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Author
Reher, Michael

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Finance and the Supply of Housing Quality*

Michael Reher†

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Abstract

I show how financial intermediaries affect rental housing quality and affordability by supplying real estate investors with financing for quality improvement projects (i.e., renovations). First, I document a historic surge in improvement activity since the Great Recession. Then, using exogenous variation generated by a 2015 change in regulatory capital requirements, I find that a reallocation of bank credit toward improvement projects accounts for 24% of quality improvements since 2015. The shock increases the supply of high-quality apartments and lowers their rent. However, it raises the average apartment's rent and accounts for 32% of historically high rent growth over 2015-16.

Keywords: Banks, Housing Quality, Rent, Financial Regulation
JEL Classification: G21, G23, G28, R30

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†University of California San Diego, Rady School of Management. Email: mreher@ucsd.edu

1 Introduction

Over one-third of U.S. households rent their home, and many of these households have experienced historically high housing costs since the Great Recession.[1] These observations have ignited policy discussion about housing affordability, and they have inspired a new research agenda focused on the rental housing market (e.g., Diamond, McQuade and Qian 2019; Howard and Liebersohn 2020; Favilukis, Mabille and Van Nieuwerburgh 2019; Molloy, Nathanson and Paciorek 2020; Gete and Reher 2018). Missing from this discussion is the fact that improvements to rental housing quality (i.e., renovations) have also surged since the Recession, as shown in Figure 1. This new observation is central to discussions about housing affordability, since improvements reduce the supply of relatively inexpensive housing units by transforming them into relatively expensive ones.

I document a recent surge in improvements to rental housing quality, a new fact with direct implications for housing affordability. Then, I show how financial intermediaries have contributed to this surge by reallocating financing toward quality improvements and away from other types of residential investment. Using an unintended regulatory spillover shock from the application of Dodd-Frank bank capital requirements to apartment loans, I find that credit supply accounts for 24% of improvement activity since 2015. Tracing the effects downstream, this shock lowers rent growth on high-quality apartments by increasing their supply, while increasing average rent growth as more apartments become high-quality. These findings exemplify how financial intermediaries can function as suppliers of housing quality and, through this role, affect housing affordability.

In more detail, I first use a variety of proprietary and public datasets to show how improvement activity in the rental housing sector has rebounded to historically high levels since the Great Recession. This surge has been accompanied by an increased share of rental housing units in the upper segments of the quality distribution along with depressed real rent growth in these segments. By contrast, average rent growth remains high by historical standards. Together, these facts are consistent with an outward shift in the supply of high-

[1] According to the Housing Vacancy Survey, 37% of households were renters in 2016, rising to 50% in urban metro areas such as Los Angeles and New York City. The median rent-to-income ratio reached 30% in 2015, its highest level since the 1980s (Gete and Reher 2018).

quality rental housing due to increased improvement activity. This conjecture motivates me to study an exogenous shock to the supply of financing for improvement projects.

I study a credit supply shock for apartment 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's implementation of Basel III international standards, and, thus, they were not motivated by specific features of the U.S. real estate market. HVCRE regulation assigned a 50% lower regulatory risk weight to loans secured by improvements on income-producing properties relative to loans for construction. Thus, this regulatory shock introduced a wedge in the cost of capital for different loan types, incentivizing banks to transfer credit to improvement projects from construction. I verify that banks respond in this way using a triple difference-in-difference methodology that compares the allocation to improvement projects by banks (i.e., "treated lenders") and specialty nonbank lenders within the apartment loan market. The results show how banks and nonbanks follow parallel trends leading up to the introduction of HVCRE regulation, after which banks reallocate loanable funds to improvements. This lender-level finding supports the shock's validity by serving as a "first-stage".

My baseline exercise is a county-level difference-in-difference research design, where a county's treatment exposure is defined by its historical reliance on bank as opposed to nonbank lending. I carry out this research design using a novel dataset on both portfolio and securitized loans originated by both bank and nonbank lenders. As in the lender-level analysis, bank-reliant counties (i.e., "treated counties") and nonbank-reliant counties exhibit parallel trends until the introduction of HVCRE regulation. After the shock, however, bank-reliant counties exhibit significantly higher real improvement activity, based on a variety of outcome measures. The effects are stronger in counties where real estate investors have fewer sources of credit and where renters appear more willing to pay for housing quality (e.g., higher-income renters), suggesting that the shock relaxes constraints on investors' demand for improvement financing.

The baseline results are internally valid insofar as bank and nonbank-reliant counties do not differ in unobserved ways that would affect improvement activity after the 2015 shock. I conduct numerous robustness tests, all of which support the baseline results' internal validity. These include: replicating the baseline analysis with an alternative dataset; controlling for changes in demand from the Government Sponsored Enterprises (GSEs); dropping improvements by unconstrained borrowers; checking that the results do not confound other specific Dodd-Frank regulations; controlling for a battery of county characteristics and state-by-year fixed effects; investigating differences in loan specialization between banks and nonbanks; and evaluating bias from undersampling small regional banks that specialize in construction. I also estimate a property-level difference-in-difference equation, which allows me to rely on a very weak identification assumption that is immune to unobserved characteristics of a county. The property-level results imply that HVCRE regulation raises a property's annual probability of an improvement by 46% (1.2 pps).

Turning downstream, I trace the shock's effects through to commonly used measures of housing affordability. Consistent with the lender-level reallocation, I find that HVCRE regulation reduces county-level apartment construction. Yet, despite this negative effect on the overall supply of apartments, the shock actually increases the supply of high-quality apartments, since it generates improvements that transform low-quality apartments into high-quality ones. This increase in supply is accompanied by lower rent growth on high-quality apartments. By contrast, the shock significantly raises rent growth on the average apartment, reflecting the joint contribution of higher average quality and reduced overall supply. In particular, the shock raises the share of households with a rent-to-income ratio above policymakers' standard definition of "cost-burdened" (e.g., JCHS 2017). The overall welfare content of this outcome is unclear because higher rent also reflects better quality. In distributional terms, however, the shock likely favors higher-income households insofar as housing quality is a normal good.

I assess the aggregate impact of HVCRE regulation using methods from the applied macroeconomics literature (e.g., Chodorow-Reich 2014). Correspondingly, I find that the shock accounts for 24% of quality improvements over 2015-16 and 32% of rent growth, in partial equilibrium. To assess the plausibility of this large effect, I perform a hedonic adjustment with an auxiliary dataset used by statistical agencies. This adjustment calculates the share of rent growth attributable to quality improvements, without taking a stance on the shocks generating these improvements. Accordingly, I find that improvements due to any shock account for 70% of observed real rent growth following the Great Recession. Therefore, the large estimated effect of the specific HVCRE shock that I study is indeed plausible.

The remainder of the paper is organized as follows. I conclude this section by situating my contribution within the related literature. Section 2 describes the paper's data. Section 3 documents several new stylized facts and clarifies my research hypothesis. Section 4 describes the regulatory shock. Section 5 contains my core, county-level analysis. Section 6 assesses internal validity. Section 7 studies implications for housing affordability. Section 8 studies aggregate implications. Section 9 concludes. The online appendix contains additional material.

Related Literature

This paper makes three contributions to the literature. First, the results show how constraining the supply of market-rate (i.e., non-subsidized) housing reduces the supply of more affordable housing through a novel channel, namely, a shift from producing high-quality housing through new construction to producing it through the improvement of low-quality housing. This quality improvement channel works in parallel to the standard supply-and-demand channel (e.g., Asquith, Mast and Reed 2019), and it is conceptually similar to the process of downward filtering (e.g., Rosenthal 2014). In fact, quality improvements themselves constitute upward filtering, and so the results help explain cross-sectional variation in such upward filtering recently documented by Liu, McManus and Yannopoulos (2020).

Second, I show how financial intermediaries play a critical role in urban change by providing financing to real estate investors. In particular, the results suggest incorporating financial frictions into equilibrium models of gentrification (e.g., Guerrieri, Hartley and Hurst 2013; Couture et al. 2020). 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 2020) or improving their own home (Benmelech, Guren and Melzer 2021). In policy terms, a number of recent papers have studied how urban policies, such as tax credits or rent stabilization, affect rental housing markets (e.g., Favilukis, Mabille and Van Nieuwerburgh 2019; Diamond, McQuade and Qian 2019; Diamond and McQuade 2019), and this paper shows how the rental market is also affected by upstream financial regulation. Lastly, a large literature has studied the effect of financial markets on housing markets in the owner-occupied sector, and this paper is among a smaller set to study that effect within the rental market (e.g., Greenwald and Guren 2020; Gete and Reher 2018).

Third, viewing housing quality as a good and improvement projects as a technology used to produce it, my findings support a large and diverse literature showing how financial intermediaries affect the level and allocation of productive resources. By estimating these effects using a regulatory spillover shock, I contribute to a literature on the unintended impact of Dodd-Frank regulations on housing markets (e.g., Defusco et al. 2020; D'Acunto and Rossi 2021).[2] Of course, the unintended effect of HVCRE regulation on housing affordability must be weighed against the intended effects on financial stability and the banking sector, as studied by Glancy and Kurtzman (2018).

2 Data

My core analysis relies on two datasets, both of which are provided by Trepp LLC. Together, they cover a representative panel of apartment properties over 2010-16. I also rely on multiple auxiliary datasets to assess the robustness of my core findings. All of these datasets are described in detail in Appendix A, and I provide a concise overview below.

Before doing so, I introduce some important terminology that will clarify the rest of the paper. I define "quality" as a structural feature of a shelter. I will use "improvement" as the general term for an increase in quality, which will include large-scale projects that require the inhabitant to vacate (e.g., renovations) as well as small-scale ones (e.g., installing an air conditioner). Most of my analysis takes place in the apartment sector, and I will use

[2] Relatedly, I contribute to a literature on the rise of shadow banks by providing new evidence that capital requirements affect nonbanks' market share (e.g., Buchak et al. 2018; Kim et al. 2018; Fuster et al. 2019; Irani et al. 2021; Chernenko, Erel and Prilmeier 2020; Ganduri 2020; Gete and Reher 2021).

the word "apartment" to refer to the individual housing unit and "property" to refer to the entire set of units under common ownership. Finally, the "borrowers" in my analysis are professional real estate investors, not owner-occupants seeking home improvement loans.

2.1 Core Datasets

The first source of data is Trepp's Anonymized Loan Level Repository (T-ALLR) dataset. The T-ALLR dataset contains regularly updated information on bank-originated loans secured by apartment properties. Importantly for the purposes of this paper, the T-ALLR dataset consists of loans that remain on the bank's balance sheet (i.e., portfolio loans). Data on portfolio loans are considered highly confidential in the U.S. and are thus difficult to acquire, but incorporating them into the analysis is important given my focus on the effects of bank capital requirements in Section 5.

While attractive due to its coverage of portfolio loans, the T-ALLR dataset does have three drawbacks. First, the dataset only includes loans originated by banks, whereas my identification strategy will require information about both bank and nonbank lenders. Second, the raw data cover only 10% of U.S. counties, or 52% on a population-weighted basis. Third, I observe whether a loan finances construction, but, among the remaining loans that are secured by income-producing properties, I cannot explicitly identify those that finance an improvement.

Given these constraints, I also incorporate Trepp's T-Loan dataset into my core analysis. The T-Loan dataset contains regularly updated information on apartment loans that have been securitized as commercial mortgage backed securities (CMBS). Consequently, the T-Loan dataset includes loans originated by both bank and nonbank originators, whom I will simply call "lenders" to maintain a consistent terminology. The largest of these lenders are listed in Appendix Table A1. In terms of breadth, the T-Loan dataset covers 90% of population-weighted counties, and it includes a rich set of property-level variables, such as rent, size, occupancy, and, importantly, the history of renovations on the property.

Comparing the two sources of data, bank-originated loans observed in the T-ALLR dataset encode stronger regulatory incentives than those in the T-Loan dataset, since securitized loans only incur capital requirements during the warehouse period or through a risk-retention ratio, as discussed in Section 4.1. In this sense, the T-ALLR dataset is superior. On the other hand, the T-Loan dataset has wider geographic coverage and richer property-level information. Given these tradeoffs, I combine the two Trepp datasets in my core analysis.

2.2 Auxiliary Datasets

I draw on multiple auxiliary datasets to ensure that my core findings from the combined Trepp dataset are robust. The two most significant of these auxiliary datasets are a dataset on apartment transactions from Real Capital Analytics (RCA) and the Census' American Housing Survey (AHS) dataset.

The RCA dataset covers transactions on apartment properties over 2009-16. I observe the lender and loan size associated with the transaction, the history of renovations on the transacting property, and its location. Together, these variables enable me to replicate my core analysis with the RCA dataset, which, as I will show in Table 3, leads to similar results. While useful for evaluating robustness, the RCA dataset contains a limited set of outcome variables, and it does not cover improvements on non-transacting properties because the raw data are collected from transactions (CREDA 2017). Therefore, given this paper's focus, I do not use the RCA dataset as my primary source of data.

The AHS dataset is nationally representative and contains information about a housing unit's rent, the demographic profile of the occupant, and granular information about the housing unit's physical features. The granularity and representativeness of the AHS dataset make it ideal for the quality adjustment exercise in Section 8.1, where I assess the plausibility of the core results' aggregate implications. However, the AHS dataset has rather coarse geographic information, as I only observe a housing unit's metropolitan statistical area (MSA) and only for a subset of 43% of MSAs. In addition, the AHS dataset's panel structure broke because of a redesign in 2015, and so I cannot follow the same housing unit throughout the entirety of my core sample period of 2011-16. For these reasons, the AHS dataset complements my core dataset but cannot substitute for it.

3 Stylized Facts

I motivate the paper by documenting two stylized facts about rental housing quality and affordability, shown in Figure 1. Panel (a) documents a surge in quality improvement activity since the Great Recession by plotting the percent of apartments that are renovated each year, based on the T-Loan dataset. This annual probability of renovation vigorously recovered from its 2008 low and surpassed its pre-Recession high by 2014. Appendix Figure A1 replicates this finding using three separate measures of improvement activity: aggregate investment in residential improvements, based on the U.S. Fixed Assets Accounts; the share of bank portfolio loans that proxy for financing an improvement, based on the T-ALLR dataset; and the probability an apartment transaction is followed by a renovation, based on the RCA dataset.

Panel (b) of Figure 1 documents a negative cross-sectional correlation between housing quality and rent growth. Using the AHS dataset, I sort renters into quintiles based on log real income relative to the MSA-year average, a proxy for quality segment. Next, I plot annualized real apartment rent growth for each segment over 2011-17. While real rent grew at least 2.9% per year for the bottom three quintiles, it only grew at a rate of 0.9% for the top quintile. This pattern is robust to a zip code-level analogue based on Zillow's apartment rent index, also shown in Appendix Figure A1.

To the best of my knowledge, the trends shown in Figure 1 have not yet been documented in the literature. My goal in the remainder of the paper is to assess how shifts in the supply of financing have contributed to them. Before doing so, I briefly describe my principal research hypothesis.

3.1 Research Hypothesis

To fix ideas, suppose there is a distribution of rental housing quality, where, as mentioned in Section 3, quality is defined as a structural feature of a shelter (e.g., air conditioning). An improvement project raises the quality of a housing unit, thereby moving it from a lower segment of the quality distribution to a higher segment. Real estate investors own the rental housing stock and perform improvement projects. To do so, they rely on outside financing, which is consistent with the observation that 70% of improvements occur on a mortgaged property according to the 2015 Rental Housing Finance Survey (RHFS).

Financial intermediaries supply real estate investors with financing to perform improvement projects. Consider a shock that incentivizes intermediaries to supply more improvement financing, such as the regulatory spillover shock described in Section 4.1. The increase in supply can take two forms: a relaxation of borrowing constraints for constrained investors (i.e., borrowers), or lower interest rates for unconstrained investors, holding credit risk fixed. Table 6 and Appendix Table A5 will respectively document these two channels. Both channels imply an increase in quality improvement activity, as Table 2 will show. The specific supply shock I study entails a reallocation of financing toward improvements and away from construction due to a wedge in regulatory capital requirements, and so I verify that it also reduces construction activity in Table 7.

The increase in quality improvement activity affects housing affordability by expanding the supply of high-quality housing at the expense of low-quality housing, as I will also show in Table 7. In particular, high-quality housing units command a premium in rent over low-quality units, reflecting households' willingness-to-pay for quality. Therefore, by raising the average housing unit's quality, the increase in quality improvement activity also raises the average housing unit's rent, as Table 8 will show. However, households must accommodate the increase in the supply of high-quality housing units, which necessitates a reduction in the quality premium. Thus, while the average housing unit's rent increases, the rent on high-quality units falls, as I will also show in Table 8.

4 Identification

My goal is to estimate the effect of financial supply on quality improvement activity, and, subsequently, to trace this effect through to measures of housing affordability. I identify the effect through a regulatory spillover shock that increases the supply of bank credit for apartment improvements. In this section, I describe the shock and provide graphical evidence of its effect.

