(\text{Renovated Properties}_{c,t}) )</td>
<td>0.725** (0.279)</td>
</tr>
</tbody>
</table>

Estimator: 2SLS
Year FE: Yes
County FE: Yes
State-Year FE: Yes
County Controls: Yes
R-squared: 0.342
F Statistic: 6.072
J Statistic (p-value): 0.335
Number of Observations: 3159

Note: Subscripts \( c \) and \( t \) denote county and year. The specification is similar to column 3 of Table 3, except that the regressor is log of total loans originated, denoted \( \log(\text{Loans}_{c,t}) \). The estimator is 2SLS, and \( \log(\text{Loans}_{c,t}) \) is instrumented for using the interaction between Post\(_t \times \text{Bank Share}_{c} \) and Post\(_t \times \text{Bank Share}_{c}^2 \). County controls are those from Table 3. Observations are county-years weighted by the average number of multifamily units over 2011-2016. The sample period is 2011-2016. Standard errors clustered by county are in parentheses. Data are from Trepp.
Table A5: Robustness to Heterogeneous Time Trends

<table>
<thead>
<tr>
<th>Outcome</th>
<th>log (Renovated Properties)</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Bank Share&lt;sub&gt;c&lt;/sub&gt; × Post&lt;sub&gt;t&lt;/sub&gt;</td>
<td>0.329**</td>
<td>0.285**</td>
<td>0.303**</td>
<td>0.247**</td>
<td>0.283**</td>
<td></td>
</tr>
<tr>
<td>Characteristic&lt;sub&gt;c&lt;/sub&gt; × Year-2012&lt;sub&gt;t&lt;/sub&gt;</td>
<td>0.061</td>
<td>-0.155**</td>
<td>-0.062*</td>
<td>-0.013</td>
<td>-0.101*</td>
<td></td>
</tr>
<tr>
<td>Characteristic&lt;sub&gt;c&lt;/sub&gt; × Year-2013&lt;sub&gt;t&lt;/sub&gt;</td>
<td>0.098**</td>
<td>-0.142**</td>
<td>-0.080*</td>
<td>-0.003</td>
<td>-0.161**</td>
<td></td>
</tr>
<tr>
<td>Characteristic&lt;sub&gt;c&lt;/sub&gt; × Year-2014&lt;sub&gt;t&lt;/sub&gt;</td>
<td>0.172**</td>
<td>-0.426**</td>
<td>-0.206**</td>
<td>-0.016</td>
<td>-0.272**</td>
<td></td>
</tr>
<tr>
<td>Characteristic&lt;sub&gt;c&lt;/sub&gt; × Year-2015&lt;sub&gt;t&lt;/sub&gt;</td>
<td>0.139**</td>
<td>-0.328**</td>
<td>-0.139**</td>
<td>-0.034</td>
<td>-0.170*</td>
<td></td>
</tr>
<tr>
<td>Characteristic&lt;sub&gt;c&lt;/sub&gt; × Year-2016&lt;sub&gt;t&lt;/sub&gt;</td>
<td>0.172**</td>
<td>-0.545**</td>
<td>-0.210**</td>
<td>-0.040</td>
<td>-0.231*</td>
<td></td>
</tr>
</tbody>
</table>

Characteristics:

- Year FE: Yes
- County FE: Yes
- State-Year FE: Yes
- R-squared: 0.726
- Number of Observations: 3159

Note: Subscripts <sub>c</sub> and <sub>t</sub> denote county and year. The specification is similar to column 2 of Table 3 with the inclusion of heterogeneous time trends for the following characteristics: Income is real income per capita for the surrounding MSA averaged over 2011-2016; Winter Storms is number of winter storms per multifamily housing unit averaged over 2011-2016; White Share is the 2010 share of inhabitants over age 16 that are white; College Education is the 2010 share of inhabitants with at least a bachelor’s degree; Saiz Elasticity is the Saiz (2010) elasticity of housing supply. Characteristics are normalized to have zero mean and unit variance. Observations are county-years weighted by the average number of multifamily units over 2011-2016. The sample period is 2011-2016. Standard errors clustered by county are in parentheses. Data are from Trepp.
Figure A12: County-Level Improvements and HVCRE Regulation in the Cross-Section

Note: This figure plots the relationship between a county’s: (i) change in log renovated apartments from the 2011-2014 period to the 2015-2016 period, and (ii) share of multifamily mortgage balances in 2010 held by banks. The plot residualizes against a state fixed effect and the change in the controls from Table 3 from the 2011-2014 to 2015-2016 periods. The regression is the same as (7) after averaging across the 2015-2016 and 2011-2014 periods for each county and taking the difference. Each observation is a county weighted by the average number of multifamily units over 2011-2016. The plot is binned. Data are from Trepp.

Figure A13: Investment by Private Equity Real Estate Funds

Note: This figure plots investment by private equity real estate funds in U.S. residential real estate as a percent of aggregate tenant occupied residential investment from the Fixed Assets Accounts. Data are from Prequin and the BEA.
Figure A14: Public Pension Presence in Improvement-Oriented Real Estate Funds

![Public Pension Presence in Value Added Real Estate](image)

Note: This figure plots the share of investors in improvement-oriented (“value added”) private equity real estate funds that are public pensions by the fund’s vintage year. Data are from Preqin.

Figure A15: Public Pension Funding Gap

![Public Pension Funding Gap](image)

(a) Funding Gap

Note: This figure plots the average public pension’s percent difference between actuarial liabilities and assets. Data are from the CRR.
B  Data Appendix

This appendix describes the paper’s main datasets and how they were cleaned. Section B.1 describes the three core datasets and Section B.2 describes auxiliary ones.

B.1  Core Datasets

B.1.1  AHS Dataset

The first core dataset is the American Housing Survey (AHS), which covers a representative panel U.S. housing units and is administered in odd numbered years. AHS data contain relatively granular information about a unit’s physical features and self-reported information about the occupant’s demographics, rent, mortgage payments, and recent moving history. AHS data do not contain information about the property’s location, which I address through extensive use of unit fixed effects.\textsuperscript{37} The AHS was introduced in 1973 but has undergone several sample redesigns since then. I use the 1997-2013 sample design in this paper.

My primary use of the AHS data is to construct the hedonic index in Section 3.1. I estimate the hedonic pricing equation (1) over 1997-2013 to utilize additional variation, but I only perform the adjustment over 2007-2013. Data on property features come from the Equipment and Appliances module. The features used to construct the index are chosen because they are available for 85% of units in the sample. Since my focus is on the rental sector, I restrict attention to units whose tenure did not change over the sample period, thus filtering out “condo conversions”. I winsorize rent data by 5% on both sides prior to aggregating quality-adjusted rent in (3).

Table B1 provides summary statistics of the AHS dataset used to construct the hedonic index.

B.1.2  Trepp Dataset

The second core dataset comes from Trepp LLC. It includes information on the property condition, operating and capital expenses, revenue, and financial condition of a geographically representative sample of multifamily properties in the U.S. over 2010-2016.\textsuperscript{38} The dataset covers 88% of U.S. counties by population. It pertains to roughly 50% of mortgaged multifamily properties, 35% of multifamily properties, and 18% of total rental properties. The raw data come from multifamily mortgage servicing records for loans which were securitized by the fourth quarter of 2017. Most

\textsuperscript{37}I only observe the unit’s MSA for a subset of 166 MSAs.
\textsuperscript{38}I work with a random sample of Trepp’s merged Property, Loan, and Loan2 file.
Table B1: Summary Statistics for AHS Dataset

<table>
<thead>
<tr>
<th>Variable</th>
<th>Observations</th>
<th>Mean</th>
<th>Standard Deviation</th>
</tr>
</thead>
<tbody>
<tr>
<td>$\Delta \log (\text{Rent}_{i,t})$</td>
<td>81733</td>
<td>0.050</td>
<td>0.964</td>
</tr>
<tr>
<td>$\Delta \text{Dishwasher}_{i,t}$</td>
<td>81733</td>
<td>0.034</td>
<td>0.182</td>
</tr>
<tr>
<td>$\Delta \text{Washing Machine}_{i,t}$</td>
<td>81733</td>
<td>0.068</td>
<td>0.252</td>
</tr>
<tr>
<td>$\Delta \text{Trash Compactor}_{i,t}$</td>
<td>81733</td>
<td>0.010</td>
<td>0.100</td>
</tr>
<tr>
<td>$\Delta \text{Disposal}_{i,t}$</td>
<td>81733</td>
<td>0.043</td>
<td>0.202</td>
</tr>
<tr>
<td>$\Delta \text{Central A/C}_{i,t}$</td>
<td>81733</td>
<td>0.042</td>
<td>0.200</td>
</tr>
<tr>
<td>$\Delta \text{A/C}_{i,t}$</td>
<td>81733</td>
<td>0.076</td>
<td>0.266</td>
</tr>
<tr>
<td>$\Delta \text{Dryer}_{i,t}$</td>
<td>81733</td>
<td>0.063</td>
<td>0.244</td>
</tr>
<tr>
<td>$\Delta \log (\text{Square Feet}_{i,t})$</td>
<td>81733</td>
<td>0.006</td>
<td>0.082</td>
</tr>
</tbody>
</table>

Note: This table presents summary statistics of the key variables from the AHS dataset. Subscripts $i$ and $t$ denote housing unit and year. $\Delta \log (\text{Rent}_{i,t})$ is the change in log rent; $\Delta \text{Dishwasher}_{i,t}$ through $\Delta \text{Dryer}_{i,t}$ indicate whether the given feature was installed; $\Delta \log (\text{Square Feet}_{i,t})$ is the increase in log square feet. A/C denotes air conditioning. Central A/C is conditional on having any air conditioning. All changes are over 2 year intervals. Observations are rental housing unit-years. The sample period is 1997-2013.

Variables are observed annually, except data on the loan’s status (e.g. delinquency), which I collapse from a monthly to yearly frequency, weighting by outstanding principal. The data pertain to around 35% of multifamily properties after accounting for the fact that approximately 70% of properties are mortgaged and half of multifamily mortgages are securitized, according to the RHFS and Rosengren (2017), respectively. I also have data on office commercial mortgages, which I use in Table A3.

There are four variables in the Trepp data which merit discussion:

1. **Rent**: I observe total property revenue, number of units, and occupancy rate. Rent is approximated as revenue per occupied unit and winsorized to attenuate measurement error.

2. **Renovation**: Renovations are defined as improvements that require the inhabitant to vacate the housing unit for some period of time. I observe the history of renovations on a property dating back prior to 2000. This allows me to backfill the time series in Figure 1a as follows. For the numerator (i.e. number of renovated units), I compute the sum of in-sample units that were renovated in $t$, conditional on the property’s loan being securitized by $t$ so that the property would have been included in a pre-2010 version of the sample.\(^{39}\) For the denominator, I regress the log number of multifamily units in the sample over 2010-2016 on the log aggregate stock of U.S. rental units from the Census’ Housing and Vacancy Survey, which is available beginning

\(^{39}\)I do not observe whether pre-2010 renovations increased the number of housing units in a property, so I approximate the number of renovated units in a property using the number of units as of 2010. This measurement error is likely to be small, because only 2% of post-2010 renovations entail a change in the number of housing units.
in 2000. Then, I backfill the number of units that would have been in a pre-2010 version of the sample. Taking the ratio of numerator and denominator gives the pre-2010 time series in Figure 1a.

Next, renovations undertaken in the latter part of the 2010-2016 period may not appear in the sample because of securitization lags. Therefore, Figure 1a weights observations by the inverse probability of appearing in the sample (Solon, Haider and Wooldridge 2015), here defined as the probability of being securitized by the fourth quarter of 2017.40

Finally, I cross-reference the renovation data in Trepp with the RHFS, which records the probability of renovation over 2010-2012 and 2013-2015 on mortgaged properties. The probability of renovation in the RHFS grew 82% between in these two periods, compared to 107% in the Trepp data.

