Mortgage Supply and Housing Rents*

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Abstract

We show that a contraction of mortgage supply after the Great Recession has increased housing rents. Our empirical strategy exploits heterogeneity in MSAs' exposure to regulatory shocks experienced by lenders over the 2010–2014 period. Tighter lending standards have increased demand for rental housing, leading to higher rents, depressed homeownership rates and an increase in rental supply. Absent the credit supply contraction, annual rent growth would have been 2.1 percentage points lower over 2010–2014 in MSAs in which lending standards rose from their 2008 levels.

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Introduction

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

The mechanism that we test was originally proposed by Linneman and Wachter (1989) and is formalized by Gete and Reher (2016).[1] It begins with a shock that contracts mortgage supply for some lenders such as, for example, greater regulatory costs because of stress testing. Then frictions to substitute across lenders lead to more difficult access to credit. Since downward house price rigidities prevent most households from buying without credit, households denied credit move from the market for homeownership to the rental market. An increase in the demand for rental housing, together with an imperfect short-run elasticity of supply, drives up housing rents and reduces homeownership rates. Lower price-to-rent ratios encourage investors to buy owner occupied units and convert them to rentals.

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

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

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

of Deposits.[2] Stein (2014) discusses how Dodd-Frank has exposed the Big-4 banks to heightened oversight and higher liquidity and capital requirements. Jayaratne and Strahan (1996) document the importance of bank branches in facilitating access to credit, and since their seminal work a number of papers have exploited bank branch distributions to create credit supply instruments (e.g., Nguyen 2016). Finally, as a third instrument, we explore the share of top 20 lenders active in 2007. D'Acunto and Rossi (2017) use this instrument to study a regressive redistribution of mortgage credit between 2011 and 2014 stemming from post-crisis financial regulation.

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

All the specifications point in the same direction: tighter credit caused higher housing rents over 2010–2014. Our baseline specification suggests that a 1-percentage-point increase in denial rates increased rent growth by 1.3 percentage points. To put this estimate into perspective it is useful to look how denial rates changed over 2010–2014. Over this period average denial rates fell by 1.6 percentage points relative to their 2008 levels. However, denial rates actually rose in 31% of MSAs. Our estimates indicate that rents would have grown at least 2.1 percentage points less in these MSAs if their denial dates had moved with the national average. This effect is equal to 70% of a cross-sectional standard deviation in 2010–2014 rent growth. Thus, elevated post-crisis credit stringency explains a meaningful amount of cross-MSA variation in recent rent behavior.

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

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

homeownership rate.

The previous results not only support our theory but also provide more evidence to rule out the possibility that unobserved housing demand shocks violate the exclusion restriction. If that were the case and the MSAs more exposed to our credit instruments also experienced positive demand shocks, then we might observe a positive and significant relationship not only between instrumented denials and rents but also between instrumented denials and prices. This is because demand shocks can generate comovement between prices and rents as shown in Gete and Reher (2016) and Gete and Zecchetto (2017), among others.[3] However, we find no evidence to support this concern. House price dynamics strongly suggest that our results are due to a credit supply contraction operating through a tenure choice channel.

The instruments' inability to explain housing rents in a placebo exercise suggests that they are valid post-crisis credit supply shocks, not that the theory is invalid in the precrisis period. To investigate whether credit affects rents in other periods, we use the Loutskina and Strahan (2015) instrument, which the literature has accepted as a valid credit supply shock. Interestingly, there is a positive and statistically significant effect of credit supply on rents over the precrisis period.[4] We interpret this result, together with the placebo exercise, as further evidence of the instruments' validity.

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

Thus, collectively, the paper uses a broad array of empirical methodologies which suggest the same result: a contraction of mortgage supply after the Great Recession caused higher housing rents. This result does not rule out alternative explanations for rent growth, but instead highlights the importance of the credit contraction theory after rigorously accounting for these other explanations.

To the best of our knowledge, in terms of contribution to the literature, this is the first paper

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

[4] The magnitude is much smaller than we found over the post-crisis period, likely because variation in lending standards over the precrisis period was much smaller.

to study the role of credit supply in the dynamics of post-crisis housing rents.[5] The existing literature on housing rents has thus far focused on other, noncredit drivers like population flows (Saiz 2007), shrinking leisure of high-income households (Edlund, Machado, and Sviatchi 2015), income growth (Hornbeck and Moretti 2015,; Muehlenbachs, Spiller and Timmins 2015) or households' expected duration of stay in a house (Halket and Pignatti 2015). Mezza et al. (2016) show that student debt has affected the demand for homeownership.

In terms of empirical strategy, our paper complements Chen, Hanson, and Stein (2017), D'Acunto and Rossi (2017), and Goodman (2017). Chen, Hanson, and Stein (2017) show that a credit supply shock experienced by the Big-4 banks led to a contraction of small business credit and caused higher unemployment. Their identification strategy is similar to our use of a Big-4 instrument, and we control carefully for the factors they highlight, like establishment creation, to alleviate concerns that local economic conditions are driving the results. D'Acunto and Rossi (2017) document that U.S. financial institutions have reduced mortgage lending for medium-sized loans and increased lending for large loans since the crisis. They conclude that this resulted from a supply-side change, namely the increase in the costs of originating mortgages imposed by Dodd-Frank. We show that our results hold if we use their instrumental variable to capture the effect of a contraction of credit on housing rents. Goodman (2017) documents that mortgage credit has become very tight in the aftermath of the financial crisis and discusses potential regulatory causes of this contraction.

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

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

1 Motivation and Theory

In this section, we describe the theory that we want to test. As Figure 1 shows, following the recent financial crisis, housing rents have increased steeply in many MSAs. The rent-to-income ratio for the median MSA has risen by more following the Great Recession than it did over the previous 25 years combined. At the same time, the U.S. homeownership rate has collapsed to historic lows.[6]

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

Higher costs for the lender, for example, because of higher capital requirements or the costs associated with stress testing, shift the credit supply curve inward. Consequently, more households are denied credit at preshock conditions. Tighter lending standards make some households unable to borrow at the conditions they want, and, given downward rigidities in house prices, they decide to rent. Higher demand for rental housing, together with an inelastic supply and imperfect convertibility between rental and owner-occupied units, lead to higher rents, lower homeownership and lower house prices. As the price-to-rent ratio falls, there are investors who buy owner occupied properties and place them for rent. That is, the tenure conversion rate increases. This "buy to let" behavior then induces a positive correlation between rents and prices. Moreover, new construction further increases the supply of rental housing.

We check that the data support the predictions of the previous theory. Sections 2 and 3 study housing rents, and Section 4 analyzes the remaining implications.

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

2 Mortgage Supply and Rent Growth

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

2.1 Database

We measure credit supply using mortgage denial rates to avoid capturing any effect from borrowers' reaction to a loan offer.[7] Our data come from the Home Mortgage Disclosure Act (HMDA) which we merge with rent data from the Zillow Rent Index (ZRI) and other controls at the MSA level.[8] The units of the ZRI are nominal dollars per month for the median property in the MSA. We study MSAs as the unit of analysis, as they are arguably the smallest geographical unit in which households cannot borrow in one location to purchase a house in another one.

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

2.2 Specification

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

$$\text{Avg. rent growth}_{m,\text{10-14}} = \beta \times \text{Avg. denial rate}_{m,\text{10-14}} + \gamma X_m + u_m, \tag{1}$$

where m indexes MSAs, Avg. denial rate$_{m,\text{10-14}}$ denotes the average denial rate over 2010–2014 and Avg. rent growth$_{m,\text{10-14}}$ denotes average annual rent growth over 2010–2014.[9] The controls

[7]Denial rates are strongly correlated with proxies for lending standards. For example, Driscoll, Kay, and Vojtech (2017) find that denial rates are closely linked to measures of tightening standards from the Senior Loan Officer Opinion Survey (SLOOS).

[8]Zillow computes this index by imputing a rent for each property in an MSA based on recent rental transactions. It does not impute rent using house prices. Figure A1 in the Online Appendix shows how the ZRI is quite similar to the St. Louis Fed's rent index, which is available for a selection of MSAs.

[9]We use average variables because with persistent but non-permanent credit supply shocks it is inappropriate to estimate (1) using growth in denials as the independent variable. This is because, as we show in Figure A2 of the Online Appendix, our credit supply shocks are strongest in the beginning of the 2010–2014 window. Thus they are positively correlated with average denial rates over this period but, because of mean reversion,

in X_m account for both precrisis dynamics and level effects, including the 2000–2008 average annual change in log median income, log median rent, log median house price, log population, log median inhabitant age, and unemployment rate; and the 2009 level of log median income, log median rent, log population, log median inhabitant age, and unemployment rate. We also include state fixed effects in all specifications.

If we estimate (1) using OLS, we would obtain biased estimates. This is because local shocks can drive both rent dynamics and mortgage supply. For example, a positive shock to an MSA's economic activity would increase amenities and thus rent growth, while raising households' income, thus reducing mortgage denials. As a result, the OLS estimate would be biased downward. Another possibility is that households rent because of lack of employment opportunities, so that OLS would produce upward bias.[10] Regardless of the direction of the bias, we aim to overcome it by proposing two credit supply instruments for which there is extensive evidence that the exclusion restriction is satisfied.

2.3 The instrumental variables

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

Our preferred instrument is MSA exposure to lenders subject to a Comprehensive Capital Analysis and Review (CCAR) stress test between 2011 and 2015. These tests are meant to ensure that the largest bank holding companies have enough capital to weather a financial crisis, but as a side-effect they have encouraged those institutions to tighten their standards in mortgage markets (Calem, Correa, and Lee 2016). We measure an MSA's exposure to these lenders using their preshock, 2008 mortgage application share. The results are similar if we instead weight by deposit share. We prefer the 2008 application share because several CCAR-tested lenders like Ally conduct their mortgage business through nondepository subsidiaries.

We also employ a second instrument which builds on how the Big-4 banks are the only major mortgage lenders officially designated as systemically important financial institutions (SIFIs) over 2010–2014. Importantly for the purposes of identification, the SIFI designation is not based on an institution's behavior in mortgage markets. Stein (2014) describes how the

negatively correlated with growth in denials.

[10] We thank an anonymous referee for pointing out this example.

Dodd-Frank Act subjected the Big-4 banks to heightened oversight and higher liquidity and capital requirements. As we show formally in the panel analysis of Section 3, these lenders have tightened credit significantly relative to other lenders since 2010, and thus differential exposure to these lenders constitutes a credit supply shock. To measure exposure to the Big-4, we compute the Big-4's branch deposit share in an MSA in 2008, using the FDIC's Summary of Deposits. The results are the same if we instead weight by the number of branches.

Our key identification assumption is that, once we control for a broad array of factors and fixed effects, exposure to the Big-4 banks and stress tested lenders is uncorrelated with other drivers of rent growth over 2010–2014. We devote Section 3 to discuss multiple tests that all suggest that the instruments satisfy this exogeneity assumption.

The second assumption is that both instruments are relevant, that is, correlated with denial rates. Figure 2 provides visual support and shows strong correlation between the instruments and average denial rates over 2010–2014. Moreover, in all our results we test for and reject underidentification.