4.1 High Volatility Commercial Real Estate

In January 2015, U.S. bank regulators increased the regulatory capital risk weight on certain apartment loans, called High Volatility Commercial Real Estate (HVCRE) loans, from 100% to 150%. This means that banks must reserve at least $\$1.50 \times K$ in equity capital for every \$1 of HVCRE credit, where K is the regulatory minimum capital ratio (e.g., 6%). HVCRE loans are for the "development or construction of real property", which I will simply refer to as "construction" (U.S. Code §1831bb(b)(1)(A)).[3] By contrast, loans for "improvements to existing income-producing real property" (U.S. Code §1831bb(b)(2)(C)) were not subject to this increase and retained the substantially more modest weight of 100%. As its name implies, HVCRE regulation affects loans secured by apartment properties because apartments are considered "commercial real estate", but it exempts loans secured by single-family homes (U.S. Code §1831bb(b)(2)(A)).[4] Importantly, the particular HVCRE risk weights were chosen to conform with international standards set by the Basel III Accords, and, thus, they are unrelated to specific features of the U.S. real estate market.

If the Modigliani-Miller theorem fails, then HVCRE regulation can increase the supply of bank credit for improvement projects based on the following logic. First, if it is sufficiently costly for banks to raise equity capital compared to debt, then regulatory capital require-

[3]In addition, the loan must satisfy any of the following underwriting conditions met by most construction projects (Chandan and Zausner 2015): the loan-to-value (LTV) ratio exceeds 80%; the terms allow capital withdrawals; or the borrower's contributed capital is less than 15% of the project's as-completed value.

[4]The institutional details described in this section are based on the codified definition of HVCRE loans, which did not appear until after HVCRE regulation became effective in 2015. In addition, the U.S. Senate modified HVCRE regulation after this paper's period of analysis as part of its Economic Growth, Regulatory Relief, and Consumer Protection Act (S. 2155).

ments bind. A large literature summarized by Dagher et al. (2016) has found this to be the empirically relevant case during periods of regulatory transition. Consequently, banks must raise $\$1.00 \times K$ in equity capital for a \$1 loan that finances an improvement project, relative to $\$1.50 \times K$ for one that finances construction. To minimize its regulatory burden while maintaining the same exposure to commercial real estate projects as a whole, a bank can respond by reallocating loanable funds toward improvements and away from construction. Table 6 will document this behavior formally, and, consistent with theory, Appendix Table A5 will show how it is especially pronounced among poorly capitalized banks. While, in principle, a bank could respond by simply originating fewer construction loans, Table 6 will show how banks actually increase their improvement lending, thereby maintaining their total exposure to commercial real estate projects. These predictions are similar to those conjectured by Greenwood et al. (2017).

The previous logic applies most strongly to portfolio loans, since they remain on a bank's balance sheet and so necessitate that the bank reserve equity capital for an extended period. In the case of securitized loans, a bank must reserve equity capital throughout the loan's warehouse period, during which it is booked as held-for-sale and subject to standard risk weights (FFIEC 2015). The average warehouse period in the T-Loan dataset is 6 months, which is long enough to incentivize some form of regulatory arbitrage. After securitization, capital requirements still bind through the retained portion of a loan. The retained portion is typically 5% due to risk retention rules (e.g., Flynn, Ghent and Tchistyi 2020; Willen 2014), although loans purchased by the Government Sponsored Enterprises (GSEs) are exempt from these rules. For these reasons, securitized loans still encode regulatory incentives, although more weakly than portfolio loans. Therefore, I incorporate both loan types into my core analysis.

Mapping to a difference-in-difference setup, banks constitute "treated lenders", whereas nonbanks are not subject to capital requirements and so constitute "control lenders" (e.g., Buchak et al. 2018). I define the post-2015 period as the "treatment period", corresponding to the period after which HVCRE regulation was introduced.[5] While the regulation's an-

[5]In studying the effect of HVCRE regulation on interest rates, Glancy and Kurtzman (2018) define the "treatment period" as the period following the regulation's announcement (i.e., post-2013). However, in

nouncement occurred in 2013 as part of the U.S. implementation of Basel III, lenders were sufficiently confused about the regulation's details that they did not adjust ex-ante (e.g., Mortgage Bankers Association 2018). As evidence of this confusion, federal regulators issued a clarificatory statement in March 2015 (FDIC 2015). Indeed, the graphical evidence presented in the next subsection shows how lenders do not adjust ex-ante, and so I take 2015 as the shock year.

4.2 Graphical Evidence

I begin with two pieces of graphical evidence supporting the interpretation of HVCRE regulation as a positive shock to the supply of financing for improvement projects. First, panel (a) of Figure 2 plots the time series of the aggregate distribution between improvement and construction projects. The distribution is stable leading up to the HVCRE shock, after which it tilts sharply toward improvements. Panel (b) shows how this shift in aggregate project composition comes with a sharp increase in quality improvement activity relative to its pre-HVCRE trend, as measured by the share of apartments that are renovated each year. The figure is based on the T-Loan dataset because of its breadth and depth, but Appendix Figure A1 documents similar dynamics using the T-ALLR and RCA datasets.

Second, Figure 3 verifies that banks (i.e., "treated lenders") increase their allocation to improvements after the HVCRE shock. The figure plots the year-by-year average difference in log loan originations between renovation vs. construction projects between bank vs. nonbank lenders, based on the T-Loan dataset in which I observed the lender's identity. There is no ex-ante adjustment before the HVCRE shock (i.e., no pre-trend), substantiating the claim made in the previous subsection. After the shock, however, banks significantly tilt their portfolio toward improvement loans relative to nonbanks. This shift in lending behavior constitutes the HVCRE shock's "first stage", and I perform numerous additional

addition to the reasons described in the text, I do not use this definition because my setting differs from Glancy and Kurtzman (2018). First, I focus on the quantity of loans originated rather than the interest rate conditional on origination, and interest rates are presumably forward-looking. Second, whereas Glancy and Kurtzman (2018) only study portfolio loans, my data also include securitized loans, for which there is less incentive to adjust ex-ante because capital requirements primarily bind during the warehouse period. Lastly, loans originated before 2015 were grandfathered once HVCRE regulation was codified, although grandfathering occurred only after codification.

tests in Section 6.8 to verify the validity of this first-stage effect. I refer to the lender-level effect as a "first stage", since it partly captures shifts in bank vs. nonbank market share and does not necessarily imply an increase in real quality improvement activity. Therefore, I conduct my core analysis at the county-level, as now described.

5 Effect on Quality Improvements

My core analysis is a county-level difference-in-difference research design in which I estimate the effect of HVCRE regulation on quality improvement activity. By definition, quality improvements raise the supply of more-expensive housing while reducing the supply of less-expensive housing. Thus, the results in this section bear directly on discussions about housing affordability, and I will use the same research design introduced below to make this link explicit in Section 7.

5.1 Baseline Specification

Aggregating to the county-level enables me to assess the real effects of HVCRE regulation, and so I estimate the following regression equation over 2011-16,

$$Y_{c,t} = \beta \left(\textit{Bank Share}_c \times \textit{Post}_t \right) + \alpha_c + \alpha_t + \gamma X_{c,t} + u_{c,t}, \tag{1}$$

where c and t index counties and years; $\textit{Bank Share}_c$ measures banks' share of apartment loan balances in 2010, as opposed to nonbanks' share; $\textit{Post}_t$ indicates if t is greater than or equal to 2015; $Y_{c,t}$ is one of several measures of quality improvement activity; and the controls in $X_{c,t}$ include contemporaneous measures of local housing and credit demand and, in some specifications, state-year fixed effects.[6]

[6] County controls are: log real income per capita for the surrounding MSA, based on data from the Bureau of Economic Analysis; log winter storms, based on data from the National Oceanic and Atmospheric Association; and the principal-weighted averages of the loan-to-value ratio, debt service coverage ratio, adjustable rate mortgage share, and 60+ day delinquency rate on existing apartment loans, based on the T-Loan dataset. I follow standard practice and add one to the variable before taking the log whenever the variable can equal zero. Observations are weighted by the county's average number of apartments over 2011-16, based on the T-Loan dataset.

Interpreting equation (1), the "treatment" is the introduction of HVCRE regulation in 2015, and "treated counties" are those that have historically relied on bank financing, as parameterized by $\textit{Bank Share}_c$. I measure $\textit{Bank Share}_c$ in three ways. First, I calculate

$$\textit{Portfolio Bank Share}_c = \frac{\textit{Bank Portfolio Balances}_c}{\textit{Bank Portfolio Balances}_c + \textit{All Securitized Balances}_c}, \tag{2}$$

where $\textit{Bank Portfolio Balances}_c$ is the value of bank portfolio loan balances in 2010, based on the T-ALLR dataset; and $\textit{All Securitized Balances}_c$ is the value of bank and nonbank securitized loan balances in 2010, based on the T-Loan dataset.[7] Since 84% of apartment loans are either securitized or held on banks' portfolios (Rosengren 2017), the denominator in equation (2) approximates the total value of apartment loan balances in 2010. To limit sample attrition, I impute a value of zero for bank portfolio balances in counties covered by the T-Loan dataset but not by T-ALLR.

Next, I complement $\textit{Portfolio Bank Share}_c$ with the following measure,

$$\textit{Securitized Bank Share}_c = \frac{\textit{Bank Securitized Balances}_c}{\textit{Bank Portfolio Balances}_c + \textit{All Securitized Balances}_c}, \tag{3}$$

where $\textit{Bank Securitized Balances}_c$ is the value of bank securitized loan balances in 2010, based on the T-Loan dataset. Many specifications control for both $\textit{Portfolio Bank Share}_c$ and $\textit{Securitized Bank Share}_c$ to assess the explanatory power of each measure. In theory, HVCRE regulation should primarily operate through $\textit{Portfolio Bank Share}_c$, since, as discussed in Section 4.1, banks' incentive to substitute toward improvement loans is weaker among loans securitized after a warehouse period of around 6 months.[8]

[7] Taking the unweighted sum of balances observed in the T-ALLR and T-Loan datasets as in the denominator of equation (2) leads to measurement error because neither dataset fully represents each U.S. county. I address this measurement error by reweighting securitized loan balances such that the share of loan balances held on banks' portfolios in the combined Trepp dataset matches the national accounts, as described in Appendix A.6.

[8] In principle, some of the loans observed in the T-Loan dataset may have been originated by banks who intended to hold the loans on their balance sheets but later sold them to CMBS conduits. Such loans would encode the same regulatory incentives as portfolio loans observed in the T-ALLR dataset. This situation may apply to loans securitized more than 15 months after origination, corresponding to 10% of volume in the T-Loan dataset. However, such loans would be exceptional because information asymmetries in the post-origination CMBS market led to unravelling in the early 2000s (e.g., An, Deng and Gabriel 2011). I thank an anonymous referee for pointing this out.

Finally, I combine equations (2) and (3) to calculate the composite measure

$$\textit{Total Bank Share}_c = \textit{Portfolio Bank Share}_c + \textit{Securitized Bank Share}_c. \tag{4}$$

This composite measure parsimoniously encodes the incentives generated by HVCRE regulation and can be calculated for 79% of population-weighted U.S. counties. Table 1 provides summary statistics for all of the exposure and outcome variables used in my core analysis.

The parameter β in equation (1) recovers the effect of HVCRE regulation on quality improvement activity, provided the following identification assumption holds,

$$\mathbb{E}\left[\textit{Bank Share}_c \times \textit{Post}_t \times u_{c,t} \middle| \alpha_c, \alpha_t, X_{c,t}\right] = 0. \tag{5}$$

In words, assumption (5) says that counties where banks have historically held a large share of the apartment loan market are not predisposed to non-HVCRE shocks to improvement activity that coincide with the introduction of HVCRE regulation. Measuring $\textit{Bank Share}_c$ in terms of balances helps support the validity of this assumption, since, unlike originations, balances reflect expectations that were formed longer in the past. Moreover, the county fixed effect α_c absorbs slow-moving, county-specific factors that encourage improvement activity (e.g., geography), while the year fixed effect α_t absorbs aggregate factors affecting all counties at the same time (e.g., interest rates).

I devote Section 6 to assessing the validity of assumption (5). However, as a first pass, Figure 4 provides evidence that the assumption is not violated because of a pre-trend in quality improvement activity. The figure plots the year-by-year average difference in log renovated properties between counties with above and below-average values of $\textit{Total Bank Share}_c$. The two sets of counties exhibit parallel trends leading up to the HVCRE shock, after which quality improvement activity increases significantly in counties with above-average exposure. This finding supports the validity of assumption (5).

5.2 Baseline Results

Table 2 reports the results from estimating equation (1). I begin by estimating equation (1) using the restricted set of counties and outcome variables observed in the T-ALLR dataset, where regulatory incentives are encoded most strongly. The results in column 1 imply that counties with a higher value of *Portfolio Bank Share*$_c$ see a significant post-HVCRE increase in bank loans for non-construction purposes, a proxy for improvement activity given that I do not directly observe improvements in the T-ALLR dataset. This specification is restrictively pure because it relies exclusively on portfolio loans, but it nonetheless implies that the HVCRE shock increases quality improvement activity. In the remainder of the table, I relax these restrictions on sample and outcome variables.

The outcome in columns 2-4 is the log number of renovated properties, based on the T-Loan dataset. The results across these columns imply that improvement activity increases after the introduction of HVCRE regulation in counties with a higher level of exposure, as measured by *Portfolio Bank Share*$_c$ and *Securitized Bank Share*$_c$ separately. Consistent with theory, the results appear to be driven by exposure to banks who originate portfolio loans (i.e., *Portfolio Bank Share*$_c$). The point estimates are similar with or without county controls or state-year fixed effects, but including these additional terms reduces the standard error because they absorb some of the residual variation.

In the remaining columns, I assess the magnitude of the effect by using the composite exposure measure, *Total Bank Share*$_c$. To interpret the point estimate in column 5, counties with a 10 pps higher initial bank share see around a 3.2% increase in renovated properties after the introduction of HVCRE regulation. In column 6, I study log number of renovated apartments. The result is qualitatively similar to its counterpart in column 5, and the larger point estimate likely reflects economies of scale that incentivize improvements on larger properties. Finally, in column 7, I study log total dollar revenue of renovated apartments and obtain a consistent result. Across columns, the point estimates are large enough to imply significant aggregate effects, as I will discuss in Section 8.

In summary, this section shows how an increase in the supply of bank-intermediated financing induced by HVCRE regulation increases real quality improvement activity over

2015-16, thereby exchanging less-expensive housing with more-expensive housing. I assess the internal validity of these results in the next section.

6 Robustness

I perform multiple tests in this section to evaluate: sensitivity to data provider, the exclusion restriction in equation (5), and the strength of the shock's first-stage effect on lending behavior. The results of all these tests support the validity of research design described in the previous section. Establishing such validity is important, since I will again use this research design in Section 7 to assess how the HVCRE-induced increase in improvement activity affects housing affordability. Appendix B contains additional robustness tests.

6.1 Sensitivity to Data Provider

I assess the baseline results' sensitivity to the use of Trepp's data by reperforming my core analysis using the RCA dataset. As mentioned in Section 2, I observe renovations in the RCA dataset and so can calculate $\textit{Renovated Properties}_{c,t}$, with the caveat that the resulting variable will omit renovations on non-transacting properties. Moreover, because the RCA dataset includes loans originated by both bank and nonbank lenders, I can calculate the exposure variable, $\textit{Total Bank Share}_c$. Importantly, the RCA dataset includes both portfolio and securitized loans, and so, as with the core Trepp dataset, the resulting exposure variable will fully encode the regulatory incentives generated by HVCRE regulation.

In columns 1-5 of Table 3, I estimate specifications analogous to those in columns 1-5 of the baseline Table 2 after calculating $\textit{Renovated Properties}_{c,t}$ using the RCA dataset. As in the baseline table, the results imply that counties more exposed to HVCRE regulation see a significant increase in quality improvement activity, and the point estimate in column 5 is almost identical to its analogue from column 5 of the baseline table. Also consistent with the baseline results, the results appear to be driven by exposure to bank portfolio lending. To reiterate, the renovations observed in the RCA dataset occur on properties financed by both portfolio and securitized loans, which means that the baseline results in Table 2 cannot be driven by the fact that *Renovated Properties*$_{c,t}$ is calculated using the T-Loan dataset in that table.

In column 6 of Table 3, I calculate both the outcome and exposure variables using the RCA dataset and find a similar result as in the analogous specification from column 5 of the baseline Table 2. Finally, in column 7 of Table 3, I calculate the outcome variable using the Trepp dataset (i.e., T-Loan) and the exposure variable using the RCA dataset, and again I find a similar result as in the baseline table. That the RCA-calculated exposure variable can also explain Trepp-calculated renovations provides strong support for the Trepp-calculated exposure variable used in my core analysis.

Summarizing, the similarity of results across datasets strongly supports the validity of the core research design. In particular, the results of this exercise imply that the baseline findings are not biased due to sample selection in either the T-ALLR or T-Loan components of the core Trepp dataset.