3. **Lender**: I observe the name of the lender who originated the property’s mortgage for 92% of the sample.41 Banks are defined as having a record in the FDIC’s Institution Directory. I do not classify independent nonbank subsidiaries as depository institutions. Based on this classification, 39% of lenders in my data are depository institutions. There are some non-depository institutions, like Prudential, which are classified as Designated Financial Companies and thus required to compute risk-based capital requirements as if they were a bank holding company. Since my focus is on the effects of capital requirements, I classify such lenders as banks. Apart from these special cases, “bank” is synonymous with “depository institution”. I observe the name of the borrower for 14% of the sample, which I use to perform the analysis in Appendix C.1.

4. **MBA/CREFC Rating**: The Mortgage Bankers Association and Commercial Real Estate Finance Council’s (MBA/CREFC) property inspection rating is regularly collected as part of the standard multifamily mortgage servicing protocol. Its purpose is to minimize agency frictions which might incentivize the borrower to not maintain the property’s competitiveness. This rating has a discrete scale from 1 to 5, where lower values indicate greater quality relative to a newly built unit reflecting “the highest current market standards”. There is a checklist of

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40I measure this probability using the empirical cumulative density function of the gap between the month of securitization and October 2017.

41To address cases where the name’s spelling changes, I use a string grouping algorithm developed by Julian Reif to aggregate different spellings under a single identifier. I manually review the matches to check accuracy. For the small minority of cases in which a property has multiple loans from different lenders, I assign the lender with the largest balance to the property.
features to help inspectors assign properties the appropriate score.\textsuperscript{42} To appropriately capture magnitudes, I transform the score for property \( i \) and year \( t \) to a relative quality measure, referred to as \( \text{Quality}_{i,t} \) in the text, such that a share \( \text{Quality}_{i,t} \) of units had the same or more inferior score in 2009.\textsuperscript{43} Thus, \( \text{Quality}_{i,t} \) has the interpretation of percent quality relative to the top of the market. Unless otherwise noted, whenever I refer to the MBA/CREFC property inspection score, I refer to the transformed measure \( \text{Quality}_{i,t} \).

This measure has the advantage of being nationally representative, standardized, and measured regularly. Moreover, it has the rare ability to capture intermediate regions of quality between that of a newly built unit and that of a unit with severe deferred maintenance.\textsuperscript{44} Online Appendix F has photographs which also help visualize the MBA/CREFC rating.\textsuperscript{45} The values of \( \text{Relative Quality}_{i,t} \) for the units in panels (a)-(d) are, in percentages: 98\%, 51\%, 4\%, 0.1\%.

A concern with the MBA/CREFC rating is the possibility that reporting standards changed over the period of analysis. To address this concern, I ask how ratings evolved for units for which agency problems might be more severe, which I proxy for using the speed of the loan’s securitization.\textsuperscript{46} Figure B1 plots the average change in log relative quality, measured by the MBA/CREFC rating, for units whose loan was or was not securitized within 3 months of origination. The time series for the two types of loans are quite similar, which suggests against changes in reporting standards.

My primary use for the Trepp dataset is the credit supply research design in Section 4, although I also use it to produce some of the stylized facts in Section 2 and when computing the structural rent index in Section 3.2. As discussed in Section 4, I work with both property and county-level

\textsuperscript{42}A score of 1 is intended to have the interpretation of “new or like-new condition”. Scores of 2 or 3 are meant to be interpreted as exhibiting “minimal” or “normal wear and tear”. Scores of 4 to 5 corresponding “deteriorating” and suffer “minor” to “severe” deferred maintenance. Since there are very few units with a score of 5, I combine them with those whose score is 4.

\textsuperscript{43}Explicitly, \( \text{Quality}_{i,t} \) is a mapping from the raw score \( \text{Raw}_{i,t} \in \{1, \ldots, 5\} \) into the unit interval such that \( \text{Quality}_{i,t}(y) = 0.5 \times (\Pr[\text{Raw}_{i,t} > y] + \Pr[\text{Raw}_{i,t} \geq y]) \), where the probabilities are computed in 2009 and weight properties by number of units. Taking the average of left and right Riemann sums ensures that no raw score maps to 0.

\textsuperscript{44}For example, the U.S. Department of Housing and Urban Development is currently undertaking an initiative to develop a new measure of housing quality that extends beyond the notion of “adequacy” (Eggers and Moumen 2013). In another example, “proptech” firms which specialize in providing intermediate measures of housing quality have grown substantially since 2010 (e.g. Rentlogic).

\textsuperscript{45}The photographs come from the website of a large real estate investor in the Dallas, TX market.

\textsuperscript{46}For example, loans that were securitized more than 3 months after origination may be subject to more stringent monitoring costs. For banks, this may be because the loans were originated with the intent of remaining on the balance sheet, but were later sold. For nonbanks, taking longer than 3 months to sell a loan may indicate poor credit quality, thus incentivizing the purchaser to ensure proper monitoring by the loan’s servicer.
Figure B1: Relative Quality Measure by Securitization Speed

Note: This figure plots the change in log relative quality based on whether the loan was securitized or on the lender’s balance sheet within 3 months of origination. Relative quality is based on the MBA/CREFC rating.

datasets. Table B2 provides summary statistics of these data. Some of the variables come from auxiliary datasets, which are mentioned in the table’s footnote and described in detail in Section B.2.

B.1.3 Preqin Dataset

The third core dataset comes from Preqin and covers fundraising and investment by private equity real estate funds. Preqin specializes in providing data on alternative asset classes, and its data are commonly used in the private equity and venture capital literatures (Kaplan and Lerner 2016). I observe yearly data at three levels of aggregation: fund, fund manager, and limited partner. Fund data include information on vintage year, size at closing, and value of investments made each year. Manager data include size and number of funds raised each year. Limited partner data include information on the type of institution and annual investment in private equity real estate funds. Preqin data tend to overrepresent fund managers that cater to large public pensions (Kaplan and Lerner 2016).

Importantly for the purposes of this paper, I observe each fund’s strategy: value added, core, or opportunistic.\footnote{I classify core-plus funds as value added, since these funds often make improvements, but at a much smaller scale. This classification does not materially impact the results because only 7.6\% of funds classified as value added are core-plus. I also drop fund-of-funds, secondaries, and real estate debt funds, which do not have a clearly stated strategy.} In addition, I observe the fund’s property sector and geographic focus. This
Table B2: Summary Statistics for Trepp Dataset

<table>
<thead>
<tr>
<th>Variable</th>
<th>Observations</th>
<th>Mean</th>
<th>Standard Deviation</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Property-Level Variables</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Probability of Renovation(_{i,t})</td>
<td>30733</td>
<td>0.026</td>
<td>0.158</td>
</tr>
<tr>
<td>Bank(_{i,t})</td>
<td>30733</td>
<td>0.473</td>
<td>0.499</td>
</tr>
<tr>
<td>New Loan(_{i,t})</td>
<td>30733</td>
<td>0.060</td>
<td>0.237</td>
</tr>
<tr>
<td>Due(_{i,t})</td>
<td>143530</td>
<td>0.016</td>
<td>0.126</td>
</tr>
<tr>
<td>∆ log (Quality(_{i,t+1}))</td>
<td>143530</td>
<td>-0.152</td>
<td>0.799</td>
</tr>
<tr>
<td>log (Balance(_{i,t}))</td>
<td>143530</td>
<td>7.856</td>
<td>7.766</td>
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<tr>
<td><strong>County-Level Variables</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Bank Share(_{c})</td>
<td>3169</td>
<td>0.667</td>
<td>0.176</td>
</tr>
<tr>
<td>log (Renovated Properties(_{c,t}))</td>
<td>3169</td>
<td>0.152</td>
<td>0.375</td>
</tr>
<tr>
<td>log (Renovated Housing Units(_{c,t}))</td>
<td>3169</td>
<td>0.921</td>
<td>2.152</td>
</tr>
<tr>
<td>log (Units(_{c,t}))</td>
<td>3169</td>
<td>9.73</td>
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</tr>
<tr>
<td>log (Rent(_{c,t}))</td>
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<tr>
<td>log (Income(_{c,t}))</td>
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<tr>
<td>log (Storms(_{c,t}))</td>
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<td>LTV(_{c,t})</td>
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<td>Delinquent(_{c,t})</td>
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<td>DSCR(_{c,t})</td>
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<td>1.555</td>
<td>0.295</td>
</tr>
<tr>
<td>ARM(_{c,t})</td>
<td>3169</td>
<td>0.053</td>
<td>0.049</td>
</tr>
</tbody>
</table>

Note: This table presents summary statistics of the key variables from the Trepp dataset. Subscripts \(i\), \(c\), and \(t\) denote property, county, and year. The upper panel summarizes property-level variables: Probability of Renovation\(_{i,t}\) indicates if the property was renovated in \(t\); Bank\(_{i,t}\) indicates if the property owner’s lender is a bank; New Loan\(_{i,t}\) indicates if a new loan was originated on \(i\) in \(t\); Due\(_{i,t}\) indicates whether the property has a loan due in \(t\); ∆ log (Quality\(_{i,t+1}\)) is the change in log relative quality, measured using the MBA/CREFC rating; log (Balance\(_{i,t}\)) is the log of end-of-period loan balance. Note that the variables Due\(_{i,t}\) through log (Balance\(_{i,t}\)) are used in the extension of Appendix C.8. The lower panel of the table summarizes county-level variables: Bank Share\(_{c}\) is the share of multifamily mortgage balances held by banks in 2010; log (Renovated Properties\(_{c,t}\)) is the log number of renovated properties; log (Renovated Housing Units\(_{c,t}\)) is the log number of renovated housing units; log (Units\(_{c,t}\)) is the number of housing units; log (Rent\(_{c,t}\)) is the log average rent per unit; LTV\(_{c,t}\) through ARM\(_{c,t}\) are the principal weighted values of the following characteristics of outstanding loans: loan-to-value ratio, debt service coverage ratio, adjustable rate mortgage share, and share of 60+ day delinquent loans. The variables log (Income\(_{c,t}\)) and log (Storms\(_{c,t}\)) are log real income per capita for the surrounding MSA and log winter storms per multifamily unit, which were merged from the BEA and NOAA datasets described in Section B.2. Observations in the upper panel are property-years over 2011-2016. Observations in the lower panel are county-years over 2011-2016, weighted by the number of multifamily units in the county over that period.

Information enables me to restrict the manager-level regressions in Table 5 to value added funds with a focus on U.S. residential real estate. I include value added funds of all property types in the pension-level specification in Table 5 because the risk taking behavior captured by Funding Gap\(_p\) × Yield Gap\(_t\) is not restricted to residential real estate. The manager-level specification is of course restricted to residential real estate. The set of managers used in estimation are those which raised a value added
Table B3: Summary Statistics for Prequin Dataset

<table>
<thead>
<tr>
<th>Pension-Level Variables</th>
<th>Observations</th>
<th>Mean</th>
<th>Standard Deviation</th>
</tr>
</thead>
<tbody>
<tr>
<td>Prob of Commitment&lt;sub&gt;VA&lt;/sub&gt;</td>
<td>655</td>
<td>0.611</td>
<td>0.488</td>
</tr>
<tr>
<td>Prob of Commitment&lt;sub&gt;Core&lt;/sub&gt;</td>
<td>655</td>
<td>0.285</td>
<td>0.452</td>
</tr>
<tr>
<td>Prob of Commitment&lt;sub&gt;Opp&lt;/sub&gt;</td>
<td>655</td>
<td>0.483</td>
<td>0.500</td>
</tr>
<tr>
<td>Funding Gap&lt;sub&gt;p&lt;/sub&gt;</td>
<td>655</td>
<td>0.190</td>
<td>0.193</td>
</tr>
<tr>
<td>log (Assets&lt;sub&gt;p,t&lt;/sub&gt;)</td>
<td>655</td>
<td>17.764</td>
<td>1.228</td>
</tr>
<tr>
<td>Bond Share&lt;sub&gt;p,t&lt;/sub&gt;</td>
<td>655</td>
<td>0.231</td>
<td>0.073</td>
</tr>
<tr>
<td>Equity Share&lt;sub&gt;p,t&lt;/sub&gt;</td>
<td>655</td>
<td>0.499</td>
<td>0.100</td>
</tr>
<tr>
<td>Cash Share&lt;sub&gt;p,t&lt;/sub&gt;</td>
<td>655</td>
<td>0.026</td>
<td>0.028</td>
</tr>
<tr>
<td>Alternatives Share&lt;sub&gt;p,t&lt;/sub&gt;</td>
<td>655</td>
<td>0.165</td>
<td>0.107</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Manager-Level Variables</th>
<th>Observations</th>
<th>Mean</th>
<th>Standard Deviation</th>
</tr>
</thead>
<tbody>
<tr>
<td>Fund Formed&lt;sub&gt;VA&lt;/sub&gt;</td>
<td>736</td>
<td>0.083</td>
<td>0.276</td>
</tr>
<tr>
<td>log (Investment&lt;sub&gt;VA&lt;/sub&gt;)&lt;sub&gt;m,t&lt;/sub&gt;</td>
<td>736</td>
<td>0.302</td>
<td>1.109</td>
</tr>
<tr>
<td>Funding Gap&lt;sub&gt;m&lt;/sub&gt;</td>
<td>736</td>
<td>0.085</td>
<td>0.109</td>
</tr>
</tbody>
</table>

