# Employment in the Great Recession: How Important were Household Credit Supply Shocks?

Daniel García*

The Johns Hopkins University

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**Abstract**

I estimate the extent to which reductions in the supply of mortgage credit explain the near 7 million decline in employment in the Great Recession. I measure exogenous variation in mortgage credit supply at the county-level, based on the interaction of liquidity-driven lender shocks over 2007-2010, with county-lender prerecession mortgage market shares. A 10 percent supply-driven, exogenous reduction in local mortgage credit is associated with a 1 percent decline in employment. The job losses are concentrated in construction and finance. The reduction in mortgage supply explains about 15 percent of the employment losses over 2007-2010.

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# 1 Introduction

In the Great Recession, home prices and nonfarm payrolls fell by about 7 and 20 percent. There are distinct explanations for the decline in economic activity, such as shocks to household net worth, uncertainty, and credit supply, though questions remain about their importance. The goal of this paper is to empirically assess one of these explanations – the credit supply view of the recession, with a specific focus on the mortgage market. To do so, I construct variation in mortgage supply (an instrument) which I argue is plausibly exogenous to contemporaneous shocks affecting employment. Under the assumption of instrument validity, I estimate the effect of changes in mortgage supply on employment, net of possibly confounding factors, such as shocks to net worth, uncertainty, and credit demand.

The paper focuses on supply disruptions in the home purchase mortgage market, for a few reasons. Purchase mortgages are typically the largest private credit category, as measured by new credit issuance, outstanding debt, or trading volume in the secondary market. Moreover, the mortgage market experienced severe supply disruptions in the crisis. Notably, the private side of the secondary market for mortgages fully collapsed in the second half of 2007 and remained inactive over the next years. To prevent the public secondary market from also collapsing, the Treasury and Federal Housing Finance Agency placed Fannie Mae and Freddie Mac into conservatorship a week before the bankruptcy of Lehman Brothers (Frame et al. 2015). In the next months, the Federal Reserve began its purchases of over $1 trillion in agency debt and mortgage-backed securities as part of the first round of quantitative easing (Hancock and Passmore 2014). These historic interventions hoped to limit the negative effects of reductions in mortgage supply on residential markets (e.g. home starts, prices) and employment, particularly in construction, though possibly in other sectors as well via industry linkages (Mian and Sufi 2009; Boldrin et al. 2012).

The main contribution of this paper is to be the first to estimate the extent to which reductions in mortgage supply explain the decline in employment in the Great Recession, pooling data from all large lenders and counties in the U.S. Eggertsson and Krugman (2012) and Guerrieri and Lorenzoni (2017) posit that reductions in household credit supply led to declines in aggregate demand and employment in the Great Recession. The gap in the

literature is that there is little empirical evidence on the employment effects of reductions in household credit supply during the Great Recession. Other empirical work quantifying the credit crunch view of the recession has either focused on corporate credit shocks (Chodorow-Reich 2014; Greenstone et al. 2015) or on a single mortgage lender (Mondragon 2017).

The identification challenge is that reductions in mortgage supply could be partly endogenous and therefore reflect other household characteristics. To address this challenge, I construct a mortgage credit supply instrumental variable at the county level that I argue is valid – both relevant and plausibly exogenous to local economic shocks affecting employment and home prices over 2007-2010. I then estimate the county-level response of employment to supply-driven reductions in mortgage credit via two stage least squares. The first stage isolates supply-driven plausibly exogenous variation in mortgage credit issuance at the county-level. The second stage estimates show the county-level response of employment to the plausibly exogenous declines in mortgage credit during the recession.