6.2 Exposure to Government Sponsored Enterprises

The Government Sponsored Enterprises (GSEs) purchase almost 50% of apartment loans and, therefore, play a critical role in this market (e.g., Passy 2019). Moreover, federal regulations affecting the GSEs' behavior changed over the period of analysis, with, for example, a relaxation of apartment lending caps in 2016. The baseline results could be biased upward if counties more exposed to HVCRE regulation are also more exposed to an expansion of GSE-financed improvement loans.[9]

To assess this possibility, I reestimate equation (1) after controlling for the share of securitized loan balances backed by the GSEs, denoted *GSE Share*$_{c,t}$. This variable absorbs the effect of changes in the GSEs' behavior. If indeed such changes drive the baseline estimates, then the estimated coefficient on *Total Bank Share*$_c$ $\times$ *Post*$_t$ should be close to zero. However, the resulting coefficient of interest in Table 4 is similar to its analogue from

[9]I thank an anonymous referee for raising this possibility.

the baseline Table 2. This finding suggests that the baseline results are not biased due to changes in the GSEs' behavior, thereby supporting their validity.

6.3 Large Borrowers

Securitized apartment loans are typically larger than portfolio loans (e.g., Ghent and Valkanov 2016), and so the T-Loan dataset may overrepresent large borrowers who are unlikely to face credit supply constraints. Therefore, since I measure many of my outcome variables using the T-Loan dataset, the baseline results may spuriously capture channels orthogonal to the HVCRE shock if they are driven by such borrowers. To evaluate whether such unconstrained borrowers drive the results, I recalculate $\textit{Renovated Properties}_{c,t}$ after excluding renovations by borrowers who obtain credit from more than one distinct source in 2010. Then, I reestimate equation (1) and report the results in columns 1-3 of Appendix Table A2. The estimated coefficient of interest is similar to that from the baseline Table 2, which implies that the baseline results are not driven by large, unconstrained borrowers.

6.4 Loan Reclassification

Banks have an incentive to underreport HVCRE lending, per the 2017 Conference of State Bank Supervisors' report. To the extent that banks reclassify "true" construction as an improvement project, the baseline results may overestimate the shock's real effect on quality improvement activity. Since I cannot verify whether a property's foundation has truly changed, thus officially qualifying the project as construction, I assess the scope for such bias by excluding potentially reclassified renovations. I define such renovations as occurring on either very new (i.e., under 3 years old) or very old properties (i.e., built before 1940), in which the property's foundation is more likely to have changed. Reperforming the baseline analysis after excluding such renovations yields similar estimates, as shown in columns 4-6 of Appendix Table A2. Thus, while reclassification may indeed occur, it does not appear to bias the results.

6.5 Constrained Demand as an Amplification Mechanism

Building on the exercise from Section 6.3, quantitative models (e.g., Bernanke and Gertler 1989) typically require borrowing constraints to generate large real effects. I now assess the role of constrained demand for improvement financing as an amplification mechanism that drives the baseline results. I test this hypothesis by reestimating equation (1) after interacting the treatment variable with county characteristics that proxy for borrowing constraints or the level of unconstrained demand for improvement financing.

Appendix Table A3 reports the results of this exercise. Column 1 shows how the effect of the HVCRE shock is stronger where the average borrower obtains credit from fewer distinct sources, a proxy for constraints on her ability to access credit due to information asymmetries. In column 2, the effect is stronger in higher-income counties, which may reflect how higher-income renters are more willing to pay for higher-quality housing (e.g., Jaravel 2019; Handbury 2019). Thus, real estate investors (i.e., borrowers) in such counties have higher unconstrained demand for improvement financing, so that an increase in financial supply has larger real effects. Similarly, column 3 implies a stronger effect in the absence of rent stabilization policies. Such policies discourage real estate investors from making improvements by limiting their ability to raise rent, thus reducing investors' unconstrained demand for improvement financing.

Together, the results in Appendix Table A3 suggest that HVCRE regulation relaxes borrowing constraints on real estate investors, per column 1, and it has stronger real effects where these investors' unconstrained demand for improvement financing is higher, per columns 2 and 3. These findings are consistent with the predictions of quantitative models, thereby supporting the validity of the baseline results.

6.6 Confounding Regulatory Shocks

In addition to capital requirements, the post-2010 period saw the introduction of the Liquidity Coverage Ratio (LCR) and the Net Stable Funding Ratio (NSFR), both associated with Basel III Accords. However, the absence of a lender-level pre-trend in Figure 3 makes it unlikely that the results confound these other two regulations. In particular, unlike capital risk weights, the liquidity risk weights associated with the LCR do not vary by project type, and Gete and Reher (2021) show that much of the adjustment to the LCR occurred in 2014. Moreover, the U.S. version of the NSFR was not proposed until May 2016.

6.7 Property-Level Analysis

Taking advantage of my detailed microdata, I construct a property-level difference-in-difference research design that is analogous to the core, county-level analysis. This approach allows me to include county-year fixed effects so that I can rely on a very weak identification assumption. The regression equation is

$$\textit{Probability of Renovation}_{i,\ell,t} = \beta\left(\textit{Bank}_{\ell} \times \textit{Post}_{t}\right) + \alpha_{c(i),t} + \alpha_{i,\ell} + u_{i,\ell,t}, \tag{6}$$

where i, ℓ, and t index properties, lenders, and years; $\textit{Bank}_{\ell}$ indicates if the property owner's lender is a bank; $\textit{Probability of Renovation}_{i,\ell,t}$ indicates whether a renovation occurs in t; and $c(i)$ denotes the county to which i belongs. 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. I use the T-Loan dataset for this exercise because it allows me to calculate all of the variables in equation (6). The associated summary statistics are shown in Appendix Table A4.

Table 5 reports the results. The point estimate implies that properties whose owner relies on bank financing have a 1.2 pps higher annual probability of renovation after the introduction of HVCRE regulation. Quantitatively, the effect is equal to 46% of the unconditional property-level probability of 2.6%. These property-level results support the validity of the baseline findings from Section 5.

6.8 Verifying the First-Stage Effect

I now verify that HVCRE regulation increases the supply of bank credit for improvement projects, which serves as the "first stage" for my main, county-level analysis in Section 5. All of the exercises in this subsection rely on the T-Loan dataset because it covers properties financed by both bank and nonbank lenders, whose identity I observe. This enables me to construct a lender-level dataset, summarized in Appendix Table A4.

6.8.1 First-Stage Effect: Triple Difference-in-Difference

First, I estimate a triple difference-in-difference equation that intuitively asks whether lenders more exposed to HVCRE regulation, namely banks, shift their lending from construction to improvement projects more than nonbanks after the policy's introduction. Separating loans by the type of project they finance allows me to include lender-year fixed effects, which absorb confounding shocks to the overall level of lending due to, say, other Dodd-Frank regulations.

I estimate the following equation over 2011-16,

$$\begin{aligned} Y_{k,\ell,t} &= \beta \left(Bank_\ell \times Post_t \times Renovation_k \right) + ... \\ &... + \gamma \left(Bank_\ell \times Renovation_k \right) + \alpha_{\ell,t} + \alpha_{k,t} + u_{k,\ell,t}, \end{aligned} \tag{7}$$

where k, ℓ, and t index loan purpose, lender, and year; $Bank_\ell$ indicates if the lender is a bank; $Renovation_k$ indicates if the purpose is a renovation, where the set of loan purposes are renovation or construction as described in Appendix A.2; $Y_{k,\ell,t}$ is the log number of loans originated or dollar volume for purpose k; and $\alpha_{\ell,t}$ and $\alpha_{k,t}$ are lender-year and purpose-year fixed effects. The parameter of interest in equation (7) is β, which captures the triple difference between treated loan types ($Renovation_k$) originated by treated lenders ($Bank_\ell$) during the treatment period ($Post_t$), and the counterfactual purpose-lender-years.

The results from estimating equation (7) are shown in columns 1-2 of Table 6. The point estimate in column 1 implies that banks increase the ratio of improvement to construction loans by 33 log points relative to nonbanks after HVCRE regulation is introduced. The magnitude is larger when studying dollar volume in column 2, which may reflect economies of scale that incentivize improvements on larger properties.

6.8.2 First-Stage Effect: Difference-in-Difference

The lender-year effects $\alpha_{\ell,t}$ in equation (7) enable me to obtain tightly identified estimates of how HVCRE regulation affects banks' portfolio composition. However, this restrictiveness prohibits inference about whether banks actually finance more improvement projects. Therefore, I next estimate the difference-in-difference equation

$$Y_{\ell,t} = \beta \left(Bank_{\ell} \times Post_{t} \right) + \alpha_t + \alpha_{\ell} + \gamma X_{\ell,t} + u_{\ell,t}, \tag{8}$$

where the notation is similar to that in equation (7), although observational units are now lender-years, as opposed to purpose-lender-years.

Columns 3-4 of Table 6 report the results from estimating equation (8). My outcome of interest, $Y_{\ell,t}$, is the log number of renovated apartments financed by new loans. The point estimate in column 3 implies that banks finance significantly more improvements relative to nonbanks in the post-HVCRE period, and the results are robust to including lender controls in column 4. Appendix Figure A2 tests for differences in average portfolio characteristics between banks and nonbanks, and the resulting similarity suggests that the estimates in Table 6 are unlikely to be biased because of differences in loan specialization.

Collectively, these findings show how banks increase their improvement lending rather than simply reducing their construction lending, which is consistent with the goal of maintaining the same total exposure to commercial real estate projects.

6.8.3 Robustness of the First-Stage Effect

In terms of robustness, I first assess whether the first-stage results are biased from undersampling small regional banks who typically originate a large share of construction loans. Such undersampling is possible because construction loans typically have a construction-to-permanent financing structure − where the lender provides a short-term note that converts to a long-term note once the project has stabilized − and these loans are more difficult to securitize prior to conversion (Black, Krainer and Nichols 2017). Therefore, I interact the treatment variable in equation (8) with the lender's ratio of construction loans to total assets in 2010, normalized to have zero mean and unit variance. Column 1 of Appendix Table A5 shows how the estimated coefficient on this interaction term is positive, suggesting that banks with a focus on construction lending are indeed represented in the T-Loan dataset and that, as expected, their behavior drives the results. In column 2, I drop the Big-4 banks from the sample and obtain almost the same point estimate as when using the full sample in Table 6, further supporting the validity of the first-stage effect.

Next, poorly capitalized banks should theoretically increase their supply of improvement loans by more than well-capitalized banks because HVCRE regulation imposes a more binding constraint on them. I test this theory by reestimating equation (8) after interacting the treatment variable with the ratio of total equity to total assets in 2010, normalized to have zero mean and unit variance. Consistent with theory, I obtain a negative estimated coefficient on the interaction term, as shown in column 3 of Appendix Table A5. This consistency with theory supports the validity of the first-stage effect.

Finally, columns 4-5 of Appendix Table A5 show that the price of credit for bank-originated improvement loans also falls after the introduction of HVCRE regulation, consistent with a movement along the credit demand curve. The modest price response may reflect a substitution toward higher-risk improvement projects or binding borrowing constraints. Using a different methodology, Glancy and Kurtzman (2018) also find that HVCRE regulation modestly affects interest rates.

7 Implications for Housing Affordability

Since quality improvements reduce the supply of relatively inexpensive housing units by transforming them into relatively expensive ones, the baseline results have direct implications for policy discussion about housing affordability. I now make these implications explicit by estimating how the HVCRE shock affects the supply of apartments and rent across the quality distribution. To do so, I use my baseline difference-in-difference setup, the validity of which is strongly supported by the robustness tests in Section 6 and Appendix B.

7.1 Supply of Apartments

The HVCRE shock affects the supply of apartments through two channels. First, by increasing the supply of financing for improvement projects − which, by definition, transform low-quality apartments into high-quality ones − the shock affects the distribution of apartment supply across quality segments (i.e., "the improvement channel"). This channel is my primary focus, given the surge in improvement activity documented in Section 3. However, HVCRE regulation increases the supply of improvement financing by reallocating resources away from construction projects. Therefore, the shock also affects the overall level of apartment supply (i.e., "the construction channel"). I test these hypotheses by reestimating my baseline regression equation (1) after replacing the outcome with a measure of the change in apartment supply.

Table 7 reports the results. In columns 1-3, I verify that the HVCRE shock reduces county-level log apartment construction and, thus, growth in overall apartment supply. This finding confirms the construction channel described in the previous paragraph. In columns 4-6, I replace the outcome with the change in the log number of apartment properties ranked in the top quality segment by professional property inspectors. The results imply that the shock increases growth in the supply of high-quality apartments, even as it reduces growth in overall supply. Both effects appear to be driven by exposure to bank portfolio loans, as suggested by columns 3 and 6.

Taken together, these two sets of results are surprising because, in practice, most new construction occurs in the upper segments of the quality distribution (Rosenthal 2014). Thus, the improvement channel more than offsets the construction channel, leading to a positive effect on the supply of rental housing quality. This positive net effect likely stems from the fact that it costs less to improve a property than to build a new one (R.S. Means 2014).

7.2 Housing Affordability

Turning more directly to the question of housing affordability, I reestimate equation (1) after replacing the outcome variable with various measures of rental housing costs. The results in columns 1-3 of Table 8 show how counties more exposed to the shock see significantly higher rent growth on the average apartment. This finding reflects a mixture of higher average quality (i.e., the improvement channel) as well as lower overall supply (i.e., the construction channel). It is beyond this paper's scope to separate these two channels, but the results in columns 7-9, discussed shortly, will support the empirical relevance of the improvement channel.

In columns 4-6, I replace the outcome with the change in the share of cost-burdened renters, conventionally defined as having a rent-to-income ratio above 30% (JCHS 2017). The results show how counties more exposed to the HVCRE shock see significant growth in the share of cost-burdened renters. Importantly, I use the term "cost-burdened" to match the language typically heard in policy discussions about housing affordability (e.g., Donovan 2014), but the true welfare content of this section's results is in fact quite complicated, as I discuss in my conclusion.

Lastly, the outcome in columns 7-9 is rent growth among high-quality apartments, here measured as the most expensive quintile of apartments in the county-year. The negative estimated coefficients imply that the HVCRE shock reduces rent growth among such apartments, which, along with the results in columns 4-6 of Table 7, is consistent with a movement along the demand curve for housing quality. While the point estimates are only significant at the 10% level when using the composite exposure variable in columns 7-8, there is a highly significant effect when focusing on exposure to bank portfolio loans, shown in column 9. As in previous tables, this finding is consistent with the stronger regulatory incentives associated with such loans.[10]

Summarizing, HVCRE regulation increases the average apartment's rent growth, but

[10]This finding also explains why the estimated coefficient on *Portfolio Bank Share*$_c$ in column 3 has the correct sign and an economically significant magnitude despite a p-value of 0.12: exposure to bank portfolio loans attenuates average rent growth by lowering rent growth on high-quality apartments.

it reduces rent growth on high-quality apartments. These findings are consistent with the research hypothesis outlined in Section 3.1: an increase in the supply of improvement financing raises average housing quality and, thus, average rent, but high-quality rent must fall in order for households to accommodate this increase in the supply of housing quality.

8 Aggregate Implications

I conclude by assessing the aggregate effect of credit supply on quality improvement activity and, subsequently, on rent growth. Consider a counterfactual without HVCRE regulation, in which capital requirements treat loans for all residential investment projects equally. I first ask how many fewer apartments would have been renovated under this counterfactual, expressed as a share of actual renovations and denoted η. In other words, η is the share of observed renovations attributable to HVCRE regulation. I introduce two additional assumptions to calculate this statistic.

Assumption 1 (Control Group) *The effect of the HVCRE shock on quality improvement activity is zero in counties whose value of* $\textit{Total Bank Share}_c$ *is below* $P_B\left(\textit{Total Bank Share}_c\right)$*, where* $P_B\left(\textit{Total Bank Share}_c\right)$ *denotes the* B^{th} *percentile of* $\textit{Total Bank Share}_c$ *across counties. These counties are defined as the control group.*

I introduce Assumption 1 because I do not know how small the exposure variable, $\textit{Total Bank Share}_c$, must be in order for a county to be effectively unexposed to HVCRE regulation (i.e., in the "control group"). Assumption 1 defines this threshold as the B^{th} percentile of $\textit{Total Bank Share}_c$ across counties. Consequently, I can write the effect of HVCRE regulation on county c as

$$\eta_c = 1 - \exp\left[-\beta \times \max\left\{\textit{Total Bank Share}_c - P_B\left(\textit{Total Bank Share}_c\right), 0\right\}\right]. \tag{9}$$

The least conservative approach would be to define the control group as counties with $\textit{Total Bank Share}_c = 0$, corresponding to $B = 0\%$ in my data (i.e., the zeroth percentile).

Therefore, following the literature (e.g., Chodorow-Reich 2014), I report results for B between 5% and 10%.

The second assumption relates to partial equilibrium. HVCRE regulation may affect quality improvement activity through general equilibrium forces, but these effects are subsumed by the year fixed effect in equation (1). It is beyond this paper's scope to quantify general equilibrium effects, and so I introduce the following assumption, which effectively states that general equilibrium effects are sufficiently small that the aggregate effect equals the sum of county-level effects.