Note: This table presents summary statistics of the key variables from the Prequin dataset. Subscripts <sub>p</sub>, <sub>m</sub>, and <sub>t</sub> denote public pension, real estate fund manager, and year. The upper panel summarizes pension-level variables: Prob of Commitment<sub>VA</sub> through Prob of Commitment<sub>Opp</sub> are the annual probability of committing capital to a value added, core, or opportunistic private real estate fund; Funding Gap<sub>p</sub> is the difference between actuarial liabilities and assets and liabilities in 2008 expressed as a share of actuarial assets. The variables log (Assets<sub>p,t</sub>) through Alternatives Share<sub>p,t</sub> are log actuarial assets, and portfolio allocation to bonds, public equity, cash, and alternative assets in <sub>t</sub>, which are merged from the CRR dataset described in Section B.2. The lower panel summarizes manager-level variables: Fund Formed<sub>VA</sub> indicates the formation of a value added fund for U.S. residential real estate with vintage <sub>t</sub>; log (Investment<sub>VA</sub>)<sub>m,t</sub> is the log annual investment by such funds between their vintage year <sub>t</sub> and 2016; Funding Gap<sub>m</sub> is the average percent difference between actuarial assets and liabilities in 2008 across <sub>m</sub>’s limited partners over 2006-2008. Observations in the upper panel are pension-years over 2009-2016 weighted by average assets over 2009-2016. Observations in the lower panel are manager-years over 2009-2016 weighted by the manager’s real estate capital raised over that period.

real estate fund over 2006-2008. I include all limited partners that committed capital to <sub>m</sub> over 2006-2008 when computing the averages Funding Gap<sub>m</sub> and Yield Gap<sub>t</sub>. Institutions other than public pensions are assigned a funding gap of 0.

My main use of the Prequin data is the private equity research design from Section 5. Many of the key variables used in that design come from the Public Pension Database from Boston College’s Center for Retirement Research, an auxiliary dataset described in Section B.2 below. I merge the Prequin and CRR data at the pension year level using a manually developed crosswalk file. I also cross reference the results using a fuzzy merge procedure developed by Michael Blasnik. Table B3 provides summary statistics of the Prequin dataset used in Section 5.
B.2 Auxiliary Datasets

The following auxiliary datasets are also used in the paper:

- **Aggregate Renovation Activity**: Data on aggregate renovation activity and share performed by institutional or mortgaged investors come from the Rental Housing Finance Survey (RHFS). The RHFS aims to provide a current and continuous measure of financial, mortgage, and property characteristics of rental housing properties in the United States. Survey respondents are owners or property managers of rental properties. The survey has been administered in 2012 and 2015.

- **Public Pension Funding**: Data on public pensions’ funding status, allocation across asset classes, and realized returns come from Boston College’s Center for Retirement Research (CRR) Public Plans Database. The data contain plan-level information on 180 public pensions from 2001-2016, of which 114 are at the state level and 66 are local. According to CRR, the sample covers 95% of U.S. public pension assets. The raw data come from pensions’ Comprehensive Annual Financial Reports (CAFRs), specifically GASB Schedules of Plan Funding and Employer Contributions. The set of public pensions used in the analysis of Section 5 are those which invested in private equity real estate, though not necessarily an improvement-oriented fund, over 2009-2016.

- **Zip Code Income Data**: Zip code level income data come from the Internal Revenue Service (IRS) SOI Tax Stats. Average income is defined as total adjusted gross income divided by number of tax returns. The following variables are also used in the analysis: number of returns and the share of returns with income from dividends, social security benefits, unemployment insurance, or childcare tax credits. These variables respectively proxy for population, stock market participation rate, elderly share of population, unemployment rate, and family household rate. Data were not available for 2016 at the time of this paper’s writing, and so I forward fill the 2016 data using an average of 2014 and 2015 values.

- **Inflation**: Nominal rent is deflated using CPI excluding shelter. Nominal investment in residential improvements is deflated using the FHFA all-transactions price index.

- **Winter Storms**: Data on winter storms come from the National Oceanic and Atmospheric Association (NOAA). Winter storms are defined as blizzards, extreme cold or wind chill, hail,
heavy rain, heavy snow, high wind, winter weather, or official winter storms. Data are at the county-year level and are merged to the Trepp county dataset.

- **MSA Income**: Data on real income per capita come from the Bureau of Economic Analysis and are at the MSA-year level. I merge them to the Trepp county dataset using the MSA associated with each county.

- **Multifamily Portfolio Loans**: Data on bank portfolio loans come from Trepp’s T-ALLR dataset. These data contain information on bank-originated loans secured by multifamily properties which remained on the lender’s balance sheet through at least 2017. I observe whether the loan’s purpose was construction and, for a small subset of loans, the location of the encumbered property. The data come from clients of Trepp’s Bank Solutions consulting, and include a majority of bank subject to CCAR stress tests and a quarter of those subject to DFAST tests. The limited geographic data is intended to protect the lender’s privacy.

- **Syndicated Loans**: Data on syndicated loans come from the WRDS-Thomson-Reuters’ LPC DealScan database. The raw data come from SEC filings, company filings, and other public reports. See Chava and Roberts (2008) or Chodorow-Reich (2014) for a more detailed data description. Developers and REITs are classified as having respective SIC codes of 6552 and 6798. I classify lenders as subject to CCAR stress tests based on their reported name, using the list of such lenders from Gete and Reher (2018a). I group subsidiaries of CCAR lenders with their parent.

- **Zillow Rent and Price Indices**: Data on zip code multifamily rent from Figure 1 are from Zillow’s Multifamily Rent Index (ZMRI). Zillow imputes a unit’s rent using a mixed hedonic and repeat listing methodology. Then, it constructs a zip code’s ZMRI as the median across multifamily units. Pre-2006 data are constructed using decennial census rent figures, using simple linear interpolating between census releases to obtain a quarterly estimate. Data on county house prices used in Figure 6 are from Zillow’s Home Value Index (ZHVI), which is constructed using a similar methodology.

- **Deposit Losses**: Data on individual bank deposit losses come from the FDIC’s Failures and Assistance Transactions report. To obtain the institution’s county, I merge this dataset with the FDIC’s Institution Directory based on FDIC certification number.
• **Historic Private Equity Real Estate Returns:** Data on historic returns for value added real estate funds come from the National Council of Real Estate Investment Fiduciaries (NCREIF) closed end value added index (CEVA). Data on historic returns for core real estate come from the NCREIF open ended diversified core index (ODCE). The CEVA and ODCE indices are a capitalization-weighted, time-weighted return index with inception years of 1997 and 1977, respectively. Data on historic opportunistic real estate fund returns come from Pagliari (2017). All real estate fund returns are net of fees.

• **REIT Bond Issuance:** Data on REIT bond issuance and underwriting come from the National Association of REITs (NAREIT). The data are collected from public sources, and include information on IPOs, secondary equity, and secondary debt offerings for listed U.S. REITs.

• **Conventional Asset Returns:** Data on AAA and high-yield bond returns come from Bank of America Merill Lynch U.S. AAA and High Yield Corporate Bond Total Return Indices. Data on historic equity returns come from the Center for Research in Security Prices (CRSP) Value Weighted U.S. Total Return Index.

• **Rent Control:** Data on MSAs with rent control or stabilization policies come from Landlord.com and are as of 2011.
C Extensions

This appendix performs extensions referenced in the text.

C.1 Relationship Persistence in Real Estate Finance

This extension estimates relationship persistence in real estate finance in three applications: multi-family mortgage lending, private equity real estate fundraising, and REIT bond underwriting. For each application, I estimate the probability that a party’s (e.g. borrower’s) sth observed transaction (e.g. new loan) involved a given counterparty (e.g. lender), conditional on that counterparty being involved in the party’s previous transaction. Following Chodorow-Reich (2014), I include counterparty fixed effects, so that the point estimate may be interpreted as the excess probability of a repeat relationship relative to the counterparty’s market share.

First, I focus on the multifamily mortgage market and estimate the probability that the sth loan for borrower b came from lender ℓ, denoted by the indicator Loan Originated\(_{b,ℓ,s}\):

\[
\text{Loan Originated}_{b,ℓ,s} = \rho \text{Loan Originated}_{b,ℓ,s-1} + \alpha_{ℓ,t} + u_{b,ℓ,s}.
\] (C1)

The pairs (b, ℓ) span each possible pair among active borrowers and lenders over 2012-2016. The results in column 1 of Table C1 show that borrowers are 52 pps more likely to obtain their next loan from their previous lender relative to the lender’s market share, captured by the lender-year fixed effect \(\alpha_{ℓ,t}\).

Column 2 shows how relationship persistence is weaker for larger borrowers, measured by log number of properties owned over the sample period, log \((\text{Properties}_b)\). This heterogeneity suggests that information asymmetries, which are plausibly smaller for large borrowers, may make relationships sticky. For example, lenders may incur screening costs when doing business with a new borrower. Alternatively, monitoring costs may be lower for repeat borrowers, to the extent that they are unwilling to default on lenders with whom they have a relationship.

Finally, Figure C1 provides complementary, stylized evidence by plotting the distribution of number of lenders per borrower in the multifamily mortgage market.\(^{48}\) The plot restricts attention to borrowers with at least 2 properties to avoid oversampling small individual investors. Even so, over half of such relatively-large landlords borrow from only 1 lender.

\(^{48}\)The figure is based on the 14\% subset of the Trepp data for which I observe the borrower’s identity.
Table C1: Relationships in Multifamily Mortgage Lending

<table>
<thead>
<tr>
<th>Outcome: Loan Originated(_{b,\ell,s-1})</th>
<th>(0.522^{**})</th>
<th>(0.651^{**})</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>((0.028))</td>
<td>((0.046))</td>
</tr>
<tr>
<td>Loan Originated(_{b,\ell,s-1}) × log((\text{Properties}_b))</td>
<td>-0.068^{**}</td>
<td></td>
</tr>
<tr>
<td></td>
<td>((0.024))</td>
<td></td>
</tr>
<tr>
<td>log((\text{Properties}_b))</td>
<td>0.001^{**}</td>
<td></td>
</tr>
<tr>
<td></td>
<td>((0.000))</td>
<td></td>
</tr>
<tr>
<td>Lender-Year FE</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.307</td>
<td>0.312</td>
</tr>
<tr>
<td>Number of Observations</td>
<td>77316</td>
<td>77316</td>
</tr>
</tbody>
</table>

Note: Subscripts \(b, \ell\) and \(s\) denote borrower, lender, and sequence of loan issued over 2012-2016. This table estimates (C1). Loan Originated\(_{b,\ell,s-1}\) indicates if a loan was originated. The pairs \((b, \ell)\) span each possible pair among active borrowers and lenders over 2012-2016. Properties\(_b\) is the number of properties owned by \(b\) over the sample period. The sample period is 2012-2016. Standard errors clustered by borrower are in parentheses. Data are from Trepp.

Figure C1: Multifamily Mortgage Relationships

Note: This figure plots the distribution of the number of distinct lending relationships per borrower in the multifamily mortgage market across borrowers with more than 1 mortgaged property over 2010-2016. Data are from Trepp.