The credit supply instrument is based on the insight that credit changes in small areas are in part driven by nonlocal supply factors – variation in the ability of multimarket lenders to extend new credit for reasons unrelated to local economic conditions (Peek and Rosengren 2000). Distress in funding markets affected lenders differently. For example, lenders more reliant on loan sales in the secondary market were less able to extend new credit in the recession. I estimate a lender-level measure of mortgage funding cost shocks that explicitly controls for variation in lender exposure to regional credit demand shocks.[1] The cost shock measure is well-explained by lenders' choice in funding strategy, and is uncorrelated with credit growth in the boom years. U.S. counties varied in their exposure to these lender shocks, because of variation in the intensity of county-lender relationships as measured by 2005-2007 mortgage market shares. In line with existing literature, I provide evidence that these credit market relationships are not easily substitutable and are instead highly persistent.[2] Because

[1]Specifically, I isolate the lender component (fixed effect) explaining variation in credit changes over 2007-2010 at the county-lender level when controlling for county fixed effects and county-lender observables such as income, a measure of credit risk, and loan characteristics. The approach is in the tradition of Khwaja and Mian (2008); see Chodorow-Reich (2014); Greenstone et al. (2015) for recent related approaches.

[2]Market shares are highly persistent in the data, with 2005 shares explaining almost 1-to-1 variation in 2007 shares. Moreover, I find very limited evidence of lenders entering new counties in the recession: of all 2008-2010 county-lender pairs, less than 8 percent were new. There is limited shopping even in 'normal' times, with Alexandrov and Koulayev (2017); Woodward and Hall (2012); Lacko and Pappalardo (2010)

of this, counties experienced larger declines in mortgage credit, all else equal, if they had existing credit relationships with lenders experiencing worse funding cost shocks in the crisis.

The identification assumption requires that county exposure to the lender shocks (the instrument) is uncorrelated with the error process affecting employment in the recession, conditional on the observed characteristics of localities. Ideally, lender location would be randomly distributed across counties. If instead counties and lenders are matched along characteristics of localities, county exposure to lender shocks could correlate with local characteristics and thereby with other employment shocks. To the extent that matching occurs along observables, I develop an econometric framework showing that controlling for those characteristics recovers 'as good as random' variation in lender location.[3] Matching might occur along unobservables, however. Lenders with worse funding shocks in the crisis may have moved in the peak boom years to counties with overly optimistic beliefs (for example). However, the main results are robust to measuring county exposure to lender shocks using 2000-2002 shares (instead of 2005-2007 shares), though estimates are noisier. The reason is the high persistence in market shares: county-lender shares in 2000 explain 72 percent of the variation in 2007 shares. In case matching could have taken place before 2000, I show that the main results of the paper are robust to controlling for region, division, or state fixed effects. This guards against the possibility of correlated regional fixed effects for employment changes and lender location.[4]

The results are as follows, when controlling for a detailed set of county covariates as well as region fixed effects. First, the credit supply instrument contains significant explanatory power over observed changes in mortgage credit originations over 2007-2010. The instrument is not weak, with the first stage F statistic above 30. This is evidence of costs to switching lenders, since in their absence the credit supply instrument would not help predict credit changes. Second, supply-driven plausibly exogenous declines in mortgage credit are statisti-

finding that borrowers shop too little, are unaware of price dispersion, and tend to overpay for mortgages.

[3] I control for various important industry, borrower, and loan characteristics of localities including the fraction of subprime borrowers, a measure of 2006 household debt to income, and the run-up in home prices over 2003-2006. For a full list see Table 3.

[4] As discussed shortly, I perform various other checks for instrument validity such as 'placebo' tests; controlling for declines in small business lending in the recession; test of over-identification using two uncorrelated mortgage supply instruments derived from county exposure to 'large' and 'small' lenders; and the inclusion of large, failed lenders (e.g. IndyMac) in the analysis.

cally associated with declines in residential activity. This is evidence of the negative effects of declines in mortgage supply on the health of local housing markets. The mechanism is that reductions in mortgage supply reduce the ability of households to buy homes. A 10 percent reduction in mortgage credit is associated with a 10 percent decline in the issuance of new residential permits, and a 5 percent decline in home prices. I also find evidence that reductions in local supply are associated with higher delinquency and foreclosure rates.