Assumption 2 (Partial Equilibrium) *The effect of the HVCRE shock on aggregate quality improvement activity is equal to the weighted sum of county-level effects, η_c. In particular, the aggregate share of renovated apartments due to the HVCRE shock is*

$$\eta = \frac{\sum_c \sum_{t \geq 2015} \eta_c \times \mathit{Renovated\ Apartments}_{c,t}}{\sum_c \sum_{t \geq 2015} \mathit{Renovated\ Apartments}_{c,t}}. \tag{10}$$

Finally, I reweight the raw statistic in equation (10) by a factor of 0.7 to reflect the fact that only 70% of apartment renovations occur on properties with a lien, according to the Rental Housing Finance Survey (RHFS). Reweighting the raw statistic helps address the fact that I can only calculate an in-sample aggregate effect.

Table 9 summarizes the results of this aggregation exercise. Focusing on column 1, HVCRE regulation accounts for between 19% and 24% of quality improvement activity over 2015-16, depending on the definition of the control group. To put the magnitudes in perspective, the property-level analysis from Section 6.7 finds that HVCRE regulation increases a property's annual probability of renovation by 46%. This comparison suggests that the results in column 1 are plausible and do not stem from aggregation assumptions, which are not necessary in the property-level analysis.

In column 2, I summarize an analogous statistic in terms of growth in average rent. The results imply that HVCRE regulation accounts for between 25% and 32% of rent growth over 2015-16. As discussed in Section 7.1, this effect reflects a mixture of higher average quality as well as lower overall supply.

8.1 Robustness of the Aggregate Effect

I assess the plausibility of the large estimates in Table 9 by using the AHS dataset to perform a hedonic quality adjustment. My goal is to assess the extent to which quality improvements have been priced into rent growth. To do so, I construct a hedonic rent index that remeasures rent after correcting for the contribution of both large-scale quality improvements (e.g., adding a floor) and small-scale ones (e.g., adding a dishwasher). The resulting difference between observed and quality-adjusted rent growth reflects the value of quality improvements. I use the AHS dataset for this exercise because it contains a much richer set of property characteristics than the core Trepp datasets. The drawback to the AHS dataset is that I can only perform this exercise over the 2007-13 period because of the AHS's redesign in 2015, mentioned in Section 2. Therefore, this exercise does not directly inform whether the HVCRE-induced improvements contribute to rent growth, but, rather, whether improvements of any origin explain a sufficiently large share of rent growth following the Great Recession to make the estimated effects of HVCRE regulation in Table 9 plausible.

The logic of a hedonic adjustment 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 housing unit with the same set of structural features. Since my interest is in quality improvements to a given housing unit, I estimate the following pricing kernel in differences

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

where i and t index housing units and years; $\Delta \log \left(Rent_{i,t}\right)$ is the change in log rent; and $\Delta\Theta_{i,t}$ is a vector of indicators for the installment of features $\theta_{i,t} \in \Theta_{i,t}$, summarized in Appendix Table A6. Note that, like most hedonic pricing kernels, equation (11) does not take a stance on the shocks generating variation in improvements, $\Delta\Theta_{i,t}$.

Given the estimates of equation (11), shown in Appendix Table A7, I compute a unit's quality-adjusted rent as

$$Rent^H_{i,t} = Rent_{i,t_0} \times \exp\left[\sum_{\tau=t_0+2}^{t} \left(\Delta \log\left(Rent_{i,\tau}\right) - \beta^{\Theta}\Delta\Theta_{i,\tau}\right)\right], \tag{12}$$

where t_0 is the initial year; and the summation begins at t_0+2 because the AHS is biennial. Finally, I aggregate $Rent^H_{i,t}$ across housing units as described in Appendix C to calculate the hedonic index.

Figure 5 summarizes annual growth in unadjusted rent and the hedonic index over the 2007-13 period. Beginning on the left, unadjusted rent grows 1.7% in real terms (i.e., excess of non-housing inflation). Moving to the right, quality-adjusted real rent growth is 0.5%, based on the hedonic index. By extension, quality improvements account for 70% (i.e., $0.70 = \frac{1.7-0.5}{1.7}$) of observed real rent growth. This finding is robust to alternative specifications of equation (11) described in Appendix C, and it appears to be unique to the period after the Great Recession, as shown in Appendix Figure A5.

The results of this hedonic adjustment imply that quality improvements − regardless of the shock generating them − explain a significant share of observed real rent growth following the Great Recession. This finding implies makes it plausible that the HVCRE shock and the improvements generated by it explain a large share of observed rent growth over my period of analysis, per the estimates in Table 9.

9 Conclusion

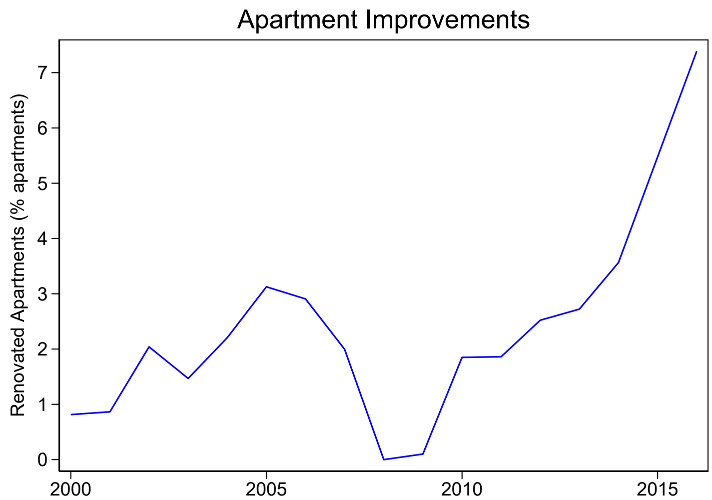
This paper shows how financial intermediaries have contributed to a newly documented surge in improvements to rental housing quality since the Great Recession, and, in so doing, have meaningfully affected housing affordability. Using the introduction of Dodd-Frank bank capital requirements as an exogenous source of variation, I find that a reallocation of bank credit toward improvements accounts for 24% of improvement activity over 2015-16. By increasing the supply of rental housing quality, this shock raises average rent growth but reduces rent growth on high-quality housing. These findings support the conclusion that financial intermediaries supply housing quality by providing financing for improvement projects, and, through their role as suppliers of quality, so affect housing affordability.

From the policy perspective, these findings exemplify an unintended, economically significant consequence of regulations that affect financial intermediaries' portfolio choice. In particular, the results show how housing quality and affordability are affected not only by traditional urban policies (e.g., rent stabilization), but also by regulation of the financial sector. Moreover, from the standpoint of urban policymakers seeking to expand the supply of affordable housing, the results imply that limiting market-rate construction actually leads to upward filtering that makes affordable housing more scarce.

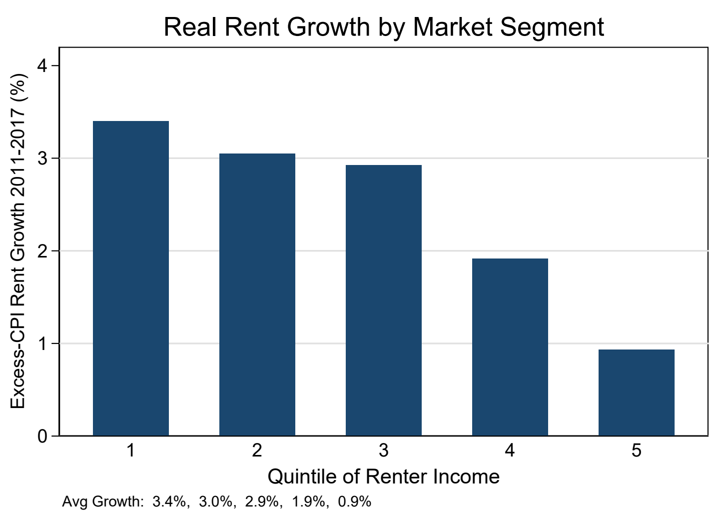
However, the welfare content of this regulatory spillover is unclear. On the one hand, the shock appears to relax borrowing constraints on improvement financing, implying a Pareto improvement. On the other hand, by channeling resources toward improvements and away from construction, the shock may reduce allocative efficiency and be Pareto-impairing. In distributional terms, the increase in the supply of housing quality likely favors higher-income renters, although I leave a formal welfare analysis for future work.

Figures and Tables

Figure 1: Facts about Rental Housing Quality and Affordability



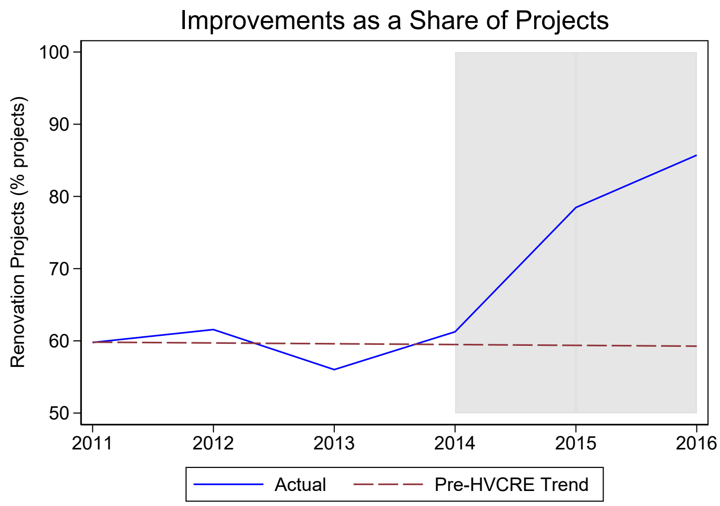
(a) Apartment Improvements



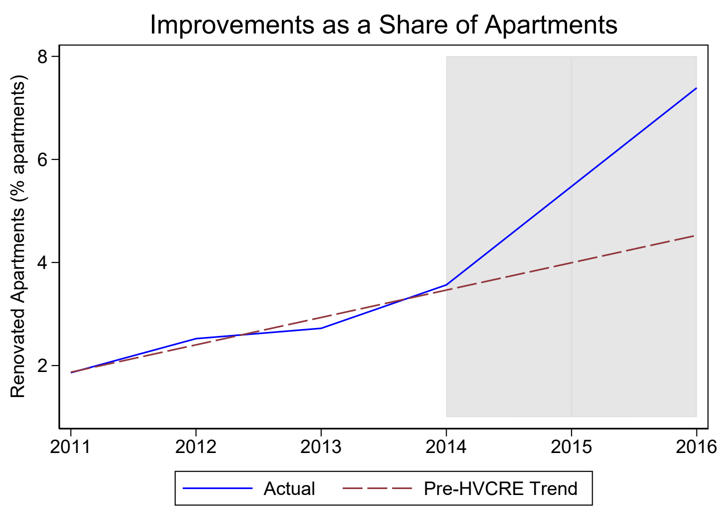
(b) Real Rent Growth by Market Segment

Note: This figure documents two new stylized facts about rental housing quality and affordability. Panel (a) plots the percent of apartments renovated each year, based on Trepp's T-Loan dataset. Panel (b) plots real (i.e., excess-CPI) growth in average apartment rent by the renter's income quintile, based on the Census' AHS dataset. Renters are sorted into quintiles by their log real income relative to the MSA-year mean and weighted using AHS survey weights.

Figure 2: Aggregate Improvement Activity around the HVCRE Regulatory Shock



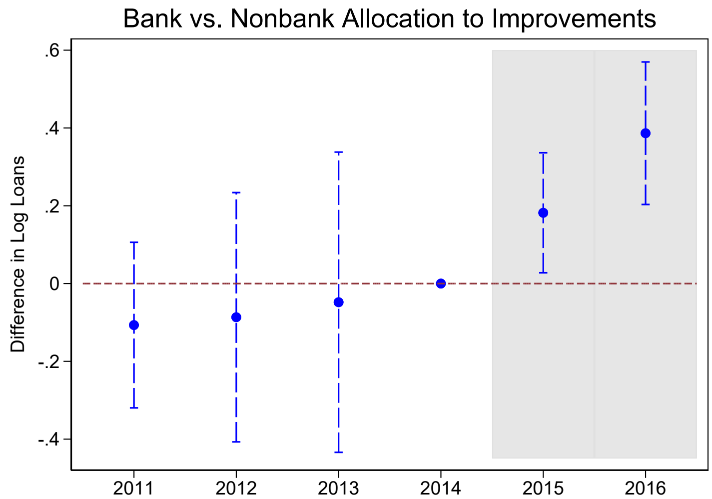
(a) Improvements as a Share of Projects



(b) Improvements as a Share of Apartments

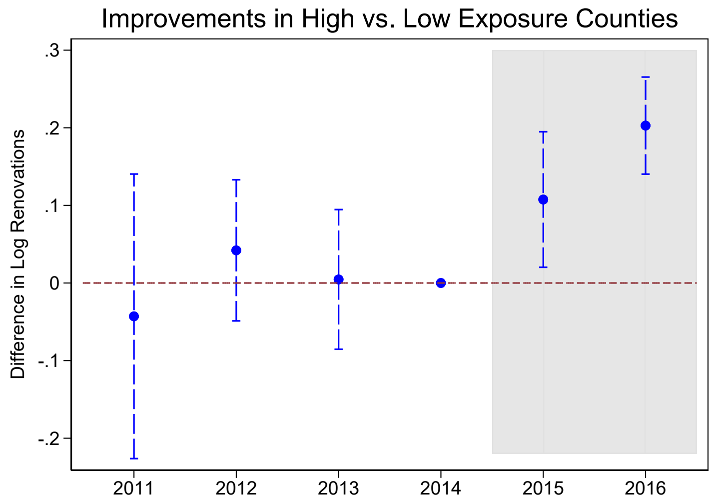
Note: This figure plots aggregate quality improvement activity around the HVCRE regulatory shock. Panel (a) plots the number of renovation projects divided by the sum of renovation projects and construction projects, all within the apartment sector. Panel (b) plots the percent of apartments renovated each year. The gray region denotes the period when HVCRE regulation is in place. Data are from Trepp's T-Loan dataset.

Figure 3: Improvement Lending around the HVCRE Regulatory Shock



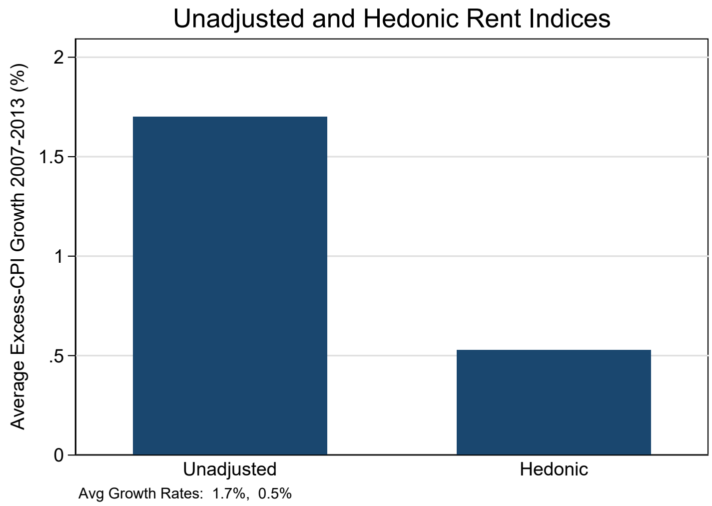
Note: This figure plots the year-by-year average difference in log loan originations between renovation and construction projects between bank and nonbank lenders, which allows for an inspection of pre-trends in relative improvement lending. The reference year is 2014. The difference is demeaned by a lender-year fixed effect, a loan type-year fixed effect, and a loan type-bank fixed effect, as in Table 6. The gray region highlights the period when HVCRE regulations are in place. Brackets are a 95% confidence interval with standard errors clustered by lender and year. Data are from Trepp's T-Loan dataset.

Figure 4: County-Level Quality Improvements around the HVCRE Regulatory Shock



Note: This figure plots the year-by-year average difference in log renovated properties between counties with high and low values of the exposure variable, which allows for an inspection of pre-trends in county-level quality improvement activity. The reference year is 2014. The exposure variable is banks' share of apartment loan balances in 2010, corresponding to the variable *Total Bank Share*$_c$. High and low are defined according to the average across counties. The difference is demeaned by a state-year fixed effect, as in Table 2. The gray region highlights the period when HVCRE regulations are in place. Brackets are a 95% confidence interval with standard errors clustered by county and year. Data for the variable on the vertical axis are from Trepp's T-Loan dataset. Data for the exposure variable are from Trepp's T-ALLR and T-Loan datasets.

Figure 5: Magnitude of the Quality Improvement Effect through a Hedonic Adjustment



Note: This figure plots the results of the hedonic quality adjustment from Section 8.1, which assesses the plausibility of the implied aggregate effect of HVCRE regulation from Section 8. The vertical axis shows average annual growth in real (i.e., excess-CPI) rent over 2007-13 for various rent indices. Unadjusted denotes average observed rent. Hedonic denotes the hedonic index defined in Section 8.1. Data are from the Census' AHS dataset.