Next, I turn to the private equity real estate market. The specification is analogous to (C1), after replacing “borrowers” with “private equity real estate fund managers” and “lenders” with “public pensions”. I estimate the probability that pension \(p\) commits capital to the \(s\)th fund for manager \(m\),
denoted by the indicator \( \text{Investment}_{p,m,s} \),

\[
\text{Investment}_{p,m,s} = \rho \text{Investment}_{p,m,s-1} + \alpha_{p,t} + u_{p,m,s}
\]  

(C2)

Similar to before, the pairs \((p, m)\) span each possible pair among active pensions and managers. The results in Table C2 show that fund managers are 22 pps more likely to raise funds from a repeat public pension (i.e. limited partner) relative to what one would predict based on the pension’s size, captured by the pension-year fixed effect \( \alpha_{p,t} \). Moreover, the effect is weaker among large fund managers, measured by log dollar value of private equity real estate funds closed over 2008-2016 and denoted \( \log(\text{Size}_m) \). As discussed above, greater stickiness for relatively small fund managers may reflect screening or monitoring costs.

Table C2: Relationships between Public Pensions and Private Equity Real Estate Fund Managers

<table>
<thead>
<tr>
<th>Outcome: Investment ( p,m,s ) (1)</th>
<th>Investment ( p,m,s ) (2)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Investment ( p,m,s-1 )</td>
<td>0.224** 0.957**</td>
</tr>
<tr>
<td></td>
<td>(0.045) (0.250)</td>
</tr>
<tr>
<td>Investment ( p,m,s-1 ) \times \log(\text{Size}_m)</td>
<td>-0.089**</td>
</tr>
<tr>
<td></td>
<td>(0.028)</td>
</tr>
<tr>
<td>\log(\text{Size}_m)</td>
<td>0.005**</td>
</tr>
<tr>
<td></td>
<td>(0.001)</td>
</tr>
<tr>
<td>Pension-Year FE</td>
<td>Yes Yes</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.093 0.103</td>
</tr>
<tr>
<td>Number of Observations</td>
<td>18060 18060</td>
</tr>
</tbody>
</table>

Note: Subscripts \( p, m \) and \( s \) public pension, private equity real estate fund manager, and sequence of private equity real estate fund formed over 2008-2016. This table estimates (C2). Investment \( p,m,s \) indicates if an investment was made. The pairs \((p, m)\) span each possible pair among active pensions and managers over 2008-2016. Size \( m \) is dollar value of private equity real estate funds closed over 2008-2016. The sample period is 2008-2016. Standard errors clustered by manager are in parentheses. Data are from Preqin.

Finally, I perform a similar exercise in the context of REIT bond underwriting. This exercise provides a lower bound on the importance of relationships in real estate finance, since REITs with access to the bond market plausibly have access to multiple banks to underwrite their next issuance. Similarly to before, I estimate the probability that bank \( u \) leads the underwriting for the \( s \)th bond issuance for REIT \( j \), denoted Lead Underwriter \( j,u,s \), conditional on whether \( u \) was the lead underwriter \( j \)’s previous issuance or was at least a participant underwriter, denoted Lead Underwriter \( j,u,s-1 \) and
Underwriter\textsubscript{j,u,s−1} respectively. The regression is

\[
\text{Lead Underwriter}_{j,u,s} = \rho_0 \text{Lead Underwriter}_{j,u,s−1} + \rho_1 \text{Underwriter}_{j,u,s−1} + \alpha_{u,t} + u_{j,u,s}, \quad (C3)
\]

and the pairs of issuers and underwriters span each possible pair among active institutions over 2000-2017.

Column 1 of Table C3 has the results of (C3). The positive estimate on Lead Underwriter\textsubscript{j,u,s−1} suggests that relationships are sticky even between large REITs and investment banks. While it is difficult to compare magnitudes across specifications, the point estimates are smaller compared to the results of the multifamily mortgage application in Table C1. This is what one would expect, since screening or monitoring costs would seem not seem to constrain REITs with access to the bond market. Column 2 shows that the results are similar when including underwriter-sector fixed effects, which account for investment bank expertise in particular sectors. Columns 3-4 replicate the results when the outcome is participation in, though not necessarily leading, the underwriting.

Table C3: Relationships in REIT Bond Underwriting

<table>
<thead>
<tr>
<th>Outcome:</th>
<th>Lead Underwriter\textsubscript{j,u,s−1}</th>
<th>Underwriter\textsubscript{j,u,s−1}</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>Lead Underwriter\textsubscript{j,u,s−1}</td>
<td>0.224**</td>
<td>0.181**</td>
</tr>
<tr>
<td></td>
<td>(0.058)</td>
<td>(0.058)</td>
</tr>
<tr>
<td>Underwriter\textsubscript{j,u,s−1}</td>
<td>0.016</td>
<td>0.009</td>
</tr>
<tr>
<td></td>
<td>(0.020)</td>
<td>(0.021)</td>
</tr>
<tr>
<td>Underwriter-Year FE</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Underwriter-Sector FE</td>
<td>No</td>
<td>Yes</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.271</td>
<td>0.319</td>
</tr>
<tr>
<td>Number of Observations</td>
<td>49268</td>
<td>49268</td>
</tr>
</tbody>
</table>

Note: Subscripts \textit{j}, \textit{u} and \textit{s} denote bond issuer (i.e. REIT), underwriter, and sequence of bond issue over 2000-2017. This table estimates (C3). Underwriter\textsubscript{j,u,s} indicates if firm \textit{u} was an underwriter of issue \textit{s} for issuer \textit{j}. Lead underwriter\textsubscript{j,u,s} indicates if \textit{u} was the lead underwriter. The pairs of issuers and underwriters span each possible pair among active institutions over 2000-2017. The sample period is 2000-2017. Standard errors clustered by issuer are in parentheses. Data are from NAREIT.

Collectively, the results of this extension indicate that relationships in real estate finance are sticky, supporting the statements made in Sections 4 and 5.
C.2 Structural Rent Index

This extension describes the setup for the structural rent index, briefly outlines its implementation, and discusses the results. It is intended for a reader who would like more information than the high-level description from Section 3.2, but, to keep this extension streamlined, mathematical details are deferred to Online Appendix G and econometric details to Online Appendix F.

C.2.1 Setup

As when constructing the hedonic index, let \( i \in I \) index properties. Define housing quality \( h_i \) as a Cobb-Douglas aggregator of space \( s_i \) and amenities \( a_i \) such that \( \log(h_i) = \mu \log(s_i) + \log(a_i) \). As discussed shortly, \( \mu > 0 \) will govern preferences for quality. Next, let \( H \) denote the highest quality in the market, \( H \equiv \sup_{i \in I} \{h_i\} \), which I will refer to as absolute quality. Finally, define the unit’s quality segment as \( \hat{h}_i \equiv \frac{h_i}{H} \in \{0, \ldots, 1\} \). For example, segment \( \hat{h}_i = 1 \) corresponds to units in Class A properties.

Households, indexed by \( j \), are endowed with income \( y_j \). They have additive random preferences over their choice of shelter with flow utility

\[
    u_{i,j} = \log(h_i) + \epsilon_{i,j},
\]

where \( \epsilon_{i,j} \) is a taste shock. Incorporating consumption does not materially change the analysis.\(^{49}\) Online Appendix G shows how these preferences give rise to a discrete choice problem where households choose a shelter to maximize a geometric average of quality \( h_i \), personal appeal \( \epsilon_{i,j} \), and inverse rent.

The structural index aims to track the utilitarian welfare associated with (C4) or, equivalently, the dollar cost required to maintain this welfare at a fixed level. This dollar cost, which I call “welfare-relevant rent”, is the sum of areas to the left of the Hicksian market demand curve for each segment \( \hat{h}_i \), and its functional form depends on the distribution of \( \epsilon_{i,j} \).\(^{50}\) For the baseline exercise, \( \epsilon_{i,j} \) follows a type 1 extreme value, or Gumbel distribution, which implies that the demand curves in each segment have a constant elasticity of substitution (CES) form.\(^{51}\) The parameter \( \sigma \equiv \mu + 1 \)

\(^{49}\)See Online Appendix G for details. All proofs are in Online Appendix G.

\(^{50}\)Focusing on the market demand curve is standard technique for studying price and welfare in durable goods markets, such as automobiles. See Anderson, de Palma and Thisse (1992) or Eaton and Kortum (2002) for examples with CES market demand. Berry, Levinsohn and Pakes (2004) study automobile consumers with preferences similar to (C4) when taste shocks do not imply CES market demand.

\(^{51}\)See Online Appendix F. This is a well-known result due to Anderson, de Palma and Thisse (1992).
governs the shape of the market demand curve. When \( \sigma \) is high, individual households have less preference for quality and view units in different segments as substitutable.

The following proposition shows how, given \( \sigma \), one can compute the growth in welfare-relevant rent using observed the rent and market share of each segment.\(^{52}\) This growth is the social cost (i.e. compensating variation) associated with a change in the distribution of rent across the quality ladder.\(^{53}\)

**Proposition C.1 (Structural Rent Index)** The compensating variation associated with a change in rent from \( t_0 \) to \( t \) is

\[
\pi_t^S = \exp \left[ \sum_{h \in \mathcal{H}} w_{h,t} \log \left( \frac{Rent_{h,t}}{Rent_{h,t_0}} \right) \right] \times \left[ \left( \frac{Rent_{1,t}}{Rent_{1,t_0}} \right)^{\sigma} \frac{Share_{1,t}}{Share_{1,t_0}} \right]^{-\frac{1}{\sigma-1}} \equiv DQ_t \times GQ_t^{-\frac{1}{\sigma-1}}, \quad (C5)
\]

where \( \mathcal{H} \subseteq [0,1] \) is the set of quality segments, \( Rent_{h,t} \) and \( Share_{h,t} \) are the rent and share of total units in segment \( h \) and year \( t \); the Sato-Vartia weights \( w_{h,t} \) are a function of \( Rent_{h,t} \) and \( Share_{h,t} \); and segment \( 1_0 \) contains units that were in segment \( 1 \) in year \( t_0 \).

The term \( DQ_t \) in (C5) depends on the distribution (hence “\( D \)”) of rent across the quality ladder.\(^{54}\) It reflects how rent growth affects the average household differently depending on whether growth is at the top (\( \hat{h} \) large) or bottom (\( \hat{h} \) small) of the ladder, where each segment’s weight \( w_{h,t} \) encodes households’ willingness to move to that segment. Next, \( GQ_t \) is growth (hence “\( G \)”) in absolute quality.\(^{55}\) When rent on top tier units is higher than units that were top tier in \( t_0 \) (i.e. \( Rent_{1,t} \) large), it reflects an increase in absolute quality (e.g. less noisy dishwashers), and especially so if households have less preference for quality (i.e. \( \sigma \) large). However, if the relative share of top tier units is low (i.e. \( Share_{1,t} \) small), then this rent premium does not reflect absolute quality, but rather a scarcity of newly renovated units. Finally, growth in absolute quality dampens effective rent because \( \sigma > 1 \), but this effect is (exponentially) discounted by \( \frac{1}{\sigma-1} \): when \( \sigma \) is large, households attach less value to quality, and thus its impact on effective rent growth is weak.

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\(^{52}\)As discussed below, there are a number of ways to partition the market into quality segments \( \hat{h} \). I use official property inspection scores in my baseline analysis.

\(^{53}\)Strictly speaking, “compensating variation” is the difference in welfare-relevant rent (i.e. the market’s minimized cost function) following a change in the observed distribution of rent. I will misuse the term slightly and refer to the growth in welfare-relevant rent as “compensating variation”.

\(^{54}\)The term is standard in price indices with CES market demand and has its origins in Diewert (1976), Sato (1976), and Vartia (1976). See Feenstra (1994) or Broda and Weinstein (2006) for additional discussion.