Declines in mortgage credit also had a negative effect on employment. The job losses explained by the mortgage supply shock are concentrated in construction and financial services, a category of employment where about a third of workers are real estate intermediaries. The mechanism is that declines in mortgage supply reduce demand for housing, which contribute to job losses in industries reliant on housing demand. As evidence for this, I find that mortgage supply-driven losses in construction are more severe in counties where housing supply is more elastic – areas where construction responds more to changes in housing demand.

A 10 percent reduction in mortgage credit is associated with a 1 percent decline in total private employment. These losses are largely concentrated in construction and finance, which together accounted for close to 35 percent of the job losses in the recession (their share in total employment is typically between 10-15 percent). The estimated effects on other categories of employment – total private employment excluding construction and finance, and nontradable employment – are positive, though close to zero and not significant. This contrasts with the OLS coefficient estimates, which are 2-3 times larger and highly significant, when regressing changes in employment in these categories on changes in mortgage credit.

Overall, I find that reductions in supply could explain 10-15 percent of the employment losses in the U.S. over 2007-2010, or about 750 thousand to 1.1 million jobs lost. This is evidence that reductions in mortgage supply mattered for employment. The imputation is based on a partial equilibrium aggregation exercise that answers the counterfactual question: what if counties, all else equal, had received the best credit shock in the sample, i.e. the credit shock of the counties in the top percentile of the distribution? The improvement in supply generates employment gains via the estimated elasticity of employment with respect to mortgage supply.[5] As for interpreting the magnitude of these effects, it is important to note

[5]This approach overcomes the generic challenge that level changes in supply cannot be recovered from

that these estimates reflect the 'mechanical' effects of reductions in mortgage credit supply; they do not reflect, for example, the effects of uncertainty shocks associated with financial distress, to the extent that those were common across localities, for example. Moreover, I argue that the estimates do not reflect the effects of other types of credit supply shocks. County exposure to specifically mortgage lending shocks can be measured distinctly from small business loan shocks, for instance, because county-lender small business loan and mortgage market shares are largely uncorrelated.

I provide evidence for instrument validity and show that the baseline results are robust to alternative specifications. The credit supply instrument passes 'placebo' tests – it helps explain changes in mortgage originations over 2007-2008 and 2008-2009 specifically, but not during any other years in the decade preceding the Great Recession. I also perform a test of over-identification using two uncorrelated credit supply instruments derived from county exposure to 'large' and 'small' mortgage lenders.[6] The results are also robust to the inclusion of lenders who failed in the recession. For most of the paper, I conservatively drop institutions that filed for bankruptcy (e.g. IndyMac) because their failure poses difficulties in credibly estimating the mortgage funding cost they experienced in the recession. Including them in the sample increases the explanatory power of the credit supply instrument, while leaving the main results of the paper unchanged. To guard against omitted variable bias, I also report regression results which include, in addition to the rich control set in the baseline, squared and cubed terms of important prerecession variables: 2006 median household debt-to-income, the fraction of subprime borrowers, and the growth in home prices over 2003-2006. Finally, the results are robust to including the realized declines in small business lending obtained from the CRA.

**Related Literature:** This paper is part of the literature exploring the extent to which credit shocks explain the fall in employment (and other negative outcomes) in the Great Recession. Theory suggests credit shocks to firms and households could have sizeable real effects (Kiyotaki and Moore 1997; Eggertsson and Krugman 2012; Guerrieri and Lorenzoni

the cross-section, by assuming that the top percentile of counties by the credit supply instrument represent a 'no credit shock' scenario. For similar approaches see Mian and Sufi (2014) and Chodorow-Reich (2014).

[6] I measure exposure to 'small' and 'large' lenders by dividing lenders into two groups. *i*) Bank of America, Wells Fargo, JPMorgan Chase, Citibank, Suntrust, and PNC, and *ii*) the remaining lenders. Doing so splits market shares 53-47.