Table 1: Summary Statistics

	Observations	Mean	Standard Deviation	Source of Data
Core Dataset:				
Portfolio Bank Share$_c$	3,128	0.192	0.168	T-ALLR, T-Loan
Securitized Bank Share$_c$	3,128	0.539	0.184	T-ALLR, T-Loan
Total Bank Share$_c$	3,128	0.731	0.163	T-ALLR, T-Loan
$\log(\textit{Bank Loans}_{c,t})$	639	1.033	0.595	T-ALLR
$\log(\textit{Renovated Properties}_{c,t})$	3,128	0.154	0.377	T-Loan
$\log(\textit{Renovated Apartments}_{c,t})$	3,128	0.934	2.164	T-Loan
$\log(\textit{Renovation Value}_{c,t})$	3,128	1.744	4.312	T-Loan
$\log(\textit{Newly-Built Properties}_{c,t})$	3,128	0.389	0.874	T-ALLR, T-Loan, BPS
$\Delta\log(\textit{Top-Quality Properties}_{c,t})$	2,540	0.205	0.654	T-Loan
$\Delta\log(\textit{Average Rent}_{c,t})$	2,494	0.027	0.062	T-Loan
$\Delta\,\textit{Cost-Burdened Share}_{c,t}$	2,577	0.019	0.116	T-Loan, IRS
$\Delta\log(\textit{Top-Quintile Rent}_{c,t})$	2,057	0.014	0.049	T-Loan
GSE Share$_{c,t}$	3,125	0.543	0.313	T-Loan
Transactions Dataset:				
Total Bank Share$_c$	2,116	0.298	0.229	RCA
$\log(\textit{Renovated Properties}_{c,t})$	2,729	0.566	0.84	RCA
Baseline Number of Counties: 554				

Note: This table presents summary statistics of key county-level variables. Subscripts c and t denote county and year. The rightmost column lists the datasets used to construct each variable: the T-ALLR and T-Loan datasets both come from Trepp, and they respectively derive from loans held on banks' portfolios and loans securitized by bank and nonbank originators; RCA denotes the Real Capital Analytics dataset, which derives from apartment transactions; BPS denotes the Census' Building Permits Survey dataset; and IRS denotes the Internal Revenue Service's SOI Tax Stats dataset. The variables in the upper panel are defined as follows: *Portfolio Bank Share*$_c$ is the ratio of bank portfolio loan balances in 2010, based on T-ALLR, to the sum of bank portfolio loan balances in 2010, based on T-ALLR, and bank and nonbank securitized loan balances in 2010, based on T-Loan; *Securitized Bank Share*$_c$ is the ratio of bank securitized loan balances in 2010, based on T-Loan, to the same denominator as in *Portfolio Bank Share*$_c$; *Total Bank Share*$_c$ is the sum of *Portfolio Bank Share*$_c$ and *Securitized Bank Share*$_c$; *Bank Loans*$_{c,t}$ is the number of bank portfolio loans originated for purposes other than construction, based on T-ALLR; *Renovated Properties*$_{c,t}$, *Renovated Apartments*$_{c,t}$, and *Renovation Value*$_{c,t}$ are the number of renovated apartment properties, number of renovated apartment units, and total revenue of renovated apartment units, respectively, based on T-Loan; *Newly-Built Properties*$_{c,t}$ is the sum of apartment construction projects observed in the T-ALLR and T-Loan datasets or, when no construction is observed in the Trepp data, the number of apartment construction permits issued according to the BPS dataset; *Top-Quality Properties*$_{c,t}$ is the number of properties times share of apartments ranked in the top quality segment by professional property inspectors, based on T-Loan; *Average Rent*$_{c,t}$ is the average rent among all apartments, based on T-Loan; *Cost-Burdened Renters*$_{c,t}$ is the share of apartments whose rent, according to the T-Loan dataset, exceeds 30% of the zip code's average income, according to the IRS dataset; *Top-Quintile Rent*$_{c,t}$ is the average rent among apartments whose rent is above the 80$^{\text{th}}$ percentile within the same county-year, based on T-Loan; and *GSE Share*$_{c,t}$ is the share of securitized loan balances backed by the Government Sponsored Enterprises (GSEs) based on T-Loan. The variables in the lower panel are defined similarly but are calculated using the RCA dataset. Observations are county-years weighted by the number of apartments in the county. The sample period is 2011-16. Appendix A provides additional details.

Table 2: The Supply of Financing and Quality Improvement Activity

Outcome:	log $\left(\textit{Improvement Measure}_{c,t}\right)$						
Measure:	*Bank Loans*	*Renovated Properties*	*Renovated Properties*	*Renovated Properties*	*Renovated Properties*	*Renovated Apartments*	*Renovation Value*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\textit{Portfolio Bank Share}_c \times \textit{Post}_t$	0.887	0.516	0.518	0.401			
	(0.014)	(0.083)	(0.040)	(0.030)			
$\textit{Securitized Bank Share}_c \times \textit{Post}_t$		0.196	0.245	0.278			
		(0.105)	(0.041)	(0.008)			
$\textit{Total Bank Share}_c \times \textit{Post}_t$					0.318	1.611	3.163
					(0.002)	(0.008)	(0.005)
Sample	Portfolio Loan	Full	Full	Full	Full	Full	Full
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County Controls	No	No	Yes	Yes	Yes	Yes	Yes
State-Year FE	No	No	No	Yes	Yes	Yes	Yes
R-squared	0.514	0.571	0.612	0.720	0.720	0.693	0.694
Number of Observations	406	3,128	3,128	3,128	3,128	3,128	3,128

Note: P-values are in parentheses. This table estimates equation (1), which is the paper's baseline equation. Subscripts c and t denote county and year. The regression equation is of the form

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

where observations are county-years; $\textit{Bank Share}_c$ is a measure of banks' share of apartment loan balances in 2010; and $\textit{Post}_t$ indicates whether t is greater than or equal to 2015. The three measures of $\textit{Bank Share}_c$ are: bank portfolio loan balances divided by the sum of bank portfolio loan, bank securitized loan, and nonbank securitized loan balances, denoted $\textit{Portfolio Bank Share}_c$; bank securitized loan balances divided by the same denominator, denoted $\textit{Securitized Bank Share}_c$; and the sum of bank portfolio and securitized loan balances divided by the same denominator, denoted $\textit{Total Bank Share}_c$. The outcome is the log of a measure of improvement activity: column 1 uses the number of bank portfolio loans originated for purposes other than construction; columns 2-5 use the number of renovated apartment properties; column 6 uses the number of renovated apartment units; and column 7 uses total revenue of renovated apartment units. Column 1 restricts the sample and outcome variable to those observed in Trepp's T-ALLR dataset, which derives exclusively from bank portfolio loans, and the remaining columns are based on the combination of Trepp's T-ALLR and T-Loan datasets. County controls are log real income per capita for the surrounding MSA, log winter storms, and the principal-weighted averages of the following characteristics of existing securitized apartment loans: loan-to-value ratio, debt service coverage ratio, adjustable rate mortgage share, and share of 60+ day delinquent loans. There are 113 and 554 counties in column 1 and columns 2-7, respectively. Observations are weighted by the number of apartments in the county. The sample period is 2011-16. Standard errors are clustered by county. Data for the outcome and exposure variables are from Trepp's T-ALLR and T-Loan datasets. Table 1 lists each variable's data source.

Table 3: Robustness to Data Provider

Outcome:	$\log\left(Renovated\ Properties_{c,t}\right)$						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$Portfolio\ Bank\ Share_c \times Post_t$	1.432	1.302	1.291	1.034			
	(0.001)	(0.009)	(0.000)	(0.000)			
$Securitized\ Bank\ Share_c \times Post_t$		-0.206	-0.109	-0.054			
		(0.369)	(0.588)	(0.759)			
$Total\ Bank\ Share_c \times Post_t$					0.317	0.407	0.204
					(0.097)	(0.001)	(0.008)
Source of Outcome Variable	RCA	RCA	RCA	RCA	RCA	RCA	Trepp
Source of Exposure Variable	Trepp	Trepp	Trepp	Trepp	Trepp	RCA	RCA
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County Controls	No	No	Yes	Yes	Yes	Yes	Yes
State-Year FE	No	No	No	Yes	Yes	Yes	Yes
R-squared	0.703	0.704	0.751	0.822	0.816	0.819	0.728
Number of Observations	2,721	2,721	2,721	2,716	2,716	2,117	2,117

Note: P-values are in parentheses. This table estimates equation (1) using data from two separate providers to measure the outcome and exposure variables. Subscripts c and t denote county and year. The specifications are similar to those in Table 2. The outcome variable is defined as in Table 2 and measured using the Real Capital Analytics (RCA) dataset in columns 1-6 and using Trepp's T-Loan dataset in column 7. The exposure variables are defined as in Table 2 and measured using Trepp's T-ALLR and T-Loan datasets in columns 1-5 and using the RCA dataset in columns 6-7. The remaining notes are the same as in Table 2.

Table 4: Robustness to GSE Exposure

Outcome:	$\log\left(Renovated\ Properties_{c,t}\right)$		
	(1)	(2)	(3)
$Total\ Bank\ Share_c \times Post_t$	0.318	0.351	0.318
	(0.071)	(0.030)	(0.002)
$GSE\ Share_{c,t}$	0.052	-0.002	-0.004
	(0.260)	(0.964)	(0.944)
Year FE	Yes	Yes	Yes
County FE	Yes	Yes	Yes
County Controls	No	Yes	Yes
State-Year FE	No	No	Yes
R-squared	0.567	0.609	0.720
Number of Observations	3,125	3,125	3,125

Note: P-values are in parentheses. This table estimates a variant of equation (1) that assesses robustness to changes in lending practices by the Government Sponsored Enterprises (GSEs). Subscripts c and t denote county and year. The specifications are similar to those in Table 2 after controlling for the share of securitized loan balances backed by the GSEs, denoted $GSE\ Share_{c,t}$, based on Trepp's T-Loan dataset. Data for the outcome variable are from Trepp's T-Loan dataset. Data for the exposure variable are from Trepp's T-ALLR and T-Loan datasets. The remaining notes are the same as in Table 2.

Table 5: Robustness to Property-Level Analysis

Outcome:	*Probability of Renovation*$_{i,\ell,t}$	
	(1)	(2)
$Bank_\ell \times Post_t$	0.012	0.012
	(0.009)	(0.010)
Property-Lender FE	Yes	Yes
County-Year FE	Yes	Yes
Zip Code Controls	No	Yes
R-squared	0.308	0.308
Number of Observations	30,733	30,733

Note: P-values are in parentheses. This table estimates equation (6), which is a property-level analogue to the baseline equation. Subscripts i, c, z, ℓ, and t denote property, county, zip code, lender, and year. The regression equation is of the form

$$\textit{Probability of Renovation}_{i,\ell,t} = \beta\,(Bank_\ell \times Post_t) + \alpha_{c(i),t} + \alpha_{i,\ell} + \gamma X_{z(i),t} + u_{i,\ell,t},$$

where observations are property-years; $Bank_\ell$ indicates whether the existing loan on the property is originated by a bank; and *Probability of Renovation*$_{i,\ell,t}$ indicates whether a renovation occurs. Zip code controls are log average income, log number of tax returns, and log average rent. There are 6,100 properties. The sample period is 2011-16. Standard errors are clustered by property. Data are from Trepp's T-Loan dataset.

Table 6: Robustness of First-Stage Effect on Lending Behavior

Specification:	Triple Difference-in-Difference		Difference-in-Difference	
Outcome:	$\log(Loans_{k,\ell,t})$	$\log(Volume_{k,\ell,t})$	$\log(Renovations_{\ell,t})$	
	(1)	(2)	(3)	(4)
$Bank_\ell \times Post_t \times Renovation_k$	0.332	5.385		
	(0.000)	(0.017)		
$Bank_\ell \times Post_t$			1.210	1.141
			(0.024)	(0.030)
Lender-Year FE	Yes	Yes		
Purpose-Year FE	Yes	Yes		
Bank × Renovation	Yes	Yes		
Lender FE			Yes	Yes
Year FE			Yes	Yes
Lender Controls			No	Yes
R-squared	0.762	0.799	0.660	0.667
Number of Observations	966	966	582	582

Note: P-values are in parentheses. This table estimates equations (7) and (8), which assess the lender-level effect of HVCRE regulation as a first stage. Subscripts k, ℓ and t denote loan purpose, lender, and year. Columns 1-2 estimate equation (7),

$$Y_{k,\ell,t} = \beta\left(Bank_\ell \times Post_t \times Renovation_k\right) + \gamma\left(Bank_\ell \times Renovation_k\right) + \alpha_{\ell,t} + \alpha_{k,t} + u_{k,\ell,t},$$

where observations are purpose-lender-years; $Bank_\ell$ denotes if lender ℓ is a bank; $Renovation_k$ indicates whether the purpose is a renovation; and the set of loan purposes are renovation or construction. $Loans_{k,\ell,t}$ is the number of loans for purpose k originated by ℓ in t, and $Volume_{k,\ell,t}$ is the corresponding dollar volume. Columns 3-4 estimate equation (8),

$$Y_{\ell,t} = \beta\left(Bank_\ell \times Post_t\right) + \alpha_t + \alpha_\ell + \gamma X_{\ell,t} + u_{\ell,t},$$

where observations are lender-years. $Renovations_{\ell,t}$ is the number of renovated apartments 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. There are 122 lenders. Observations are weighted by the lender's market share. The sample period is 2011-16. Standard errors are clustered by lender and year in columns 1-2 and by lender in columns 3-4. Data are from Trepp's T-Loan dataset.

Table 7: Implications of Quality Improvement Shock for the Supply of Apartments

Outcome:	$\log\left(\textit{Newly-Built Properties}_{c,t}\right)$			$\Delta\log\left(\textit{Top-Quality Properties}_{c,t}\right)$		
	(1)	(2)	(3)	(4)	(5)	(6)
$\textit{Total Bank Share}_c \times \textit{Post}_t$	-0.427	-0.404		0.578	0.612	
	(0.017)	(0.023)		(0.005)	(0.003)	
$\textit{Portfolio Bank Share}_c \times \textit{Post}_t$			-0.532			0.811
			(0.098)			(0.004)
$\textit{Securitized Bank Share}_c \times \textit{Post}_t$			-0.323			0.486
			(0.107)			(0.018)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
County Controls	No	Yes	Yes	No	Yes	Yes
R-squared	0.556	0.559	0.559	0.117	0.125	0.126
Number of Observations	3,128	3,128	3,128	2,530	2,530	2,530

Note: P-values are in parentheses. This table estimates a variant of equation (1) that assesses how the HVCRE shock affects the supply of apartments. Subscripts c and t denote county and year. The specifications are similar to those in Table 2 with a different outcome variable that measures growth in the supply of apartment properties: $\log\left(\textit{Newly-Built Properties}_{c,t}\right)$ is the log of number of apartment construction projects; and $\Delta\log\left(\textit{Top-Quality Properties}_{c,t}\right)$ is the change in the log of number of properties times share of apartments ranked in the top quality segment by professional property inspectors. Data for the outcome variables are from Trepp's T-ALLR and T-Loan datasets and other datasets shown in Table 1. Data for the exposure variables are from Trepp's T-ALLR and T-Loan datasets. The remaining notes are the same as in Table 2.

Table 8: Implications of Quality Improvement Shock for Housing Affordability

Outcome:	$\Delta \log\left(\textit{Average Rent}_{c,t}\right)$			$\Delta\, \textit{Cost-Burdened Share}_{c,t}$			$\Delta \log\left(\textit{Top-Quintile Rent}_{c,t}\right)$		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$\textit{Total Bank Share}_c \times \textit{Post}_t$	0.043	0.044		0.072	0.072		-0.048	-0.051	
	(0.024)	(0.013)		(0.003)	(0.003)		(0.096)	(0.076)	
$\textit{Portfolio Bank Share}_c \times \textit{Post}_t$			0.036			0.078			-0.070
			(0.120)			(0.011)			(0.013)
$\textit{Securitized Bank Share}_c \times \textit{Post}_t$			0.050			0.068			-0.040
			(0.006)			(0.007)			(0.185)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
R-squared	0.241	0.262	0.262	0.366	0.374	0.374	0.280	0.290	0.292
Number of Observations	2,480	2,480	2,480	2,565	2,565	2,565	2,003	2,003	2,003

Note: P-values are in parentheses. This table estimates a variant of equation (1) that assesses how the HVCRE shock affects various outcomes related to housing affordability. Subscripts c and t denote county and year. The specifications are similar to those in Table 2 with a different outcome variable: $\Delta \log\left(\textit{Average Rent}_{c,t}\right)$ is the change in the log average rent among all apartments; $\Delta\, \textit{Cost-Burdened Share}_{c,t}$ is the change in the share of apartments with rent greater than 30% of the zip code's average income; and $\Delta \log\left(\textit{Top-Quintile Rent}_{c,t}\right)$ is the change in the log average rent among apartments in the top 20% by level of rent relative to the county-year. A rent-to-income ratio above 30% is the standard threshold for being considered cost-burdened (JCHS 2017). Columns 7-9 also include a vector of state-year fixed effects. Data for the outcome variables are from Trepp's T-Loan dataset and other datasets shown in Table 1. Data for the exposure variables are from Trepp's T-ALLR and T-Loan datasets. The remaining notes are the same as in Table 2.

