



Essays on Mortgage and Housing Markets

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Essays on Mortgage and Housing Markets

A dissertation presented

by

Michael Reher

to

The Department of Economics

in partial fulfillment of the requirements

for the degree of

Doctor of Philosophy

in the subject of

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Abstract

This dissertation consists of three essays that investigate the relationship between financial intermediaries, households, and real activity. In the first chapter, Pedro Gete and I show how reductions in the supply of mortgage credit increase rent growth and lower homeownership rates. We identify the effect using geographic variation in exposure to a regulation-induced contraction in bank lending. The second chapter, also joint with Pedro Gete, shows how a regulation-induced increase in the liquidity of mortgage-backed securities lowers shadow banks' lending standards and thus raises their market share. In the third chapter, I show that greater supply of financing for residential improvement projects has increased rental housing quality during a period when quality accounts for a significant share of real rent growth. I arrive at this finding by studying two natural experiments that increase the supply of bank credit and private equity financing for residential improvement projects. A common conclusion throughout these three essays is that financial intermediaries affect households and real activity through the market for real estate financing.

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Preface

One of the most central questions in financial economics is how financial intermediaries, households, and real activity are related. I investigate this relationship in three essays, throughout which two common themes appear: the effect of financial regulation on intermediaries' investment behavior; and households' choice of renting or owning their home.

In the first chapter, "Mortgage Supply and Housing Rents", Pedro Gete and I show how a contraction of mortgage credit supply by lenders affected by post-Recession financial regulations has increased rent growth and lowered homeownership rates. This chapter shows how intermediaries affect real activity through households' choice of renting or owning their home: households who are denied mortgage credit live in the rental sector, which increases equilibrium rent and the supply of rental housing.

The next chapter, "Market Liquidity and Shadow Bank Lending", also joint with Pedro Gete, studies how mortgage-backed securities' liquidity affects the market share of shadow banks in the primary mortgage market. We find that a regulation-induced increase in liquidity lowers shadow banks' lending standards and thus raises their market share. While less-focused on real activity, this chapter shows how the distribution of market share across lenders affects households' ability to access mortgage credit.

In the final chapter, "Financial Intermediaries as Suppliers of Housing Quality", I show that greater supply of financing for residential improvement projects has increased rental housing quality during a period when quality accounts for a majority of real rent growth. This chapter illustrates how intermediaries can affect the types of real residential projects that are performed, which in turn affects households through the supply of housing quality.

As in the previous chapters, regulation is the root cause of the shift in the supply of financing.

Collectively, these essays illustrate how financial intermediaries' behavior in the market for real estate financing affects both households and real activity. Future research might investigate this relationship in the context of other alternative asset classes, such as non-real estate private equity, or how financial innovation, like regulation, can affect intermediaries' behavior.

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Two-thirds of this dissertation represents joint work with Professor Pedro Gete. As a co-author, I am grateful to Pedro for his dedication to our shared venture, and, as a friend, I am grateful for his thoughtful advice.

Finally, I would like to thank my parents, who ceaselessly willed my good. Their loving encouragement and engagement were one of my most powerful resources, and for them I dedicate this dissertation.

For my parents

Chapter 1

Mortgage Supply and Housing Rents ¹

1.1 Introduction

This paper shows that a contraction of mortgage credit supply has been a significant driver of housing rents and homeownership since the 2008 crisis. Following the crisis, homeownership rates collapsed to historic lows while housing rents increased rapidly in many U.S. cities. For example, real rents grew by more than 23% in the top 10% of fastest growing MSAs over the 2011-2014 period. During these years, the median U.S. rent-to-income ratio increased by more than in the previous 35 years. The large number of cost-burdened renters has prompted policy debates about what to do (Fernald et al. 2015).

The mechanism that we test was originally proposed by Linneman and Wachter (1989) and is formalized by Gete and Reher (2016).² It begins with a shock that contracts mortgage

¹This chapter represents joint work with Pedro Gete.

²Ambrose and Diop (2014) and Acolin et al. (2016) provide empirical support using different periods and identification strategies.

supply for some lenders such as, for example, greater regulatory costs because of stress-testing. Then, frictions to substitute across lenders lead to more difficult access to credit. Since downward house price rigidities prevent most households from buying without credit, households denied credit move from the market for homeownership to the rental market. An increase in the demand for rental housing, together with an imperfect short-run elasticity of supply, drives up housing rents and reduces homeownership rates. Lower price-to-rent ratios encourage investors to buy owner occupied units and convert them to rentals.

Our identification strategy exploits heterogeneity across MSAs in exposure to lenders which suffered regulatory shocks following the Dodd-Frank Act, approved in 2010. We ask whether MSAs with greater exposure to these credit supply shocks experienced higher rent growth. The challenge for our identification is to isolate credit supply shocks from other shocks that drive both housing rents and mortgage denial rates, our measure of mortgage supply. For example, an OLS regression of mortgage denial rates on housing rents would be biased if a negative shock to local activity results in credit stringency, while also dampening rent growth through reduced amenities.

We use an instrumental variables approach to surmount the previous challenge. Our preferred instrument is the 2008 mortgage application share of lenders which underwent a capital stress test between 2011-2015. Since the bank distribution that we use was determined prior to Dodd-Frank, there is no risk of reverse causality. Calem, Correa, and Lee (2016) document that stress tests are associated with tightened standards in mortgage markets. We also explore as a second instrument MSA exposure to the Big-4 banks using a pre-determined measure of bank distribution across markets, the branch deposit share in 2008 from the

FDIC's Summary of Deposits.³ Stein (2014) discusses how Dodd-Frank has exposed the Big-4 banks to heightened oversight and higher liquidity and capital requirements. Jayaratne and Strahan (1996) document the importance of bank branches in facilitating access to credit, and since their seminal work a number of papers have exploited bank branch distributions to create credit supply instruments (e.g. Nguyen 2016). Finally, we explore as a third instrument the share of top 20 lenders active in 2007. D'Acunto and Rossi (2017) use this instrument to study a regressive redistribution of mortgage credit between 2011 and 2014 stemming from post-crisis financial regulation.

We rigorously assess the validity of the instruments. First, we control thoroughly for an array of local activity shocks, pre-crisis trends and borrower and lender characteristics, making it unlikely that the error term reflects common movers of both mortgage supply and rents. Second, we provide extensive evidence that in the pre-Dodd-Frank period the instruments do not correlate with either higher rents or with other factors that cause rent growth. For example, before 2010 there are parallel patterns between MSAs with the highest and lowest exposure to the Big-4 and stress tested lenders. Third, placebo tests confirm that the instruments only capture post-crisis credit supply shocks. Fourth, overidentification tests are supportive of the instruments' validity. This suggests that we are identifying similar credit supply effects with different underlying variation.

All the specifications point in the same direction: tighter credit caused higher housing rents over 2010-2014. Our baseline specification suggests that a 1 percentage point increase in denial rates increased rent growth by 1.3 percentage points. To put this estimate into

³The Big-4 banks are Bank of America, Citigroup, JP Morgan Chase, and Wells Fargo.

perspective it is useful to look how denial rates changed over 2010-2014. Over this period average denial rates fell by 1.6 percentage points relative to their 2008 levels. However, denial rates actually rose in 31% of MSAs. Our estimates indicate that rents would have grown at least 2.1 percentage points less in these MSAs if their denial rates had moved with the national average. This effect is equal to 70% of a cross-sectional standard deviation in 2010-2014 rent growth. Thus, elevated post-crisis credit stringency explains a meaningful amount of cross-MSA variation in recent rent behavior.

Consistent with the theory, the credit shock captured by our instruments lowered price-to-rent ratios and had a non-positive effect on housing prices. The effect is more negative for starter homes, which are more likely priced by constrained buyers. In MSAs more exposed to the credit supply shock, the correlation between prices and rents is negative, and especially so where more households face binding borrowing constraints, proxied by a higher minority share. The credit shock encouraged the conversion of owner occupied units to rentals and lowered the homeownership rate.

The previous results are not only supportive of our theory, but they also provide more evidence to rule out the possibility that unobserved housing demand shocks violate the exclusion restriction. If that were the case and the MSAs more exposed to our credit instruments also experienced positive demand shocks, then we might observe not only a positive and significant relationship between instrumented denials and rents but also between instrumented denials and prices. This is because demand shocks can generate comovement between prices and rents as shown in Gete and Reher (2016) and Gete and Zecchetto (2017) among oth-

ers.⁴ However, we find no evidence to support this concern. House price dynamics strongly suggest that our results are due to a credit supply contraction operating through a tenure choice channel.

The instruments' inability to explain housing rents in a placebo exercise suggests that they are valid post-crisis credit supply shocks, not that the theory is invalid in the pre-crisis period. To investigate whether credit affects rents in other periods, we use the Loutskina and Strahan (2015) instrument which the literature has accepted as a valid credit supply shock. Interestingly, there is a positive and statistically significant effect of credit supply on rents over the pre-crisis period.⁵ We interpret this result, together with the placebo exercise, as further evidence of the instruments' validity.

As a complement to the core cross-sectional analysis, we also employ a panel identification strategy that exploits within-MSA variation following various techniques in the literature. The results are qualitatively and quantitatively similar to the baseline cross-sectional study. The placebo tests are reassuring because post-crisis shocks do not explain pre-crisis rent growth. Moreover, the panel analysis shows that the divergence in lending standards between Big-4 and non-Big-4 banks, and between stress-tested and non-stress-tested lenders, is a post-2010 phenomenon.

Thus, collectively, the paper uses a broad array of empirical methodologies which suggest the same result: a contraction of mortgage supply after the Great Recession caused higher

⁴It is also possible for demand shocks to generate no comovement if households are constrained, but we check that this does not drive our results by extensively controlling for local business cycles. We thank an anonymous referee for pointing out this alternative possibility.

⁵The magnitude is much smaller than we found over the post-crisis period, likely because variation in lending standards over the pre-crisis period was much smaller.

housing rents. This result does not rule out alternative explanations for rent growth, but instead highlights the importance of the credit contraction theory after rigorously accounting for these other explanations.

In terms of contribution to the literature, to the best of our knowledge this is the first paper to study the role of credit supply in the dynamics of post-crisis housing rents.⁶ The existing literature on housing rents has thus far focused on other, non-credit drivers like population flows (Saiz 2007), shrinking leisure of high-income households (Edlund, Machado and Sviatchi 2015), income growth (Hornbeck and Moretti 2015, or Muehlenbachs, Spiller and Timmins, 2015) or households' expected duration of stay in a house (Halket and Pignatti 2015). Mezza et al. (2016) show that student debt has affected the demand for homeownership.

In terms of empirical strategy, our paper complements Chen, Hanson and Stein (2017), D'Acunto and Rossi (2017) and Goodman (2017). Chen, Hanson and Stein (2017) show that a credit supply shock experienced by the Big-4 banks led to a contraction of small business credit and caused higher unemployment. Their identification strategy is similar to our use of a Big-4 instrument, and we control carefully for the factors they highlight, like establishment creation, to alleviate concerns that local economic conditions are driving the results. D'Acunto and Rossi (2017) document that U.S. financial institutions have reduced mortgage lending for medium-sized loans and increased lending for large loans since the crisis. They conclude that this resulted from a supply-side change, namely the increase in the costs

⁶There is a large literature that analyzes whether easy access to credit caused the pre-crisis increase in house prices. See, for example, Albanesi, DeGiorgi and Nosal (2016), Anenberg et al. (2017), Adelino, Schoar and Severino (2016), Ben-David (2011), DiMaggio and Kermani (2017), Driscoll, Kay and Vojtech (2016), Favara and Imbs (2015), Foote, Loewenstein and Willen (2016), Glaeser, Gottlieb and Gyourko (2012), or Mian and Sufi (2009) among others.

of originating mortgages imposed by Dodd-Frank. We show that our results hold if we use their instrumental variable to capture the effect of a contraction of credit on housing rents. Goodman (2017) documents that mortgage credit has become very tight in the aftermath of the financial crisis and discusses potential regulatory causes of this contraction.

There is an ongoing debate on what caused the crisis and what the appropriate policy responses are. Mian and Sufi (2009) provide evidence pointing to excessive credit supply towards low-income households as the cause of the crisis. Adelino, Schoar and Severino (2016) or Foote, Loewenstein and Willen (2016) argue that loans to low-income households were not the dominant driver of pre-crisis credit flows, and thus policies should not necessarily aim to restrict credit accessibility for these borrowers. Our results show that policy reforms have especially reduced the flow of credit towards households on the margin of homeownership and caused higher housing rents. However, these increases should be transitory since we also show an increase in rental supply. From a welfare perspective, it is not clear whether the decrease in homeownership is good or bad. For example, we document that pre-financial crisis lending standards were exceptionally low. The standards have tightened since the crisis, perhaps overshooting the pre-boom conditions.

The rest of the paper is organized as follows: Section 1.2 discusses the underlying theory; Section 1.3 has our baseline analysis; Section 1.4 provides multiple tests to assess the exclusion restriction; Section 1.5 studies house prices, homeownership and rental supply; and Section 1.6 concludes. The appendix explains our data sources, and the online appendix contains additional results and details on the panel methodology.

1.2 Motivation and Theory

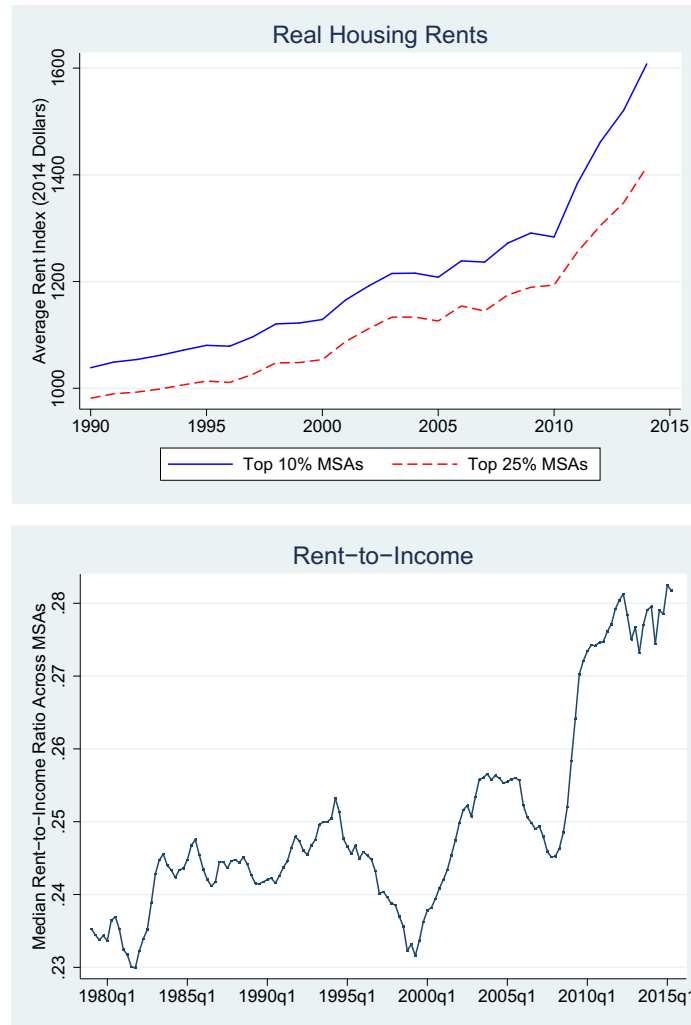
In this section we describe the theory that we want to test. As Figure 1 shows, following the recent financial crisis, housing rents have increased steeply in many MSAs. The rent-to-income ratio for the median MSA has risen by more following the Great Recession than it did over the previous 25 years combined. At the same time, the U.S. homeownership rate has collapsed to historic lows.⁷

These previous facts suggest an important role for the extensive margin of rental demand, which is analyzed theoretically in Gete and Reher (2016) and Gete and Zecchetto (2017). Here we briefly sketch the main mechanisms that we will test later in the paper. Households can decide to buy or to rent. Thus there are two housing stocks: one for owner occupied units and another for rentals. The rental stock is owned by the wealthy households (e.g. landlords or investors). Since houses are large and indivisible goods, their purchase requires mortgage credit for all except for the wealthiest households. Households decide their tenure choice by comparing the utility from rental versus owner occupied housing, the price-to-rent ratio, and the cost and availability of mortgage credit. Mortgage lenders set their lending standards such that lenders' expected revenue, after taking into the account the possibility of default, equals their cost of funds.

Higher costs for the lender, for example, because of higher capital requirements or the costs associated with stress-testing, shift the credit supply curve inward. Consequently, more households are denied credit at pre-shock conditions. Tighter lending standards make some households unable to borrow at the conditions they want and, given downward rigidities

⁷In the second quarter of 2016, the homeownership rate fell to 62.9%, its lowest level since 1965.

Figure 1.1: Dynamics of Real Housing Rents and Rent-to-Income



Note: The top panel plots real housing rents over the 1991-2014 period in 2014 dollars for MSAs ranking in the top 10% and top 25% of 2008-2014 rent growth, respectively. Nominal rents are measured using the Zillow Rent Index (ZRI), which has the interpretation of dollars per month. The translation to real rents is done using the Consumer Price Index excluding shelter. The bottom panel plots the median ratio of rent-to-income for the MSAs in our sample.

in house prices, they decide to rent. Higher demand for rental housing, together with an inelastic supply and imperfect convertibility between rental and owner-occupied units, lead to higher rents, lower homeownership and lower house prices. As the price-to-rent ratio falls, there are investors who buy owner occupied properties and place them for rent. That is, the tenure conversion rate increases. This "buy to let" behavior then induces a positive correlation between rents and prices. Moreover, new construction further increases the supply of rental housing.

We check that the data support the predictions of the previous theory. Sections 1.3 and 1.4 study housing rents, and Section 1.5 analyzes the remaining implications.

1.3 Mortgage Supply and Rent Growth

This section estimates the effect of credit supply on housing rents. The next section discusses the validity of the instrumental variables that we use to identify credit supply.

1.3.1 Database

We measure credit supply using mortgage denial rates to avoid capturing any effect from borrowers' reaction to a loan offer.⁸ Our data come from the Home Mortgage Disclosure Act (HMDA) which we merge with rent data from the Zillow Rent Index (ZRI) and other controls at the MSA level.⁹ The units of the ZRI are nominal dollars per month for the

⁸Denial rates are strongly correlated with proxies for lending standards. For example, Vojtech, Kay and Driscoll (2016) find that denial rates are closely linked to measures of tightening standards from the Senior Loan Officer Opinion Survey (SLOOS).

⁹Zillow computes this index by imputing a rent for each property in an MSA based on recent rental transactions. It does not impute rent using house prices. Figure A1 in the online appendix shows how the

median property in the MSA. We study MSAs as the unit of analysis, as they are arguably the smallest geographical unit in which households cannot borrow in one location to purchase a house in another one.

To focus on households contemplating whether to rent or own, we only study applications for the purchase of owner-occupied, 1-to-4 family dwellings, which include single-family houses and also individual units within multi-unit buildings, such as condominiums. Table 1.1 contains summary statistics of the key variables in our analysis. A detailed description of all the data sources and cleaning procedures is in the Data Appendix.

1.3.2 Specification

We focus on differences at the MSA level over the 2010-2014 period, since 2010 was the year when Dodd-Frank was approved. Our baseline specification is

$$\text{Avg Rent Growth}_{m,10-14} = \beta \times \text{Avg Denial Rate}_{m,10-14} + \gamma X_m + u_m, \quad (1.1)$$

where m indexes MSAs, $\text{Avg Denial Rate}_{m,10-14}$ denotes the average denial rate over 2010-2014 and $\text{Avg Rent Growth}_{m,10-14}$ denotes average annual rent growth over 2010-2014.¹⁰ The controls in X_m account for both pre-crisis dynamics as well as level effects, including: the 2000-2008 average annual change in log median income, log median rent, log median house

ZRI is quite similar to the St. Louis Fed's rent index, which is available for a selection of MSAs.

¹⁰We use average variables because with persistent but non-permanent credit supply shocks it is inappropriate to estimate (1.1) using growth in denials as the independent variable. This is because, as we show in Figure A2 of the online appendix, our credit supply shocks are strongest in the beginning of the 2010-2014 window. Thus they are positively correlated with average denial rates over this period but, because of mean reversion, negatively correlated with growth in denials.

Table 1.1: Summary Statistics

	Obs	Mean	Std. Dev.	Min	Max
Avg Rent Growth $_{m,10-14}$	302	2.641	3.004	-5.637	19.057
Avg Denial Rate $_{m,10-14}$	303	11.147	3.064	4.236	30.211
Big-4 Deposit Share $_{m,08}$	303	5.048	11.945	0	79.931
CCAR Tested Share $_{m,08}$	303	27.119	12.832	.301	64.338
Avg House Price Growth $_{m,10-14}$	263	1.423	2.887	-5.363	11.391
Avg Starter House Price Growth $_{m,10-14}$	250	2.621	15.197	-27.786	51.94
Price-Rent Ratio Growth $_{m,t}$	264	-60.392	189.174	-1291.651	593.778
Tenure Conversion Rate $_{m,11-13}$	96	4.316	4.349	0	24.041
Avg Homeownership Growth $_{m,10-14}$	64	-.731	1.253	-3.85	1.75
Avg Multi-Family Permits Growth $_{m,11-14}$	280	11.926	50.525	-274.084	317.805
Avg Unemployment Growth $_{m,10-14}$	298	-.82	.554	-3.05	.825
Avg Labor Force Part. Growth $_{m,10-14}$	298	-.316	.517	-2.275	1.325
Avg Establishment Growth $_{m,10-14}$	298	-.372	.722	-1.578	2.852
Avg Real GDP Growth $_{m,10-14}$	298	.415	1.552	-6.378	4.738
Avg Wage Growth $_{m,10-14}$	205	3.073	12.674	-41.114	58.629
Avg Rent Growth $_{m,00-08}$	303	3.218	1.824	-3.344	8.2
Avg House Price Growth $_{m,00-08}$	264	2.688	1.435	-1.866	6.182
Avg Population Growth $_{m,00-08}$	302	11.096	10.807	-2.188	47.806
Avg Income Growth $_{m,00-08}$	302	5.679	1.199	2.428	9.855
Avg Unemployment Growth $_{m,00-08}$	296	.387	.243	-.257	1.257
Avg Age Growth $_{m,00-08}$	296	.242	.661	-3.559	1.683
Financial Services Share $_{m,08}$	299	5.847	1.825	2.001	17.265

Note: This table presents summary statistics of the key variables in our analysis. All variables are at the MSA level. Avg Rent Growth denotes average annual change in log rent. Avg Denial Rate denotes the average denial rate among mortgage applications for the purchase of single-family homes in the MSA, based on HMDA data. Big-4 Deposit Share $_{m,08}$ and CCAR Tested Share $_{m,08}$ are, respectively the branch deposit share of the Big-4 banks in 2008 and the 2008 mortgage application share of lenders which underwent a stress test between 2011-2015. Rent and House Price denote the Zillow Rent and Home Value indices, respectively. Starter house prices are based on Zillow's Bottom Tier Index. Tenure Conversion Rate denotes the fraction of rental units in an MSA that were converted from owner occupied units over the indicated period. Labor Force Part. denotes the labor force participation rate. Establishment refers to the number of establishments. Real GDP is in per capita terms. Wages are the median hourly wage in the MSA. Age and Income refer to the median in the MSA. Multi-Family Permits denotes permits for the construction of multifamily units. Homeownership refers to the homeownership rate in the MSA. Financial Services Share is the fraction of workers in financial services. All variables are in units of percentage points, up to a log approximation. Full details on our data sources and cleaning procedures are in the Data Appendix.

price, log population, log median inhabitant age, and unemployment rate; and the 2009 level of log median income, log median rent, log population, log median inhabitant age, and unemployment rate. We also include state fixed effects in all specifications.

If we estimate (1.1) using OLS, we would obtain biased estimates. This is because local shocks can drive both rent dynamics and mortgage supply. For example, a positive shock to an MSA's economic activity would increase amenities and thus rent growth, while raising households' income, thus reducing mortgage denials. As a result, the OLS estimate would be biased downward. Another possibility is that households rent due to a lack of employment opportunities, so that OLS would produce upward bias.¹¹ Regardless of the direction of the bias, we aim to overcome it by proposing two credit supply instruments for which there is extensive evidence that the exclusion restriction is satisfied.

1.3.3 The Instrumental Variables

We study two instrumental variables that capture an MSA's exposure to lenders facing regulatory risk over the 2010-2014 period, where the exposure is measured with pre-determined variables unrelated to the factors the literature has identified as drivers of housing rents. After describing the instruments, we provide evidence that they are uncorrelated with local shocks but indeed correlated with denial rates.

Our preferred instrument is MSA exposure to lenders subject to a Comprehensive Capital Analysis and Review (CCAR) stress test between 2011 and 2015. These tests are meant to ensure that the largest bank holding companies have enough capital to weather a financial

¹¹We thank an anonymous referee for pointing out this example.

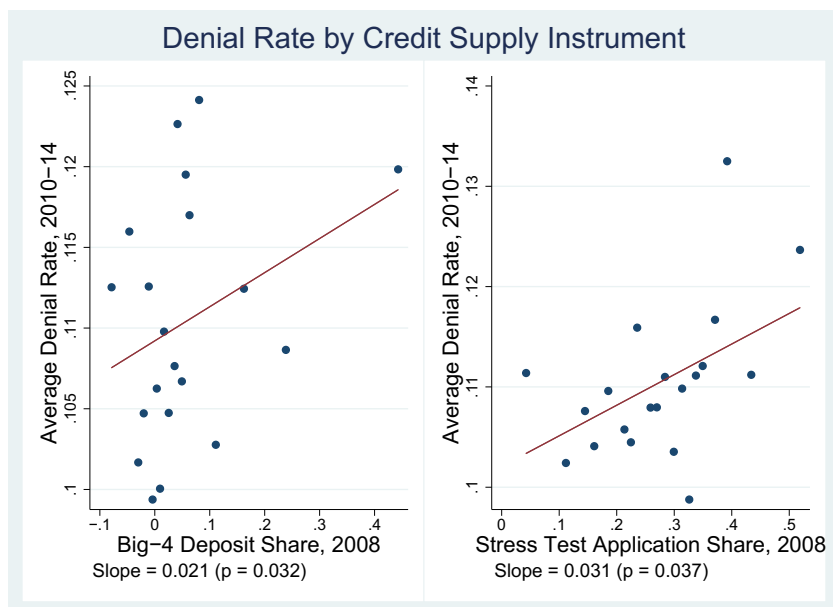
crisis, but as a side-effect they have encouraged those institutions to tighten their standards in mortgage markets (Calem, Correa, and Lee 2016). We measure an MSA's exposure to these lenders using their pre-shock, 2008 mortgage application share. The results are similar if we instead weight by deposit share. We prefer the 2008 application share because several CCAR-tested lenders like Ally conduct their mortgage business through non-depository subsidiaries.

We also employ a second instrument which builds on how the Big-4 banks are the only major mortgage lenders officially designated as systemically-important financial institutions (SIFIs) over 2010-2014. Importantly for the purposes of identification, the SIFI designation is not based on an institution's behavior in mortgage markets. Stein (2014) describes how the Dodd-Frank Act subjected the Big-4 banks to heightened oversight and higher liquidity and capital requirements. As we show formally in the panel analysis of Section 1.4, these lenders have tightened credit significantly relative to other lenders since 2010, and thus differential exposure to these lenders constitutes a credit supply shock. To measure exposure to the Big-4, we compute the Big-4's branch deposit share in an MSA in 2008, using the FDIC's Summary of Deposits. The results are the same if we instead weight by the number of branches.

Our key identification assumption is that, once we control for a broad array of factors and fixed effects, exposure to the Big-4 banks and stress tested lenders is uncorrelated with other drivers of rent growth over 2010-2014. We devote Section 1.4 to discuss multiple tests that all suggest that the instruments satisfy this exogeneity assumption. The second assumption is that both instruments are relevant, that is, correlated with denial rates. Figure 1.2 provides visual support and shows strong correlation between the instruments and average denial

rates over 2010-2014. Moreover, in all our results we test for and reject underidentification.

Figure 1.2: Denial Rates and Credit Supply Instruments



Note: This figure plots denial rates against the cross-sectional credit supply instruments. The plot controls for the same variables as the baseline analysis in Table 1.2.

1.3.4 Baseline Results

Table 1.2 contains the estimates of the baseline specification (1.1). In the first column we estimate (1.1) using OLS, finding a positive but statistically insignificant point estimate. However, after accounting for the endogeneity of denial rates in the second column of the table, the instrumental variables estimate suggests an economically and statistically significant impact of mortgage supply on rent growth over 2010-2014. A 1 percentage point increase in denial rates increased rent growth by 1.3 percentage points.

To put the results from Table 1.2 into perspective, it is useful to notice that the average MSA's denial rate fell by 1.6 percentage points over 2010-2014 relative to its 2008 level. However, denial rates actually rose in 31% of MSAs in our sample. If instead denial rates

Table 1.2: Rent Growth and Credit Supply: Baseline Specification

Outcome:	Avg Rent Growth $_{m,10-14}$	
Avg Denial Rate $_{m,10-14}$	0.105 (0.193)	1.309 (0.018)
Estimation	OLS	IV
MSA Controls	Yes	Yes
State Fixed Effects	Yes	Yes
Underidentification test (p-value)		0.017
J-statistic (p-value)		0.652
Number of Observations	257	257

Note: P-values are in parentheses. The instruments for Avg Denial Rate are: (i) the branch deposit share of the Big-4 banks in 2008; and (ii) the 2008 mortgage application share of lenders which underwent a stress test between 2011-2015. MSA controls are the 2009 log median income, log median rent, log population, log median inhabitant age, unemployment rate, and the 2000-2008 average annual change in log median income, log median rent, log median house price, log population, log median inhabitant age, and unemployment rate. The underidentification test is that of Kleibergen and Paap (2006). Each observation is an MSA. Standard errors are heteroskedasticity robust.

in these MSAs had fallen with the national average, then, based on our estimate from Table 1.2, rents would have grown at least 2.1 percentage points less in these MSAs (1.6×1.3). The cross-sectional standard deviation in 2010-2014 rent growth was 3 percentage points. Thus, elevated post-crisis credit stringency explains a meaningful amount of cross-MSA variation in recent rent behavior.

1.4 Validity of the Instruments

This section is devoted to assessing the instruments' validity and in particular the exclusion restriction. To address the exclusion restriction we perform the following exercises: 1) parallel trends analysis; 2) inspection of correlation with standard drivers of housing rents; 3) extensive local business cycle controls; 4) overidentification tests and sensitivity to alternative instruments; 5) placebo tests; 6) robustness of the results using county-level data and

geographic subsamples. Moreover, we check that the results are robust to functional form using a panel approach popular in the literature since Favara and Imbs (2015).

1.4.1 Parallel Trends

Figure 1.3 plots annual rent growth for MSAs ranking in the top and bottom 25% of exposure to each instrument. The year 2010 is the critical year when the Financial Stability Oversight Council was created and CCAR stress tests were announced as part of Dodd-Frank. For both instruments, we notice a substantial divergence in post-2010 rent growth between MSAs with high versus low exposure. However, prior to the shock, there are parallel dynamics between treated and control groups. That is, the instruments appear to only be driving rents in the post-crisis period.

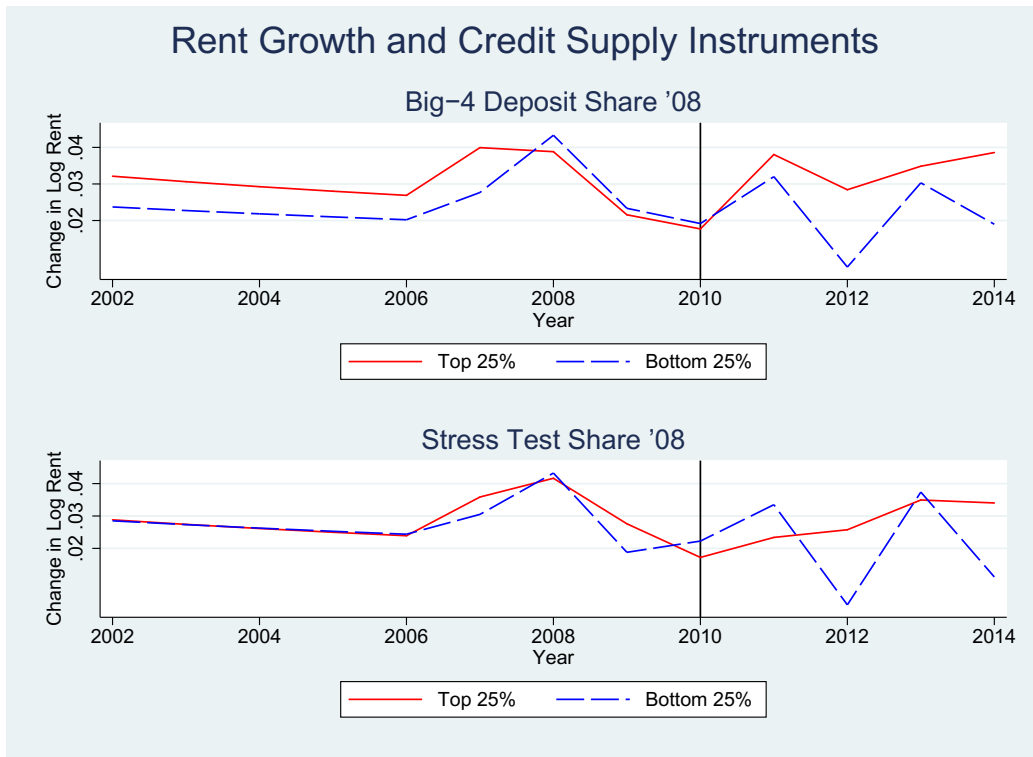
1.4.2 Correlation with Standard Drivers of Housing Rents

As an alternative test, in Table 1.3 we regress each of our instruments on a variety of pre-crisis trends and MSA controls. To better gauge the magnitude of these partial correlations, the table normalizes all variables to have a variance of one. This allows us to assess both the magnitude and statistical significance of any correlations.¹²

While it is impossible to directly test the exclusion restriction, Table 1.3 suggests that the instruments satisfy it as there is no relevant correlation between common drivers of rent growth and exposure to either stress tested lenders or the Big-4 banks. Moreover, as

¹²In Table 1.3 we use homeownership data from the decennial census because it covers a larger cross-section of MSAs than our core homeownership data from the Housing Vacancy Survey (HVS), which is available quarterly but only for 60 MSAs in our sample. We also measure house prices using starter homes, which are likely the relevant prices for constrained buyers. In online appendix Table A.3, we produce an analogous table with data from the HVS, and the conclusions are the same as we discuss here.

Figure 1.3: Credit Supply Instruments and Rent Growth



Note: This figure plots annual change in log rent for MSAs ranking in the top and bottom 25% of exposure to each credit supply instrument: (i) the branch deposit share of the Big-4 banks in 2008; and (ii) the 2008 mortgage application share of lenders which underwent a CCAR stress test between 2011-2015. In all plots, the red solid line denotes MSAs with a high (top 25%) exposure to the shock, and the blue dashed line denotes MSAs with a low (bottom 25%) exposure.

Table 1.3: Credit Supply Instruments and Drivers of Housing Rents

Outcome:	Tested _{$m,08$}	Big-4 _{$m,08$}
Avg Rent Growth _{$m,00-08$}	-0.116 (0.221)	-0.032 (0.784)
log(Rent _{$m,09$})	-0.048 (0.550)	-0.200 (0.205)
log(House Price _{$m,09$})	0.304 (0.010)	0.178 (0.233)
log(Population _{$m,09$})	-0.009 (0.899)	0.228 (0.024)
log(Income _{$m,09$})	0.141 (0.193)	0.036 (0.826)
Avg Unemp. Growth _{$m,10-14$}	-0.084 (0.167)	0.063 (0.495)
Avg Price Growth _{$m,10-14$}	0.064 (0.467)	-0.135 (0.226)
Financial Services Share _{$m,08$}	0.055 (0.364)	0.101 (0.484)
Homeownership Rate _{$m,09$}	0.064 (0.389)	-0.020 (0.851)
State Fixed Effects	Yes	Yes
R-squared	0.701	0.416
Number of Observations	220	220

Note: P-values are in parentheses. All variables are normalized to have a standard deviation of 1. The outcome in each column is one of our credit supply instruments: (i) the 2008 mortgage application share of lenders which underwent a stress test between 2011-2015; and (ii) the branch deposit share of the Big-4 banks in 2008. House prices for starter homes are based on Zillow's Bottom Tier Price Index. Homeownership rates are from the 2010 Census. Each observation is an MSA. Standard errors are heteroskedasticity robust.

Mian, Sufi, and Rao (2013) point out, fixed differences such as in the level of house prices or population will be differenced out in our baseline specification. Most importantly, all our regressions include an expansive set of controls.

1.4.3 Business Cycle Effects

To rule out the possibility that local business cycles drive the results, or that the results are a side effect of the small business loan contractions studied by Chen, Hanson, and Stein (2017), Table 1.4 re-estimates our baseline instrumental variables specification from Table 1.2 after controlling for a wide range of local business cycle variables.

In particular, Table 1.4 controls for five measures of contemporaneous economic activity in an MSA: average annual growth in unemployment, labor force participation, log number of establishments, log real GDP per capita, and log median hourly wage from 2010-2014. Moreover, we control for a manufacturing labor demand shock following Adelino, Ma, and Robinson (2017).¹³

Regardless of which measure we use, Table 1.4 shows that the point estimate for the effect of mortgage denials on rent growth is consistently between 1.1 and 1.3 and statistically significant. Moreover, the various business cycle measures all enter with the correct sign. This suggests that regional business cycles and mortgage supply are both important for rent growth, but they operate independently.

