Financial Intermediaries as Suppliers of Housing Quality: Evidence from Banks and Public Pensions*

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Abstract

A surge in residential improvements has amplified post-Recession rent growth, and financial intermediaries have contributed to this effect by reallocating financing to improvement projects from other types of residential investment. I study two shifts in the supply of financing which explain 44% and 15% of improvement activity over 2015-16 and 2010-16, respectively. The first shift reallocates bank credit to improvements from construction projects, due to a change in regulatory capital requirements. The second shift reallocates private equity to improvements from buy-and-hold projects, due to changes in underfunded public pensions' risk-taking incentives. Improvements collectively account for 65% of post-Recession rent growth.

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1 Introduction

Over one-third of U.S. households rent their home, and many of these households have experienced a significant increase in housing costs since the Great Recession.[1] These observations, coupled with record-high levels of residential improvement activity shown in Figure 1, have ignited policy discussion about housing affordability (Donovan 2014). In particular, improvements constrict the supply of cheap homes by transferring them to the expensive end of the market. Like any other investment project, improvements must be financed. However, both academic and popular discussion of urban change frequently overlook the role of finance in this shift toward better housing quality. Could greater supply of financing for improvement projects be contributing to better housing quality and thus higher rent growth?

I find that two supply shifts have contributed to the recent increase in improvement activity by channeling financing to improvements and away from other types of residential investment. First, a 2015 change in regulatory capital requirements incentivizes banks to reallocate credit from construction projects to improvements. Second, declining safe yields since 2008 coupled with government accounting rules incentivize public pensions to reallocate financing from safe private equity funds, which perform buy-and-hold projects, to riskier funds, which perform improvements. In both cases, a reallocation of financing across project types increases real improvement activity, and this occurs during a period when improvements account for a majority of real rent growth. Outside a housing context, these results illustrate the more general point that portfolio reallocation by financial intermediaries can induce a reallocation across different types of real activity.

My analysis is partitioned according to the debt and equity financing of improvement projects. On the debt side, I study a credit supply shift 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 effective cost of funds for different loan types, incentivizing banks to transfer credit

[1] According to the Housing Vacancy Survey, 37% of households were renters in 2016. The median rent-to-income ratio reached a historically high level of 30% in 2015. See Section 2.

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 assess the 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 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 2015-16, in partial equilibrium.

Next, I ask whether greater supply of equity financing can also increase improvement activity. This exercise complements the credit supply analysis both practically and theoretically. In practical terms, it informs whether the credit supply results pertain to a one-time occurrence, or whether they support the more general conclusion that the supply of financing routinely affects urban change. In theoretical terms, greater supply of equity financing can increase improvement activity if real estate investors are credit-constrained, as suggested by the credit supply analysis, provided the market for equity financing is itself imperfect.

I study a shift in the supply of financing for private equity real estate funds. 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 40%. Public pensions, for their part, are known to exhibit risk-shifting behavior: they take greater risk the more underfunded they are, and this behavior is especially pronounced when safe yields are low (e.g. Andonov, Bauer and Cremers 2017; Mohan and Zhang 2014). Governmental Accounting Standards Board (GASB) rules provide an incentive for such risk-shifting, since they allow public pensions to use their assumed rate of return to set required contributions and discount actuarial liabilities, in contrast to using a risk-free return (e.g. Novy-Marx and Rauh 2011; Rauh 2017).

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. To trace reallocation at the pension-level down to real investment, I use the fact that fundraising relationships are sticky. I find that real estate fund managers who were historically more reliant on more-underfunded pensions increase their real investment in improvements over 2009-16 relative to other managers. A back-of-envelope calculation suggests that private equity investment in improvements would have been 47% less had all public pensions had been fully funded in 2008, which approximately maps to 15% of aggregate investment. Taken alongside the credit supply results, this finding shows how financial intermediary portfolio reallocation has had significant effects on housing quality through the distribution of resources across different types of residential investment.

These results have implications for both the level and cross-section of rent growth. First, I perform a quality-adjustment exercise to compute the wedge between observed rent and a measure of quality-adjusted rent. I apply a traditional hedonic 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-Recession real rent growth. By extension, greater supply of financing for improvements has contributed to higher rent growth through the channel of better average housing quality. Turning to the cross-section, rent has grown less quickly in high-quality segments compared to low-quality ones, consistent with an increase in the supply of quality. In addition, improvements appear to be targeted toward higher-income households. Together, these cross-sectional patterns provide suggestive evidence that shifts in the supply of financing can have distributional effects, here disproportionately lowering effective housing costs for higher-income households.

This paper makes two principal contributions to the literature. First, I show how financial intermediaries contribute to urban change by providing financing to real estate investors. In particular, the results suggest that finance should play a role in equilibrium models of gentrification (e.g. Guerrieri, Hartley and Hurst 2013; Couture et al. 2018). Moreover, my focus on the supply of housing quality complements 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). Finally, a number of recent papers have studied how urban policies, such as tax credits or rent control, affect rental markets (e.g. Diamond et al 2018; Diamond et al 2018b), and this paper shows how the rental market is also affected by financial regulation.[2]

Second, viewing construction, improvements, and buy-and-hold projects as separate technologies that firms (i.e. real estate investors) use to produce housing services, I provide direct evidence that financial 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 2014b; Greenstone, Mas and Nguyen 2015; Gan 2007). Methodologically, this paper is among a set of recent papers using capital requirements to obtain identification (e.g. Blattner, Farinha and Rebelo 2018; Koijen and Yogo 2015), and it is among the first to study firm-level effects of regulations associated with Dodd-Frank. In particular, I find that capital requirements can shift bank versus nonbank market share 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 2019).[3]

The remainder of the paper is organized as follows. Section 2 presents background facts and an organizing framework; Sections 3 studies the effect of credit supply on real improvement activity; Section 4 studies the analogous effect of private equity supply; Section 5 assesses implications for rent growth; and Section 6 concludes.

[2]On a more general level, a large literature has studied the effect of financial markets on housing markets in the owner-occupied sector, and this paper is among a few to study that effect in the rental market (e.g. Gete and Reher 2018).

[3]Conceptually, 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).

2 Facts and Framework

This section aims to clarify the paper's argument 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 small model to which relates them to my main empirical analysis.

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 use "financial intermediary" as a general term for the financiers of residential investment projects, which includes banks, nonbank commercial real estate lenders, and public pensions.

2.1 Data

I rely on three main datasets and numerous auxiliary ones which are discussed in turn. The full details, including summary statistics, are in Appendix A. 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 dataset comes from Trepp LLC and covers units in multifamily properties over 2010-16. The underlying data come from multifamily mortgage servicing records for loans which were eventually securitized, and have detailed information about property improvements. The second dataset comes from Preqin, and it covers fundraising and investment activity by private equity real estate funds. The third 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 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.

2.2 Facts

Figure 1 documents two important trends in housing quality: (a) a surge in renovation activity since the Great Recession; and (b) a negative cross-sectional correlation between housing quality and rent growth. The first trend is illustrated in panel (a), which plots the percent of multifamily housing units that are renovated each year. This annual probability of renovation vigorously recovered from its 2008 low and surpassed its pre-Recession high by 2014. Appendix Figure E2 replicates this finding using aggregate investment in residential improvements.[4]

Panel (b) plots 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.[5]

Together, these observations are consistent with an increase in the supply of improvement projects, notwithstanding a likely increase in demand associated with declining homeownership rates. Specifically, improvements transform low-quality units into high-quality ones, thereby increasing the relative supply of high-quality units and lowering their relative rent, as suggested by panel (b) of the previous figure. In this paper, I study shifts in the supply of improvement projects that stem from shifts in the supply of financing for these projects.

2.3 Framework

My core analysis revolves around two natural experiments that increase the supply of financing for improvement projects. In both instances, financial intermediaries channel financing

[4]Relatedly, Appendix Figure E3 documents a reduction in the rate at which rental units drift down the quality ladder, which I measure by income filtering (Rosenthal 2014). See Appendix D.2 for cross-sectional characteristics of improvement activity. Appendix Figure E1 documents broader trends in rent growth referenced in the introduction.

[5]Appendix Figure E5 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 E4 shows how the cross-sectional distribution of log rent became more compressed over this period.

toward improvements and away from other types of residential investment. I begin with a small model which places these two natural experiments within a common framework and disciplines their associated empirical analyses.

Consider a one-period economy with a numeraire and a set of projects that transform this numeraire into housing services. There are two project types, which are called "improvements" and the "reservation project". Real estate investors specialize in performing one of these projects, but they require outside financing to do so. I assume the market for real estate financing is segmented according to predetermined relationships, consistent with anecdotal and empirical evidence presented shortly. Consequently, each investor is endowed with 1 financial intermediary to whom she can turn for financing. Investors collectively produce housing services according to the production function $F\left(w^I, 1-w^I\right)$, where w^I is the share of aggregate resources allocated to improvements, and the remaining $1-w^I$ is allocated to the reservation project.

Financial intermediaries are endowed with 1 unit of numeraire to allocate across projects. They incur an additional cost of providing financing, $C\left(w^I\right)$, which may depend on their allocation to improvements. I assume that intermediaries and investors split the surplus from investment, and therefore intermediaries solve

$$\max_{w^I}\left\{F\left(w^I, 1-w^I\right)-C\left(w^I\right)\right\}. \tag{1}$$

If intermediaries have a uniform cost of providing financing (i.e. $C'=0$), then the solution to (1) implies that they equalize the marginal product of financing across projects.

Suppose, however, that some friction makes it relatively less costly to provide financing for improvements (i.e. $C'<0$). It is straightforward to show that this friction increases the allocation to improvements, w^I. In this paper's first natural experiment, such a friction arises because of a change in bank regulatory capital requirements. In the second natural experiment, the friction arises because of declining safe yields coupled with accounting rules that govern underfunded public pensions. Both situations entail a reallocation of financing to improvements from a reservation project. My principal research question is whether this convergence of financing has had a first-order effect on the increase in real improvement activity documented in Figure 1, among other plausible first-order shifts (e.g. demand).

3 Credit Supply and Improvements

I begin with the debt financing of improvements, and I study a shift in the supply of credit for improvement projects that was generated by a change in regulatory bank capital requirements.

3.1 Setting

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.[6] HVCRE loans are for the "development or construction of real property", which I will simply refer to as "construction".[7] By contrast, loans for "improvements to existing income-producing real property" were not subject to this increase and retained the substantially more modest weight of 100%.[8]

Within the framework from Section 2.3, the introduction of HVCRE regulation should increase the supply of credit for improvements. Specifically, I interpret the cost function C as the bank's cost of funds,

$$C\left(w^I\right) = \left(1 + R^b\right)\left[w^I + \kappa\left(1 - w^I\right)\right], \tag{2}$$

where the reservation project is construction; R^b is the effective cost of funds to finance

[6] 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.

[7] 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 satisfy any of the following conditions: the loan-to-value (LTV) ratio is greater than 80 percent; the terms allow capital withdrawals; or the borrower's contributed capital is less than 15 percent of the project's "as completed" value. These conditions are met by most construction projects (Chandan and Zausner 2015).

[8] 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.

improvement loans; and $\kappa \geq 1$ represents a markup over this rate due to the potentially-higher equity capital required to be held against construction loans. Provided the Modigliani-Miller theorem fails so that equity capital is costly for banks, then a binding HVCRE capital requirement implies $\kappa > 1$ and thus $C' < 0$. The resulting cost wedge leads banks to transfer loanable funds to improvements from construction. In principle, banks could also respond by securitizing loans more quickly, lowering the warehouse period during which the standard risk weight binds.[9] However, the evidence provided shortly suggests that banks do not respond entirely along this alternative margin. Anecdotally, many banks indeed shifted resources to loans unaffected by HVCRE regulation and curtailed their lending for construction projects (Mortgage Bankers Association 2018), consistent with the aggregate behavior shown in Appendix Figure E6.

Viewing HVCRE regulation as a positive shift in the supply of credit for improvement projects, there are three key details which allow me to estimate the real effects of this shift. First, specialty nonbank lenders play an important role in multifamily mortgage markets, and they are not subject to capital requirements.[10] 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 confusion over the precise details of implementation (Mortgage Bankers Association 2018) as well as the grandfathering of pre-2015 loans.[11]

My goal is to assess the aggregate, partial equilibrium effect of HVCRE regulation on real improvement activity. As a necessary first step, I estimate the lender-level effect in

[9]Securitization dilutes capital requirements through the risk retention ratio, but it does not eliminate them. 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.

[10]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 2012-16 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.

[11]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 8, a formal clarification of HVCRE regulation including a full description of grandfathering status did not come until 2017.

Section 3.2, followed by a property-level specification in Section 3.3. Then I turn to the main county-level specification in Section 3.4 and discuss the aggregation procedure in Section 3.5. Unless otherwise stated, data used in this section come from Trepp.

3.2 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 equation over 2011-16,

$$Y_{k,\ell,t} = \beta\left(\text{Bank}_\ell \times \text{Post}_t \times \text{Imp}_k\right) + \gamma\left(\text{Bank}_\ell \times \text{Imp}_k\right) + \alpha_{\ell,t} + \alpha_{k,t} + u_{k,\ell,t}, \tag{3}$$

where k, ℓ, and t index loan purpose, lender, and year.[12] Bank_ℓ indicates if the lender is a bank, 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. Imp_k indicates if the purpose is an improvement, where the set of loan purposes are improvement or construction.[13] The parameter of interest in (3) is β, which captures the triple difference between treated loan types (Imp_k) originated by treated lenders (Bank_ℓ) during the treatment period (Post_t), and the counterfactual purpose-lender-years. For the rest of the paper, I economize on notation by repeatedly using β 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 purpose k.[14]

The main drawback to (3) 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

[12] To avoid overweighting idiosyncratic shocks to small lenders, observations in (3) and the following specification (4) are weighted by multifamily mortgage market share over 2011-16.

[13] 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. See Appendix A for details on how I classify improvement and construction loans.

[14] 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.

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 equation

$$Y_{\ell,t} = \beta\left(\mathrm{Bank}_{\ell} \times \mathrm{Post}_{t}\right) + \alpha_{t} + \alpha_{\ell} + \gamma X_{\ell,t} + u_{\ell,t}, \tag{4}$$

where the notation is the same as in (3), 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 $\alpha_{\ell,t}$ in (3). Omitted variables related to a lender's business model may still lead to bias in (4). However, Appendix Figure E7 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 (3) 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 log points relative to nonbanks after HVCRE regulation is introduced.[15] The magnitude is larger when studying dollar volume in column 2, which may reflect economies of scale that incentivize improvements on larger properties, as well as a scaling back of construction lending along the intensive margin. Figure 2 illustrates the effect over time by replacing Post_t with a series of year interactions. After the regulation is introduced, banks significantly tilt their portfolio allocation toward improvement loans relative to nonbanks, while there is no significant ex-ante adjustment. This finding suggests that the results are not due to the other two major regulations associated with Basel III, the Liquidity Coverage Ratio (LCR) and the Net Stable Funding Ratio (NSFR). In particular, unlike capital risk weights, the liquidity risk weights associated with the LCR do not vary by project type, and Gete and Reher (2019) show that most of the adjustment to the LCR occurred in 2014. Moreover, the U.S. version of the NSFR was not proposed until May 2016.

Columns 3-5 of Table 1 report the results of (4). My outcome of interest is the log num-

[15] Using a log approximation, the point estimate suggests that a 40% (i.e. $\log(1.5) - \log(1.0)$) reduction in the relative cost of equity capital for a particular loan type leads to a 28% increase in relative originations for that loan type. In other words, the cross-elasticity of substitution is around 0.7 (i.e. $\frac{0.28}{0.40}$). Originations are normalized to have unit variance within lender-purposes to account for different business models.

ber of renovated units financed by new loans. Studying this outcome facilitates continuity with the county-level analysis in Section 3.4, 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 3 suggests that banks finance significantly more improvements relative to nonbanks in the post-HVCRE period, and the results are similar after including lender controls in column 4. Consistent with a movement along the credit demand curve, Appendix Table E2 shows that the price of credit for bank-originated improvement loans also falls after the introduction of HVCRE regulation. The price response is quantitatively small, which may reflect a substitution toward higher-risk improvement projects.

In column 5, I test the theory that banks' response to the regulation depends on their securitization technology. I interact the treatment variable, $\text{Bank}_\ell \times \text{Post}_t$, with a measure of the lender's securitization sluggishness in the pre-2011 period, Sec Lag_ℓ, normalized to have zero mean and unit variance. Banks with a higher value of Sec Lag_ℓ have a longer typical warehouse period, and, consistent with the theory described above, their estimated response to the regulation is much stronger.

Appendix B.2 describes an analogous exercise in the context of the syndicated loan market, which is meant 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.

3.3 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 D.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 4.

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 3.4. I estimate the following difference-in-difference equation,

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

where i, ℓ, and t index properties, lenders, and years, and Bank_ℓ 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.[16] 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 (5) 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 β in (5) requires a "parallel trends" assumption: bank and nonbank-financed properties do not differ in ways that would affect improvement activity after the

[16]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.

introduction of HVCRE regulation. Explicitly, my identification assumption is

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

where the conditioning arguments $\alpha_{c(i),t}$ and $\alpha_{i,\ell}$ make clear that identification comes from within the 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 E7 and its associated discussion in Section 3.2, 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 (5). The outcome $Y_{i,\ell,t}$ is an indicator for whether a renovation occurs in t, denoted $\text{Renovation}_{i,\ell,t}$. 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 2010-16. Since all variation comes from within the same property-lender bin, the effect is identified by new loans on bank-financed properties. 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.

Appendix B.3 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. These idiosyncratic demand shifts arise because of institutional features of the multifamily mortgage market which incentivize postponing improvements until the time of loan renewal, and this timing appears to be effectively exogenous. 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 shift. The results of this exercise are qualitatively similar to those in Table 2.