2.4 Baseline results

Table 2 contains the estimates of the baseline specification (1). In the first column we estimate (1) using OLS, finding a positive but statistically insignificant point estimate. However, after accounting for the endogeneity of denial rates in the second column of the table, the instrumental variables estimate suggests an economically and statistically significant impact of mortgage supply on rent growth over 2010–2014. A 1-percentage-point increase in denial rates increased rent growth by 1.3 percentage points.

To put the results from Table 2 into perspective, it is useful to notice that the average MSA's denial rate fell by 1.6 percentage points over 2010–2014 relative to its 2008 level. However, denial rates actually rose in 31% of MSAs in our sample. If instead denial rates in these MSAs had fallen with the national average, then, based on our estimate from Table 2, rents would have grown at least 2.1 percentage points less in these MSAs (1.6×1.3). The cross-sectional standard deviation in 2010–2014 rent growth was 3 percentage points. Thus, elevated post-crisis credit stringency explains a meaningful amount of cross-MSA variation in recent rent behavior.

3 Validity of the Instruments

This section is devoted to assessing the instruments' validity and in particular the exclusion restriction. To address the exclusion restriction, we perform the following exercises: (1) parallel trends analysis; (2) inspection of correlation with standard drivers of housing rents; (3) extensive local business-cycle controls; (4) overidentification tests and sensitivity to alternative instruments; (5) placebo tests; and (6) robustness of the results using county-level data and geographic subsamples. Moreover, we check that the results are robust to functional form using a panel approach popular in the literature since Favara and Imbs (2015).

3.1 Parallel trends

Figure 3 plots annual rent growth for MSAs ranking in the top and bottom 25% of exposure to each instrument. The year 2010 is the critical year when the Financial Stability Oversight Council was created and CCAR stress tests were announced as part of Dodd-Frank. For both instruments, we notice a substantial divergence in post-2010 rent growth between MSAs with high versus low exposure. However, prior to the shock, there are parallel dynamics between treated and control groups. That is, the instruments appear to only be driving rents in the post-crisis period.

3.2 Correlation with standard drivers of housing rents

As an alternative test, in Table 3 we regress each of our instruments on a variety of precrisis trends and MSA controls. To better gauge the magnitude of these partial correlations, the table normalizes all variables to have a variance of one. This allows us to assess both the magnitude and statistical significance of any correlations.[11]

While it is impossible to directly test the exclusion restriction, Table 3 suggests that the instruments satisfy it as there is no relevant correlation between common drivers of rent growth and exposure to either stress tested lenders or the Big-4 banks. Moreover, as Mian, Rao, and Sufi (2013) point out, fixed differences, like in the level of house prices or population, will be differenced out in our baseline specification. Most importantly, all our regressions include an

[11] In Table 3 we use homeownership data from the decennial census because it covers a larger cross-section of MSAs than our core homeownership data from the Housing Vacancy Survey (HVS), which is available quarterly but only for 60 MSAs in our sample. We also measure house prices using starter homes, which are likely the relevant prices for constrained buyers. In the Online Appendix Table A3, we produce an analogous table with data from the HVS, and the conclusions are the same as we discuss here.

expansive set of controls.

3.3 Business-cycle effects

To rule out the possibility that local business-cycles drive the results or that the results are a side effect of the small business loan contractions studied by Chen, Hanson, and Stein (2017), we reestimate our baseline instrumental variables specification from Table 2 in Table 4 after controlling for a wide range of local business-cycle variables.

In particular, Table 4 controls for five measures of contemporaneous economic activity in an MSA: average annual growth in unemployment, labor force participation, log number of establishments, log real gross domestic product (GDP) per capita, and log median hourly wage from 2010 to 2014. Moreover, we control for a manufacturing labor demand shock following Adelino, Ma, and Robinson (2017).[12]

Regardless of which measure we use, Table 4 shows that the point estimate for the effect of mortgage denials on rent growth is consistently between 1.1 and 1.3 and statistically significant. Moreover, the various business-cycle measures all enter with the correct sign. This suggests that regional business cycles and mortgage supply are both important for rent growth, but they operate independently.

3.4 Overidentification tests and alternative instruments

We now exploit overidentification to assess the validity of the instrument set. First, the highly insignificant J-statistic in Table 2 shows that we cannot reject the null hypothesis of the instruments' exogeneity. As an additional test, Table 5 checks the robustness of our results when using the D'Acunto and Rossi (2017) instrument: the 2007 origination share of the top 20 mortgage lenders that year.

The first column of Table 5 shows that the estimated effect of denial rates is 1.3 when using the top 20 instrument instead of Big-4 share. This result is almost the same as that in Table 2 and is statistically significant. Moreover, the overidentification test continues to support the validity of all the instruments.

To second and third columns of Table 5 use as alternative instruments the 2008 mortgage application share of lenders ranked between 20 and 50 and between 50 and 150 that year,

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

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

3.5 Placebo test

In Figure 4 we visually inspect the impact of the instruments on annualized rent growth and average denial rates over 2010–2014. The scatterplot controls for the same variables as regression (1). It is binned so that each point represents around 12 MSAs. The top panel of the figure demonstrates strong positive correlation between each instrument and rent growth over 2010–2014. This role is absent in the pre-2008 placebo version of this figure that is in the bottom panel of Figure 4. This evidence suggests that the instruments are not contaminated by precrisis rent growth.

To rigorously assess the intuition from Figure 4, we conduct various placebo tests over the 2002–2006, 2001–2005, and 2000–2004 periods. We ask if, when using a specification analogous to (1), the credit supply shocks can explain rent growth over any of these periods. We should expect no effect of our instruments on precrisis rent growth because the instruments correspond to specific shocks to U.S. mortgage lenders over 2010–2014, unrelated to other drivers of housing rents. The placebo point estimates in Table 6 are insignificant across periods, and with the opposite sign relative to Table 3. This result suggests that the instruments are truly capturing post-crisis credit supply shocks.

3.6 Sample sensitivity

To address sample sensitivity, we do two things in the Online Appendix Table A2: first we reestimate (1) on the subsample of MSAs in states far from where the Big-4 have their headquarters, and then we reestimate (1) using county-level data. The first column reports quantitatively similar results when dropping MSAs close to a Big-4 headquarters. This makes it unlikely that the results are due to idiosyncratic location decisions by the major lenders. The second column shows a positive and significant point estimate when reperforming our analysis at the county level. However, the magnitude of the point estimate is smaller at 0.5, consistent with it being easier to substitute across lenders in different counties than in different MSAs.

3.7 Panel analysis

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

$$\Delta \log(\text{Rent}_{m,t}) = \beta \times \Delta\text{Denied}_{m,t} + \gamma X_{m,t} + \alpha_m + \alpha_t + u_{m,t}, \tag{2}$$

where $\Delta\text{Denied}_{m,t}$ denotes the one year change in the denial rate in MSA m between year $t-1$ and year t. This methodology allows us to hold fixed unobserved drivers of average rent growth over the sample period. However, it necessitates the use of credit supply instruments which vary over time. We study several candidates: (1) a well-known instrument, the conforming loan limit instrument popularized by Loutskina and Strahan (2015), which we use to study the precrisis period and then modify for use after the 2008 Economic Stimulus Act; (2) the panel versions of the cross-sectional instruments studied in Section 2 that we create using the methodology of Khwaja and Mian (2008); and (3) in the spirit of Greenstone, Mas and Nguyen (2015) an instrument that is agnostic about which lenders are subject to shocks.[13]

3.7.1 Credit and rents after the crisis: Panel analysis

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

Figure A4 shows that the Big-4 banks tightened standards after the implementation of Dodd-Frank and other major regulations in 2011.[14] Interestingly, we see little significant difference between Big-4 and non-Big-4 lenders over the 2000-2003 period. This result is consistent with Big-4 exposure representing a post-crisis credit supply shock.[15]

Figure A5 shows that denial propensities by stress tested lenders remained elevated throughout the post-crisis period, and they increased in 2012. This was the first year that CCAR results were made public. The placebo precrisis period in the bottom panel shows little significant difference between the two groups of lenders, or significant difference relative to the reference

[13] The construction of these instruments is described in the Online Appendix.

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

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

lender-year (nontested lenders in 2004). This is again consistent with exposure to stress testing representing an exclusively post-crisis shock.

Table A4 contains the baseline panel results. Like in the cross-sectional analysis, we begin by estimating (2) using OLS. The result suggests no significant impact of credit supply of rents. However, after correcting for endogeneity with the instruments, the second column obtains a point estimate of 2.1 for the parameter of interest, which is very close to the estimate from Section 2.4. Furthermore, we reject the null hypothesis that the model is underidentified, and the highly insignificant J-statistic provides evidence of the instruments' exogeneity.

Table A5 performs a panel placebo test.[16] The Big-4 and stress test panel instruments should fail to explain rents during the precrisis placebo window. Indeed Table A5 shows no economic or statistical significance. Moreover, the point estimates are negative. This finding suggests that the instruments capture credit supply shocks unique to the post-crisis period.

3.7.2 Credit and rents before the crisis: the conforming loan limit instrument

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

The results in the Online Appendix Table A6 suggest a positive and statistically significant effect of credit supply on rents in the precrisis period. However, the magnitude is much smaller than we found over the post-crisis period, as it suggests a 1-percentage-point increase in denials led to a 0.07-percentage-point increase in rent growth.[17] Most importantly for this paper, Table A6 suggests that credit supply can affect rents in any period. We interpret this result, together with the result that none of the instruments used in Section 2 can explain housing rents in the precrisis period, as further evidence that those instruments just capture post-crisis credit

[16] The Online Appendix provides other validity tests, including tests of instrument sensitivity.

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

supply shocks.

4 Channels

The previous two sections robustly documented that tight credit supply has increased rent growth. To assess whether tenure choice is indeed the relevant mechanism, we now test five additional implications of the theory discussed in Section 1. First, mortgage denials should lead to lower price-to-rent ratios and have a nonpositive effect on house price growth.[18] Second, as rents rise, "buy to let" investors convert owner occupied units to rentals. Third, the homeownership rate must fall due to the combined effects of tight credit and expanding rental supply. Fourth, rental demand stimulates construction of multifamily units. Fifth, the credit-to-rent channel should be stronger where it is more difficult to substitute across lenders, for example because of different regulatory requirements across mortgage markets.

4.1 House prices

Our theory implies that, at least in the short run, price-to-rent ratios should fall and effects on house prices should be zero or possibly negative. To test this hypothesis, the first column of Table 7 reestimates (1) replacing the outcome variable with the average growth in the price-to-rent ratio over 2010–2014. The point estimate is negative and statistically significant, consistent with the theory.

In columns two and three of Table 7, we study house price growth directly. The second column restricts attention to starter homes, since these houses are more likely to be priced by households denied mortgage credit and are thus more likely to have a negative price response.[19] In the third column we study all homes. In neither column do we find a significant effect of mortgage denials on house prices, and the point estimate for starter homes is indeed substantially more negative in magnitude than the estimate obtained using all homes.

Figure 5 provides complementary visual evidence of the relationship between rent and price growth for starter homes. For MSAs with high exposure to the credit supply instruments, defined as an above-median value for both instruments, there is a negative relationship between rent and price growth. By contrast, the relationship between rents and prices is positive for

[18] We are very grateful to the editor for this suggestion.