¹³In our setting this shock is the 2008 employment share of each 4-digit manufacturing industry in an MSA multiplied by the average 2010-2014 national log employment growth in that industry.

Table 1.4: Robustness: Business Cycle Effects

Outcome:	Avg Rent Growth $_{m,10-14}$						
Avg Denial Rate $_{m,10-14}$	1.295 (0.017)	1.166 (0.017)	1.140 (0.015)	1.296 (0.019)	1.314 (0.017)	1.323 (0.009)	1.179 (0.004)
Avg Unemp. Growth $_{m,10-14}$	-0.996 (0.118)						-1.039 (0.130)
Avg LFP Growth $_{m,10-14}$		0.824 (0.086)					1.159 (0.059)
Avg Estab. Growth $_{m,10-14}$			2.582 (0.000)				3.181 (0.000)
Avg Real GDP Growth $_{m,10-14}$				0.111 (0.536)			-0.191 (0.385)
Manufacturing Shock $_{m,10-14}$					0.284 (0.514)		0.246 (0.582)
Avg Wage Growth $_{m,10-14}$						-0.000 (0.992)	-0.003 (0.913)
Estimation	IV	IV	IV	IV	IV	IV	IV
MSA Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Underidentification test (p-value)	0.015	0.010	0.015	0.017	0.017	0.009	0.004
J-statistic (p-value)	0.580	0.535	0.698	0.661	0.666	0.919	0.605
Number of Observations	257	257	257	257	257	179	179

Note: P-values are in parentheses. Avg Unemployment Growth $_{m,10-14}$, Avg Labor Force Participation Growth $_{m,10-14}$, Avg Establishment Growth $_{m,10-14}$, Avg Real GDP Growth $_{m,10-14}$ and Avg Wage Growth $_{m,10-14}$ denote the average annual change in those variables in MSA m from 2010-2014. Manufacturing Shock $_{m,10-14}$ is the Bartik manufacturing shock used by Adelino, Ma, and Robinson (2017), which in our setting is the 2008 employment share of each 4-digit manufacturing industry in MSA m multiplied by the average 2010-2014 national log employment growth in that industry. The instruments for Avg Denial Rate and other MSA controls are the same as in Table 1.2. The underidentification test is that of Kleibergen and Paap (2006). Each observation is an MSA. Standard errors are heteroskedasticity robust.

1.4.4 Overidentification Tests and Alternative Instruments

We now exploit overidentification to assess the validity of the instrument set. First, the highly insignificant J -statistic in Table 1.2 shows that we cannot reject the null hypothesis of the instruments' exogeneity. As an additional test, Table 1.5 checks the robustness of our results when using the D'Acunto and Rossi (2017) instrument: the 2007 origination share of the top 20 mortgage lenders that year.

Table 1.5: Rent Growth and Credit Supply: Sensitivity to Lender Size

Outcome:	Avg Rent Growth $_{m,10-14}$		
Avg Denial Rate $_{m,10-14}$	1.287 (0.021)	-1.248 (0.492)	-0.498 (0.762)
Estimation	IV	IV	IV
Instruments	Top 20, Tested	Top 20-50	Top 50-150
MSA Controls	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes
Underidentification test (p-value)	0.017	0.340	0.382
J-statistic (p-value)	0.494		
Number of Observations	257	257	257

Note: P-values are in parentheses. Tested denotes the 2008 mortgage application share of lenders which underwent a stress test between 2011-2015. Top 20-50 and Top 50-100 denote the 2008 application share of lenders ranking between 20 and 50 and between 50 and 100 in terms of total originations that year. Top-20 is the D'Acunto and Rossi (2017) instrument, which in our setting is the 2007 origination share of the top 20 mortgage lenders that year. The remaining notation and controls are the same as in Table 1.2. The underidentification test is that of Kleibergen and Paap (2006). Each observation is an MSA. Standard errors are heteroskedasticity robust.

The first column of Table 1.5 shows that the estimated effect of denial rates is 1.3 when using the top 20 instrument instead of Big-4 share. This result is almost the same as in Table 1.2, and it is statistically significant. Moreover, the overidentification test continues to support the validity of all the instruments.

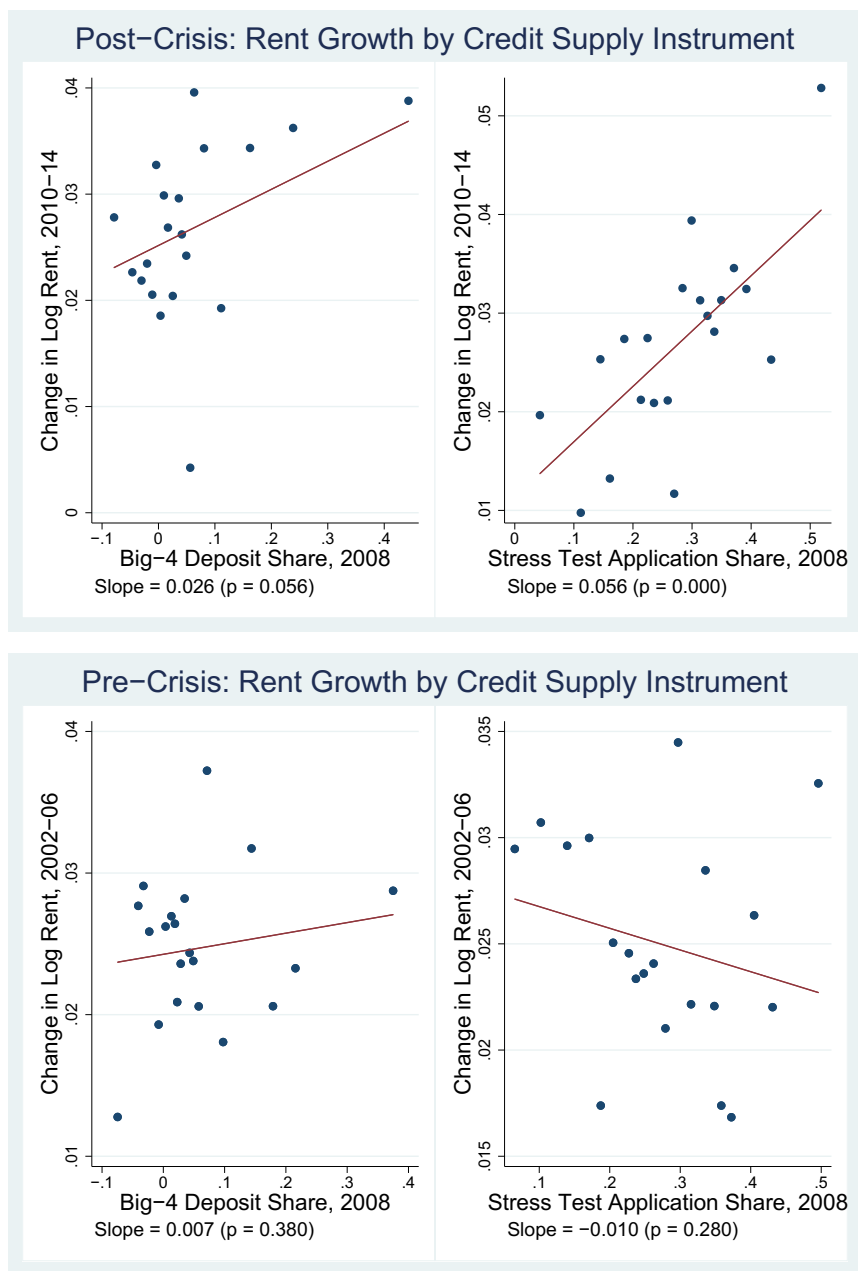
To second and third columns of Table 1.5 use as alternative instruments the 2008 mort-

gage application share of lenders ranked between 20 and 50 and between 50 and 150 that year, respectively. These groupings are chosen to capture the spectrum of mid-tier lenders. In neither column do we find a statistically significant effect of denials on rent growth. This suggests that our results are not driven by local economic conditions since those factors would affect all lenders and thus be reflected in these columns.

1.4.5 Placebo Test

In Figure 1.4 we visually inspect the impact of the instruments on annualized rent growth and average denial rates over 2010-2014. The scatterplot controls for the same variables as regression (1.1). It is binned so that each point represents around 12 MSAs. The top panel of the figure demonstrates strong positive correlation between each instrument and rent growth over 2010-2014. This role is absent in the pre-2008 placebo version of this figure that is in the bottom panel of Figure 1.4. This evidence suggests that the instruments are not contaminated by pre-crisis rent growth.

Figure 1.4: Pre and Post-2010 Rent Growth against Credit Supply Instruments



Note: The top panel plots 2010-2014 average annual change in log rent against the credit supply instruments: (i) the branch deposit share of the Big-4 banks in 2008; and (ii) the 2008 mortgage application share of lenders which underwent a CCAR stress test between 2011-2015. The bottom panel plots the same variables over 2002-2006. The top panel controls are the controls used in the baseline analysis in Table 1.2. The bottom controls are the controls used in the placebo analysis in Table 1.6.

Table 1.6: Placebo: Credit Supply and Rents Before the Crisis

Outcome:	Avg Rent Growth $_{m,period}$		
Avg Denial Rate $_{m,period}$	-0.292 (0.160)	-0.230 (0.232)	-0.266 (0.191)
Period	2002-2006	2000-2004	2001-2005
Estimation	IV	IV	IV
MSA Controls	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes
Underidentification test (p-value)	0.021	0.085	0.030
J-statistic (p-value)	0.414	0.194	0.244
Number of Observations	173	173	173

Note: P-values are in parentheses. The outcome in each column is average rent growth over the specified period. The instruments for Avg Denial Rate are the variables from Table 1.2. MSA controls are the 2000 log median income, log median rent, log population, log median inhabitant age, log median house price, and unemployment rate. The underidentification test is that of Kleibergen and Paap (2006). Each observation is an MSA. Standard errors are heteroskedasticity robust.

To rigorously assess the intuition from Figure 1.4, we conduct various placebo tests over the 2002-2006, 2001-2005, and 2000-2004 periods. We ask if, when using a specification analogous to (1.1), the credit supply shocks can explain rent growth over any of these periods. We should expect no effect of our instruments on pre-crisis rent growth because the instruments correspond to specific shocks to U.S. mortgage lenders over 2010-2014, unrelated to other drivers of housing rents. The placebo point estimates in Table 1.6 are insignificant across periods, and with the opposite sign relative to Table 1.3. This result suggests that the instruments are truly capturing post-crisis credit supply shocks.

1.4.6 Sample Sensitivity

To address sample sensitivity, we do two things in online appendix Table A.2: first we re-estimate (1.1) on the sub-sample of MSAs in states far from where the Big-4 have

their headquarters, and then we re-estimate (1.1) using county-level data. The first column reports quantitatively similar results when dropping MSAs close to a Big-4 headquarters. This makes it unlikely that the results are due to idiosyncratic location decisions by the major lenders. The second column shows a positive and significant point estimate when reperforming our analysis at the county level. However, the magnitude of the point estimate is smaller at 0.5, consistent with it being easier to substitute across lenders in different counties than in different MSAs.

1.4.7 Panel Analysis

In this subsection we check the robustness of the results using a panel analysis that exploits within-MSA variation. Following Favara and Imbs (2015) we estimate

$$\Delta \log(\text{Rent}_{m,t}) = \beta \times \Delta \text{Denied}_{m,t} + \gamma X_{m,t} + \alpha_m + \alpha_t + u_{m,t}, \quad (1.2)$$

where $\Delta \text{Denied}_{m,t}$ denotes the one year change in the denial rate in MSA m between year $t - 1$ and year t . This methodology allows us to hold fixed unobserved drivers of average rent growth over the sample period. However, it necessitates the use of credit supply instruments which vary over time. We study several candidates: first, a well-known instrument, the conforming loan limit instrument popularized by Loutskina and Strahan (2015), which we use to study the pre-crisis period and then modify for use after the 2008 Economic Stimulus Act; then the panel versions of the cross-sectional instruments studied in Section 1.3 that we create using the methodology of Khwaja and Mian (2008); and finally an instrument that is agnostic about which lenders are subject to shocks, in the spirit of Greenstone, Mas and

Nguyen (2015).¹⁴

Credit and Rents After the Crisis: Panel Analysis

The Khwaja and Mian (2008) methodology extracts a measure of lenders' propensity to deny a loan that is purged of borrower, MSA, and time effects. Figures A4 and A5 in the online appendix plot these denial propensities based on partitioning lenders according to Big-4 versus non Big-4 lenders, and according to stress tested lenders versus non-tested lenders.

Figure A4 shows that the Big-4 banks tightened standards after the implementation of Dodd-Frank and other major regulations in 2011.¹⁵ Interestingly, we see little significant difference between Big-4 and non Big-4 lenders over the 2000-2003 period. This result is consistent with Big-4 exposure representing a post-crisis credit supply shock.¹⁶

Figure A5 shows that denial propensities by stress tested lenders remained elevated throughout the post-crisis period, and they increased in 2012. This was the first year that CCAR results were made public. The placebo pre-crisis period in the bottom panel shows little significant difference between the two groups of lenders, nor is there significant difference relative to the reference lender-year (non-tested lenders in 2004). This is again consistent with exposure to stress-testing representing an exclusively post-crisis shock.

Table A.4 contains the baseline panel results. As in the cross-sectional analysis, we

¹⁴The construction of these instruments is described in the online appendix.

¹⁵Figure A6 shows how this effect was especially pronounced among FHA loans, which are intended for lower-income borrowers.

¹⁶In the top panel, the reference lender-year is non Big-4 lenders in 2007, and in the bottom panel the reference lender-year is non Big-4 lenders in 2004. The magnitudes in Figure A4 are the excess probability of Big-4 or non Big-4 lenders rejecting a borrower in a given year relative to this reference lender-year.

begin by estimating (1.2) using OLS. The result suggests no significant impact of credit supply of rents. However, after correcting for endogeneity with the instruments, the second column obtains a point estimate of 2.1 for the parameter of interest, which is very close to the estimate from Section 1.3.4. Furthermore, we reject the null hypothesis that the model is underidentified, and the highly insignificant J -statistic provides evidence of the instruments' exogeneity.

Table A.5 performs a panel placebo test.¹⁷ The Big-4 and stress test panel instruments should fail to explain rents during the pre-crisis placebo window. Indeed Table A.5 shows no economic or statistical significance. Moreover, the point estimates are negative. This finding suggests that the instruments capture credit supply shocks unique to the post-crisis period.

Credit and Rents Before the Crisis: the Conforming Loan Limit Instrument

None of the instrumental variables specifications that we studied before was able to explain housing rents in the pre-crisis period. We believe this suggests that the instruments are valid post-crisis credit supply shocks, not that the theory is invalid in the pre-crisis period. To investigate whether credit affects rents in other periods, we use the Loutskina and Strahan (2015) instrument which the literature has accepted as a valid credit supply shock. Thus, we use the triple product of: (a) the fraction of applications from MSA m in year $t - 1$ within 5% of the conforming loan limit in year t ; (b) MSA m 's elasticity of housing supply as estimated by Saiz (2010); and (c) the change in the log conforming loan limit between year $t - 1$ and year t .

The results in online appendix Table A.6 suggest a positive and statistically significant

¹⁷The online appendix provides other validity tests, including tests of instrument sensitivity.

effect of credit supply on rents in the pre-crisis period. However, the magnitude is much smaller than we found over the post-crisis period, as it suggests a 1 percentage point increase in denials led to a 0.07 percentage point increase in rent growth.¹⁸ Most importantly for this paper, Table A.6 suggests that credit supply can affect rents in any period. We interpret this result, together with the result that none of the instruments used in Section 1.3 can explain housing rents in the pre-crisis period, as further evidence that those instruments just capture post-crisis credit supply shocks.

1.5 Channels

The previous two sections robustly documented that tight credit supply has increased rent growth. To assess whether tenure choice is indeed the relevant mechanism, we now test five additional implications of the theory discussed in Section 1.2. First, mortgage denials should lead to lower price-to-rent ratios and have a non-positive effect on house price growth.¹⁹ Second, as rents rise, "buy to let" investors convert owner occupied units to rentals. Third, the homeownership rate must fall due to the combined effects of tight credit and expanding rental supply. Fourth, rental demand stimulates construction of multifamily units. Fifth, the credit-to-rent channel should be stronger where it is more difficult to substitute across lenders, for example because of different regulatory requirements across mortgage markets.

¹⁸One explanation is that there was little variation in credit supply over the pre-crisis period, as suggested by the bottom panels of Figures A4 and A5 discussed below. Other possibilities are that households' tenure choice was less responsive to credit supply in that period, or that there were fewer frictions to substitute between lenders.

¹⁹We are very grateful to the editor for this suggestion.

1.5.1 House Prices

Our theory implies that, at least in the short run, price-to-rent ratios should fall and there should be zero or possibly a negative effect on house prices. To test this hypothesis, the first column of Table 1.7 re-estimates (1.1) replacing the outcome variable with the average growth in the price-to-rent ratio over 2010-2014. The point estimate is negative and statistically significant, consistent with the theory.

Table 1.7: Price-to-Rents, House Prices and Credit Supply

Outcome:	Avg Price-to-Rent Growth $_{m,10-14}$	Avg Price Growth $_{m,10-14}$	
Avg Denial Rate $_{m,10-14}$	-60.904 (0.028)	-1.334 (0.526)	0.295 (0.414)
Home Type	All	Starter	All
Estimation	IV	IV	IV
MSA Controls	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes
Underidentification test (p-value)	0.017	0.131	0.041
J-statistic (p-value)	0.291	0.085	0.213
Number of Observations	257	208	257

Note: P-values are in parentheses. Price Growth $_{m,10-14}$ denotes the average annual change in the log of MSA median house price over 2010-2014, and Avg Price-to-Rent Growth $_{m,10-14}$ denotes the analogous change in the price-to-rent ratio. The first and third columns use all homes, based on Zillow's Home Value Index (ZHVI). The second column uses starter homes, based on Zillow's Bottom Tier Price Index. The instruments for Avg Denial Rate $_{m,10-14}$ are the variables from Table 1.2. MSA controls are those from Table 1.2 and, in columns two and three, 2009 log house prices for the indicated home type. The underidentification test is that of Kleibergen and Paap (2006). Each observation is an MSA. Standard errors are heteroskedasticity robust.

In columns two and three of Table 1.7, we study house price growth directly. The second column restricts attention to starter homes, since these houses are more likely to be priced by households denied mortgage credit and are thus more likely to have a negative price response.²⁰ In the third column we study all homes. In neither column do we find

²⁰The price of starter homes is measured using Zillow's Bottom Tier Index, which tracks the median home

a significant effect of mortgage denials on house prices, and the point estimate for starter homes is indeed substantially more negative in magnitude than the estimate obtained using all homes.

Figure 1.5 provides complementary visual evidence of the relationship between rent and price growth for starter homes. For MSAs with high exposure to the credit supply instruments, defined as an above-median value for both instruments, there is a negative relationship between rent and price growth. By contrast, the relationship between rents and prices is positive for MSAs with low exposure. Consistent with Table 1.7, the credit supply shock led to a substitution between rental and owner occupied properties for households denied a mortgage.

Online appendix Table A.8 corroborates the previous finding by estimating an OLS specification where the key independent variables are the 2008 mortgage application share of stress tested lenders, our preferred credit supply instrument, and its interaction with an indicator of whether the MSA had an above-median share of mortgage applications from blacks or Hispanics in 2009.²¹ The outcome variable is the average change in house prices over 2010-2014. The idea is that minority borrowers are more likely on the margin of homeownership. Thus markets with a high minority share may even see a negative relationship

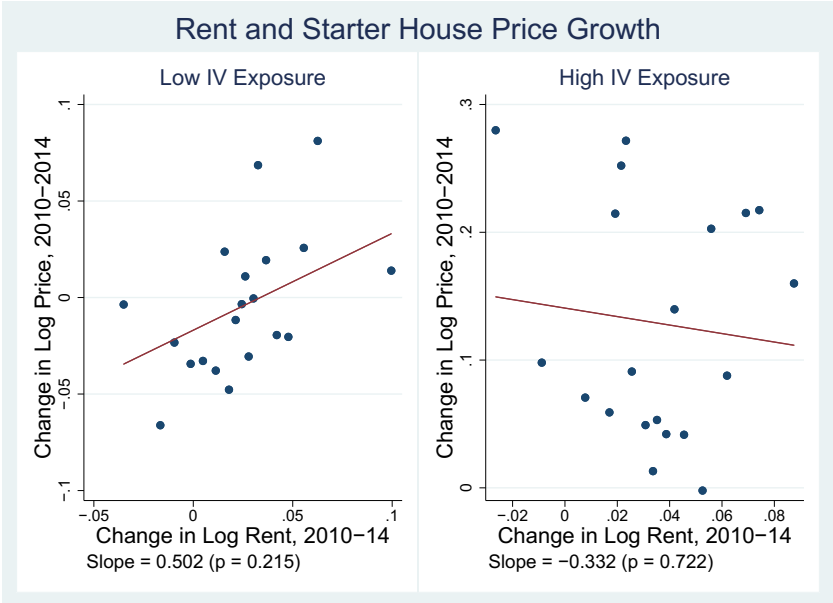
value among houses in the bottom third of the market.

²¹Following Angrist and Pischke (2009) we avoid estimating instrumental variable models with interactions and instead Table A.8 estimates:

$$\text{Avg House Price Growth}_{m,10-14} = \beta_1 \times \text{Tested}_{m,08} + \beta_2 \times \text{Tested}_{m,08} \times \text{High Minority}_{m,08} + \gamma X_m + u_m, \quad (1.3)$$

where Avg House Price Growth_{m,10-14} is the average annual change in the log of the Zillow Home Value Index over 2010-2014, Tested_{m,08} is the 2008 mortgage application share of lenders which underwent a stress test between 2011-2015, and High Minority_{m,08} indicates whether the MSA had an above-median share of mortgage applications from blacks or Hispanics in 2008. As in Table 1.2 we control for the 2009 value of the outcome variable and the other controls of Table 1.2.

Figure 1.5: Rent and Starter House Price Growth by Exposure to Credit Supply Instruments



Note: This figure plots the average change in log rents and log price of starter homes over 2010-2014. Each observation is an MSA. The left panel is based on MSAs with a below-median deposit share of the Big-4 banks and a below-median mortgage application share to stress tested lenders in 2008, and analogously the right panel has MSAs with an above-median share for both lender groups. Rents are measured using the ZRI, and starter house prices are measured using Zillow’s Bottom Tier House Price Index.

between the credit supply shock and house prices as these borrowers substitute between rental and owner occupied properties. This is indeed what we find, with a negative and significant point estimate on the interaction term.

These results are not only supportive of our theory, but they also contribute to rule out the possibility that unobserved housing demand shocks violate the exclusion restriction. If that were the case and the MSAs more exposed to our credit instruments also had positive demand shocks, then we should observe not only a positive and significant effect between instrumented denials and rents but also between instrumented denials and prices. This is because demand shocks generate comovement between prices and rents as shown in Gete and Reher (2016) and Gete and Zecchetto (2017) among others. Table 1.7 shows no evidence supporting that argument. Thus, the dynamics of prices reported in Table 1.7 strongly suggest that our results in Table 1.2 are due to a credit supply contraction operating through a tenure choice channel.

1.5.2 Tenure Conversion

The decoupling of rent and price growth from Table 1.7 suggests a profitable opportunity for "buy to let" investors. One would therefore expect to see increased conversion of owner occupied properties to rental units. Using data from the American Housing Survey (AHS), which tracks the same housing unit over time, we compute the fraction of rental units in an MSA which were owner occupied in the previous period.²² Then, we test the "buy to let" channel by re-estimating (1.1) replacing the outcome variable with the MSA's tenure

²²We exclude vacant units in our analysis.

conversion rate over 2011-2013.²³

Table 1.8: Tenure Conversion and Credit Supply

Outcome:	Tenure Conversion Rate _m	
Avg Denial Rate _{m,10-14}	1.059 (0.020)	-0.069 (0.949)
Conversion Window	2011-2013	2003-2013
Estimation	IV	IV
MSA Controls	Yes	Yes
State Fixed Effects	Yes	Yes
Underidentification test (p-value)	0.050	0.050
J-statistic (p-value)	0.425	0.862
Number of Observations	89	89

Note: P-values are in parentheses. Tenure Conversion Rate_m denotes the fraction of rental units in MSA m that were converted from owner occupied units over the indicated conversion window. The instruments for Avg Denial Rate_{m,10-14} are the variables from Table 1.2. MSA controls are those from Table 1.2 and the fraction of non-vacant units in 2009 that were owner occupied. The underidentification test is that of Kleibergen and Paap (2006). Each observation is an MSA. Standard errors are heteroskedasticity robust.

Table 1.8 contains the results of this exercise. In the first column, we find a positive and statistically significant effect of mortgage denial rates on tenure conversion. This is consistent with investors responding to a credit-induced demand for rentals by purchasing owner occupied units and subsequently renting them out.

In the second column of Table 1.8, we look for a longer-term effect by replacing the outcome variable with the tenure conversion rate over 2003-2013. The highly insignificant point estimate suggests that the post-crisis credit supply shock did not raise tenure conversion rates relative to pre-crisis levels. This finding relates to the welfare question of whether the shock led to abnormally tight standards and high rental demand, or whether it helped correct abnormally loose standards and low rental demand during the boom period. For example,

²³We use 2011-2013 because the AHS is only available in odd numbered years.

online appendix Figure A3 shows how the spike in mortgage denials in 2010 did not raise the denial rate substantially above pre-boom levels. We leave welfare questions for future research.

1.5.3 Homeownership

A key implication of our theory is that the homeownership rate falls as households cannot obtain mortgage credit and the stock of rental units grows. Using data on MSA-level homeownership rates from the Housing Vacancy Survey, we replace the outcome in (1.1) with an MSA’s average growth in homeownership from 2010-2014. The results in Table 1.9 indicate that a one percentage point increase in mortgage denials over 2010-2014 reduced homeownership growth by 0.7 percentage points. This effect is significant with a p-value of 0.05 despite the relatively small sample size. This again provides evidence that tight mortgage supply raised rents through households’ tenure choice.

Table 1.9: Homeownership and Credit Supply

Outcome:	Avg Homeownership Growth $_{m,10-14}$
Avg Denial Rate $_{m,10-14}$	-0.706 (0.053)
Estimation	IV
MSA Controls	Yes
State Fixed Effects	Yes
Underidentification test (p-value)	0.010
J-statistic (p-value)	0.863
Number of Observations	60

Note: P-values are in parentheses. Avg Homeownership Growth $_{m,10-14}$ denotes the average annual change in the homeownership rate in MSA m over 2010-2014. The instruments and controls are the variables from Table 1.2 plus the homeownership rate in 2009. The underidentification test is that of Kleibergen and Paap (2006). Each observation is an MSA. Standard errors are heteroskedasticity robust.

1.5.4 Multifamily Construction

We now ask whether the supply response documented in Section 1.5.2 was also accompanied by construction of new multifamily units. Specifically, we look at the growth in permits for the construction of multifamily units.²⁴ We replace the outcome variable in (1.1) with the average growth in log multifamily permits over 2011-2014, where we offset the outcome window by one year to account for a lag in the supply-side response because of lengthy permitting procedures (Gyourko, Saiz, and Summers 2008). Table A.9 in the online appendix suggests that the recent rent growth we have documented may dissipate as rental supply expands.

1.5.5 Lending Frictions

Implicit in our previous analysis is the notion that borrowers cannot easily substitute between lenders of different stringency. To measure the ease of substitutability, we utilize geographic variation in the regulation of mortgage brokers. According to Backley et al. (2006), states with additional licensing requirements for mortgage brokers have less competition, and likely stickier broker-lender relationships. That is, brokers may keep referring customers to the same lenders even if their standards are higher.²⁵

We test the strength of these frictions with an OLS regression in which the key independent variables are the stress test credit supply instrument and its interaction with an

²⁴We define multifamily units as the sum of 2 unit shelters, 3-4 unit shelters, and 5+ structure shelters. We cannot disentangle whether the new buildings will contain rental or owner occupied units.

²⁵18 states listed in the Data Appendix impose the additional requirement that individual mortgage brokers be licensed.

indicator of whether the MSA is in a state requiring such licensing.²⁶ Online appendix Table A.10 has the results. Notably, the estimated interaction term is positive and significant. This suggests a role for lending frictions in strengthening the credit-to-rent mechanism on which our theory is based.

1.6 Conclusion

In this paper, we showed that tighter mortgage credit can explain a significant component of rent growth following the 2008 financial crisis. Our empirical strategy used variation among MSAs in exposure to lenders more subject to regulatory costs and stress testing. We controlled for an array of local shocks and performed a battery of tests to check the validity of all instruments. The credit supply shocks used in our identification cannot explain pre-crisis housing rents and are unrelated to standard drivers of housing rents documented in the literature.

Moreover, consistent with our theory that credit supply operated through a housing tenure choice channel, we show that our identified mortgage supply contraction also caused lower price-to-rent ratios, had a non-positive effect on house price growth with a more negative effect for housing segments priced by constrained borrowers (starter homes and minority neighborhoods), lowered homeownership rates, and led to an expansion of rental supply, both through "buy to let" investors and higher multifamily construction.

²⁶Specifically, following Angrist and Pischke (2009), the regression equation is

$$\text{Avg Rent Growth}_{m,10-14} = \beta_1 \times \text{Tested}_{m,08} + \beta_2 \times \text{Tested}_{m,08} \times \text{License}_m + \gamma X_m + u_m, \quad (1.4)$$

where $\text{Tested}_{m,08}$ is the 2008 mortgage application share of lenders which underwent a stress test between 2011-2015 and License_m indicates whether the MSA is in a state requiring individual brokers to be licensed.

The previous result suggests that recent regulatory changes may have unintended consequences, resulting in less accessible credit for some borrowers and higher housing rents. Ambrose, Conklin and Yoshida (2016) present findings that point in the same direction. On the other hand, the tighter lending standards may also correct the excessively lax standards during the housing boom. Evaluating the socially optimal levels of homeownership and mortgage standards is an open avenue for future research.

The results also indicate that the price effect of the resulting rental demand will weaken as supply expands to accommodate more renters. This finding may signal that high rent growth is self-moderating through increased supply, without the need for rent controls. An interesting question for future work is the role of the "buy to let" investors in housing markets.

Chapter 2

Market Liquidity and Shadow Bank

Lending ²⁷

2.1 Introduction

A critical function of securitization is to give borrowers access to capital markets by transforming illiquid loans into liquid asset-backed securities (e.g. Strahan 2012). This process of liquidity transformation has generated intense policy debate since the 2008 crisis, with allegations that it destabilized the financial system by channeling credit to risky *borrowers*.²⁸ However, a more liquid secondary market might also affect financial stability by channeling market share to more fragile *lenders*. This lender-oriented view is particularly relevant given the recent expansion of the nonbank lending sector, often called the shadow banking system.

²⁷This chapter represents joint work with Pedro Gete.

²⁸For example, Willen (2014) discusses how popular backlash against securitization contributed to the adoption of the Risk Retention Ratio.

In the mortgage space, nonbanks now originate around 80% of loans insured by the Federal Housing Administration (FHA) and more than 50% of all mortgages, shown in Figure 1. This trend concerns policymakers, because many of the nonbanks that were active before the financial crisis either failed or were restructured.²⁹

We find that greater mortgage-backed securities' (MBS) liquidity lowers nonbanks' lending standards in the primary mortgage market, thereby increasing their market share relative to banks. Our period of analysis is 2010-15, during which the introduction of the U.S. Liquidity Coverage Ratio increased the secondary market liquidity of FHA-insured loans. Relative to banks, nonbanks respond to this increase in MBS liquidity by denying fewer FHA borrowers, especially low-income borrowers on the margin of homeownership. While we focus on the period after the Great Recession because of the aforementioned regulatory shock, we also provide evidence that MBS liquidity contributed nonbanks' growth in market share over 2000-06. Our results thus illustrate how recurring fluctuations in secondary market liquidity can affect not only the supply of credit, but also the types of lenders that intermediate that credit.

The underlying theory stems from variation in lenders' funding liquidity, defined as in Brunnermeier and Pedersen (2008). Unlike banks, nonbanks lack stable deposit funding and cannot hold illiquid loans on their balance sheet (e.g. Hanson et al 2015). Instead, they finance lending through short-term funding arrangements, such as repurchase agreements, in which the loans they have originated are used as collateral (e.g. Echeverry, Stanton, and Wallace 2016). This originate-to-securitize model makes nonbanks more reliant on secondary

²⁹See Wallace (2016) or Pinto and Oliner (2015).

market liquidity: when it becomes easier to sell loans on the secondary market, nonbanks can more easily unwind their funding arrangements and their effective funding costs fall. Thus, greater MBS liquidity should allow nonbanks to extend more credit in the primary mortgage market, either by approving more applicants or by lowering the price of credit for a given applicant.

Two econometric hurdles make it challenging to test this hypothesis. The first is omitted variables bias: unobserved factors, such as expectations about the housing market, affect both primary market lending and secondary market liquidity. To overcome this challenge, we develop a novel empirical strategy based on the cross-section of MBS liquidity. Broadly-speaking, the U.S. MBS market is segmented into two categories: securities insured by Ginnie Mae (GNMA); and securities insured by the government-sponsored enterprises (GSEs), namely Fannie Mae (FNMA) or Freddie Mac (FHLMC).³⁰ This market segmentation allows us to difference out common shocks to MBS submarkets and study the relative supply of credit across their corresponding primary markets. In particular, only loans to borrowers satisfying specific requirements stipulated by the Federal Housing Administration (FHA) can be securitized into GNMA MBS. Thus, according to our theory, an increase in the liquidity of GNMA MBS relative to, say, FNMA MBS should increase the relative supply of credit by nonbank lenders in the FHA market.

The second econometric challenge is reverse causality: lending behavior affects the supply of collateral and thus MBS liquidity. We address this challenge by appealing to a natural experiment: the introduction of the U.S. Liquidity Coverage Ratio (LCR). Proposed in Oc-

³⁰A third category, the private label market, evaporated in the years following the 2008 crisis, and so we focus on GNMA and GSE-backed MBS.

tober 2013, the LCR is intended to ensure that sufficiently large financial institutions have enough liquidity-weighted assets to survive a 30-day stress period. However, by assigning a preferential regulatory weight to GNMA MBS, this policy also stimulated GNMA demand and consequently increased the market liquidity of GNMA MBS relative to other securities. Using an event study research design, we find that the introduction of LCR increased the liquidity discount of GNMA MBS, lowering their expected return by 22% (55 basis points). Since the LCR announcement was largely unexpected and unrelated to contemporaneous trends in the U.S. housing market, it provides exogenous variation in the cross-section of MBS liquidity. We use this variation to identify the effect of liquidity on the relative supply of nonbank credit.

Our baseline exercise is a difference-in-difference research design, where “treated lenders” are nonbanks and the “treatment” is the LCR-induced increase in GNMA liquidity. We find that nonbanks respond to the increase in GNMA liquidity by denying 15% fewer FHA loan applicants. To confirm that funding liquidity is the key channel, we obtain similar results when defining “treated lenders” as those with less historical reliance on core deposit funding or greater historical reliance on securitization. In fact, the results are almost the same when dropping nonbanks from the sample, consistent with substantial heterogeneity in bank funding liquidity (e.g. Loutskina 2011; Cornett et al 2011; Dagher and Kazimov 2015). Our baseline outcome is a lender’s denial rate, but in an extension we show that nonbanks also disproportionately lower the price of FHA credit in response to an increase in GNMA liquidity. For every 1 percentage point increase in GNMA liquidity, the average lender reduces interest rates by 0.3 percentage points, whereas nonbanks reduce interest rates by an additional 17%. Thus, by focusing on the extensive margin of credit (i.e. denial rates), our

baseline results may understate the full impact of MBS liquidity on nonbank lending.

We assess the aggregate implications of our findings by conducting a similar difference-in-difference exercise at the census tract level. By aggregating to the census tract level, the point estimates reflect how nonbanks both deny fewer applicants and, through offering more favorable terms, attract more applications. We use our central point estimate to compute nonbanks' counterfactual market share in the absence of LCR regulation. This back-of-envelope calculation indicates that the LCR-induced increase in GNMA liquidity accounts for 23% (2.2 percentage points) of nonbanks' growth in FHA market share between 2013-15.

Turning to welfare implications, the baseline results are strongest for borrowers with high loan-to-income ratios, who are often on the margin of homeownership. Motivated by this finding, we ask whether nonbanks' expansion in credit supply may have attenuated the post-crisis collapse in homeownership rates. Based on a cross-sectional regression across zip codes, we find that zip codes with greater reliance on nonbanks in 2011 see a less severe decline in homeownership over 2011-15. This finding makes the welfare content of our results ambiguous: whereas greater MBS liquidity increases the market share of fragile nonbank lenders, it also facilitates access to homeownership.