\(^{55}\)This term relies on a similar insight as Redding and Weinstein (2018), who also exploit the CES first order condition to obtain an expression for the change in quality. However, Redding and Weinstein (2018) do not impose a hierarchy of quality and do not allow quality to grow on average.
C.2.2 Implementation

I compute the structural rent index $\pi_t^S$ in (C5) using the Trepp data, which, as mentioned in Section 2.1, cover multifamily properties over 2010-2016. These data have detailed property improvement records that help me identify the index’s key parameter, $\sigma$, which I estimate using three strategies: (1) a property-level strategy utilizing idiosyncratic variation in payment timing, which is similar to that used in Section 4.2; (2) a zip code level version of the property-level strategy; and (3) a zip code level strategy based on a widely-used GMM estimator proposed by Feenstra (1994). Due to space constraints, I defer details on these strategies to Appendix F.1. The average estimate for $\sigma$ is 6.5, as shown in Table E1. Figure E1 performs an introspective exercise with photographs to interpret this magnitude.

The second piece of information needed to compute $\pi_t^S$ is a sorting variable to partition units into quality segments $\hat{h}$. I partition the market using official property inspection ratings conducted by the Mortgage Bankers Association and Commercial Real Estate Finance Council (MBA/CREFC), although the results are similar when sorting properties by effective age.\(^{56}\)

C.2.3 Results

Figure C2 summarizes growth in the structural rent index $\pi_t^S$ and various related indices over 2010-2016. The baseline CES index saw real growth of 0.2% compared to 1.4% growth in average rent, which implies that improving quality can account for 86% of 2010-2016 real rent growth.\(^{57}\) As before, the effect is stronger when benchmarking to an age adjusted index similar to that used by statistical agencies. Discounted growth in absolute quality, $GQ_t^{\frac{1}{\sigma} - 1}$, accounts for 73% of the wedge between age adjusted and structural rent growth.\(^{58}\) This is consistent with descriptions of an “amenities arms race” in the apartment industry.\(^{59}\)

In an accompanying document, I show how to use a quasi hedonic methodology to infer growth in absolute quality, which can be applied to an arbitrary price index formula. This approach uses the same intuition from Proposition C.1 that growth in absolute quality can be inferred from the

\(^{56}\)This rating captures a property’s quality relative to the top of its market, and it is regularly collected as part of the multifamily mortgage servicing protocol with the intent of minimizing agency frictions. Markets are defined as a geographic zone of competition and are between a county and an MSA in size. Appendix B provides more details, including evidence that the MBA/CREFC rating does not suffer misreporting bias.

\(^{57}\)The Trepp data are at the property-level, and so I weight properties by the number of units.

\(^{58}\)Explicitly, fixing $GQ_t^{\frac{1}{\sigma} - 1} = 1$ leads to growth of 1.3% compared to 0.2% when using its estimated value. Growth in absolute quality therefore accounts for $\frac{1.3 - 0.2}{1.3} = 73\%$ of the wedge between age adjusted and structural rent growth.

\(^{59}\)See the Washington Post article “An amenities arms race heats up in the apartment industry” (Orton, 2017).
Figure C2: Summary of Structural Rent Index

![Figure C2: Summary of Structural Rent Index](image)

Note: This figure plots average annual growth in real rent over 2010-2016 for various rent indices. Unadjusted denotes average rent. Age Adjusted rent growth performs an age adjustment similar to that used by statistical agencies and is described in Appendix C.5. Baseline denotes the structural index from (C5). Data are from Trepp.

premium of top tier units over units that were top tier in the base period. These non-CES indices all have real growth rates between 0.1% and 0.2%. These results are available upon request.

C.3 Implications for Inequality in Housing Consumption

This extension asks how effective rent has varied by income. My focus is on heterogeneity across geographic markets and submarkets. The structural index is the more natural tool for this exercise because it better accommodates heterogeneous valuations of quality, which would arise due to, say, non-homothetic preferences. I use the Trepp data because of its detailed information on property location.

For part of this analysis, I allow the preference parameter $\sigma$ to vary in the cross-section. Following Jaravel (2018), I partition the sample into brackets by zip code real income and then reestimate $\sigma$ for each bracket. The results in Table E2 show that the highest income zip codes have

---

60Markets are typically between a county and MSA in size, and submarkets are between a zip code and a county, per the MBA/CREFC property inspection guidelines.

61Appendix G.1 provides a microfoundation based on the notion that space $s_i$, unlike amenities $a_i$, is a necessity.

62Income is measured using average adjusted gross income from the IRS, as described in Appendix B. The real income brackets are 0-30%, 30-65%, and 65%-100%. I estimate $\sigma$ with each of the three methodologies described in
the lowest value of $\sigma$ (4.9) and thus the highest willingness to pay for quality. Next, I partition the set of zip codes into an above and below median cohort according to average income over 2010-2016. Then I recompute $\pi^S_t$ for the two cohorts using each zip code $z$’s estimated preference parameter $\sigma_z$. One should interpret $\pi^S_t$ as welfare-relevant rent for the average household in a given cohort.

Figure C3 plots real growth in real unadjusted rent and the structural index $\pi^S_t$ over 2010-2016 by income cohort. Beginning with the left column, unadjusted real rent growth was, coincidentally, 1.3% for both cohorts. The middle column accounts for differences in quality, but constrains preferences to be the same. Whereas improving quality can explain all of real rent growth for the high income cohort, quality actually fell somewhat in low income markets. The right column relaxes the constraint on preferences, after which welfare-relevant rent growth falls by an additional 0.9 pps for the high income cohort. Altogether, household surplus from improving quality was 2.5 pps greater in high income markets, of which 64% (1.6 pps) was due to material changes in quality and 36% (0.9 pps) was due to greater preferences for it. The joint importance of quality and preferences is consistent with a model where investors make improvements where the equilibrium price of quality is highest.

Figure C4 obtains a similar finding when partitioning by within-MSA income, either averaged over 2010-2016 or as measured initially in 2010. The divergence in the gains from quality is strongest when partitioning by initial income, consistent with a view of “super gentrification” and the Guerrieri, Hartley and Hurst (2013) model of endogenous provision of amenities.

Finally, I utilize the microdata associated with the hedonic index to ask which inhabitants of a given housing unit experience quality discounts. Specifically, I estimate a repeat income model (Rosenthal 2014) across housing units $i$ and years $t$,

\[
\text{Effect of Improvements}_{i,t} = \beta \text{Income Percentile}_{i,t} + \alpha_i + \alpha_t + u_{i,t}
\]  

where Effect of Improvements$_{i,t}$ is the difference between growth in observed rent and the hedonic index and Income Percentile$_{i,t}$ is the householder’s U.S. income percentile. The results in Figure C5 reveal a positive and significant relationship. To interpret, within a given housing unit, increases in quality necessitate parallel increases in the householder’s income. Moreover, within a given year, improvements occur where incomes are higher, corroborating the conclusion from Figure C3 that improvements are targeted toward where the equilibrium price of quality is greater. This finding suggests improvements may make it difficult for low income households within high income markets .

Section 3.2 and average across them.
Figure C3: Structural Rent Index by Income

Note: This figure plots average 2010-2016 growth in unadjusted and structural rent indices for properties in zip codes with high or low income. High is defined as having average household income over 2010-2016 above the median across zip codes, and low is defined conversely. The leftmost column plots unadjusted average rent growth. The rightmost column plots growth in the baseline CES index in (C.1) using each zip code’s estimated demand parameter $\sigma_z$. The middle column fixes $\sigma_z$ at the average value for the low income cohort. Data are from Trepp.

to find an appropriate place in which to live.
Note: This figure plots average 2010-2016 growth in unadjusted and structural rent indices for properties in zip codes with a high or low value of the indicated variable. High is defined as above median, and low is defined conversely. Overall Income denotes average household income over 2010-2016. MSA Relative Income denotes average household income over 2010-2016 after demeaning by the surrounding MSA. Initial MSA Relative Income denotes average household income in 2010 after demeaning by the surrounding MSA. The structural index is the baseline CES index in (C.1) using each zip code’s income-based demand parameter $\sigma_z$. Data are from Trepp.

Note: This figure plots the relationship between: (i) the difference between growth in observed rent and the hedonic index and (ii) the householder’s U.S. income percentile. The plot is residualized against housing unit and year fixed effects, and the regression is in (C6). Each observation is a housing unit-year. The plot is binned. Data are from the AHS.
C.4 Implications for Asset Pricing

This extension studies how improvements vary by initial real estate valuations in a market. According to the logic of standard asset pricing, the cap rate (i.e. dividend-price ratio) should convey information about a unit’s future rent (i.e. dividend) growth (Campbell and Shiller 1988). To test this hypothesis, I sort zip codes – which I will call submarkets in this extension – within each MSA according to the 2010 cap rate on multifamily properties and then compute quality-adjusted rent growth over 2010-2016 for zip codes with an above or below average value. The results in Figure C6 show how quality-adjusted rent growth was substantially lower in submarkets with a high initial cap rate (i.e. dividend-price ratio). This is consistent with rational expectations and the view that cheap properties necessitated substantial improvement to command their observed rent. By contrast, quality-adjusted rent growth was actually higher than observed growth in submarkets where the initial cap rate was lower. Panel (b) of Figure C6 sorts submarkets by house price decline during the 2006-2009 collapse and reveals a similar result: submarkets where property values fell by more during the crash saw subsequently greater improvements in housing quality.

C.5 Relationship to Official Rent Indices

This section relates the results from Section 3 to what one would obtain from an age adjustment procedure similar to that used by statistical agencies. Following Gallin and Verbrugge (2007), I define the age adjustment regression as

$$\log (Rent_{i,t}) = \gamma (Age_{i,t}; X_{i,t}) + u_{i,t},$$  \hspace{1cm} (C7)

where Rent$_{i,t}$, Age$_{i,t}$, and X$_{i,t}$ are, respectively, a unit’s rent, the age of the property, and a vector of structural features. Then, one computes a unit’s age adjusted rent as Rent$_{A,i,t} \equiv Rent_{i,t}e^{-\frac{\partial \gamma}{\partial Age_{i,t}}}$ and aggregates Rent$_{A,i,t}$ across units to produce an average rent $\pi_{t}^A$ that is benchmarked to the reference

---

63 The cap rate equals the ratio of net operating income to appraised property value and is therefore similar to a dividend-price ratio.

64 Age is the primary attribute the Bureau of Labor Statistics (BLS) corrects for when computing the Rent of Primary Residence (Ptacek 2013). The other corrections pertain to the changes in the inclusion of parking or utilities in rent, and the addition of a new room or central air conditioning.

65 The function $\gamma (Age_{i,t}; X_{i,t})$ approximates that used by the BLS as closely as possible given a different dataset. It includes age, its square, and its interaction with: the number of units in the property and an indicator for whether the property is over 85 years old. Since I do not observe a unit’s location and thus neighborhood features in the AHS data, I estimate (C14) as a panel regression and include a property fixed effect. When using the Trepp data, I weight observations in (C14) by number of units because the data are at the property-level.
Figure C6: Forecasting Quality Growth with Submarket Indicators

Note: This figure plots average 2010-2016 growth in adjusted and structural rent indices for properties in zip codes with a high or low value of the indicated variable. High is defined as above the average of the surrounding MSA, and low is defined conversely. House Price Decline 2006-09 denotes the change in the zip code level Zillow Home Value Index between 2006 and 2009. Cap Rate 2010 denotes the average ratio of net operating income to appraised value in the zip code in 2010. The structural index is the baseline CES index in (C.1). Data are from Trepp.

period, similarly to (3),

$$
\pi^A_t = \frac{\sum_{i \in I} Rent^A_{i,t}}{\sum_{i \in I} Rent_{i,t_0}},
$$

(C8)

In the Online Appendix, I show age adjustments used to construct official indices can be biased upward and decomposes the bias into two terms. The first term relates to the rate at which housing units depreciate, and the second relates to growth in absolute quality. Interviewing landlords, as opposed to tenants as currently practiced, would provide more information about improvements that could be used to address the first source of bias. Addressing bias from growth in absolute quality is conceptually more difficult. However, new tools from the price adjustment literature, such as Redding and Weinstein (2018) or the methodology briefly described in Section 3.2 of this paper, are steps toward addressing the challenge.