2017; Midrigan and Philippon 2016). The majority of empirical work has focused on the employment effects of corporate credit shocks.[7] Chodorow-Reich (2014) estimates corporate credit shocks could explain between one-third and one-half of job losses in the syndicated loan market, based on prerecession firm reliance on lenders more exposed to the bankruptcy of Lehman Brothers. Greenstone et al. (2015) focus on small business lending using data from the Community Reinvestment Act (CRA). They find that credit shocks help explain declines in credit borrowing, but produce only small employment effects.[8] The approach in this paper is complementary and similar in methodology, though the emphasis here is on the mortgage market. County-lender market shares from HMDA (mortgages) and the CRA (small business lending) are weakly correlated, as shown in Figure 3. As a result, the mortgage supply instrument helps explain local declines in mortgage credit, but not declines in small business lending. Moreover, the results of this paper are robust to controlling for the realized declines in small business lending over 2007-2010.

On the household side, this paper and Mondragon (2017) are the first to measure the employment effects of reductions in mortgage supply in the Great Recession. He uses county-level 2005-2006 exposure to Wachovia Bank to instrument for local credit changes. Wachovia's bankruptcy was precipitated by the acquisition of a lender (Golden West) active in a different part of the country as Wachovia, and so constitutes a plausibly exogenous shock to the counties where Wachovia traditionally operated in, mainly in the South Atlantic. He estimates that a 10 percent 'exogenous' reduction in mortgage credit is associated with a 3 percent decline in employment, an elasticity two (three) times as large as the OLS counterpart (my own estimate). The South Atlantic, Wachovia's historical home base, experienced particularly severe employment losses in the recession. The estimates would be biased up if regional fixed effects for Wachovia location and employment losses are positively correlated. In fact, when using division or state fixed effects, the Wachovia instrument significantly weakens.[9] In contrast, this paper pools information from all large lenders located across the

[7] A related empirical literature studies the international transmission of the financial crisis through the banking sector such as Cetorelli and Goldberg 2011; Haas and Lelyveld 2014; Schnabl 2012.

[8] Glancy (2015) finds that county-level shocks, measured using the exposure of local banks to real estate losses in other geographies, help explain employment losses particularly in young or financially dependent firms.

[9] I obtain Wachovia 2005-2006 purchase shares from HMDA and restrict the sample to counties in the South and Northeast as in Mondragon (2017). The first stage F statistic associated with the Wachovia

U.S., employs a richer set of county controls, and so is more robust to potential concerns about non-random lender-region matching. The results in this paper are essentially the same when using region, division, or state fixed effects for example.

More broadly, this paper is part of the literature studying the effects of changes in mortgage supply on housing and labor markets. Most empirical work has focused on the former. Favara and Imbs (2015) use US branching deregulations in the mid 90s to instrument for credit changes, and find that the credit changes induced by deregulation contributed to rising home prices. Related work includes Mian and Sufi (2011); Adelino et al. (2012); Berrospide et al. (2016); Anenberg et al. (2016); Vojtech et al. (2016); Gropp et al. (2014); Gete and Reher (2016). This paper contributes to this literature by highlighting the construction and finance employment effects of changes in mortgage supply, which only a few other papers do. Focusing on the mid 2000s, DiMaggio and Kermani (2016) use a federal preemption of national banks from local anti-predatory lending laws in 2004 to estimate the elasticity of home prices and nontradable employment with respect to mortgage supply. Passmore and Sherlund (2016) find that counties more reliant on GSEs for mortgage credit experienced healthier local housing and labor markets in the Great Recession.

# 2 Data and Summary Statistics

I assemble a detailed county-level dataset including home prices, home sales, employment, mortgage credit, credit scores, demographics, borrower characteristics, industry composition, and various other local characteristics. The main source for mortgage data is the Home Mortgage Disclosure Act (HMDA). Mortgage lenders with offices in metropolitan areas are required to disclose to the public detailed information each year, including the dollar amount and number of mortgages issued, as well as the location (census tract, county, etc) of the property securing the loan. HMDA covered throughout the mid and late 2000s over 90% of residential mortgage lending by dollar amount (Dell′Ariccia et al. 2012). Figure 1 plots aggregate trends in mortgage originations, total private employment, and the S&P Case-

instrument is 14.47, absent other controls including regional fixed effects. When including division (state) fixed effects, the F statistic drops to 4.33 (0.93). Observations are weighted by 2006 population, and standard errors are clustered by state.