We focus on the 2010-15 period because of the exogenous variation in MBS liquidity generated by the introduction of LCR. However, we document a similar relationship between MBS liquidity and nonbank lending over 2000-06. While we cannot rule out the possibility of reverse causality over that period, this finding suggests that fluctuations in nonbanks' market share can occur routinely as a byproduct of fluctuations in market liquidity. It also suggests that our baseline results are not due to spurious correlation between the introduction of LCR and other time-varying factors. Indeed, based on a wide variety of robustness exercises, we

find no evidence that our baseline result is driven by: increased litigation risk associated with the False Claims Act; the introduction of the Net Stable Funding Ratio; regulatory arbitrage; changing credit quality of nonbank and FHA loan applicants; the Fed’s quantitative easing program; or a pre-trend in nonbank denial rates. For further robustness, we estimate a triple difference-in-difference specification that obtains identification from the triple product of treated lenders (i.e. nonbanks), treated loan types (i.e. FHA loans), and the treatment (i.e. GNMA liquidity). This strategy allows us to include lender-year, MSA-year, and MSA-lender fixed effects. We again find that nonbanks respond to greater GNMA liquidity by denying fewer FHA applicants.

Our paper makes three contributions to the literature. First, a large number of papers have studied how securitization affects the quantity and quality of credit in primary lending markets (e.g. Loutskina and Strahan 2009; Keys, Mukherjee, Seru, and Vig 2010; Keys, Seru, and Vig 2012; Benmelech, Dlugosz, and Ivashina 2012; Nadauld and Sherlund 2013). These papers study how liquid secondary markets affect the distribution across types of *loans* that are originated in the primary market. By contrast, we study how secondary market liquidity affects the distribution across types of *lenders*.

Second, we contribute to a growing number of papers on the consequences and causes of recent growth in the nonbank lending sector. In terms of consequences, Kim et al (2018) highlight the systemic risks associated with greater reliance on nonbanks. In terms of causes, the existing literature has found that nonbanks’ market share depends on regulatory arbitrage (Buchak et al 2018), technological innovation (Fuster et al 2018), bank capitalization (Irani et al 2018; Chernenko, Erel, and Prilmeier 2018), and creditor protection in the warehouse lending market (Ganduri 2018). Our paper shows how secondary market liquidity is also

a force that significantly affects nonbanks' market share, in addition to the aforementioned forces.

Third, there is growing interest in how financial regulations introduced in the wake of the Great Recession affect U.S. housing markets. To date, papers have documented important effects related to stress tests (Calem, Correa and Lee 2016; Gete and Reher 2018), qualified-mortgage requirements (De Fusco, Johnson, and Mondragon 2019), litigation risk (D'Acunto and Rossi 2017; Gissler, Oldfather, and Ruffino 2016), and capital requirements (Reher 2019). We provide the first evidence that the Liquidity Coverage Ratio also affects the housing market in meaningful ways, leading to greater MBS liquidity, a higher nonbank share in the primary market, and more homeownership.

The remainder of the paper proceeds as follows: Section 3.2.3 briefly describes our theory and presents stylized facts about our setting, the U.S. mortgage market; Section 2.3 describes our identification strategy and the details of the Liquidity Coverage Ratio shock; Section 2.4 contains our main analysis; Section 2.5 performs a variety of robustness exercises; Section 2.6 studies implications for homeownership; and Section 3.6 concludes. All figures and tables may be found at the end of the main text. The online appendix has additional material.

2.2 Framework and Setting

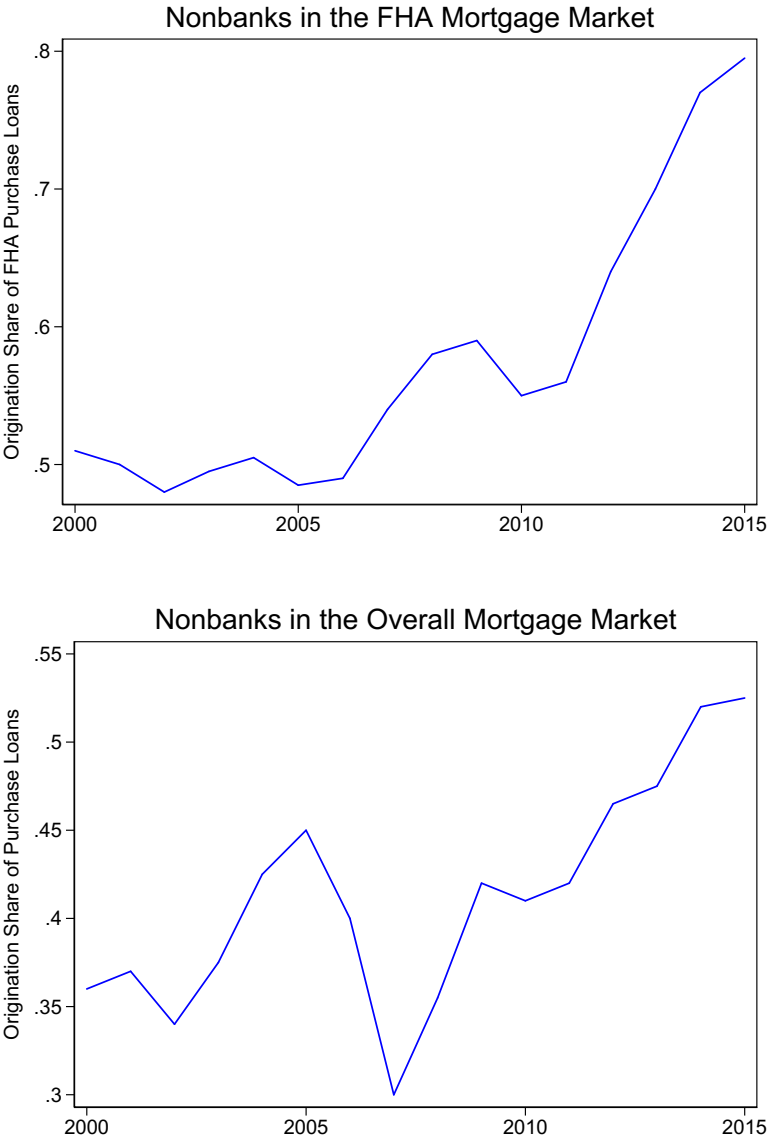
Our empirical analysis is grounded in a simple framework in which lenders have different levels of funding liquidity. Unlike banks, nonbanks do not have access to stable deposit funding, and thus they cannot hold loans on their balance sheet. Instead, they finance

lending through short-term arrangements such as repurchase agreements or warehouse lines of credit, using the loans they have originated as collateral. Greater secondary market liquidity increases the collateral value of these loans, enabling nonbanks to obtain more funding. In addition, a more liquid secondary market makes it easier for nonbanks to sell the loans they originate and thus unwind their funding arrangements. Consequently, nonbanks' supply of credit is more sensitive to secondary market liquidity. In a mortgage setting, nonbanks should respond to greater MBS liquidity by denying fewer loan applications. Or, they may offer more favorable loan terms to attract clients from other lenders, which can occur if mortgage lenders have limited market power (e.g. Scharfstein and Sunderam 2016).

We investigate this theory in the context of the U.S. mortgage market. Figure 2.1 shows that nonbanks' for-purchase mortgage origination share has increased dramatically since the financial crisis.³¹ In the top panel, we see that nonbanks historically comprised around 50% of the FHA market. Their share grew during the crisis, fell around 2010, and has seen sustained rapid growth since then. The bottom panel shows how nonbanks historically held a smaller share of the overall mortgage market, although their share grew markedly during the boom period. Since the crisis their share has grown, and now they comprise over half of all for-purchase mortgage originations.

³¹Since all depository institutions are subject to a federal supervisor, we use the associated Home Mortgage Disclosure Act (HMDA) codes and identify nonbanks as lenders without a federal supervisor (that is, lenders not under the regulatory oversight of OCC, FRS, FDIC, NCUA, or OTS). Demyanyk and Loutskina (2016) and Huszar and Yu (2017) follow the same criteria. We cross-checked that our sample, which comes from HMDA and covers the vast majority of originators in the U.S. mortgage market, is consistent with Buchak et al (2018), who manually define nonbanks as non-depository institution and focus on the largest lenders (50% of total originations). Appendix Table B.1 provides a list of the top 50 nonbanks in our data based on their FHA originations in 2013 and 2014.

Figure 2.1: Market Share of Non-depository Institutions Among FHA and All Loans for Home Purchases



Note: The figure shows the percentage of FHA mortgage dollar volume (top) and of total mortgage dollar volume (bottom) originated by nonbanks for home purchases. Source: HMDA.

2.3 Identification Strategy

The framework discussed in Section 3.2.3 predicts that greater MBS liquidity increases the relative supply of mortgage credit by nonbank lenders. We test this hypothesis using a novel methodology that has two key features: (a) we obtain identification through the cross-sectional distribution of MBS liquidity; (b) we utilize an exogenous, regulatory shock to this cross-sectional distribution.

First, by turning to the cross-section of MBS liquidity, we address the challenge of omitted variables bias. Specifically, we focus on the liquidity of GNMA MBS *relative* to either FNMA or FHLMC MBS. This technique differences out common shocks to the MBS market, such as expected housing demand or the Fed’s quantitative easing program, which also affect outcomes in the primary mortgage market. Correspondingly, in our main analysis we study how increases in the relative liquidity of GNMA MBS affect nonbanks’ market share among borrowers whose loans are eligible for securitization as GNMA MBS, called FHA loans.³²

Second, we address the question of reverse causality by turning to a natural experiment: the introduction of the U.S. Liquidity Coverage Ratio (LCR). Since exogenous changes in nonbanks’ FHA lending standards affect the supply of collateral for GNMA MBS, it is possible that fluctuations in GNMA liquidity reflect shocks to the primary market – the

³²As mentioned in the introduction, these borrowers must satisfy specific requirements stipulated by the Federal Housing Administration (FHA), which are meant to facilitate access to homeownership for first-time homebuyers with stable incomes. Specifically, FHA borrowers must typically have a FICO credit score above 580 and a debt-to-income ratio under 43%, although there is some discretion over the debt-to-income ceiling based on “compensating factors”. FHA loans feature down payments as low as 3.5%, but they require a mortgage insurance premium. Thus, FHA loans require a lower up-front payment at the cost of higher payments over the life of the loan. They are therefore appealing to first-time homebuyers with stable incomes but limited resources to finance a down payment.

reverse of the causal relationship we are interested in estimating. Thus, we perform our analysis over a period during which there was an exogenous shift in the GNMA liquidity premium due to the introduction of LCR, which we now describe.

2.3.1 A Natural Experiment: The Liquidity Coverage Ratio

The U.S. Liquidity Coverage Ratio was introduced as part of the post-crisis regulatory overhaul, and it was intended to ensure that sufficiently large financial institutions have enough liquid assets to survive a 30-day period of cash outflows. The policy assigned different regulatory liquidity weights to assets, where a higher weight implies more favorable regulatory treatment.³³ In particular, the rule favored GNMA MBS with a weight of 1, as opposed to 0.85 for FNMA and FHLMC MBS. This distinction reflects the explicit government guarantee associated with GNMA MBS, versus the implicit guarantee associated with FNMA and FHLMC MBS due to government conservatorship. The regulation was proposed on October 24, 2013 and finalized in September 2014. Before this proposal, there was uncertainty over the institutional details of LCR, since in 2011 Governor Daniel Tarullo of the Federal Reserve suggested that the U.S. implementation of LCR would differ from international standards, but he did not indicate how it would differ.³⁴

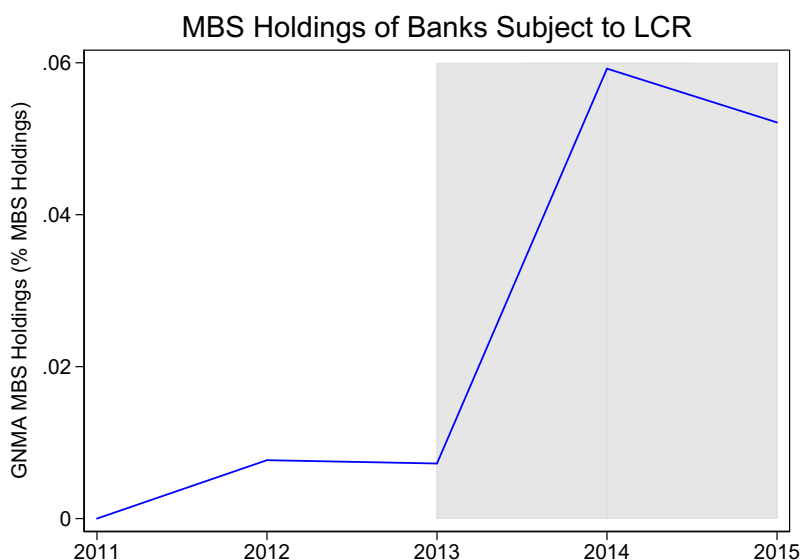
Given these details, one might expect the introduction of LCR to lead to: (a) an increase in affected banks' GNMA MBS holdings; and (b) consequently, an increase in the

³³Explicitly, a bank's liquidity coverage ratio is defined as the sum of liquidity-weighted assets divided by 30-day cash outflows. This ratio is required to exceed 1 for affected banks. See the report by the Basel Committee on Bank Supervision (2013) or Diamond and Kashyap (2016) for discussion of additional institutional details and the policy's motivation.

³⁴See the November 4, 2011 speech "The International Agenda for Financial Regulation" and Getter (2014).

market liquidity of GNMA MBS. In Figure 2.2 we examine the direct effect of the LCR shock by plotting the portfolio holdings of banks affected by the LCR rule. Affected banks substantially increase the amount of GNMA MBS on their balance sheets in the year after the LCR proposal.

Figure 2.2: MBS Holdings of Institutions Affected by Liquidity Regulation



Note: This figure plots the holdings of GNMA backed MBS as a percent of all securities held by banks subject to the LCR policy. The shaded region corresponds to the period after LCR rules were proposed on October 24th, 2013. Source: Call Reports (FRY-9C)

Next, we ask whether GNMA liquidity, an equilibrium object, also increases because of the LCR shock. We begin in Figure 2.3 by plotting MBS prices from the To-Be-Announced market for GNMA and FNMA MBS.³⁵ The price of both GNMA and FNMA MBS increase following the LCR proposal, consistent with both classes of MBS receiving positive regulatory

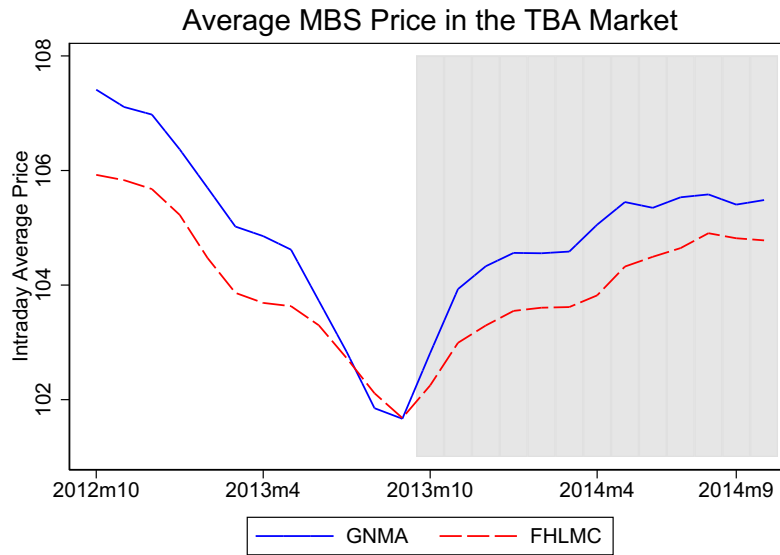
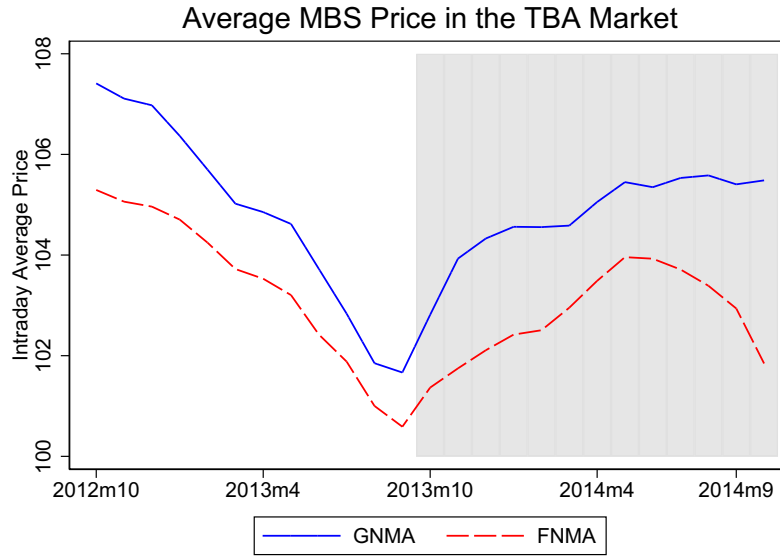
³⁵Following Echeverry, Stanton, and Wallace (2016), we consider MBS prices in the To-Be-Announced (TBA) market for 30-year fixed-rate mortgages. For each trading day, we take the price of the most-commonly traded bond in terms of settlement date and coupon. Our data source is FINRA's TRACE database. Because securities change from day to day, we smooth the data by taking the monthly average MBS price in the TBA market. Vickery and Wright (2013) and Gao, Schultz and Song (2017) discuss the TBA market in more detail.

weights. As expected, the price of GNMA MBS increases by more. We see a similar effect when considering FHLMC MBS in the bottom panel of Figure 2.3. In terms of quantities, Figure 2.4 documents an increase in securitization activity for FHA loans coinciding with the introduction of LCR. As with TBA prices, there is also an increase for conforming conventional loans, which are eligible for securitization as FNMA or FHLMC MBS, but it is much more modest. By contrast, there is no change in securitization activity for jumbo loans, which can only be securitized as private label MBS, as shown in Appendix Figure B.1.

The previous results provide qualitative evidence that the introduction of LCR increased the market liquidity of GNMA MBS, in both absolute terms and relative to non-GNMA MBS. We provide more rigorous evidence by conducting an event study which estimates the GNMA liquidity discount generated by the introduction of LCR. From here on, we use the word “premium” instead of “discount” to emphasize that liquidity increases the value of GNMA MBS from the perspective of a nonbank lender. To keep the paper focused, we defer details on this research design to the online appendix. Briefly, our central estimate in Appendix Table B.8 suggests that the introduction of LCR lowered the expected total return to GNMA MBS relative to FNMA MBS by 55 basis points.³⁶ This liquidity premium is equal to 22% of the average real total return to GNMA MBS over 2000-15 and 0.9 standard deviations of the FNMA-GNMA spread. We obtain similar results when studying the option-adjusted spread (OAS) as opposed to total return, which ensures that the results are not driven by changes in prepayment risk. Finally, Appendix Figure B.6 shows how the total

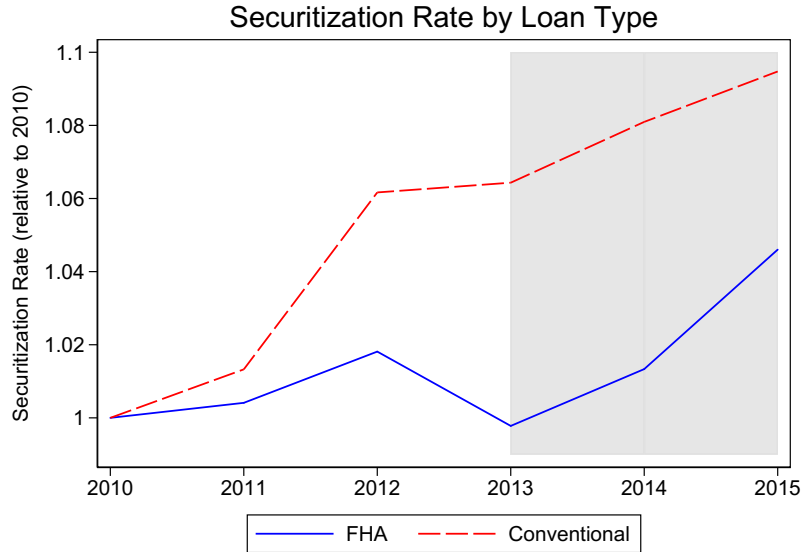
³⁶Following Diep, Eisfeldt, and Richardson (2017), we focus on MBS total returns measured using the Bloomberg-Barclays Total Return Index, since total returns are less model-dependent than an option-adjusted spread (OAS). Based on the law of iterated expectations, the realized total return from t to $t+k$ equals the expected total return, on average.

Figure 2.3: GNMA, FNMA and FHLMC MBS Prices in the TBA Market



Note: The price corresponds to the monthly average of the most-commonly traded bond on a given day. We smooth prices using a 3-month moving average. The shaded region corresponds to the period after LCR rules were proposed on October 24th, 2013. Source: Trade Reporting and Compliance Engine (TRACE).

Figure 2.4: Securitization by Loan Type



Note: This figure shows the fraction of FHA and conventional loans that are securitized, normalized by the 2010 value. The shaded region corresponds to the period after LCR rules were proposed on October 24th, 2013. Source: HMDA.

return profiles of GNMA and FNMA MBS track each other closely in the months leading up to the LCR announcement, after which they diverge markedly.

2.4 Main Analysis

Our parameter of interest is the effect of an increase in the liquidity of GNMA MBS on the supply of nonbank credit for FHA-eligible borrowers, recalling that only FHA loans can be securitized as GNMA MBS. As discussed in Section 3.2.3, this increase in credit supply can occur through two channels: (a) lower denial rates, taking the number of applications as given; and (b) more favorable loan terms, which increases the number of applicants. We focus on the former channel, denial rates, for two reasons. First, we do not observe interest rates in our core data (HMDA). In Section 2.5, we use an auxiliary dataset to study interest

rates. Second, focusing on application-level denial rates as opposed to an aggregated measure of credit supply (e.g. number of loans) allows us to use microdata, and thus we can include multiple fixed effects to absorb confounding factors.

2.4.1 Data

The core dataset is a merge of the Home Mortgage Disclosure Act (HMDA) mortgage application registry with bank Call Reports. HMDA data contain information on the borrower and outcome of almost all mortgage applications in the U.S. We retain FHA and conventional loan applications for the purchase of owner-occupied, single-family dwellings, where we use the term “conventional” to describe non-FHA loans whose value is below the associated conforming loan limit (i.e. non-jumbo loans). We focus on lenders which received at least 10 applications each year, and which have a record in HMDA from 2011 through 2015.³⁷ This gives a sample of 396 lenders over the 2010-15 period, 123 of which are non-depository institutions, which we call “nonbanks”. We then construct an analogous dataset over the 2000-06 period.³⁸ The upper two panels of Table 2.1 summarize the resulting two datasets. For computational convenience, we perform our application-level analysis on a 25% random sample of the full data.

³⁷The latter condition ensures we have a balanced sample around the introduction of the Liquidity Coverage Ratio.

³⁸We intentionally omit the 2007-09 period because of the Great Recession.

Table 2.1: Summary Statistics

Variable	Number of Observations	Mean	Standard Deviation
<u>Application-Level Variables, 2010-15:</u>			
Denial	13,114,592	0.112	0.316
Nonbank	13,114,592	0.495	0.5
FHA	13,114,592	0.32	0.467
Securitization Rate	10,409,953	0.828	0.263
Non-Core Funding	10,646,461	0.723	0.351
Loan-to-Income	13,114,592	2.786	2.361
<u>Application-Level Variables, 2000-06:</u>			
Denial	53,476,760	0.157	0.364
Nonbank	53,476,760	0.419	0.493
FHA	53,476,760	0.091	0.288
<u>Zip Code-Level Variables:</u>			
Δ Homeownership	3,585	-0.023	0.077
Homeownership	3,585	0.744	0.166
Nonbank Share	3,585	0.42	0.159
FHA Share	3,585	0.375	0.195
<u>Time-Series Variables:</u>			
GNMA Total Return (pps)	16	5.012	2.739
FNMA Spread (pps)	16	0.075	0.559
FHLMC Spread (pps)	16	0.035	0.624

Note: In the Application-Level panels, each observation is a loan application for the purchase of an owner-occupied single-family dwelling over the indicated time period, and the variables are defined as follows: Denial indicates if the application was denied; Nonbank indicates if the lender is a non-depository institution; FHA indicates if the application is for an FHA loan; Securitization Rate is the lender's ratio of securitized loans to total originations in 2010; Non-Core Funding is 1 minus the ratio of core deposits total assets in 2010, which equals 1 for nonbanks by definition; Loan-to-Income is the ratio of the applicant's requested loan to her reported annual income. In the Zip Code-Level panel, each observation is a zip code, and the variables are defined as follows: Δ Homeownership is the change in homeownership rate between 2011 and 2015; Homeownership is the 2011 homeownership rate; Nonbank Share and FHA Share are the 2011 share of mortgage applications which are to nonbanks and which were for FHA loans, respectively. In the Time-Series panel, each observation is a year over the 2000-2015 window, and the variables are defined as follows: GNMA Total Return is the average 12-month-ahead total return among months in a given year, where total returns are measured using the Bloomberg Barclays MBS Total Return indices; FNMA Spread is the difference between FNMA Total Return and GNMA Total Return; and FHLMC Spread is analogously defined in terms of FHLMC Total Return. The time-series variables have units of percentage points (pps).

2.4.2 Baseline Specification

Our baseline analysis consists of two exercises. In our primary exercise, we estimate a difference-in-difference specification across lenders and years. The difference-in-difference specification allows us to study the effect of GNMA MBS liquidity on the level of nonbanks' FHA lending, relative to banks' lending. In our secondary exercise, we estimate a triple difference-in-difference specification across lenders, years, and loan types (i.e. FHA versus non-FHA loans). This specification provides the most tightly-identified estimates, but its interpretation is limited because we cannot infer whether the point estimates reflect a change in the level of FHA lending, or simply a contraction of non-FHA lending.

Level Effect: Difference-in-Difference

We begin with a difference-in-difference exercise, which we perform on the subset of FHA loan applications. Our specification is

$$\text{Denial}_{i,l,t} = \beta (\text{Nonbank}_l \times \text{GNMA-Premium}_t) + \gamma X_{i,t} + \alpha_{m(i),t} + \alpha_{m(i),l} + u_{i,l,t}, \quad (2.1)$$

where: i , l , and t denote borrower (i.e. loan applicant), lender, and year, respectively; $\text{Denial}_{i,l,t}$ indicates if the application was denied; and Nonbank_l indicates if the lender is a nonbank. GNMA-Premium_t is a measure of the liquidity of GNMA MBS relative to non-GNMA MBS.

Our first measure of GNMA-Premium_t is an indicator for whether Liquidity Coverage Ratio (LCR) regulations are in place. Specifically, we use an indicator for whether $t \geq 2014$, the first full-year after the LCR announcement in October 2013. To more directly measure

the effect of MBS liquidity, we also measure GNMA-Premium_{*t*} using the spread in the one-year-ahead total return between FNMA and GNMA MBS.³⁹ For interpretive purposes, we normalize the FNMA-GNMA spread by 55 basis points, which is the estimated effect of LCR regulations discussed in Section 3.2.3 and estimated in the Online Appendix.

The identification assumption implicit in (2.1) is

$$0 = \mathbb{E} [\text{Nonbank}_l \times \text{GNMA-Premium}_t \times u_{i,l,t} | \alpha_{m(i),t}, \alpha_{m(i),l}, X_{i,t}]. \quad (2.2)$$

Under this assumption, the parameter β may be interpreted as the effect of GNMA MBS liquidity on nonbanks' denial rate relative to banks. Note that this effect is conditional on an MSA-year fixed effect $\alpha_{m(i),t}$, which subsumes the direct effect of GNMA-Premium_{*t*} and captures contemporaneous shocks to local demand in borrower *i*'s MSA of residence $m(i)$. These contemporaneous demand shocks might otherwise bias the estimate to the extent that they also affect a borrower's propensity of being denied (e.g. expected income growth). We also restrict variation to the same geographic lending relationship by including an MSA-lender fixed effect $\alpha_{m(i),l}$. This fixed effect rules out the possibility that nonbanks sort into markets where their applicant pool is of better credit quality. Finally, the borrower controls $X_{i,t}$ account for time-variation in the observable credit quality of bank versus nonbank applicants. We devote Section 2.5 to investigating the validity of (2.1). As a first pass, Appendix Figure B.2 shows that bank and nonbank FHA denial rates follow parallel trends leading up to the introduction of LCR, after which nonbank denial rates fall disproportionately.

³⁹We take the average 12-month-ahead total return among months in year *t*, where total returns are measured using the Bloomberg Barclays MBS Total Return indices. As mentioned in Section 2.3.1, the one-year-ahead return is equal to the expected return on average, based on the law of iterated expectations.

The first three columns of Table 2.2 contain results from estimating (2.1) over the 2010-15 period.⁴⁰ In the first column, we find that nonbanks are 2.0 pps less likely to deny an FHA loan in the post-LCR period, relative to banks. To make the channel more precise, the second column suggests that the increase in the FNMA-GNMA spread due to the introduction of LCR lowered nonbanks relative denial rate by 1.4 pps. We obtain a similar result when considering the FHLMC-GNMA spread in the third column. Appendix Table B.2 verifies that the results are robust to using the option-adjusted spread to measure GNMA-Premium_t , which suggests that the baseline results are not driven by either spurious correlation or changes in the relative prepayment risk of GNMA versus non-GNMA MBS. Collectively, the results suggest that greater MBS liquidity due to the introduction of LCR lowered nonbank denial rates by 1-2 pps, or roughly 15% of the unconditional denial rate of 11.2%.

MBS liquidity should, in principle, affect the relative supply of credit by nonbanks in other periods as well. To test this hypothesis, we reestimate (2.1) over the 2000-06 period and present the results in the rightmost two columns of Table 2.2. For the sake of a consistent interpretation, we continue to normalize MBS spreads by 55 basis points. On one hand, the point estimates from the 2000-06 period are less informative because this period lacks an exogenous source of variation in the cross-section of MBS liquidity. On the other hand, the results are both qualitatively and quantitatively consistent with those obtained in the context of the LCR natural experiment. This result strengthens the evidence that MBS liquidity affects the relative supply of nonbank credit, and it suggests that any bias due to

⁴⁰We cluster standard errors by lender-year bins, since the “treatment” is administered at the lender-year level.

Table 2.2: GNMA Premium and Nonbank Lending in the FHA Market

Outcome:	Denial _{<i>i,l,t</i>} Period: 2010-15			Denial _{<i>i,l,t</i>} Period: 2000-06	
	Nonbank _{<i>l</i>} × GNMA-Premium _{<i>t</i>}	-0.020 (0.051)	-0.014 (0.000)	-0.012 (0.000)	-0.017 (0.029)
Premium Measure	Post- LCR	FNMA Spread	FHLMC Spread	FNMA Spread	FHLMC Spread
Lender-MSA FE	Yes	Yes	Yes	Yes	Yes
MSA-Year FE	Yes	Yes	Yes	Yes	Yes
Borrower Controls	Yes	Yes	Yes	Yes	Yes
R-squared	0.117	0.117	0.117	0.132	0.132
Number of Observations	1040927	1040927	1040927	1056661	1056661

Note: P-values are in parentheses. This table estimates equation (2.1). Subscripts i , l , and t denote borrower, lender, and year, respectively. Each observation is a loan application. Denial indicates whether the application was denied. Nonbank indicates whether the lender is a nonbank. Each column interacts Nonbank with a measure of the GNMA liquidity premium: Post-LCR indicates whether $t \geq 2014$, the first full year after LCR regulations were announced; FNMA Spread is the spread in expected total return between FNMA and GNMA MBS; and FHLMC Spread is the analogous spread between FHLMC and GNMA MBS. Expected total return is measured using the average 12-month-ahead total return among months in year t , where total returns are measured using the Bloomberg Barclays MBS Total Return indices. We normalize the FNMA and FHLMC spreads by 55 basis points, which is the estimated effect of LCR regulations as discussed in Section 2.3.1. Borrower controls are requested loan-to-income ratio, log income, and an indicator of whether the borrower is black or Hispanic. The sample consists of applications for FHA loans for the purchase of an owner-occupied single-family dwelling. The sample period is 2010-15 in columns 1-3 and 2000-06 in columns 4-5. Standard errors are clustered by lender-year bins.

unobserved time-series dynamics in our baseline analysis is likely to be small.

Theoretically, the channel through which MBS spreads increase nonbank lending is funding liquidity: because nonbanks do not have access to stable deposit funding, their lending capacity is more dependent on demand from MBS investors. This conjecture motivates us to estimate a more general variant of equation (2.1),

$$\text{Denial}_{i,l,t} = \beta (F_l \times \text{GNMA-Premium}_t) + \gamma X_{i,t} + \alpha_{m(i),t} + \alpha_{m(i),l} + u_{i,l,t}, \quad (2.3)$$

where F_l is a measure of lender l 's funding illiquidity. Our first measure is the lender's ratio of securitized loans to total originations in 2010, which we call the lender's "securitization rate". This variable is meant to proxy for technological specialization in an originate-to-distribute model, which might arise from a lack of funding liquidity.⁴¹ Our second measure, called "non-core funding", is 1 minus the ratio of core deposits total assets in 2010. By definition, nonbanks have non-core funding equal to 1. We normalize a lender's securitization and non-core funding rates to have a mean of 0 and variance of 1.

Table 2.3 contains the results of the more general specification (2.3). The estimates in the first column suggest that lenders with a 1 standard deviation higher securitization rate respond to the LCR-induced GNMA premium by denying 1.5 pps fewer loan applicants. We obtain a similar result in terms of non-core funding in the rightmost two columns. Together, the results from Table 2.3 lend support to a theory where greater secondary market liquidity increases the relative supply of primary market credit by funding-illiquid lenders, of which nonbanks are a prime example.

⁴¹While there is little variation in nonbank securitization rates, bank securitization rates vary substantially, with a mean of 0.40 and standard deviation of 0.37.

Table 2.3: GNMA Premium and FHA Lending by Lender Funding Liquidity

Outcome:	Denial _{<i>i,l,t</i>}			
Securitization Rate _{<i>l</i>} × GNMA-Premium _{<i>t</i>}	-0.015 (0.008)	-0.014 (0.005)		
Non-Core Funding _{<i>l</i>} × GNMA-Premium _{<i>t</i>}			-0.020 (0.000)	-0.018 (0.000)
Premium Measure	FNMA Spread	FHLMC Spread	FNMA Spread	FHLMC Spread
Lender-MSA FE	Yes	Yes	Yes	Yes
MSA-Year FE	Yes	Yes	Yes	Yes
Borrower Controls	Yes	Yes	Yes	Yes
R-squared	0.117	0.117	0.118	0.118
Number of Observations	841475	841475	919025	919025

Note: P-values are in parentheses. This table estimates equation (2.3). Subscripts i , l , and t denote borrower, lender, and year, respectively. Each observation is a loan application. Securitization Rate is the lender's ratio of securitized loans to total originations in 2010. Non-Core Funding is 1 minus the ratio of core deposits total assets in 2010, which equals 1 for nonbanks by definition. The remaining notation is the same as in Table 2.2. The sample consists of applications for FHA loans for the purchase of an owner-occupied single-family dwelling from 2010-15. Standard errors are clustered by lender-year bins.

The effect of an increase in the GNMA premium on the supply of nonbank credit for conventional loans is theoretically unclear. If nonbanks face funding constraints, then one would predict an increase in conventional denial rates as nonbanks transfer loanable funds to the FHA market. By contrast, if nonbanks are unconstrained, then the effect should depend on the change in non-GNMA liquidity. If non-GNMA liquidity falls, then one would again predict an increase in the denial rate among conventional loans, since their value as a securitized product is lower. Otherwise, one would predict either no effect or, in the case where non-GNMA liquidity actually increases, a decrease in conventional denial rates. We investigate these questions by reestimating (2.1) on the subsample of conventional loans. Consistent with the theoretical ambiguity, there is variation in the sign and significance of the resulting point estimates, shown in Appendix Table B.3. In general, however, the results suggest a weakly positive effect on conventional denial rates, which may reflect a role for funding constraints.⁴²

Reallocation Effect: Triple Difference-in-Difference

Complementing our primary difference-in-difference exercise, we now estimate the following triple difference-in-difference specification,

$$\begin{aligned} \text{Denial}_{i,l,s,t} = & \beta (\text{Nonbank}_l \times \text{GNMA-Premium}_t \times \text{FHA}_s) + \gamma X_{i,t} + \alpha_{l,t} + \alpha_{m(i),t} + \alpha_{m(i),l} + \dots \\ & \dots + \alpha_{s,t} + \alpha_{s,l} + u_{i,l,t}, \end{aligned} \tag{2.4}$$

⁴²The null result when using the FNMA and FHLMC spreads over 2010-15 likely reflects an LCR-induced increase in the value of FNMA and FHLMC MBS, as suggested by Figure 2.3, albeit a smaller increase than that associated with GNMA MBS.

where s indexes loan type, which now can be either FHA or conventional. Thus, while our difference-in-difference specification (2.1) obtained identification from the double product of “treated lenders” (Nonbank_l) in “treated years” (GNMA-Premium_t), in (2.4) we obtain identification from the additional product with “treated loan types” (FHA_s).

The advantage to estimating a triple difference-in-difference specification is that it allows us to include lender-year fixed effects, $\alpha_{l,t}$. Thus, any confounding shock coinciding with GNMA-Premium_t would not only need to disproportionately affect nonbanks, but it would also have to affect nonbanks’ willingness to approve FHA over conventional loans. The type-year fixed effects $\alpha_{s,t}$ absorb time variation in lending standards for FHA loan applications due to, say, greater litigation risk. In addition, the type-lender fixed effect $\alpha_{s,l}$ accounts for the effect of lenders’ sorting into FHA or conventional loans. As in (2.1), we continue to limit variation to borrowers within the same MSA-year bin ($\alpha_{m(i),t}$), geographic lending relationship ($\alpha_{m(i),l}$), and with similar observable profiles ($X_{i,t}$).