Ambrose, Coulson and Yoshida (2018) argue that reporting lags are an additional rationale for interviewing landlords.
C.6 Implications for Innovation in Property Management

In the Online Appendix, I discuss the relationship between improving quality and recent “proptech” innovation, including the possibility that innovation in property management software may be a response to the growing market for high quality housing (Acemoglu and Linn 2004).

C.7 Bank Lending in the Syndicated Loan Market

Due to institutional and data differences, the specification must be modified from (5). First, although nonbanks play an important role in syndicated loan markets, they are often pensions or insurance companies (Ivashina and Scharfstein 2010). Unlike the specialty lenders in the multifamily mortgage market, these nonbanks are subject to substantial oversight, and some are even subject to HVCRE regulation. This makes it more difficult to identify the effect of HVCRE regulation off of the difference between nonbank and bank behavior. Instead, I appeal to a literature which has documented how Comprehensive Capital Analysis and Review (CCAR) stress tests have encouraged banks to exercise more cautious lending behavior (Calem, Correa and Lee 2016; Gete and Reher 2018a). These tests, first implemented in 2011, are meant to ensure that the largest bank holding companies have enough capital to weather a financial crisis, and their standards are substantially more stringent than ordinary DFAST stress tests. Accordingly, lenders subject to CCAR tests have an incentive to closely adhere to HVCRE regulation, and they are the “treated lenders” in this research design.

Second, unlike with the multifamily mortgage data, I do not observe whether a loan finances an improvement versus a construction project. However, I do observe whether the borrower’s primary business activity is construction based on their associated SIC code. Developers therefore represent “treated borrowers”, in contrast to the control group, REITs, which perform both property improvements and construction.68

I therefore estimate the following specification

\[
\text{New Loan}_{b,\ell,t} = \beta (\text{CCAR}_t \times \text{Post}_t \times \text{Developer}_b) + \alpha_{\ell,t} + \alpha_{b,t} + \alpha_{b,\ell} + u_{b,\ell,t},
\]

where \( b, \ell, \) and \( t \) index borrowers, lenders, and years, \( \text{New Loan}_{b,\ell,t} \) indicates whether a new secured

---

67There are some non-depository institutions, like Prudential, which are classified as Designated Financial Companies and thus required to compute risk-based capital requirements as if they were a bank holding company. Since my focus is on the effects of capital requirements, I classify such lenders as banks in the baseline analysis.

68There is not a clear industry classification for firms that specialize in property improvements. While REITs do perform both construction and improvements, their return profile more closely resembles private value added funds, which specialize in improvements, rather than opportunistic ones, which specialize in construction (Morningstar 2011).
Table C4: Loans to Developers and HVCRE Regulation

<table>
<thead>
<tr>
<th>Outcome: New Loan_{b,\ell,t}</th>
<th>(1)</th>
<th>(2)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Developer_{b} \times \text{Post}<em>t \times \text{CCAR}</em>\ell</td>
<td>-0.026**</td>
<td>-0.028**</td>
</tr>
<tr>
<td>(0.008)</td>
<td>(0.009)</td>
<td></td>
</tr>
<tr>
<td>Lender-Borrower FE</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Lender-Year FE</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Borrower-Year FE</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Sample</td>
<td>All</td>
<td>No Bond</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.439</td>
<td>0.452</td>
</tr>
<tr>
<td>Number of Observations</td>
<td>42120</td>
<td>15990</td>
</tr>
</tbody>
</table>

Note: Subscripts \(b\), \(\ell\), and \(t\) denote borrower, lender, and year. This table estimates (C9). New Loan_{b,\ell,t} indicates if a new secured loan was originated. Developer_{b} indicates if the firm is a land developer as opposed to a REIT. CCAR_{\ell} indicates if the lender is subject to CCAR stress tests. The pairs of borrowers and lenders span each possible pair among institutions active in the syndicated loan market over 2012-2016. Pairs are weighted by the lender’s loan issuance over this period. The second column drops REITs with access to the bond market over 2012-2016. The sample period is 2012-2016. Standard errors two-way clustered by borrower and lender are in parentheses. Data are from DealScan.

The parameter of interest in (C9) is \(\beta\), which is the effect of the triple interaction between treated borrowers (Developer_{b}) of treated lenders (CCAR_{\ell}) in the treatment period (Post_{t}). The fixed effects \(\alpha_{b,\ell}\) and \(\alpha_{\ell,t}\) restrict the variation used to identify \(\beta\) to two sources. First, within a borrower-lender pair, treated lenders and borrowers may exhibit different deal-signing behavior after the introduction of HVCRE regulations. Second, within the same lender and year, a treated lender in the post-HVCRE period may behave differently towards treated borrowers.

The results in Table C4 show that CCAR lenders, for whom the regulatory cost of a low capital ratio is greater, were less likely to lend to developers after the introduction of HVCRE regulations. Interpreting the first column, developers were 2.8 pps less likely to receive a loan from a CCAR lender in the post-HVCRE period. As discussed in the text, the real effects of this shock depend on borrowers’ ability to substitute between lenders. It is therefore important to check whether the results are driven by REITs with access to the bond market, for whom this substitutability is plausibly higher. The second column of Table C4 drops such borrowers from the sample, which yields a similar result. This suggests that HVCRE regulations transferred capital from firms specializing in

\[\text{As in the baseline specification (5), pairs are weighted by the lender’s loan issuance over this period.}\]
construction to firms which perform improvements, consistent with the baseline results in Table 1.

C.8 Property-Level Effect with Idiosyncratic Payment Timing

I estimate a property-level specification that makes use of idiosyncratic variation in payment timing and is methodologically similar to Almeida, Campello, Laranjeira and Weisbenner (2012). This variation generates effectively exogenous credit demand shocks, and the logic of the exercise is to ask whether these shocks resulted in more improvement activity when the supply curve also shifted out because of HVCRE regulation. Thus, this approach can limit variation to very narrow bins and requires weak identification assumptions.

The methodology is similar to that used to estimate the parameters of the structural rent index in Section 3.2. It begins with the observation that most commercial mortgages – of which multifamily mortgages are an example – are balloon loans that do not permit refinancing during the interim period. Consequently, property owners with an impending loan due have an incentive to postpone improvements until after renewal because of the possibility of cheaper financing. I verify this behavior by estimating

\[ Y_{i,\ell,t} = \sum_{r=-1}^{1} \beta_r \text{Due}_{i,t+r} + \alpha_{i,\ell} + \alpha_{z,t} + \alpha_{\ell,t} + u_{i,\ell,t}, \] (C10)

where \( i, \ell, \) and \( t \) index properties, lenders, and years, and \( \text{Due}_{i,t} \) indicates whether property \( i \) has a mortgage due in \( t \). The property-lender fixed effect \( \alpha_{i,\ell} \) limits variation to the same relationship, and the zip code-year and lender-year fixed effects \( \alpha_{z,t} \) and \( \alpha_{\ell,t} \) account for contemporaneous local demand and credit supply shocks, respectively.

The outcome \( Y_{i,\ell,t} \) is a measure of quality improvement. One option would be to study renovations, which are the focus of the county-level analysis because they can be mapped to aggregate statistics. However, because the annual renovation hazard is only 3.4%, there is not enough variation to feasibly pursue this route. Instead, I study changes in the MBA/CREFC rating, a professional property inspection score that captures a property’s quality segment and is regularly collected as part of the multifamily mortgage servicing protocol. This measure captures more modest improvements in quality (e.g. repainting common areas), and thus there is enough variation in estimate (C10). My

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70The modal term in the sample is 10 years, and 99% of outstanding balances are on balloon loans.

71I cannot include borrower-lender fixed effects because I only observe the borrower’s identity, used to construct Figure C1, for 14% of the sample.

72Appendix B has details on why this rating is collected, its interpretation, and scope for misreporting bias.
Figure C7: Quality Improvements and Credit Risk Related to Loan Due Dates

Note: This figure plots results from regressions similar to column 1 of Table C5. Panel (a) estimates a regression of the change in a property’s log relative quality on indicators for whether the property’s loan is due the subsequent, current, or previous year. Quality is based on the MBA/CREFC rating, and the change in log quality is normalized to have unit variance. Panel (b) estimates a regression of the variable on the horizontal axis on an indicator for whether the property has a loan due in the current or subsequent year, denoted Impending, and log loan term. Variables are normalized to have unit variance. Rate Spread denotes the difference between the loan’s interest rate and the average interest rate among loans with the same year of origination. LTV is the current loan-to-value ratio. Log Property Size is the log of the property’s size, in units. In both panels, the regressions include property-lender, lender-year, and zip code-year fixed effects. Observations are property-years. The sample period is 2010-2016. Brackets are a 95% confidence interval with standard errors clustered by property. Data are from Trepp.

outcome $Y_{i,ℓ,t}$ is the change in the log MBA/CREFC rating from $t-1$ to $t$, denoted $\Delta \log \left( \text{Quality}_{i,ℓ,t} \right)$, which is normalized to have unit variance.\footnote{There is a 6.8% annual hazard of improvement according to the MBA/CREFC rating. See Appendix B for additional summary statistics.}

Panel (a) of Figure C7 plots the estimated coefficients from (C10). Consistent with the incentives provided by the structure of multifamily mortgages, a property’s quality declines as the due date approaches, indicated by the negative point estimates for $t \leq 0$. This behavior sharply reverses afterward as improvements are made, shown by the positive estimate for $t = 1$. Because all variation comes from the same lending relationship, this pattern does not reflect an outside purchase-and-fix transaction.

Panel (b) of the figure asks whether borrowers with an impending loan due see a deterioration in property quality because they are inherently riskier. For example, perhaps they borrowed at the peak of the pre-crisis boom. To do this I reestimate (C10) replacing the dependent variable with a measure
of credit risk and the independent variable with an indicator whether the loan is due in $t$ or $t+1$, denoted $\text{Impending}_{i,t}$.\footnote{Explicitly, $\text{Impending}_{i,t} = \max\{\text{Due}_{i,t}, \text{Due}_{i,t+1}\}$. The measures of credit risk are the difference between the loan’s interest rate and the average interest rate among loans with the same year of origination, current loan-to-value ratio, log of the property’s size in number of units, and property’s occupancy rate. These variables are normalized to have unit variance.} The results in Figure C7b indicate that borrowers with an impending loan due are not riskier than the rest of the sample, suggesting that $\text{Due}_{i,t}$ indeed captures idiosyncratic variation.

If having a loan due is an idiosyncratic demand shock for improvement financing, then the effect of this shock on improvement activity should depend on the shape of the credit supply curve, which may have shifted out because of HVCRE regulation. I test this hypothesis by estimating

$$
\Delta \log (\text{Quality}_{i,\ell,t,t+1}) = \beta (\text{Bank}_{\ell} \times \text{Post}_{t} \times \text{Due}_{i,t}) + \alpha_{i,t} + \alpha_{z,t} + \alpha_{\ell,t} + ... \tag{C11}
\quad ... + \alpha_{t} \times \text{Due}_{i,t} + \alpha_{\ell} \times \text{Due}_{i,t} + u_{i,\ell,t}.
$$

The parameter $\beta$ represents an HVCRE-induced movement along borrowers’ demand curve for making improvement projects ($\text{Bank}_{\ell} \times \text{Post}_{t}$), conditional on this demand curve experiencing an outward shift ($\text{Due}_{i,t}$). As discussed in the text, what makes (C11) unique is that both demand and supply shocks are observed, and thus identification can come from their product. By contrast, the conventional approach would be to remove demand shocks as a fixed effect (e.g. Khwaja and Mian 2008). Moreover, all variation comes from within lender-years, so that any confounding variation would need to reflect a difference between coming-due and other borrowers of treated lenders, which seems unlikely given Figure C7b.

The results are in Table C5. Column 1 provides necessary context by estimating the effect of having a loan due on subsequent growth in quality.\footnote{Specifically, column 1 estimates (C10) when restricting the lag terms to $\tau = -1$.} The estimates of (C11) are in column 2, and they suggest that HVCRE regulation increased the effect of having a loan due by 0.13 standard deviations, or 170% of the baseline effect in column 1. As in Section 4.2, this finding shows how changes in credit supply can affect the number and quality of completed projects by firms, in this case property investors.