3.4 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 exercise, which will facilitate the subsequent aggregation procedure, is to estimate a county-level difference-in-difference equation,

$$Y_{c,t} = \beta \left(\text{Bank Share}_c \times \text{Post}_t \right) + \alpha_c + \alpha_t + \gamma X_{c,t} + u_{c,t}, \tag{6}$$

where c and t index counties and years, and 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.[17] 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 the treatment (Goldsmith-Pinkham, Sorkin and Swift 2018). In particular, the assumption is

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

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 5, may have a higher price of quality and thus would be disproportionately affected by the trend. Measuring Bank Share$_c$ 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 3 plots the geographic distribution of banks' initial market share across states. The distribution is fairly uniform, and this uniformity is also

[17] 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 overweighting idiosyncratic shocks to small counties, I weight observations in (4) by average number of multifamily units over 2011-16.

borne out when zooming into the county-level within high growth states, shown in Appendix Figure E8.

The importance of borrower-lender relationships suggests that historical episodes may partly determine banks' market share. To that end, Figure 4 investigates the source of treatment assignment. I divide counties into high and low exposure cohorts according to their initial exposure, Bank Share$_c$. 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 3, 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 real estate speculation. Counties with a high value of Bank Share$_c$ experienced less severe crises in the 1980s, supporting the idea that Bank Share$_c$ is determined by historical episodes coupled with relationship stickiness.

Table 3 has the results of the baseline specification (6). The outcome in columns 1 through 3 is log number of renovated properties. The estimate in column 1 implies that counties with a 10 pps higher initial bank share see around 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.[18] In Figure 5, I study the effect over time by replacing Post$_t$ with a series of year interactions. 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.

In column 4, I study 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

[18]Applying an Oster (2017) correction for omitted variable bias leads to a slightly higher point estimate of 0.298, based on a maximum R-squared of 0.75 and the default selection parameter $\delta = 1$. Some 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$_c$ $\times$ Post$_t$, 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.

economies of scale discussed in the context of Table 1. Finally, in column 5 I study 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 documented by Reher (2019). I conduct additional robustness exercises in Appendix B.1.

Next, I study how the treatment effect varies in the cross-section of counties. Based on a simple model of mortgage markets with asymmetric information and imperfectly competitive lenders, one might expect the treatment effect to be stronger where: households are more willing to pay for quality; more borrowers face binding credit constraints; banks have less market power and thus pass on more of a reduction in their cost of funds; and rent control does not limit the incentive for making improvements. Table 4 investigates these predictions by reestimating column 2 of Table 3, interacting the treatment variable $\text{Bank Share}_c \times \text{Post}_t$ with relevant county characteristics.[19]

I focus on the economic intuition associated with $\text{Bank Share}_c \times \text{Post}_t \times \text{Interaction}_c$ in Table 4. For example, column 1 shows how the policy's effect is stronger in high income counties, which may reflect a greater willingness-to-pay for quality in such counties as discussed in Section 5. 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 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.

Finally, I consider how HVCRE regulation affects other county-level outcomes, such as new construction and rent growth. In the interest of space, I report the results in Ap-

[19] The characteristics are: average real income per capita over 2011-16; 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.

pendix Table E1. Briefly, treated counties experience reduced multifamily construction after the introduction of HVCRE regulation, which suggests portfolio reallocation at the lender level affects the distribution of real project types. These counties also experience growth in homelessness, which may reflect a constriction of low-quality units because of increased improvement activity. Rent growth increases by a quantitatively significant amount in treated counties after HVCRE regulation is introduced, but it is not clear how much of this stems from better quality housing − my channel of interest − versus reduced construction. Lastly, rents grow more quickly on low-quality units than on high-quality ones in treated counties during the post-HVCRE period, consistent with an increase in the supply of quality as documented in Figure 1b.

3.5 Aggregate Effect

I conclude this research design by using the county-level estimates to calculate an aggregate, partial equilibrium effect of credit supply on real improvement activity, reweighted according to sample representability. My counterfactual is a world without HVCRE regulation in which capital requirements treat loans for all residential investment projects equally. I ask 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). This migration would disincentivize improvement activity in low exposure counties, since the willingness-to-pay for 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 B.4 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.[20]

Under these two assumptions, one can compute the aggregate effect of HVCRE regulation by reweighting the in-sample effect. Define the in-sample effect as the sum of county-level effects,

$$\text{Effect}^{\text{Sample}} = \frac{\sum_c \sum_{t\geq 2015} \text{Renovated Housing Units}_{c,t} \times \left[1 - e^{-\beta^{\text{HVCRE}}\text{Bank Share}_c}\right]}{\sum_c \sum_{t\geq 2015} \text{Renovated Housing Units}_{c,t}}, \tag{7}$$

where c and t index counties and years in the sample, Renovated Housing Units$_{c,t}$ is the number of renovated units in the sample, and β^{HVCRE} is the estimate from column 4 of Table 3.[21]

[20]Specifically, 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 B.4 also uses the within lender-year specification (3) to show that banks reduce the rate at which they securitized improvement loans following regulation, suggesting the in-sample estimates are biased toward zero.

[21]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 5. They can also be mapped to

The implied in-sample effect equals 63% of renovated units over 2015-16. However, the regulation only affects mortgaged properties, which account for 70% of multifamily renovations over 2010-15 according to the Rental Housing Finance Survey (RHFS). One then reweights to obtain an aggregate, partial equilibrium effect equal to 44% (i.e. 0.63×0.7) of multifamily renovations over 2015-16. This magnitude is large, but it is consistent with a sharp aggregate reallocation from construction to improvement projects, shown in Appendix Figure E6, and the large increase in renovation probability shown in Figure 1a.

4 Private Equity Supply and Improvements

This section turns to the equity financing of improvement projects, and I study a shift in the supply of financing for private equity real estate funds. Pursuing this additional research design complements the credit supply analysis in two important ways. Principally, it informs whether the credit supply results pertain to a singular incident, or whether they exemplify a more regular phenomenon wherein the supply of financing affects the supply of housing quality. In addition, it enables me to test the basic theory that access to outside equity can increase real improvement activity when, as implied by the former analysis, credit market frictions place limits on debt financing.

4.1 Setting

The logic of this research design is to apply a more general result on public pension risk-shifting to the private equity real estate market. Accordingly, there are two sets of institutional details: one concerning public pensions, and another concerning private equity real estate funds.

First, 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) rules (e.g. Aubry and Crawford 2019; Andonov, Bauer and Cremers 2017; Mohan and Zhang 2014; Novy-Marx and Rauh 2011). GASB rules allow public pensions to use their assumed investment return to set required contributions and to discount future obligations. Consequently,

aggregate data.

a pension with a high assumed return requires fewer contributions from its members. Moreover, its actuarial funding position appears stronger, since future obligations are discounted at a higher rate. Together, these features incentivize underfunded pensions to set aggressive return assumptions, and consequently to take greater risk to meet those returns, a behavior which I will call "risk-shifting". This behavior is not unlike the risk-shifting exhibited by private pensions (e.g. Bergstresser, Desai and Rauh 2006) and insurance companies (e.g. Becker and Ivashina 2015), which also face institutional incentives to set and subsequently meet aggressive return targets.

Public pensions' risk-shifting incentives are stronger when the yield on safe assets is low. To make this point clear, I apply the simple framework from Section 2.3 to this setting. I interpret the production function F from equation (1) as describing a pension's actuarial assets. The cost C represents the pension's actuarial liabilities, which can here be written

$$C\left(w^I\right) = \frac{\text{Obligations}}{w^I\left(\bar{R}^I - \bar{R}^f\right) + \bar{R}^f}, \tag{8}$$

where the numerator in (8) represents obligations to future pensioners; the denominator in (8) represents expected investment return; $\bar{R}^I$ is the expected return to improvements; and $\bar{R}^f$ is the expected return to the reservation project. As discussed shortly, safe buy-and-hold projects are the reservation project in this setting, so that $\bar{R}^I > \bar{R}^f$ and thus $C' < 0$. Equation (8) implies that the cost reduction from investing in improvement projects is greater when: (a) the return to safe projects is low; and (b) the pension has more outstanding obligations, and thus a higher funding gap. The interaction between these two effects generates a shift in the supply of improvement financing. Other papers have used the product between pension funding status and low safe yields as a supply shifter for relatively-risky investment (e.g. Andonov, Bauer and Cremers 2017, Chodorow-Reich 2014a), and so one should view this research design as applying already-established insights to real estate.

The second set of details concerns private equity real estate funds. These funds constitute half of aggregate investment in rental markets, shown in Appendix Figure E13, and they typically take an equity stake in residential investment projects.[22] Conveniently, funds are

[22]Private equity real estate 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

strictly classified by the type of project they perform, called the fund's "strategy". 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.[23] Panel (a) of Figure 6 plots the historical return and total volatility for these various strategies and other conventional assets. Notice that value added funds − again, whose economic function is to perform improvements − have a level of total risk and return similar to that of a high-yield bond. By contrast, core funds are intended to be safer and more closely resemble a AAA bond. For this reason, public pensions have traditionally preferred to invest in core funds (Pagliari 2010).

Applying public pensions' risk-shifting behavior 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. Panel (b) of Figure 6 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 2009-16, during which safe yields fell on average.[24] Conversely, they decrease their allocation to buy-and-hold funds, as documented in Appendix Figure E14.

Such a reallocation could potentially have meaningful real effects for two reasons. First, public pensions are dominant financiers of private equity real estate funds, comprising roughly 40% of limited partners as shown in Appendix Figure E15. Second, there is considerable fundraising stickiness between private equity real estate fund managers and their limited partners (e.g. public pensions). For example, Appendix D.1 shows how the probability a 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.

[23]There 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.

[24]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.

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 6, 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 real improvement projects. This hypothesis is the focus of my analysis.

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.[25]

4.2 Pension-Level Reallocation

My first question is whether more-underfunded public 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 observation that more-underfunded pensions take greater risk when safe yields are low. My approach is similar to Andonov, Bauer and Cremers (2017) and Chodorow-Reich (2014a), and I estimate

$$Y_{p,t} = \beta \left(\text{Funding Gap}_p \times \text{Yield Gap}_t\right) + \alpha_t + \alpha_p + \gamma X_{p,t} + u_{p,t}, \tag{9}$$

[25] 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. Appendix C.2 shows how pensions' substitution into these very risky funds was positive, but of weaker magnitude relative to the more moderate improvement-oriented ("value added") funds. Studying value added funds is conceptually cleaner, since opportunistic funds occasionally perform extreme rehabilitations, per footnote 23.

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.[26] 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), other time-varying characteristics. I estimate (9) over 2009-16 using the Preqin dataset merged with public pension data from Boston College's Center for Retirement Research (CRR).

Columns 1-2 of Table 5 contain the results of the pension-level specification (9). My outcome of interest is an indicator for whether the pension commits capital to an improvement-oriented ("value added") real estate fund in t, denoted Prob of $\text{Commitment}_{p,t}^{\text{VA}}$. The treatment variable Funding $\text{Gap}_p \times$ Yield Gap_t has been normalized to have unit variance. Correspondingly, the point estimate in column 1 implies that a 1 standard deviation increase in the treatment corresponds to a 17 pps, or 0.4 standard deviation, higher annual probability of investing in an improvement-oriented fund. By contrast, the estimates are negative when the outcome is investment in safer buy-and-hold (i.e. "core") funds, as discussed in Appendix C.2. Thus, relatively-underfunded pensions appear to respond to declining safe yields by reallocating their real estate portfolio toward funds that perform riskier projects.

To be clear, (9) does not seek to test the causal effect of low safe yields on public pension real estate investment. Rather, (9) serves as a "first-stage" for my principal research hypothesis, which is whether pension investment behavior affects real improvement activity. For example, the parameter β does not distinguish between the effect of low safe yields and other dynamics that covary with Yield Gap_t and disproportionately affect relatively-underfunded pensions, such as trend growth in obligations to pensioners. However, all that is necessary to identify the effect of pension investment behavior on real improvement activity is that such dynamics do not also covary with the fundamentals of improvement projects, as stated more formally below.

To better understand the source of time-series variation which identifies β, I instru-

[26] I measure the safe yield using the yield on a 10-year TIPS bond. Note that (9) 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 α_p. I weight observations by the pension's average assets over 2009-16 to avoid overweighting idiosyncratic shocks to small pensions.

ment for Yield Gap_t using the change in safe yields attributable to unconventional monetary policy surprises, per Chodorow-Reich (2014a). The results in Appendix Table E13 suggest that short-term fluctuations in the safe yield, including these monetary policy surprises, are what appear to influence pension investment behavior. Additional robustness exercises are described in Appendix C.

4.3 Real Investment in Improvements

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 real improvement projects. Mirroring (9), I next estimate

$$Y_{m,t} = \beta\left(\text{Funding Gap}_m \times \text{Yield Gap}_t\right) + \alpha_t + \alpha_m + u_{m,t}, \tag{10}$$

where m and t index private equity real estate fund manager and year, and Funding Gap_m is the average of its analogue from (9) across m's limited partners. To interpret, treated units in (10) 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).[27]

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 (10) 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

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

Appendix Figure E12 and its associated discussion support this assumption, providing evidence that managers with high and low exposures to underfunded pensions are similar on observable characteristics. In particular, high-exposure managers do not appear to be lo-

[27] 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 (10) by the manager's total real estate capital raised over 2009-16.

cated in states whose pensions have a significantly higher funding gap, which suggests that managers are not responding to local economic conditions near their headquarters.

Columns 3-4 report the estimates of the manager-level specification (10). The outcome in column 3 is the annual probability of forming an improvement-oriented fund, which I denote Fund Formed$^{\mathrm{VA}}_{m,t}$. Interpreting the point estimate, managers with a 1 standard deviation higher pension investment shift, Funding Gap$_m$ $\times$ Yield Gap$_t$, have a 7.0 pps, or 0.3 standard deviation, higher probability of forming such a fund. To assess whether this reflects an overall shift in the supply of private equity versus a specific one for improvement-oriented funds, I also estimate a triple difference-in-difference specification with manager-year affects, which has a similar form as (3). Consistent with reallocation at the pension level, the results discussed in Appendix C.8 suggest that managers substitute away from safer buy-and-hold-oriented funds toward improvement-oriented funds.

The outcome in Column 4 is log annualized investment by improvement-oriented funds formed by m between the fund's vintage year, t, and 2016. This variable is an approximation to total improvement activity created by the fund which m formed in t. Interpreting the estimated coefficient, managers with a 1 standard deviation higher pension investment shift in t invest 122 log points more per year in real 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. Like in the credit supply research design, relationship stickiness is the bridge that enables reallocation by financial intermediaries to have real effects. See Appendix D.1 for additional discussion.

4.4 Aggregate Effect

I conclude this section by relating the estimates to overall improvement activity. Consider a counterfactual in which all public pensions were fully funded in 2008, equal to the 92$^{\mathrm{nd}}$ percentile of funding status that year. Using the point estimate from column 4 of Table 5, I calculate how much less investment by improvement-oriented funds there would have been over 2010-16 under this counterfactual. The procedure is similar to that undertaken in Section 3.5 and described in detail in Appendix C.10. This calculation implies that there would have been 47% less investment by improvement-oriented funds over 2010-16. It is difficult to map this effect to aggregate improvement activity, but I obtain an approximate order of magnitude by noting that improvement-oriented funds account for roughly 31% of aggregate investment in existing rental housing units over 2010-16. By extension, portfolio reallocation by underfunded public pensions would account for around 15% (0.31×0.47) of aggregate investment in existing rental units over that period.

The results of this exercise suggest that greater supply of private equity financing has had a first-order effect on real improvement activity. Relative to the credit supply analysis, however, there is greater uncertainty over the precise magnitude of the effect. In particular, it is difficult to reweight the in-sample effect to match an appropriate aggregate statistic. This constraint reflects a more general challenge of data availability faced by the literature on private equity and alternative asset classes (Kaplan and Lerner 2016).

5 Implications for Rental Housing Costs

An increase in the supply of real improvement projects, such as that generated by the previously-studied financial supply shifts, has implications for the level of rent growth. Theoretically, such an increase shifts the distribution of housing quality to the right, making low-quality units relatively-scarce and high-quality ones relatively-abundant. Consequently, the rent on the average housing unit rises because there are more units in the expensive end of the market. However, on a quality-adjusted basis, average rent may actually fall following an increase in the supply of improvement projects. Appendix Figure E16 illustrates this argument diagrammatically.

In this section, I perform a hedonic quality-adjustment exercise which maps improvements into rent growth. My goal is to compute the share of observed rent growth that is attributable to quality improvements. Importantly, I include all improvements in this exercise, and I do not attempt to restrict improvements to those created by the previously-studied financial supply shifts. I take this route because tracing the effect of financial shifts on rent growth *through improvement activity* requires additional equilibrium structure, which lies outside the scope of this paper. For example, the credit supply shift from Section 3 may affect rent growth through equilibrium channels that are distinct from improved housing quality, such as in-migration of higher-income households or reduced construction. Thus, in the interest of transparency, I do not take a stand on which improvements are demand or supply-driven, and so one should interpret this exercise as estimating the equilibrium price of quality.

5.1 Hedonic Index

Following a tradition in the housing literature summarized by Sheppard (1999), I 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 quality-adjusted 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, which are not observed in the Trepp and Preqin datasets. The data are also representative of the entire U.S. housing stock, and they allow me to study single family rentals, whereas I have only been able to study the multifamily sector to this point. As mentioned in Section 2.1, my data end in 2013 because of a sample redesign, and so, given my emphasis on the post-Recession period, I construct a hedonic index over 2007-13.

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

$$\Delta \log \left(\text{Rent}_{i,t}\right) = \beta^{\Theta} \Delta \Theta_{i,t} + \alpha_i + \alpha_t + u_{i,t}, \tag{11}$$

where i and t index housing units and years, $\Delta \log \left(\text{Rent}_{i,t}\right)$ is the change in log rent, and $\Delta \Theta_{i,t}$ is a vector of indicators for the installment of features $\theta_{i,t} \in \Theta_{i,t}$.[28] Thus, (11) 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 α_i and α_t account

[28] The features in $\Theta_{i,t}$ 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, $\Delta \theta_{i,t}$ is the increase in log square feet and not an indicator.

for the possibility that improvements only occur in some locations or in certain years.