The interpretation of β in (2.4) is the effect of GNMA MBS liquidity on nonbanks’ portfolio allocation between FHA and conventional loans, relative to banks. To be clear, (2.4) does not allow us to infer whether nonbanks actually increase their supply of credit for FHA loans: this effect is subsumed by the lender-year fixed effect, $\alpha_{l,t}$. That said, (2.4) provides a useful complement to the difference-in-difference specification (2.1) due to the stringency of its identification assumption.

The results in Table 2.4 suggest that nonbanks respond to an increase in GNMA liquidity by denying fewer FHA loans than conventional loans. Specifically, their relative denial rate on FHA loans falls by 0.7-2.1 pps. We obtain a similar result in Appendix Table B.4 when replacing Nonbank_l with the lender’s securitization rate. As discussed above,

a lender’s securitization rate captures its funding illiquidity, and so Appendix Table B.4 provides additional support for the more general mechanism through which MBS liquidity disproportionately affects nonbanks.

Table 2.4: GNMA Premium and Nonbank Portfolio Reallocation

Outcome:	Denial _{<i>i,l,s,t</i>}		
Nonbank _{<i>l</i>} × GNMA-Premium _{<i>t</i>} × FHA _{<i>s</i>}	-0.021 (0.000)	-0.008 (0.000)	-0.007 (0.000)
Premium Measure	Post- LCR	FNMA Spread	FHLMC Spread
Loan Type-Lender FE	Yes	Yes	Yes
Loan Type-Year FE	Yes	Yes	Yes
Lender-Year FE	Yes	Yes	Yes
Lender-MSA FE	Yes	Yes	Yes
MSA-Year FE	Yes	Yes	Yes
Borrower Controls	Yes	Yes	Yes
R-squared	0.116	0.116	0.116
Number of Observations	3267670	3267670	3267670

Note: P-values are in parentheses. This table estimates equation (2.4). Subscripts *i*, *l*, *s*, and *t* denote borrower, lender, loan type, and year, respectively. Each observation is a loan application. FHA indicates whether the loan’s type is FHA, where the possible types are FHA and Conforming Non-FHA, which we call “conventional” in the text. The remaining notation is the same as in Table 2.2. The sample consists of FHA and conventional loan applications for the purchase of an owner-occupied single-family dwelling from 2010-15. Standard errors are clustered by lender-year bins.

2.4.3 Heterogeneous Effects: Risky Borrowers

There are two reasons to suspect that the effect of GNMA liquidity on nonbank denial rates might be greater for borrowers in riskier markets. First, viewed through the lens of a credit rationing model, these markets have a greater mass of borrowers on the extensive margin of credit. Second, while FHA borrowers are subject to debt-to-income ceilings, lenders

can increase this ceiling by invoking “compensating factors”.⁴³ Thus, lenders have more discretion over denial rates for risky borrowers with a high debt-to-income ratio.

We test this hypothesis by reestimating (2.1) and interacting the treatment effect, $\text{Nonbank}_i \times \text{GNMA-Premium}_t$, with the average requested loan-to-income ratio (LTI) in the applicant’s MSA of residence.⁴⁴ The results in Table 2.5 indicate that nonbanks lower their denial rates by an additional 0.3 pps (25%) in MSAs with a 1 standard deviation higher LTI. This finding suggests that nonbanks respond to MBS liquidity by disproportionately lowering their standards for higher-risk borrowers.

2.4.4 Aggregate Effects

While attractive for the purposes of identification, an application-level analysis is unsuitable for making inferences about the aggregate effects of an increase in MBS liquidity. This limitation reflects how such an analysis takes the number of bank versus nonbank applications as given. In reality, nonbanks may attract a greater applicant pool by offering more favorable loan terms, or possibly through an increase in advertising. To capture this additional effect, we aggregate our microdata to the census tract level and reproduce the baseline analysis. One should think of each census tract as a representative household which has relationships with multiple lenders. Carrying the baseline intuition into this setting, our research hypothesis is that lending relationships involving a nonbank should see growth in FHA originations following an increase in GNMA MBS liquidity.

⁴³Examples of compensating factors include cash reserves or residual income.

⁴⁴The results are the same when including the interaction with the borrower’s requested LTI. Taking the MSA average reduces the effect of measurement error from potential misreporting (e.g. Mian and Sufi 2009). Note that the direct effect of an MSA’s average LTI is subsumed by $\alpha_{m(i),t}$.

Table 2.5: GNMA Premium and Nonbank Loan-to-Income Standards in the FHA Market

Outcome:	Denial _{<i>i,l,t</i>}		
Nonbank _{<i>l</i>} × GNMA-Premium _{<i>t</i>}	-0.020 (0.046)	-0.014 (0.000)	-0.012 (0.000)
Nonbank _{<i>l</i>} × GNMA-Premium _{<i>t</i>} × LTI _{<i>m(i),t</i>}	-0.006 (0.117)	-0.003 (0.009)	-0.003 (0.002)
Premium Measure	Post- LCR	FNMA Spread	FHLMC Spread
Lender-MSA FE	Yes	Yes	Yes
MSA-Year FE	Yes	Yes	Yes
Borrower Controls	Yes	Yes	Yes
R-squared	0.117	0.117	0.117
Number of Observations	1040927	1040927	1040927

Note: P-values are in parentheses. This table estimates a variant of equation (2.1). Subscripts i , l , and t denote borrower, lender, and year, respectively. Each observation is a loan application. LTI denotes the average loan-to-income ratio among borrowers in the applicants MSA of residence, $m(i)$; it is normalized to have a variance of 1 and a mean of 0. The remaining notation is the same as in Table 2.2. The sample consists of applications for FHA loans for the purchase of an owner-occupied single-family dwelling. The sample period is 2010-15. Standard errors are clustered by lender-year bins.

Our tract-level specification is

$$\log(\text{Loans Originated}_{c,l,t}) = \beta (\text{Nonbank}_l \times \text{GNMA-Premium}_t) + \alpha_{c,t} + \alpha_{c,l} + u_{c,l,t}, \quad (2.5)$$

where c , l , and t index census tract, lender, and year; $\text{Loans Originated}_{c,l,t}$ is the number of FHA loans originated within each tract-lender-year triplet; and $\alpha_{c,l}$ is a tract-lender fixed effect, which has the interpretation of a lender's steady-state market share in tract c . The tract-year fixed effect $\alpha_{c,t}$ nonparametrically captures time-varying credit demand shocks, and this technique is conceptually similar to that used in the literature studying bank-firm lending relationships (e.g. Amiti and Weinstein 2018; Greenstone, Mas, and Nguyen 2017; Khwaja and Mian 2008).

The identification assumption implicit in (2.5) is not that fluctuations in the GNMA premium coincide with credit demand shocks in tracts dominated by nonbanks, which would be subsumed by $\alpha_{c,t}$. Rather, we assume that these fluctuations do not coincide with shocks affecting the distribution of credit between banks and nonbanks in a given tract-year bin. This assumption is similar to (2.2), with the additional restriction that the treatment effect, $\text{Nonbank}_l \times \text{GNMA-Premium}_t$, be orthogonal to shocks that affect the number of FHA applications to nonbanks, as opposed to just denial rates. Put differently, we assume that FHA borrowers do not switch from applying to banks to applying to nonbanks when the GNMA premium is higher. This assumption seems plausible, since census tracts are relatively granular geographic units comprising around 4,000 people. Thus, there is limited scope for demographic variation within a census tract, which might bias the results if nonbanks cater to a certain demographic subpopulation and this subpopulation experiences a credit demand

shock.

Table 2.6 contains the results of (2.5). Consistent with the application-level results, a higher GNMA premium due to the introduction of LCR leads to a relative increase in nonbank loan originations, as reflected by the positive and significant point estimates. We next ask how much smaller nonbanks' FHA market share would have been in 2015 absent the LCR-induced increase in GNMA liquidity. Explicitly, let η_{15} denote nonbanks' FHA market share in 2015, where

$$\eta_{15} = \frac{\sum_c \sum_l \text{Loans Originated}_{c,l,15} \times \text{Nonbank}_l}{\sum_c \sum_l \text{Loans Originated}_{c,l,15}} \quad (2.6)$$

Empirically, $\eta_{15} = 0.80$. We are interested in computing the market share $\hat{\eta}_{15}$ that would have arisen had the GNMA premium not increased due to the introduction of LCR. Using (2.5), this counterfactual share can be written

$$\hat{\eta}_{15} = \frac{\sum_c \sum_l \text{Loans Originated}_{c,l,15} \times \text{Nonbank}_l \times e^{-\beta^{LCR}}}{\sum_c \sum_l \text{Loans Originated}_{c,l,15} \times [(1 - \text{Nonbank}) + \text{Nonbank}_l \times e^{-\beta^{LCR}}]}, \quad (2.7)$$

where $\beta^{LCR} = 0.13$ is the average point estimate across columns in Table 2.5. The resulting counterfactual market share is $\hat{\eta}_{15} = 0.77$, which is 2.2 pps lower than the true market share.⁴⁵ To place these numbers in perspective, nonbanks' FHA market share grew by 9.5 pps from 2013 to 2015, so that the LCR-induced increase in GNMA liquidity can account for around 23% of nonbanks' 2013-15 growth in market share.

⁴⁵Note that because (2.5) is specified in logs and our focus is on nonbanks' counterfactual *share* of originations, the unestimated effect of GNMA liquidity on all lenders cancels out when taking the ratio in (2.7).

Table 2.6: GNMA Premium and Nonbank Lending at the Census Tract Level

Outcome:	log (Loans Originated _{c,l,t})		
Nonbank _l × GNMA-Premium _t	0.244 (0.000)	0.081 (0.000)	0.070 (0.000)
Premium Measure	Post- LCR	FNMA Spread	FHLMC Spread
Lender-Tract FE	Yes	Yes	Yes
Tract-Year FE	Yes	Yes	Yes
R-squared	0.625	0.623	0.623
Number of Observations	1377027	1377027	1377027

Note: P-values are in parentheses. This table estimates equation (2.5). Subscripts c , l , and t denote census tract, lender, and year, respectively. Each observation is a tract-lender-year triplet. Loans Originated is the number FHA loans originated within each triplet. The remaining notation is the same as in Table 2.2. The sample consists of all triplets that featured at least 1 FHA loan application for the purchase of an owner-occupied single-family dwelling from 2010-15. Standard errors are clustered by lender-year bins.

2.5 Robustness

In this section we investigate our primary identification assumption, the exclusion restriction (2.2). While we conduct part of our baseline analysis over 2000-06, we focus our attention on the 2010-15 period and the introduction of LCR, which we argue is an exogenous source of variation in MBS liquidity.

2.5.1 Litigation Risk

Beginning with a 2011 suit against Deutsche Bank, the U.S. Department of Justice sued a number of large banks over 2011-15, alleging that their FHA lending behavior violated the False Claims Act. To the extent that an increase in expected litigation activity coincided with the introduction of LCR, the baseline results may reflect heightened legal risk rather

than greater GNMA liquidity. However, there are two reasons that make litigation risk an unlikely source of bias. First, large nonbank lenders, such as Quicken Loans, were also subject to lawsuits related to their lending in FHA markets. Second, the Department of Justice also sued large lenders over their behavior in conventional mortgage markets.⁴⁶ Thus, if litigation risk is a significant source of bias, one would expect to see similar results among conventional loans. However, as discussed above, the corresponding results in Appendix Table B.3 are either null or of the opposite sign.

To more directly address bias from litigation risk, we reestimate our baseline specifications (2.1) and (2.4) on the set of lenders with less than 2% of the total mortgage market in 2010, measured by origination share. The results in Appendix Table B.5 are qualitatively similar to those in Table 2.2 and 2.4.

2.5.2 Net Stable Funding Ratio

The Basel III accords involved not only a Liquidity Coverage Ratio, but also a complementary Net Stable Funding Ratio (NSFR). The NSFR aimed to ensure that banks “maintain sufficient levels of stable funding, thereby reducing liquidity risk in the banking system”, per the Federal Reserve’s press release on May 3, 2016. However, the NSFR was not proposed in the U.S. until May 2016, more than two years after the LCR proposal. It is thus unlikely that the NSFR is affecting the results. Nonetheless, it is possible that lenders updated their expectations following the LCR announcement, and that banks with less funding liquidity

⁴⁶For example, in 2012 the Department of Justice alleged that Bank of America violated the Financial Institutions Reform, Recovery, and Enforcement Act of 1989 by selling low-quality loans to Fannie Mae and Freddie Mac.

subsequently aimed to shrink their balance sheets.

The previous logic contradicts Appendix Table B.6, where we reestimate (2.3) and (2.4) after excluding nonbanks from the sample. The results suggest that banks with greater historical reliance on securitization denied fewer FHA applicants after the increase in GNMA liquidity. While the standard errors increase due to the reduced sample size, the point estimates are quite similar to their counterparts from Tables 2.3 and 2.4 and are all statistically significant at the 10% threshold.

2.5.3 Regulatory Arbitrage

As documented by Buchak et al. (2018), regulatory arbitrage has been a key driver of nonbanks' increasing market share. Thus, our baseline analysis may capture differential costs of regulation across lenders rather than a response to LCR-induced changes in MBS liquidity. Such bias is unlikely for three reasons. First, we obtain similar results on the subsample of banks, as just discussed in the context of Appendix Table B.6. Second, we obtain similar results after excluding relatively large lenders, as previously discussed in the context of Appendix Table B.5. Finally, Table 2.2 documents a strong link between MBS liquidity and the relative supply of credit by nonbanks in the 2000-06 period, before the post-crisis regulatory overhaul.

2.5.4 Changing Applicant Pool

Since our core analysis is at the application level, it takes as given the distribution of borrower quality across different loan types. If FHA loan applicants are becoming less

risky, this alone would not generate the results in Section 2.4. One would further need that lenders with less exposure to GNMA MBS have some cost of adjusting to the new quality of FHA borrowers. However, Appendix Figure B.3 shows that the requested loan-to-income ratio (LTI) for FHA and non-FHA applicants have grown at approximately the same rate. If anything, Figure B.3 suggests that FHA applicants have become slightly riskier, in terms of LTI, relative to non-FHA applicants. This feature may stem from nonbanks' willingness to approve high-LTI borrowers, documented in Table 2.5, which encourages such borrowers to apply for mortgage credit. In any case, the dynamics shown in Figure B.3 make it unlikely that the results are biased due to exogenous changes in the pool of FHA borrowers.

Similarly, one might wonder whether the pool of applicants to nonbanks is changing over time. If so, then the point estimates may reflect the improving quality of applicants to nonbanks. However, Appendix Figure B.4 provides evidence to the contrary. The top panel of Appendix Figure B.4 shows how the gap in the requested loan-to-income ratio of applicants to banks versus nonbanks has been remarkably stable over time.

2.5.5 Quantitative Easing

The third round of MBS purchases by the Fed overlapped with the introduction of LCR, as it lasted from 2012 to 2014. The Fed bought MBS sponsored by the GSEs (i.e. FNMA and FHLMC) and by GNMA, with a tilt towards GSE MBS per the report by the Board of Governors (2016). Appendix Figure B.5 shows that the ratio of Fed's purchases was weighted against GNMA MBS, and so these purchases are unlikely to account for the increase in GNMA MBS prices relative to GSE MBS.

2.5.6 Monthly Frequency and Interest Rates

One drawback to the baseline analysis is that HMDA data are only available at the yearly frequency, which increases the possibility of spurious time-series correlation. We address this issue by using data from the HUD FHA Single Family Portfolio Snap Shot to perform a similar analysis at the monthly frequency. Relative to our core HMDA dataset, we only observe originated FHA loans in the HUD data, as opposed to applications. Thus, we turn our attention away from denial rates (“quantity of credit”) to the interest rates charged by nonbanks (“price of credit”).

We estimate a similar specification as (2.1) over the 2012-15 period,

$$\text{Rate}_{i,l,t} = \beta (\text{Nonbank}_l \times \text{GNMA-Premium}_t) + \gamma Z_{i,t} + \alpha_{m(i),t} + \alpha_{m(i),l} + u_{i,l,t}, \quad (2.8)$$

where i , l , and t denote borrowers, lenders, and months; each observation is an originated loan; and $\text{Rate}_{i,l,t}$ is the interest rate on the loan. Unlike in our baseline analysis, we do not normalize GNMA-Premium_t by the implied effect of LCR, since our outcome variable is now an interest rate. The controls in $Z_{i,t}$ are log loan size and an indicator for whether the loan is a fixed-rate mortgage. The remaining notation is the same as in prior equations.⁴⁷

Mortgage interest rates typically fall when the GNMA premium rises, measured using either total return or option-adjusted spreads. Thus, the parameter β captures nonbanks’ rate of pass-through from MBS liquidity to lower mortgage rates, relative to banks. The first two columns of Appendix Table B.7 show that nonbanks’ rate of pass-through is 5 percentage

⁴⁷We classify lenders as nonbanks if their parent company’s name does not contain “Bank”, “Credit Union”, or variant spellings of these terms.

points greater than banks'. To place this number in perspective, the unconditional pass-through of the FNMA-GNMA Spread to mortgage interest rates is 30%, so that nonbanks have a 17% higher pass-through rate. The rightmost columns obtain a similar result when using the option-adjusted spread to measure GNMA-Premium_t .

Collectively, these results provide suggest that our baseline methodology is not misidentified because of the yearly frequency. The results also suggest that nonbanks disproportionately lower the price of credit, in addition to approving more loans, following an increase in MBS liquidity.

2.6 Welfare Implications: Homeownership

Our results suggest that greater MBS liquidity has an ambiguous effect on welfare. On one hand, a more liquid secondary market increases the share of primary market lending intermediated by fragile nonbanks. This observation suggests that MBS liquidity may have negative welfare implications by reducing financial stability. That said, our primary source of variation in MBS liquidity is the introduction of the Liquidity Coverage Ratio, which itself may promote financial stability by improving large banks' funding liquidity.

In the opposite direction, MBS liquidity enables borrowers constrained by credit frictions to obtain a mortgage, which may constitute a positive welfare implication. It is important to stress that most of our analysis occurs in the context of the FHA market, which caters to households on the margin of homeownership. Thus, a natural question is whether the increase in nonbank-intermediated credit influences homeownership rates. We address this question by using zip code level data from the American Housing Survey's 5-year estimates,

in which we observe a zip code’s homeownership rate in 2011 and 2015.⁴⁸ Because the 5-Year estimates are designed to study medium-to-long run changes in homeownership, we depart from a panel specification and run a cross-sectional regression. The specification is

$$\Delta \text{Homeownership}_{z,11-15} = \beta (\text{Nonbank Share}_{z,11} \times \text{FHA Share}_{z,11}) + \gamma X_{z,11} + \alpha_{c(z)} + u_z, \quad (2.9)$$

where: z indexes zip codes; $\Delta \text{Homeownership}_{z,11-15}$ denotes the change in the homeownership rate between 2011 and 2015; $\text{Nonbank Share}_{z,11}$ and $\text{FHA Share}_{z,11}$ are the 2011 share of mortgage applications which are to nonbanks and which were for FHA loans, respectively; and $\alpha_{c(z)}$ is a county fixed effect. The zip code controls in $X_{z,11}$ are: $\text{Nonbank Share}_{z,11}$; $\text{FHA Share}_{z,11}$; the 2011 homeownership rate; the 2011 average requested loan-to-income ratio; and the 2011 share of applications from black or Hispanic borrowers.

The treatment group in (2.9) consists of zip codes with (i) a high initial nonbank share and (ii) a high share of FHA applicants. Building on the core analysis in Section 2.4, these are the groups most likely to experience a loosening of standards due to the effect of MBS liquidity on nonbanks. Importantly, the controls in $X_{z,11}$ include both the initial nonbank and the initial FHA application share, and therefore our results are not affected by unobserved features of nonbank-prevalent or FHA-prevalent markets that correlate with homeownership. Moreover, the county fixed effect $\alpha_{c(z)}$ means that all variation comes from within the same county. Thus, it cannot be that the results are driven by county-level shocks such as ease of construction (e.g. Saiz 2010). That is, we are comparing two zip codes in the same county with the same initial nonbank and FHA exposure. Our source of identification is the fact

⁴⁸Zip codes are typically larger than census tracts. We merge each zip code to a census tract in our core HMDA data using the HUD’s crosswalk file, and then we aggregate to the zip code level.

that nonbanks loosened standards for a particular type of loan, namely FHA loans.

The result in Table 2.7 shows how zip codes more exposed to nonbanks' expansion in the FHA market see a less severe decline in homeownership. Taking the average zip code's FHA share of 0.43, the point estimate implies that homeownership rates fall 1.3 pps less in zip codes with full exposure to nonbanks in 2011 relative to zip codes with no nonbank exposure. Given that the average zip code saw a 2.4 pp decline in homeownership over 2011-15, the effect is quantitatively significant. This finding further complicates the welfare interpretation of an increase in MBS liquidity. In particular, it suggests that the resulting increase in the relative supply of nonbank credit has facilitated access to homeownership in a period when the U.S. homeownership rate has collapsed to historic lows.

Table 2.7: Nonbanks and Homeownership at the Zip Code Level

	$\Delta\text{Homeownership}_{z,11-15}$
Nonbank Share $_{z,11} \times$ FHA Share $_{z,11}$	0.030 (0.014)
County FE	Yes
Zip code controls	Yes
R-squared	0.287
Number of Observations	3519

Note: P-values are in parentheses. This table estimates equation (2.9). Subscript z denotes zip code. $\Delta\text{Homeownership}_{z,11-15}$ denotes the change of homeownership rate between 2011 and 2015 in zip code z . Nonbank Share $_{z,11}$ and FHA Share $_{z,11}$ are the 2011 share of mortgage applications which are to nonbanks and which were for FHA loans, respectively. Zip code controls are: Nonbank Share $_{z,11}$; FHA Share $_{z,11}$; the 2011 homeownership rate; the 2011 average requested loan-to-income ratio; and the 2011 share of applications from black or Hispanic borrowers. Observations are zip codes weighted by 2011 renter population.

2.7 Conclusion

In this paper we found that changes in MBS liquidity can significantly affect the size of the shadow banking sector and the amount of credit risk in the primary mortgage market. Specifically, we used variation in the cross-section of MBS premia induced by the introduction of the U.S. Liquidity Coverage Ratio (LCR) to identify the effect of MBS liquidity on the supply of nonbank credit. We show that LCR regulations, designed to prevent runs in secondary mortgage markets, have attracted nonbanks to the FHA market and lowered their lending standards. Thus, as an unintended consequence, LCR regulations may have increased the credit risk borne by U.S. taxpayers by making the FHA more exposed to nonbanks.

It is unclear how the LCR-induced increase in nonbanks' market share affects welfare. On one hand, the financial system may have become more fragile. On the other hand, the expansion in nonbank credit appears to have bolstered homeownership in a period when the U.S. homeownership rate has been at historic lows. Moreover, while the LCR shock is a focal point of our paper, we also find that MBS liquidity affects nonbanks' market share in periods without major a regulatory overhaul. This last finding shows how fluctuations in the size of the shadow banking sector are not necessarily inefficient, and they can also be a natural and routine byproduct of fluctuations in market liquidity.

Chapter 3

Financial Intermediaries as Suppliers of Housing Quality

3.1 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.⁴⁹ 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

⁴⁹According 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 3.2.

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 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.⁵⁰

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

⁵⁰On 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 2018b).

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; Kojien 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. 2018b; Buchak et al. 2018b; Fuster et al. 2018b; Irani et al. 2018b; Gete and Reher 2019).⁵¹

The remainder of the paper is organized as follows. Section 3.2 presents background facts and an organizing framework; Sections 3.3 studies the effect of credit supply on real improvement activity; Section 3.4 studies the analogous effect of private equity supply; Section 3.5 assesses implications for rent growth; and Section 3.6 concludes.

3.2 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

⁵¹Conceptually, 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).

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.

3.2.1 Data

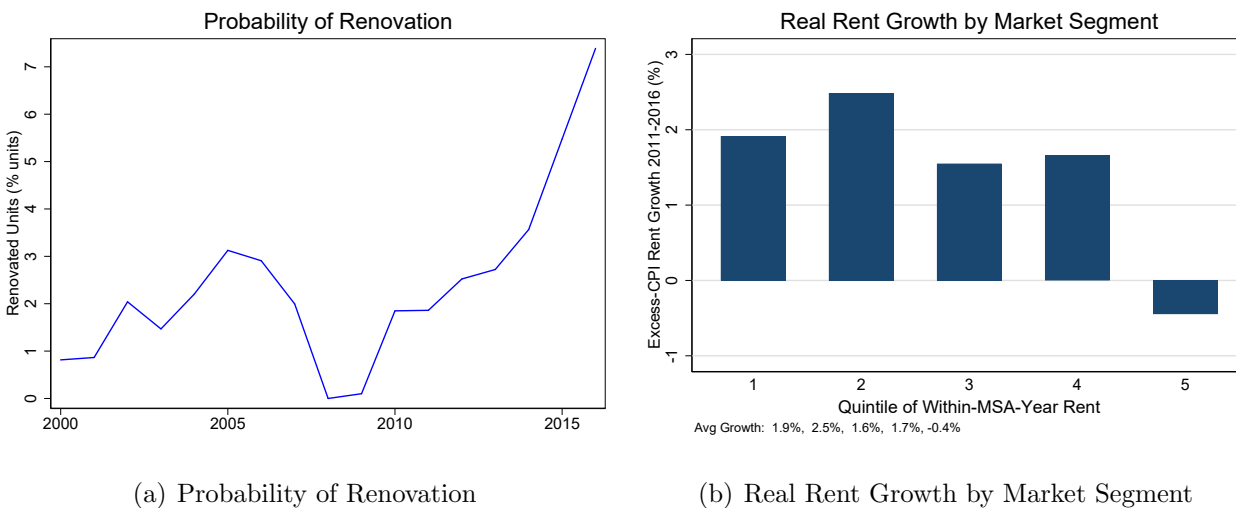
I rely on three main datasets and numerous auxiliary ones which are discussed in turn. The full details, including summary statistics, are in Appendix C.1. The datasets vary in observational unit and sample period, and I will be clear about which dataset I am using for a given analysis.

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.

3.2.2 Facts

Figure 3.1 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 C.2 replicates this finding using aggregate investment in residential improvements.⁵²

Figure 3.1: Facts about Quality Improvement Activity



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.

Panel (b) plots the cross-section of rent growth across quality segments. Using Zillow's

⁵²Relatedly, Appendix Figure C.3 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 C.4.2 for cross-sectional characteristics of improvement activity. Appendix Figure C.1 documents broader trends in rent growth referenced in the introduction.

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.⁵³

Together, these observations are consistent with an increase in the supply of improvement projects, notwithstanding a likely increase in household demand. 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.

3.2.3 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 toward improvements and away from other types of residential investment. I begin with simple theory 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 “im-

⁵³Appendix Figure C.5 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 C.4 shows how the cross-sectional distribution of log rent became more compressed over this period.

provements” 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 $F(w^I, 1 - w^I)$, 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 or consume. If they invest, they incur an adjustment cost $C(w^I)$, which depends on their allocation to improvements. Practically, $C(w^I)$ will map to a bank’s cost of funds or a pension’s actuarial liabilities. I abstract from contract design and assume that investors and intermediaries split the surplus from investment. Therefore, intermediaries solve

$$\max_{w^I} \{F(w^I, 1 - w^I) - C(w^I)\}. \quad (3.1)$$

In a frictionless world, C does not depend on w^I , and the corresponding solution to (3.1) implies that intermediaries equalize the marginal product of financing across projects.

Suppose, however, that some friction makes it relatively less costly to provide improvement financing, so that C becomes decreasing in w^I . It is straightforward to show that this friction increases intermediaries’ allocation to improvement projects.⁵⁴ In this paper’s

⁵⁴To make this point explicit, write $C(w^I) = \bar{C} + \xi \hat{C}(w^I)$ where $\hat{C}'(w^I) < 0$. Then the objective in (3.1) exhibits increasing differences in ξ and w^I , so that Topkis’ Theorem applies and the solution to (3.1) is increasing in ξ .

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 meaningful effect on the increase in real improvement activity documented in Figure 3.1.

3.3 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.

3.3.1 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.⁵⁵ HVCRE loans are for the “development or construction of real property”, which I will simply refer to as “construction”.⁵⁶ By contrast, loans for “improve-

⁵⁵If 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.

⁵⁶The 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).

ments to existing income-producing real property” were not subject to this increase and retained the substantially more modest weight of 100%.⁵⁷

Within the framework from Section 3.2.3, 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,

$$C(w^I) = (1 + R^b) [w^I + \kappa (1 - w^I)], \quad (3.2)$$

where the reservation project is construction; R^b is the effective cost of funds to finance 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.⁵⁸ 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

⁵⁷There 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.

⁵⁸Securitization 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 (2014b) for more discussion of the lending incentives associated with risk retention ratios.

construction projects (Mortgage Bankers Association 2018), consistent with the aggregate behavior shown in Appendix Figure C.6.

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.⁵⁹ 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.⁶⁰

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 Section 3.3.2, followed by a property-level specification in Section 3.3.3. Then I turn to the main county-level specification in Section 3.3.4 and discuss the aggregation procedure in Section 3.3.5. Unless otherwise stated, data used in this section come from Trepp.

⁵⁹This 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.

⁶⁰HVCRE 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 57, a formal clarification of HVCRE regulation including a full description of grandfathering status did not come until 2017.

3.3.2 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,

$$Y_{k,\ell,t} = \beta (\text{Bank}_\ell \times \text{Post}_t \times \text{Imp}_k) + \gamma (\text{Bank}_\ell \times \text{Imp}_k) + \alpha_{\ell,t} + \alpha_{k,t} + u_{k,\ell,t}, \quad (3.3)$$

where k , ℓ , and t index loan purpose, lender, and year.⁶¹ 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.⁶² The parameter of interest in (3.3) 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 .⁶³

⁶¹To avoid overweighting idiosyncratic shocks to small lenders, observations in (3.3) and the following specification (3.4) are weighted by multifamily mortgage market share over 2011-16.

⁶²Improvements 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 C.1 for details on how I classify improvement and construction loans.

⁶³I 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.

The main drawback to (3.3) 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 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

$$Y_{\ell,t} = \beta (\text{Bank}_{\ell} \times \text{Post}_t) + \alpha_t + \alpha_{\ell} + \gamma X_{\ell,t} + u_{\ell,t}, \quad (3.4)$$

where the notation is the same as in (3.3), 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 (3.3). Omitted variables related to a lender's business model may still lead to bias in (3.4). However, Appendix Figure C.7 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 (3.3) are in columns 1-2 of Table 3.1. 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.⁶⁴ 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

⁶⁴Using 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.

intensive margin. Figure 3.2 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 3.1 report the results of (3.4). My outcome of interest is the log number of renovated units financed by new loans. Studying this outcome facilitates continuity with the county-level analysis in Section 3.3.4, 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 C.2 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.

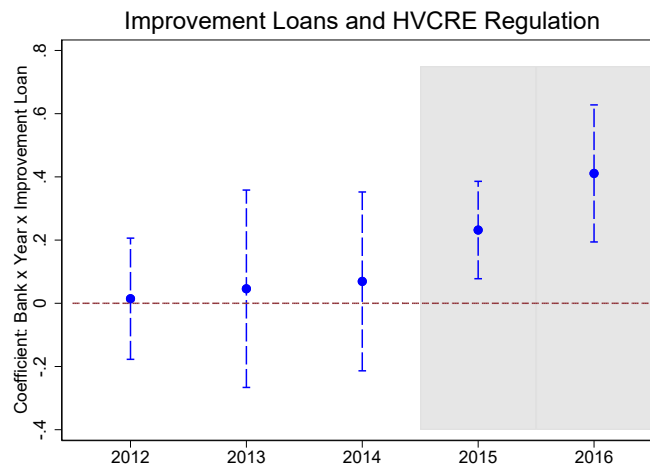
In column 5, I test the theory that banks' response to the regulation depends on their securitization technology. I interact the treatment variable, $\text{Bank}_\ell \times \text{Post}_t$, with a measure of the lender's securitization sluggishness in the pre-2011 period, Sec Lag_ℓ , normalized to have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

Table 3.1: Improvement Financing by HVCRE-Affected Lenders

Specification:	Triple Difference-in-Difference		Difference-in-Difference		
Outcome:	$\log(\text{Loans}_{k,\ell,t})$	$\log(\text{Volume}_{k,\ell,t})$	$\log(\text{Renovated Units}_{\ell,t})$		
	(1)	(2)	(3)	(4)	(5)
$\text{Bank}_\ell \times \text{Post}_t \times \text{Imp}_k$	0.281** (0.142)	5.329** (2.024)			
$\text{Bank}_\ell \times \text{Post}_t$			1.210** (0.528)	1.142** (0.521)	1.564** (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 \times 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 (3.3) and columns 3-5 estimate equation (3.4). 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 \times 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. $\text{Renovated Units}_{\ell,t}$ is the number of renovated housing 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.

Figure 3.2: Improvement Financing and the Timing of HVCRE Regulation



Note: This figure plots the estimated coefficients from a variant of equation (3.3). 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 3.1. 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.

warehouse period, and, consistent with the theory described above, their estimated response to the regulation is much stronger.

Appendix C.2.2 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.

3.3.3 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 C.4.1 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 3.4.

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

$$Y_{i,\ell,t} = \beta (\text{Bank}_\ell \times \text{Post}_t) + \alpha_{c(i),t} + \alpha_{i,\ell} + \gamma X_{z(i),t} + u_{i,\ell,t}, \quad (3.5)$$

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.⁶⁵ 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 (3.5) 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

⁶⁵It 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.

the treatment effect β in (3.5) requires a “parallel trends” assumption: bank and nonbank-financed properties do not differ in ways that would affect improvement activity after the introduction of HVCRE regulation. Explicitly, my identification assumption is

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

where the conditioning arguments $\alpha_{c(i),t}$ and $\alpha_{i,\ell}$ make clear that identification comes from within the 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 C.7 and its associated discussion in Section 3.3.2, 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 3.2 has the results of (3.5). 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 show 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.

Table 3.2: Property-Level Improvement Activity and HVCRE Regulation

Outcome:	Renovation $_{i,\ell,t}$	
	(1)	(2)
Bank $_{\ell} \times$ Post $_t$	0.012** (0.004)	0.012** (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 (3.5). Bank $_{\ell}$ 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.

Appendix C.2.3 confirms Table 3.2 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 3.2.

3.3.4 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,

$$Y_{c,t} = \beta (\text{Bank Share}_c \times \text{Post}_t) + \alpha_c + \alpha_t + \gamma X_{c,t} + u_{c,t}, \quad (3.6)$$

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 2015.⁶⁶ 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

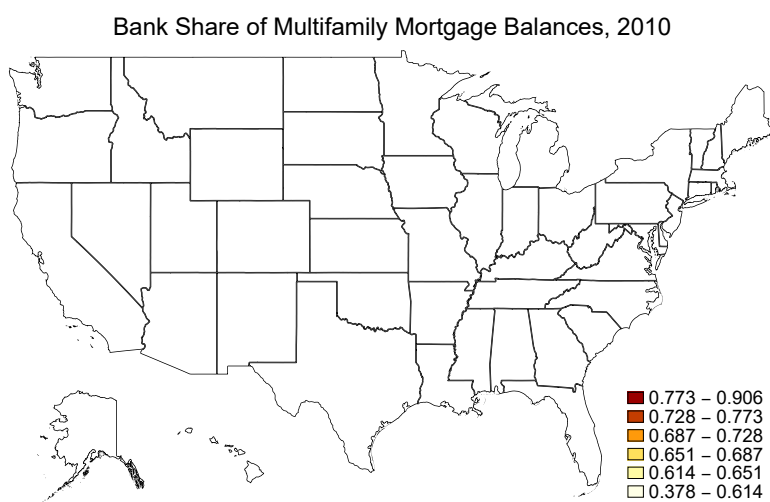
$$\mathbb{E}[\text{Bank Share}_c \times \text{Post}_t \times u_{c,t} | \alpha_c, \alpha_t, X_{c,t}] = 0.$$

This assumption would be violated if, for example, there is a secular trend in improvement activity and banks locate in high income markets which, per the discussion in Section 3.5, 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

⁶⁶The 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 3.1. To avoid overweighting idiosyncratic shocks to small counties, I weight observations in (3.4) by average number of multifamily units over 2011-16.

concern since, unlike originations, balances reflect expectations that were formed longer in the past. More substantively, Figure 3.3 plots the geographic distribution of banks' initial market share across states. The distribution is fairly uniform, and this uniformity is also borne out when zooming into the county-level within high growth states, shown in Appendix Figure C.8.

Figure 3.3: Geographic Distribution of Initial Bank Share

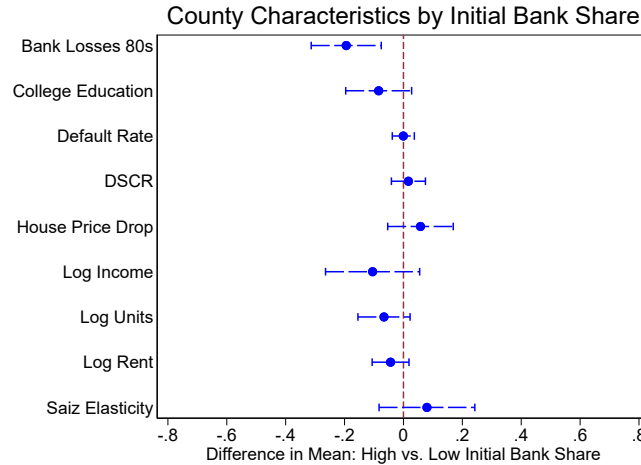


Note: This figure plots banks' share of multifamily mortgage balances in 2010 across states. Data are from Trepp.