Schiller U.S. National Home Price Index, with the series indexed to their 2006 value.

Table 1 shows summary statistics for about 850 of the largest counties in the United States, which account for 86% of the population, and Table 3 provides definitions and sources for the data used throughout the paper. While mortgage credit declined over 2007-2010 in virtually all counties, there is significant cross-sectional variation in the decline, with credit falling by more than 63% in ten percent of the counties in the sample and falling by less than 24% in the top decile. Figure 2 shows that variation in local credit changes is associated with changes in various local outcomes. Counties experiencing more severe declines in credit experienced more severe declines in home prices, total private employment, construction employment, and nontradable employment, a measure of local retail, mostly food and drink (Mian and Sufi 2014).

Data on delinquency rates, foreclosure rates, home sales, and home prices are obtained from CoreLogic. Data on building permits comes from the Census. For employment, I rely on two sources, both of which are establishment-based and provide near full-coverage of private employment: the Quarterly Census of Employment and Wages (QCEW), and the County Business Patterns (CBP). I use the CBP to measure tradable and nontradable employment using the definitions in Mian and Sufi (2014), and the QCEW for the other employment data.

I obtain lender-level data from HMDA, which provides loans by lender subsidiaries (respondents) and locality. I match subsidiaries belonging to the same parent company using the crosswalk maintained by Robert Avery, and aggregate to the level of the parent company (bank holding company, for banking institutions).[10] To calculate changes in lending at the lender level without including changes due to acquisitions, I use the standard approach (see, for example, Bernanke and Lown (1991) or Greenstone et al. (2015)) of treating the acquired and acquiring institutions as part of the same entity throughout the sample period, which in this paper, is over 2005-2010.

In measuring the exposure of counties to lenders, I focus on large multikmarket lenders operating in multiple counties who did not file for bankruptcy during the crisis. In order to isolate the differences in lending at the lender level that are not attributable to variation in

[10] Available upon request at Robert.Avery@fhfa.gov

their exposure to local economies, I need to observe lenders operating in multiple localities. Therefore, I focus on lenders who were operating in at least 100 counties in 2007, and who issued over $1 billion in mortgage originations in the same year. These 56 lenders (Table 2 provides lender-specific statistics) account for 75% of mortgage lending over 2005-2007, so they cover the majority of lending by market share, even though there were over 6,000 mortgage lenders in that period. I roll up the remaining small institutions into a single entity.[11] Lender shocks by these large, multimarket lenders, along with prerecession variation in the intensity of lender-county relations, form the basis of the identification strategy.

For most of the paper, I do not include failed institutions in the sample, because the portion of lending changes that is nonlocal cannot be plausibly isolated for these lenders, since lending for these institutions fell by 100% everywhere (there is no variation). Dropping these institutions is a conservative choice: it reduces the potential for biased estimates at the expense of statistical power. I show in the Results section of the paper that the inclusion of failed institutions indeed increases the explanatory power of the credit supply instrument, while leaving coefficient estimates essentially unchanged.

# 3 Differences in Lender Supply

This section describes how I measure differences in lender supply across lenders for nonlocal reasons. Those differences are largely driven by mortgage funding cost shocks: 72 percent of the variation in supply for the banks in the sample is explained by prerecession exposure to fragile funding sources such as wholesale debt and the originate-to-distribute lending model. In contrast, credit growth in the boom years (2003-2006) does not help explain either differences in lender supply over 2007-2010 or differences in credit growth, as shown in Figure 5. Therefore, I interpret the supply differences as largely reflecting exposure to unexpected funding cost shocks.

The challenge in recovering differences in supply from the data is that lending changes might be driven by supply or demand changes, a variant of the reflection problem in Manski

[11] To make sure this adjustment does not influence the main results of the paper, I re-estimate the paper's main equations without counties where over 50 percent of the prerecession market shares is accounted for by these small lenders. The results are essentially the same.