[19] The price of starter homes is measured using Zillow's Bottom Tier Index, which tracks the median home value among houses in the bottom third of the market.

MSAs with low exposure. Consistent with Table 7, the credit supply shock led to a substitution between rental and owner occupied properties for households denied a mortgage.

The Online Appendix Table A8 corroborates the previous finding by estimating an OLS specification where the key independent variables are the 2008 mortgage application share of stress tested lenders, our preferred credit supply instrument, and its interaction with an indicator of whether the MSA had an above-median share of mortgage applications from blacks or Hispanics in 2009.[20] The outcome variable is the average change in house prices over 2010–2014. The idea is that minority borrowers are more likely on the margin of homeownership. Thus markets with a high minority share may even see a negative relationship between the credit supply shock and house prices as these borrowers substitute between rental and owner occupied properties. This is indeed what we find, with a negative and significant point estimate on the interaction term.

These results are not only supportive of our theory, but they also contribute to rule out the possibility that unobserved housing demand shocks violate the exclusion restriction. If that were the case and the MSAs more exposed to our credit instruments also had positive demand shocks, then we should observe not only a positive and significant effect between instrumented denials and rents but also between instrumented denials and prices. This is because demand shocks generate comovement between prices and rents as shown in Gete and Reher (2016) and Gete and Zecchetto (2017) among others. Table 7 shows no evidence supporting that argument. Thus, the dynamics of prices reported in Table 7 strongly suggest that our results in Table 2 are due to a credit supply contraction operating through a tenure choice channel.

4.2 Tenure conversion

The decoupling of rent and price growth from Table 7 suggests a profitable opportunity for "buy to let" investors. One would therefore expect to see increased conversion of owner occupied properties to rental units. Using data from the American Housing Survey (AHS), which tracks the same housing unit over time, we compute the fraction of rental units in an

[20]Following Angrist and Pischke (2009), we avoid estimating instrumental variable models with interactions and instead Table A8 estimates:

$$\text{Avg. house price growth}_{m,10\text{-}14} = \beta_1 \times \text{Tested}_{m,08} + \beta_2 \times \text{Tested}_{m,08} \times \text{High minority}_{m,08} + \gamma X_m + u_m, \quad (3)$$

where Avg. house price growth$_{m,10\text{-}14}$ is the average annual change in the log of the Zillow Home Value Index over 2010–2014, Tested$_{m,08}$ is the 2008 mortgage application share of lenders that underwent a stress test between 2011 and 2015, and High minority$_{m,08}$ indicates whether the MSA had an above-median share of mortgage applications from blacks or Hispanics in 2008. Like in Table 2, we control for the 2009 value of the outcome variable and the other controls of Table 2.

MSA which were owner occupied in the previous period.[21] Then, we test the "buy to let" channel by reestimating (1) replacing the outcome variable with the MSA's tenure conversion rate over 2011-2013.[22]

Table 8 contains the results of this exercise. In the first column, we find a positive and statistically significant effect of mortgage denial rates on tenure conversion. This is consistent with investors responding to a credit-induced demand for rentals by purchasing owner occupied units and subsequently renting them out.

In the second column of Table 8, we look for a longer-term effect by replacing the outcome variable with the tenure conversion rate over 2003-2013. The highly insignificant point estimate suggests that the post-crisis credit supply shock did not raise tenure conversion rates relative to precrisis levels. This finding relates to the welfare question of whether the shock led to abnormally tight standards and high rental demand, or whether it helped correct abnormally loose standards and low rental demand during the boom period. For example, the Online Appendix Figure A3 shows how the spike in mortgage denials in 2010 did not raise the denial rate substantially above preboom levels. We leave welfare questions for future research.

4.3 Homeownership

A key implication of our theory is that the homeownership rate falls as households cannot obtain mortgage credit and the stock of rental units grows. Using data on MSA-level homeownership rates from the Housing Vacancy Survey, we replace the outcome in (1) with an MSA's average growth in homeownership from 2010 to 2014. The results in Table 9 indicate that a 1-percentage-point increase in mortgage denials over 2010–2014 reduced homeownership growth by 0.7 percentage points. This effect is significant with a p-value of .05 despite the relatively small sample size. This again provides evidence that tight mortgage supply raised rents through households' tenure choice.

4.4 Multifamily construction

We now ask whether the supply response documented in Section 4.2 was also accompanied by construction of new multifamily units. Specifically, we look at the growth in permits for the

[21] We exclude vacant units in our analysis.

[22] We use 2011–2013 because the AHS is only available in odd-numbered years.

construction of multifamily units.[23] We replace the outcome variable in (1) with the average growth in log multifamily permits over 2011–2014, where we offset the outcome window by one year to account for a lag in the supply-side response because of lengthy permitting procedures (Gyourko, Saiz, and Summers 2008). Table A9 in the Online Appendix suggests that the recent rent growth we have documented may dissipate as rental supply expands.

4.5 Lending frictions

Implicit in our previous analysis is the notion that borrowers cannot easily substitute between lenders of different stringency. To measure the ease of substitutability, we utilize geographic variation in the regulation of mortgage brokers. According to Backley et al. (2006), states with additional licensing requirements for mortgage brokers have less competition, and likely stickier broker-lender relationships. That is, brokers may keep referring customers to the same lenders even if their standards are higher.[24]

We test the strength of these frictions with an OLS regression in which the key independent variables are the stress test credit supply instrument and its interaction with an indicator of whether the MSA is in a state requiring such licensing.[25] The Online Appendix Table A10 has the results. Notably, the estimated interaction term is positive and significant. This suggests a role for lending frictions in strengthening the credit-to-rent mechanism on which our theory is based.

5 Conclusions

In this paper, we showed that tighter mortgage credit can explain a significant component of rent growth following the 2008 financial crisis. Our empirical strategy used variation among MSAs in exposure to lenders more subject to regulatory costs and stress testing. We controlled for an array of local shocks and performed a battery of tests to check the validity of all in-

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

[24] Eighteen states listed in the appendix impose the additional requirement that individual mortgage brokers be licensed.

[25] Specifically, following Angrist and Pischke (2009), the regression equation is

$$\text{Avg. rent growth}_{m,\text{10-14}} = \beta_1 \times \text{Tested}_{m,08} + \beta_2 \times \text{Tested}_{m,08} \times \text{License}_m + \gamma X_m + u_m, \tag{4}$$

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

struments. The credit supply shocks used in our identification cannot explain precrisis housing rents and are unrelated to standard drivers of housing rents documented in the literature.

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

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

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

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Appendix: Data Sources

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

A.1 Housing Rents and Prices

Our rent data cover 302 MSAs from 2007 through 2014. Data for rents and prices are from Zillow. To measure rents, we use the Quarterly Historic Metro Zillow Rent Index (ZRI). The ZRI measures the median monthly rent for each MSA and has units of nominal dollars per month. Zillow imputes this rent based on a proprietary machine learning model taking into account the specific characteristics of each home and recent rent listings for homes with similar characteristics. Importantly, the ZRI does not impute a property's rent from its price. The median rent is computed across all homes in an MSA, not only those that are currently for rent. Thus, unlike pure repeat-listing indices, the ZRI is not biased by the current composition of for-rent properties. To measure house prices, we use the Quarterly Historic Metro Zillow Home Value Index (ZHVI). The ZHVI is computed using a methodology analogous to that of the ZRI. Although the ZRI and the ZHVI are available quarterly, we only retain the values corresponding to the fourth quarter of each year because our mortgage data are at the yearly frequency. To measure the price of starter homes, we use the Zillow's Bottom Tier Index, which measures the median house price among homes in the bottom third of the market.

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

A.2 Mortgage Data

Data on mortgage credit come from the Home Mortgage Disclosure Act (HMDA). The frequency of the data is yearly. HMDA data contain application-level information on the requested loan size, loan purpose, property type, and application status. We observe the self-reported income, race, and gender of the borrower, as well as an identifier of the lender receiving the application. Since our focus is on how credit affects rents through housing tenure choice, we only retain mortgage applications for the purchase of a owner-occupied home for 1 to 4 families. In terms of HMDA variables, we retain applications satisfying the following conditions: occupancy = 1 (owner occupied), property type = 1 (1- to- 4 families), loan purpose = 1 (for-purchase), and action taken $\neq$ 6 (loan not purchased by institution). To maximize data quality, we additionally require that applications were not flagged for data quality concerns (edit status = "NA") and have a nonempty MSA code. We identify denied and originated loans as those with action taken = 3 and action taken = 1, respectively. FHA loans are those with loan type = 2.

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

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

We merge the HMDA's application-level data by lender and year with the HMDA reporter panel. The reporter panel contains each lender's name, total assets, and top holding company. Within each year, we classify a lender as belonging to the Big-4 if its top holding company is one of the Big-4 banks. To account for slight changes in institutional names over time, we identify the Big-4 banks as those whose names possess the strings "WELLS FARGO," "BANK OF AMERICA," "CITIG," or "JP." Using our classification scheme, if a Big-4 bank acquires another institution in, say, 2010, then that institution would be classified as a non-Big-4 lender in 2009 but as belonging to the Big-4 in 2010. We computed the top 20 share using the shares of mortgages originated in 2007, like D'Acunto and Rossi (2017).

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

Financial Corp., Santander Holdings USA Inc., State Street Corp., Suntrust Banks Inc., TD Group US Holdings LLC, MUFG Americas Holding Corp., US Bancorp, Wells Fargo & Co., and Zions Corp.

A.3 Deposit, homeownership, and vacancy data

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

Our data on licensing rules for mortgage brokers come from Backley et al. (2006), who, as of 2006, reports that 48 states require mortgage brokerage firms to carry a license, whereas 18 states impose the additional requirement that individual brokers also be licensed. These 18 states are Arkansas, California, Florida, Hawaii, Idaho, Louisiana, Maryland, Montana, Nevada, North Carolina, Ohio, Oklahoma, South Carolina, Texas, Utah, Washington, West Virginia and Wisconsin.

Homeownership data come from the U.S. Census Bureau's Housing Vacancy Survey (HVS). The HVS is a supplement of the Current Population Survey (CPS) to provide current information on the rental and homeowner vacancy rates. These data are used extensively by public and private sector organizations. They cover 60 MSAs over our sample period. We only retain the fourth-quarter value for homeownership rates, to match the annual frequency of our mortgage data. In Table 3 we approximate the 2009 value using the 2010 Census value, which covers more MSAs but it is decennial.