The importance of borrower-lender relationships suggests that historical episodes may partly determine banks' market share. To that end, Figure 3.4 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 3.3, 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.

Figure 3.4: 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 (2010b) elasticity of housing supply. The remaining variables are those from Table 3.3 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 C.1.

Table 3.3 has the results of the baseline specification (3.6). 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.⁶⁷ In Figure 3.5, 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. 2018b). However, once HVCRE regulation is introduced, bank dominated counties see a substantial increase in improvement activity.

Table 3.3: County-Level Improvement Activity and HVCRE Regulation

Outcome Measure:	log (Renovation Measure _{c,t})				
		Properties		Housing Units	Revenue
	(1)	(2)	(3)	(4)	(5)
Bank Share _c × 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

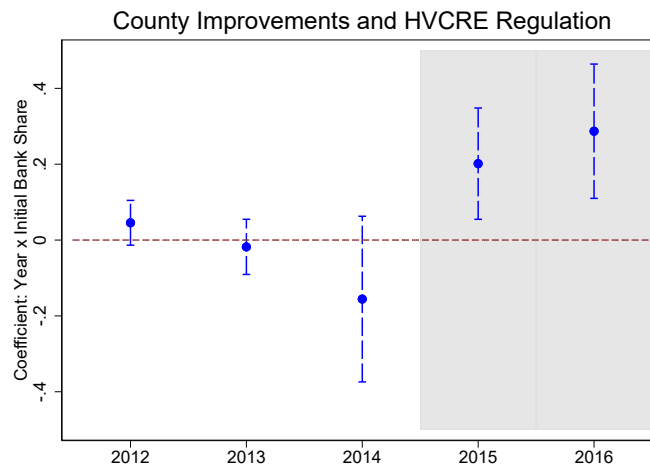
Note: Subscripts c and t denote county and year. This table estimates equation (3.6). Bank Share_c is banks’ share of multifamily mortgage balances in 2010. Post_t indicates if t is greater than or equal to 2015. The outcome is the log of a measure of renovation activity: columns 1-3 use the number of 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 3.1. 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.

In column 4, I study log number of renovated housing units. The result is qualitatively

⁶⁷Applying 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 × 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.

similar to its counterpart in column 3, and the larger point estimate likely reflects the economies of scale discussed in the context of Table 3.1. 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 (2019b). I conduct additional robustness exercises in Appendix C.2.1.

Figure 3.5: Improvement Activity and the Timing of HVCRE Regulation



Note: This figure plots the estimated coefficients from a variant of equation (3.6). 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 3.3. 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.

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 3.4 investigates these predictions by reestimating column 2 of Table 3.3, interacting the treatment variable $\text{Bank Share}_c \times \text{Post}_t$ with relevant county characteristics.⁶⁸

Table 3.4: Heterogeneous Effects Across Counties

Outcome	$\log(\text{Renovated Properties}_{c,t})$			
	(1)	(2)	(3)	(4)
$\text{Bank Share}_c \times \text{Post}_t$	0.291** (0.110)	0.279** (0.103)	0.213** (0.082)	0.271** (0.106)
$\text{Bank Share}_c \times \text{Post}_t \times \text{Interaction}_c$	0.181* (0.104)	-0.170** (0.078)	-0.185* (0.097)	-0.282* (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 (3.6). The specification is the similar to column 2 of Table 3.3 after interacting $\text{Bank Share}_c \times \text{Post}_t$ 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.

I focus on the economic intuition associated with $\text{Bank Share}_c \times \text{Post}_t \times \text{Interaction}_c$

⁶⁸The 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.

in Table 3.4. 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 3.5. 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 Appendix Table C.1. 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 3.1b.

3.3.5 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 2016b). The incentive to substitute toward improvement loans is stronger in the latter case, which implies that the observed effect is a lower bound on the true one. To substantiate this logic, Appendix C.2.4 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 3.3 indeed provides conservative estimates.⁶⁹

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,

$$\text{Effect}^{\text{Sample}} = \frac{\sum_c \sum_{t \geq 2015} \text{Renovated Housing Units}_{c,t} \times \left[1 - e^{-\beta^{\text{HVCRE Bank Share}_c}} \right]}{\sum_c \sum_{t \geq 2015} \text{Renovated Housing Units}_{c,t}}, \quad (3.7)$$

where c and t index counties and years in the sample, $\text{Renovated Housing Units}_{c,t}$ is the

⁶⁹Specifically, I reestimate a version of Table 3.3 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 3.3 as conservative. Appendix C.2.4 also uses the within lender-year specification (3.3) to show that banks reduce the rate at which they securitized improvement loans following regulation, suggesting the in-sample estimates are biased toward zero.

number of renovated units in the sample, and β^{HVCRE} is the estimate from column 4 of Table 3.3.⁷⁰

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 C.6, and the large increase in renovation probability shown in Figure 3.1a.

3.4 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.

⁷⁰I reason on improved housing units, as opposed to properties, because they are the more relevant level of analysis in the context of the quality-adjustment exercise from Section 3.5. They can also be mapped to aggregate data.

3.4.1 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, 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 3.2.3 to this setting. I interpret the production function F from equation (3.1) as describing a pension’s actuarial

assets. The cost C represents the pension’s actuarial liabilities, which can here be written

$$C(w^I) = \frac{\text{Obligations}}{w^I (\bar{R}^I - \bar{R}^f) + \bar{R}^f}, \quad (3.8)$$

where the numerator in (3.8) represents obligations to future pensioners; the denominator in (3.8) 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 (3.8) 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 C.13, and they typically take an equity stake in residential investment projects.⁷¹ Conveniently, funds are 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 per-

⁷¹Private 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 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.

formed by “core” funds, and improvements are performed by “value added” funds.⁷² Panel (a) of Figure 3.6 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).

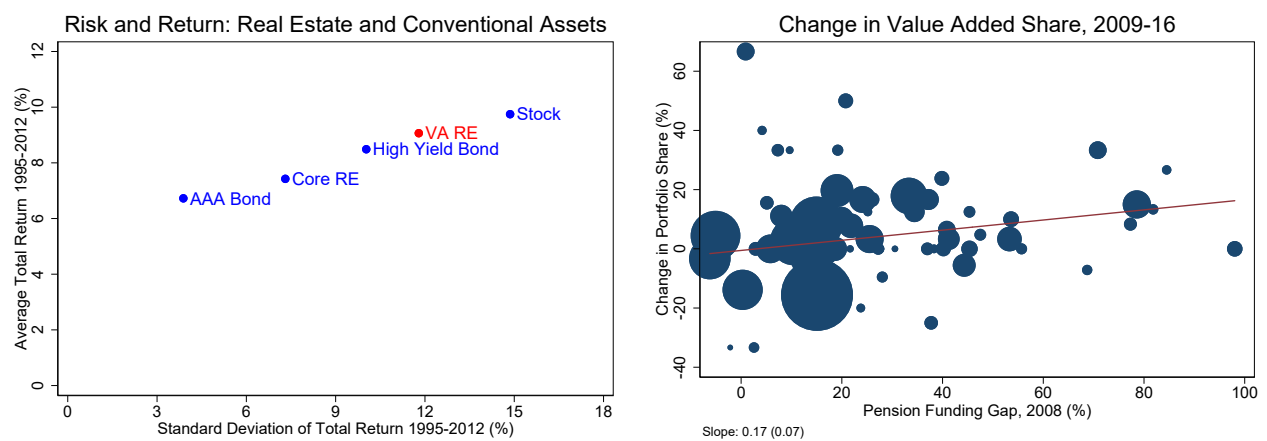
Applying public pensions’ risk-shifting behavior to real estate, one would expect more underfunded pensions to tilt their portfolio toward riskier improvement-oriented (“value added”) real estate funds and away from safer buy-and-hold (“core”) funds during a period of declining safe yields. Panel (b) of Figure 3.6 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.⁷³ Conversely, they decrease their allocation to buy-and-hold funds, as documented in Appendix Figure C.14.

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

⁷²There 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.

⁷³The 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.

Figure 3.6: 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.

roughly 40% of limited partners as shown in Appendix Figure C.15. Second, there is considerable fundraising stickiness between private equity real estate fund managers and their limited partners (e.g. public pensions). For example, Appendix C.4.1 shows how the probability a fund manager turns to an existing limited partner in her next fundraising round is 22 pps higher than what one would predict based on the limited partner’s market share. Like in the credit supply analysis, relationship stickiness is what enables me to identify the real effects of pension portfolio reallocation. In light of Figure 3.6, 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.⁷⁴

⁷⁴There 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.3.2 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 72.

3.4.2 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

$$Y_{p,t} = \beta (\text{Funding Gap}_p \times \text{Yield Gap}_t) + \alpha_t + \alpha_p + \gamma X_{p,t} + u_{p,t}, \quad (3.9)$$

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 .⁷⁵ 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), and other time-varying characteristics. I estimate (3.9) 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 3.5 contain the results of the pension-level specification (3.9). 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 $\text{Prob of Commitment}_{p,t}^{\text{VA}}$.

⁷⁵I measure the safe yield using the yield on a 10-year TIPS bond. Note that (3.9) 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.

The treatment variable $\text{Funding Gap}_p \times \text{Yield Gap}_t$ 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.3.2. Thus, relatively-underfunded pensions appear to respond to declining safe yields by reallocating their real estate portfolio toward funds that perform riskier projects.

Table 3.5: Value Added Investment and Public Pension Risk Taking

Specification:	Pension-Level		Manager-Level	
Outcome:	Prob of Commitment $_{p,t}^{\text{VA}}$		Fund Formed $_{m,t}^{\text{VA}}$	log (Investment $_{m,t}^{\text{VA}}$)
	(1)	(2)	(3)	(4)
Funding Gap $_p \times \text{Yield Gap}_t$	0.169** (0.059)	0.155** (0.061)		
Funding Gap $_m \times \text{Yield Gap}_t$			0.070** (0.021)	1.223** (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 (3.9) and columns 3-4 estimate equation (3.10). 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 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 Formed $_{m,t}^{\text{VA}}$ indicates whether m formed an improvement-oriented fund for U.S. residential real estate with vintage t . 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 Prequin and the CRR.

To be clear, (3.9) does not seek to test the causal effect of low safe yields on public pension real estate investment. Rather, (3.9) 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 instrument 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 C.13 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.3.

3.4.3 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 (3.9), I next estimate

$$Y_{m,t} = \beta (\text{Funding Gap}_m \times \text{Yield Gap}_t) + \alpha_t + \alpha_m + u_{m,t}, \quad (3.10)$$

where m and t index private equity real estate fund manager and year, and Funding Gap_m is the average of its analogue from (3.9) across m 's limited partners. To interpret, treated units in (3.10) 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).⁷⁶

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 (3.10) 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

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

Appendix Figure C.12 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 located 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 (3.10). The outcome in column 3 is the annual probability of forming an improvement-oriented fund, which I denote $\text{Fund Formed}_{m,t}^{\text{VA}}$. Interpreting the point estimate, managers with a 1 standard deviation higher pension investment shift, $\text{Funding Gap}_m \times \text{Yield Gap}_t$, have a 7.0 pps, or 0.3

⁷⁶The 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 (3.10) by the manager's total real estate capital raised over 2009-16.

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 (3.3). Consistent with reallocation at the pension level, the results discussed in Appendix C.3.8 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 C.4.1 for additional discussion.

3.4.4 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 92nd percentile of funding status that year. Using the point estimate from column 4 of Table 3.5, 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 3.3.5 and described in detail in Appendix C.3.10. 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).

3.5 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 C.16 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 3.3 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.

3.5.1 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 3.2.1, 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

$$\Delta \log(\text{Rent}_{i,t}) = \beta^\Theta \Delta \Theta_{i,t} + \alpha_i + \alpha_t + u_{i,t}, \quad (3.11)$$

where i and t index housing units and years, $\Delta \log(\text{Rent}_{i,t})$ is the change in log rent, and $\Delta \Theta_{i,t}$ is a vector of indicators for the installment of features $\theta_{i,t} \in \Theta_{i,t}$.⁷⁷ Thus, (3.11) 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 for the possibility that improvements only occur in some locations or in certain years.

Given the estimates from (3.11), shown in Appendix Table C.18, I compute a unit’s quality-adjusted rent as

$$\text{Rent}_{i,t}^H = \text{Rent}_{i,t_0} \times e^{\sum_{\tau=t_0+1}^t [\Delta \log(\text{Rent}_{i,\tau}) - \beta^\Theta \Delta \Theta_{i,\tau}]} \quad (3.12)$$

⁷⁷The 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.

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

$$\pi_t^H = \frac{\sum_{i \in \mathcal{I}} \text{Rent}_{i,t}^H}{\sum_{i \in \mathcal{I}} \text{Rent}_{i,t_0}}. \quad (3.13)$$

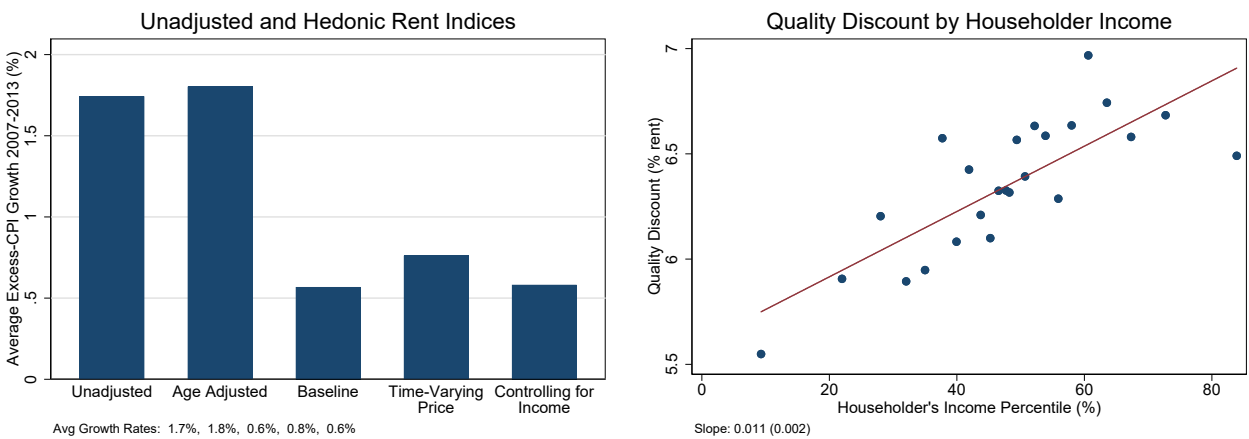
As described in Appendix C.1, I drop units that experienced a change in tenure (e.g. “condo conversions”) from my analysis. The aggregation in (3.13) has the same basic form as that used by the BLS after accounting for the fact that I work at a biennial frequency (Gallin and Verbrugge 2007).

Figure 3.7a summarizes 2007-13 annual growth in π_t^H and other related indices. The baseline hedonic index, shown in the center of the figure, saw 0.6% real growth. Moving to the left, I perform an age adjustment similar to that used by the BLS and described in Reher (2019b). 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.⁷⁸ 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 (2019b), 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 3.7a perform two robustness checks.

⁷⁸Average annual real growth in the CPI’s rent of primary residence was 0.7%, which is within a standard range of the growth rates in Figure 3.7 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).

Figure 3.7: 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 (2019b). Baseline denotes the hedonic index from (3.13). Time-Varying Price denotes the baseline index after allowing the coefficients in (3.11) 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.

First, I reestimate (3.11) after allowing the price vector β^Θ to vary by year. This results in a similar growth rate of 0.8%. The slight increase means that the price of quality improvements is lower when more of them occur, consistent with the existence of supply 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 (3.11). This leaves the growth rate almost unchanged at 0.6%, which is again consistent with a role for supply. Appendix Figure C.17 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 C.18.

In panel (b) of Figure 3.7, 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.⁷⁹ There is a strong positive relationship, and, as in Table 3.4, 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 (2019b) 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).

⁷⁹The 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.

3.5.2 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 3.3 and 3.4 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 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 3.3.4 discussed evidence that greater supply of credit for improvement projects has increased homelessness. Third, Figure 3.7b 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.

3.6 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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Appendix A

Appendix to Chapter 1

A.1 Data Sources

In this section, we describe our data sources, how we cleaned them, and the key variables used in our analysis.

Housing Rents and Prices

Our rent data cover 302 MSAs from 2007 through 2014. Data for rents and prices are from Zillow. To measure rents, we use the Quarterly Historic Metro Zillow Rent Index (ZRI). The ZRI measures the median monthly rent for each MSA and has units of nominal dollars per month. Zillow imputes this rent based on a proprietary machine learning model taking into account the specific characteristics of each home and recent rent listings for homes with similar characteristics. Importantly, the ZRI does not impute a property's rent from its price. The median rent is computed across all homes in an MSA, not only those which are currently for rent. Thus, unlike pure repeat-listing indices, the ZRI is not biased

by the current composition of for-rent properties. To measure house prices, we use the Quarterly Historic Metro Zillow Home Value Index (ZHVI). The ZHVI is computed using a methodology analogous to that of the ZRI. Although the ZRI and the ZHVI are available quarterly, we only retain the values corresponding to the fourth quarter of each year because our mortgage data are at the yearly frequency. To measure the price of starter homes, we use the Zillow's Bottom Tier Index, which measures the median house price among homes in the bottom third of the market.

We merge all datasets based on year and the MSA's 2004 core based statistical area (CBSA) code. For sub-metro areas of the largest MSAs, we use the CBSA division code. After merging with the MSAs for which we have the mortgage data described below, we have rent data for 302 MSAs.

Mortgage Data

Data on mortgage credit come from the Home Mortgage Disclosure Act (HMDA). The frequency of the data is yearly. HMDA data contain application-level information on the requested loan size, loan purpose, property type, and application status. We observe the self reported income, race, and gender of the borrower, as well as an identifier of the lender receiving the application. Since our focus is on how credit affects rents through housing tenure choice, we only retain mortgage applications for the purchase of a 1-to-4 family, owner-occupied home. In terms of HMDA variables, we retain applications satisfying the following conditions: occupancy = 1 (owner occupied), property type = 1 (1-to-4 family), loan purpose = 1 (for-purchase), and action taken \neq 6 (loan not purchased by institution).

To maximize data quality, we additionally require that applications were not flagged for data quality concerns (edit status = "NA") and have a non-empty MSA code. We identify denied and originated loans as those with action taken = 3 and action taken = 1, respectively. FHA loans are those with loan type = 2.

Our data on MSA population and income also come from HMDA as part of the FFIEC Census Report. The FFIEC directly reports median family income for each MSA and census tract, and the population for each census tract. We compute MSA-level population by summing across census tracts belonging to an MSA. In terms of demographics, we identify applicants as black if the applicant's primary race = 3 and as Hispanic if the applicant's primary race = 5 and the applicant's ethnicity = 1.

Some lenders require applicants to go through a pre-approval process before allowing them to formally apply. After excluding applications that underwent pre-approval, the denial rate over 2008-2014 was 13%; since this is close to the unconditional average of 11.1%, we perform our analysis including applications that underwent pre-approval beforehand, around 15% of the sample. We checked that this decision does not affect the results.

We merge the HMDA's application-level data by lender and year with the HMDA reporter panel. The reporter panel contains each lender's name, total assets, and top holding company. Within each year, we classify a lender as belonging to the Big-4 if its top holding company is one of the Big-4 banks. To account for slight changes in institutional names over time, we identify the Big-4 banks as those whose names possess the strings "WELLS FARGO", "BANK OF AMERICA", "CITIG", or "JP". Using our classification scheme, if a Big-4 bank acquires another institution in, say, 2010, then that institution would be classified as a non-Big-4 lender in 2009 but as belonging to the Big-4 in 2010. We computed

the top 20 share using the shares of mortgages originated in 2007, like D'Acunto and Rossi (2017).

Similarly, we classify lenders in HMDA as being subject to a CCAR stress test between 2011-2015 if their top holder was subject to this test. These holding companies are: Ally Financial Inc, American Express Co, Bancwest Co, Bank of America Corp, Bank of NY Mellon Corp, BB&T Corp, BBVA Compass Bancshares, BMO Financial Corp, Capital One Financial Corp, Citigroup Inc, Comerica Inc, Deutsche Bank, Discover Financial Services, Fifth Third Bancorp, Goldman Sachs Group, HSBC North America Holdings Inc, Huntington Bancshares Inc, JP Morgan Chase & Co., Keycorp, M&T Bank Corp, MetLife Inc, Morgan Stanley, Northern Trust Corp, PNC Financial Services Group Inc, RBS/Citizens, Regions Financial Corp, Santander Holdings USA Inc, State Street Corp, Suntrust Banks Inc, TD Group US Holdings LLC, MUFG Americas Holding Corp, US Bancorp, Wells Fargo & Co, and Zions Corp.

Deposit, Homeownership and Vacancy Data

To obtain deposit shares we use the FDIC's Summary of Deposits. We first group Big-4 and non Big-4 banks together and aggregate deposits for each group to the MSA level, using the variable DEPSUMBR.

Our data on licensing rules for mortgage brokers come from Backley et al. (2006), according to whom, as of 2006, 48 states require mortgage brokerage firms to carry a license, while 18 states impose the additional requirement that individual brokers also be licensed. These 18 states are Arkansas, California, Florida, Hawaii, Idaho, Louisiana, Maryland, Mon-

tana, Nevada, North Carolina, Ohio, Oklahoma, South Carolina, Texas, Utah, Washington, West Virginia and Wisconsin.

Homeownership data come from the U.S. Census Bureau's Housing Vacancy Survey (HVS). The HVS is a supplement of the Current Population Survey (CPS) to provide current information on the rental and homeowner vacancy rates. These data are used extensively by public and private sector organizations. They cover 60 MSAs over our sample period. We only retain the fourth-quarter value for homeownership rates, to match the annual frequency of our mortgage data. In Table 1.3 we approximate the 2009 value using the 2010 Census value, which covers more MSAs but it is decennial.

Other Variables

We also rely on the following data sources:

- Age data, unemployment data, and labor force participation data at the MSA level are from the American Community Survey 1-Year Estimates, provided by the U.S. Census Bureau. This is also our source of data for the share of workers in financial services. Since the American Community Survey 1-Year Estimates did not exist before 2005, for the pre-crisis analysis we instead use controls from the 2000 Census and log median household income as imputed by Zillow.
- Data on establishment growth come from the Business Dynamics Statistics.
- Data on MSA-level real GDP growth come from the Bureau of Economic Analysis.
- Data on MSA-level wage growth come from the Bureau of Labor Statistics.

- Data on manufacturing industry shares used to construct the Adelino, Ma, and Robinson (2017) shock come from the County Business Patterns dataset.
- Data on tenure conversion rates come from the American Housing Survey. The conversion rate is defined as the fraction non-owner occupied units that were converted from owner occupied units over the given time period, excluding all vacant units. We focus on 2011-2013 because the survey is conducted in odd numbered years.
- Data on multifamily permits come from the Census Bureau’s annual Building Permits Survey. We define multifamily units as the sum of 2 unit shelters, 3-4 unit shelters, and 5+ structure shelters.
- Our data on conforming loan limits is at the county-year level and begins in 2008. The data are provided by the Federal Housing Finance Agency (FHFA). We merge this dataset to our HMDA dataset by county and year. Then we collapse them to the MSA-year level. For MSAs that have counties with different conforming loan limits, we take the application-weighted average conforming loan limit among counties.

To summarize, there are 257 MSAs with a full set of controls, mortgage, and rent data which we use in the core cross-sectional regressions.

A.2 Panel Analysis

In this appendix we discuss in detail the panel analysis summarized in Section 1.4.7.

A.2.1 Lenders' Propensity to Deny

Following the methodology of Khwaja and Mian (2008), we estimate a fixed effect for a given lender or group of lenders. Specifically, let L denote the set of lenders we observe in HMDA, and consider a partition of L into disjoint subsets l_1, l_2, \dots, l_n . For example, we can partition lenders according to whether or not they are held by a Big-4 bank, corresponding to $l_1 = \{\text{Big-4}\}, l_2 = \{\text{non Big-4}\}$.

To extract a credit supply shock experienced by lenders of set l_j , we estimate the probability of loan denial at the application level, $\Pr(\text{Denied}_{i,m,t,l_j} = 1)$, as a linear probability model,

$$\Pr(\text{Denied}_{i,m,t,l_j} = 1) = \sum_j \Lambda_{t,l_j} + \gamma X_{i,m,t,l_j} + \alpha_{m,t} + \alpha_{m,l_j}, \quad (\text{A.1})$$

where our focus is on the Λ_{t,l_j} , which is a vector of fixed effects for lenders of set l_j in year t .⁸⁰ The controls in X_{i,m,t,l_j} account for the characteristics of borrowers: income, requested loan-to-income, and race of borrower i applying for a loan from lender type l_j in MSA m in year t .⁸¹ The terms $\alpha_{m,t}$ and α_{m,l_j} control for lender, time, and regional shocks. The value $\alpha_{m,t}$ is the coefficient on an indicator variable which equals 1 if the borrower applies from MSA m in year t and equals 0 otherwise. Likewise the indicator variable α_{m,l_j} equals 1 if the borrower applies from MSA m to a lender of type l_j and equals 0 otherwise.

The vector Λ_{t,l_j} captures the lender specific component of denial rates. For example, it may reflect a higher cost of funds or greater regulatory risk borne by lenders of set l_j in

⁸⁰We estimate the Λ_{t,l_j} using a series of indicator functions for whether the application was received by lenders of set l_j in year t . The reference category will be applications to lenders of some set l_r in some year t_r .

⁸¹We use 21,709,935 observations to estimate (A.1) over 2007-2014.

a given year. Importantly, Λ_{t,l_j} does not confound either borrower or regional effects, since these are already captured by X_{i,m,t,l_j} and the pair $(\alpha_{m,t}, \alpha_{m,l_j})$, respectively. To emphasize this interpretation, we refer to Λ_{t,l_j} as the propensity to deny.

A.2.2 Panel Instruments

We use four instruments to conduct the panel analysis. The first two are based on the denial propensities for Big-4 and stress tested lenders. First, we proceed by estimating (A.1) using the partition $L = \{\text{Big-4}, \text{NonBig-4}\}$ and construct the Big-4 shock as

$$V_{m,t} = (\Lambda_{t,\text{Big-4}} - \Lambda_{t,\text{NonBig-4}}) \times \text{Big-4 Deposit Share}_{m,08}. \quad (\text{A.2})$$

In words, $V_{m,t}$ captures the relative stringency of the Big-4's approval standards in a given year $(\Lambda_{t,\text{Big-4}} - \Lambda_{t,\text{NonBig-4}})$ and the degree to which this tightening is felt in a given MSA as measured by the share of deposits in 2008 held with Big-4 banks (Big-4 Deposit Share $_{m,08}$). The results are similar if we instead use the Big-4's share of branches in an MSA.

Second, we use the partition $L = \{\text{Tested}, \text{NotTested}\}$ to estimate (A.1) and analogously define the stress test shock as

$$S_{m,t} = (\Lambda_{t,\text{Tested}} - \Lambda_{t,\text{NotTested}}) \times \text{Stress Test Share}_{m,08}. \quad (\text{A.3})$$

As in our cross-sectional analysis, we define stress-tested lenders as those which underwent a CCAR test between 2011-2015, and Stress Test Share $_{m,08}$ as the 2008 mortgage application share of these lenders. The interpretation of $S_{m,t}$ is similar to that of $V_{m,t}$, in that it captures

the relative stringency of stress-tested lenders in a given year and an MSA’s exposure to those lenders.

The third instrument does not partition the set of lenders L according to regulatory criteria. This addresses any concern that we impose the wrong prior on which lenders are subject to common credit supply shocks. In the spirit of Greenstone, Mas and Nguyen (2015) or Amiti and Weinstein (2013), we estimate a separate fixed effect $\Lambda_{t,k}$ for each lender $k \in \{1, \dots, 20\}$ among the top 20 by national application share in year t , and an additional fixed effect $\Lambda_{t,21}$ for the remaining lenders, collectively.⁸² We then define the credit supply shock $G_{m,t}$ as

$$G_{m,t} = \sum_{k=1}^{21} \Lambda_{t,k} \times \text{Share}_{k,m,t}, \quad (\text{A.4})$$

where $\text{Share}_{k,m,t}$ denotes the mortgage application share of lender k from MSA m in year t .⁸³

Our fourth instrument follows Loutskina and Strahan (2015). Lenders are more willing to approve loan applications below the conforming loan limit because they come with an implicit guarantee from the Government Sponsored Enterprises. Prior to 2008, changes in these limits were determined at the national level. The 2008 Economic Stimulus Act revised this methodology so that changes in the conforming loan limit are now tied to the cost of living in a given county. To account for this, we compute national average conforming limit excluding MSA m . Then, for MSA m , we use the fraction of mortgage applications

⁸²For computational simplicity, we estimate the denial propensity (A.1) year-by-year. The reference lenders for each year are those outside the top-20, l_{21} .

⁸³We do not use application shares from some base year because it is not always clear how to track individual lenders over time. For example, Taylor, Bean & Whitaker was a top-20 lender in 2008, but shut down its operations in 2009.

from MSA m in year $t - 1$ within 5% of this national average. By excluding MSA m when computing the national average, we avoid capturing the local factors driving changes in the conforming limits, as with the instruments used by Loutskina and Strahan (2015).

We use the time-varying credit supply instruments to estimate (1.2). Since Figures A4 and A5 indicated that much of the temporal variation in credit tightness occurred after 2010, we begin the analysis in 2009. Our MSA-year controls in $X_{m,t}$ are the lagged first-difference in: log median household income, log median inhabitant age, log population, and the unemployment rate. We intentionally exclude lagged rent growth as a control because models with lagged dependent variables are usually misspecified (Angrist and Pischke 2009), and we cluster standard errors by MSA to allow for serial correlation throughout our sample period.⁸⁴ Finally, we follow Favara and Imbs (2015) and lag our credit supply shocks by one period.⁸⁵

A.2.3 Validity of the Panel Instruments

Table A.7 reestimates (1.2) after individually removing each one of the instruments. Regardless of which instrument we remove, the point estimates are consistently significant and between 2.0 and 2.1. Moreover, we perform the difference-in-Sargan test that the removed instrument is exogenous. The corresponding C -statistics are highly insignificant across specifications, which suggests that the instruments are valid. Taken together, our results from this section and our cross-sectional analysis suggest that a 1 percentage point increase in

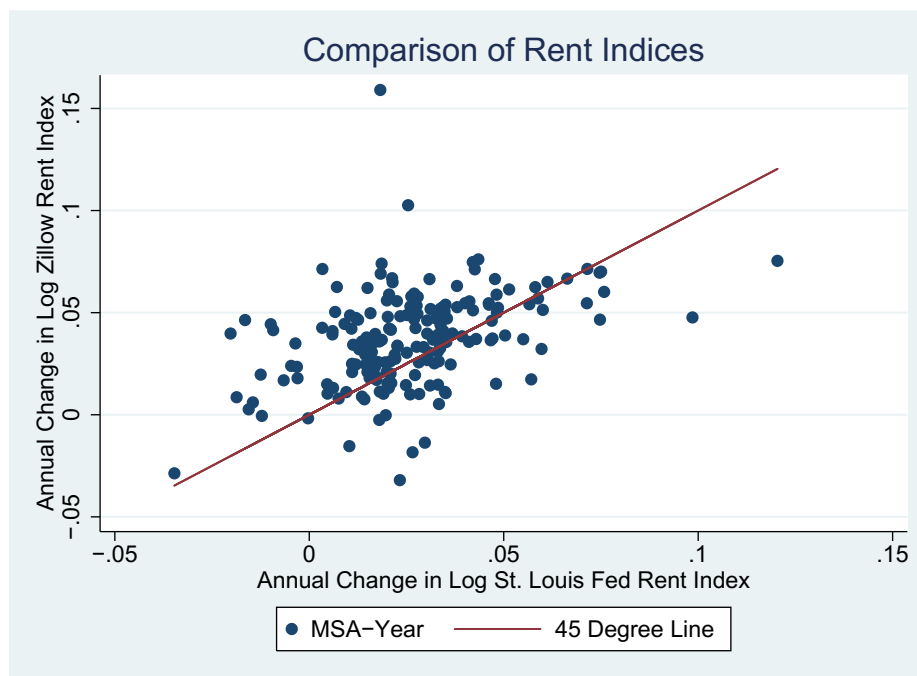
⁸⁴We thank an anonymous referee who brought both points to our attention.

⁸⁵For example, we use $V_{m,t-1}$ as an instrument for $\Delta \text{Denied}_{m,t} \equiv \text{Denied}_{m,t} - \text{Denied}_{m,t-1}$.

denial rates has led to between a 1.3 and 2 percentage point increase in annualized rent growth over the post-crisis period.

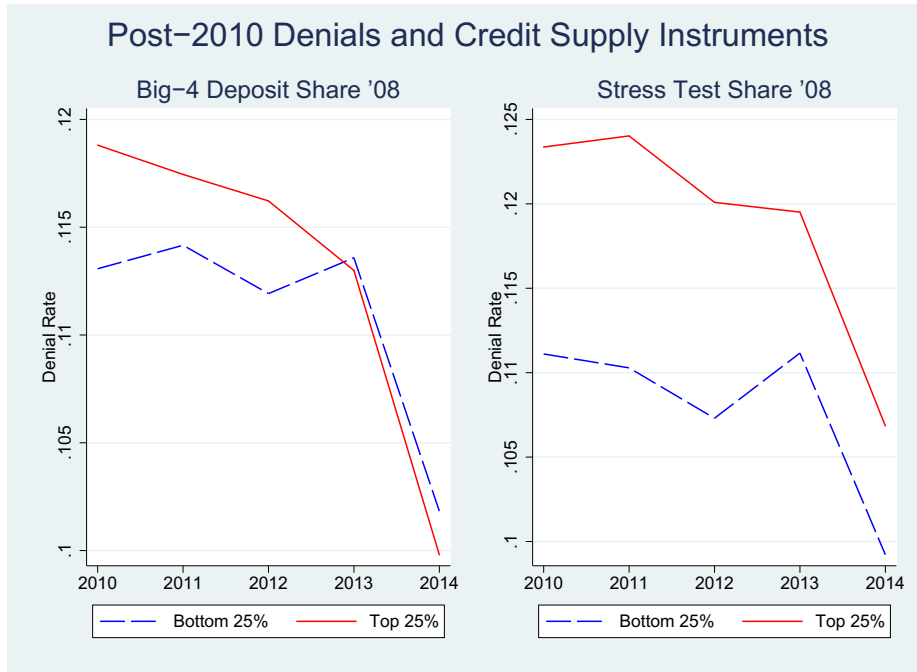
A.3 Additional Figures and Tables

Figure A.1: Comparison of Rent Indices



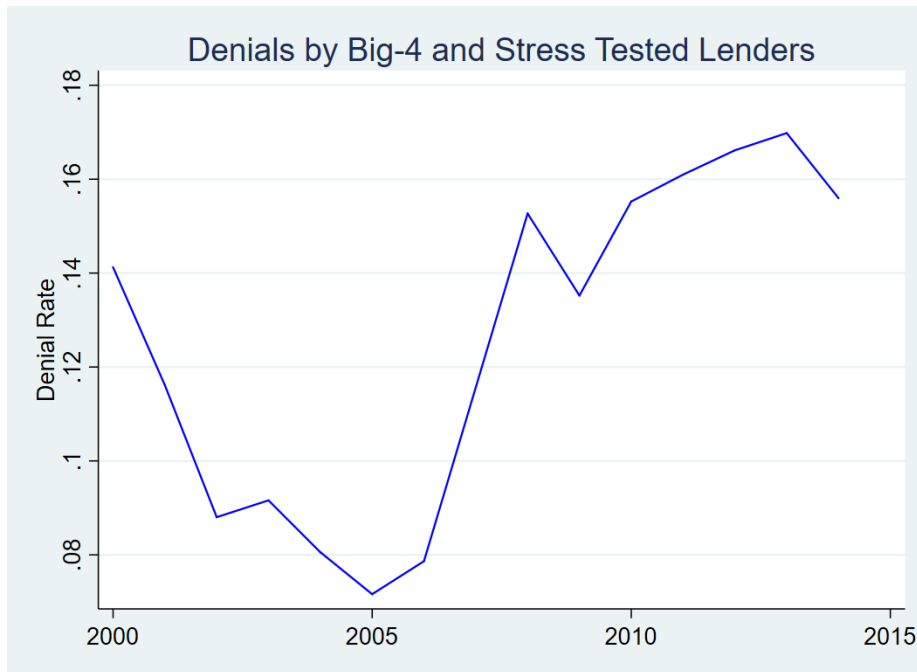
Note: This figure plots annual change in log rents based on the Zillow Rent Index, used in this paper, and the St. Louis Fed Rent Index over 1983-2015. The St. Louis Index covers 10 MSAs (Atlanta, GA; Anchorage, AK; Phoenix-Mesa, AZ; Kansas City, MO; Pittsburgh, PA; Honolulu, HI; Minneapolis-St. Paul, MN; San Diego, CA; Tampa Bay-St. Petersburg-Clearwater, FL; St. Louis, MO). The 45-degree line is in red.