Given the estimates from (11), shown in Appendix Table E18, I compute a unit's quality-adjusted rent as

$$\text{Rent}^H_{i,t} = \text{Rent}_{i,t_0} \times e^{\sum_{\tau=t_0+1}^{t}\left[\Delta \log(\text{Rent}_{i,\tau}) - \beta^\Theta \Delta\Theta_{i,\tau}\right]} \tag{12}$$

where Rent_{i,t_0} is the property's rent in the base period t_0. Then, I define the hedonic index π^H_t as the normalized average of $\text{Rent}^H_{i,t}$ across rental units $i \in \mathcal{I}$,

$$\pi^H_t = \frac{\sum_{i\in\mathcal{I}} \text{Rent}^H_{i,t}}{\sum_{i\in\mathcal{I}} \text{Rent}_{i,t_0}}. \tag{13}$$

As described in Appendix A, I drop units that experienced a change in tenure (e.g. "condo conversions") from my analysis. The aggregation in (13) 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 7a summarizes 2007-13 annual growth in π^H_t 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 an age adjustment similar to that used by the BLS and described in Reher (2019). 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.[29] Quantitatively, the result suggests that quality improvements can account for 65% (i.e. $\frac{1.7-0.6}{1.7}$) of real rent growth, relative to a counterfactual of no such improvements. The remaining 0.6 pps (35%) reflect, for example, growth in the value of land. This result is quantitatively similar to Reher (2019), who computes the compensating variation associated with improvements, and finds that improving quality can account for 86% of real rent growth over 2010-16.

The indices to the right of the baseline in Figure 7a perform two robustness checks. First, I reestimate (11) after allowing the price vector β^Θ to vary by year. This results in a

[29] 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 7 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).

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 shifts. Second, because this is a measurement exercise, the primary challenge to interpretation is that the improvements $\Delta\Theta_{i,t}$ correlate with unobserved shocks (e.g. renter demand) unrelated to quality that would have raised rent anyway. Fortunately, I observe 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 (11). This leaves the growth rate almost unchanged at 0.6%, which is again consistent with a role for supply. Appendix Figure E17 suggests that the dominant role of improvements is largely a post-crisis phenomenon, as there is little difference between unadjusted and quality-adjusted rent growth over 1997-2007 or 2001-05. I explore the contributions of particular improvements in Appendix Figure E18.

In panel (b) of Figure 7, I plot the relationship between a householder's income and the quality discount on her home, defined as the percent difference between observed and quality-adjusted rent.[30] There is a strong positive relationship, and, as in Table 4, one interpretation of it is that real estate investors target improvements toward where the willingness-to-pay for quality is highest. Albouy and Zabek (2016) and Reher (2019) provide complementary evidence that higher-income households disproportionately experience increases in housing quality. Outside a housing context, this result may exemplify a more general phenomenon wherein innovations in product quality or variety are targeted toward higher-income households (e.g. Jaravel 2018; Acemoglu and Linn 2004).

5.2 Synthesis

The results from this section indicate that quality improvements account for a dominant share of post-Recession rent growth. By extension, the reallocation of financing toward improvement projects from Sections 3 and 4 has contributed to higher rent growth through the channel of better housing quality. One can approximate this contribution by multiplying the share of post-Recession real rent growth attributable to improvements by the share of improvements attributable to shifts in the supply of financing. Correspondingly, the credit

[30]The plot is residualized against a property fixed effect, which absorbs unobserved locational effects on a unit's average quality discount, and it conditions on having a positive discount.

supply shift can explain roughly 30% of real rent growth over 2015-16 through the channel of quality improvements, while the private equity supply shift can explain roughly 10% over 2010-16. This is, of course, a back-of-envelope calculation, since variable definitions and sample period vary across datasets. However, it suggests that greater supply of financing for improvement projects has not only had a first-order impact on real improvement activity, but also on rental housing costs.

There is also suggestive evidence that these financial supply shifts have had distributional effects, in that they appear to disproportionately benefit higher-income households. First, by definition, improvements reduce the supply of low-quality housing – a "good" that is more likely to be consumed by lower-income households due to either tighter budget constraints or non-homothetic preferences – while increasing the supply of high-quality housing. Second, Section 3.4 discussed evidence that greater supply of credit for improvement projects has increased homelessness. Third, Figure 7b shows how improvements are targeted toward higher-income households, possibly reflecting these households' greater willingness-to-pay for quality. However, the evidence of distributional effects should be viewed as suggestive, since a rigorous investigation of these effects would require introducing a preference structure and accounting for equilibrium responses (e.g. migration), which lie outside the scope of this paper.

6 Conclusion

I found that greater supply of financing for residential improvement projects has contributed to the recent surge in real improvement activity, and, by extension, contributed to higher rent growth. First, the introduction of bank capital requirements increases the supply of credit for improvements, which accounts for 44% of real improvement activity over 2015-16. Similarly, the interaction between public pension risk-shifting incentives and declining safe yields increases the supply of private equity for improvements, which accounts for 15% of real improvement activity over 2010-16. I conclude with a quality-adjustment exercise, finding that improvements collectively account for 65% of post-Recession rent growth.

Stepping outside a housing context, these results exemplify how portfolio reallocation by financial intermediaries − or financial regulations that induce such a reallocation − can affect the types of real projects that are performed. In this paper's setting, a reallocation of financing toward improvement projects and away from other types of residential investment increases real improvement activity. In addition, the results suggest that shifts in the supply of financing can have distributional implications, since improvements reduce the supply of relatively-affordable housing units by transforming them into relatively-expensive units. Investigating these distributional implications in an equilibrium model is an avenue for future research.

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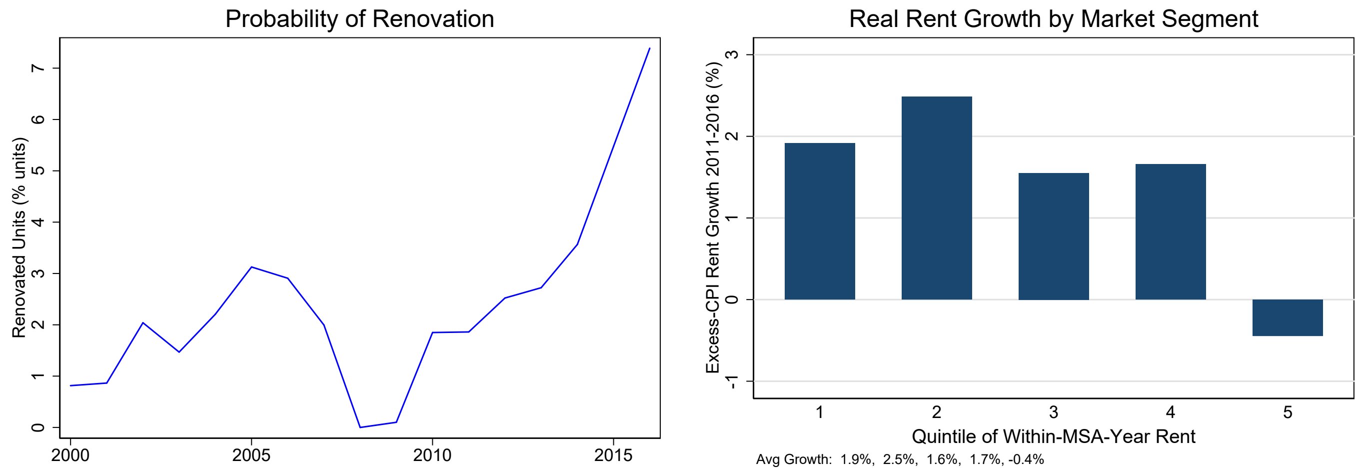
Figures

Figure 1: Facts about Quality Improvement Activity

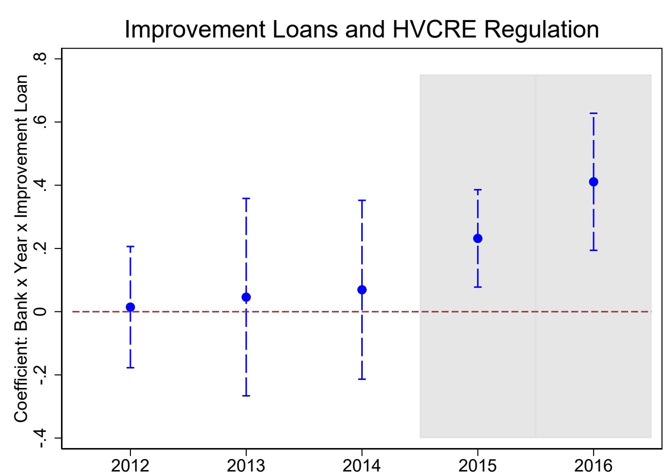


(a) Probability of Renovation

(b) Real Rent Growth by Market Segment

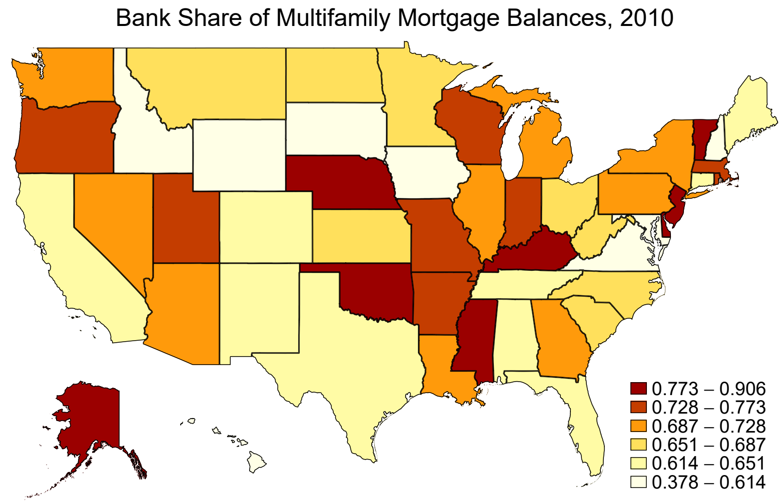
Note: Panel (a) plots the percent of multifamily units renovated each year. Panel (b) plots average real (i.e. excess-CPI) zip code level multifamily rent growth by rent quintile. The plot sorts zip codes into quintiles by rent relative to the MSA-year mean. Data in panels (a) and (b) are from Trepp and Zillow, respectively.

Figure 2: Improvement Financing and the Timing of HVCRE Regulation



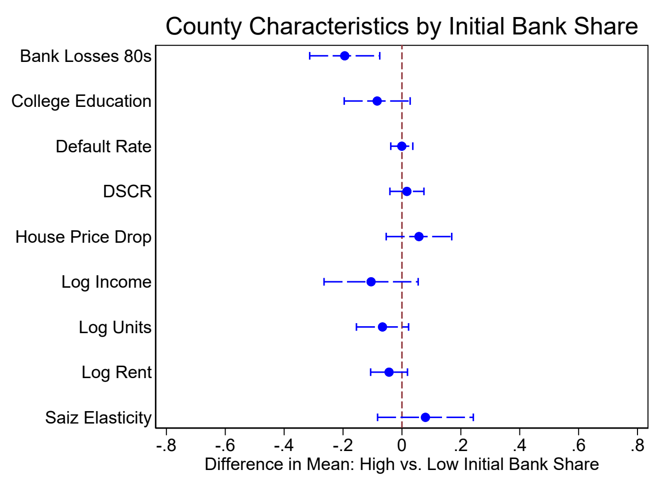
Note: This figure plots the estimated coefficients from a variant of equation (3). The regression is of log originated loans on the interaction between: (a) an indicator for whether the lender is a bank, (b) an indicator for whether the loan's purpose is an improvement, and (c) a series of year indicators. 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 twoway clustered by lender and year.

Figure 3: Geographic Distribution of Initial Bank Share



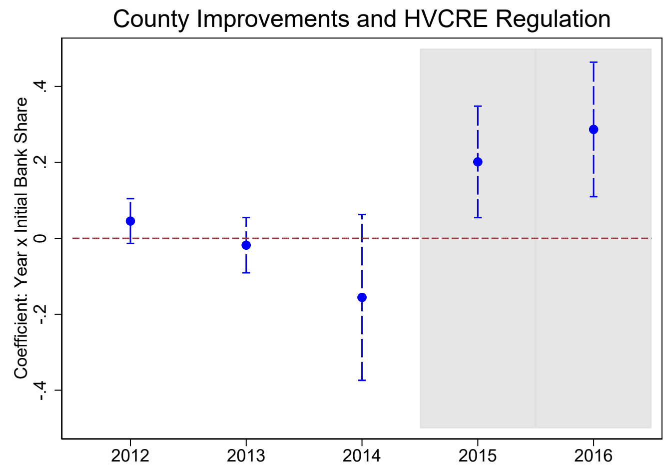
Note: This figure plots banks' share of multifamily mortgage balances in 2010 across states. Data are from Trepp.

Figure 4: County Characteristics by Initial Bank Share



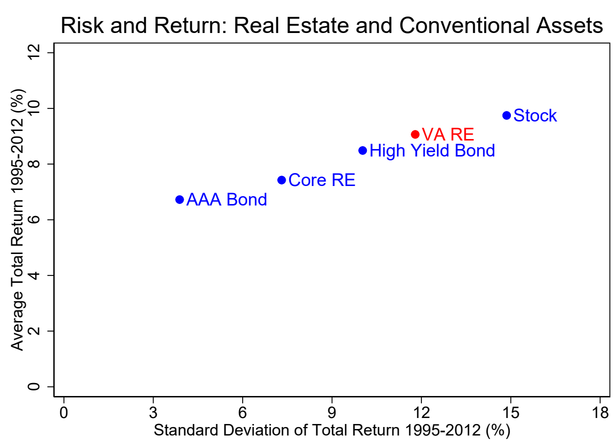
Note: This figure plots the difference in mean for the indicated variable between counties with a high and low bank share of multifamily mortgage balances in 2010. High and low are defined according to the median across counties. Variables are normalized to have unit variance and demeaned by state. Bank 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 2006-12 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 2011-16. 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 A.

Figure 5: Improvement Activity and the Timing of HVCRE Regulation

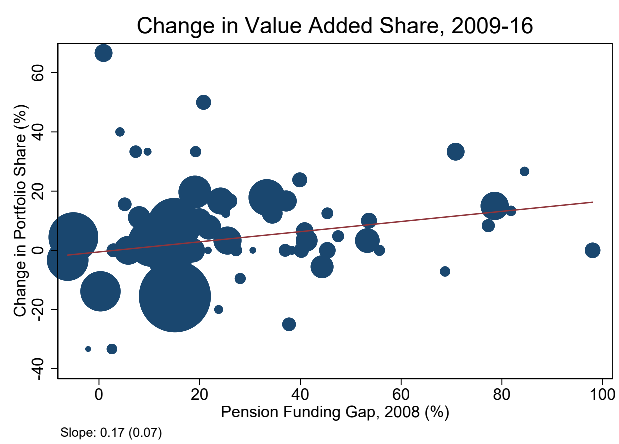


Note: This figure plots the estimated coefficients from a variant of equation (6). The regression is of log renovated properties in a county on the interaction between: (a) banks' share of multifamily mortgage balances in 2010 and (b) a series of year indicators. The rest of the specification is the 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 twoway clustered by county and year. Data are from Trepp.

Figure 6: Private Equity Real Estate Funds and Pension Investment



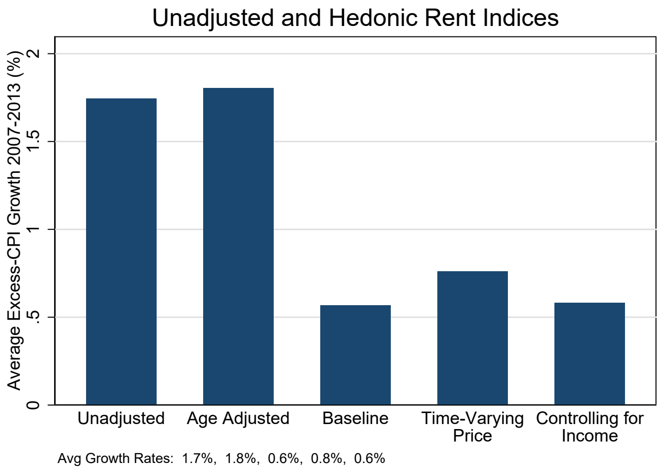
(a) Risk and Return in Private Equity Real Estate



(b) Public Pensions and Improvement-Oriented Funds

Note: Panel (a) plots the average and standard deviation of realized total returns over 1995-2012 for various assets. Core RE and VA respectively denote core and value added private equity real estate funds, whose returns are time-weighted. Panel (b) 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 2009-12 period to the 2014-16 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 in panel (a) come from: CRSP value-weighted stock index; Bank of America U.S. bond indices; and NCREIF core (ODCE) and value added (CEVA) indices. Data in panel (b) are from Preqin.

Figure 7: Summary of Hedonic Rent Index



(a) Comparison of Indices

(b) Quality Discount by Income

Note: Panel (a) plots average annual growth in real (i.e. excess-CPI) rent over 2007-13 for various rent indices. Unadjusted denotes average observed rent. Age Adjusted performs an age adjustment similar to that used by statistical agencies and described in Reher (2019). Baseline denotes the hedonic index from (13). Time-Varying Price denotes the baseline index after allowing the coefficients in (11) to vary by year. Controlling for Income denotes the baseline index after controlling for the change in the inhabitant's income percentile. Panel (b) plots the relationship between: a housing unit's quality discount; and the householder's income percentile among U.S. renters. The Quality Discount is defined as the percent difference between observed rent and the hedonic index. The plot is residualized against housing unit fixed effects and conditions on having a non-zero discount. The plot is binned, and each point corresponds to around 1,300 housing unit-years. Data are from the AHS.