A.4 Other Variables

We also rely on the following data sources:

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

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

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

Figures and Tables

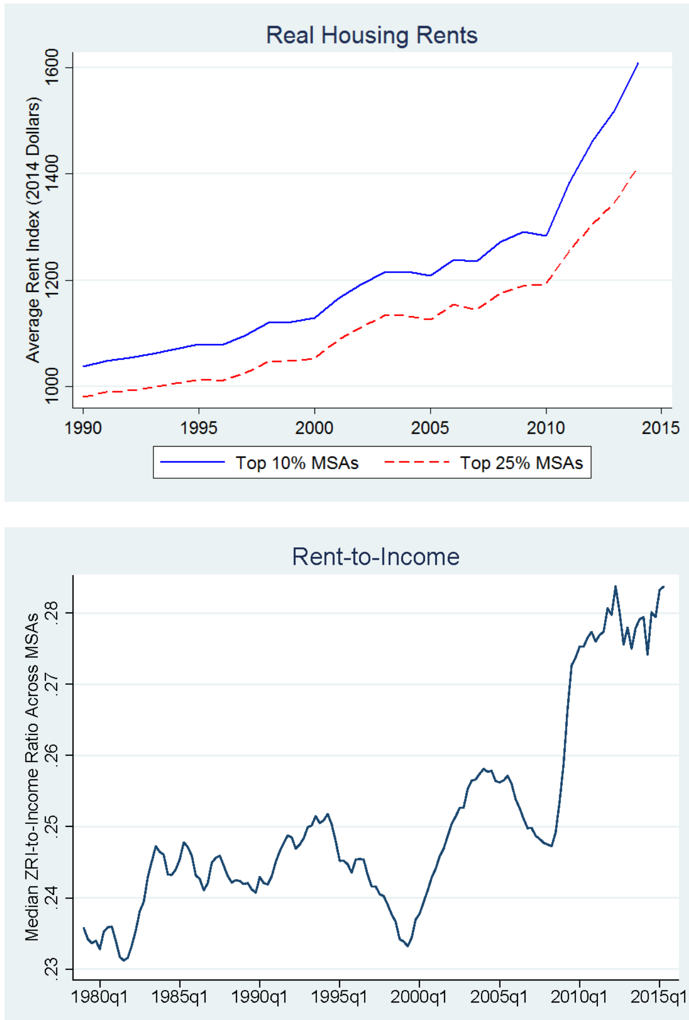


Figure 1. Dynamics of real housing rents and tent-to-income. The top panel plots real housing rents over the 1991-2014 period in 2014 dollars for MSAs ranking in the top 10% and top 25% of 2008–2014 rent growth, respectively. Nominal rents are measured using the Zillow Rent Index (ZRI), which has the interpretation of dollars per month. The translation to real rents is done using the Consumer Price Index excluding shelter. The bottom panel plots the median ratio of rent-to-income for the MSAs in our sample.

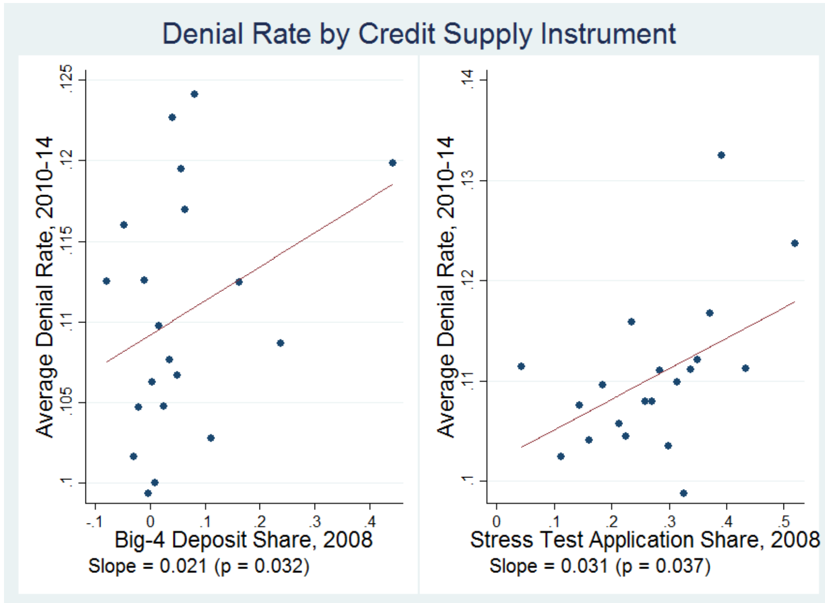


Figure 2. Denial rates and credit supply instruments. This figure plots denial rates against the cross-sectional credit supply instruments. The plot controls for the same variables as the baseline analysis in Table 2.

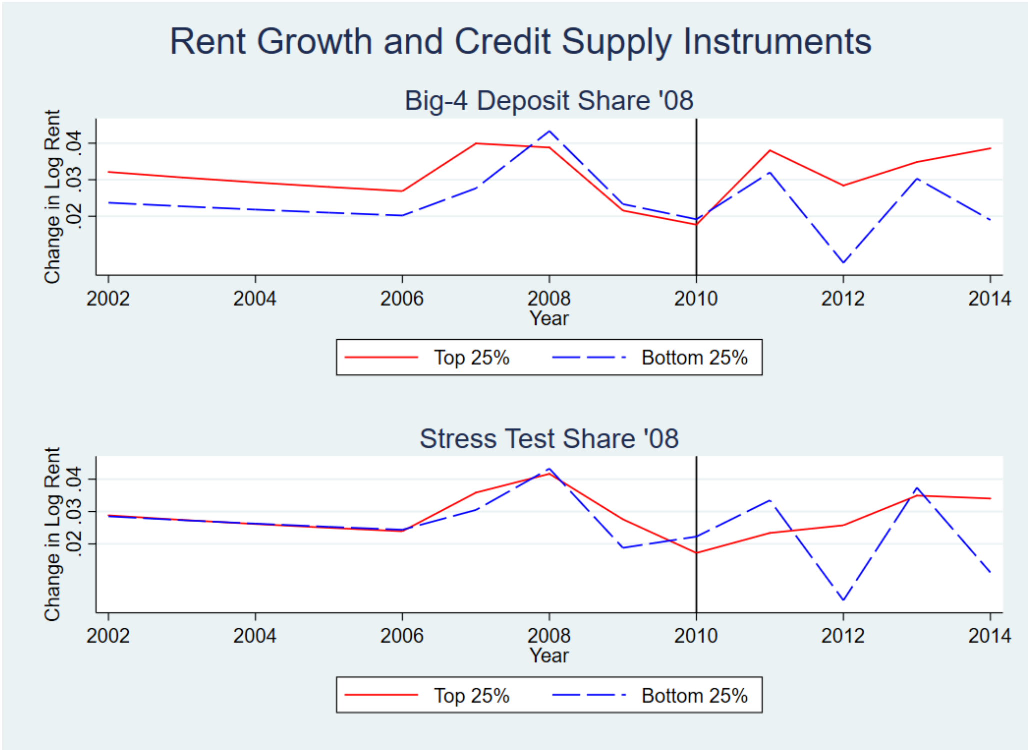


Figure 3. Credit supply instruments and rent growth. This figure plots annual change in log rent for MSAs ranking in the top and bottom 25% of exposure to each credit supply instrument: (1) the branch deposit share of the Big-4 banks in 2008; and (2) the 2008 mortgage application share of lenders that underwent a CCAR stress test between 2011 and 2015. In all plots, the red solid line represents MSAs with high (top 25%) exposure to the shock, and the blue dashed line represents MSAs with low (bottom 25%) exposure.

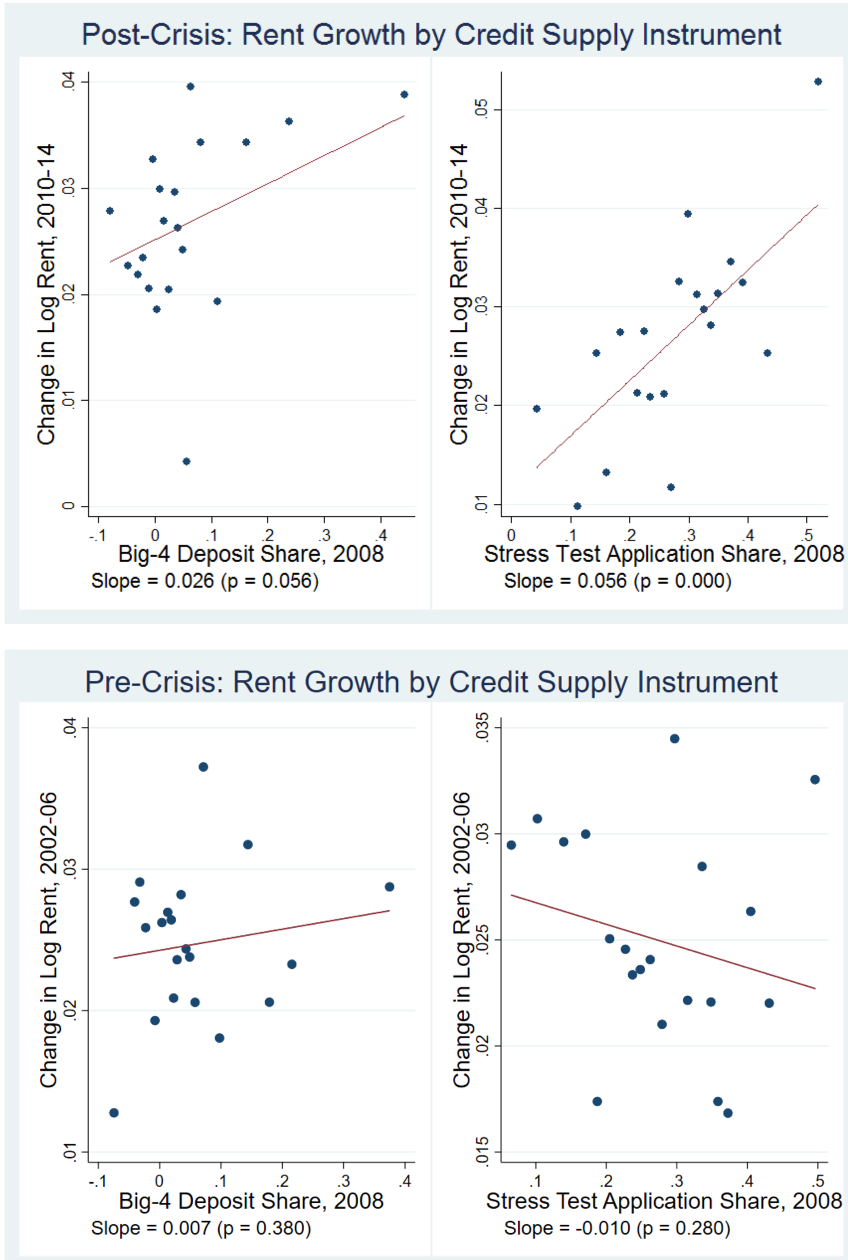


Figure 4. Pre- and post-2010 rent growth against credit supply instruments. The top panel plots 2010–2014 average annual change in log rent against the credit supply instruments: (1) the branch deposit share of the Big-4 banks in 2008; and (2) the 2008 mortgage application share of lenders that underwent a CCAR stress test between 2011 and 2015. The bottom panel plots the same variables over 2002-2006. The top panel controls are the controls used in the baseline analysis in Table 2. The bottom controls are the controls used in the placebo analysis in Table 6.