To facilitate interpretation and build a bridge with the first part of the paper, column 3 uses the triple interaction as an instrument for the property’s log loan balance, normalized to have unit variance.\footnote{I use a 2SLS estimator. The first stage F-statistic is 19.93 and coefficient on $\text{Bank}_{\ell} \times \text{Post}_{t} \times \text{Due}_{i,t}$ is 0.43.} The point estimate suggests that a 1 standard deviation increase in credit raises subsequent
Table C5: Quality Improvements after Loan Renewal

<table>
<thead>
<tr>
<th>Outcome:</th>
<th>$\Delta \log (Quality_{i,t,t+1})$</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
</tr>
</thead>
<tbody>
<tr>
<td>$Due_{i,t}$</td>
<td>0.074** (0.025)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$Bank_\ell \times Post_t \times Due_{i,t}$</td>
<td>0.129** (0.048)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$\log (Balance_{i,t})$</td>
<td></td>
<td>0.300** (0.113)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Property-Lender-FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td></td>
</tr>
<tr>
<td>Zip Code-Year FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td></td>
</tr>
<tr>
<td>Lender-Year FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td></td>
</tr>
<tr>
<td>Due-Lender FE</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
<td></td>
</tr>
<tr>
<td>Due-Year FE</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
<td></td>
</tr>
<tr>
<td>Estimator</td>
<td>OLS</td>
<td>OLS</td>
<td>IV</td>
<td></td>
</tr>
<tr>
<td>R-squared</td>
<td>0.496</td>
<td>0.497</td>
<td>0.482</td>
<td></td>
</tr>
<tr>
<td>F Statistic</td>
<td>19.930</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Number of Observations</td>
<td>143530</td>
<td>143530</td>
<td>143530</td>
<td></td>
</tr>
</tbody>
</table>

Note: Subscripts $i$, $\ell$, and $t$ denote property, lender, and year. Column 1 estimates (C10) and columns 2-3 estimate (C11). $Due_{i,t}$ indicates if the property owner’s loan is due in year $t$. $Bank_\ell$ denotes if lender $\ell$ is a bank. $Post_t$ indicates if $t$ is greater than or equal to 2015. $Balance_{i,t}$ is the end-of-period loan balance. The outcome $\Delta \log (Quality_{i,t,t+1})$ is the one-year change in log quality, measured with the MBA/CREFC rating. The variables $\Delta \log (Quality_{i,t,t+1})$ and $\log (Balance_{i,t})$ are normalized to have unit variance. The estimator is OLS except for column 3, where $\log (Balance_{i,t})$ is instrumented with the triple interaction between $Bank_\ell$, $Due_{i,t}$, and $Post_t$. Due-Lender fixed effects are a set of interactions between an indicator for whether the loan is due and the current lender, and Due-Year fixed effects are similarly defined in terms of interactions with year indicators. Observations are property-years. The sample period is 2010-2016. Standard errors two-way clustered by property and year are in parentheses. Data are from Trepp.

growth in quality by 0.3 standard deviations. Using the estimated willingness to pay for quality of $\frac{1}{\sigma} = 0.15$ and the associated estimating equation from Online Appendix F.1.1, it also leads to a 3.6% increase in rent.\(^77\)

C.9 External Validity of Bank Lending Estimates

I now assess the external validity of the bank lending estimates from Section 4. The particular issue, described in Section 4.4, is that the Trepp dataset only includes units in properties whose loan was eventually securitized. HVCRE regulation would still affect origination incentives for such loans because of non-trivial lags between origination and securitization (i.e. warehouse periods), risk retention requirements, and the possibility that the loan was not originated with the intent

\(^77\)Explicitly, the standard deviation of $\Delta \log (Quality_{i,t,t+1})$ is 0.80, so that, per column 2 of Table E3, a 1 standard deviation increase in credit in $t$ increases rent in $t+1$ by $0.3 \times 0.8 \times 0.15 = 3.6\%$. 

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of securitization, which is plausible for 43% of the sample. However, the effect would presumably be stronger among loans that were never securitized, so that the estimates from Table 3 may be considered a lower bound. The following exercises lend support to this conservative interpretation.

First, Table C6 reestimates the lender-year specification (4) replacing the outcome with an indicator for whether the loan was securitized within 3 months, a relatively standard warehouse period (Echeverry, Stanton and Wallace 2016). The result shows that banks decreased the rate at which they securitized improvement loans relative to construction ones after HVCRE regulation. Thus, I observe fewer improvement loans than were actually originated, again consistent with the baseline results being a lower bound.

Table C6: Securitization Speed by Loan Purpose

<table>
<thead>
<tr>
<th>Outcome:</th>
<th>Sec in 3 Months$_{k,\ell,t}$</th>
</tr>
</thead>
<tbody>
<tr>
<td>Bank$<em>{\ell} \times$ Post$</em>{t} \times$ Imp$_{k}$</td>
<td>-0.445** (0.156)</td>
</tr>
<tr>
<td>Lender-Purpose FE</td>
<td>Yes</td>
</tr>
<tr>
<td>Lender-Year FE</td>
<td>Yes</td>
</tr>
<tr>
<td>Purpose-Year FE</td>
<td>Yes</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.725</td>
</tr>
<tr>
<td>Number of Observations</td>
<td>366</td>
</tr>
</tbody>
</table>

Note: Subscripts $k$, $\ell$, and $t$ denote loan purpose, lender, and year. The specification is similar to column 1 of Table 1 except that the outcome differs. Sec in 3 Months$_{k,\ell,t}$ is the principal-weighted share of loans for purpose $k$ securitized within 3 months of origination. The remaining notes are the same as in Table 1. Standard errors clustered by lender are in parentheses.

Second, I reestimate the county-level specification (7) using a novel dataset on bank portfolio loans, and then I compare the estimates with those obtained using the baseline data. The new dataset, called T-ALLR, is also provided by Trepp and described in Appendix B.2. These data have some limitations that make them inappropriate for the baseline analysis. Most importantly, I cannot observe whether the loan financed an improvement and only observe the location of the encumbered property for a small subset of loans. With these data constraints in mind, I estimate the specification from column 3 of Table 3 without state-year fixed effects, and the outcome variable is now log loan originations for purposes other than construction.
### Table C7: County-Level Non-Construction Lending

<table>
<thead>
<tr>
<th>Outcome</th>
<th>(1)</th>
<th>(2)</th>
</tr>
</thead>
<tbody>
<tr>
<td>log (Non-Dev Loans(_{c,t}))</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Bank Share(_{c}) × Post(_t)</td>
<td>0.588</td>
<td>0.312**</td>
</tr>
<tr>
<td></td>
<td>(1.154)</td>
<td>(0.099)</td>
</tr>
</tbody>
</table>

**Note:** Subscripts \(_c\) and \(_t\) denote county and year. The specification is similar to column 1 of Table 7 except that the outcome differs. Bank Share\(_c\) is banks’ share of multifamily mortgage balances in 2011. Post\(_t\) indicates if \(_t\) is greater than or equal to 2015. Non-Dev Loans\(_{c,t}\) is the number of loans issued for non-construction purposes. County controls are those from Table 3. Observations are county-years weighted by the average number of multifamily units over 2011-2016. The sample period is 2011-2016. Standard errors are in parentheses. Data on the outcome variable come from T-ALLR in column 1 and the baseline Trepp dataset in column 2.

The results in column 1 of Table C7 suggest that counties with a 10 pps higher bank exposure received 59% more non-construction loans after the introduction of HVCRE regulation. While the standard error is understandably large given the small sample size, it is instructive to compare the point estimate to that obtained using the baseline Trepp data. The estimated coefficient in column 2 is roughly half that obtained using the portfolio loan data. This finding supports the interpretation of the baseline results from Section 4.3 as a lower bound.

### C.10 Robustness of Pension Research Design

This extension performs robustness tests related to the pension research design from Section 5.

1. **Manager Skill:** It is possible that well-funded pensions are run by skilled managers, and the results are driven by a declining alpha of value added real estate funds. However, if this were the case the point estimates should change substantially after the inclusion of pension controls, including realized return, in Table 5. By contrast, the point estimates are very similar regardless of whether these controls are included.

2. **Nominal Yields:** Underfunded pensions may have stronger cost-of-living adjustments (COLAs) and would thus be drawn to real estate investments because they hedge inflation risk. This sorting could generate the results if declines in the TIPS yield over the sample period primarily
reflected higher inflation expectations. In that case, one would expect to find no effect when replacing the TIPS yield with a nominal yield of the same credit risk and maturity. However, the first three columns of Table C8 reveal similar results when using nominal 10-year Treasury or Aaa corporate bond yields to measure Yield Gap_t.

Table C8: Public Pension Investment in Value Added by Yield Measure

<table>
<thead>
<tr>
<th>Outcome: Prob of Commitment^{VA}_{p,t}</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Funding Gap_p × Yield Gap_t</td>
<td>0.110**</td>
<td>0.153**</td>
<td>0.161**</td>
</tr>
<tr>
<td></td>
<td>(0.046)</td>
<td>(0.052)</td>
<td>(0.053)</td>
</tr>
<tr>
<td>Yield Measure</td>
<td>Treasury</td>
<td>Corp</td>
<td>TIPS</td>
</tr>
<tr>
<td>Pension FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>State-Year FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.712</td>
<td>0.715</td>
<td>0.715</td>
</tr>
<tr>
<td>Number of Observations</td>
<td>520</td>
<td>520</td>
<td>520</td>
</tr>
</tbody>
</table>

Note: Subscripts p and t denote pension fund and year. The specification is similar to column 1 of Table 5 using different measures of the safe yield. Yield Gap_t is the difference between the indicated yield measure in 2008 and in t. Treasury, Corp, and TIPS indicate the 10-year Treasury real yield, Moody’s Aaa corporate bond real yield, and the 10-year TIPS yield. Standard errors clustered by pension are in parentheses. Data are from Preqin and the CRR.

3. Safe and Very Risky Fund Strategies: If the pension-level results were driven by changing real estate fundamentals, one would expect to see a similar effect for safer core funds. However, Table C9 provides evidence that underfunded pensions weakly decreased their investment in core real estate funds when safe yields fell. Table C10 performs a symmetric test with respect to opportunistic real estate funds, which perform construction projects and command a high risk premium.\(^{78}\) Unlike core funds, underfunded pensions became more likely to invest in opportunistic real estate funds when safe yields fell.

\(^{78}\)Opportunistic funds have a historic average net return of 13.5% with a standard deviation of 19.2% (Pagliari 2017).
Table C9: Safe Real Estate Investments and Pension Risk Taking

<table>
<thead>
<tr>
<th>Outcome:</th>
<th>Prob of Commitment(^\text{Core}_{p,t})</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
</tr>
<tr>
<td>Funding Gap(_p) × Yield Gap(_t)</td>
<td>-0.101(^*)</td>
</tr>
<tr>
<td></td>
<td>(0.054)</td>
</tr>
<tr>
<td>Yield Measure</td>
<td>Treasury</td>
</tr>
<tr>
<td>Pension FE</td>
<td>Yes</td>
</tr>
<tr>
<td>State-Year FE</td>
<td>Yes</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.633</td>
</tr>
<tr>
<td>Number of Observations</td>
<td>520</td>
</tr>
</tbody>
</table>

Note: Subscripts \(p\) and \(t\) denote pension fund and year. The specification is similar to column 1 of Table C8 except that the outcome differs. Prob of Commitment\(^\text{Core}_{p,t}\) indicates an investment in a core real estate fund. Yield Gap\(_t\) is the difference between the indicated yield measure in 2008 and in \(t\). Treasury, Corp, and TIPS indicate the 10-year Treasury real yield, Moody’s Aaa corporate bond real yield, and the 10-year TIPS yield. Standard errors clustered by pension are in parentheses. Data are from Preqin and the CRR.