Tables

Table 1: Improvement Financing by HVCRE-Affected Lenders

Specification:	Triple Difference-in-Difference		Difference-in-Difference		
Outcome:	$\log(\text{Loans}_{k,\ell,t})$	$\log(\text{Volume}_{k,\ell,t})$	$\log\left(\text{Renovated Housing Units}_{\ell,t}\right)$		
	(1)	(2)	(3)	(4)	(5)
$\text{Bank}_\ell \times \text{Post}_t \times \text{Imp}_k$	0.281**	5.329**			
	(0.142)	(2.024)			
$\text{Bank}_\ell \times \text{Post}_t$			1.210**	1.142**	1.564**
			(0.528)	(0.521)	(0.440)
$\text{Bank}_\ell \times \text{Post}_t \times \text{Sec Lag}_\ell$					1.947**
					(0.841)
Lender-Year FE	Yes	Yes			
Purpose-Year FE	Yes	Yes			
Bank × Imp	Yes	Yes			
Lender FE			Yes	Yes	Yes
Year FE			Yes	Yes	Yes
Lender Controls			No	Yes	Yes
Sec Lag-Year FE			No	No	Yes
R-squared	0.763	0.800	0.660	0.667	0.678
Number of Observations	966	966	582	582	582

Note: Subscripts k, ℓ and t denote loan purpose, lender, and year. Columns 1-2 estimate equation (3) and columns 3-5 estimate equation (4). 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 2011-16. Bank_ℓ denotes if lender ℓ 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_ℓ and Imp_k. $\text{Loans}_{k,\ell,t}$ is the number of loans for purpose k originated by ℓ in t, and $\text{Volume}_{k,\ell,t}$ is corresponding dollar volume. $\text{Renovated Housing Units}_{\ell,t}$ is the number of renovated units financed by a new loan by lender ℓ 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_ℓ is the average number of months between origination and securitization for loans originated by ℓ before 2011, normalized to have zero mean and unit variance. Sec Lag-Year FE are interactions between Sec Lag_ℓ and year indicators. The sample period is 2011-16. Standard errors clustered by lender are in parentheses. Data are from Trepp.

Table 2: Property-Level Improvement Activity and HVCRE Regulation

Outcome:	$\text{Renovation}_{i,\ell,t}$	
	(1)	(2)
$\text{Bank}_\ell \times \text{Post}_t$	0.012^{**}	0.012^{**}
	(0.004)	(0.004)
Property-Lender-FE	Yes	Yes
County-Year FE	Yes	Yes
Zip Code Controls	No	Yes
R-squared	0.308	0.308
Number of Observations	30733	30733

Note: Subscripts i, ℓ, and t denote property, lender, and year. This table estimates equation (5). Bank_ℓ denotes if lender ℓ 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 2011-16. Standard errors clustered by property are in parentheses. Data are from Trepp.

Table 3: County-Level Improvement Activity and HVCRE Regulation

Outcome	$\log(\text{Renovation Measure}_{c,t})$				
Measure:		Properties		Housing Units	Revenue
	(1)	(2)	(3)	(4)	(5)
$\text{Bank Share}_c \times \text{Post}_t$	0.228^{*}	0.255^{**}	0.279^{**}	1.598^{**}	2.991^{**}
	(0.128)	(0.101)	(0.100)	(0.605)	(1.120)
Year FE	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes
State-Year FE	No	Yes	Yes	Yes	Yes
County Controls	No	No	Yes	Yes	Yes
R-squared	0.565	0.705	0.721	0.694	0.695
Number of Observations	3159	3159	3159	3159	3159

Note: Subscripts c and t denote county and year. This table estimates equation (6). 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 2011-16. The sample period is 2011-16. Standard errors clustered by county are in parentheses. Data are from Trepp.

Table 4: Heterogeneous Effects Across Counties

Outcome	log (Renovated Properties$_{c,t}$)			
	(1)	(2)	(3)	(4)
Bank Share$_c$ × Post$_t$	0.291**	0.279**	0.213**	0.271**
	(0.110)	(0.103)	(0.082)	(0.106)
Bank Share$_c$ × Post$_t$ × Interaction$_c$	0.181*	-0.170**	-0.185*	-0.282*
	(0.104)	(0.078)	(0.097)	(0.159)
Interaction Variable	Income	Borrower Credit Access	Bank Concentration	Rent Control
Year FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes	Yes
Interaction-Year FE	Yes	Yes	Yes	Yes
R-squared	0.727	0.706	0.708	0.705
Number of Observations	3159	3159	3159	3159

Note: Subscripts c and t denote county and year. This table estimates a variant of equation (6). The specification is the similar to column 2 of Table 3 after interacting Bank Share$_c$ × Post$_t$ with the following terms: Income is real income per capital for the surrounding MSA averaged over 2011-16; 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 2011-16. The sample period is 2011-16. Standard errors clustered by county are in parentheses. Data are from Trepp.

Table 5: Value Added Investment and Public Pension Risk Taking

Specification:	Pension-Level		Manager-Level	
Outcome:	Prob of $\text{Commitment}_{p,t}^{\text{VA}}$		Fund $\text{Formed}_{m,t}^{\text{VA}}$	$\log\left(\text{Investment}_{m,t}^{\text{VA}}\right)$
	(1)	(2)	(3)	(4)
Funding $\text{Gap}_p \times$ Yield Gap_t	0.169**	0.155**		
	(0.059)	(0.061)		
Funding $\text{Gap}_m \times$ Yield Gap_t			0.070**	1.223**
			(0.021)	(0.421)
Pension FE	Yes	Yes		
State-Year FE	Yes	Yes		
Pension Controls	No	Yes		
Manager FE			Yes	Yes
Year FE			Yes	Yes
R-squared	0.715	0.724	0.255	0.184
Number of Observations	501	501	736	736

Note: Subscripts p, m, and t denote pension, private equity real estate fund manager, and year. Columns 1-2 estimate equation (9) and columns 3-4 estimate equation (10). Observations in columns 1-2 are public pension-years weighted by average assets over 2009-16, and observations in columns 3-4 are private equity real estate fund manager-years weighted by the manager's real estate capital raised over 2009-16. Funding Gap_p is the percent difference between the pension's actuarial liabilities and assets in 2008. Yield Gap_t is the difference between the yield on a 10-year TIPS bond in 2008 and in t. Prob of $\text{Commitment}_{p,t}^{\text{VA}}$ indicates an investment in an improvement-oriented ("value added") fund. Funding Gap_m is the average percent difference between actuarial liabilities and assets in 2008 across manager m's limited partners. Fund $\text{Formed}_{m,t}^{\text{VA}}$ indicates whether m formed an improvement-oriented fund for U.S. residential real estate with vintage t. $\text{Investment}_{m,t}^{\text{VA}}$ is the annualized investment by such funds between their vintage year t and 2016. Pension controls are: log actuarial assets, and allocations to cash, bonds, equity, and alternative assets. The sample period is 2009-16. 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.

Online Appendix for

"Financial Intermediaries as Suppliers of Housing Quality: Evidence from Banks and Public Pensions"

This document contains additional material related to the paper "Financial Intermediaries as Suppliers of Housing Quality: Evidence from Banks and Public Pensions". Appendix A describes the data and presents summary statistics. Appendices B and C perform extensions related to the credit supply and private equity supply research designs, respectively. Appendix D performs additional extensions referenced in the main text. Additional figures and tables may be found in Appendix E.

A Data Appendix

This appendix describes the paper's main datasets and how they were cleaned. Section A.1 describes the three core datasets and Section A.2 describes auxiliary ones. Summary statistics are presented in this appendix.

A.1 Core Datasets

A.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.[31] 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 5.1. I estimate the hedonic pricing equation (11) over 1997-2013 to utilize additional variation, but I only perform the adjustment over 2007-13. 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

[31] I only observe the unit's MSA for a subset of 166 MSAs.

Table A1: Summary Statistics for AHS Dataset

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

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; $\Delta \log\left(\text{Square Feet}_{i,t}\right)$ 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.

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 (13).

Table A1 provides summary statistics of the AHS dataset used to construct the hedonic index.

A.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 2010-16.[32] 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 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 E3.

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 approxi-

[32] I work with a random sample of Trepp's merged Property, Loan, and Loan2 file.

mated 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. They differ from new construction in that the building's foundation remains unchanged. 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.[33] For the denominator, I regress the log number of multifamily units in the sample over 2010-16 on the log aggregate stock of U.S. rental units from the Census' Housing and Vacancy Survey, which is available beginning 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 2010-16 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.[34]

   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.[35] 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

[33] 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.

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

[35] To 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.

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 D.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 features to help inspectors assign properties the appropriate score.[36] 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.[37] 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 $\text{Quality}_{i,t}$.

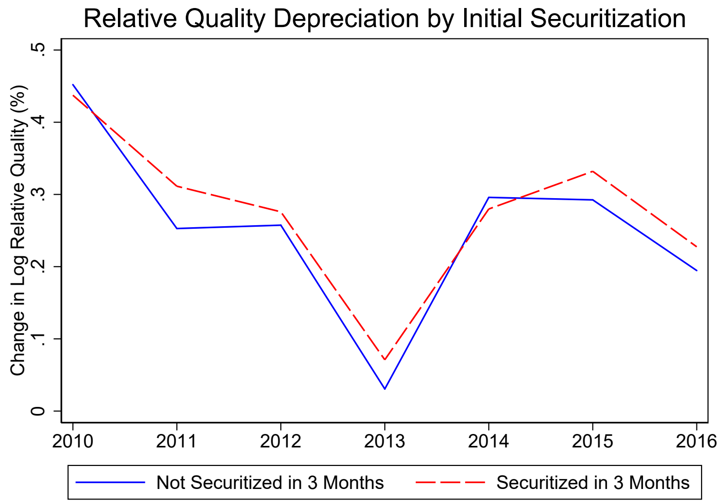
   This measure has the advantage of being nationally representative, standardized, and measured regularly. Moreover, it has the rare ability to capture intermediate regions of quality between that of a newly built unit and that of a unit with severe deferred maintenance.[38] See Reher (2019) for photographs of example apartments by MBA/CREFC rating.

[36] 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.

[37] Explicitly, $\text{Quality}_{i,t}$ is a mapping from the raw score $\text{Raw}_{i,t} \in \{1, ..., 5\}$ 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.

[38] 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).

Figure A1: 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.

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.[39] Figure A1 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 3, although I also use it to produce some of the stylized facts in Section 2. As discussed in Section 3, I work with both property and county-level datasets. Table A2 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 A.2.

[39]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.

Table A2: Summary Statistics for Trepp Dataset

	Observations	Mean	Standard Deviation
Property-Level Variables:			
Probability of $\text{Renovation}_{i,t}$	30733	0.026	0.158
$\text{Bank}_{i,t}$	30733	0.473	0.499
New $\text{Loan}_{i,t}$	30733	0.060	0.237
$\text{Due}_{i,t}$	143530	0.016	0.126
$\Delta \log \left(\text{Quality}_{i,t+1}\right)$	143530	-0.152	0.799
$\log \left(\text{Balance}_{i,t}\right)$	143530	7.856	7.766
County-Level Variables:			
Bank Share_c	3169	0.667	0.176
$\log \left(\text{Renovated Properties}_{c,t}\right)$	3169	0.152	0.375
$\log \left(\text{Renovated Housing Units}_{c,t}\right)$	3169	0.921	2.152
$\log \left(\text{Units}_{c,t}\right)$	3169	9.73	1.428
$\log \left(\text{Rent}_{c,t}\right)$	3169	6.475	0.161
$\log \left(\text{Income}_{c,t}\right)$	3169	10.772	0.242
$\log \left(\text{Storms}_{c,t}\right)$	3169	-7.657	1.507
$\text{LTV}_{c,t}$	3169	0.879	0.164
$\text{Delinquent}_{c,t}$	3169	0.034	0.048
$\text{DSCR}_{c,t}$	3169	1.555	0.295
$\text{ARM}_{c,t}$	3169	0.053	0.049

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 $\text{Renovation}_{i,t}$ indicates if the property was renovated in t; $\text{Bank}_{i,t}$ indicates if the property owner's lender is a bank; New $\text{Loan}_{i,t}$ indicates if a new loan was originated on i in t; $\text{Due}_{i,t}$ indicates whether the property has a loan due in t; $\Delta \log \left(\text{Quality}_{i,t}\right)$ is the change in log relative quality, measured using the MBA/CREFC rating; $\log \left(\text{Balance}_{i,t}\right)$ is the log of end-of-period loan balance. Note that the variables $\text{Due}_{i,t}$ through $\log \left(\text{Balance}_{i,t}\right)$ are used in the extension of Appendix B.3. 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; $\log \left(\text{Renovated Properties}_{c,t}\right)$ is the log number of renovated properties; $\log \left(\text{Renovated Housing Units}_{c,t}\right)$ is the log number of renovated housing units; $\log \left(\text{Units}_{c,t}\right)$ is the number of housing units; $\log \left(\text{Rent}_{c,t}\right)$ is the log average rent per unit; $\text{LTV}_{c,t}$ through $\text{ARM}_{c,t}$ 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 $\log \left(\text{Income}_{c,t}\right)$ and $\log \left(\text{Storms}_{c,t}\right)$ 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 A.2. Observations in the upper panel are property-years over 2011-16. Observations in the lower panel are county-years over 2011-16, weighted by the number of multifamily units in the county over that period.

A.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.[40] In addition, I observe the fund's property sector and geographic focus. This 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 $\text{Gap}_p \times$ 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 real estate fund over 2006-2008. I include all limited partners that committed capital to m over 2006-2008 when computing the averages Funding Gap_m and Yield Gap_t. Institutions other than public pensions are assigned a funding gap of 0.

My main use of the Preqin data is the private equity research design from Section 4. 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 A.2 below. I merge the Preqin 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 A3 provides summary statistics of the Preqin dataset used in Section 4.

A.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,

[40] 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.

Table A3: Summary Statistics for Preqin Dataset

	Observations	Mean	Standard Deviation
Pension-Level Variables:			
Prob of $\text{Commitment}_{p,t}^{\text{VA}}$	655	0.611	0.488
Prob of $\text{Commitment}_{p,t}^{\text{Core}}$	655	0.285	0.452
Prob of $\text{Commitment}_{p,t}^{\text{Opp}}$	655	0.483	0.500
Funding Gap_p	655	0.190	0.193
$\log(\text{Assets}_{p,t})$	655	17.764	1.228
Bond $\text{Share}_{p,t}$	655	0.231	0.073
Equity $\text{Share}_{p,t}$	655	0.499	0.100
Cash $\text{Share}_{p,t}$	655	0.026	0.028
Alternatives $\text{Share}_{p,t}$	655	0.165	0.107
Manager-Level Variables:			
Fund $\text{Formed}_{m,t}^{\text{VA}}$	736	0.083	0.276
$\log\left(\text{Investment}_{m,t}^{\text{VA}}\right)$	736	0.302	1.109
Funding Gap_m	736	0.085	0.109

Note: This table presents summary statistics of the key variables from the Preqin dataset. Subscripts p, m, and t denote public pension, real estate fund manager, and year. The upper panel summarizes pension-level variables: Prob of $\text{Commitment}_{p,t}^{\text{VA}}$ through Prob of $\text{Commitment}_{p,t}^{\text{Opp}}$ are the annual probability of committing capital to a value added, core, or opportunistic private real estate fund; Funding Gap_p is the difference between actuarial liabilities and assets and liabilities in 2008 expressed as a share of actuarial assets. The variables $\log(\text{Assets}_{p,t})$ through Alternatives $\text{Share}_{p,t}$ are log actuarial assets, and portfolio allocation to bonds, public equity, cash, and alternative assets in t, which are merged from the CRR dataset described in Section A.2. The lower panel summarizes manager-level variables: Fund $\text{Formed}_{m,t}^{\text{VA}}$ indicates the formation of a value added fund for U.S. residential real estate with vintage t; $\log\left(\text{Investment}_{m,t}^{\text{VA}}\right)$ is the log annual investment by such funds between their vintage year t and 2016; Funding Gap_m is the average percent difference between actuarial assets in liabilities in 2008 across m's limited partners over 2006-2008. Observations in the upper panel are pension-years over 2009-16 weighted by average assets over 2009-16. Observations in the lower panel are manager-years over 2009-16 weighted by the manager's real estate capital raised over that period.

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 4 are those which invested in private equity real estate, though not necessarily an improvement-oriented fund, over 2009-16.

- *Construction:* Data on financed construction projects come from Trepp. 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). When conducting county-level analysis, I use the number of multifamily permits issued according to the Census' Building Permits Survey in cases where I do not observe construction in Trepp.

- *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 (2014b) 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 (2018). 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 4 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.

- *Homeless Population:* Data on homelessness come from HUD's Point-in-Time (PIT) count. The PIT count is a count of sheltered and unsheltered homeless persons on a single night in January. The PIT is administered at the Continuum of Care (CoC) level. A CoC can encompass multiple counties, and so I manually create a crosswalk file to merge the homelessness data to my core county-level dataset.

B Extensions to Credit Supply Research Design

This appendix contains extensions related to the credit supply research design from Section 3 of the text.

B.1 County-Level Extensions

The baseline county-level result is also borne out when measuring exposure using banks' share of the office commercial mortgage market, shown in Appendix Table E3, and when including heterogeneous time trends for time-invariant county characteristics, shown in Appendix Table E4. In Appendix Figure E9, I estimate a cross-sectional version of (6) to investigate nonlinear treatment effects. There is variation around the best-fit line, but there is no clear nonlinear functional form.

B.2 Bank Lending in the Syndicated Loan Market

In this extension, I estimate the effect of HVCRE regulation on bank lending in the syndicated loan market. Due to institutional and data differences, the specification must be modified from its counterpart in (4). 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.[41] 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 2018). 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

[41] 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 in the baseline analysis.

"treated borrowers", in contrast to the control group, REITs, which perform both property improvements and construction.[42]

I therefore estimate the following specification

$$\text{New Loan}_{b,\ell,t} = \beta\left(\text{CCAR}_{\ell} \times \text{Post}_{t} \times \text{Developer}_{b}\right) + \alpha_{\ell,t} + \alpha_{b,t} + \alpha_{b,\ell} + u_{b,\ell,t}, \tag{B1}$$

where b, ℓ, and t index borrowers, lenders, and years, New Loan$_{b,\ell,t}$ indicates whether a new secured loan was made, Developer$_b$ indicates if the borrower is a developer, and 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 2012-16.[43]

The parameter of interest in (B1) is β, 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 β 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 E5 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 E5 drops such borrowers from the sample, which yields a similar result. This suggests that HVCRE regulations transferred capital from firms specializing in construction to firms which perform improvements, consistent with the baseline results in Table 1.