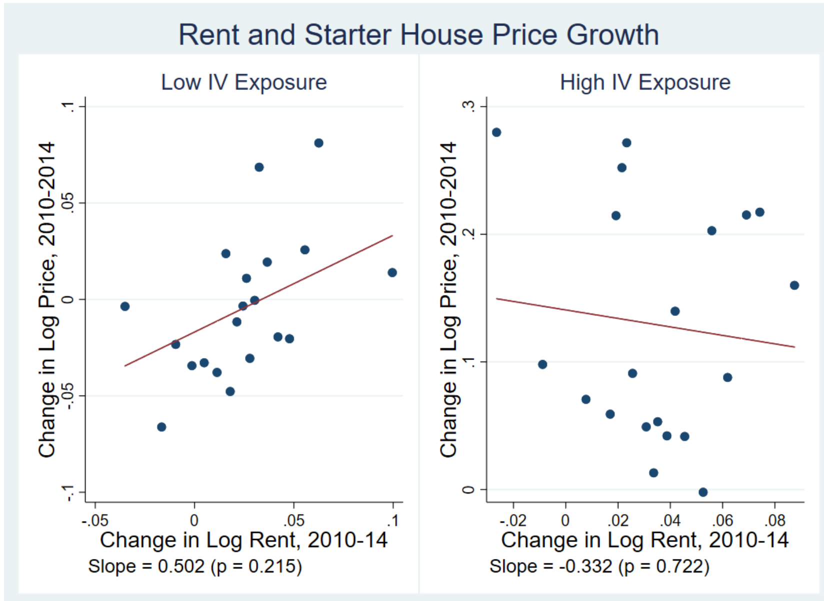


Figure 5. Rent and starter house price growth by exposure to credit supply instruments. This figure plots the average change in log rents and log price of starter homes over 2010–2014. Each observation is an MSA. The left panel is based on MSAs with a below-median deposit share of the Big-4 banks and a below-median mortgage application share to stress tested lenders in 2008, and analogously the right panel has MSAs with an above-median share for both lender groups. Rents are measured using the ZRI, and starter house prices are measured using Zillow's Bottom Tier House Price Index.

Table 1: Summary statistics

	Obs	Mean	SD	Min	Max
Avg. rent growth$_{m,10-14}$	302	2.641	3.004	-5.637	19.057
Avg. denial rate$_{m,10-14}$	303	11.147	3.064	4.236	30.211
Big-4 deposit share$_{m,08}$	303	5.048	11.945	0	79.931
CCAR tested share$_{m,08}$	303	27.119	12.832	.301	64.338
Avg. house price growth$_{m,10-14}$	263	1.423	2.887	-5.363	11.391
Avg. starter house price growth$_{m,10-14}$	250	2.621	15.197	-27.786	51.94
Price-rent ratio growth$_{m,t}$	264	-60.392	189.174	-1291.651	593.778
Tenure conversion rate$_{m,11-13}$	96	4.316	4.349	0	24.041
Avg. homeownership growth$_{m,10-14}$	64	-.731	1.253	-3.85	1.75
Avg. multifamily permits growth$_{m,11-14}$	280	11.926	50.525	-274.084	317.805
Avg. unemployment growth$_{m,10-14}$	298	-.82	.554	-3.05	.825
Avg. labor force part. growth$_{m,10-14}$	298	-.316	.517	-2.275	1.325
Avg. establishment growth$_{m,10-14}$	298	-.372	.722	-1.578	2.852
Avg. real GDP growth$_{m,10-14}$	298	.415	1.552	-6.378	4.738
Avg. wage growth$_{m,10-14}$	205	3.073	12.674	-41.114	58.629
Avg. rent growth$_{m,00-08}$	303	3.218	1.824	-3.344	8.2
Avg. house price growth$_{m,00-08}$	264	2.688	1.435	-1.866	6.182
Avg. population growth$_{m,00-08}$	302	11.096	10.807	-2.188	47.806
Avg. income growth$_{m,00-08}$	302	5.679	1.199	2.428	9.855
Avg. unemployment growth$_{m,00-08}$	296	.387	.243	-.257	1.257
Avg. age growth$_{m,00-08}$	296	.242	.661	-3.559	1.683
Financial services share$_{m,08}$	299	5.847	1.825	2.001	17.265

This table presents summary statistics of the key variables in our analysis. All variables are at the MSA level. Avg. rent growth denotes average annual change in log rent. Avg. denial rate denotes the average denial rate among mortgage applications for the purchase of single-family homes in the MSA, based on HMDA data. Big-4 deposit share$_{m,08}$ and CCAR tested share$_{m,08}$ are, respectively the branch deposit share of the Big-4 banks in 2008 and the 2008 mortgage application share of lenders that underwent a stress test between 2011 and 2015. Rent and House price denote the Zillow Rent and Home value indices, respectively. Starter house prices are based on Zillow's bottom tier index. Tenure conversion rate denotes the fraction of rental units in an MSA that were converted from owner occupied units over the indicated period. Labor force part. denotes the labor force participation rate. Establishment refers to the number of establishments. Real GDP is in per capita terms. Wages are the median hourly wage in the MSA. Age and income refer to the median in the MSA. Multifamily permits denotes permits for the construction of multifamily units. Homeownership refers to the homeownership rate in the MSA. Financial services share is the fraction of workers in financial services. All variables are in units of percentage points, up to a log approximation. Full details on our data sources and cleaning procedures are in the appendix.

Table 2: Rent Growth and Credit Supply: Baseline Specification

Outcome:	Avg. rent growth$_{m,10-14}$	
Avg. denial rate$_{m,10-14}$	0.105	1.309
	(0.193)	(0.018)
Estimation	OLS	IV
MSA controls	Yes	Yes
State fixed effects	Yes	Yes
Underidentification test (p-value)		0.017
J-statistic (p-value)		0.652
Number of observations	257	257

p-values are in parentheses. The instruments for Avg. denial rate are: (1) the branch deposit share of the Big-4 banks in 2008; and (2) the 2008 mortgage application share of lenders that underwent a stress test between 2011 and 2015. MSA controls are the 2009 log median income, log median rent, log population, log median inhabitant age, unemployment rate, and the 2000–2008 average annual change in log median income, log median rent, log median house price, log population, log median inhabitant age, and unemployment rate. The underidentification test is that of Kleibergen and Paap (2006). Each observation is an MSA. Standard errors are heteroscedasticity robust.

Table 3: Credit Supply Instruments and Drivers of Housing Rents

Outcome:	$\text{Tested}_{m,08}$	$\text{Big-4}_{m,08}$
Avg. rent $\text{growth}_{m,00-08}$	-0.116	-0.032
	(0.221)	(0.784)
$\log(\text{rent}_{m,09})$	-0.048	-0.200
	(0.550)	(0.205)
$\log(\text{house price}_{m,09})$	0.304	0.178
	(0.010)	(0.233)
$\log(\text{population}_{m,09})$	-0.009	0.228
	(0.899)	(0.024)
$\log(\text{income}_{m,09})$	0.141	0.036
	(0.193)	(0.826)
Avg. unemp. $\text{growth}_{m,10-14}$	-0.084	0.063
	(0.167)	(0.495)
Avg. price $\text{growth}_{m,10-14}$	0.064	-0.135
	(0.467)	(0.226)
Financial services $\text{share}_{m,08}$	0.055	0.101
	(0.364)	(0.484)
Homeownership $\text{rate}_{m,09}$	0.064	-0.020
	(0.389)	(0.851)
State fixed effects	Yes	Yes
R-squared	0.701	0.416
Number of observations	220	220

p-values are in parentheses. All variables are normalized to have a standard deviation of 1. The outcome in each column is one of our credit supply instruments: (1) the 2008 mortgage application share of lenders that underwent a stress test between 2011 and 2015; and (2) the branch deposit share of the Big-4 banks in 2008. House prices for starter homes are based on Zillow's bottom tier price index. Homeownership rates are from the 2010 Census. Each observation is an MSA. Standard errors are heteroscedasticity robust.

Table 4: Robustness: Business Cycle Effects

Outcome:	Avg. rent growth$_{m,10-14}$						
Avg. denial rate$_{m,10-14}$	1.295	1.166	1.140	1.296	1.314	1.323	1.179
	(0.017)	(0.017)	(0.015)	(0.019)	(0.017)	(0.009)	(0.004)
Avg. unemp. growth$_{m,10-14}$	-0.996						-1.039
	(0.118)						(0.130)
Avg. LFP growth$_{m,10-14}$		0.824					1.159
		(0.086)					(0.059)
Avg. estab. growth$_{m,10-14}$			2.582				3.181
			(0.000)				(0.000)
Avg. real GDP growth$_{m,10-14}$				0.111			-0.191
				(0.536)			(0.385)
Manufacturing shock$_{m,10-14}$					0.284		0.246
					(0.514)		(0.582)
Avg. wage growth$_{m,10-14}$						-0.000	-0.003
						(0.992)	(0.913)
Estimation	IV	IV	IV	IV	IV	IV	IV
MSA controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Underidentification test (p-value)	0.015	0.010	0.015	0.017	0.017	0.009	0.004
J-statistic (p-value)	0.580	0.535	0.698	0.661	0.666	0.919	0.605
Number of observations	257	257	257	257	257	179	179

p-values are in parentheses. Avg. unemployment growth$_{m,10-14}$, Avg. Labor force participation growth$_{m,10-14}$, Avg. establishment growth$_{m,10-14}$, Avg. real GDP growth$_{m,10-14}$ and Avg. wage growth$_{m,10-14}$ denote the average annual change in those variables in MSA m from 2010–2014. Manufacturing shock$_{m,10-14}$ is the Bartik manufacturing shock used by Adelino, Ma, and Robinson (2017), which in our setting is the 2008 employment share of each 4-digit manufacturing industry in MSA m multiplied by the average 2010–2014 national log employment growth in that industry. The instruments for Avg. denial rate and the other MSA controls are the same as those used in Table 2. The underidentification test is that of Kleibergen and Paap (2006). Each observation is an MSA. Standard errors are heteroscedasticity robust.

Table 5: Rent Growth and Credit Supply: Sensitivity to Lender Size

Outcome:	Avg. rent growth$_{m,10-14}$		
Avg. denial rate$_{m,10-14}$	1.287	-1.248	-0.498
	(0.021)	(0.492)	(0.762)
Estimation	IV	IV	IV
Instruments	Top 20, Tested	Top 20-50	Top 50-150
MSA controls	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes
Underidentification test (p-value)	0.017	0.340	0.382
J-statistic (p-value)	0.494		
Number of observations	257	257	257

p-values are in parentheses. Tested denotes the 2008 mortgage application share of lenders that underwent a stress test between 2011 and 2015. Top 20-50 and Top 50-100 denote the 2008 application share of lenders ranking between 20 and 50 and between 50 and 100 in terms of total originations that year. Top-20 is the D'Acunto and Rossi (2017) instrument, which in our setting is the 2007 origination share of the top 20 mortgage lenders that year. The remaining notation and controls are the same as those used in Table 2. The underidentification test is that of Kleibergen and Paap (2006). Each observation is an MSA. Standard errors are heteroscedasticity robust.