(1993). The approach is to control for factors associated with changes in the credit-worthiness of borrowers. To do so, I take advantage of the richness in the HMDA data, which provides credit changes at the county-lender level as well as various borrower characteristics.[12]

I isolate the lender component (fixed effect) explaining variation in credit changes over 2007-2010 at the county-lender level when controlling for county fixed effects, to account for differential credit demand shocks by geography. For example, I interpret the fact that Bank of America within-county lending changes were below-average to imply that Bank of America cut supply relative to most lenders for nonlocal, idiosyncratic reasons. I experiment with different specifications that control for differences in the income and risk profile of borrowers at the county-lender level; using census tract fixed effects; or only high-income loans. The approach is robust to these alternatives, with estimated differences in supply highly correlated (close to one).

Specifically, I estimate versions of the following equation:[13]

$$\Delta L_{i,b} = \alpha_i + \phi_b + \gamma D_{i,b} + \upsilon_{i,b} \tag{1}$$

where $\Delta L_{i,b}$ are percent changes in mortgage credit originations at the county-lender level over 2007-2010, $\alpha_i$ are locality fixed effects (county or census-tract), $\phi_b$ are lender fixed effects, and $D_{i,b}$ are prerecession county-lender characteristics. The parameters of interest are those associated with the vector of lender fixed effects $\phi^b$, which capture the idiosyncratic lender factor explaining variation in credit changes, net of locality fixed effects and prerecession county-lender characteristics.

In the baseline specification, I control only for county fixed effects. In this case the identifying assumption is that within-county credit demand shocks are uncorrelated with lender shocks. Supply contractions for Bank of America would be overestimated, for instance, if Bank of America borrowers tend to be low-income, and low-income borrowers experienced worse credit demand shocks within-counties than average. This concern is directly addressed

[12]The approach is based on Khwaja and Mian (2008); for related approaches see Chodorow-Reich (2014) and Greenstone et al. (2015).

[13]The lender fixed effects coefficients are estimated using 27,624 county-lender observations, for the 56 lenders in my sample and the 1,067 counties that have more than 3 of those lenders. The lender fixed effects explain about a fifth of the variation in within-county lending changes over 2007-2010.

by controlling for prerecession county-lender characteristics $D_{i,b}$. When doing so, however, the estimated differences in lender supply are highly correlated (0.91), as shown in Figure 6. The county-lender characteristics observed in HMDA are borrower income, fraction of loans classified as being high-risk, race, type of loan (owner-occupier), and credit growth in the peak boom years 2003-2006 by county-lender.

I also estimate equation 1 using census tract fixed effects and using only high income loans. In each case, the estimated lender shocks are highly correlated (close to one). For example, I re-estimate the equation using only loans to borrowers with income over $77 thousand, the median reported borrower income in 2007. Estimating differences in supply using only high-income (or low-income) loans are highly correlated (0.88). This helps to rule out the potential concern that some lenders might have specialized in the low income market, and that demand in the low income market collapsed. I also re-estimate equation 1 using census tract fixed effects rather than county fixed effects, and also find that the estimated differences in supply are highly correlated.[14]

Table 6 shows sample statistics for a selected sample (by size) of 30 of the 56 lenders in the sample of lender ranking changes in mortgage lending over 2007-2010 (Column 2), and by the estimated differences in lender supply in the baseline (Column 3). Changes in the ranking (going from Column 2 to 3) indicate differences in the degree to which lending changes were driven by geographic exposure to credit shocks. For example, the drop in Bank of the West's ranking from $6^{th}$ to $15^{th}$ indicates that lending changes for this bank remained relatively robust in the recession partly because of its exposure to above-average geographies (in this case the Midwest). Conversely, the improvement in the ranking of JPMorgan Chase from $50^{th}$ to $41^{th}$ indicates that part of its lending decline was driven by exposure to underperforming areas.