Figure A.2: Post-2010 Denial Rates and Credit Supply Instruments



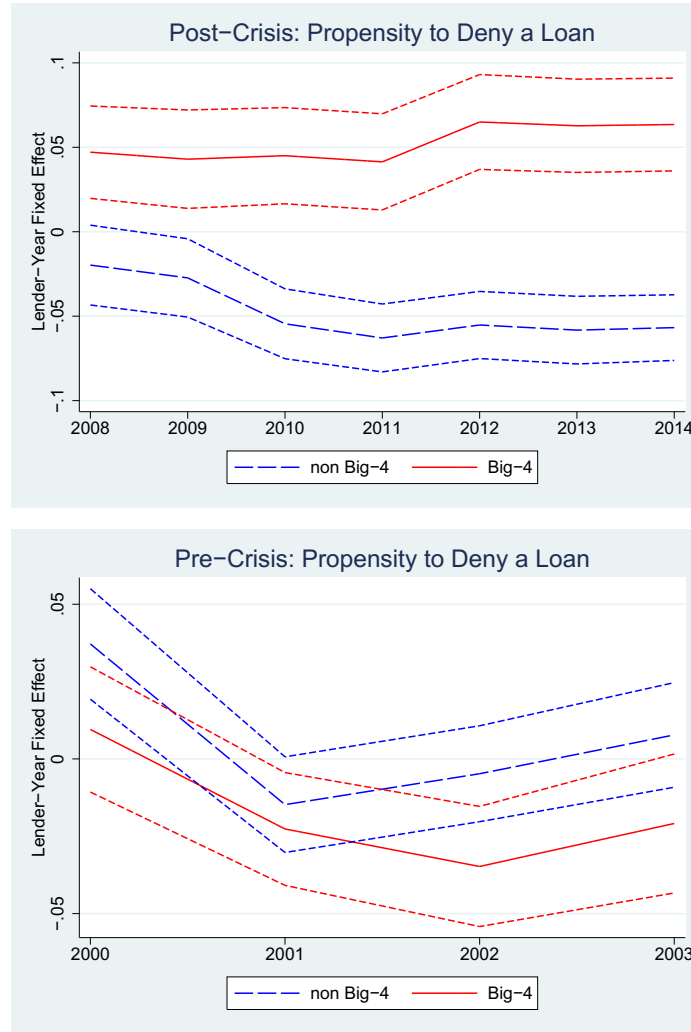
Note: This figure plots average denial rates based on the credit supply instruments. We first residualize denial rates based on the controls in Table 1.2. In all plots, the red line denotes MSAs with a high (25%) exposure to the shock, and the blue dashed line denotes MSAs with a low (bottom 25%) exposure.

Figure A.3: Denial Rates for Big-4 and Stress Tested Lenders



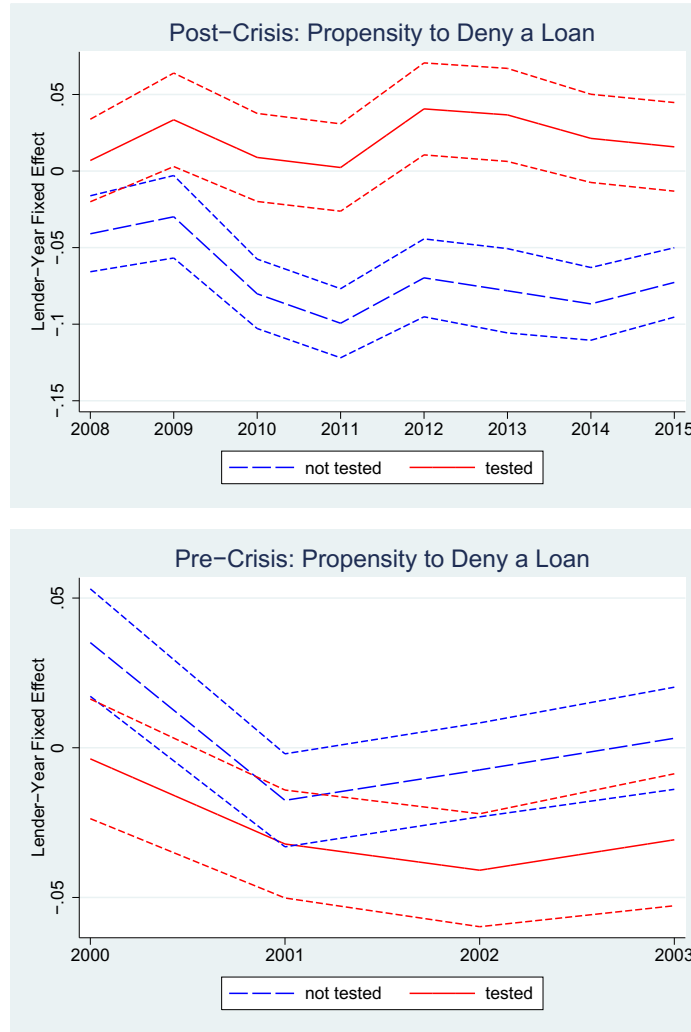
Note: This figure plots the mortgage denial rate for the Big-4 banks and lenders subject to a stress test between 2011-2015.

Figure A.4: Propensity to Deny a Mortgage based on Big-4 Exposure



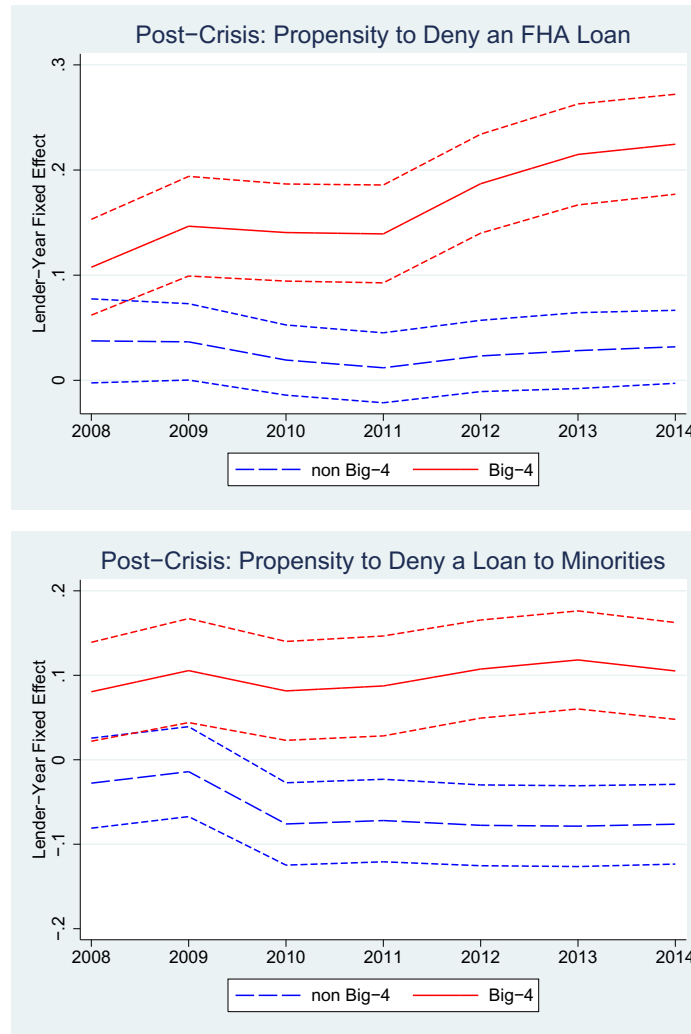
Note: The top panel plots the lender-year fixed effects estimated in equation A.1 for Big-4 and non Big-4 lenders over 2008-2014. Specifically, equation A.1 is a linear probability model of mortgage denial which controls for the applicant's log income, requested loan-to-income, race, and MSA-year, lender-MSA, and lender-year fixed effects. The dashed lines correspond to a 95% confidence interval, computed with heteroskedasticity robust standard errors. The reference lender-year category is non Big-4 lenders in 2007, for which the denial probability was 15.6%. The bottom panel has an analogous figure for the 2000-2003 period, where the reference category is non Big-4 lenders in 2004.

Figure A.5: Propensity to Deny a Mortgage based on Exposure to Stress-Tested Lenders



Note: The top panel plots lender-year fixed effects estimated as in equation A.1 for stress-tested and non stress-tested lenders over 2000-2003, where stress-tested lenders are those which underwent a CCAR test between 2011-2015. The dashed lines correspond to a 95% confidence interval, computed with heteroskedasticity robust standard errors. The reference lender-year category is non stress-tested lenders in 2007. The bottom panel has an analogous plot for the 2000-2003 period, and the reference category is non stress-tested lenders in 2004.

Figure A.6: Propensity to Deny Mortgages to FHA Borrowers and to Blacks or Hispanics



Note: The top panel plots the lenders' fixed effects estimated as in equation A.1 for FHA loans. The dashed lines correspond to a 95% confidence interval, computed with heteroskedasticity robust standard errors. The reference lender-year category is non Big-4 lenders in 2007, for which the denial probability for FHA loans was 14.8%. The bottom panel plots the lenders' fixed effects estimated as in equation

A.1 for loan applications by blacks and Hispanics, which we call minority loans. The dashed lines correspond to a 95% confidence interval, computed with heteroskedasticity robust standard errors. The reference lender-year category is as in the top panel and the corresponding denial probability for minority loans was 25.6%.

Table A.1: Correlation Matrix

	Big-4	Tested	Top 20
Big-4	1		
Tested	0.191	1	
Top 20	0.312	0.710	1

Note: This table shows the correlation matrix for the credit supply instruments. Big-4 denotes the branch deposit share of the Big-4 banks in 2008. Tested denotes the 2008 mortgage application share of lenders which underwent a stress test between 2011-2015. Top 20 is the D’Acunto and Rossi (2017) instrument, that is, the 2007 origination share of the top 20 mortgage lenders that year.

Table A.2: Rent Growth and Credit Supply: Sample Sensitivity

Outcome:	Avg Rent Growth _{<i>m</i>,10–14}	Avg Rent Growth _{<i>c</i>,10–14}
Avg Denial Rate _{<i>msa</i>,10–14}	0.988 (0.021)	
Avg Denial Rate _{<i>county</i>,10–14}		0.487 (0.008)
Sample	Non-Headquarter	Full
Geographic Unit	MSA	County
MSA Controls	Yes	Yes
State Fixed Effects	Yes	Yes
Underidentification test (p-value)	0.007	0.001
J-statistic (p-value)	0.125	0.466
Number of Observations	215	556

Note: P-values are in parentheses. The first column re-estimates our baseline specification from Table 1.2 excluding MSAs in a state with or adjacent to a Big-4 headquarter (CA, NC, NY, CT, NJ). The second column uses the full sample, but at the county level, so that each observation is a county, not an MSA. The instruments for Avg Denial Rate and the MSA controls are the same as in Table 1.2. The underidentification test is that of Kleibergen and Paap (2006). Standard errors are heteroskedasticity robust.

Table A.3: Robustness: Credit Supply Instruments and Drivers of Housing Rents

Outcome:	Tested _{<i>m</i>,08}	Big-4 _{<i>m</i>,08}	Tested _{<i>m</i>,08}	Big-4 _{<i>m</i>,08}
Avg Rent Growth _{<i>m</i>,00–08}	-0.319 (0.330)	-0.131 (0.892)	-0.192 (0.010)	-0.005 (0.952)
log(Rent _{<i>m</i>,09})	-0.181 (0.643)	0.124 (0.912)	0.022 (0.773)	-0.207 (0.186)
log(House Price _{<i>m</i>,09})	0.647 (0.158)	-0.368 (0.788)	0.376 (0.000)	0.146 (0.392)
log(Population _{<i>m</i>,09})	0.125 (0.755)	0.471 (0.650)	0.026 (0.678)	0.243 (0.018)
log(Income _{<i>m</i>,09})	0.296 (0.438)	-0.075 (0.952)	0.051 (0.607)	0.043 (0.781)
Avg Unemp. Growth _{<i>m</i>,10–14}	-0.170 (0.496)	-0.081 (0.928)	-0.030 (0.569)	0.070 (0.327)
Avg Price Growth _{<i>m</i>,10–14}	0.123 (0.608)	-0.210 (0.832)	0.113 (0.095)	-0.081 (0.332)
Financial Services Share _{<i>m</i>,08}	-0.274 (0.214)	0.433 (0.430)	0.056 (0.410)	0.047 (0.704)
Homeownership Rate _{<i>m</i>,09}	-0.064 (0.566)	0.062 (0.853)		
State Fixed Effects	Yes	Yes	Yes	Yes
R-squared	0.832	0.503	0.680	0.373
Number of Observations	60	60	255	255

Note: P-values are in parentheses. All variables are standardized to have a standard deviation of 1. The outcome in each column is one of our credit supply instruments: (i) the 2008 mortgage application share of lenders which underwent a stress test between 2011-2015; and (ii) the branch deposit share of the Big-4 banks in 2008. Homeownership rates are from the U.S. Census Bureau's Housing Vacancy Survey. Each observation is an MSA. Standard errors are heteroskedasticity robust.

Table A.4: Panel Analysis: Credit Supply and Rent Growth

Outcome:	$\Delta \log(\text{Rent}_{m,t})$	
$\Delta \text{Denied}_{m,t}$	-0.017	2.074
	(0.823)	(0.003)
Estimation	OLS	IV
MSA-Year Controls	Yes	Yes
Year FE	Yes	Yes
MSA FE	Yes	Yes
Underidentification test (p-value)		0.001
J-statistic (p-value)		0.916
Number of Observations	1542	1542

Note: Standard errors are clustered by MSA. P-values are in parentheses. $\Delta \log(\text{Rent}_{m,t})$ and $\Delta \text{Denied}_{m,t}$ denote the change in log rents and denial rate from year t-1 to year t, respectively. The instruments for $\Delta \text{Denied}_{m,t}$ are: (i) $V_{m,t-1}$, the Big-4's branch deposit share in 2008 in MSA m multiplied by the difference in denial propensity between the Big-4 and non Big-4 lenders in year t-1; (ii) $S_{m,t-1}$, the mortgage application share of stress-tested lenders in 2008 multiplied by the difference in denial propensity between stress-tested and non stress-tested lenders in year t-1. Stress-tested lenders are those subject to CCAR between 2011-2015; (iii) $G_{m,t-1}$, the weighted average denial propensity among the top 20 lenders in an MSA in year t-1, with weights determined by mortgage application shares in that year; (iv) the fraction of applications from MSA m in year t-1 within 5% of the national average conforming loan limit in year t, where the average excludes MSA m. Instrument (iv) is a version of that used by Loutskina and Strahan (2015) suitable for the post-2008 period. The online appendix contains a thorough description of each instrument. MSA controls are the lagged changes in: log median household income, log median inhabitant age, log population, and the unemployment rate. The underidentification test is that of Kleibergen and Paap (2006). The sample period is 2009-2014. Each observation is an MSA-year.

Table A.5: Panel Placebo: Credit Supply and Rents Before the Crisis

Outcome:	$\Delta \log(\text{Rent}_{m,t})$	
$\Delta \text{Denied}_{m,t}$	-0.062 (0.518)	-0.004 (0.121)
Credit Supply IV	Tested	Big-4
MSA-Year Controls	Yes	Yes
Year FE	Yes	Yes
MSA FE	Yes	Yes
Underidentification test (p-value)	0.477	0.016
Number of Observations	495	495

Note: P-values are in parentheses. $\Delta \log(\text{Rent}_{m,t})$ and $\Delta \text{Denied}_{m,t}$ denote the change in log rents and denial rate from year t-1 to year t, respectively. The instruments for $\Delta \text{Denied}_{m,t}$ are: (i) in column 1, $S_{m,t-1}$, the branch deposit share of stress-tested lenders in 2008 multiplied by the difference in denial propensity between stress-tested and non stress-tested lenders in year t-1. Stress-tested lenders are those subject to CCAR between 2011-2015; and (ii) in column 2, $V_{m,t-1}$, the Big-4's branch deposit share in 2008 in MSA m multiplied by the difference in denial propensity between the Big-4 and non Big-4 lenders in year t-1. The online appendix contains a thorough description of each instrument. MSA controls are the lagged changes in log median household income. The underidentification test is that of Kleibergen and Paap (2006). The sample period is 2001-2003. Each observation is an MSA-year. Standard errors are clustered by MSA.

Table A.6: Credit and Rents Pre-Crisis using the Loutskina and Strahan (2015) IV

Outcome:	$\Delta \log(\text{Rent}_{m,t})$
$\Delta \text{Denied}_{m,t}$	0.070 (0.054)
Credit Supply IV	CLL Shock
MSA-Year Controls	Yes
Year FE	Yes
MSA FE	Yes
Underidentification test (p-value)	0.009
Number of Observations	495

Note: P-values are in parentheses. $\Delta \log(\text{Rent}_{m,t})$ and $\Delta \text{Denied}_{m,t}$ denote the change in log rents and denial rate from year t-1 to year t, respectively. The instrument for $\Delta \text{Denied}_{m,t}$ is the triple product of: (a) the fraction of applications from MSA m in year t-1 within 5% of the conforming loan limit in year t; (b) MSA m's elasticity of housing supply estimated by Saiz (2010); and (c) the change in the log conforming loan limit between year t-1 and year t. We refer to this instrument, originally used by Loutskina and Strahan (2015), as the Conforming Loan Limit (CLL) Shock. MSA controls are those from Table A.5. The underidentification test is that of Kleibergen and Paap (2006). The sample period is 2001-2003. Each observation is an MSA-year. Standard errors are clustered by MSA.

Table A.7: Instrument Sensitivity in Panel Analysis

Outcome:	$\Delta \log(\text{Rent}_{m,t})$			
$\Delta \text{Denied}_{m,t}$	2.069 (0.004)	2.063 (0.006)	2.071 (0.097)	2.088 (0.003)
Excluded Panel IV	Big-4	Tested	MSA Average	CLL Fraction
MSA-Year Controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
MSA FE	Yes	Yes	Yes	Yes
Underidentification test (p-value)	0.001	0.001	0.088	0.000
J-statistic (p-value)	0.802	0.774	0.774	0.972
C-statistic (p-value)	0.786	0.959	0.949	0.499
Number of Observations	1542	1542	1542	1542

Note: P-values are in parentheses. $\Delta \log(\text{Rent}_{m,t})$ and $\Delta \text{Denied}_{m,t}$ denote the change in log rents and denial rate from year t-1 to year t, respectively. The instruments for $\Delta \text{Denied}_{m,t}$ are those from Table A.4. In each column, we exclude one of the instruments, as indicated in the row Excluded IV. The C-Statistic corresponds to the difference-in-Sargan test of the hypothesis that the excluded instrument is valid; it is based on the difference in J-Statistics when using the full instrument set and when excluding the instrument in question. MSA controls are those from Table A.4. The underidentification test is that of Kleibergen and Paap (2006). The sample period is 2009-2014. Each observation is an MSA-year. Standard errors are clustered by MSA.

Table A.8: House Prices and Credit Supply

Outcome:	Avg House Price Growth $_{m,10-14}$
Tested $_{m,08}$	0.026 (0.096)
Tested $_{m,08} \times \text{High Minority}_{m,08}$	-0.028 (0.007)
Home Type	All
Estimation	OLS
MSA Controls	Yes
State Fixed Effects	Yes
Number of Observations	257

Note: P-values are in parentheses. House Price Growth $_{m,10-14}$ denotes the average annual change in the log of MSA m's median house price for all homes, based on Zillow's Home Value Index (ZHVI). High Minority $_{m,08}$ denotes whether the MSA had an above-median share of mortgage applications from blacks or Hispanics in 2009. Tested is the stress test instrumental variable defined in Table 1.2. MSA controls are those from Table 1.2 and 2009 log house price. The underidentification test is that of Kleibergen and Paap (2006). Each observation is an MSA. Standard errors are heteroskedasticity robust.

Table A.9: Multifamily Construction and Credit Supply

Outcome:	Avg Multi-Family Permits Growth $_{m,11-14}$
Avg Denial Rate $_{m,10-14}$	14.965 (0.074)
Estimation	IV
MSA Controls	Yes
State Fixed Effects	Yes
Underidentification test (p-value)	0.026
J-statistic (p-value)	0.159
Number of Observations	229

Note: P-values are in parentheses. Avg Multi-Family Permits Growth $_{m,11-14}$ denotes the average annual change in log multifamily permits in MSA m over 2011-2014, to allow for a one year lag in the supply response. The instruments for Avg Denial Rate $_{m,10-14}$ are the variables from Table 1.2. MSA controls are those from Table 1.2 and 2010 log multifamily permits. The underidentification test is that of Kleibergen and Paap (2006). Each observation is an MSA. Standard errors are heteroskedasticity robust.

Table A.10: Rent Growth, Credit Supply, and Lending Frictions

Outcome:	Avg Rent Growth $_{m,10-14}$
Tested $_{m,08}$	0.023 (0.376)
Tested $_{m,08} \times$ License $_m$	0.070 (0.045)
Estimation	OLS
MSA Controls	Yes
State Fixed Effects	Yes
Number of Observations	257

Note: P-values are in parentheses. License $_m$ denotes whether the MSA is in a state requiring mortgage brokers to be licensed. Tested is the stress test instrumental variable defined in Table 1.2. MSA controls are those from Table 1.2. The underidentification test is that of Kleibergen and Paap (2006). Each observation is an MSA. Standard errors are heteroskedasticity robust.

Appendix B

Appendix to Chapter 2

B.1 Estimating the Liquidity Premium

This appendix estimates the effect of Liquidity Coverage Ratio (LCR) regulations on the liquidity of GNMA MBS. As mentioned in Section 2.3.1, we call this effect a “liquidity premium” to emphasize that liquidity increases the value of GNMA MBS from the perspective of nonbank lenders, even though strictly-speaking it is a “liquidity discount”. Summarizing the details from Section 2.3.1, the U.S. version of LCR regulation was proposed on October 24, 2013 and finalized in September 2014. The purpose of this extension is to substantiate the claim that LCR regulation increased nonbanks’ and other originate-to-securitize lenders’ incentives to originate FHA loans, which are eligible for securitization as GNMA MBS.

Following Diep, Eislefeldt, and Richardson (2017), we focus on MBS total returns measured using the Bloomberg-Barclays Total Return Index, since total returns are less model-dependent than an option-adjusted spread (OAS). Our interest is in the expected total return to MBS of type $s \in \{\text{GNMA}, \text{FNMA}\}$. Consider a simple two-factor model in which the

total return between periods t and $t+1$ depends on a liquidity factor $\ell_{t \rightarrow t+1}$ and a separate composite factor $f_{t \rightarrow t+1}$ which captures, for example, the marketwide price of credit, prepayment, or interest-rate risk in period t . In particular, let $\mathbb{E}_t [R_{t \rightarrow t+1}^s]$ denote the expected excess total return to MBS s from t to $t+1$, and suppose this expected return can be written

$$\mathbb{E}_t [R_{t \rightarrow t+1}^s] = \lambda_t^s \bar{\ell} + \phi_t^s \bar{f}, \quad (\text{B.1})$$

where $\bar{\ell} \equiv \mathbb{E}_t [\ell_{t \rightarrow t+1}]$ denotes the marketwide price of liquidity, which for the sake of argument we assume to be time-invariant, and an analogous statement holds regarding \bar{f} . The loadings λ_t^s and ϕ_t^s may be interpreted as the quantity of liquidity or other risk, respectively, for MBS of type s in period t .

Taking the cross-sectional difference in (B.1) between GNMA and FNMA MBS yields

$$\mathbb{E}_t [R_{t \rightarrow t+1}^{FNMA} - R_{t \rightarrow t+1}^{GNMA}] = (\lambda_t^{FNMA} - \lambda_t^{GNMA}) \bar{\ell} + (\phi_t^{FNMA} - \phi_t^{GNMA}) \bar{f}. \quad (\text{B.2})$$

We model the announcement of LCR regulation as disproportionately increasing the liquidity of GNMA MBS, thus reducing the quantity of liquidity risk λ_t^{GNMA} . While LCR may have also raised FNMA MBS liquidity, thus lowering λ_t^{FNMA} , the more favorable liquidity weights granted to GNMA MBS should theoretically lower λ_t^{GNMA} by more. In particular, we suppose the difference $\lambda_t^{FNMA} - \lambda_t^{GNMA}$ increases by some amount λ^{LCR} because of the regulation.

Moving to a regression equation, (B.2) becomes

$$R_{t \rightarrow t+12}^{FNMA} - R_{t \rightarrow t+12}^{GNMA} = \beta_0 + \beta_1 \text{Post-LCR}_t + u_t, \quad (\text{B.3})$$

where t indexes months. Under the assumption that LCR only affected the cross-sectional distribution of liquidity risk, then $\beta_1 \equiv \lambda^{LCR} \bar{\ell}$ is the LCR-induced liquidity premium. This assumption seems plausible, since GSE conservatorship implies approximately equal levels of credit risk over our sample period. Moreover, because we obtain identification from the cross-section of MBS premia, any baseline difference in FNMA versus GNMA prepayment risk is differenced out in (B.3). Thus, any confounding shock to the relative quantity of non-liquidity risk (i.e. $\phi_t^{FNMA} - \phi_t^{GNMA}$) would need to coincide exactly with the introduction of LCR regulation. To further rule out this possibility, we obtain similar results using Bloomberg’s Option-Adjusted Spread (OAS), which, in principle, strips out the effect of embedded options and thus the quantity of prepayment risk.

The results of (B.3) are in Table B.8. We measure GNMA and FNMA returns using the Bloomberg Barclays GNMA and FNMA Total Return indices, respectively. The baseline point estimate in column 1 suggests that LCR increased the expected return to FNMA MBS by 42 bps relative to GNMA MBS. This effect is equal to 0.7 standard deviations of the FNMA-GNMA spread, or around 17% of the average real return to GNMA MBS over 2000-2015 (2.5%). To account for the possibility that the Post-LCR_t indicator captures spurious time variation, we include a linear time trend in column 2, which yields a larger point estimate. Column 3 restricts the sample period to 2011-2015, which also gives a slightly higher point estimate of 55 bps. Finally, the outcome in column 4 is Bloomberg’s Option-Adjusted Spread (OAS) which, as mentioned above, is model-dependent and aims to strip out prepayment risk.⁸⁶ We find that the FNMA-GNMA OAS was 13 bps higher in

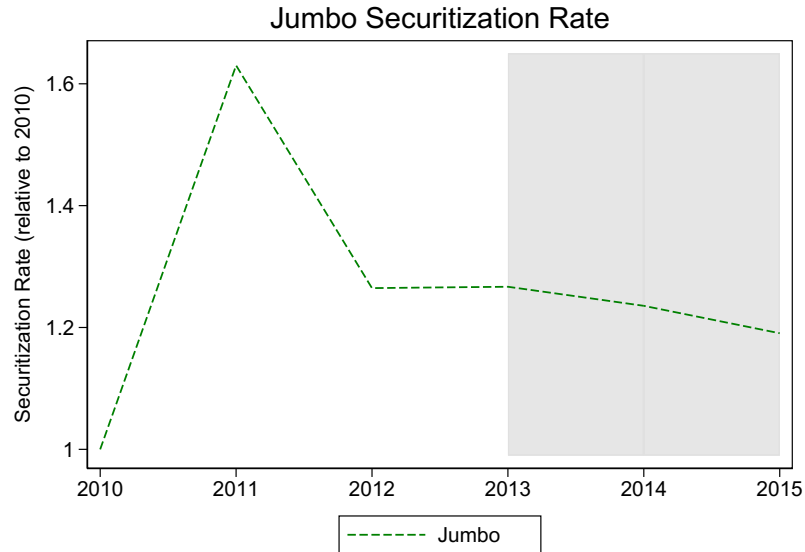
⁸⁶Boyarchenko, Fuster, and Lucca (2015), Gabaix, Krishnamurthy and Vigneron (2007) and Diep, Eisfeldt and Richardson (2017) show that the risk of homeowner prepayment is priced in the MBS market.

the post-LCR period, equal to 0.8 standard deviations. This effect is equal to 29% of the average GNMA OAS over the period.

Figure B.6 visualizes the results in Table B.8. We plot the 12-month-ahead cumulative total return for GNMA and FNMA MBS, where cumulative total return is measured using the Bloomberg-Barclays Total Return Index. Up to a normalization, this variable is the one-year holding period return as of the indicated month. Notice that investors who purchase FNMA MBS on or after the announcement of LCR regulations would need to be compensated with a positive premium relative to GNMA MBS. By contrast, this differential was absent in the pre-announcement period.

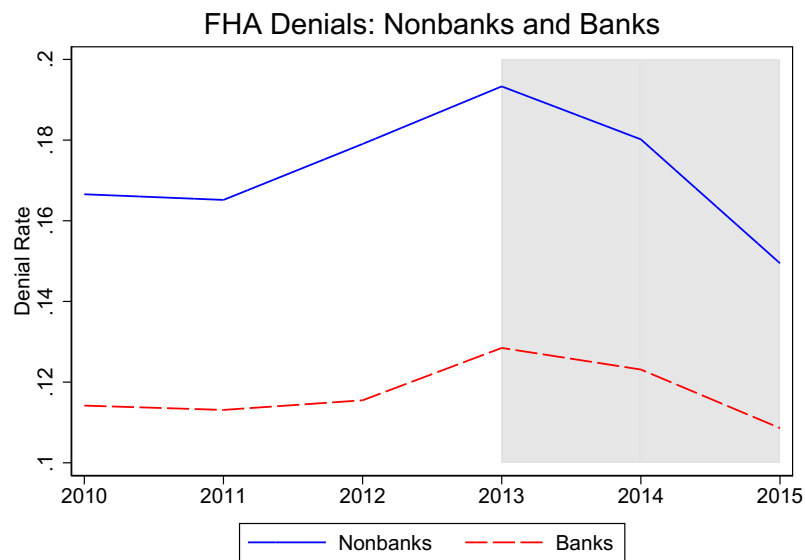
B.2 Additional Figures and Tables

Figure B.1: Securitization among Jumbo Loans



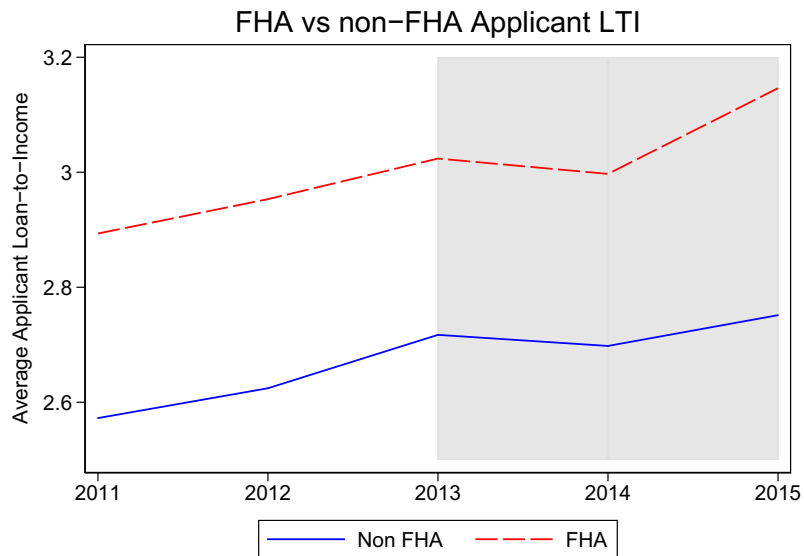
Note: This figure shows the fraction of jumbo loans that are securitized, normalized by the 2010 value. The shaded region corresponds to the period after LCR rules were proposed on October 24th, 2013.

Figure B.2: Denial Rate among FHA Loans



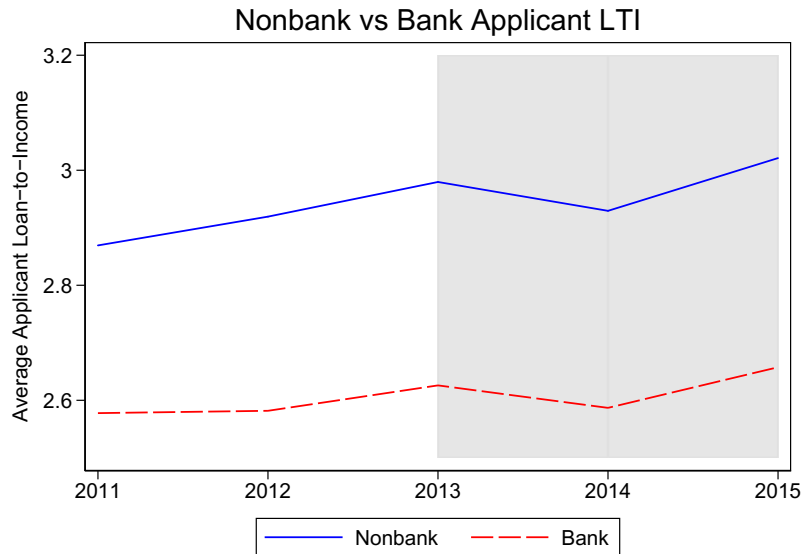
Note: This figure plots banks' and nonbanks' denial rate among FHA loans. The shaded region corresponds to the period after LCR rules were proposed on October 24th, 2013.

Figure B.3: Credit Quality of FHA Applicants



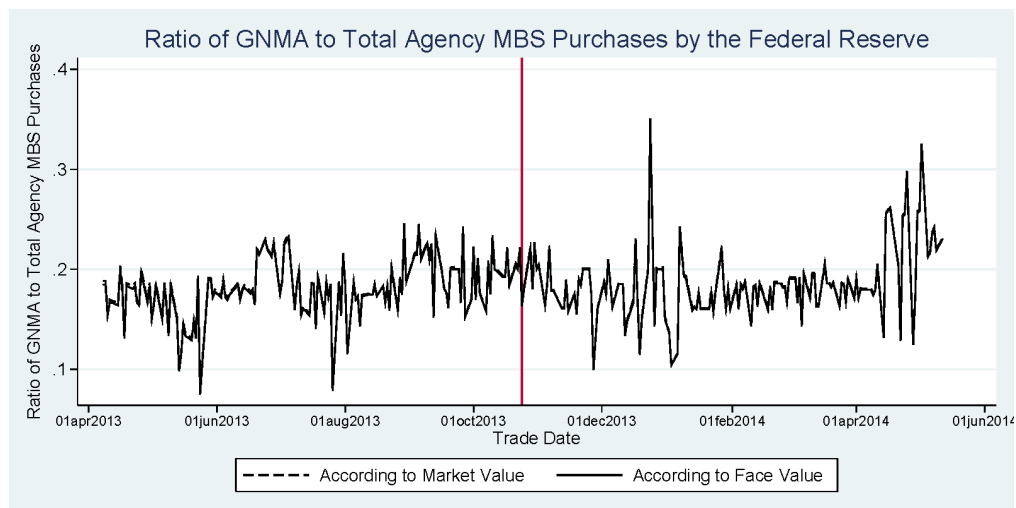
Note: This figure plots the average loan-to-income ratio for FHA versus non-FHA loans over our main sample period. The shaded region corresponds to the period after LCR rules were proposed on October 24th, 2013.

Figure B.4: Credit Quality of Applicants to Banks and Nonbanks



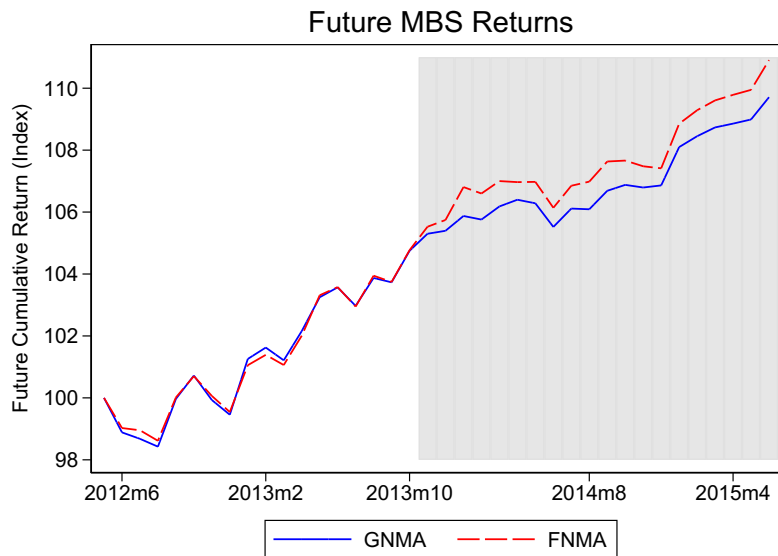
Note: This figure plots the average loan-to-income ratio among applicants to banks versus nonbanks over our main sample period. The shaded region corresponds to the period after LCR rules were proposed on October 24th, 2013.

Figure B.5: Share of GNMA to Total Agency MBS



Note: This figure plots the share of GNMA in the MBS purchases by the Fed. The vertical line corresponds to October 24th, 2013, when the LCR rules were proposed. Source: federalreserve.gov.

Figure B.6: Future MBS Returns



Note: This figure plots the 12-month-ahead cumulative total return for different types of MBS. Cumulative total return is measured using the Bloomberg-Barclays Total Return Index. The shaded region corresponds to the period after LCR rules were proposed on October 24th, 2013.