Table C10: Riskiest Real Estate Investments and Pension Risk Taking

<table>
<thead>
<tr>
<th>Outcome:</th>
<th>Prob of Commitment(^\text{Opp}_{p,t})</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
</tr>
<tr>
<td>Funding Gap(_p) × Yield Gap(_t)</td>
<td>0.103(^{**})</td>
</tr>
<tr>
<td></td>
<td>(0.046)</td>
</tr>
<tr>
<td>Yield Measure</td>
<td>Treasury</td>
</tr>
<tr>
<td>Pension FE</td>
<td>Yes</td>
</tr>
<tr>
<td>State-Year FE</td>
<td>Yes</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.633</td>
</tr>
<tr>
<td>Number of Observations</td>
<td>520</td>
</tr>
</tbody>
</table>

Note: Subscripts \(p\) and \(t\) denote pension fund and year. The specification is similar to column 1 of Table C8 except that the outcome differs. Prob of Commitment\(^\text{Opp}_{p,t}\) indicates an investment in an opportunistic real estate fund. Yield Gap\(_t\) is the difference between the indicated yield measure in 2008 and in \(t\). Treasury, Corp, and TIPS indicate the 10-year Treasury real yield, Moody’s Aaa corporate bond real yield, and the 10-year TIPS yield. Standard errors clustered by pension are in parentheses. Data are from Preqin and the CRR.

4. Other Alternative Asset Classes: It is possible that value added real estate funds load differently on real estate fundamentals than other real estate funds. The pension-level results could therefore reflect growth in this value added beta. In this situation, one would expect to find no effect in alternative asset classes with a similar overall return profile as value added real estate. I investigate this possibility by reestimating (10) after replacing the outcome with investment in private distressed debt funds, excluding real estate debt. Distressed debt has historically commanded a similar total return as value added real estate.\(^{79}\) Like value added funds, the

\(^{79}\)According to Preqin, the average historic net IRR for private distressed debt funds is 12.4% compared to 12.8%
underlying project payoffs have a baseline income (i.e. value of the distressed firm) plus the potential for appreciation (i.e. post-restructuring value). The results in Table C11 show that underfunded pensions behaved similarly toward distressed debt funds as with value added real estate. This is consistent with the interpretation of underfunded pensions making investments with greater scope for appreciation to meet their obligations.

Table C11: Robustness of Public Pension Risk Taking to Distressed Debt

<table>
<thead>
<tr>
<th>Outcome: Funding Gap(_p\times) Yield Gap(_t)</th>
<th>Prob of Commitment(_{DD,p,t})</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
</tr>
<tr>
<td>Funding Gap(_p\times) Yield Gap(_t)</td>
<td>0.134*</td>
</tr>
<tr>
<td></td>
<td>(0.076)</td>
</tr>
<tr>
<td>Pension FE</td>
<td>Yes</td>
</tr>
<tr>
<td>State-Year FE</td>
<td>Yes</td>
</tr>
<tr>
<td>Pension Controls</td>
<td>No</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.705</td>
</tr>
<tr>
<td>Number of Observations</td>
<td>343</td>
</tr>
</tbody>
</table>

Note: Subscripts \(p\) and \(t\) denote pension fund and year. The specification is similar to column 1 of Table C8 except that the outcome differs. Prob of Commitment\(_{DD,p,t}\) indicates an investment in a private distressed debt fund, excluding real estate debt. Standard errors clustered by pension are in parentheses. Data are from Preqin and the CRR.

5. **Time Variation in Yield Gap and Placebo**: If the yield gap is monotonically increasing over the sample period, the pension-level results could reflect heterogeneous time trends by a pension’s funding status unrelated to risk taking behavior. However, Figure C8 shows that the yield gap is non-monotonic over the sample period. In addition, Table C12 performs a placebo test over the 2003-2007 period. Over this period there was an average increase in the TIPS yield, and so one should not expect to find a significant effect, consistent with the table.\(^8^0\) Moreover, this period is located at approximately the same stage in the pre-crisis real estate cycle as the baseline 2009-2016 period. This timing helps address concerns that the results are driven by real estate cyclicality.

\(^8^0\)The regression is the same as in (10) replacing the base year with 2002.
Figure C8: Variation in Yield Gap

Note: This figure plots the average difference between public pensions’ assumed rate of return in 2008 and the 10-year TIPS yield.

Table C12: Placebo Test of Public Pension Investment, 2003-2007

<table>
<thead>
<tr>
<th>Outcome: Funding Gap_{p,02} × Yield Gap_t</th>
<th>Prob of Commitment^{VA}_{p,t}</th>
</tr>
</thead>
<tbody>
<tr>
<td>Pension FE</td>
<td>Yes</td>
</tr>
<tr>
<td>State-Year FE</td>
<td>Yes</td>
</tr>
<tr>
<td>Pension Controls</td>
<td>No</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.682</td>
</tr>
<tr>
<td>Number of Observations</td>
<td>230</td>
</tr>
</tbody>
</table>

Note: Subscripts p and t denote pension fund and year. The specification is similar to column 1 of Table 5 except that the sample period is 2003-2007. The set of pensions in the sample is the same. Funding Gap_{p,02} is the percent difference between the fund’s actuarial liabilities and assets in 2002. Yield Gap_t is the difference between the 10-year TIPS yield in 2002 and in t. The change in this yield over 2003-2007 was +0.22 pps. Standard errors clustered by pension are in parentheses. Data are from Preqin and the CRR.

6. Changes in Accounting Rules: GASB accounting rules changed in 2012 such that public pensions had less scope for discounting liabilities at the same rate of return as their assets. This rule change should theoretically reduce underfunded pensions’ risk taking incentive, but Munnell et al. (2012) and Rauh (2017) discuss how it had little practical effect. I address the rule change by obtaining the Munnell et al. (2012) list of public pensions whose discount rate would be affected by it. Then, Table C13 reestimates (10) including a separate time trend for these
pensions. The results are somewhat weaker, but still significant.

Table C13: Public Pension Risk Taking and GASB Changes

<table>
<thead>
<tr>
<th>Outcome:</th>
<th>Prob of Commitment $^{VA}_{p,t}$</th>
</tr>
</thead>
<tbody>
<tr>
<td>Funding Gap$_p$ × Yield Gap$_t$</td>
<td>0.110*</td>
</tr>
<tr>
<td></td>
<td>(0.055)</td>
</tr>
<tr>
<td>GASB Change-Year FE</td>
<td>Yes</td>
</tr>
<tr>
<td>Pension FE</td>
<td>Yes</td>
</tr>
<tr>
<td>State-Year FE</td>
<td>Yes</td>
</tr>
<tr>
<td>Pension Controls</td>
<td>No</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.753</td>
</tr>
<tr>
<td>Number of Observations</td>
<td>501</td>
</tr>
</tbody>
</table>

Note: Subscripts $p$ and $t$ denote pension fund and year. The specification is similar to column 1 of Table 5. GASB Change-Year FE are interactions between year indicators and an indicator for whether $p$’s discount rate was affected by the GASB accounting rule change. Standard errors clustered by pension are in parentheses. Data are from Preqin and the CRR.

7. **Manager-Year Fixed Effects**: I estimate a manager-strategy-year level specification so that I can include manager-year fixed effects. The regression is

$$Y_{m,t} = \beta (\text{Funding Gap}_m \times \text{Yield Gap}_t \times \text{VA}_k) + \alpha_{m,t} + \alpha_{k,t} + \alpha_{m,k} + \gamma X_{m,t} + u_{m,t}, \quad (C12)$$

where $k$ indexes fund strategy, and the set of fund strategies are value added and not value added. This strategy is methodologically similar to the lender-year specification (4), and I obtain identification from the triple difference between treated managers (Funding Gap$_m$) in treated years (Yield Gap$_t$) and treated fund strategies (VA$_k$). The results in Table C14 again provide evidence that pension risk taking encouraged real estate fund managers to tilt their portfolio toward value added (i.e. improvement-oriented) funds. Unlike the baseline specification (11), one cannot infer whether managers increased formation of value added funds or simply stopped forming other funds. However, the inclusion of manager-year fixed effects does address the concern that the baseline results in Table 5 are driven by shocks to managers’ overall fundraising and investment activity.
Table C14: Value Added Investment with Manager-Year Fixed Effects

<table>
<thead>
<tr>
<th>Outcome:</th>
<th>Fund Formed(_{m,k,t})</th>
</tr>
</thead>
<tbody>
<tr>
<td>Funding Gap(_m) × Yield Gap(_t) × VA(_k)</td>
<td>0.176**</td>
</tr>
<tr>
<td>Manager-Year FE</td>
<td>Yes</td>
</tr>
<tr>
<td>Strategy-Year FE</td>
<td>Yes</td>
</tr>
<tr>
<td>Manager-Strategy FE</td>
<td>Yes</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.681</td>
</tr>
<tr>
<td>Number of Observations</td>
<td>1472</td>
</tr>
</tbody>
</table>

Note: Subscripts \(m\), \(k\), and \(t\) denote private real estate manager, strategy, and year. Fund Formed\(_{m,k,t}\) indicates the formation of a private real estate fund with strategy \(k\). The set of strategies are value added and not value added. Observations are manager-strategy-years weighted by the manager’s real estate capital raised over 2009-2016. The sample period is 2009-2016. Standard errors twoway clustered by manager and year are in parentheses. Data are from Preqin.

8. Manager-Pension Matching: The main identification assumption in (11) is that managers with a higher average funding gap across limited partners are not predisposed to shocks that would increase their formation of value added funds and subsequent investment in improvements. Figure C9 investigates this assumption by performing a similar exercise as Figure 6 in the credit supply research design. I divide managers into high and low exposure cohorts according to their exposure, Funding Gap\(_m\), and then perform a series of pairwise tests for a difference in mean in variables of interest, all normalized to have unit variance. There are few significant differences between managers with high and low exposure to underfunded public pensions. The exception is the average equity allocation across pension limited partners, which is likely a direct byproduct of the observation that underfunded pensions take more risk (e.g. Mohan and Zhang 2014). Turning to the last row, high-exposure managers do not appear to be located in states whose pensions have a significantly higher funding gap. This suggests that managers are not responding to local economic conditions near their headquarters.
Note: This figure plots the difference in mean for the indicated variable between managers with a high and low average funding gap across limited partners in 2008. High and low are defined according to the median across managers. Variables are normalized to have unit variance. Log Funds Raised is log of total real estate capital raised by the manager over 2009-2016. The variables LP Alternative Share through LP Realized Return are 2008 averages across the manager’s public pension limited partners of the following variables: allocation to alternative asset classes, log total assets, allocation to equities, and 7-year realized return. State funding gap is the average funding gap across public pensions in the state where the manager is located. Observations are managers weighted by real estate capital raised over 2009-2016. Brackets are a 95% confidence interval with heteroskedasticity robust standard errors. Data are from Preqin and the CRR.

9. Manager vs. Pension Size: A final concern is whether real estate fund managers are too large for relationship persistence documented in Appendix C.1 to plausibly matter. Figure C10 plots the size distribution of managers’ fundraising alongside that of pensions’ increase in real estate holdings over 2009-2016, from the CRR.\(^8\) While many managers are large, so are many pensions’ real estate investment. The median pension’s growth in real estate holdings of $182 million is still 35% of the median manager’s fundraising of $527 million.

\(^8\)This is a proxy for total real estate investment because I have limited information on committed capital to private equity real estate funds.
C.11 Magnitude of Pension Investment Effect

This extension describes the procedure for obtaining the back-of-envelope calculation referenced in Section 5. First, using the estimates of the manager-level specification from Table 5, I define the in-sample effect on total investment by improvement-oriented funds as

\[
\text{Effect}^\text{Sample} = \frac{\sum_m \sum_{t=2010}^{2016} \text{Investment}_{m,t}^{VA} \times \left[ 1 - e^{-\beta^P \times \text{Yield Gap}_t \times \max\{\text{Funding Gap}_m, 0\}} \right] \times \Delta t}{\sum_m \sum_{t=2010}^{2016} \text{Investment}_{m,t}^{VA} \times \Delta t},
\]

where \(\beta^P\) is the estimate from column 6 of Table 5. As in the text, \(\text{Investment}_{m,t}^{VA}\) is annualized investment by improvement-oriented ("value added") funds from their vintage year \(t\) through 2016. It has the interpretation of real investment created by funds formed in \(t\), and, because it is annualized, it is multiplied by \(\Delta t \equiv 2017 - t\). The implied in-sample effect is equal to 56% of investment by improvement-oriented funds over 2010-2016.