Table 9: Share of Quality Improvements and Rent Growth due to the Shock

Outcome:	Renovated Apartments	Rent Growth
Share of Outcome due to Shock, $B = 5\%$	24%	32%
Share of Outcome due to Shock, $B = 10\%$	19%	25%
Source of β:	Table 2, Column 6	Table 8, Column 2

Note: This table shows the implied contribution of HVCRE regulation to the aggregate value of the indicated outcome variable over 2015-16, denoted η in the text and expressed as a share of the actual value of the outcome variable over 2015-16. The implied effect is based on Assumption 1 (Control Group) and Assumption 2 (Partial Equilibrium). The parameter B indexes the set of counties defined to be in the control group and, thus, not affected by HVCRE regulation. The first and second rows respectively define the control group as counties where banks' initial share of apartment loan balances (i.e., $\textit{Total Bank Share}_c$) is below the fifth or tenth percentile across counties, respectively. Explicitly, letting $P_B\left(\textit{Total Bank Share}_c\right)$ denote the B^{th} percentile of $\textit{Total Bank Share}_c$ across counties, the expression for the statistic shown in column 1 is

$$\eta_1 = \frac{\sum_c \sum_{t \geq 2015} \left(1 - \exp\left[-\beta \times \max\left\{\textit{Total Bank Share}_c - P_B\left(\textit{Total Bank Share}_c\right), 0\right\}\right]\right) \times \textit{Renovated Apartments}_{c,t}}{\sum_c \sum_{t \geq 2015} \textit{Renovated Apartments}_{c,t}},$$

and the expression for the statistic in column 2 is

$$\eta_2 = \frac{\sum_c \sum_{t \geq 2015} w_c \times \beta \times \max\left\{\textit{Total Bank Share}_c - P_B\left(\textit{Total Bank Share}_c\right), 0\right\}}{\sum_c \sum_{t \geq 2015} w_c \times \Delta \log\left(\textit{Average Rent}_{c,t}\right)},$$

where c and t index counties and years; and w_c is the number of apartments in county c. I multiply the previous two expressions by a factor of 0.70 to reflect the fact that only 70% of improvements occur on apartments with a lien, according to the 2015 RHFS. Data for the outcome variables are from Trepp's T-Loan dataset.

Online Appendix

This document contains additional material referenced in the text. Appendix A describes the paper's datasets and variables in detail. Appendix B contains additional robustness exercises referenced in Section 6. Appendix C provides methodological details on the hedonic adjustment used to evaluate the robustness of the results in Section 8.

A Data

This appendix describes the raw collection and subsequent cleaning of the datasets referenced in Section 2 of the text. These datasets are: Trepp's T-ALLR (A.1) and T-Loan datasets (A.2); the Real Capital Analytics transactions dataset (A.3); and the Census' AHS dataset (A.4). Section A.5 describes additional datasets used in the paper. Section A.6 describes how the county-level variables used in the paper's main tables are constructed.

A.1 T-ALLR Dataset

The paper's first core dataset is Trepp's Anonymized Loan Level Repository (T-ALLR) dataset. The T-ALLR dataset contains information on bank portfolio loans secured by apartment properties. Trepp collects the raw data from clients of its Bank Solutions consulting service. The raw data cover 10% of U.S. counties, or 52% of counties on a population-weighted basis.

A.1.1 Timespan

The T-ALLR dataset is structured as a quarterly loan-level panel over 2013-19. I observe the loan's year of origination, which allows me to compute the aggregate time series shown in Figure A1b. To calculate the value of bank portfolio loan balances in a county in 2010 (i.e., $\textit{Bank Portfolio Balances}_c$), I first retain all loans originated in 2010 or earlier. Then, I amortize each loan's balance from the first year it is observed in the sample back to 2010 according to the reported amortization schedule. For amortizing loans, I apply a reverse straight-line amortization based on the loan's term. After amortizing each loan's balance back to 2010, I calculate the sum of balances in the county.

A.1.2 Lender-Level Information

I do not observe the bank's identity, but, according to Trepp, the T-ALLR dataset includes a majority of banks subject to CCAR stress tests and a quarter of those subject to DFAST tests. Moreover, again according to Trepp, no bank accounts for more than 16% of total loan balances. Thus, although the T-ALLR dataset only covers Trepp's clients, it is not skewed toward any one particular bank.

A.1.3 Property-Level Information

There is a unique identifier for each loan, but I do not observe identifying information about the encumbered property. This latter feature is intended to protect the lender's privacy. To further protect the lender's privacy, all dollar variables are scaled by a random factor between 95% and 105%. I observe whether the loan's purpose is construction, the terms of the loan (e.g., interest rate), and the property's occupancy. I do not observe whether the loan finances an improvement, the history of renovations on the property, the property's rent, the number of units in the property, or any other physical characteristics about the property apart from the zip code and county in which it is located. Reporting geographic information is optional, and for 15% of loans the bank chooses not to do so.

As stated in the note to Figure A1, I use two proxy measures for whether a loan in the T-ALLR dataset finances an improvement. First, I use an indicator for whether the loan's purpose is not construction. Second, I impute whether the loan finances a renovation using observable loan characteristics applied to Trepp's T-Loan dataset. Specifically, using the T-Loan dataset, I regress an indicator for whether the loan finances a renovation on the following characteristics of the loan: interest rate, loan-to-value ratio, occupancy, and a fixed effect for the property's county. Then, I use the estimated coefficients to predict the probability a loan in the T-ALLR dataset finances a renovation. I classify a loan as financing a renovation if this probability exceeds the empirical probability in the T-Loan dataset (8.6%). This procedure correctly classifies 85% of renovation loans in the T-Loan dataset.

A.2 T-Loan Dataset

The paper's second core dataset is a random sample of Trepp's merged Property, Loan, and Loan2 datasets, which I simply refer to as "T-Loan" in the text. The T-Loan dataset contains information on loans secured by apartment properties that have been securitized as commercial mortgage backed securities (CMBS). The raw data come from CMBS servicing records for loans that were securitized by the fourth quarter of 2017. Trepp utilizes all resources available to construct these data, including Annex A's, Servicer Set-Up files, CREFC Loan and Property Periodic Files, and various third party resources. The raw data cover 47% of U.S. counties, or 90% of counties on a population-weighted basis.

A.2.1 Timespan

The T-Loan dataset is structured as a monthly loan-level panel over 2010-16. Unlike the T-ALLR data, the T-Loan dataset has an identifier for both the loan and the encumbered property, and so I collapse the T-Loan dataset to an annual property-level panel. Since the T-Loan dataset begins in 2010, I can directly calculate the value of securitized loan balances in a county (i.e., $\textit{All Securitized Balances}_c$, $\textit{Bank Securitized Balances}_c$).

As mentioned below, I observe the history of renovations on a property dating back to 2000. This allows me to backfill the time series in Figure 1a as follows. For the numerator (i.e., number of renovated units), I compute the sum of in-sample units that were renovated in t, conditional on the property's loan being securitized by t so that the property would have been included in a pre-2010 version of the sample. For the denominator, I regress the log number of apartments in the sample over 2010-16 on the log aggregate stock of U.S. rental units from the Census' Housing and Vacancy Survey, which is available beginning in 2000. Then, I backfill the number of units that would have been in a pre-2010 version of the sample. Taking the ratio of numerator and denominator gives the pre-2010 time series in Figure 1a. Renovations undertaken in the latter part of the 2010-16 period may not appear in the sample because of securitization lags. Therefore, Figure 1a weights observations by the inverse probability of appearing in the sample (Solon, Haider and Wooldridge 2015), here defined as the empirical probability of being securitized by the fourth quarter of 2017.

A.2.2 Lender-Level Information

I observe the name of the loan's originator for 92% of the T-Loan sample, where, as mentioned in the text, I use the terms "originator" and "lender" synonymously for simplicity. I observe the name of the borrower for 14% of the sample. I address cases where the name's spelling changes using Stata's string grouping algorithm *strgroup*, 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.

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, that 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". In Section 6.8, I normalize originations to have unit variance within lender-purposes to account for different business models.

A.2.3 Property-Level Information

The T-Loan dataset has substantially more property-level information than the T-ALLR dataset. In particular, I observe: the latitude and longitude of the encumbered property; the number of units, occupancy, and revenue of the property, which I use to calculate rent as described in Section A.6 below; the terms on the property's loan (e.g., interest rate); the property's physical condition based on professional property inspection ratings; the year the property was built; whether the loan's purpose is construction; and the history of renovations dating back to 2000. 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 classify a loan as financing a renovation if it is originated within 1 year of a renovation.

I classify a loan as financing construction if its stated purpose is construction or if it was originated within 3 years of the property being built. 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 note that converts to a long-term note once the project has stabilized. Such loans are more difficult to securitize prior to conversion (Black, Krainer and Nichols 2017).

A.3 RCA Dataset

In Section 6.1, I evaluate robustness to data provider by replicating the baseline analysis using a dataset on apartment transactions from Real Capital Analytics (RCA). The raw data come from transaction records for apartment properties and cover 40% of U.S. counties, or 88% of counties on a population-weighted basis.

A.3.1 Timespan

The RCA dataset spans 2009-16 and includes one observation per transaction. I calculate the value of loan balances in a county in 2010 using the value of loans originated in 2010.

A.3.2 Lender-Level Information

I observe the name of the lender who originates the loan associated with the transaction. Importantly, the RCA dataset includes both portfolio and securitized loans. I classify the lender as a bank if its name contains the terms “Bank”, “Bk”, “Bnk”, “B&T”, “Banc”, “BANK”, “bank”, “FSB”, “Savings”, “Credit Union”, or “CU”, since savings banks and credit unions are present in the RCA dataset and are also subject to HVCRE regulation. I manually review the resulting classification for lenders with over 0.1% of origination volume over 2009-16 and adjust the classification based on whether the lender has a record in the FDIC’s Institution Directory, accessed through the FFIEC’s Institution Lookup Tool.

A.3.3 Property-Level Information

I observe the county of the transacting property and the year when the property was renovated. I do not observe the property’s rent, occupancy, physical characteristics, or information about the loan associated with the transaction, apart from the loan’s size in dollars.

A.4 AHS Dataset

I use the Census’ American Housing Survey (AHS) dataset to produce Figure 1b and to perform the hedonic quality adjustment in Section 8.1. The raw data come from a survey administered by the U.S. Census Bureau in odd numbered years. I observe very little geographic information, but, in principle, the AHS dataset is representative of the universe of U.S. counties.

A.4.1 Timespan

The AHS was introduced in 1973 but has undergone several sample redesigns since then. In particular, the dataset is structured as a biennial panel of housing units, but the panel broke in 1995 and again in 2015 due to sample redesigns. Consequently, I can only track the same housing unit over time from 1997-2013.

A.4.2 Information about Housing Units

I observe the housing unit's rent, demographic information about the occupant, and relatively granular information about the housing unit's physical features (e.g., presence of a dishwasher), based on the AHS's Equipment and Appliances module. I clean the raw data as follows: I winsorize rent by 5% on both sides prior to aggregating rent in equation (C3); I restrict attention to housing units whose tenure did not change over the sample period, thus filtering out "condo conversions"; I define improvements as the installation of new features, as described in Section 8.1 of the text. Since the AHS panel broke in 1995 and 2015, I only observe such improvements over the 1997-2013 period. Table A6 provides summary statistics of improvements in the AHS dataset.

The public-use AHS dataset does not contain information about the housing unit's county. The only geographic observation I observe is the housing unit's MSA for a subset of 166 MSAs out of 384, or 43%. I use this limited geographic information to sort renters into quintiles by their log real income relative to the MSA-year mean, as described in the note to Figure 1b. Prior to sorting, I restrict renters to those living in apartments (i.e., multifamily properties) and bottom-code income at $12,000 in 2017 dollars. Income is calculated using the AHS's family income variable.

A.5 Additional Datasets

A.5.1 Call Reports

Data on bank balance sheets used in Table A5 come from Schedule FR Y-9C (i.e., the Call Reports). To merge Call Report data with the baseline Trepp dataset, I create a manual crosswalk file by cross-referencing each bank's name from the Trepp data with its closest match in the FDIC's Institution Directory, accessed through the FFIEC's Institution Lookup Tool. For cases where a bank's name in Trepp does not unambiguously map to a single name in the FDIC's Institution Directory, I match the Trepp name to the name of the largest bank within the set of candidates in the FDIC's Institution Directory, by assets. For example, if the bank is called "First Bank" in the Trepp data, and the closest matches in the FDIC's Institution Directory are the names "First Savings Bank" and "First Bank, N.A.", I match First Bank with the larger of First Savings Bank and First Bank, N.A., by assets.

A.5.2 Income

Data on county-level 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-level dataset using the MSA associated with each county. 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. 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.

A.5.3 Inflation

Data on inflation come from the Bureau of Labor Statistics (BLS) and the Federal Housing Finance Agency (FHFA). I deflate nominal rent using CPI excluding shelter, from the BLS. I deflate aggregate investment in residential improvements using the FHFA all-transactions price index, as shown in Figure A1a.

A.5.4 Rent Control

Data on states with rent control or stabilization policies come from Landlord.com and are as of 2011.

A.6 Description of Variables

(a) *Portfolio Bank Share*$_c$: This variable is defined as the ratio of bank portfolio loan balances in county c in 2010 to the sum of bank portfolio loan, bank securitized loan, and nonbank securitized loan balances in county c in 2010. Data on bank portfolio loan balances are from the T-ALLR dataset. Since the T-ALLR dataset begins in 2013, I amortize portfolio loan balances back to 2010 using the amortization method described in Section A.1. I impute a value of zero for portfolio loan balances in counties observed in the T-Loan dataset but not in T-ALLR. Data on securitized loan balances are from the T-Loan dataset.

Taking the unweighted sum of balances observed in T-ALLR and T-Loan leads to measurement error because neither dataset is fully representative of each U.S. county. I attenuate this measurement error by reweighting securitized loan balances by a factor such that the share of loan balances held on banks' portfolios within each MSA matches the aggregate share as reported in the 2010 Financial Accounts of the United States. I apply this reweighting whenever I take the sum of quantities observed in the T-ALLR and T-Loan datasets.

(b) *Securitized Bank Share*$_c$: This variable is defined in the same way as in (a), except that the numerator is replaced by bank securitized loan balances in county c in 2010.

(c) *Total Bank Share*$_c$: This variable is defined in the same way as in (a), except that the numerator is replaced by the sum of bank portfolio and bank securitized loan balances in county c in 2010. In Table 3, I calculate this variable using the RCA dataset and define it as the ratio of bank portfolio loan origination volume in county c in 2010 to total loan origination volume in county c in 2010, since I do not observe outstanding balances in the RCA dataset.

(d) $\log(\textit{Bank Loans}_{c,t})$: This variable is defined as the log of one plus the number of bank portfolio loans originated in county c and year t for purposes other than construction. Data are from the T-ALLR dataset.

(e) $\log\left(\textit{Renovated Properties}_{c,t}\right)$: This variable is defined as the log of one plus the number of properties experiencing a renovation in county c and year t. Data are from the T-Loan dataset. In Table 3, I calculate this variable using the RCA dataset and define it in the same way as when using the T-Loan dataset. With both datasets, I exclude renovations occurring before the first year the property appears in the sample, and I include renovations that do not coincide with a new loan.

(f) $\log\left(\textit{Renovated Apartments}_{c,t}\right)$: This variable is defined as the log of one plus the product of the number of properties experiencing a renovation in county c and year t times the number of units in each of those properties. Data are from the T-Loan dataset. I exclude renovations occurring before the first year the property appears in the sample, and I include renovations that do not coincide with a new loan.

(g) $\log(\textit{Renovation Value}_{c,t})$: This variable is defined as the log of one plus the sum of revenue across properties experiencing a renovation in county c and year t. Data are from the T-Loan dataset. I exclude renovations occurring before the first year the property appears in the sample, and I include renovations that do not coincide with a new loan.

(h) $\log\left(\textit{Newly-Built Properties}_{c,t}\right)$: This variable is defined as the log of one plus the sum of construction projects observed in the T-ALLR and T-Loan datasets in county c and year t. If there are no construction projects observed in the combined Trepp dataset, then I replace this variable with the log of one plus the number of apartment construction permits issued, based on data from the Census' Building Permits Survey.

(i) $\Delta\log\left(\textit{Top-Quality Properties}_{c,t}\right)$: This variable is defined as the one-year change in the log of one plus the product of the number of properties in county c and year t times share of apartments ranked in the top quality segment by professional property inspectors. Property inspection ratings are based on the Mortgage Bankers Association and Commercial Real Estate Finance Council's (MBA/CREFC) property inspection rating. This rating is regularly collected as part of the standard apartment loan servicing protocol. Its purpose is to minimize agency frictions that 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 unit that reflects "the highest current market standards". There is a checklist of features to help inspectors assign properties the appropriate score. Data are from the T-Loan dataset.