Table 6: Placebo: Credit Supply and Rents Before the Crisis

Outcome:	Avg. rent growth$_{m,period}$		
Avg. denial rate$_{m,period}$	-0.292	-0.230	-0.266
	(0.160)	(0.232)	(0.191)
Period	2002-2006	2000-2004	2001-2005
Estimation	IV	IV	IV
MSA controls	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes
Underidentification test (p-value)	0.021	0.085	0.030
J-statistic (p-value)	0.414	0.194	0.244
Number of observations	173	173	173

p-values are in parentheses. The outcome in each column is average rent growth over the specified period. The instruments for Avg. denial rate are the variables from Table 2. MSA controls are the 2000 log median income, log median rent, log population, log median inhabitant age, log median house price, and unemployment rate. The underidentification test is that of Kleibergen and Paap (2006). Each observation is an MSA. Standard errors are heteroscedasticity robust.

Table 7: Price-to-Rents, House Prices and Credit Supply

Outcome:	Avg. price-to-rent growth$_{m,10-14}$	Avg. price growth$_{m,10-14}$	
Avg. denial rate$_{m,10-14}$	-60.904	-1.334	0.295
	(0.028)	(0.526)	(0.414)
Home type	All	Starter	All
Estimation	IV	IV	IV
MSA controls	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes
Underidentification test (p-value)	0.017	0.131	0.041
J-statistic (p-value)	0.291	0.085	0.213
Number of observations	257	208	257

p-values are in parentheses. Price growth$_{m,10-14}$ denotes the average annual change in the log of MSA m median house price over 2010–2014, and Avg. price-to-rent growth$_{m,10-14}$ denotes the analogous change in the price-to-rent ratio. The first and third columns use all homes, based on Zillow's Home Value Index (ZHVI). The second column uses starter homes, based on Zillow's bottom tier price Index. The instruments for Avg. denial rate$_{m,10-14}$ are the variables from Table 2. MSA controls are those from Table 2 and, 2009 log house prices are in Columns 2 and 3 for the indicated home type. The underidentification test is that of Kleibergen and Paap (2006). Each observation is an MSA. Standard errors are heteroscedasticity robust.

Table 8: Tenure Conversion and Credit Supply

Outcome:	Tenure conversion rate_m	
Avg. denial $\text{rate}_{m,10-14}$	1.059	-0.069
	(0.020)	(0.949)
Conversion window	2011-2013	2003-2013
Estimation	IV	IV
MSA controls	Yes	Yes
State FE	Yes	Yes
Underidentification test (p-value)	0.050	0.050
J-statistic (p-value)	0.425	0.862
Number of observations	89	89

p-values are in parentheses. Tenure conversion rate_m denotes the fraction of rental units in MSA m that were converted from owner occupied units over the indicated conversion window. The instruments for Avg. denial $\text{rate}_{m,10-14}$ are the variables from Table 2. MSA controls are those from Table 2 and the fraction of non-vacant units in 2009 that were owner occupied. The underidentification test is that of Kleibergen and Paap (2006). Each observation is an MSA. Standard errors are heteroscedasticity robust.

Table 9: Homeownership and Credit Supply

Outcome:	Avg. homeownership growth$_{m,10-14}$
Avg. denial rate$_{m,10-14}$	-0.706
	(0.053)
Estimation	IV
MSA controls	Yes
State fixed effects	Yes
Underidentification test (p-value)	0.010
J-statistic (p-value)	0.863
Number of observations	60

p-values are in parentheses. Avg. homeownership growth$_{m,10-14}$ denotes the average annual change in the homeownership rate in MSA m over 2010–2014. The instruments and controls are the variables from Table 2 plus the homeownership rate in 2009. The underidentification test is that of Kleibergen and Paap (2006). Each observation is an MSA. Standard errors are heteroscedasticity robust.

ONLINE APPENDIX. NOT-FOR-PUBLICATION.

In this appendix we discuss in detail the panel analysis summarized in Section 3.7.

Lenders' Propensity to Deny

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

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

$$\Pr(\text{Denied}_{i,m,t,l_j} = 1) = \sum_j \Lambda_{t,l_j} + \gamma X_{i,m,t,l_j} + \alpha_{m,t} + \alpha_{m,l_j}, \qquad \text{(A1)}$$

where our focus is on the Λ_{t,l_j}, which is a vector of fixed effects for lenders of set l_j in year t.[26] The controls in X_{i,m,t,l_j} account for the characteristics of borrowers: income, requested loan-to-income, and race of borrower i applying for a loan from lender type l_j in MSA m in year t.[27] The terms $\alpha_{m,t}$ and α_{m,l_j} control for lender, time, and regional shocks. The value $\alpha_{m,t}$ is the coefficient on an indicator variable which equals 1 if the borrower applies from MSA m in year t and equals 0 otherwise. Likewise the indicator variable α_{m,l_j} equals 1 if the borrower applies from MSA m to a lender of type l_j and equals 0 otherwise.

The vector Λ_{t,l_j} captures the lender specific component of denial rates. For example, it may reflect a higher cost of funds or greater regulatory risk borne by lenders of set l_j in a given year. Importantly, Λ_{t,l_j} does not confound either borrower or regional effects, since these are already captured by X_{i,m,t,l_j} and the pair $(\alpha_{m,t}, \alpha_{m,l_j})$, respectively. To emphasize this interpretation, we refer to Λ_{t,l_j} as the propensity to deny.

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

[27] We use 21,709,935 observations to estimate (A1) over 2007-2014.

Panel Instruments

We use four instruments to conduct the panel analysis. The first two are based on the denial propensities for Big-4 and stress tested lenders. First, we proceed by estimating (A1) using the partition $L = \{\text{Big-4}, \text{NonBig-4}\}$ and construct the Big-4 shock as

$$V_{m,t} = (\Lambda_{t,\text{Big-4}} - \Lambda_{t,\text{NonBig-4}}) \times \text{Big-4 Deposit Share}_{m,08}. \tag{A2}$$

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

Second, we use the partition $L = \{\text{Tested}, \text{NotTested}\}$ to estimate (A1) and analogously define the stress test shock as

$$S_{m,t} = (\Lambda_{t,\text{Tested}} - \Lambda_{t,\text{NonTested}}) \times \text{Stress Test Share}_{m,08}. \tag{A3}$$

As in our cross-sectional analysis, we define stress-tested lenders as those which underwent a CCAR test between 2011 and 2015, and Stress Test Share$_{m,08}$ as the 2008 mortgage application share of these lenders. The interpretation of $S_{m,t}$ is similar to that of $V_{m,t}$, in that it captures the relative stringency of stress-tested lenders in a given year and an MSA's exposure to those lenders.

The third instrument does not partition the set of lenders L according to regulatory criteria. This addresses any concern that we impose the wrong prior on which lenders are subject to common credit supply shocks. In the spirit of Greenstone, Mas and Nguyen (2015) or Amiti and Weinstein (2013), we estimate a separate fixed effect $\Lambda_{t,k}$ for each lender $k \in \{1, ..., 20\}$ among the top 20 by national application share in year t, and an additional fixed effect $\Lambda_{t,21}$ for the remaining lenders, collectively.[28] We then define the credit supply shock $G_{m,t}$ as

$$G_{m,t} = \sum_{k=1}^{21} \Lambda_{t,k} \times \text{Share}_{k,m,t}, \tag{A4}$$

where Share$_{k,m,t}$ denotes the mortgage application share of lender k from MSA m in year t.[29]

[28] For computational simplicity, we estimate the denial propensity (A1) year-by-year. The reference lenders for each year are those outside the top-20, l_{21}.

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

Our fourth instrument follows Loutskina and Strahan (2015). Lenders are more willing to approve loan applications below the conforming loan limit because they come with an implicit guarantee from the Government Sponsored Enterprises. Prior to 2008, changes in these limits were determined at the national level. The 2008 Economic Stimulus Act revised this methodology so that changes in the conforming loan limit are now tied to the cost of living in a given county. To account for this, we compute national average conforming limit excluding MSA m. Then, for MSA m, we use the fraction of mortgage applications from MSA m in year $t-1$ within 5% of this national average. By excluding MSA m when computing the national average, we avoid capturing the local factors driving changes in the conforming limits, as with the instruments used by Loutskina and Strahan (2015).

We use the time-varying credit supply instruments to estimate (2). Since Figures A4 and A5 indicated that much of the temporal variation in credit tightness occurred after 2010, we begin the analysis in 2009. Our MSA-year controls in $X_{m,t}$ are the lagged first-difference in: log median household income, log median inhabitant age, log population, and the unemployment rate. We intentionally exclude lagged rent growth as a control because models with lagged dependent variables are usually misspecified (Angrist and Pischke 2009), and we cluster standard errors by MSA to allow for serial correlation throughout our sample period.[30] Finally, we follow Favara and Imbs (2015) and lag our credit supply shocks by one period.[31]

Validity of the Panel Instruments

Table A7 reestimates (2) after individually removing each one of the instruments. Regardless of which instrument we remove, the point estimates are consistently significant and between 2.0 and 2.1. Moreover, we perform the difference-in-Sargan test that the removed instrument is exogenous. The corresponding C-statistics are highly insignificant across specifications, which suggests that the instruments are valid. Taken together, our results from this section and our cross-sectional analysis suggest that a 1-percentage-point increase in denial rates has led to between a 1.3- and 2-percentage-points increase in annualized rent growth over the post-crisis period.

[30] We thank an anonymous referee who brought both points to our attention.

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

Figures and Tables for the Online Appendix

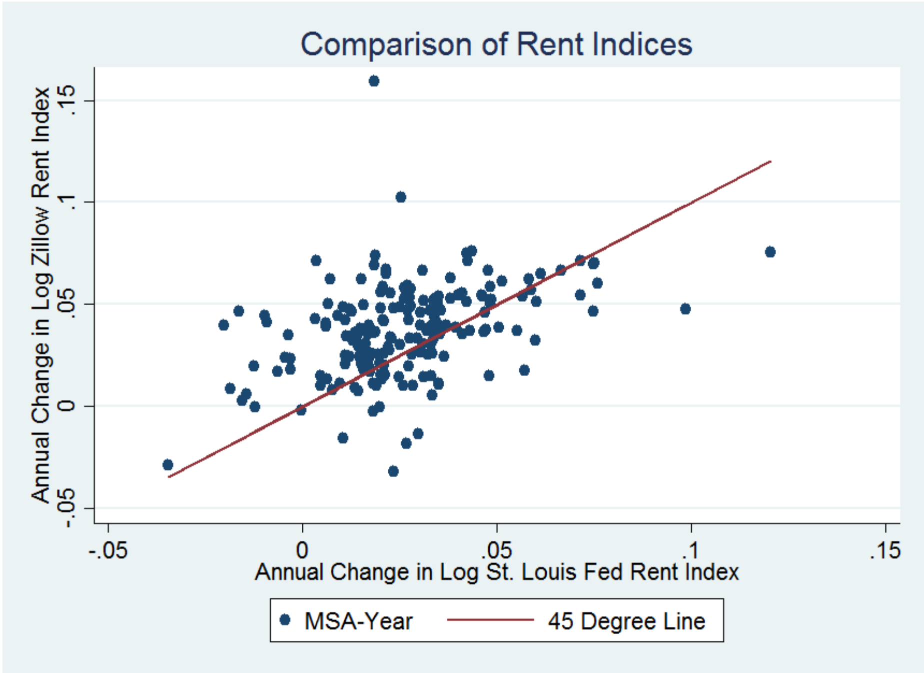


Figure A1. Comparison of rent indices. This figure plots annual change in log rents based on the Zillow Rent Index, used in this paper, and the St. Louis Fed Rent Index over 1983-2015. The St. Louis Index covers 10 MSAs (Atlanta, GA; Anchorage, AK; Phoenix-Mesa, AZ; Kansas City, MO; Pittsburgh, PA; Honolulu, HI; Minneapolis-St. Paul, MN; San Diego, CA; Tampa Bay-St. Petersburg-Clearwater, FL; St. Louis, MO). The 45-degree line is in red.