B.3 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 et al. (2012). This variation generates effectively exogenous credit demand shocks, and the logic of the exercise is to ask whether these shocks resulted in more

[42] There 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).

[43] As in the baseline specification (4), pairs are weighted by the lender's loan issuance over this period.

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 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.[44] 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

$$Y_{i,\ell,t} = \sum_{\tau=-1}^{1} \beta_\tau \text{Due}_{i,t+\tau} + \alpha_{i,\ell} + \alpha_{z,t} + \alpha_{\ell,t} + u_{i,\ell,t}, \tag{B2}$$

where i,ℓ, 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.[45]

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.[46] This measure captures more modest improvements in quality (e.g. repainting common areas), and thus there is enough variation in estimate (B2). My outcome $Y_{i,\ell,t}$ is the change in the log MBA/CREFC rating from $t-1$ to t, denoted $\Delta \log \left(\text{Quality}_{i,\ell,t}\right)$, which is normalized to have unit variance.[47]

Panel (a) of Figure E10 plots the estimated coefficients from (B2). 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.

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

[45] I cannot include borrower-lender fixed effects because I only observe the borrower's identity, used to construct Figure E19, for 14% of the sample.

[46] Appendix A has details on why this rating is collected, its interpretation, and scope for misreporting bias.

[47] There is a 6.8% annual hazard of improvement according to the MBA/CREFC rating. See Appendix A for additional summary statistics.

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 (B2) 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 $\text{Impending}_{i,t}$.[48] The results in Figure E10b 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

$$\Delta \log \left(\text{Quality}_{i,\ell,t+1}\right) = \beta \left(\text{Bank}_\ell \times \text{Post}_t \times \text{Due}_{i,t}\right) + \alpha_{i,\ell} + \alpha_{z,t} + \alpha_{\ell,t} + ... \tag{B3}$$
$$... + \alpha_t \times \text{Due}_{i,t} + \alpha_\ell \times \text{Due}_{i,t} + u_{i,\ell,t}.$$

The parameter β 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 (B3) 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 E10b.

The results are in Table E6. Column 1 provides necessary context by estimating the effect of having a loan due on subsequent growth in quality.[49] The estimates of (B3) 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 3.3, 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, column 3 uses the triple interaction as an instrument for the property's log loan balance, normalized to have unit variance.[50] The point estimate suggests that a 1 standard deviation increase in credit raises subsequent growth in quality by 0.3 standard deviations.

[48]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 regression includes the log of the loan's term to remove the mechanical effect of term premia.

[49]Specifically, column 1 estimates (B2) when restricting the lag terms to $\tau = -1$.

[50]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.

B.4 External Validity of Bank Lending Estimates

I now assess the external validity of the bank lending estimates from Section 3. The particular issue, described in Section 3.5, 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 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 E7 reestimates the lender-year specification (3) 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.

Second, I reestimate the county-level specification (6) 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 A.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.

The results in column 1 of Table E8 suggest that counties with a 10 pps higher bank exposure received approximately 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 3.4 as a lower bound.

C Extensions to Pension Research Design

This appendix contains extensions related to the private equity research design from Section 4 of the text.

C.1 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 E9 reveal similar results when using nominal 10-year Treasury or Aaa corporate bond yields to measure Yield Gap_t.

C.2 Safe and Very Risky Fund Strategies

In Table E10, I perform the previous exercise among safer core real estate funds, which perform buy-and-hold projects. The estimates imply that relatively-underfunded pensions weakly decrease their investment in core real estate funds when safe yields fall. In Table E11, I perform a symmetric test with respect to opportunistic real estate funds, which perform construction projects and command a high risk premium.[51] Unlike core funds, underfunded pensions become more likely to invest in risky opportunistic real estate funds when safe yields fall. However, the magnitude is somewhat weaker relative to the effect observed among more moderate value added funds.

C.3 Unconventional Monetary Policy

In this extension I investigate the source of time-series variation used to identify β in (9). First, note from Figure E11 that the yield gap is non-monotonic over the sample period and, in particular, it has both a trend and a cyclical component. Identification based on the cyclical component is preferable, albeit not necessary, since short-term fluctuations can more easily be linked to concrete shocks. To investigate the role of short-term fluctuations, I reestimate (9) after interacting the exposure variable, Funding $\text{Gap}_{p,08}$, with a linear time trend. Thus, the only time-series variation in the treatment variable, Funding $\text{Gap}_{p,08} \times$ Yield Gap_t, is cyclical. The positive point estimate in column 1 of Table E13 suggests that short-term fluctuations in the safe yield indeed influence pension investment behavior.

[51] Opportunistic funds have a historic average net return of 13.5% with a standard deviation of 19.2% (Pagliari 2017).

The introduction of unconventional monetary policy is a leading source of variation in the non-trend component of safe yields over the period of analysis. To investigate the role of fluctuations induced by monetary policy, I instrument for Yield Gap_t using the cumulative change in safe yields in year t attributable to unconventional monetary policy surprises that year. I follow Chodorow-Reich (2014a) closely in this respect, with two differences. First, the set of surprises in Chodorow-Reich (2014a) ends in September 2013, and so I augment it with the set of FOMC statements about forward guidance and balance sheet policies (i.e. quantitative easing) from October 2013 through December 2016, which the Fed makes available in its Timelines of Policy Actions and Communications. Second, whereas Chodorow-Reich (2014a) uses intraday data, I measure the effect of the surprise as the change in the 5-year Treasury yield from the day before the surprise to the day after it, based on the CRSP 5-Year Noncallable Treasury Note Index. Table E12 lists the set of monetary surprises and the change in safe yields attributable to them.

In column 2 of Table E13, I restrict variation in safe yields to that which is attributable to unconventional monetary policy surprises. Specifically, I instrument for the treatment, Funding $\text{Gap}_p \times$ Yield Gap_t, using the product between Funding Gap_p and the cumulative change in safe yields in year t attributable to unconventional monetary policy surprises that year. The resulting point estimate implies that the surprise-induced fluctuations in the safe yield are an important source of variation used in identification. While this result does not rule out the possibility that other time-varying dynamics affect pensions' investment, it does provide a concrete example of variation in safe yields that influences pension investment behavior.

C.4 Placebo

Table E14 performs a placebo test over the 2003-07 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.[52] Moreover, this period is located at approximately the same stage in the pre-crisis real estate cycle as the baseline 2009-16 period. This timing helps address concerns that the results are driven by real estate cyclicality.

C.5 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

[52] The regression is the same as in (9) replacing the base year with 2002.

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 E15 reestimates (9) including a separate time trend for these pensions. The results are somewhat weaker, but still significant.

C.6 Manager Skill

If well-funded pensions are run by skilled managers, the results could be 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.

C.7 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 (9) 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.[53] Like value added funds, the 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 E16 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 sufficient expected return to meet their obligations.

C.8 Manager-Year Fixed Effects

I estimate a manager-strategy-year level specification so that I can include manager-year fixed effects. The regression is

$$Y_{m,t} = \beta \left(\text{Funding Gap}_m \times \text{Yield Gap}_t \times \text{VA}_k\right) + \alpha_{m,t} + \alpha_{k,t} + \alpha_{m,k} + \gamma X_{m,t} + u_{m,t}, \qquad \text{(C1)}$$

[53] According to Preqin, the average historic net IRR for private distressed debt funds is 12.4% compared to 12.8% for value added funds.

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 (3), 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 E17 again provide evidence that pension risk taking encourages real estate fund managers to tilt their portfolio toward value added (i.e. improvement-oriented) funds. Unlike the baseline specification (10), one cannot infer whether managers increase formation of value added funds or simply stop 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.

C.9 Manager-Pension Matching

The main identification assumption in (10) 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 E12 investigates this assumption by performing a similar exercise as Figure 4 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. While I only have access to a relatively small set of observable variables, there are no significant differences between managers with high and low exposure to underfunded public pensions. 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 finding suggests that managers are not responding to local economic conditions near their headquarters.

C.10 Magnitude of Pension Investment Effect

This extension describes the procedure for obtaining the aggregate effect referenced in Section 4. 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

$$\text{Effect}^{\text{Sample}} = \frac{\sum_m \sum_{t=2010}^{2016} \text{Investment}_{m,t}^{\text{VA}} \times \left[1 - e^{-\beta^P \times \text{Yield Gap}_t \times \max\{\text{Funding Gap}_m, 0\}}\right] \times \Delta t}{\sum_m \sum_{t=2010}^{2016} \text{Investment}_{m,t}^{\text{VA}} \times \Delta t}, \tag{C2}$$

where β^P is the estimate from column 4 of Table 5. As in the text, $\text{Investment}_{m,t}^{\text{VA}}$ 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 47% of investment by improvement-oriented funds over 2010-16.

As mentioned in the text, it is difficult to map this effect to aggregate improvement activity. I obtain an approximate order of magnitude by noting that investment by private equity real estate funds from Preqin accounts for 48% of aggregate investment in existing rental units from the Fixed Assets Accounts over 2010-16. Improvement-oriented (i.e. value-added) funds account for 64% of private equity real estate investment among funds with a known strategy. By extension, the in-sample effect maps to approximately 15% ($0.48 \times 0.64 \times 0.47$) of aggregate investment in existing rental units over that period.

D Additional Extensions

This appendix contains additional extensions referenced in the text.

D.1 Relationship Persistence in Real Estate Finance

This extension estimates relationship persistence in real estate finance in three applications: multifamily 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 (2014b), 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$_{b,\ell,s}$,

$$\text{Loan Originated}_{b,\ell,s} = \rho \text{Loan Originated}_{b,\ell,s-1} + \alpha_{\ell,t} + u_{b,\ell,s}. \tag{D1}$$

The pairs (b, ℓ) span each possible pair among active borrowers and lenders over 2012-16. The results in column 1 of Table E19 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 $\alpha_{\ell,t}$.

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 E19 provides complementary, stylized evidence by plotting the distribution of number of lenders per borrower in the multifamily mortgage market.[54] 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.

Next, I turn to the private equity real estate market. The specification is analogous to (D1), after replacing "borrowers" with "private equity real estate fund managers" and "lenders" with "public

[54] The figure is based on the 14% subset of the Trepp data for which I observe the borrower's identity.

pensions". I estimate the probability that pension p commits capital to the sth fund for manager m, denoted by the indicator $\text{Investment}_{p,m,s}$,

$$\text{Investment}_{p,m,s} = \rho \text{Investment}_{p,m,s-1} + \alpha_{p,t} + u_{p,m,s} \tag{D2}$$

Similarly to before, the pairs (p, m) span each possible pair among active pensions and managers. The results in Table E20 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 2008-16 and denoted $\log(\text{Size}_m)$. As discussed above, greater stickiness for relatively small fund managers may reflect screening or monitoring costs.

The relationship persistence documented in Table E20 may seem puzzling in light of the fact that private equity real estate fund managers are relatively large compared to the borrowers studied in Table E19. In Figure E20 I plot the size distribution of managers' fundraising alongside that of pensions' increase in real estate holdings over 2009-16, from the CRR.[55] While many managers are large, so also 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. Given these relative magnitudes, it is therefore not implausible for relationships to be sticky in the private equity real estate market.

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 sth bond issuance for REIT j, denoted $\text{Lead Underwriter}_{j,u,s}$, conditional on whether u was the lead underwriter j's previous issuance or was at least a participant underwriter, denoted $\text{Lead Underwriter}_{j,u,s-1}$ and $\text{Underwriter}_{j,u,s-1}$ respectively. The regression is

$$\text{Lead Underwriter}_{j,u,s} = \rho_0 \text{Lead Underwriter}_{j,u,s-1} + \rho_1 \text{Underwriter}_{j,u,s-1} + \alpha_{u,t} + u_{j,u,s}, \tag{D3}$$

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

[55] This is a proxy for total real estate investment because I have limited information on committed capital to private equity real estate funds.

2017.

Column 1 of Table E21 has the results of (D3). The positive estimate on Lead Underwriter$_{j,u,s-1}$ 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 E19. 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.

Collectively, the results of this extension indicate that relationships in real estate finance are sticky, supporting the statements made in Sections 3 and 4.

D.2 Quality Improvements in the Cross-Section

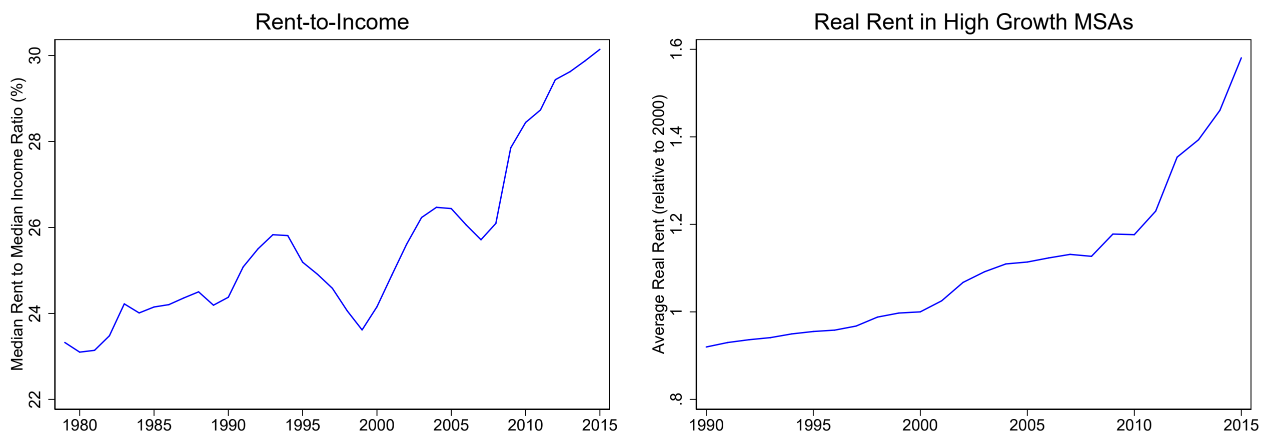
This section describes cross-sectional characteristics of quality improvements referenced in Section 2 of the paper. In Table E22, I regress the share of renovated units in an MSA against the MSA's log elasticity of housing supply, as estimated by Saiz (2010), log average income, college education share, and an indicator for whether rent control or stabilization practices are in place. All variables are normalized to have unit variance. There is a positive, albeit statistically-weak relationship between an MSA's renovation share and the MSA's elasticity of housing supply. Low values of this elasticity capture natural or regulatory constraints that make it difficult to build new housing units, so that this result is consistent with real estate investors substituting from construction to improvement projects.

An MSA's renovation share is lower in where there is rent control. This correlation is quite intuitive, since rent control directly counteracts investors' reward for making improvements. In addition, there is a positive, though somewhat weak correlation between income and improvement activity. This correlation is consistent with the results of Section 5, specifically the finding that improvements appear targeted toward higher-income households.

E Additional Figures and Tables

Additional Figures

Figure E1: Rent-to-Income Ratio and Real Rent

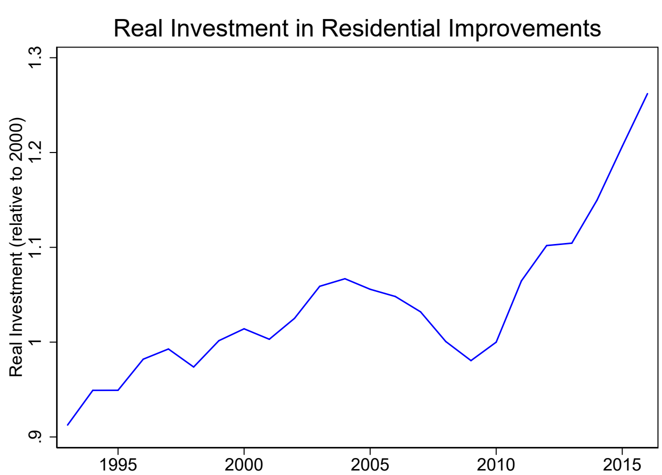


(a) Rent-to-Income

(b) Real Rent in Major MSAs

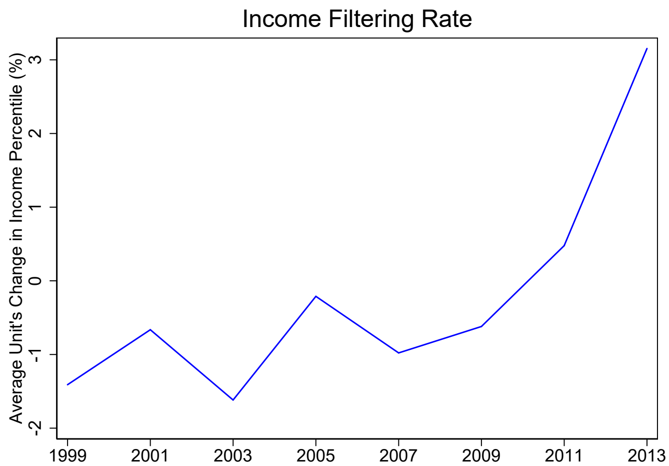
Note: Panel (a) plots the ratio of median rent to median household income. Panel (b) plots average real rent across the top quartile of MSAs sorted by 2008-2015 rent growth. Data are from Zillow and the Census Bureau.

Figure E2: Aggregate Spending on Improvements



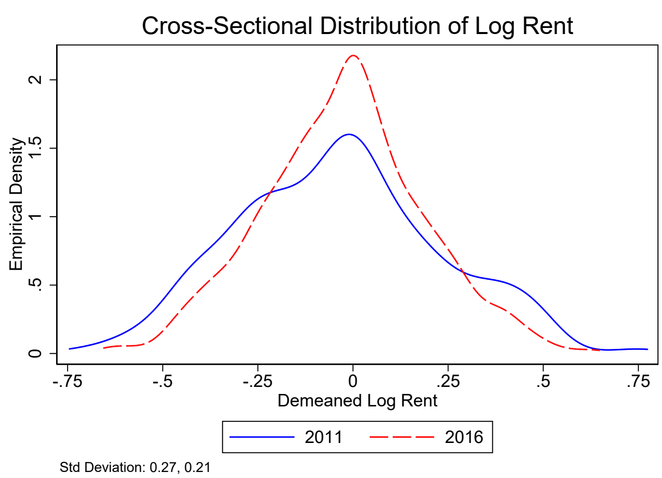
Note: This figure plots real aggregate investment in residential improvements from the Fixed Assets Accounts. Data are from the BEA.