(j) $\Delta \log \left(\textit{Average Rent}_{c,t} \right)$: This variable is defined as the one-year change in the log of the weighted average rent across properties in county c and year t, weighting by the number of units in each property. A property's rent is calculated as revenue per occupied unit. I replace this variable with an empty value when it exceeds 25 log points. Data are from the T-Loan dataset.

(k) $\Delta \textit{Cost-Burdened Share}_{c,t}$: This variable is defined as the one-year change in the weighted share of properties in county c and year t whose rent exceeds 30% of the average income for its corresponding zip code in year t, weighting by the number of units in each property. A property's rent is calculated as revenue per occupied unit. Data on rent are from the T-Loan dataset. Average zip code-level income is defined as total adjusted gross income divided by the number of tax returns, based on data from the IRS SOI Tax Stats.

(l) $\Delta \log \left(\textit{Top-Quintile Rent}_{c,t} \right)$: This variable is defined as the one-year change in the log of the weighted average rent across properties in county c and year t whose rent is above the 80^{th} percentile within c and t, weighting by the number of units in each property. A property's rent is calculated as revenue per occupied unit. I replace this variable with an empty value when $\Delta \log \left(\textit{Average Rent}_{c,t} \right)$ exceeds 25 log points. Data are from the T-Loan dataset.

(m) $\textit{GSE Share}_{c,t}$: This variable is defined as the share of securitized loan balances in county c and year t that are backed by the Government Sponsored Enterprises (GSEs). Data are from the T-Loan dataset. I identify loans backed by the GSEs as those whose CMBS deal begins with the strings "fn" or "fh".

B Additional Robustness

This appendix describes additional robustness tests referenced in Section 6.

B.1 Time-Varying Local Demand

I reperform the baseline analysis after allowing for nonparametric time trends by county characteristics related to household demand and physical supply. This exercise allows me to test whether the treatment variable, $\textit{Total Bank Share}_c \times \textit{Post}_t$, spuriously captures time-varying, non-financial shocks to improvement activity. The estimated effect in Table A8 is stable across specifications, making it unlikely that such unobserved shocks bias the results.

B.2 Measuring Exposure with the Office Sector

In Table A9, I measure exposure to HVCRE regulation using loans secured by office buildings, based on a version of the T-Loan dataset that covers the office sector. The corresponding results are similar to those from Table 2, supporting their validity.

B.3 Sensitivity to Functional Form

I estimate a cross-sectional version of equation (1) and plot the results in Figure A3. There is no clear nonlinearity in the treatment effect, which suggests that the linear functional form in equation (1) is a best-approximation.

B.4 Geographic Distribution of Exposure

In Figure A4, I plot the distribution of banks' share of apartment loan balances in 2010 across states to assess geographic clustering. If the effect of any such clustering on improvement activity is slow-moving, then it would be subsumed by the county fixed effect in equation (1). On the other hand, if such clustering predisposes a county to changes in improvement activity that coincide with the introduction of HVCRE regulation, then the baseline results could be biased. However, Figure A4 shows how the distribution is fairly uniform across states, suggesting that the scope for bias due to geographic clustering is small.

C Quality-Adjusted Rent

This appendix provides additional methodological details and results related to the hedonic adjustment referenced in Section 8 of the text. As mentioned in the text, the purpose of this exercise is to assess the plausibility of the large estimated aggregate effect of HVCRE regulation shown in Table 9.

Hedonic adjustments have a long tradition in the housing literature, as summarized by Sheppard (1999). The pricing kernel shown in equation (11) of the text combines elements of repeat-"sale" (i.e., repeat-rent) and hedonic indices, which has several well-known advantages (e.g., Wallace and Meese 1997). Reproducing that equation below, I estimate

$$\Delta \log \left(Rent_{i,t} \right) = \beta^{\Theta} \Delta \Theta_{i,t} + \alpha_i + \alpha_t + u_{i,t}, \tag{C1}$$

where the notation is the same as in Section 8.1 of the text. All changes are over 2 years because the AHS is administered biennially. I estimate equation (C1) over 1997-2013 to utilize additional variation, but I only perform the adjustment over 2007-13. 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.

The features in $\Theta_{i,t}$ are: a dishwasher, trash compactor, garbage disposal, washing machine, dryer, air conditioning (A/C), central A/C conditional on installing A/C, and log square feet. For the case of square feet, $\Delta\theta_{i,t}$ is the increase in log square feet and not an indicator for the installment of the feature. I choose these features because they are available for 85% of units in the sample. The estimated loadings on each feature are shown in Appendix Table A7.

I next compute a housing unit's quality-adjusted rent as shown in equation (12), reproduced below as

$$Rent^{H}_{i,t} = Rent_{i,t_0} \times \exp \left[\sum_{\tau=t_0+2}^{t} \left(\Delta \log \left(Rent_{i,\tau} \right) - \beta^{\Theta} \Delta \Theta_{i,\tau} \right) \right] \tag{C2}$$

where the notation is again the same as in Section 8.1. Then, I define the hedonic index π^H_t as the normalized average of $Rent^H_{i,t}$ across housing units i,

$$\pi^H_t = \frac{\sum_i Rent^H_{i,t}}{\sum_i Rent_{i,t_0}}. \tag{C3}$$

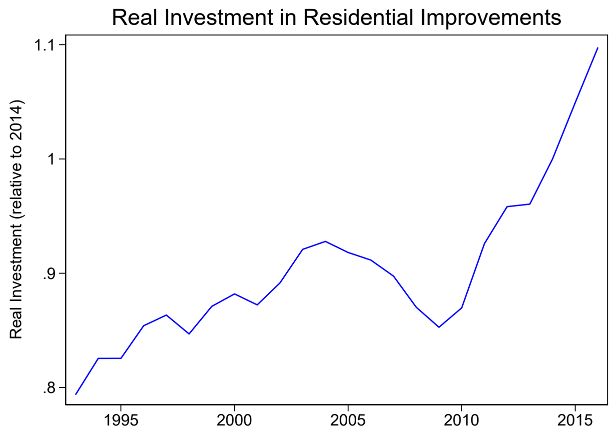
As described in Appendix A, I drop units that experienced a change in tenure (e.g., "condo conversions") from my analysis. The aggregation in equation (C3) has the same basic form as that used by the Bureau of Labor Statistics (BLS) after accounting for the fact that I work at a biennial frequency (Gallin and Verbrugge 2007).

Figure A5a summarizes 2007-13 annual growth in π^H_t and other related indices. The baseline hedonic index, shown in the center of the figure, saw 0.5% real growth, as in Figure 5. Moving to the left, I perform an age adjustment similar to that used by the BLS. 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, since rent growth based on the AHS is on average 0.8 pps higher than CPI-based rent growth (McCarthy, Peach and Ploenzke 2015). The indices to the right of the baseline in Figure A5a perform two robustness checks. First, I reestimate equation (C1) after allowing the price vector β^{Θ} to vary by year. This results in a similar growth rate of 0.7%. Second, I control for the change in the renter's income in the pricing equation (C1). While non-standard, this additional control absorbs the effect of time-varying characteristics of the housing unit that correlate with both improvements and the renter's income growth. The resulting growth rate is almost unchanged at 0.5%. Finally, Figure A5b recalculates the hedonic index over various subperiods. The results suggest that the large contribution of improvements to rent growth is largely a post-Recession phenomenon, as there is little difference between unadjusted and quality-adjusted rent growth over 1997-2007 or 2001-05.

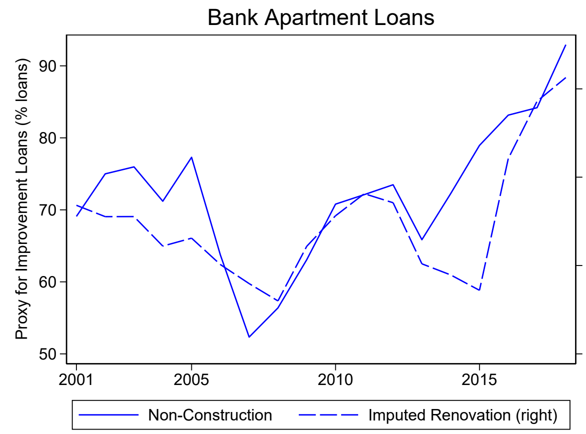
Collectively, the results of this hedonic adjustment imply that quality improvements explain a significant share (e.g., 70%) of observed real rent growth over 2007-13. This finding lends plausibility to the large effect of HVCRE regulation on rent growth over 2015-16 shown in Table 9.

Additional Figures and Tables

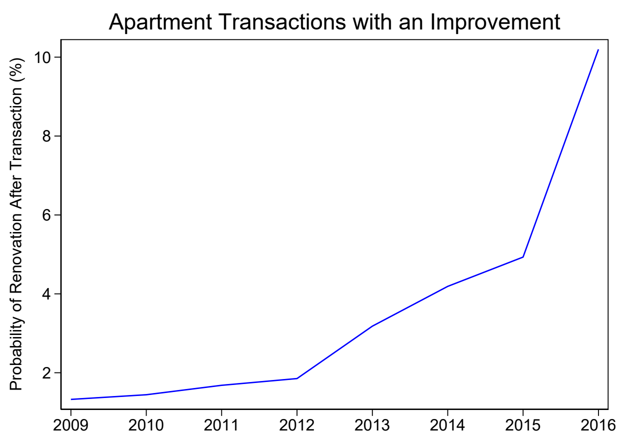
Figure A1: Robustness of Stylized Facts



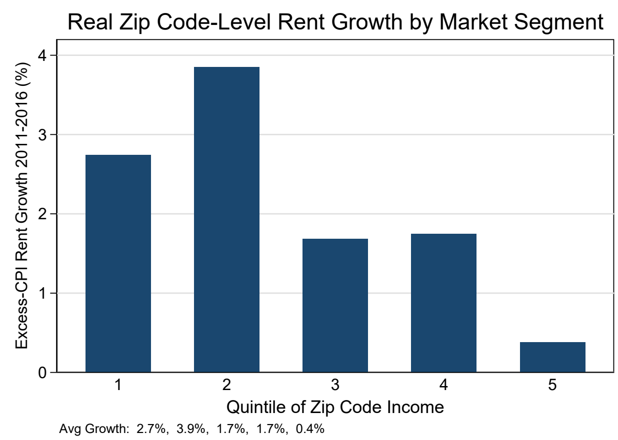
(a) Aggregate Improvement Spending



(b) Bank Apartment Improvement Loans



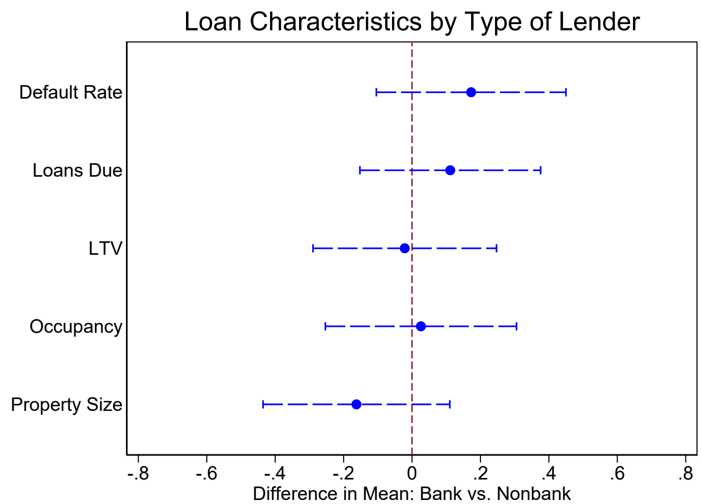
(c) Apartment Transactions with an Improvement



(d) Zillow-Based Real Rent Growth by Segment

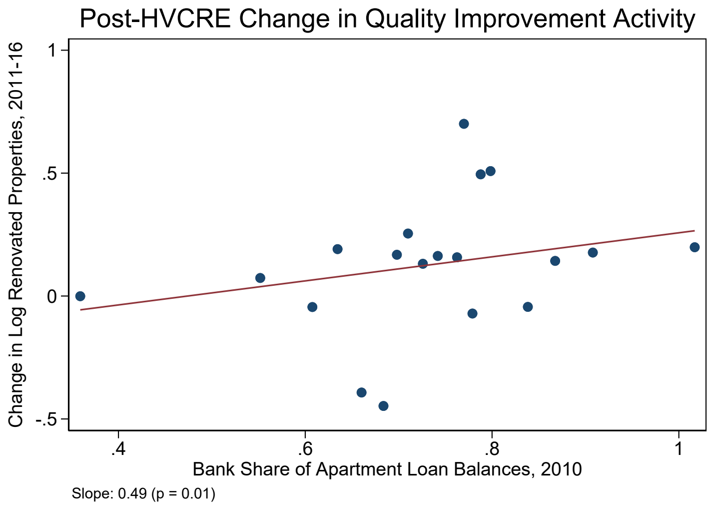
Note: This figure assesses the robustness of the stylized facts in Figure 1 to alternative datasets and measures of improvement activity. Panel (a) plots real aggregate investment in residential improvements, based on the U.S. Fixed Assets Accounts. Panel (b) plots the share of bank loans that are for apartment improvements, based on Trepp's T-ALLR dataset. The plot uses two proxy measures for whether the loan finances an improvement: whether the loan's purpose is not construction (Non-Construction); or whether the loan is imputed as financing a renovation using observable loan characteristics applied to Trepp's T-Loan dataset, as described in Appendix A.1.3 (Imputed Renovation). Panel (c) plots the share of property-years experiencing a renovation conditional on the property transacting in the year shown on the horizontal axis or in the year after it, based on the RCA dataset. Panel (d) plots real growth in average zip code level apartment rent by income quintile, based on Zillow's Zip Code Level Multifamily Rent Index. Zip codes are sorted into quintiles by log average real income in the zip code relative to the MSA-year mean, based on the IRS SOI Tax Stats.

Figure A2: Difference in Loan Specialization between Banks and Nonbanks



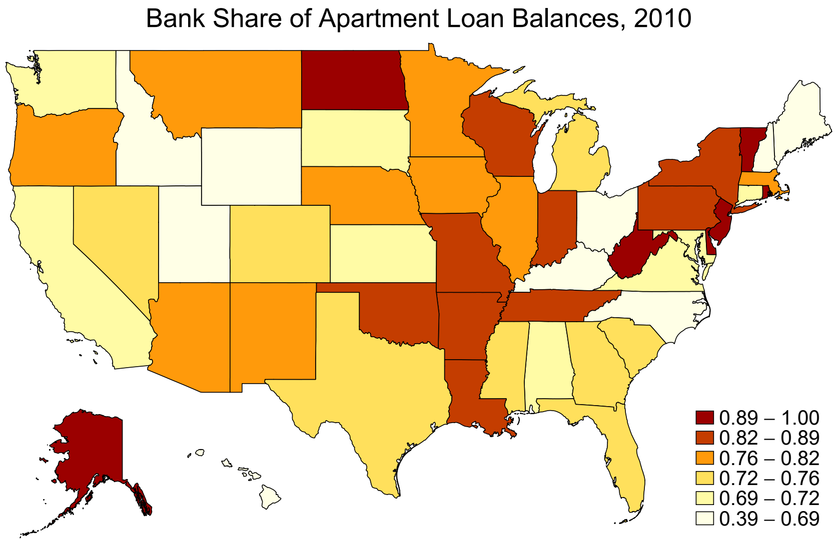
Note: This figure assesses the exclusion restriction associated with the first-stage results in Table 6 by plotting the average difference in the indicated variable between bank and nonbank lenders. Variables are normalized to have zero mean and unit variance and aggregated to the lender-level by averaging across loans 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 apartment units. Brackets are a 95% confidence interval with heteroskedasticity robust standard errors. Data are from Trepp's T-Loan dataset.