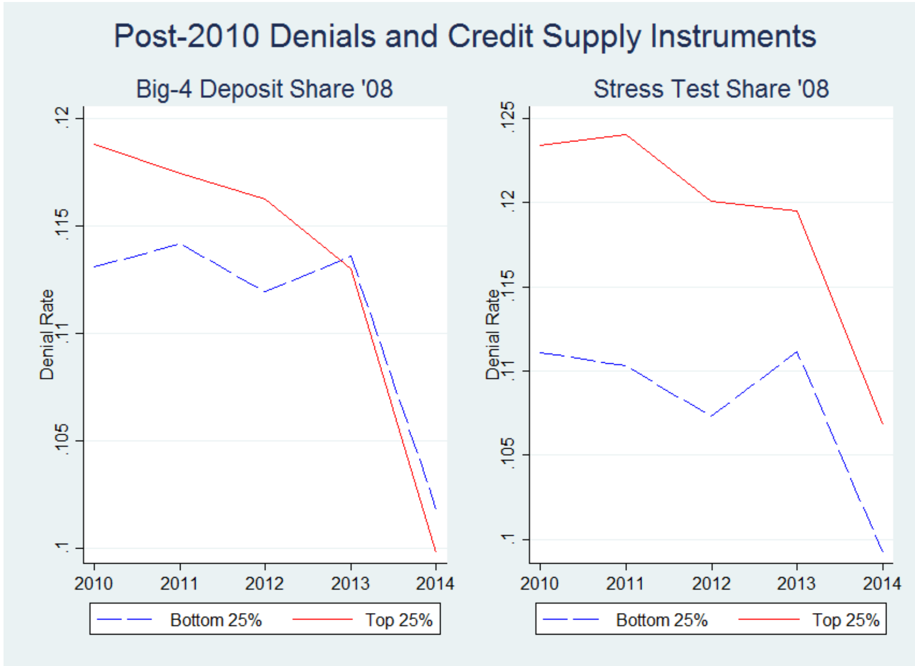


Figure A2. Post-2010 denial rates and credit supply instruments. This figure plots average denial rates based on the credit supply instruments. We first residualize denial rates based on the controls in Table 2. In all plots, the red line denotes MSAs with a high (25%) exposure to the shock, and the blue dashed line denotes MSAs with a low (bottom 25%) exposure.

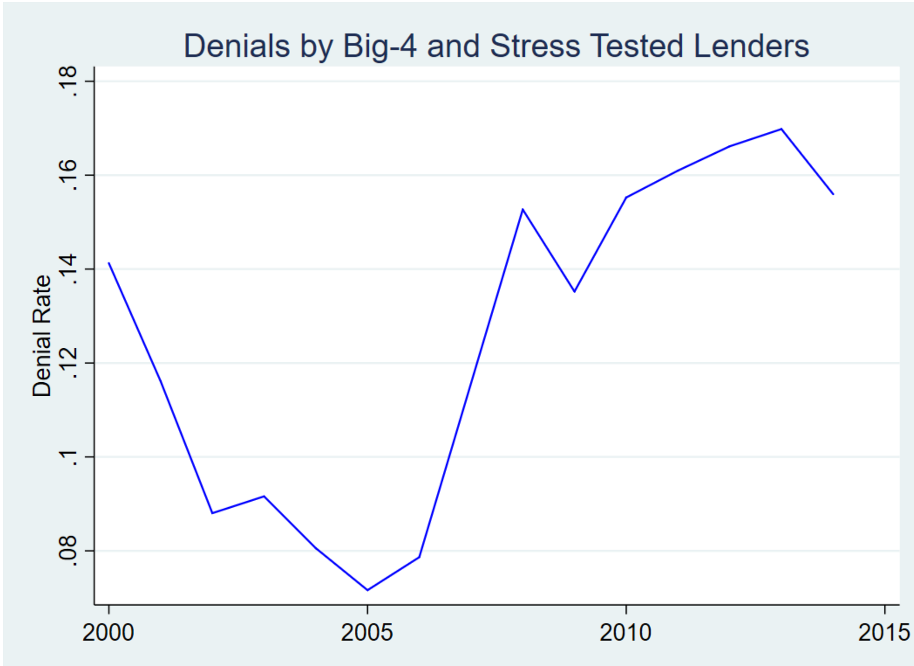


Figure A3. Denial rates for Big-4 and stress tested lenders. This figure plots the mortgage denial rate for the Big-4 banks and lenders subject to a stress test between 2011 and 2015.

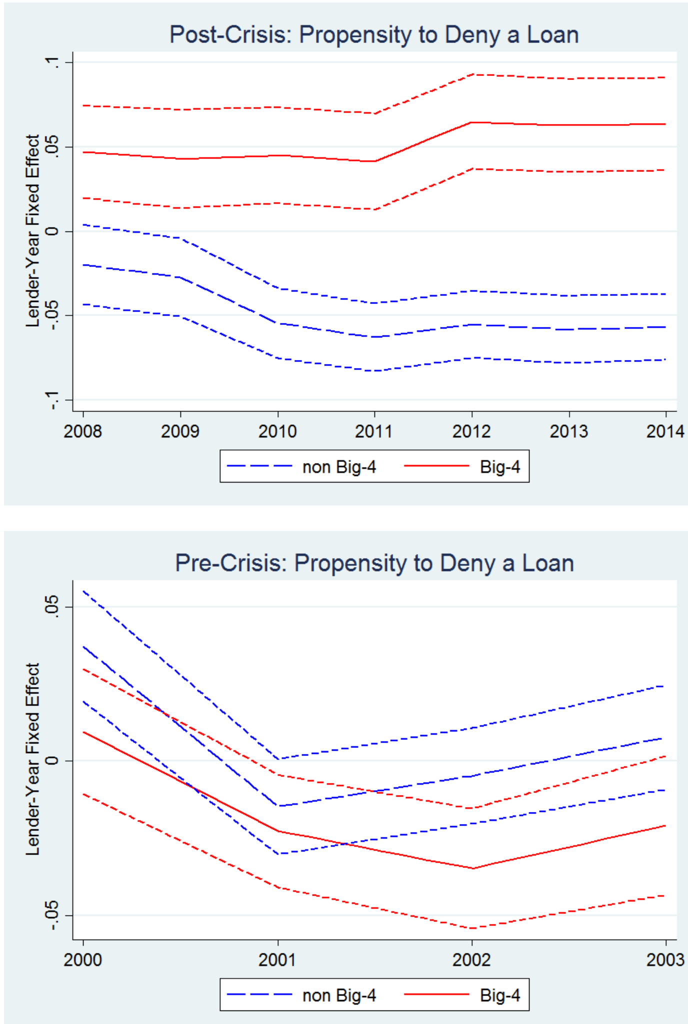
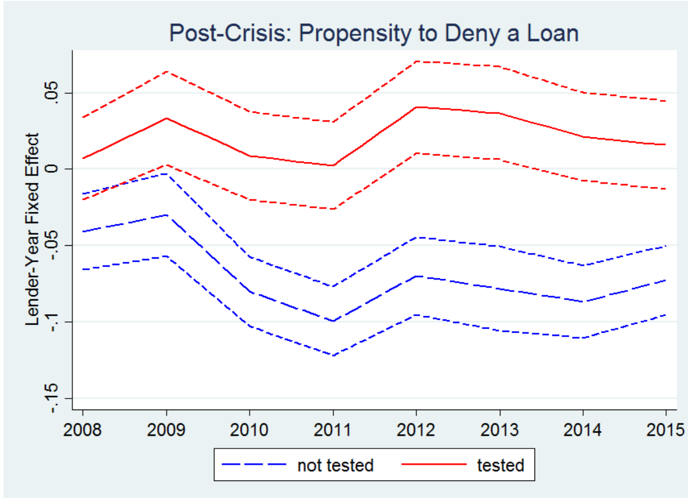


Figure A4. Propensity to deny a mortgage based on Big-4 exposure. The top panel plots the lender-year fixed effects estimated in equation $A1$ for Big-4 and non-Big-4 lenders over 2008–2014. Specifically, equation $A1$ is a linear probability model of mortgage denial which controls for the applicant's log income, requested loan-to-income, race, and MSA-year, lender-MSA, and lender-year fixed effects. The dashed lines correspond to a 95% confidence interval, computed with heteroskedasticity robust standard errors. The reference lender-year category is non-Big-4 lenders in 2007, for which the denial probability was 15.6%. The bottom panel has an analogous figure for the 2000–2003 period, where the reference category is non-Big-4 lenders in 2004.



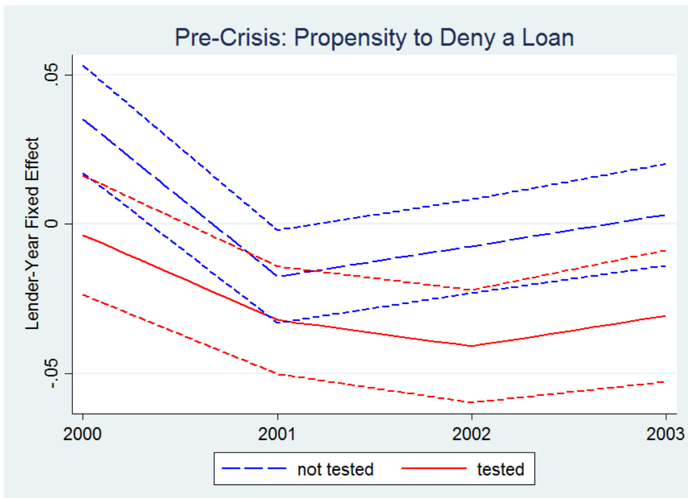
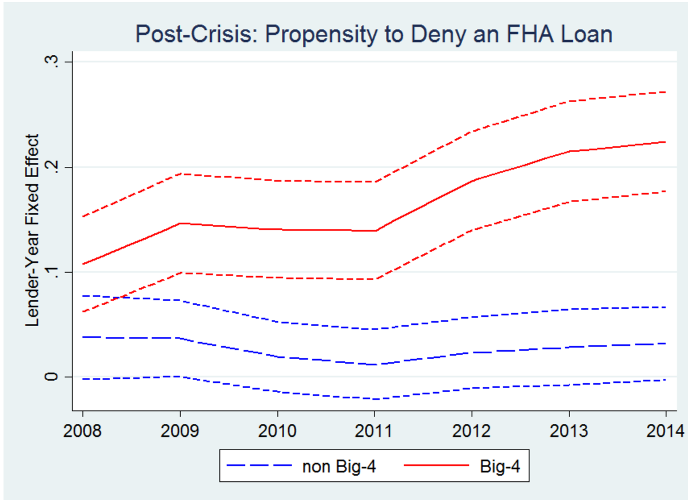


Figure A5. Propensity to deny a mortgage based on exposure to stress-tested lenders. The top panel plots lender-year fixed effects estimated as in equation $A1$ for stress-tested and non stress-tested lenders over 2000-2003, where stress-tested lenders are those that underwent a CCAR test between 2011 and 2015. The dashed lines correspond to a 95% confidence interval, computed with heteroskedasticity robust standard errors. The reference lender-year category is non stress-tested lenders in 2007. The bottom panel has an analogous plot for the 2000–2003 period, and the reference category is non stress-tested lenders in 2004.