Table B.1: Nonbanks in the FHA Market

<i>Name</i>	<i>Number of Originations in 2013 and 2014</i>
QUICKEN LOANS	20,905
GUILD MORTGAGE COMPANY	15,692
PRIMARY RESIDENTIAL MORTGAGE	13,321
STEARNS LENDING	12,185
HOMEBRIDGE FINANCIAL SERVICES, PROSPECT MORTGAGE LLC	12,029
FAIRWAY INDEPENDENT MORT CORP	11,477
STONEGATE MORTGAGE CORPORATION	10,399
PACIFIC UNION FINANCIAL, LLC	9,352
MOVEMENT MORTGAGE, LLC	9,327
CORNERSTONE HOME LENDING, INC.	9,113
PLAZA HOME MORTGAGE, INC.	8,946
EVERETT FINANCIAL INC	8,936
FRANLKIN AMERICAN MORTGAGE CO	8,547
ACADEMY MORTGAGE CORPORATION	8,518
DHI MORTGAGE COMPANY LIMITED	8,187
GUARANTEED RATE INC	7,984
UNIVERSAL AMERICAN MTG. CO.LLC	7,726
PINNACLE CAPITAL MORTGAGE	7,602
CALIBER HOME LOANS	7,397
SECURITYNATIONAL MORTGAGE COMP	7,342
UNITED SHORE FINANCIAL SERVICE	7,113
PARAMOUNT RESIDENTIAL MORTGAGE	7,111
LOANDEPOT.COM, LLC	7,087
CARRINGTON MORTGAGE SERVICES	6,927
PHH HOME LOANS	6,457
NOVA HOME LOANS	6,057
FREEDOM MORTGAGE CORPORATION	5,930
NTFN, INC.	5,888
AMERICAN PACIFIC MORTGAGE CORP	5,346
SIERRA PACIFIC MORTGAGE	5,294
SUN WEST MORTGAGE COMPANY, INC	5,196
AMCAP MORTGAGE LTD	4,968
CMG FINANCIAL, INC	4,706
SWBC MORTGAGE CORPORATION	4,671
W. J. BRADLEY MORTGAGE CAPITAL	4,658
IMORTGAGE.COM, INC.	4,487
FIRST MORTGAGE CORP	4,395
MICHIGAN MUTUAL, INC.	4,118
WR STARKEY MORTGAGE, LLP	4,053
MORTGAGE 1 INCORPORATED	3,992
RESIDENTIAL MORTGAGE SERVICES	3,820
NATIONSTAR MORTGAGE LLC	3,654
COBALT MORTGAGE INC	3,641
NETWORK FUNDING LP	3,623
BROKER SOLUTIONS, INC.	3,573
CITYWIDE HOME LOANS, A UTAH CO	3,550
DAS ACQUISITION COMPANY, LLC	3,507
ENVOY MORTGAGE, LTD.	3,360
CALIBER FUNDING LLC	3,357
	3,354

Table B.2: Robustness to using the OAS GNMA Premium

Outcome:	Denial _{<i>i,l,t</i>}	
Nonbank _{<i>l</i>} × GNMA-Premium _{<i>t</i>}	-0.018 (0.009)	-0.016 (0.007)
Premium Measure	FNMA OAS Spread	FHLMC OAS Spread
Lender-MSA FE	Yes	Yes
MSA-Year FE	Yes	Yes
Borrower Controls	Yes	Yes
R-squared	0.117	0.117
Number of Observations	1040927	1040927

Note: P-values are in parentheses. This table estimates equation (2.1). Subscripts i , l , and t denote borrower, lender, and year, respectively. Each observation is a loan application. FNMA OAS Spread is the difference in option-adjusted spread between FNMA and GNMA MBS, and FHLMC OAS Spread is the analogous difference between FHLMC and GNMA MBS. Option-adjusted spreads are computed by Bloomberg. We normalize the FNMA and FHLMC OAS spreads by 13 basis points, which is the estimated effect of LCR regulations as discussed in Section 2.3.1. The remaining notation is the same as in Table 2.2. The sample consists of applications for FHA loans for the purchase of an owner-occupied single-family dwelling from 2010-15. Standard errors are clustered by lender-year bins.

Table B.3: GNMA Premium and Nonbank Lending in the Conventional Market

Outcome:	Denial _{<i>i,l,t</i>} Period: 2010-15			Denial _{<i>i,l,t</i>} Period: 2000-06	
	Nonbank _{<i>l</i>} × GNMA-Premium _{<i>t</i>}	0.013 (0.012)	-0.001 (0.760)	-0.001 (0.707)	0.034 (0.096)
Premium Measure	Post- LCR	FNMA Spread	FHLMC Spread	FNMA Spread	FHLMC Spread
Lender-MSA FE	Yes	Yes	Yes	Yes	Yes
MSA-Year FE	Yes	Yes	Yes	Yes	Yes
Borrower Controls	Yes	Yes	Yes	Yes	Yes
R-squared	0.117	0.117	0.117	0.197	0.197
Number of Observations	2219363	2219363	2219363	10085110	10085110

Note: P-values are in parentheses. This table estimates equation (2.1) in the conventional mortgage market. Subscripts *i*, *l*, and *t* denote borrower, lender, and year, respectively. Each observation is a loan application. The remaining notation is the same as in Table 2.2. The sample consists of applications for conforming non-FHA loans for the purchase of an owner-occupied single-family dwelling. The sample period is 2010-15 in columns 1-3 and 2000-06 in columns 4-5. Standard errors are clustered by lender-year bins.

Table B.4: GNMA Premium and Portfolio Reallocation by Lender Funding Liquidity

Outcome:	Denial _{<i>i,l,s,t</i>}		
Securitization Rate _{<i>l</i>} × GNMA-Premium _{<i>t</i>} × FHA _{<i>s</i>}	-0.019 (0.008)	-0.012 (0.000)	-0.011 (0.000)
Premium Measure	Post- LCR	FNMA Spread	FHLMC Spread
Loan Type-Lender FE	Yes	Yes	Yes
Loan Type-Year FE	Yes	Yes	Yes
Lender-Year FE	Yes	Yes	Yes
Lender-MSA FE	Yes	Yes	Yes
MSA-Year FE	Yes	Yes	Yes
Borrower Controls	Yes	Yes	Yes
R-squared	0.115	0.115	0.115
Number of Observations	2594800	2594800	2594800

Note: P-values are in parentheses. This table estimates a variant of equation (2.4). Subscripts i , l , s , and t denote borrower, lender, loan type, and year, respectively. Each observation is a loan application. Securitization Rate is the lender's ratio of securitized loans to total originations in 2010, as in Table 2.3. The remaining notation is the same as in Table 2.4. The sample consists of FHA and conforming non-FHA loan applications for the purchase of an owner-occupied single-family dwelling from 2010-15. Standard errors are clustered by lender-year bins.

Table B.5: Robustness to Excluding Lenders with Over 2% of the Market

Outcome:	Denial _{<i>i,l,t</i>}			
	Diff-in-Diff		Triple Diff-in-Diff	
Nonbank _{<i>l</i>} × GNMA-Premium _{<i>t</i>}	-0.007 (0.000)	-0.007 (0.000)		
Nonbank _{<i>l</i>} × GNMA-Premium _{<i>t</i>} × FHA _{<i>s</i>}			-0.004 (0.001)	-0.004 (0.000)
Premium Measure	FNMA Spread	FHLMC Spread	FNMA Spread	FHLMC Spread
Loan Type-Lender FE	No	No	Yes	Yes
Loan Type-Year FE	No	No	Yes	Yes
Lender-Year FE	No	No	Yes	Yes
Lender-MSA FE	Yes	Yes	Yes	Yes
MSA-Year FE	Yes	Yes	Yes	Yes
Borrower Controls	Yes	Yes	Yes	Yes
R-squared	0.119	0.119	0.122	0.122
Number of Observations	866326	866326	2734287	2734287

Note: P-values are in parentheses. Columns 1-2 of this table estimate equation (2.1), and columns 3-4 estimate equation (2.4). Subscripts *i*, *l*, *s*, and *t* denote borrower, lender, loan type, and year, respectively. The sample excludes lenders with over 2% of the total mortgage market in 2010, measured by origination share. The remaining notation and remarks on sample for columns 1-2 and columns 3-4 are the same as in Tables 2.2 and 2.4, respectively. Standard errors are clustered by lender-year bins.

Table B.6: Robustness to Excluding Nonbanks

Outcome:	Denial _{<i>i,l,t</i>}			
	Diff-in-Diff		Triple	Diff-in-Diff
Securitization Rate _{<i>l</i>} × GNMA-Premium _{<i>t</i>}	-0.019 (0.083)	-0.018 (0.050)		
Securitization Rate _{<i>l</i>} × GNMA-Premium _{<i>t</i>} × FHA _{<i>s</i>}			-0.015 (0.015)	-0.014 (0.007)
Premium Measure	FNMA Spread	FHLMC Spread	FNMA Spread	FHLMC Spread
Loan Type-Lender FE	No	No	Yes	Yes
Loan Type-Year FE	No	No	Yes	Yes
Lender-Year FE	No	No	Yes	Yes
Lender-MSA FE	Yes	Yes	Yes	Yes
MSA-Year FE	Yes	Yes	Yes	Yes
Borrower Controls	Yes	Yes	Yes	Yes
R-squared	0.106	0.106	0.110	0.110
Number of Observations	324350	324350	1331695	1331695

Note: P-values are in parentheses. Columns 1-2 of this table estimate equation (2.3), and columns 3-4 estimate a variant of equation (2.4). Subscripts *i*, *l*, *s*, and *t* denote borrower, lender, loan type, and year, respectively. Securitization Rate is the lender's ratio of securitized loans to total originations in 2010, normalized to have a mean of 0 and variance of 1. The sample excludes nonbanks. The remaining notation and remarks on sample for columns 1-2 and columns 3-4 are the same as in Tables 2.2 and 2.4, respectively. Standard errors are clustered by lender-year bins.

Table B.7: Interest Rate Pass-Through by Nonbanks at a Monthly Frequency

Outcome:	Rate _{<i>i,l,t</i>}			
Nonbank _{<i>l</i>} × GNMA-Premium _{<i>t</i>}	-0.052 (0.000)	-0.053 (0.000)	-0.124 (0.000)	-0.125 (0.000)
Premium Measure	FNMA Spread	FHLMC Spread	FNMA OAS Spread	FHLMC OAS Spread
Lender-MSA FE	Yes	Yes	Yes	Yes
MSA-Year FE	Yes	Yes	Yes	Yes
Borrower Controls	Yes	Yes	Yes	Yes
R-squared	0.616	0.616	0.616	0.616
Number of Observations	2130962	2130962	2130962	2130962

Note: P-values are in parentheses. This table estimates equation (2.8). Subscripts i , l , and t denote borrower, lender, and month, respectively. Each observation is a new loan. Rate is the loan's interest rate, in percentage points. All measures of GNMA-Premium have units of percentage points and are not normalized. OAS spreads are based on Bloomberg's option-adjusted spread, as described in Table B.2. Borrower controls are log loan amount and an indicator for whether the loan is a fixed-rate mortgage. The remaining notation is the same as in Table 2.2. The sample consists of originated FHA loans for the purchase of a single-family dwelling from 2012-15. Standard errors are clustered by lender-year bins.

Table B.8: Liquidity Coverage Ratio and the GNMA Liquidity Premium

Outcome:	$R_{t \rightarrow t+12}^{FNMA} - R_{t \rightarrow t+12}^{GNMA}$			$OAS_t^{FNMA} - OAS_t^{GNMA}$
Post- LCR_t	0.422 (0.009)	0.757 (0.025)	0.546 (0.034)	0.133 (0.001)
Sample	2000-15	2000-15	2011-15	2011-15
Time Trend	No	Yes	Yes	Yes
Number of Observations	181	181	49	49

Note: P-values are in parentheses. This table estimates equation (B.3). Subscript t denotes month. $R_{t \rightarrow t+12}^{GNMA}$ is the change in log Bloomberg-Barclays GNMA Total Return Index from t to $t+12$, multiplied by 100. $R_{t \rightarrow t+12}^{FNMA}$ is defined analogously in terms of the Bloomberg-Barclays FNMA index. The sample period in columns 1 and 2 is October 2000 through October 2015, and the sample period is October 2011 through October 2015 in columns 3 and 4. Columns 2 through 4 include a linear time trend. Each observation is a month. Standard errors are Newey-West with a lag of 4 months.

Appendix C

Appendix to Chapter 3

This document contains additional material related to the paper “Financial Intermediaries as Suppliers of Housing Quality: Evidence from Banks and Public Pensions”. Appendix C.1 describes the data and presents summary statistics. Appendices C.2 and C.3 perform extensions related to the credit supply and private equity supply research designs, respectively. Appendix C.4 performs additional extensions referenced in the main text. Additional figures and tables may be found in Appendix C.5.

C.1 Data Appendix

This section describes the paper’s main datasets and how they were cleaned. Section C.1.1 describes the three core datasets and Section C.1.2 describes auxiliary ones. Summary statistics are presented in this appendix.

C.1.1 Core Datasets

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.⁸⁷ The AHS was introduced in 1973 but has undergone several sample redesigns since then. I use the 1997-2013 sample design in this paper.

My primary use of the AHS data is to construct the hedonic index in Section 3.5.1. I estimate the hedonic pricing equation (3.11) 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 did not change over the sample period, thus filtering out “condo conversions”. I winsorize rent data by 5% on both sides prior to aggregating quality-adjusted rent in (3.13).

Table C.19 provides summary statistics of the AHS dataset used to construct the hedonic index.

⁸⁷I only observe the unit’s MSA for a subset of 166 MSAs.

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 2010-16.⁸⁸ 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 C.3.

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

1. **Rent:** I observe total property revenue, number of units, and occupancy rate. Rent is approximated as revenue per occupied unit and winsorized to attenuate measurement error.
2. **Renovation:** Renovations are defined as improvements that require the inhabitant to vacate the housing unit for some period of time. 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

⁸⁸I work with a random sample of Trepp's merged Property, Loan, and Loan2 file.

Figure 3.1a 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.⁸⁹ 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 3.1a.

Next, renovations undertaken in the latter part of the 2010-16 period may not appear in the sample because of securitization lags. Therefore, Figure 3.1a 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 2017.⁹⁰

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.

⁸⁹I 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.

⁹⁰I measure this probability using the empirical cumulative density function of the gap between the month of securitization and October 2017.

3. **Lender:** I observe the name of the lender who originated the property’s mortgage for 92% of the sample.⁹¹ Banks are defined as having a record in the FDIC’s Institution Directory. I do not classify independent nonbank subsidiaries as depository institutions. Based on this classification, 39% of lenders in my data are depository institutions. There are some non-depository institutions, like Prudential, which are classified as Designated Financial Companies and thus required to compute risk-based capital requirements as if they were a bank holding company. Since my focus is on the effects of capital requirements, I classify such lenders as banks. Apart from these special cases, “bank” is synonymous with “depository institution”. I observe the name of the borrower for 14% of the sample, which I use to perform the analysis in Appendix C.4.1.

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.⁹² To appropriately capture magnitudes, I transform

⁹¹To 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.

⁹²A score of 1 is intended to have the interpretation of “new or like-new condition”. Scores of 2 or 3 are

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 2009.⁹³ 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.⁹⁴ See Reher (2019b) for photographs of example apartments by 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.⁹⁵ Figure C.19 plots the average change in log

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.

⁹³Explicitly, $\text{Quality}_{i,t}$ is a mapping from the raw score $\text{Raw}_{i,t} \in \{1, \dots, 5\}$ 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.

⁹⁴For 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).

⁹⁵For 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

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 3.3, although I also use it to produce some of the stylized facts in Section 3.2. As discussed in Section 3.3, I work with both property and county-level datasets. Table C.20 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 C.1.2.

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.⁹⁶ In addition, I observe the fund's property sector and geographic

loan's servicer.

⁹⁶I classify core-plus funds as value added, since these funds often make improvements, but at a much

focus. This information enables me to restrict the manager-level regressions in Table 3.5 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 3.5 because the risk taking behavior captured by $\text{Funding Gap}_p \times \text{Yield Gap}_t$ 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 3.4. 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 C.1.2 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 C.21 provides summary statistics of the Preqin dataset used in Section 3.4.

C.1.2 Auxiliary Datasets

The following auxiliary datasets are also used in the paper:

- *Aggregate Renovation Activity*: Data on aggregate renovation activity and share per-

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.

formed by institutional or mortgaged investors come from the Rental Housing Finance Survey (RHFS). The RHFS aims to provide a current and continuous measure of financial, mortgage, and property characteristics of rental housing properties in the United States. Survey respondents are owners or property managers of rental properties. The survey has been administered in 2012 and 2015.

- *Public Pension Funding:* Data on public pensions' funding status, allocation across asset classes, and realized returns come from Boston College's Center for Retirement Research (CRR) Public Plans Database. The data contain plan-level information on 180 public pensions from 2001-2016, of which 114 are at the state level and 66 are local. According to CRR, the sample covers 95% of U.S. public pension assets. The raw data come from pensions' Comprehensive Annual Financial Reports (CAFRs), specifically GASB Schedules of Plan Funding and Employer Contributions. The set of public pensions used in the analysis of Section 3.4 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 multi-

family 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 (2018b). I group subsidiaries of CCAR lenders with their parent.
- *Zillow Rent and Price Indices*: Data on zip code multifamily rent from Figure 3.1 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 3.4 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 Merrill 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.

C.2 Extensions to Credit Supply Research Design

This appendix contains extensions related to the credit supply research design from Section 3.3 of the text.

C.2.1 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 C.3, and when including heterogeneous time trends for time-invariant county characteristics, shown in Appendix Table C.4. In Appendix Figure C.9, I estimate a cross-sectional version of (3.6) to investigate nonlinear treatment effects. There is variation around the best-fit line, but there is no clear nonlinear functional form.

C.2.2 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 (3.4). 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.⁹⁷ This makes it more difficult to identify the effect of HVCRE regulation off of

⁹⁷There 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.

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 2016b; Gete and Reher 2018b). These tests, first implemented in 2011, are meant to ensure that the largest bank holding companies have enough capital to weather a financial crisis, and their standards are substantially more stringent than ordinary DFAST stress tests. Accordingly, lenders subject to CCAR tests have an incentive to closely adhere to HVCRE regulation, and they are the “treated lenders” in this research design.

Second, unlike with the multifamily mortgage data, I do not observe whether a loan finances an improvement versus a construction project. However, I do observe whether the borrower’s primary business activity is construction based on their associated SIC code. Developers therefore represent “treated borrowers”, in contrast to the control group, REITs, which perform both property improvements and construction.⁹⁸

I therefore estimate the following specification

$$\text{New Loan}_{b,\ell,t} = \beta (\text{CCAR}_{\ell} \times \text{Post}_t \times \text{Developer}_b) + \alpha_{\ell,t} + \alpha_{b,t} + \alpha_{b,\ell} + u_{b,\ell,t}, \quad (\text{C.1})$$

where b , ℓ , and t index borrowers, lenders, and years, $\text{New Loan}_{b,\ell,t}$ indicates whether a new secured loan was made, Developer_b indicates if the borrower is a developer, and CCAR_{ℓ} 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-

⁹⁸There 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).

The parameter of interest in (C.1) is β , which is the effect of the triple interaction between treated borrowers (Developer_b) of treated lenders (CCAR_ℓ) 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 C.5 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 C.5 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 3.1.

⁹⁹As in the baseline specification (3.4), pairs are weighted by the lender's loan issuance over this period.

C.2.3 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 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.¹⁰⁰ 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

$$Y_{i,\ell,t} = \sum_{\tau=-1}^1 \beta_{\tau} \text{Due}_{i,t+\tau} + \alpha_{i,\ell} + \alpha_{z,t} + \alpha_{\ell,t} + u_{i,\ell,t}, \quad (\text{C.2})$$

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.¹⁰¹

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

¹⁰⁰The modal term in the sample is 10 years, and 99% of outstanding balances are on balloon loans.

¹⁰¹I cannot include borrower-lender fixed effects because I only observe the borrower's identity, used to construct Figure C.20, for 14% of the sample.

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.¹⁰² This measure captures more modest improvements in quality (e.g. repainting common areas), and thus there is enough variation in estimate (C.2). My outcome $Y_{i,\ell,t}$ is the change in the log MBA/CREFC rating from $t-1$ to t , denoted $\Delta \log(\text{Quality}_{i,\ell,t})$, which is normalized to have unit variance.¹⁰³

Panel (a) of Figure C.10 plots the estimated coefficients from (C.2). Consistent with the incentives provided by the structure of multifamily mortgages, a property’s quality declines as the due date approaches, indicated by the negative point estimates for $t \leq 0$. This behavior sharply reverses afterward as improvements are made, shown by the positive estimate for $t = 1$. Because all variation comes from the same lending relationship, this pattern does not reflect an outside purchase-and-fix transaction.

Panel (b) of the figure asks whether borrowers with an impending loan due see a deterioration in property quality because they are inherently riskier. For example, perhaps they borrowed at the peak of the pre-crisis boom. To do this I reestimate (C.2) 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 $\text{Impending}_{i,t}$.¹⁰⁴ The results in Figure

¹⁰²Appendix C.1 has details on why this rating is collected, its interpretation, and scope for misreporting bias.

¹⁰³There is a 6.8% annual hazard of improvement according to the MBA/CREFC rating. See Appendix C.1 for additional summary statistics.

¹⁰⁴Explicitly, $\text{Impending}_{i,t} = \max\{\text{Due}_{i,t}, \text{Due}_{i,t+1}\}$. The measures of credit risk are the difference between

C.10b 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

$$\begin{aligned} \Delta \log (\text{Quality}_{i,\ell,t+1}) = & \beta (\text{Bank}_\ell \times \text{Post}_t \times \text{Due}_{i,t}) + \alpha_{i,\ell} + \alpha_{z,t} + \alpha_{\ell,t} + \dots & (\text{C.3}) \\ & \dots + \alpha_t \times \text{Due}_{i,t} + \alpha_\ell \times \text{Due}_{i,t} + u_{i,\ell,t}. \end{aligned}$$

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 (C.3) 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 2008b). 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 C.10b.

The results are in Table C.6. Column 1 provides necessary context by estimating the effect of having a loan due on subsequent growth in quality.¹⁰⁵ The estimates of (C.3) are

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.

¹⁰⁵Specifically, column 1 estimates (C.2) when restricting the lag terms to $\tau = -1$.

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 3.3.3, 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.¹⁰⁶ The point estimate suggests that a 1 standard deviation increase in credit raises subsequent growth in quality by 0.3 standard deviations.

C.2.4 External Validity of Bank Lending Estimates

I now assess the external validity of the bank lending estimates from Section 3.3. The particular issue, described in Section 3.3.5, 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 3.3 may be considered a lower bound. The following exercises lend support to this conservative interpretation.

First, Table C.7 reestimates the lender-year specification (3.3) replacing the outcome with an indicator for whether the loan was securitized within 3 months, a relatively standard warehouse period (Echeverry, Stanton and Wallace 2016b). The result shows that banks

¹⁰⁶I 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.

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 (3.6) 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 C.1.2. 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 3.3 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 C.8 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 3.3.4 as a lower bound.

C.3 Extensions to Pension Research Design

This appendix contains extensions related to the private equity research design from Section 3.4 of the text.

C.3.1 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 C.9 reveal similar results when using nominal 10-year Treasury or Aaa corporate bond yields to measure Yield Gap_t .

C.3.2 Safe and Very Risky Fund Strategies

In Table C.10, 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 C.11, I perform a symmetric test with respect to opportunistic real estate funds, which perform construction projects and command a high risk premium.¹⁰⁷ Unlike core funds, underfunded pensions become more likely to invest in risky opportunistic real estate funds

¹⁰⁷Opportunistic funds have a historic average net return of 13.5% with a standard deviation of 19.2% (Pagliari 2017).

when safe yields fall. However, the magnitude is somewhat weaker relative to the effect observed among more moderate value added funds.

C.3.3 Unconventional Monetary Policy

In this extension I investigate the source of time-series variation used to identify β in (3.9). First, note from Figure C.11 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 (3.9) after interacting the exposure variable, $\text{Funding Gap}_{p,08}$, with a linear time trend. Thus, the only time-series variation in the treatment variable, $\text{Funding Gap}_{p,08} \times \text{Yield Gap}_t$, is cyclical. The positive point estimate in column 1 of Table C.13 suggests that short-term fluctuations in the safe yield indeed influence pension investment behavior.

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 C.12 lists the set of monetary surprises and the change in safe yields attributable to them.

In column 2 of Table C.13, I restrict variation in safe yields to that which is attributable to unconventional monetary policy surprises. Specifically, I instrument for the treatment, $\text{Funding Gap}_p \times \text{Yield Gap}_t$, 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.3.4 Placebo

Table C.14 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.¹⁰⁸ 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 cyclicity.

¹⁰⁸The regression is the same as in (3.9) replacing the base year with 2002.

C.3.5 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 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 C.15 reestimates (3.9) including a separate time trend for these pensions. The results are somewhat weaker, but still significant.

C.3.6 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 3.5. By contrast, the point estimates are very similar regardless of whether these controls are included.

C.3.7 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 (3.9) 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.¹⁰⁹ 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 C.16 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.3.8 Manager-Year Fixed Effects

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

$$Y_{m,t} = \beta (\text{Funding Gap}_m \times \text{Yield Gap}_t \times \text{VA}_k) + \alpha_{m,t} + \alpha_{k,t} + \alpha_{m,k} + \gamma X_{m,t} + u_{m,t}, \quad (\text{C.4})$$

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 (3.3), 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 C.17 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 (3.10), one cannot infer whether managers increase formation of value added funds or simply stop forming other funds. However, the inclusion of manager-year fixed

¹⁰⁹According to Prequin, the average historic net IRR for private distressed debt funds is 12.4% compared to 12.8% for value added funds.

effects does address the concern that the baseline results in Table 3.5 are driven by shocks to managers' overall fundraising and investment activity.

C.3.9 Manager-Pension Matching

The main identification assumption in (3.10) 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 C.12 investigates this assumption by performing a similar exercise as Figure 3.4 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.3.10 Magnitude of Pension Investment Effect

This extension describes the procedure for obtaining the aggregate effect referenced in Section 3.4. First, using the estimates of the manager-level specification from Table 3.5, I define the in-sample effect on total investment by improvement-oriented funds as

$$\text{Effect}^{\text{Sample}} = \frac{\sum_m \sum_{t=2010}^{2016} \text{Investment}_{m,t}^{\text{VA}} \times \left[1 - e^{-\beta^P \times \text{Yield Gap}_t \times \max\{\text{Funding Gap}_m, 0\}} \right] \times \Delta t}{\sum_m \sum_{t=2010}^{2016} \text{Investment}_{m,t}^{\text{VA}} \times \Delta t}, \quad (\text{C.5})$$

where β^P is the estimate from column 4 of Table 3.5. 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.

C.4 Additional Extensions

This appendix contains additional extensions referenced in the text.

C.4.1 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) s th 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 s th loan for borrower b came from lender ℓ , denoted by the indicator $\text{Loan Originated}_{b,\ell,s}$,

$$\text{Loan Originated}_{b,\ell,s} = \rho \text{Loan Originated}_{b,\ell,s-1} + \alpha_{\ell,t} + u_{b,\ell,s}. \quad (\text{C.6})$$

The pairs (b, ℓ) span each possible pair among active borrowers and lenders over 2012-16. The results in column 1 of Table C.22 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 C.20 provides complementary, stylized evidence by plotting the distribution of number of lenders per borrower in the multifamily mortgage market.¹¹⁰ 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 (C.6), after replacing “borrowers” with “private equity real estate fund managers” and “lenders” with “public pensions”. I estimate the probability that pension p commits capital to the s th fund for manager m , denoted by the indicator $\text{Investment}_{p,m,s}$,

$$\text{Investment}_{p,m,s} = \rho \text{Investment}_{p,m,s-1} + \alpha_{p,t} + u_{p,m,s} \quad (\text{C.7})$$

Similarly to before, the pairs (p, m) span each possible pair among active pensions and managers. The results in Table C.23 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

¹¹⁰The figure is based on the 14% subset of the Trepp data for which I observe the borrower’s identity.

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 C.23 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 C.22. In Figure C.21 I plot the size distribution of managers' fundraising alongside that of pensions' increase in real estate holdings over 2009-16, from the CRR.¹¹¹ 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 s th 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

$$\text{Lead Underwriter}_{j,u,s} = \rho_0 \text{Lead Underwriter}_{j,u,s-1} + \rho_1 \text{Underwriter}_{j,u,s-1} + \alpha_{u,t} + u_{j,u,s}, \quad (\text{C.8})$$

¹¹¹This is a proxy for total real estate investment because I have limited information on committed capital to private equity real estate funds.

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

Column 1 of Table C.24 has the results of (C.8). The positive estimate on the previous lead underwriter indicator 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 C.22. 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 3.3 and 3.4.

C.4.2 Quality Improvements in the Cross-Section

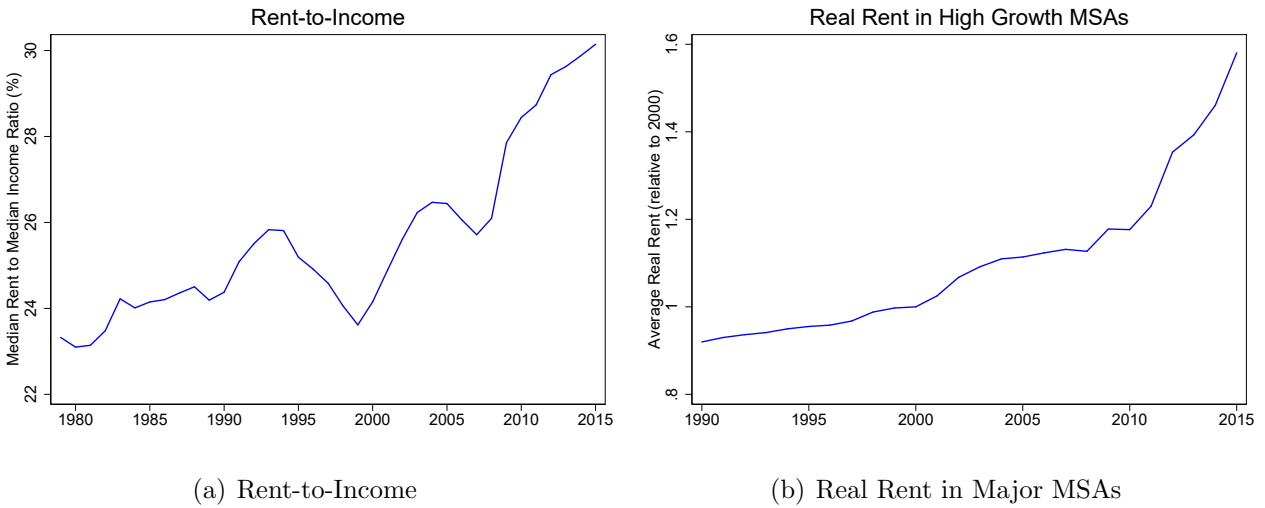
This section describes cross-sectional characteristics of quality improvements referenced in Section 3.2 of the paper. In Table C.25, I regress the share of renovated units in an MSA against the MSA's log elasticity of housing supply, as estimated by Saiz (2010b), log average income, college education 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 3.5, specifically the finding that improvements appear targeted toward higher-income households.

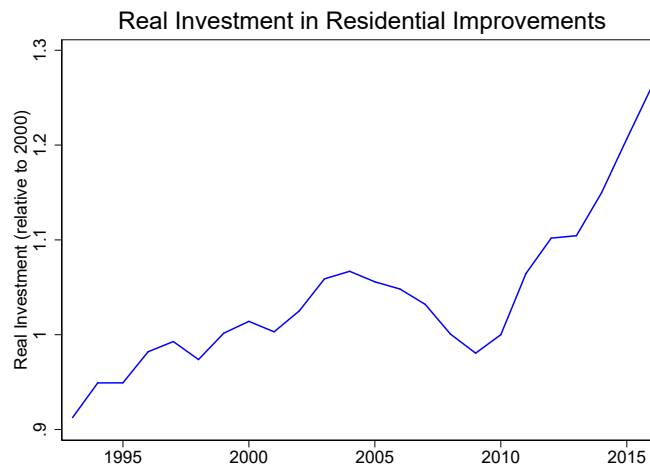
C.5 Additional Figures and Tables

Figure C.1: Rent-to-Income Ratio and Real Rent



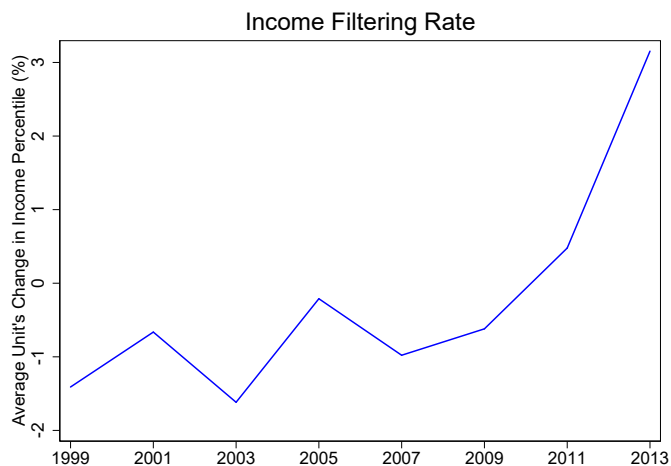
Note: Panel (a) plots the ratio of median rent to median household income. Panel (b) plots average real rent across the top quartile of MSAs sorted by 2008-2015 rent growth. Data are from Zillow and the Census Bureau.

Figure C.2: Aggregate Spending on Improvements



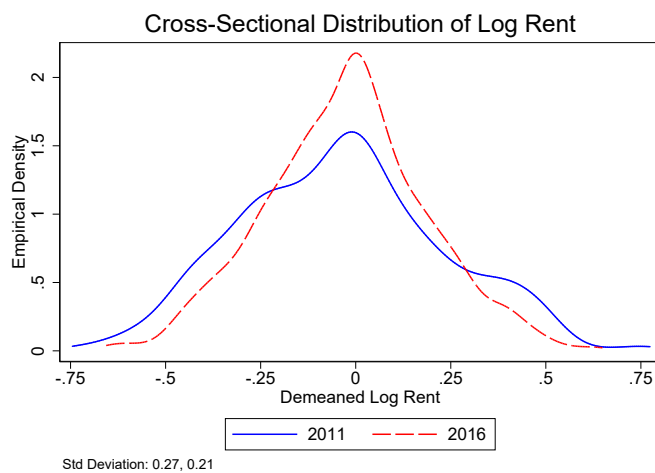
Note: This figure plots real aggregate investment in residential improvements from the Fixed Assets Accounts. Data are from the BEA.

Figure C.3: Rate of Income Filtering



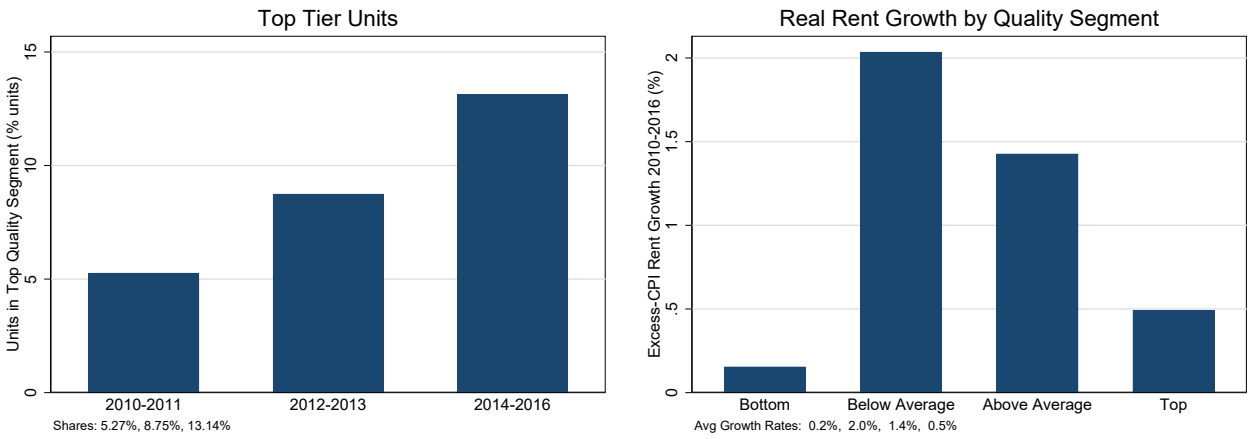
Note: This figure plots the average rental unit's change in its inhabitant's overall income percentile. Data are from the AHS.