Figure A3: County-Level Improvements and HVCRE Regulation in the Cross-Section



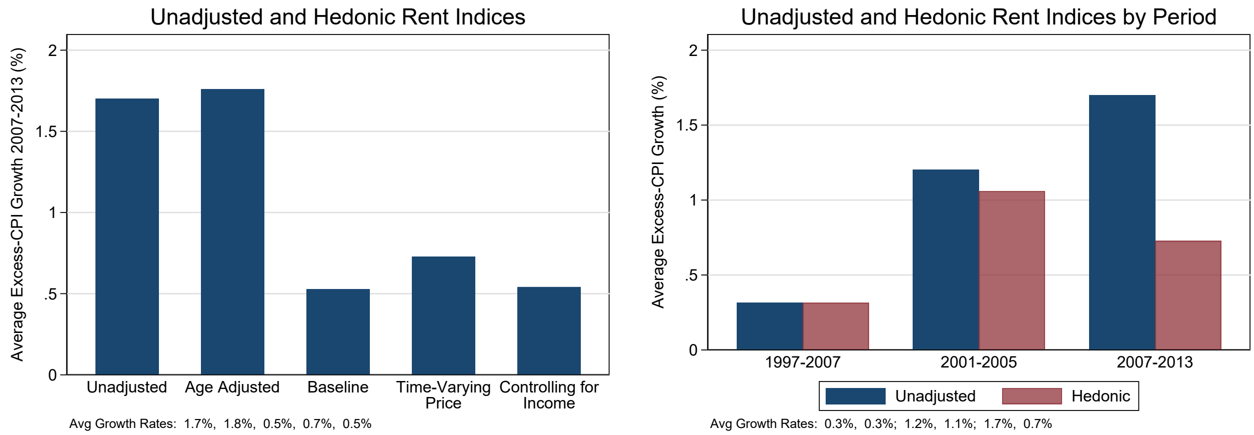
Note: This figure plots the results of a cross-sectional version of equation (1), which assesses robustness to functional form. The vertical axis shows the change in log renovated apartment properties from the 2011-14 period to the 2015-16 period. The horizontal axis shows banks' share of apartment loan balances in 2010, corresponding to the variable *Total Bank Share*$_c$. Variables are demeaned by state. The plot is binned so that each point corresponds to roughly 30 counties. Data for the variable on the vertical axis are from Trepp's T-Loan dataset. Data for the variable on the horizontal axis are from Trepp's T-ALLR and T-Loan datasets.

Figure A4: Geographic Distribution of Exposure Variable



Note: This figure plots banks' share of apartment loan balances in 2010 across states to assess whether exposure to HVCRE regulation exhibits geographic clustering. Data are from Trepp's T-ALLR and T-Loan datasets.

Figure A5: Robustness of the Hedonic Adjustment



Note: This figure plots additional results described in Appendix C that assess the robustness of the baseline hedonic adjustment in Section 8.1. 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. Baseline denotes the hedonic index. Time-Varying Price denotes the baseline index after allowing the coefficients in equation (11) to vary by year. Controlling for Income denotes the baseline index after controlling for the change in the renter's income percentile. Panel (b) plots average annual real growth in unadjusted rent and the hedonic index for various periods, allowing the coefficients in equation (11) to vary by year. Data are from the Census' AHS dataset.

Table A1: Largest Originators over 2011-16

Rank	Originator	Type of Originator
1	CBRE Capital	Nonbank
2	Berkadia	Nonbank
3	Holliday Fenoglio Fowler	Nonbank
4	Walker & Dunlop	Nonbank
5	Berkeley Point Capital	Nonbank
6	Wells Fargo	Bank
7	NorthMarq Capital	Nonbank
8	KeyCorp	Bank
9	Capital One	Bank
10	Jones Lang LaSalle	Nonbank
11	Grandbridge Real Estate	Nonbank
12	JP Morgan	Bank
13	Beech Street Capital	Nonbank
14	PNC	Bank
15	Prudential	Bank

Note: This table shows the largest 15 originators of securitized apartment loans over 2011-16 based on Trepp's T-Loan dataset. Originators are ranked by volume of loans originated. Prudential is classified as a bank because it is a Designated Financial Company, as described in Appendix A.2.

Table A2: Robustness to Excluding Improvements by Large Borrowers or Reclassified Improvements

Outcome:			$\log\left(Renovated\ Properties_{c,t}\right)$			
	(1)	(2)	(3)	(4)	(5)	(6)
$Total\ Bank\ Share_c \times Post_t$	0.323	0.352	0.323	0.326	0.353	0.318
	(0.068)	(0.029)	(0.002)	(0.066)	(0.030)	(0.002)
Excluded Renovations	Large Borrowers	Large Borrowers	Large Borrowers	Potentially Reclassified	Potentially Reclassified	Potentially Reclassified
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
County Controls	No	Yes	Yes	No	Yes	Yes
State-Year FE	No	No	Yes	No	No	Yes
R-squared	0.566	0.609	0.719	0.569	0.610	0.723
Number of Observations	3,128	3,128	3,128	3,128	3,128	3,128

Note: P-values are in parentheses. This table estimates equation (1), the paper's baseline equation. Columns 1-3 exclude renovations by borrowers (i.e., real estate investors) with more than one distinct source of credit in 2010 to assess whether oversampling of large, unconstrained borrowers biases the baseline estimates. Columns 4-6 exclude renovations that could potentially be classified as construction projects to assess whether reclassification incentives bias the baseline estimates. Subscripts c and t denote county and year. Potentially reclassified renovations are defined as occurring on properties built within the previous 3 years or built before 1940. Data for the outcome variable are from Trepp's T-Loan dataset. Data for the exposure variable are from Trepp's T-ALLR and T-Loan datasets. The remaining notes are the same as in Table 2.

Table A3: Robustness of Constrained Demand as an Amplification Mechanism

Outcome:	$\log\left(\textit{Renovated Properties}_{c,t}\right)$		
	(1)	(2)	(3)
$\textit{Total Bank Share}_c \times \textit{Post}_t$	0.629	0.320	-0.115
	(0.003)	(0.002)	(0.186)
$\textit{Total Bank Share}_c \times \textit{Post}_t \times \textit{Interaction}_{c,t}$	-0.224	0.112	0.298
	(0.043)	(0.022)	(0.000)
Definition of Interaction	Borrower Credit Sources	Renter Income	No Rent Stabilization
Control for Interaction	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
County FE	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes
R-squared	0.705	0.712	0.705
Number of Observations	3,128	3,128	3,128

Note: P-values are in parentheses. This table estimates a variant of equation (1) that assesses whether the baseline effect is amplified when there are borrowing constraints and the unconstrained demand for improvement financing is higher. Subscripts c and t denote county and year. The regression equation is of the form

$$Y_{c,t} = \beta_0\left(\textit{Total Bank Share}_c \times \textit{Post}_t\right) + \beta_1\left(\textit{Total Bank Share}_c \times \textit{Post}_t \times \textit{Interaction}_{c,t}\right) + \alpha_c + \alpha_t + \gamma X_{c,t} + u_{c,t},$$

where $\textit{Interaction}_{c,t}$ is a characteristic of county c in year t; and the remaining notation is the same as in Table 2. All specifications control for $\textit{Interaction}_{c,t}$. The interaction variables are defined as follows: Borrower Credit Sources is the product between $\textit{Post}_t$ and the average borrower's (i.e., real estate investor's) number of distinct sources of credit in 2010, based on Trepp's T-Loan dataset; Renter Income is log average real income per capita in t, based on data from the Bureau of Economic Analysis; No Rent Stabilization is the product between $\textit{Post}_t$ and an indicator for whether the county is outside a state where rent control or stabilization policies are in place, based on data from Landlord.com. I only observe the borrower's identity for 14% of properties, and for the remaining 86% I impute the borrower's number of distinct sources of credit from a linear projection onto 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. Interaction variables are normalized to have zero mean and unit variance. The first interaction variable inversely proxies for borrowing constraints, and the second two interactions proxy for the unconstrained demand for improvement financing. Data for the outcome variable are from Trepp's T-Loan dataset. Data for the exposure variable are from Trepp's T-ALLR and T-Loan datasets. The remaining notes are the same as in Table 2.

Table A4: Summary Statistics for the T-Loan Property-Level and Lender-Level Datasets

	Observations	Mean	Standard Deviation
Property-Level Variables:			
$Bank_{\ell}$	30,733	0.473	0.499
$Probability\ of\ Renovation_{i,\ell,t}$	30,733	0.026	0.158
Lender-Level Variables:			
$Bank_{\ell}$	582	0.306	0.461
$\log(Renovations_{\ell,t})$	582	5.153	3.153
Baseline Number of Properties: 6,100			
Baseline Number of Lenders: 122			

Note: This table presents summary statistics of the key variables from the property-level and lender-level datasets used in Section 6. Subscripts i, ℓ, and t denote property, lender, and year. The upper panel summarizes property-level variables: $Bank_{\ell}$ indicates if the existing loan on the property is originated by a bank; and $Probability\ of\ Renovation_{i,\ell,t}$ indicates if the property is renovated in t. Observations are property-years. The lower panel summarizes lender-level variables: $Bank_{\ell}$ indicates if ℓ is a bank; and $Renovations_{\ell,t}$ is the number of renovated apartments financed by a new loan in t. Observations are lender-years weighted by market share. The sample period is 2011-16. Data are from Trepp's T-Loan dataset.

Table A5: Additional Robustness of the First-Stage Effect

Outcome:	$\log(\mathit{Renovations}_{\ell,t})$			$\mathit{Interest\ Rate}_{\ell,t}$	$\mathit{ARM\ Margin}_{\ell,t}$
	(1)	(2)	(3)	(4)	(5)
$\mathit{Bank}_\ell \times \mathit{Post}_t$	3.538	1.282	3.419	-0.140	-0.141
	(0.000)	(0.075)	(0.008)	(0.086)	(0.040)
$\mathit{Bank}_\ell \times \mathit{Post}_t \times \mathit{Construction\text{-}to\text{-}Assets}_\ell$	5.446				
	(0.000)				
$\mathit{Bank}_\ell \times \mathit{Post}_t \times \mathit{Capital\ Ratio}_\ell$			-3.574		
			(0.002)		
Sample	All	Non Big-4	All	All	All
Post × Construction-to-Assets	Yes	No	No	No	No
Post × Capital Ratio	No	No	Yes	No	No
Lender FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
R-squared	0.661	0.654	0.656	0.834	0.674
Number of Observations	366	512	366	424	424

Note: P-values are in parentheses. This table estimates a variant of equation (8) that assesses the robustness of the first-stage effect documented in Table 6. Subscripts ℓ and t denote lender and year. $\mathit{Construction\text{-}to\text{-}Assets}_\ell$ and $\mathit{Capital\ Ratio}_\ell$ are the 2010 ratios of construction loans to total assets and total equity to total assets, respectively, based on data from the Call Reports. Both variables are normalized to have zero mean and unit variance, and nonbanks are assigned a value of zero. $\mathit{Interest\ Rate}_{\ell,t}$ and $\mathit{ARM\ Margin}_{\ell,t}$ are the principal-weighted average interest rate and adjustable-rate mortgage (ARM) margin on loans for renovations originated by ℓ as of t, respectively. Column 2 reestimates equation (8) after dropping the Big-4 banks: JP Morgan, Citi, Bank of America, and Wells Fargo. Data are from the Call Reports and Trepp's T-Loan dataset. The remaining notes are the same as in Table 6.

Table A6: Summary Statistics for the AHS Dataset

	Observations	Mean	Standard Deviation
$\Delta \log (Rent_{i,t})$	81,733	0.050	0.964
Installment of:			
$Dishwasher_{i,t}$	81,733	0.034	0.182
$Washing\ Machine_{i,t}$	81,733	0.068	0.252
$Trash\ Compactor_{i,t}$	81,733	0.010	0.100
$Disposal_{i,t}$	81,733	0.043	0.202
$Central\ A/C_{i,t}$	81,733	0.042	0.200
$A/C_{i,t}$	81,733	0.076	0.266
$Dryer_{i,t}$	81,733	0.063	0.244
$\log \left(Square\ Feet_{i,t}\right)$	81,733	0.006	0.082
Baseline Number of Housing Units: 13,186			

Note: This table presents summary statistics of the key variables from the Census' AHS dataset. Subscripts i and t denote housing unit and year. The variable $\Delta \log (Rent_{i,t})$ is the change in log rent. The remaining variables are indicators for the installment of the given feature, except for $\log \left(Square\ Feet_{i,t}\right)$ 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 two-year intervals. Observations are rental housing unit-years. The sample period is 1997-2013.

Table A7: Pricing Kernel for Hedonic Rent Index

Outcome:	$\Delta \log (Rent_{i,t})$
Installment of:	
$Dishwasher_{i,t}$	0.118
	(0.000)
$Washing\ Machine_{i,t}$	0.097
	(0.000)
$Disposal_{i,t}$	0.031
	(0.117)
$Trash\ Compactor_{i,t}$	0.013
	(0.749)
$Central\ A/C_{i,t}$	0.023
	(0.259)
$A/C_{i,t}$	0.063
	(0.000)
$Dryer_{i,t}$	-0.007
	(0.811)
$\log \left(Square\ Feet_{i,t}\right)$	0.121
	(0.012)
Property FE	Yes
Year FE	Yes
R-squared	0.065
Number of Observations	76,148

Note: P-values are in parentheses. This table estimates equation (11), which is the pricing kernel used to construct the hedonic rent index in Section 8.1 that assesses the plausibility of the implied aggregate effect of HVCRE regulation. Subscripts i and t denote housing unit and year. The regression equation is

$$\Delta \log (Rent_{i,t}) = \beta^{\Theta} \Delta\Theta_{i,t} + \alpha_i + \alpha_t + u_{i,t},$$

where observations are rental housing unit-years; and $\Delta \log (Rent_{i,t})$ is the change in log rent. The vector of regressors, $\Delta\Theta_{i,t}$, are indicators for the installment of the given feature, except for $\log \left(Square\ Feet_{i,t}\right)$ 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 two-year intervals. There are 13,186 housing units. The sample period is 1997-2013. Data are from the Census' AHS dataset.

Table A8: Robustness to Including Heterogeneous Time Trends

Outcome:	$\log\left(\textit{Renovated Properties}_{c,t}\right)$				
	(1)	(2)	(3)	(4)	(5)
$\textit{Total Bank Share}_c \times \textit{Post}_t$	0.345	0.304	0.319	0.306	0.326
	(0.002)	(0.003)	(0.002)	(0.003)	(0.009)
$\textit{Characteristic}_c \times \textit{Year-2012}_t$	0.064	-0.003	-0.063	-0.013	-0.102
	(0.099)	(0.870)	(0.082)	(0.510)	(0.083)
$\textit{Characteristic}_c \times \textit{Year-2013}_t$	0.101	0.029	-0.081	-0.003	-0.161
	(0.002)	(0.379)	(0.075)	(0.921)	(0.010)
$\textit{Characteristic}_c \times \textit{Year-2014}_t$	0.178	-0.017	-0.209	-0.015	-0.272
	(0.020)	(0.702)	(0.005)	(0.760)	(0.014)
$\textit{Characteristic}_c \times \textit{Year-2015}_t$	0 0.138	-0.014	-0.134	-0.037	-0.168
	(0.017)	(0.678)	(0.015)	(0.320)	(0.054)
$\textit{Characteristic}_c \times \textit{Year-2016}_t$	0.172	0.049	-0.206	-0.043	-0.229
	(0.044)	(0.169)	(0.016)	(0.547)	(0.081)
Characteristic	Income	Winter Storms	White Share	College Education	Supply 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	0.725	0.708	0.722	0.706	0.728
Number of Observations	3,128	3,128	3,128	3,128	2,620

Note: P-values are in parentheses. This table estimates a variant of equation (1) that assesses the robustness of the baseline results to heterogeneous time trends. Subscripts c and t denote county and year. The regression equation is of the form

$$Y_{c,t} = \beta\left(\textit{Total Bank Share}_c \times \textit{Post}_t\right) + \sum_{t=2012}^{2016} \delta_t\left(\textit{Characteristic}_c \times \textit{Year}_t\right) + \alpha_c + \alpha_t + \gamma X_{c,t} + u_{c,t},$$

where $\textit{Year}_t$ is a year indicator; $\textit{Characteristic}_c$ is a characteristic of county c; and the remaining notation is the same as in Table 2. The characteristics are defined as follows: Income is real income per capita for the surrounding MSA averaged over 2011-16, based on data from the Bureau of Economic Analysis; Winter Storms is the average number of winter storms per year over 2011-16, based on data from the National Oceanic and Atmospheric Association; White Share is the 2010 share of inhabitants over age 16 that are white, based on the 2010 Census; College Education is the 2010 share of inhabitants with at least a bachelor's degree, based on the 2010 Census; Supply Elasticity is the elasticity of housing supply as estimated by Saiz (2010). Characteristics are normalized to have zero mean and unit variance. Data for the outcome variable are from Trepp's T-Loan dataset. Data for the exposure variable are from Trepp's T-ALLR and T-Loan datasets. The remaining notes are the same as in Table 2.

Table A9: Robustness to Measuring Exposure with the Office Sector

Outcome:	$\log\left(Renovated\ Properties_{c,t}\right)$ (1)	(2)
$Bank\ Office\ Share_c \times Post_t$	0.171	0.462
	(0.078)	(0.049)
Base Period	2010	2001-09
Year FE	Yes	Yes
County FE	Yes	Yes
R-squared	0.565	0.586
Number of Observations	3,159	3,159

Note: P-values are in parentheses. This table estimates a variant of equation (1) that assesses the robustness of the baseline results to using loans secured by office buildings to measure exposure to HVCRE regulation. Subscripts c and t denote county and year. Each column uses a different measure of banks' share of office loan balances, denoted $Bank\ Office\ Share_c$. The measure in column 1 is bank securitized office loan balances in 2010 divided by the sum of bank and nonbank securitized office loan balances in 2010. The measure in column 2 is similarly defined using loan balances averaged over 2001-09. Data for the outcome variable are from Trepp's T-Loan dataset. Data for the exposure variable are from a version of the T-Loan dataset that covers the office sector. The remaining notes are the same as in Table 2.

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