Figure E3: Rate of Income Filtering



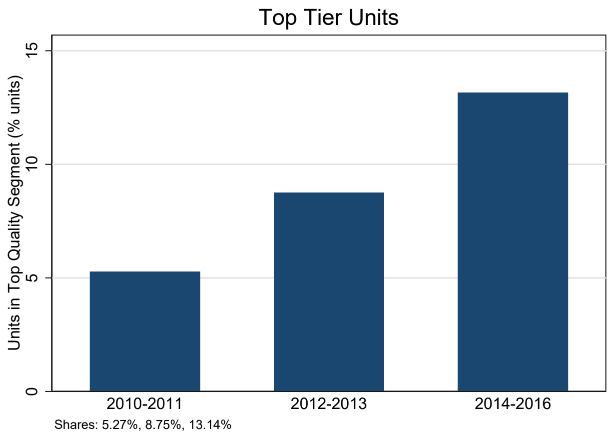
Note: This figure plots the average rental unit's change in its inhabitant's overall income percentile. Data are from the AHS.

Figure E4: Cross-Sectional Distribution of Log Rent

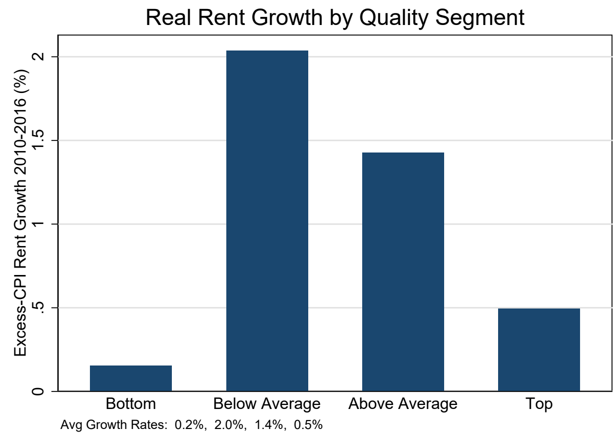


Note: This figure plots the cross-sectional empirical density of zip code level multifamily log rent in 2011 and 2016. Log rent is demeaned by MSA and year. The density is constructed using a Gaussian kernel. The plot excludes observations more than 3 standard deviations from the mean. Data are from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

Figure E5: Quantity and Rent Growth of Top Tier Units



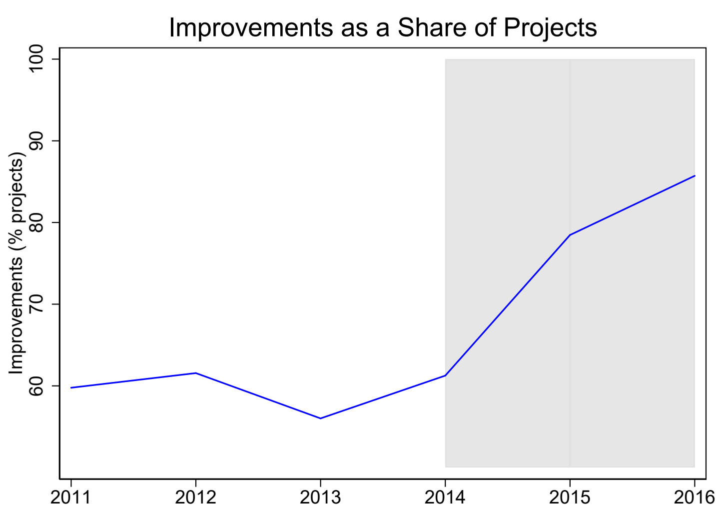
(a) Top Quality Segment



(b) Real Rent Growth by Market Segment

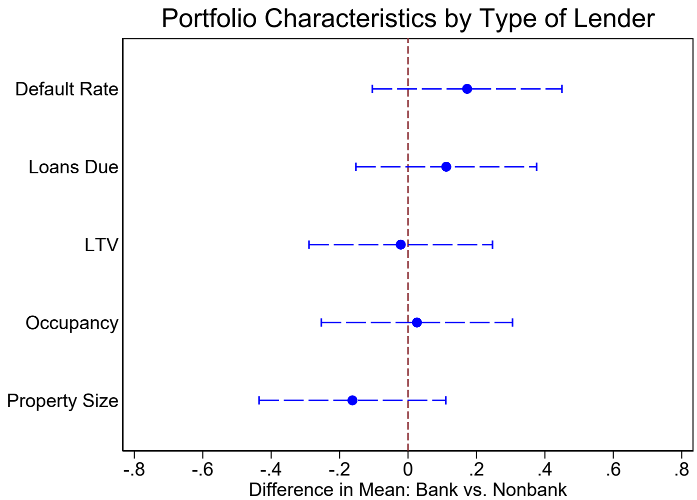
Note: Panel (a) plots the percent of multifamily units in the top quality segment, based on the MBA/CREFC rating. Panel (b) plots average real rent growth for properties in the top segment, above and below average segments, and bottom segment, based on the MBA/CREFC rating. Data are from Trepp

Figure E6: Improvements as a Share of Projects



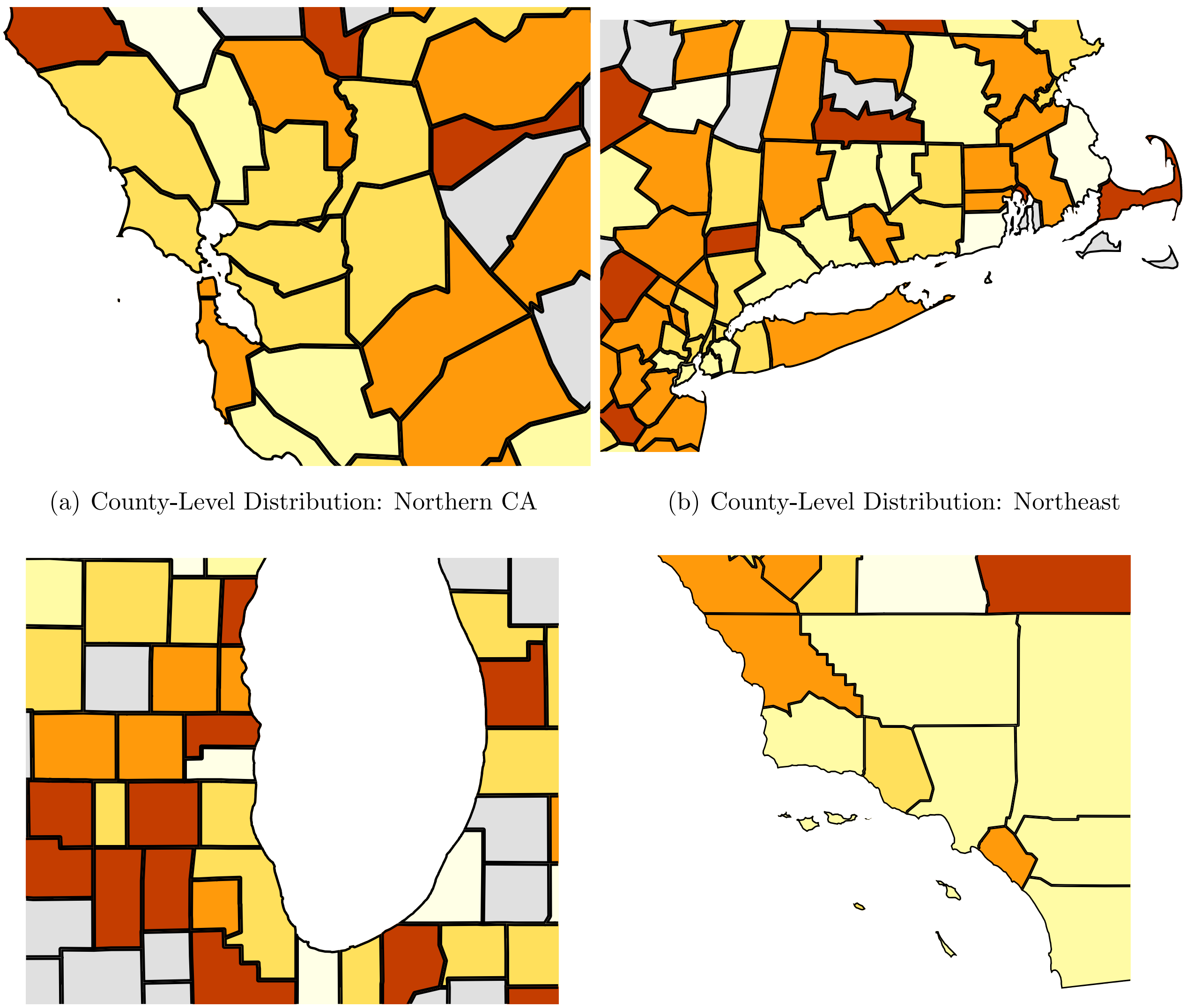
Note: This figure plots the number of renovated multifamily units divided by the number of renovated multifamily units plus the number of newly built multifamily units. The gray region indicates the period when HVCRE regulations are in place. Data are from Trepp.

Figure E7: 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 2011-16, 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.

Figure E8: Distribution of Initial Bank Share in High Growth Areas



(a) County-Level Distribution: Northern CA

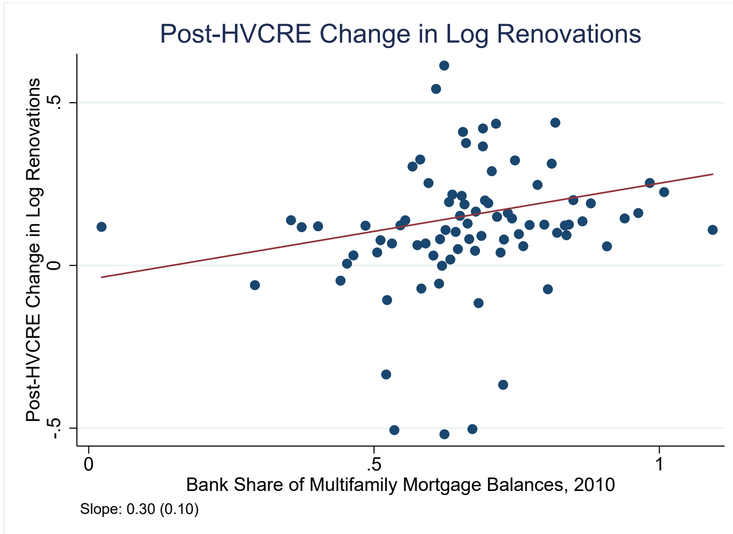
(b) County-Level Distribution: Northeast

(c) County-Level Distribution: Chicagoland

(d) County-Level Distribution: Southern CA

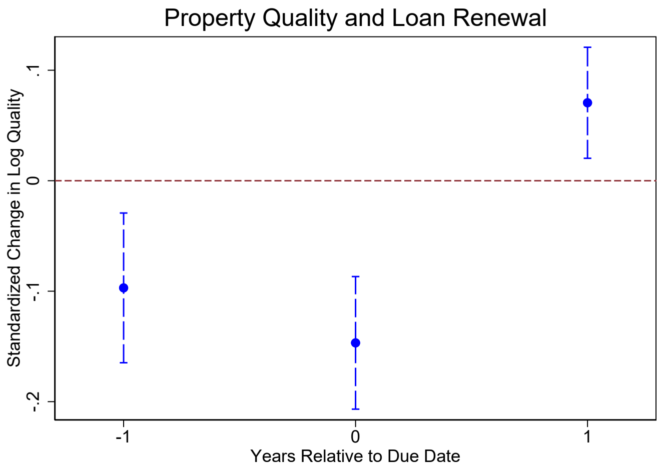
Note: This figure plots banks' share of multifamily mortgage balances in 2010 across counties in high growth metro areas. Panels (a)-(d) plot this share across counties in northern California, northeast states, the Chicagoland area, and southern California, respectively. The plot is analogous to the state-level map in Figure 3. Warmer colors indicate a higher share.

Figure E9: County-Level Improvements and HVCRE Regulation in the Cross-Section

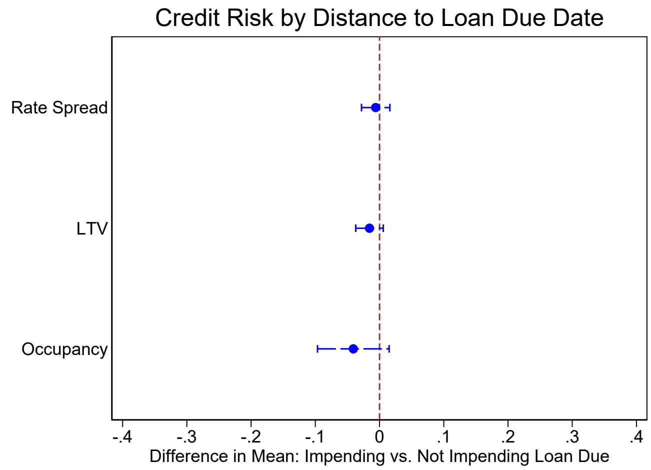


Note: This figure plots the relationship between a county's: (i) change in log renovated apartments from the 2011-14 period to the 2015-16 period, and (ii) share of multifamily mortgage balances in 2010 held by banks. The plot residualizes against a state fixed effect and the change in the controls from Table 3 from the 2011-14 to 2015-16 periods. The regression is the same as (6) after averaging across the 2015-16 and 2011-14 periods for each county and taking the difference. Each observation is a county weighted by the average number of multifamily units over 2011-16. The plot is binned. Data are from Trepp.

Figure E10: Quality Improvements and Credit Risk Related to Loan Due Dates



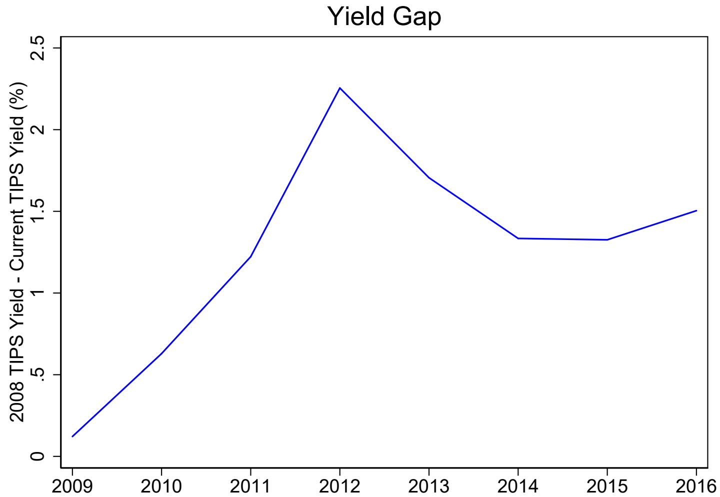
(a) Change in Quality Around Due Date



(b) Credit Risk of Coming-Due Borrowers

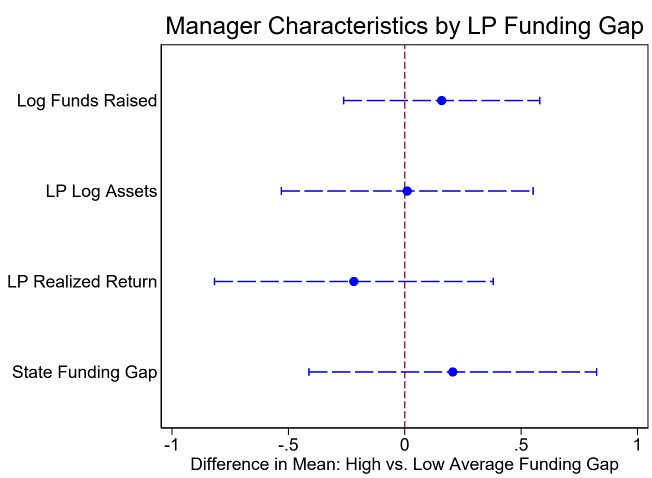
Note: This figure plots results from regressions similar to column 1 of Table E6. Panel (a) estimates a regression of the change in a property's log relative quality on indicators for whether the property's loan is due the subsequent, current, or previous year. Quality is based on the MBA/CREFC rating, 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 2010-16. Brackets are a 95% confidence interval with standard errors clustered by property. Data are from Trepp.

Figure E11: Variation in Yield Gap



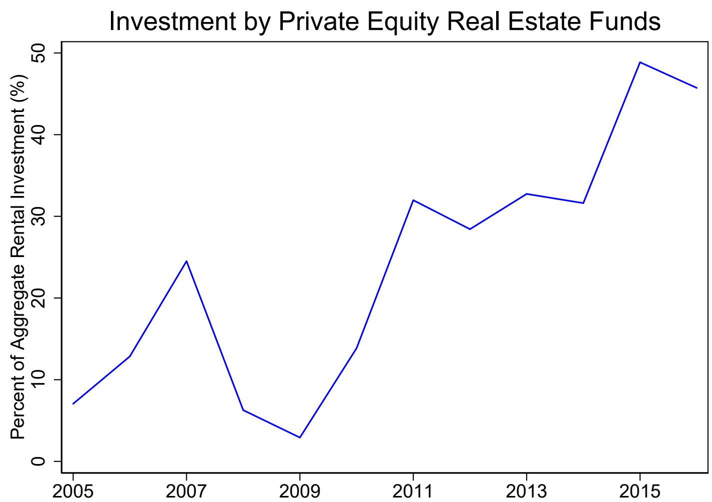
Note: This figure plots the average difference the 10-year TIPS yield in 2008 and the 10-year TIPS yield in the indicated year.