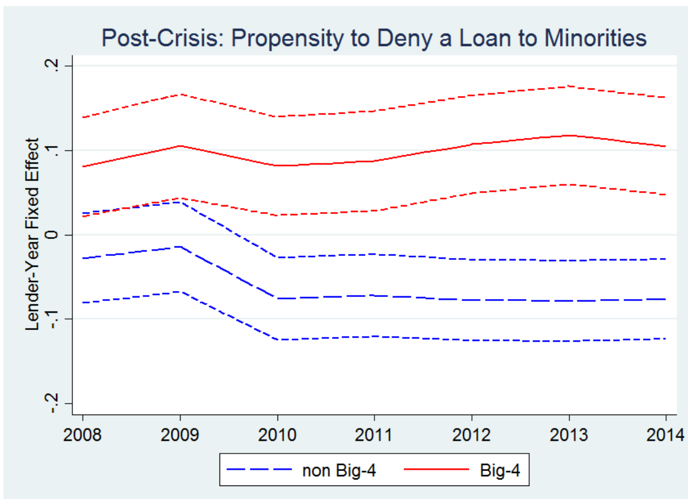


Figure A6. Propensity to deny mortgages to FHA borrowers and to blacks or hispanics. The top panel plots the lenders' fixed effects estimated as in equation $A1$ for FHA loans. The dashed lines correspond to a 95% confidence interval, computed with heteroskedasticity robust standard errors. The reference lender-year category is non-Big-4 lenders in 2007, for which the denial probability for FHA loans was 14.8%. The bottom panel plots the lenders' fixed effects estimated as in equation $A1$ for loan applications by blacks and Hispanics, which we call minority loans. The dashed lines correspond to a 95% confidence interval, computed with heteroskedasticity robust standard errors. The reference lender-year category is as in the top panel and the corresponding denial probability for minority loans was 25.6%.

Table A1: Correlation Matrix

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

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

Table A2: Rent Growth and Credit Supply: Sample Sensitivity

Outcome:	Avg. rent growth$_{m,10-14}$	Avg. rent growth$_{c,10-14}$
Avg. denial rate$_{msa,10-14}$	0.988	
	(0.021)	
Avg. denial rate$_{county,10-14}$		0.487
		(0.008)
Sample	Non-Headquarter	Full
Geographic unit	MSA	County
MSA controls	Yes	Yes
State fixed effects	Yes	Yes
Underidentification test (p-value)	0.007	0.001
J-statistic (p-value)	0.125	0.466
Number of observations	215	556

p-values are in parentheses. The first column re-estimates our baseline specification from Table 2 excluding MSAs in a state with or adjacent to a Big-4 headquarter (CA, NC, NY, CT, NJ). The second column uses the full sample, but at the county level, so that each observation is a county, not an MSA. The instruments for Avg. denial rate and the MSA controls are the same as in Table 2. The underidentification test is that of Kleibergen and Paap (2006). Standard errors are heteroscedasticity robust.

Table A3: Robustness: Credit Supply Instruments and Drivers of Housing Rents

Outcome:	$\text{Tested}_{m,08}$	$\text{Big-4}_{m,08}$	$\text{Tested}_{m,08}$	$\text{Big-4}_{m,08}$
Avg. rent $\text{growth}_{m,00-08}$	-0.319	-0.131	-0.192	-0.005
	(0.330)	(0.892)	(0.010)	(0.952)
$\log(\text{ent}_{m,09})$	-0.181	0.124	0.022	-0.207
	(0.643)	(0.912)	(0.773)	(0.186)
$\log(\text{house price}_{m,09})$	0.647	-0.368	0.376	0.146
	(0.158)	(0.788)	(0.000)	(0.392)
$\log(\text{population}_{m,09})$	0.125	0.471	0.026	0.243
	(0.755)	(0.650)	(0.678)	(0.018)
$\log(\text{Income}_{m,09})$	0.296	-0.075	0.051	0.043
	(0.438)	(0.952)	(0.607)	(0.781)
Avg. unemp. $\text{growth}_{m,10-14}$	-0.170	-0.081	-0.030	0.070
	(0.496)	(0.928)	(0.569)	(0.327)
Avg. price $\text{growth}_{m,10-14}$	0.123	-0.210	0.113	-0.081
	(0.608)	(0.832)	(0.095)	(0.332)
Financial services $\text{share}_{m,08}$	-0.274	0.433	0.056	0.047
	(0.214)	(0.430)	(0.410)	(0.704)
Homeownership $\text{rate}_{m,09}$	-0.064	0.062		
	(0.566)	(0.853)		
State fixed effects	Yes	Yes	Yes	Yes
R-squared	0.832	0.503	0.680	0.373
Number of observations	60	60	255	255

p-values are in parentheses. All variables are standardized to have a standard deviation of 1. The outcome in each column is one of our credit supply instruments: (1) the 2008 mortgage application share of lenders that underwent a stress test between 2011 and 2015; and (2) the branch deposit share of the Big-4 banks in 2008. Homeownership rates are from the U.S. Census Bureau's Housing Vacancy Survey. Each observation is an MSA. Standard errors are heteroskedasticity robust.

Table A4: Panel Analysis: Credit Supply and Rent Growth

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

Standard errors are clustered by MSA. p-values are in parentheses. $\Delta\log(\text{Rent}_{m,t})$ and $\Delta\text{Denied}_{m,t}$ denote the change in log rents and denial rate from year t-1 to year t, respectively. The instruments for $\Delta\text{Denied}_{m,t}$ are: (1) $V_{m,t-1}$, the Big-4's branch deposit share in 2008 in MSA m multiplied by the difference in denial propensity between the Big-4 and non Big-4 lenders in year t-1; (2) $S_{m,t-1}$, the mortgage application share of stress-tested lenders in 2008 multiplied by the difference in denial propensity between stress-tested and non stress-tested lenders in year t-1. Stress-tested lenders are those subject to CCAR between 2011-2015; (3) $G_{m,t-1}$, the weighted average denial propensity among the top 20 lenders in an MSA in year t-1, with weights determined by mortgage application shares in that year; (4) the fraction of applications from MSA m in year t-1 within 5% of the national average conforming loan limit in year t, where the average excludes MSA m. Instrument (5) is a version of that used by Loutskina and Strahan (2015) suitable for the post-2008 period. The Online Appendix contains a thorough description of each instrument. MSA controls are the lagged changes in: log median household income, log median inhabitant age, log population, and the unemployment rate. The underidentification test is that of Kleibergen and Paap (2006). The sample period is 2009–2014. Each observation is an MSA-year.

Table A5: Panel Placebo: Credit Supply and Rents Before the Crisis

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

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

Table A6: Credit and Rents PreCrisis using the Loutskina and Strahan (2015) IV

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

p-values are in parentheses. $\Delta \log(\text{Rent}_{m,t})$ and $\Delta\text{Denied}_{m,t}$ denote the change in log rents and denial rate from year t-1 to year t, respectively. The instrument for $\Delta\text{Denied}_{m,t}$ is the triple product of: (a) the fraction of applications from MSA m in year t-1 within 5% of the conforming loan limit in year t; (b) MSA m's elasticity of housing supply estimated by Saiz (2010); and (c) the change in the log conforming loan limit between year t-1 and year t. We refer to this instrument, originally used by Loutskina and Strahan (2015), as the Conforming Loan Limit (CLL) Shock. MSA controls are those from Table A5. The underidentification test is that of Kleibergen and Paap (2006). The sample period is 2001–2003. Each observation is an MSA-year. Standard errors are clustered by MSA.

Table A7: Instrument Sensitivity in Panel Analysis

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

p-values are in parentheses. $\Delta \log(\text{Rent}_{m,t})$ and $\Delta\text{Denied}_{m,t}$ denote the change in log rents and denial rate from year t-1 to year t, respectively. The instruments for $\Delta\text{Denied}_{m,t}$ are those from Table A4. In each column, we exclude one of the instruments, as indicated in the row Excluded IV. The C-Statistic corresponds to the difference-in-Sargan test of the hypothesis that the excluded instrument is valid; it is based on the difference in J-Statistics when using the full instrument set and when excluding the instrument in question. MSA controls are those from Table A4. The underidentification test is that of Kleibergen and Paap (2006). The sample period is 2009–2014. Each observation is an MSA-year. Standard errors are clustered by MSA.

Table A8: House Prices and Credit Supply

Outcome:	Avg. house price growth$_{m,10-14}$
Tested$_{m,08}$	0.026
	(0.096)
Tested$_{m,08}$ $\times$ High minority$_{m,08}$	-0.028
	(0.007)
Home type	All
Estimation	OLS
MSA controls	Yes
State fixed effects	Yes
Number of observations	257

p-values are in parentheses. House Price Growth$_{m,10-14}$ denotes the average annual change in the log of MSA m's median house price for all homes, based on Zillow's Home Value Index (ZHVI). High minority$_{m,08}$ denotes whether the MSA had an above-median share of mortgage applications from blacks or Hispanics in 2009. Tested is the stress test instrumental variable defined in Table 2. MSA controls are those from Table 2 and 2009 log house price. The underidentification test is that of Kleibergen and Paap (2006). Each observation is an MSA. Standard errors are heteroscedasticity robust.

Table A9: Multifamily Construction and Credit Supply

Outcome:	Avg. multifamily permits growth$_{m,11-14}$
Avg. denial rate$_{m,10-14}$	14.965
	(0.074)
Estimation	IV
MSA controls	Yes
State fixed effects	Yes
Underidentification test (p-value)	0.026
J-statistic (p-value)	0.159
Number of observations	229

p-values are in parentheses. Avg. multifamily permits growth$_{m,11-14}$ denotes the average annual change in log multifamily permits in MSA m over 2011-2014, to allow for a one year lag in the supply response. The instruments for Avg. denial rate$_{m,10-14}$ are the variables from Table 2. MSA controls are those from Table 2 and 2010 log multifamily permits. The underidentification test is that of Kleibergen and Paap (2006). Each observation is an MSA. Standard errors are heteroskedasticity robust.

Table A10: Rent Growth, Credit Supply, and Lending Frictions

Outcome:	Avg. rent growth$_{m,10-14}$
Tested$_{m,08}$	0.023
	(0.376)
Tested$_{m,08}$ $\times$ License$_m$	0.070
	(0.045)
Estimation	OLS
MSA controls	Yes
State FE	Yes
Number of observations	257

p-values are in parentheses. License$_m$ denotes whether the MSA is in a state requiring mortgage brokers to be licensed. Tested is the stress test instrumental variable defined in Table 2. MSA controls are those from Table 2. The underidentification test is that of Kleibergen and Paap (2006). Each observation is an MSA. Standard errors are heteroscedasticity robust.