Figure C.4: 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 C.5: Quantity and Rent Growth of Top Tier Units

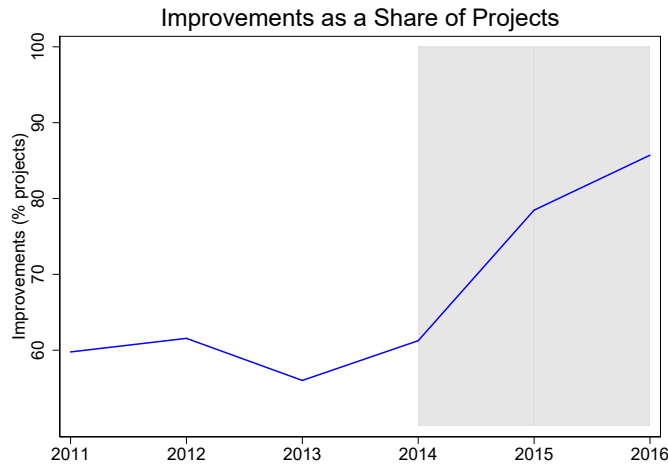


(a) Top Quality Segment

(b) Real Rent Growth by Market Segment

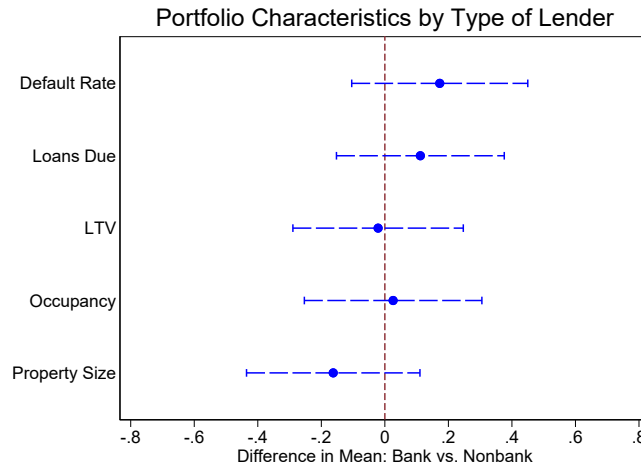
Note: Panel (a) plots the percent of multifamily units in the top quality segment, based on the MBA/CREFC rating. Panel (b) plots average real rent growth for properties in the top segment, above and below average segments, and bottom segment, based on the MBA/CREFC rating. Data are from Trepp

Figure C.6: Improvements as a Share of Projects



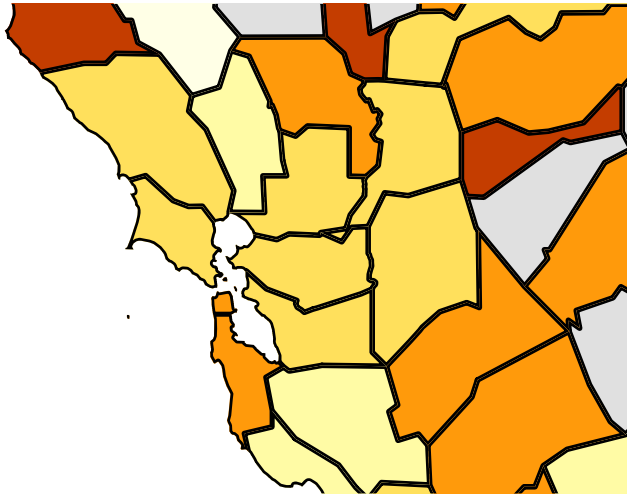
Note: This figure plots the number of renovated multifamily units divided by the number of renovated multifamily units plus the number of newly built multifamily units. The gray region indicates the period when HVCRE regulations are in place. Data are from Trepp.

Figure C.7: Portfolio Characteristics by Type of Lender

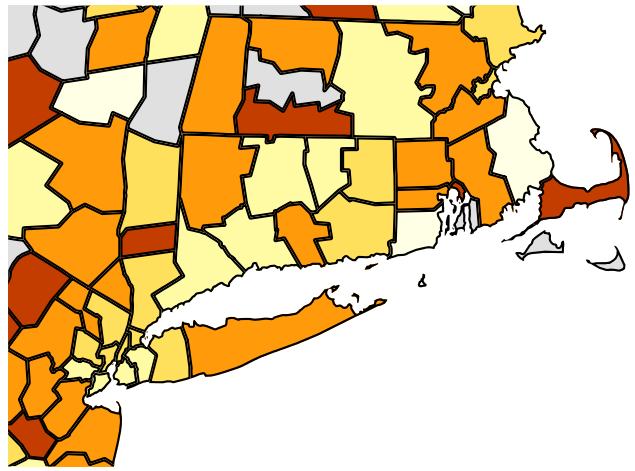


Note: This figure plots the difference in mean for the indicated variable between bank and nonbank lenders. Variables are normalized to have zero mean and unit variance and aggregated to the lender-level by averaging across loans in the lender's portfolio over 2011-16, weighting by loan principal. Default 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.

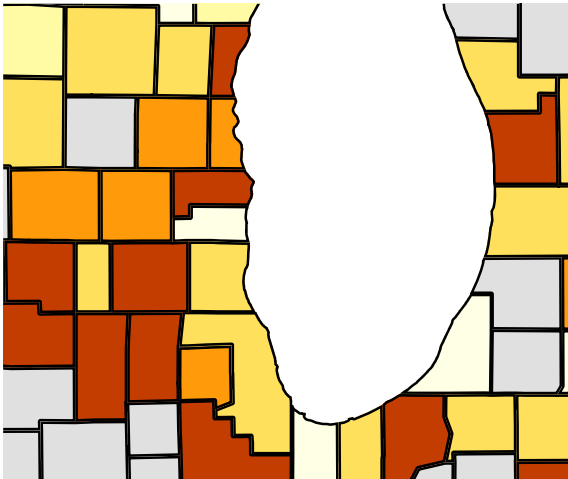
Figure C.8: Distribution of Initial Bank Share in High Growth Areas



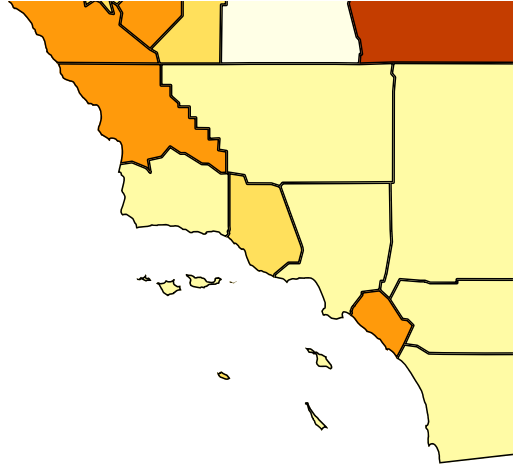
(a) County-Level Distribution: Northern CA



(b) County-Level Distribution: Northeast



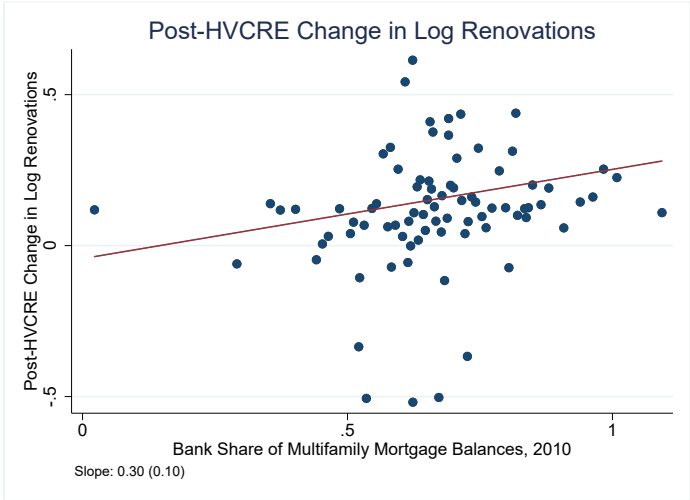
(c) County-Level Distribution: Chicagoland



(d) County-Level Distribution: Southern CA

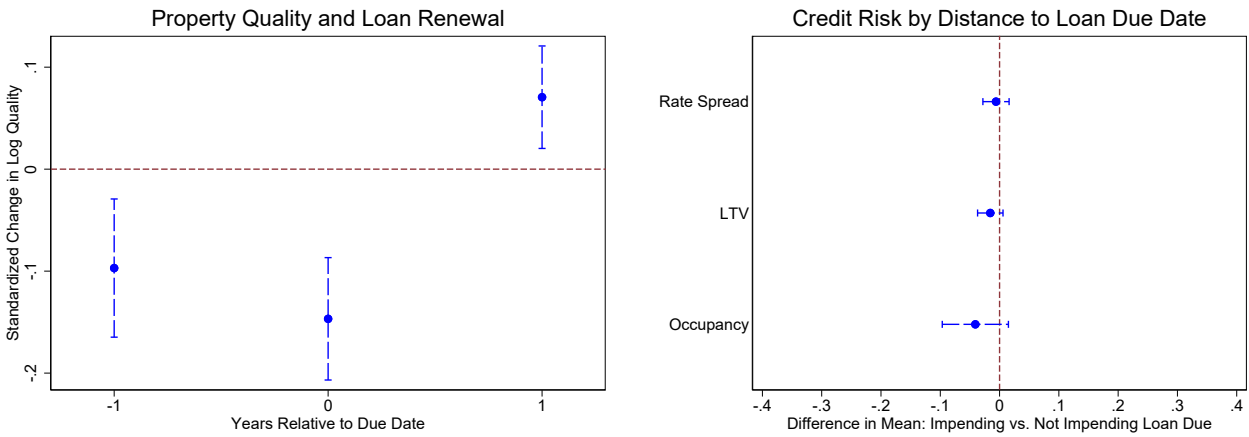
Note: This figure plots banks' share of multifamily mortgage balances in 2010 across counties in high growth metro areas. Panels (a)-(d) plot this share across counties in northern California, northeast states, the Chicagoland area, and southern California, respectively. The plot is analogous to the state-level map in Figure 3.3. Warmer colors indicate a higher share.

Figure C.9: 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 3.3 from the 2011-14 to 2015-16 periods. The regression is the same as (3.6) 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 C.10: Quality Improvements and Credit Risk Related to Loan Due Dates

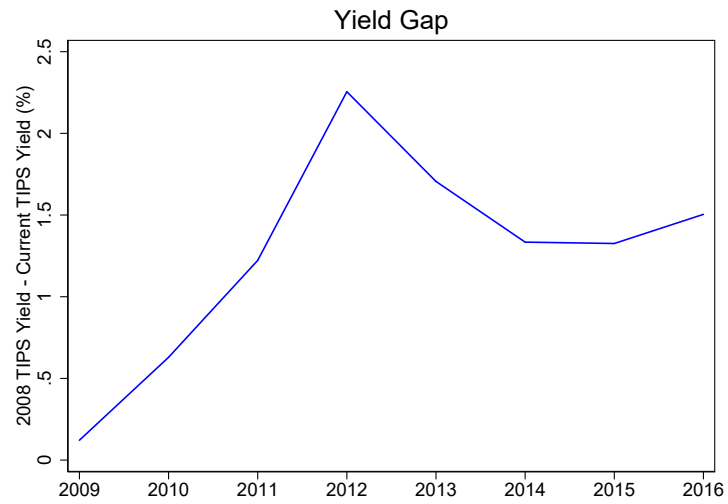


(a) Change in Quality Around Due Date

(b) Credit Risk of Coming-Due Borrowers

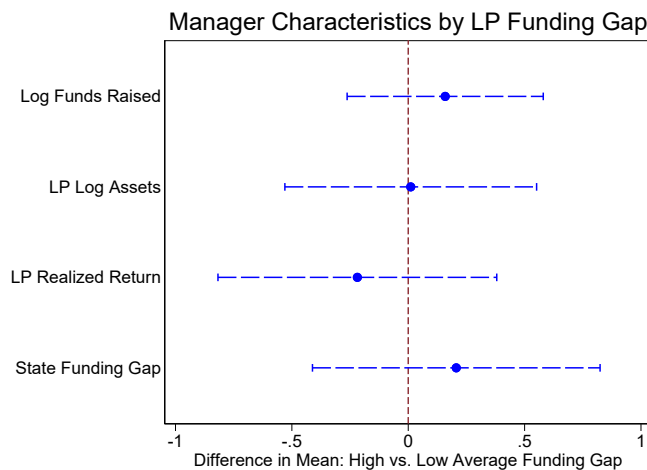
Note: This figure plots results from regressions similar to column 1 of Table C.6. Panel (a) estimates a regression of the change in a property's log relative quality on indicators for whether the property's loan is due the subsequent, current, or previous year. Quality is based on the MBA/CREFC rating, 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 C.11: Variation in Yield Gap



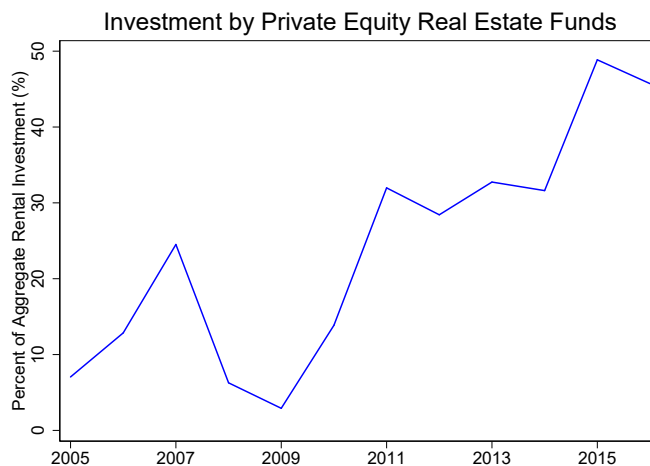
Note: This figure plots the average difference the 10-year TIPS yield in 2008 and the 10-year TIPS yield in the indicated year.

Figure C.12: Manager Characteristics by Average Limited Partner Funding Gap



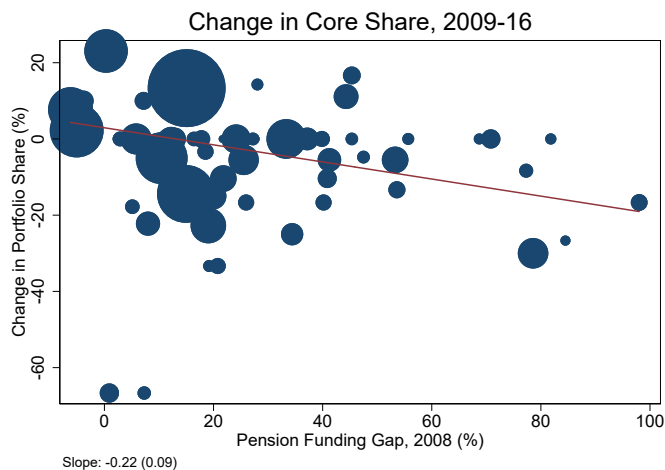
Note: This figure plots the difference in mean for the indicated variable between managers with a high and low average funding gap across limited partners in 2008. High and low are defined according to the median across managers. Variables are normalized to have unit variance. Log Funds Raised is log 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 C.13: Investment by Private Equity Real Estate Funds



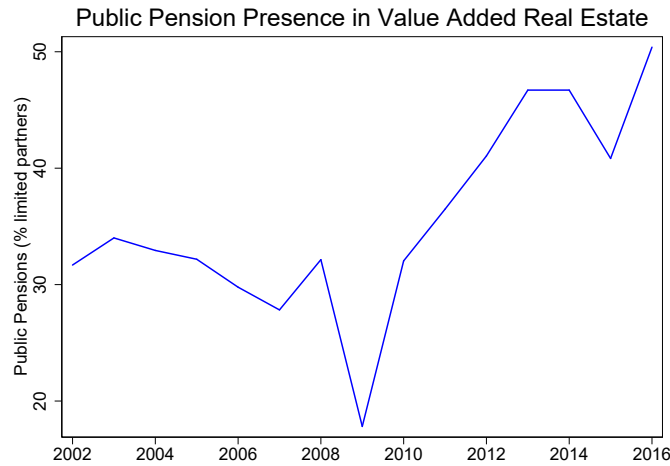
Note: This figure plots investment by private equity real estate funds in U.S. residential real estate as a percent of aggregate tenant occupied residential investment from the Fixed Assets Accounts. Data are from Prequin and the BEA.

Figure C.14: Public Pensions and Buy-and-Hold Funds



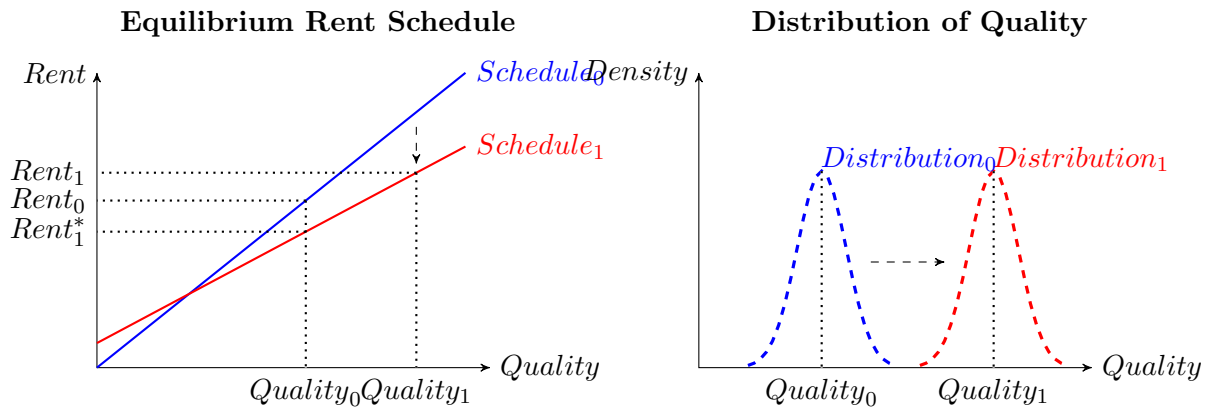
Note: This figure plots the relationship between a pension's: (i) change in the share of private equity real estate portfolio allocated toward buy-and-hold ("core") funds from the 2009-12 period to the 2014-16 period, and (ii) the percent difference between the pension's actuarial liabilities and assets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

Figure C.15: Public Pension Presence in Improvement-Oriented Real Estate Funds



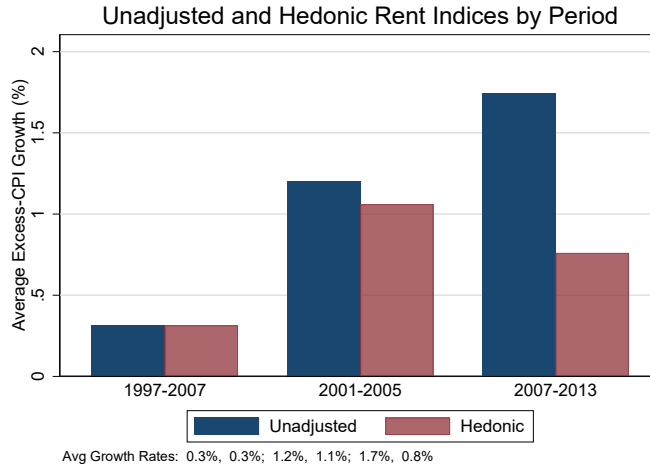
Note: This figure plots the share of investors in improvement-oriented (“value added”) private equity real estate funds that are public pensions by the fund’s vintage year. Data are from Preqin.

Figure C.16: Rent Schedule and Quality Distribution After Increase in Supply of Improvements



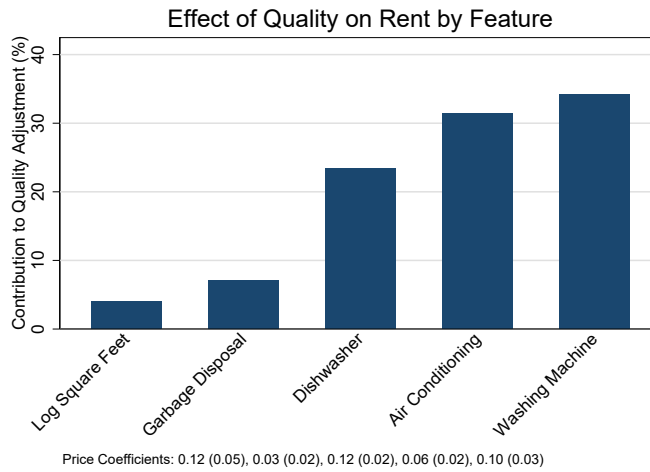
Note: The solid upward sloping line $Schedule_0$ is the equilibrium relationship between a housing unit’s quality and its rent. The blue dashed bell curve $Distribution_0$ is the distribution of quality across the housing stock. Average quality in the initial distribution is $Quality_0$ and the corresponding 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 C.17: Hedonic Rent Index by Period



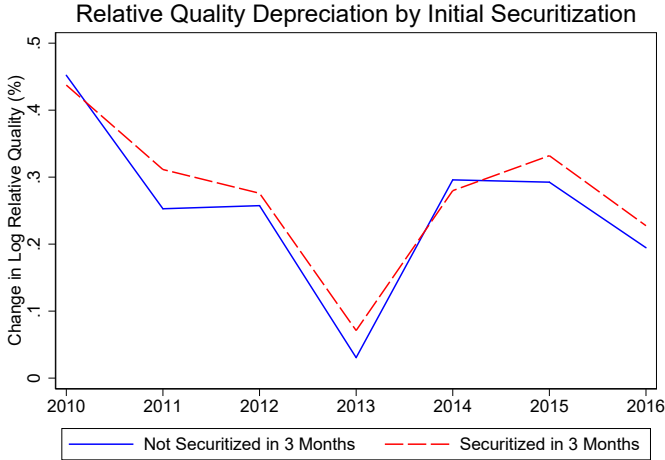
Note: This figure plots unadjusted and hedonic real rent growth for various periods allowing the coefficients in (3.11) to vary by year. Data are from the AHS.

Figure C.18: Contribution to Hedonic Index by Feature



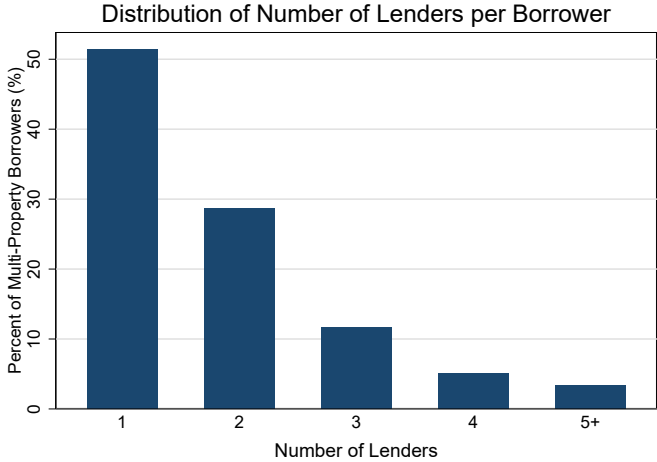
Note: This figure plots the contribution of each feature to the hedonic index, defined as the feature's average price effect from (3.11) across properties and years, divided by the sum of price effects across features. The contribution of feature f is $\frac{\sum_i \sum_t \beta^\Theta \Delta \theta_{i,t}}{\sum_f \sum_i \sum_t \beta^\Theta \Delta \theta_{i,t}}$ 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 (3.11), and so the area underneath the bars sums to 100. Data are from the AHS.

Figure C.19: Relative Quality Measure by Securitization Speed



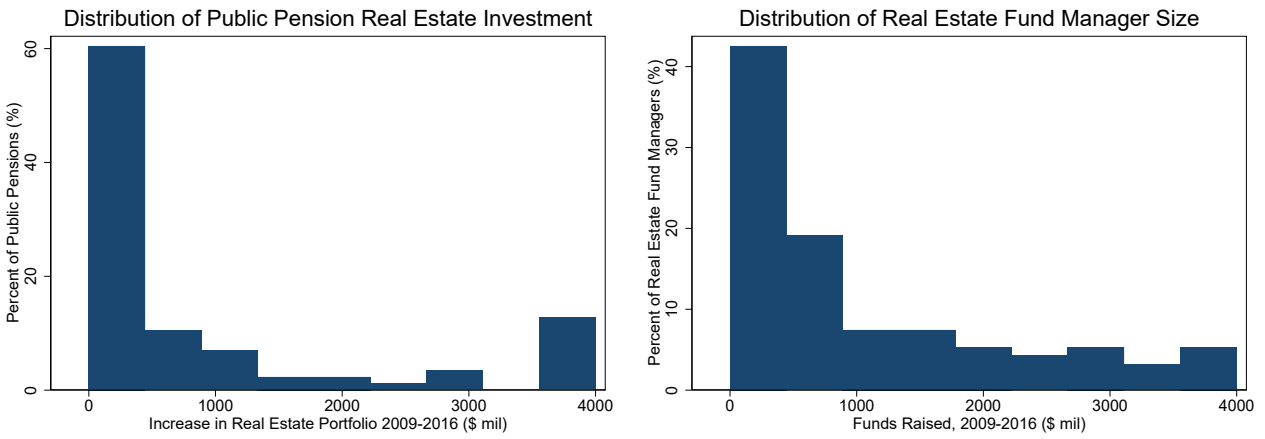
Note: This figure plots the change in log relative quality based on whether the loan was securitized or on the lender’s balance sheet within 3 months of origination. Relative quality is based on the MBA/CREFC rating.

Figure C.20: 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 C.21: 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.

Table C.1: HVCRE Regulation and Other County-Level Outcomes

Outcome:	$\log(\text{Housing Quantity Measure}_{c,t})$		$\text{Rent Growth Measure}_{c,t}$	
Measure:	Construction	Homeless Persons	Average Rent	Quality Premium
	(1)	(2)	(3)	(4)
Bank Share _c × Post _t	-0.719* (0.370)	0.830** (0.334)	0.029* (0.016)	-0.186* (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 (3.6). The specification is similar to column 1 of Table 3.3 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 C.2: Price of Improvement Loans by HVCRE-Affected Lenders

Outcome:	Interest Rate _{ℓ,t}	ARM Margin _{ℓ,t}
Bank _ℓ × Post _t	-0.140* (0.081)	-0.141** (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 (3.4). The specification is similar to column 3 of Table 3.1 with different outcome variables. Interest Rate_{ℓ,t} and ARM Margin_{ℓ,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 C.3: Measuring HVCRE Exposure with the Office Sector

Outcome:	log (Renovated Properties _{c,t})	
	(1)	(2)
Bank Exposure _c × Post _t	0.152* (0.081)	0.335** (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 (3.6). The specification is similar to column 1 of Table 3.3 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 C.5: Loans to Developers and HVCRE Regulation

Outcome:	New Loan _{b,ℓ,t}	
	(1)	(2)
Developer _b × Post _t × CCAR _ℓ	-0.026** (0.008)	-0.028** (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 (C.1). New Loan_{b,ℓ,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_ℓ 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 C.4: Robustness to Heterogeneous Time Trends

Outcome	$\log(\text{Renovated Properties}_{c,t})$				
	(1)	(2)	(3)	(4)	(5)
Bank Share _c × Post _t	0.329** (0.113)	0.285** (0.100)	0.303** (0.102)	0.247** (0.097)	0.283** (0.125)
Characteristic _c × Year-2012 _t	0.061 (0.037)	-0.155** (0.070)	-0.062* (0.035)	-0.013 (0.020)	-0.101* (0.058)
Characteristic _c × Year-2013 _t	0.098** (0.031)	-0.142** (0.071)	-0.080* (0.045)	-0.003 (0.029)	-0.161** (0.062)
Characteristic _c × Year-2014 _t	0.172** (0.074)	-0.426** (0.122)	-0.206** (0.073)	-0.016 (0.050)	-0.272** (0.111)
Characteristic _c × Year-2015 _t	0.139** (0.056)	-0.328** (0.089)	-0.139** (0.055)	-0.034 (0.036)	-0.170* (0.087)
Characteristic _c × Year-2016 _t	0.172** (0.083)	-0.545** (0.124)	-0.210** (0.085)	-0.040 (0.070)	-0.231* (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 (3.6). The specification is the similar to column 2 of Table 3.3 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 (2010b) 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 C.6: Quality Improvements after Loan Renewal

Outcome:	$\Delta \log (\text{Quality}_{i,\ell,t+1})$		
	(1)	(2)	(3)
Due _{<i>i,t</i>}	0.074** (0.025)		
Bank _{ℓ} \times Post _{<i>t</i>} \times Due _{<i>i,t</i>}		0.129** (0.048)	
log (Balance _{<i>i,t</i>})			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 (C.2) and columns 2-3 estimate equation (C.3). 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. Balance_{*i,t*} is the end-of-period loan balance. The outcome $\Delta \log (\text{Quality}_{i,\ell,t+1})$ is the one-year change in log quality, measured with the MBA/CREFC rating. The variables $\Delta \log (\text{Quality}_{i,\ell,t+1})$ and $\log (\text{Balance}_{i,t})$ are normalized to have unit variance. The estimator is OLS except for column 3, where $\log (\text{Balance}_{i,t})$ is instrumented with the triple interaction between Bank _{ℓ} , 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 C.7: Securitization Speed by Loan Purpose

Outcome:	Sec in 3 Months _{<i>k,ℓ,t</i>}
Bank _{<i>ℓ</i>} × Post _{<i>t</i>} × Imp _{<i>k</i>}	-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 (3.3). The specification is similar to column 1 of Table 3.1 except that the outcome differs. Sec in 3 Months _{k,ℓ,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 C.8: County-Level Non-Construction Lending

Outcome:	log (Non-Dev Loans _{<i>c,t</i>})	
	(1)	(2)
Bank Share _{<i>c</i>} × Post _{<i>t</i>}	0.588 (1.154)	0.312** (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 (3.6). The specification is similar to column 1 of Table 3.6 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 Loans _{c,t} is the number of loans issued for non-construction purposes. County controls are those from Table 3.3. 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 C.9: Public Pension Investment in Value Added by Yield Measure

Outcome:	Prob of Commitment ^{VA} _{<i>p,t</i>}		
	(1)	(2)	(3)
Funding Gap _{<i>p</i>} × Yield Gap _{<i>t</i>}	0.141** (0.059)	0.183** (0.062)	0.171** (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 (3.9). The specification is similar to column 1 of Table 3.5 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 Prequin and the CRR.

Table C.10: Safe Real Estate Investments and Pension Risk Taking

Outcome:	Prob of Commitment ^{Core} _{<i>p,t</i>}		
	(1)	(2)	(3)
Funding Gap _{<i>p</i>} × Yield Gap _{<i>t</i>}	-0.130* (0.069)	-0.142* (0.073)	-0.031 (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 (3.9). The specification is similar to column 1 of Table C.9 except that the outcome differs. Prob of Commitment^{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 Prequin and the CRR.

Table C.11: Riskiest Real Estate Investments and Pension Risk Taking

Outcome:	Prob of Commitment ^{Opp} _{<i>p,t</i>}		
	(1)	(2)	(3)
Funding Gap _{<i>p</i>} × Yield Gap _{<i>t</i>}	0.132** (0.059)	0.171** (0.067)	0.145** (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 (3.9). The specification is similar to column 1 of Table C.9 except that the outcome differs. Prob of Commitment^{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 C.12: 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 C.13. 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 C.13: Unconventional Monetary Policy and Short-Term Fluctuations in Yield Gaps

Outcome:	Prob of Commitment ^{VA} _{<i>p,t</i>}	
	(1)	(2)
Funding Gap _{<i>p</i>} × Yield Gap _{<i>t</i>}	0.118* (0.061)	0.446** (0.163)
Funding Gap _{<i>p</i>} × Time-Trend _{<i>t</i>}	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 (3.9). The specification is similar to column 1 of Table 3.5. 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 C.12. 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 Prequin and the CRR.

Table C.14: Placebo Test of Public Pension Investment, 2003-07

Outcome:	Prob of Commitment ^{VA} _{<i>p,t</i>}	
	(1)	(2)
Funding Gap _{<i>p,02</i>} × Yield Gap _{<i>t</i>}	-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 (3.9). The specification is similar to column 1 of Table 3.5 Funding Gap_{*p,02*} is the percent difference between the fund's actuarial liabilities and assets in 2002. Yield Gap_{*t*} is the difference between the 10-year TIPS yield in 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 Prequin and the CRR.

Table C.15: Public Pension Risk Taking and GASB Changes

Outcome:	Prob of Commitment ^{VA} _{<i>p,t</i>}
Funding Gap _{<i>p</i>} × Yield Gap _{<i>t</i>}	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 (3.9). The specification is similar to column 1 of Table 3.5. 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 C.16: Robustness of Public Pension Risk Taking to Distressed Debt

Outcome:	Prob of Commitment ^{DD} _{<i>p,t</i>}	
	(1)	(2)
Funding Gap _{<i>p</i>} × Yield Gap _{<i>t</i>}	0.143* (0.081)	0.134* (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 (3.9). The specification is similar to column 1 of Table C.9 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 C.17: Value Added Investment with Manager-Year Fixed Effects

Outcome:	Fund Formed _{<i>m,k,t</i>}
Funding Gap _{<i>m</i>} × Yield Gap _{<i>t</i>} × VA _{<i>k</i>}	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 C.19: 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
$\Delta \log(\text{Square Feet}_{i,t})$	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; $\Delta \log(\text{Square Feet}_{i,t})$ is the increase in log square feet. A/C denotes air conditioning. Central A/C is conditional on having any air conditioning. All changes are over 2 year intervals. Observations are rental housing unit-years. The sample period is 1997-2013.

Table C.18: Rent Growth and New Features

Outcome:	$\Delta \log(\text{Rent}_{i,t})$
<u>Installment of:</u>	
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 (3.11). 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 C.20: Summary Statistics for Trepp Dataset

	Observations	Mean	Standard Deviation
<u>Property-Level Variables:</u>			
Probability of Renovation $_{i,t}$	30733	0.026	0.158
Bank $_{i,t}$	30733	0.473	0.499
New Loan $_{i,t}$	30733	0.060	0.237
Due $_{i,t}$	143530	0.016	0.126
$\Delta \log(\text{Quality}_{i,t+1})$	143530	-0.152	0.799
$\log(\text{Balance}_{i,t})$	143530	7.856	7.766
<u>County-Level Variables:</u>			
Bank Share $_c$	3169	0.667	0.176
$\log(\text{Renovated Properties}_{c,t})$	3169	0.152	0.375
$\log(\text{Renovated Housing Units}_{c,t})$	3169	0.921	2.152
$\log(\text{Units}_{c,t})$	3169	9.73	1.428
$\log(\text{Rent}_{c,t})$	3169	6.475	0.161
$\log(\text{Income}_{c,t})$	3169	10.772	0.242
$\log(\text{Storms}_{c,t})$	3169	-7.657	1.507
LTV $_{c,t}$	3169	0.879	0.164
Delinquent $_{c,t}$	3169	0.034	0.048
DSCR $_{c,t}$	3169	1.555	0.295
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 Renovation $_{i,t}$ indicates if the property was renovated in t ; Bank $_{i,t}$ indicates if the property owner's lender is a bank; New Loan $_{i,t}$ indicates if a new loan was originated on i in t ; Due $_{i,t}$ indicates whether the property has a loan due in t ; $\Delta \log(\text{Quality}_{i,t})$ is the change in log relative quality, measured using the MBA/CREFC rating; $\log(\text{Balance}_{i,t})$ is the log of end-of-period loan balance. Note that the variables Due $_{i,t}$ through $\log(\text{Balance}_{i,t})$ are used in the extension of Appendix C.2.3. 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; $\log(\text{Renovated Properties}_{c,t})$ is the log number of renovated properties; $\log(\text{Renovated Housing Units}_{c,t})$ is the log number of renovated housing units; $\log(\text{Units}_{c,t})$ is the number of housing units; $\log(\text{Rent}_{c,t})$ is the log average rent per unit; LTV $_{c,t}$ through ARM $_{c,t}$ are the principal weighted values of the following characteristics of outstanding loans: loan-to-value ratio, debt service coverage ratio, adjustable rate mortgage share, and share of 60+ day delinquent loans. The variables $\log(\text{Income}_{c,t})$ and $\log(\text{Storms}_{c,t})$ are log real income per capita for the surrounding MSA and log winter storms per multifamily unit, which were merged from the BEA and NOAA datasets described in Section C.1.2. 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.

Table C.21: Summary Statistics for Preqin Dataset

	Observations	Mean	Standard Deviation
<u>Pension-Level Variables:</u>			
Prob of Commitment $_{p,t}^{VA}$	655	0.611	0.488
Prob of Commitment $_{p,t}^{Core}$	655	0.285	0.452
Prob of Commitment $_{p,t}^{Opp}$	655	0.483	0.500
Funding Gap $_p$	655	0.190	0.193
$\log(\text{Assets}_{p,t})$	655	17.764	1.228
Bond Share $_{p,t}$	655	0.231	0.073
Equity Share $_{p,t}$	655	0.499	0.100
Cash Share $_{p,t}$	655	0.026	0.028
Alternatives Share $_{p,t}$	655	0.165	0.107
<u>Manager-Level Variables:</u>			
Fund Formed $_{m,t}^{VA}$	736	0.083	0.276
$\log(\text{Investment}_{m,t}^{VA})$	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 Commitment $_{p,t}^{VA}$ through Prob of Commitment $_{p,t}^{Opp}$ 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 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 C.1.2. The lower panel summarizes manager-level variables: Fund Formed $_{m,t}^{VA}$ indicates the formation of a value added fund for U.S. residential real estate with vintage t ; $\log(\text{Investment}_{m,t}^{VA})$ 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.

Table C.22: Relationships in Multifamily Mortgage Lending

Outcome:	Loan Originated _{<i>b,ℓ,s</i>}	
	(1)	(2)
Loan Originated _{<i>b,ℓ,s-1</i>}	0.522** (0.028)	0.651** (0.046)
Loan Originated _{<i>b,ℓ,s-1</i>} × log (Properties _{<i>b</i>})		-0.068** (0.024)
log (Properties _{<i>b</i>})		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 (C.6). Loan Originated_{*b,ℓ,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 C.23: Relationships between Pensions and Private Equity Fund Managers

Outcome:	Investment _{<i>p,m,s</i>}	
	(1)	(2)
Investment _{<i>p,m,s-1</i>}	0.224** (0.045)	0.957** (0.250)
Investment _{<i>p,m,s-1</i>} × log (Size _{<i>m</i>})		-0.089** (0.028)
log (Size _{<i>m</i>})		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 (C.7). 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 C.24: 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** (0.058)	0.181** (0.058)	-0.031 (0.057)	-0.029 (0.055)
Underwriter $_{j,u,s-1}$	0.016 (0.020)	0.009 (0.021)	0.288** (0.030)	0.233** (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 (C.8). 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 C.25: MSA Correlates with Improvement Activity

Outcome:	Renovation Probability $_m$
log (Saiz Elasticity $_m$)	-0.120* (0.070)
log (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 (2010b). 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 C.1.