Figure E12: Manager Characteristics by Average Limited Partner Funding Gap



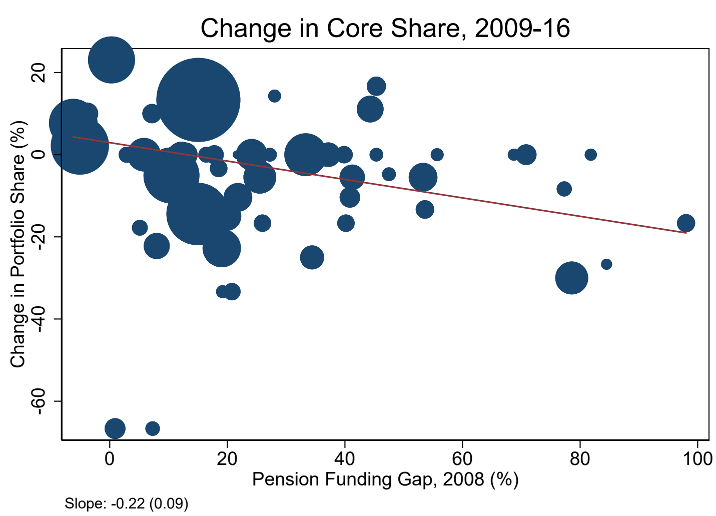
Note: This figure plots the difference in mean for the indicated variable between managers with a high and low average funding gap across limited partners in 2008. High and low are defined according to the median across managers. Variables are normalized to have unit variance. Log Funds Raised is log of total real estate capital raised by the manager over 2009-16. LP Log Assets and LP Realized Return are 2008-10 averages of log total assets and 7-year realized return across the manager's public pension limited partners. 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 2009-16. Brackets are a 95% confidence interval with heteroskedasticity robust standard errors. Data are from Preqin and the CRR.

Figure E13: Investment by Private Equity Real Estate Funds



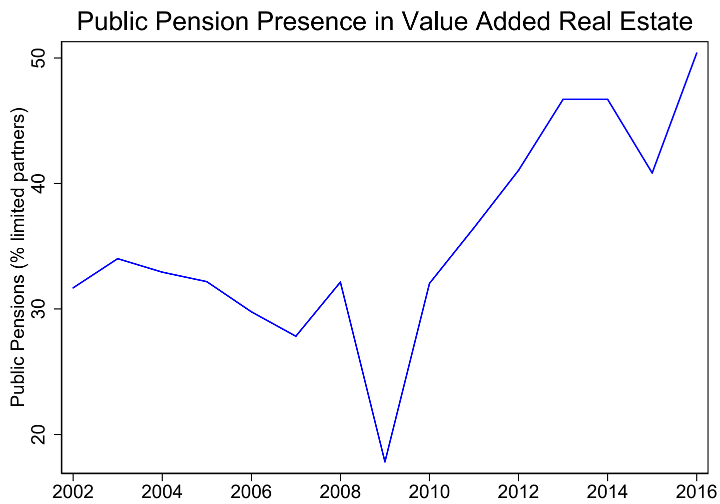
Note: This figure plots investment by private equity real estate funds in U.S. residential real estate as a percent of aggregate tenant occupied residential investment from the Fixed Assets Accounts. Data are from Preqin and the BEA.

Figure E14: Public Pensions and Buy-and-Hold Funds



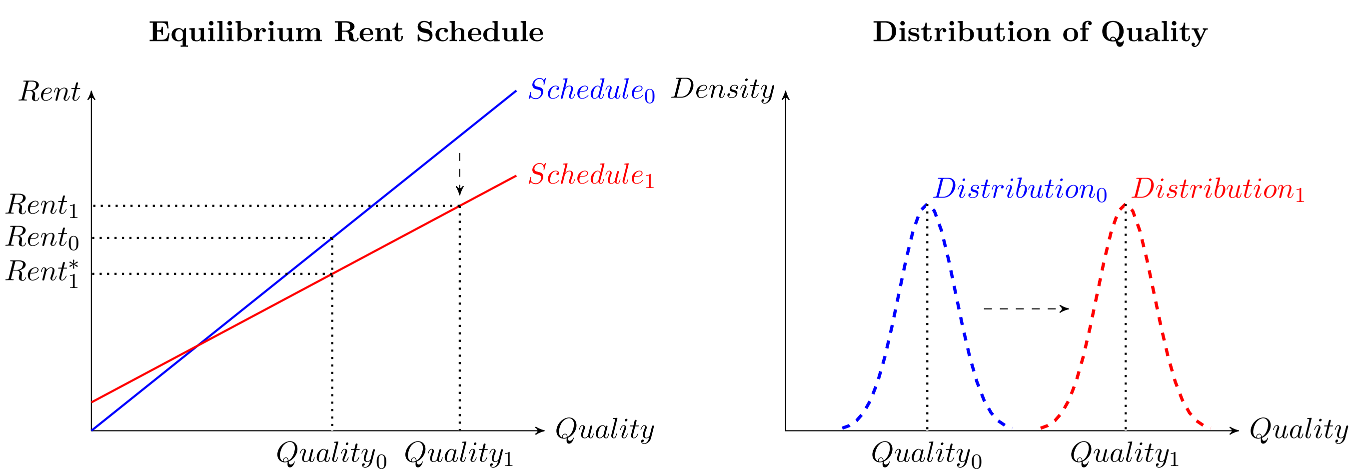
Note: This figure plots the relationship between a pension's: (i) change in the share of private equity real estate portfolio allocated toward buy-and-hold ("core") funds from the 2009-12 period to the 2014-16 period, and (ii) the percent difference between the pension's actuarial liabilities and assets in 2008. Each observation is a public pension. Larger dots correspond to larger pensions by total assets. Data are from Preqin.

Figure E15: Public Pension Presence in Improvement-Oriented Real Estate Funds



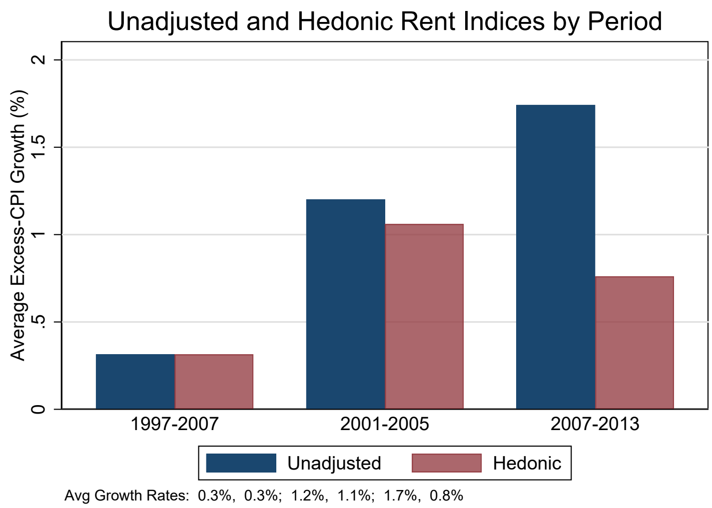
Note: This figure plots the share of investors in improvement-oriented ("value added") private equity real estate funds that are public pensions by the fund's vintage year. Data are from Preqin.

Figure E16: Rent Schedule and Quality Distribution After Increase in Supply of Improvements



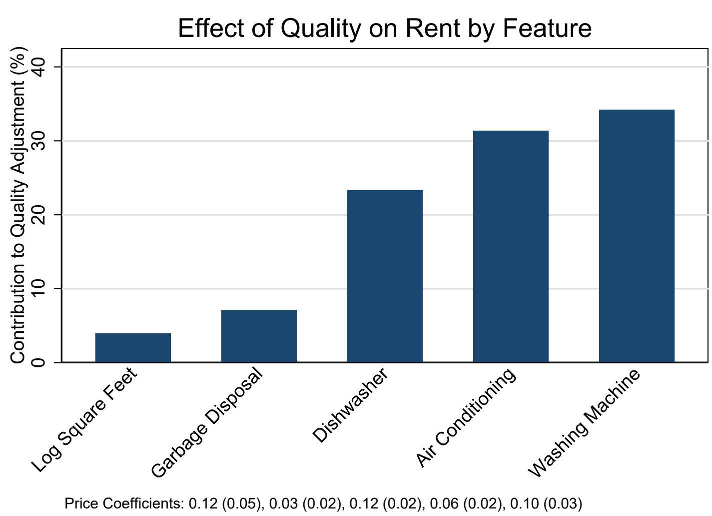
Note: The solid upward sloping line $Schedule_0$ is the equilibrium relationship between a housing unit's quality and its rent. The blue dashed bell curve $Distribution_0$ is the distribution of quality across the housing stock. Average quality in the initial distribution is $Quality_0$ and the corresponding observed rent is $Rent_0$. An increase in the supply of improvements shifts the distribution to $Distribution_1$ and the rent schedule to $Schedule_1$. Average quality shifts to $Quality_1$, with corresponding observed and quality-adjusted rent $Rent_1$ and $Rent_1^*$.

Figure E17: Hedonic Rent Index by Period



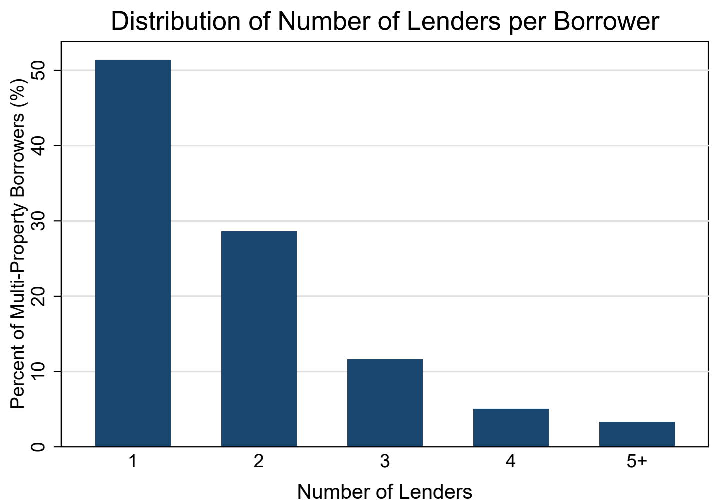
Note: This figure plots unadjusted and hedonic real rent growth for various periods allowing the coefficients in (11) to vary by year. Data are from the AHS.

Figure E18: Contribution to Hedonic Index by Feature



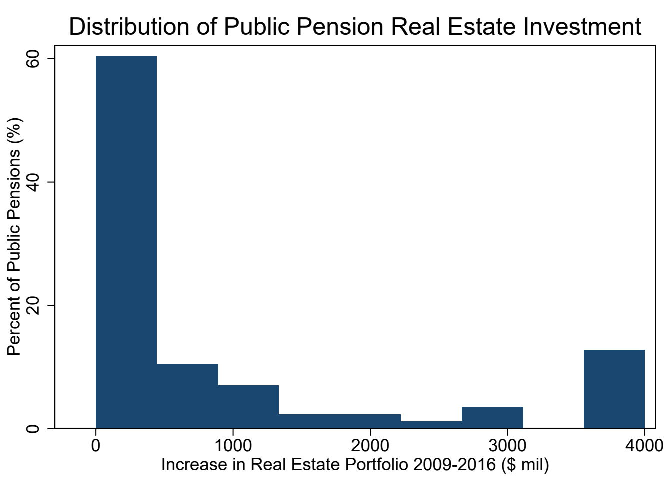
Note: This figure plots the contribution of each feature to the hedonic index, defined as the feature's average price effect from (11) across properties and years, divided by the sum of price effects across features. The contribution of feature f is $\frac{\sum_i \sum_t \beta^{\Theta} \Delta\theta_{i,t}}{\sum_f \sum_i \sum_t \beta^{\Theta} \Delta\theta_{i,t}}$ for $t \in \{2009, 2011, 2013\}$, $i \in \mathcal{I}$, and $\theta \in \Theta$. The plot is restricted to the top 5 features sorted by price coefficient β^{Θ} from (11), and so the area underneath the bars sums to 100. Data are from the AHS.

Figure E19: Multifamily Mortgage Relationships

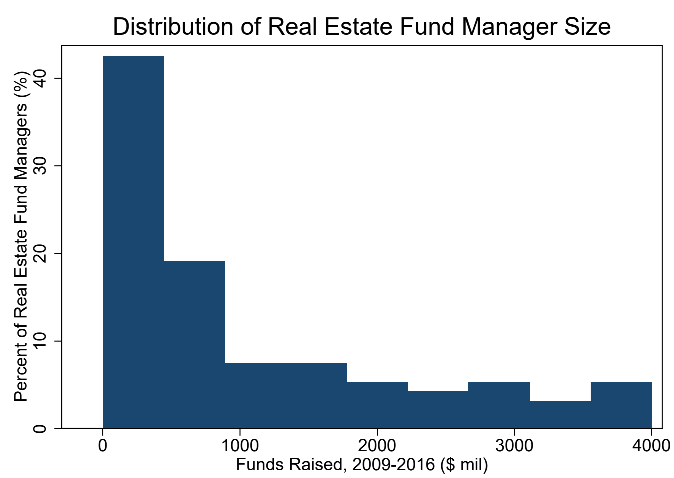


Note: This figure plots the distribution of the number of distinct lending relationships per borrower in the multifamily mortgage market across borrowers with more than 1 mortgaged property over 2010-16. Data are from Trepp.

Figure E20: Distribution of Pension and Real Estate Fund Manager Size



(a) Public Pension Real Estate Investment



(b) Real Estate Fund Manager Size

Note: Panel (a) plots the distribution of the increase in real estate portfolio over 2009-16 across public pensions. Panel (b) plots the distribution of private real estate funds raised over 2009-16 across fund managers. Both distributions are top-coded at $4 billion. The set of pensions and fund managers are those in the sample for the paper's baseline regressions. Data in panels (a) and (b) are from the CRR and Preqin.

Additional Tables

Table E1: HVCRE Regulation and Other County-Level Outcomes

Outcome:	$\log\left(\text{Housing Quantity Measure}_{c,t}\right)$		Rent Growth Measure$_{c,t}$	
Measure:	Construction	Homeless Persons	Average Rent	Quality Premium
	(1)	(2)	(3)	(4)
Bank Share$_c$ × Post$_t$	-0.719*	0.830**	0.029*	-0.186*
	(0.370)	(0.334)	(0.016)	(0.109)
Year FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
R-squared	0.667	0.926	0.147	0.184
Number of Observations	3159	3159	3151	685

Note: Subscripts c and t denote county and year. This table estimates a variant of equation (6). The specification is the similar to column 1 of Table 3 with different outcome variables. The outcome in columns 1-2 is the log of a measure related to the quantity of housing: Construction is the number of multifamily construction projects financed; and Homeless Persons is the overall number of homeless persons. The outcome in columns 3-4 is the one-year change in the log of a measure of rent: Average Rent is the average rent among multifamily units; and Quality Premium is the ratio of average rent among multifamily units in the top quality segment to units in the remaining segments, where quality segment is measured using the MBA/CREFC property inspection rating. Observations are county-years weighted by the average number of multifamily units over 2011-16. The sample period is 2011-16. Standard errors clustered by county are in parentheses. Data on new construction is from Trepp and, for observations where no new construction is observed, the number of new multifamily building permits from the Census' Building Permits Survey. Data on homelessness are from HUD's Point-in-Time Survey. The remaining data are from Trepp.

Table E2: Price of Improvement Loans by HVCRE-Affected Lenders

Outcome:	Interest Rate$_{\ell,t}$	ARM Margin$_{\ell,t}$
Bank$_\ell$ × Post$_t$	-0.140*	-0.141**
	(0.081)	(0.068)
Lender FE	Yes	Yes
Year FE	Yes	Yes
R-squared	0.834	0.674
Number of Observations	424	424

Note: Subscripts ℓ and t denote lender and year. This table estimates a variant of equation (4). The specification is similar to column 3 of Table 1 with different outcome variables. Interest Rate$_{\ell,t}$ and ARM Margin$_{\ell,t}$ are the principal-weighted average interest rate and adjustable-rate mortgage (ARM) margin on improvement loans originated by ℓ as of t. Observations are lender-years weighted by the lender's multifamily mortgage market share over 2011-16. The sample period is 2011-16. Standard errors clustered by lender are in parentheses. Data are from Trepp.

Table E3: Measuring HVCRE Exposure with the Office Sector

Outcome:	$\log\left(\text{Renovated Properties}_{c,t}\right)$	
	(1)	(2)
Bank Exposure$_c$ × Post$_t$	0.152*	0.335**
	(0.081)	(0.167)
Exposure Sector	Office	Office
Base Period	2010	2001-09
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. This table estimates a variant of equation (6). The specification is 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 2001-09. Observations are county-years weighted by the average number of multifamily units over 2011-16. The sample period is 2011-16. Standard errors clustered by county are in parentheses. Data are from Trepp.

Table E5: Loans to Developers and HVCRE Regulation

Outcome:	New Loan$_{b,\ell,t}$	
	(1)	(2)
Developer$_b$ × Post$_t$ × CCAR$_\ell$	-0.026**	-0.028**
	(0.008)	(0.009)
Lender-Borrower FE	Yes	Yes
Lender-Year FE	Yes	Yes
Borrower-Year FE	Yes	Yes
Sample	All	No Bond
R-squared	0.439	0.452
Number of Observations	42120	15990

Note: Subscripts b, ℓ and t denote borrower, lender, and year. This table estimates equation (B1). 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 2012-16. Pairs are weighted by the lender's loan issuance over this period. The second column drops REITs with access to the bond market over 2012-16. The sample period is 2012-16. Standard errors twoway clustered by borrower and lender are in parentheses. Data are from DealScan.

Table E4: Robustness to Heterogeneous Time Trends

Outcome	$\log\left(\text{Renovated Properties}_{c,t}\right)$				
	(1)	(2)	(3)	(4)	(5)
Bank Share$_c$ × Post$_t$	0.329**	0.285**	0.303**	0.247**	0.283**
	(0.113)	(0.100)	(0.102)	(0.097)	(0.125)
Characteristic$_c$ × Year-2012$_t$	0.061	-0.155**	-0.062*	-0.013	-0.101*
	(0.037)	(0.070)	(0.035)	(0.020)	(0.058)
Characteristic$_c$ × Year-2013$_t$	0.098**	-0.142**	-0.080*	-0.003	-0.161**
	(0.031)	(0.071)	(0.045)	(0.029)	(0.062)
Characteristic$_c$ × Year-2014$_t$	0.172**	-0.426**	-0.206**	-0.016	-0.272**
	(0.074)	(0.122)	(0.073)	(0.050)	(0.111)
Characteristic$_c$ × Year-2015$_t$	0.139**	-0.328**	-0.139**	-0.034	-0.170*
	(0.056)	(0.089)	(0.055)	(0.036)	(0.087)
Characteristic$_c$ × Year-2016$_t$	0.172**	-0.545**	-0.210**	-0.040	-0.231*
	(0.083)	(0.124)	(0.085)	(0.070)	(0.131)
Characteristic	Income	Winter Storms	White Share	College Education	Saiz Elasticity
Year FE	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes	Yes	Yes
R-squared	0.726	0.761	0.722	0.706	0.728
Number of Observations	3159	3159	3159	3159	2631

Note: Subscripts c and t denote county and year. This table estimates a variant of equation (6). The specification is the 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 2011-16; Winter Storms is number of winter storms per multifamily housing unit averaged over 2011-16; 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 2011-16. The sample period is 2011-16. Standard errors clustered by county are in parentheses. Data are from Trepp.

Table E6: Quality Improvements after Loan Renewal

Outcome:	$\Delta \log\left(\text{Quality}_{i,\ell,t+1}\right)$		
	(1)	(2)	(3)
$\text{Due}_{i,t}$	0.074**		
	(0.025)		
$\text{Bank}_\ell \times \text{Post}_t \times \text{Due}_{i,t}$		0.129**	
		(0.048)	
$\log\left(\text{Balance}_{i,t}\right)$			0.300**
			(0.113)
Property-Lender-FE	Yes	Yes	Yes
Zip Code-Year FE	Yes	Yes	Yes
Lender-Year FE	Yes	Yes	Yes
Due-Lender FE	No	Yes	Yes
Due-Year FE	No	Yes	Yes
Estimator	OLS	OLS	IV
R-squared	0.496	0.497	0.482
F Statistic			19.930
Number of Observations	143530	143530	143530

Note: Subscripts i, ℓ, and t denote property, lender, and year. Column 1 estimates equation (B2) and columns 2-3 estimate equation (B3). $\text{Due}_{i,t}$ indicates if the property owner's loan is due in year t. Bank_ℓ denotes if lender ℓ is a bank. Post_t indicates if t is greater than or equal to 2015. $\text{Balance}_{i,t}$ is the end-of-period loan balance. The outcome $\Delta \log\left(\text{Quality}_{i,\ell,t+1}\right)$ is the one-year change in log quality, measured with the MBA/CREFC rating. The variables $\Delta \log\left(\text{Quality}_{i,\ell,t+1}\right)$ and $\log\left(\text{Balance}_{i,t}\right)$ are normalized to have unit variance. The estimator is OLS except for column 3, where $\log\left(\text{Balance}_{i,t}\right)$ is instrumented with the triple interaction between Bank_ℓ, $\text{Due}_{i,t}$, 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 2010-16. Standard errors twoway clustered by property and year are in parentheses. Data are from Trepp.

Table E7: Securitization Speed by Loan Purpose

Outcome:	Sec in 3 $\text{Months}_{k,\ell,t}$
$\text{Bank}_\ell \times \text{Post}_t \times \text{Imp}_k$	-0.445**
	(0.156)
Lender-Purpose FE	Yes
Lender-Year FE	Yes
Purpose-Year FE	Yes
R-squared	0.725
Number of Observations	366

Note: Subscripts k, ℓ, and t denote loan purpose, lender, and year. This table estimates a variant of equation (3). The specification is similar to column 1 of Table 1 except that the outcome differs. Sec in 3 $\text{Months}_{k,\ell,t}$ is the principal-weighted share of loans for purpose k securitized within 3 months of origination. Observations are purpose-lender-years weighted by the lender's multifamily mortgage market share over 2011-16. The sample period is 2011-16. Standard errors clustered by lender are in parentheses. Data are from Trepp.

Table E8: County-Level Non-Construction Lending

Outcome:	$\log(\text{Non-Dev Loans}_{c,t})$ (1)	(2)
$\text{Bank Share}_c \times \text{Post}_t$	0.588	0.312^{**}
	(1.154)	(0.099)
Loan Data	Portfolio	Baseline
Year FE	Yes	Yes
County FE	Yes	Yes
County Controls	Yes	Yes
State-Year FE	No	No
R-squared	0.910	0.884
Number of Observations	36	3169

Note: Subscripts c and t denote county and year. This table estimates a variant of equation (6). The specification is similar to column 1 of Table 6 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 $\text{Loans}_{c,t}$ 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 2011-16. The sample period is 2011-16. 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.

Table E9: Public Pension Investment in Value Added by Yield Measure

Outcome:	$\text{Prob of Commitment}^{\text{VA}}_{p,t}$ (1)	(2)	(3)
$\text{Funding Gap}_p \times \text{Yield Gap}_t$	0.141^{**}	0.183^{**}	0.171^{**}
	(0.059)	(0.062)	(0.057)
Yield Measure	Treasury	Corp	TIPS
Pension FE	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes
R-squared	0.712	0.715	0.715
Number of Observations	520	520	520

Note: Subscripts p and t denote pension fund and year. This table estimates equation (9). 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. Observations are public pension-years weighted by average assets over 2009-16. The sample period is 2009-16. Standard errors clustered by pension are in parentheses. Data are from Preqin and the CRR.

Table E10: Safe Real Estate Investments and Pension Risk Taking

Outcome:	Prob of Commitment$^{\text{Core}}_{p,t}$		
	(1)	(2)	(3)
Funding Gap$_p$ × Yield Gap$_t$	-0.130*	-0.142*	-0.031
	(0.069)	(0.073)	(0.079)
Yield Measure	Treasury	Corp	TIPS
Pension FE	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes
R-squared	0.633	0.634	0.629
Number of Observations	520	520	520

Note: Subscripts p and t denote pension fund and year. This table estimates a variant of equation (9). The specification is similar to column 1 of Table E9 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. Observations are public pension-years weighted by average assets over 2009-16. The sample period is 2009-16. Standard errors clustered by pension are in parentheses. Data are from Preqin and the CRR.

Table E11: Riskiest Real Estate Investments and Pension Risk Taking

Outcome:	Prob of Commitment$^{\text{Opp}}_{p,t}$		
	(1)	(2)	(3)
Funding Gap$_p$ × Yield Gap$_t$	0.132**	0.171**	0.145**
	(0.059)	(0.067)	(0.054)
Yield Measure	Treasury	Corp	TIPS
Pension FE	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes
R-squared	0.741	0.743	0.742
Number of Observations	520	520	520

Note: Subscripts p and t denote pension fund and year. This table estimates a variant of equation (9). The specification is similar to column 1 of Table E9 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. Observations are public pension-years weighted by average assets over 2009-16. The sample period is 2009-16. Standard errors clustered by pension are in parentheses. Data are from Preqin and the CRR.

Table E12: Unconventional Monetary Policy Surprises

Date	Time	Source	Program	Effect (bps)
12/1/2008	1:45pm	Bernanke speech	QE1	-30.1
12/16/2008	2:21pm	FOMC statement	QE1	-11.8
1/28/2009	2:15pm	FOMC statement	QE1	26.5
3/18/2009	2:17pm	FOMC statement	QE1	-32.9
9/23/2009	2:16pm	FOMC statement	QE1	-7.4
8/10/2010	2:14pm	FOMC statement	QE2	-7.6
9/21/2010	2:14pm	FOMC statement	QE2	-8.8
8/9/2011	2:18pm	FOMC statement	FG	-15.2
1/25/2012	12:28pm	FOMC statement	FG	-14.8
9/13/2012	12:31pm	FOMC statement	QE3	2.2
5/22/2013	10:30am	Bernanke testimony	QE3	7.3
6/19/2013	2:00pm	FOMC speech	QE3	23.7
7/10/2013	4:45pm	Bernanke speech	QE3	-10.4
9/18/2013	2:00pm	FOMC statement	QE3	-11.7
11/20/2013	2:00pm	FOMC statement	QE3	1.6
12/18/2013	2:00pm	FOMC statement	QE3	11.3
3/19/2014	2:00pm	FOMC statement	FG	15.4
9/17/2014	2:00pm	FOMC statement	QE3	6.1
10/29/2014	2:00pm	FOMC statement	QE3	4.7
12/17/2014	2:00pm	FOMC statement	FG	14.3
3/18/2015	2:00pm	FOMC statement	FG	-8.6
7/29/2015	2:00pm	FOMC statement	FG	2.2
10/28/2015	2:00pm	FOMC statement	FG	16.4
12/16/2015	2:00pm	FOMC statement	FG	0.8

Note: This table lists the unconventional monetary policy surprises used in Table E13. The surprises prior to 11/20/13 are from Chodorow-Reich (2014a). The remaining surprises come from the Fed's Timelines of Policy Actions and Communications. The set of policy programs are the three rounds of quantitative easing (QE1, QE2, QE3) and forward guidance (FG). Column 5 lists the change in the 5-year Treasury yield from the day before the surprise to the day after it, based on the CRSP 5-Year Noncallable Treasury Note Index, in basis points (bps).

Table E13: Unconventional Monetary Policy and Short-Term Fluctuations in the Yield Gap

Outcome:	Prob of $\text{Commitment}^{VA}_{p,t}$	
	(1)	(2)
Funding Gap_p × Yield Gap_t	0.118*	0.446**
	(0.061)	(0.163)
Funding Gap_p × Time-Trend_t	0.077	
	(0.049)	
Estimator	OLS	IV
Pension FE	Yes	Yes
State-Year FE	Yes	Yes
R-squared	0.716	0.697
Number of Observations	520	520

Note: Subscripts p and t denote pension fund and year. This table estimates a variant of equation (9). The specification is similar to column 1 of Table 5. Column 1 includes the interaction between Funding Gap_p and a linear time trend. Column 2 instruments for Funding Gap_p × Yield Gap_t using the product between Funding Gap_p and the cumulative change in safe yields in year t attributable to unconventional monetary policy surprises in t. The set of surprises and their effects are listed in Table E12. Observations are public pension-years weighted by average assets over 2009-16. The sample period is 2009-16. Standard errors clustered by pension are in parentheses. Data are from Preqin and the CRR.

Table E14: Placebo Test of Public Pension Investment, 2003-07

Outcome:	Prob of $\text{Commitment}^{VA}_{p,t}$
Funding $\text{Gap}_{p,02}$ × Yield Gap_t	-0.046
	(0.047)
Pension FE	Yes
State-Year FE	Yes
R-squared	0.672
Number of Observations	270

Note: Subscripts p and t denote pension fund and year. This table estimates a variant of equation (9). The specification is similar to column 1 of Table 5 Funding $\text{Gap}_{p,02}$ 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 2007 and in t. The change in this yield over 2003-07 was +0.22 pps. Observations are public pension-years weighted by average assets over 2009-16. The sample period is 2003-07. Standard errors clustered by pension are in parentheses. Data are from Preqin and the CRR.

Table E15: Public Pension Risk Taking and GASB Changes

Outcome:	Prob of Commitment$^{VA}_{p,t}$
Funding Gap$_p$ × Yield Gap$_t$	0.118**
	(0.056)
GASB Change-Year FE	Yes
Pension FE	Yes
State-Year FE	Yes
R-squared	0.751
Number of Observations	520

Note: Subscripts p and t denote pension fund and year. This table estimates a variant of equation (9). 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. Observations are public pension-years weighted by average assets over 2009-16. The sample period is 2009-16. Standard errors clustered by pension are in parentheses. Data are from Preqin and the CRR.

Table E16: Robustness of Public Pension Risk Taking to Distressed Debt

Outcome:	Prob of Commitment$^{DD}_{p,t}$	
	(1)	(2)
Funding Gap$_p$ × Yield Gap$_t$	0.143*	0.134*
	(0.081)	(0.079)
Pension FE	Yes	Yes
State-Year FE	Yes	Yes
Pension Controls	No	Yes
R-squared	0.705	0.714
Number of Observations	343	343

Note: Subscripts p and t denote pension fund and year. This table estimates a variant of equation (9). The specification is similar to column 1 of Table E9 except that the outcome differs. Prob of Commitment$^{DD}_{p,t}$ indicates an investment in a private distressed debt fund, excluding real estate debt. Observations are public pension-years weighted by average assets over 2009-16. The sample period is 2009-16. Standard errors clustered by pension are in parentheses. Data are from Preqin and the CRR.

Table E17: Value Added Investment with Manager-Year Fixed Effects

Outcome:	Fund Formed$_{m,k,t}$
Funding Gap$_m$ $\times$ Yield Gap$_t$ $\times$ VA$_k$	0.175**
	(0.050)
Manager-Year FE	Yes
Strategy-Year FE	Yes
Manager-Strategy FE	Yes
R-squared	0.680
Number of Observations	1472

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 2009-16. The sample period is 2009-16. Standard errors twoway clustered by manager and year are in parentheses. Data are from Preqin.

Table E18: Rent Growth and New Features

Outcome:	$\Delta \log(\text{Rent}_{i,t})$
Installment of:	
Dishwasher$_{i,t}$	0.118**
	(0.022)
Washing Machine$_{i,t}$	0.097**
	(0.026)
Disposal$_{i,t}$	0.031
	(0.020)
Trash Compactor$_{i,t}$	0.013
	(0.040)
Central A/C$_{i,t}$	0.023
	(0.021)
A/C$_{i,t}$	0.063**
	(0.015)
Dryer$_{i,t}$	-0.007
	(0.027)
$\log(\text{Square Feet}_{i,t})$	0.121**
	(0.048)
Property FE	Yes
Year FE	Yes
R-squared	0.065
Number of Observations	76148

Note: This table estimates equation (11). 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 $\log(\text{Square Feet}_{i,t})$ 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.

Table E19: Relationships in Multifamily Mortgage Lending

Outcome:	Loan Originated$_{b,\ell,s}$ (1)	(2)
Loan Originated$_{b,\ell,s-1}$	0.522**	0.651**
	(0.028)	(0.046)
Loan Originated$_{b,\ell,s-1}$ $\times \log(\text{Properties}_b)$		-0.068**
		(0.024)
$\log(\text{Properties}_b)$		0.001**
		(0.000)
Lender-Year FE	Yes	Yes
R-squared	0.307	0.312
Number of Observations	77316	77316

Note: Subscripts b, ℓ and s denote borrower, lender, and sequence of loan issued over 2012-16. This table estimates equation (D1). Loan Originated$_{b,\ell,s}$ indicates if a loan was originated. The pairs (b, ℓ) span each possible pair among active borrowers and lenders over 2012-16. Properties$_b$ is the number of properties owned by b over the sample period. The sample period is 2012-16. Standard errors clustered by borrower are in parentheses. Data are from Trepp.

Table E20: Relationships between Pensions and Private Equity Real Estate Fund Managers

Outcome:	Investment$_{p,m,s}$ (1)	(2)
Investment$_{p,m,s-1}$	0.224**	0.957**
	(0.045)	(0.250)
Investment$_{p,m,s-1}$ $\times \log(\text{Size}_m)$		-0.089**
		(0.028)
$\log(\text{Size}_m)$		0.005**
		(0.001)
Pension-Year FE	Yes	Yes
R-squared	0.093	0.103
Number of Observations	18060	18060

Note: Subscripts p, m and s public pension, private equity real estate fund manager, and sequence of private equity real estate fund formed over 2008-16. This table estimates equation (D2). Investment$_{p,m,s}$ indicates if an investment was made. The pairs (p, m) span each possible pair among active pensions and managers over 2008-16. Size$_m$ is dollar value of private equity real estate funds closed over 2008-16. The sample period is 2008-16. Standard errors clustered by manager are in parentheses. Data are from Preqin.

Table E21: Relationships in REIT Bond Underwriting

Outcome:	Lead Underwriter$_{j,u,s}$		Underwriter$_{j,u,s}$	
	(1)	(2)	(3)	(4)
Lead Underwriter$_{j,u,s-1}$	0.224**	0.181**	-0.031	-0.029
	(0.058)	(0.058)	(0.057)	(0.055)
Underwriter$_{j,u,s-1}$	0.016	0.009	0.288**	0.233**
	(0.020)	(0.021)	(0.030)	(0.032)
Underwriter-Year FE	Yes	Yes	Yes	Yes
Underwriter-Sector FE	No	Yes	No	Yes
R-squared	0.271	0.319	0.344	0.389
Number of Observations	49268	49268	49268	49268

Note: Subscripts j, u and s denote bond issuer (i.e. REIT), underwriter, and sequence of bond issue over 2000-17. This table estimates equation (D3). Underwriter$_{j,u,s}$ indicates if firm u was an underwriter of issue s for issuer j. Lead underwriter$_{j,u,s}$ indicates if u was the lead underwriter. The pairs of issuers and underwriters span each possible pair among active institutions over 2000-17. The sample period is 2000-17. Standard errors clustered by issuer are in parentheses. Data are from NAREIT.

Table E22: MSA Correlates with Improvement Activity

Outcome:	Renovation Probability$_m$
$\log(\text{Saiz Elasticity}_m)$	-0.120*
	(0.070)
$\log(\text{Income}_m)$	0.108*
	(0.056)
Rent Control$_m$	-0.071**
	(0.029)
College Education$_m$	-0.044
	(0.075)
R-squared	0.048
Number of Observations	211

Note: Subscript m denotes MSA. Renovation Probability$_m$ is the share of multifamily units that were renovated between 2010 and 2016. Saiz Elasticity$_m$ is the elasticity of housing supply as estimated by Saiz (2010). Income$_m$ is average real income per capita over 2010-16. College Education$_m$ is the share of inhabitants with a bachelor's degree in 2010. Rent control$_m$ indicates if the MSA has rent control or stabilization policies. All variables are normalized to have unit variance. Observations are MSAs weighted by number of multifamily units over 2010-16. Heteroskedasticity robust standard errors are in parentheses. Data are from Trepp and other data sources described in Appendix A.