[14]Census tracts are statistical subdivisions of counties, each generally having a population size between 1,200 and 8,000 people. Census tract-income groups are more homogeneous than the county – in 2007, the median within-group standard deviation of HMDA borrowers in the census tract-income groups was $92 thousand, 27% lower than in counties. I rank census tracts within a county by borrower income, and divide the census tracts into four equal-sized groups by income, i.e. the top quartile consists of the high-income census tracts in the county.

## 3.1 Funding Fragility and Differences in Supply

I now show that variation in lender supply is well-explained by funding strategy choices. Mortgage loans are usually funded in one of three ways: (i) trough retention of the loan on the balance sheet; (ii) loan sales to government-sponsored agencies (GSEs), such as Fannie Mae, Freddie Mac, or Ginnie Mae; and (iii) loan sales to private investors, such as private-label securitizers, but also other financial entities such as other banks, mortgage companies, or insurance providers, as discussed for example in Passmore et al. (2005).

Table 5 shows that differences in prerecession funding strategies explain 72 percent of the variation in lender supply (see also Figure 8). The first three rows of Column 1 shows that higher pre-recession reliance on wholesale funding, loan sales in general, and particularly to private investors, are each independently associated with more severe declines in supply.[15] Lower pre-recession capital ratios are also associated with declines in credit supply (Column 2), though this factor is relatively minor, judging by its 5 percentage point contribution to the R-squared. Column 3 shows that pre-recession credit growth (over 2003-2006) is not helpful in explaining variation in differences in supply during the Great Recession. Observations are weighted by the dollar amount of mortgage originations in 2007, although the weighting is not critical, as the last column shows.

I measure bank-level exposure to wholesale funding as the ratio of non-core funding (sum of large time deposits, foreign deposits, repo sold, other borrowed money, subordinated debt, and federal funds purchased) to total assets, from the Federal Reserve's FRY-9C form, a standard definition in the literature (Irani and Meisenzahl 2017).[16] To measure lender exposure to the secondary market, I use data from HMDA, which provides loan sales in the secondary market by year and type of buyer. To measure general reliance on loan sales, I compute for each lender the dollar amount of loans that were originated and sold over 2005-2007 divided by total originations during that period. To measure the exposure to private investors, I impute the fraction of loan sales that were sold to private investors, such

[15] In complementary work, Dagher and Kazimov (2012) find that mortgage lenders more reliant on wholesale funding had higher mortgage denial rates, after controlling for various borrower characteristics.

[16] As is well known, funding markets were severely disrupted during the financial crisis (Ivashina and Scharfstein 2010; Cornett et al. 2011; Brunnermeier 2008; Gorton and Metrick 2012; Kacperczyk and Schnabl 2010; Ramcharan et al. 2016).

as private-label securitizers and other financial institutions, to total loan sales during that period.[17]

Measuring reliance on loan sales to private buyers is important since private-label residential mortgage securitization, which funded about 30% of mortgages over 2005-2007, went to essentially zero in 2008-2010 (Frame et al. 2015); see also Avery et al. (2011), and Nadauld and Sherlund (2009)). Because private investors stopped purchasing nongovernment-insured mortgages, lenders reliant on those sales likely cut supply during 2008-2010. For example, Calem et al. (2013) find that banks who were pre-recession more dependent on loan sales experienced more severe declines in jumbo lending, which are loans too large to be purchased by GSEs, and thus can only be sold to private investors, during the recession.

Loan sales to GSEs also became more expensive. G-fees, the monthly insurance fee GSEs charge as a fixed fraction of the loan balance, increased from about 20 basis points in 2005-2007 to 30 basis points in 2008-2010 (Fuster et al. 2013). Putback risk also increased in 2008. Lenders are required to repurchase loans sold to GSEs if it is found that those loans fail to satisfy original underwriting standards. While putbacks were rare, they rose during the recession, with Fannie Mae estimating that 3.7 percent of single-family loans purchased over 2005-2008 were putback to lenders, whereas the figure in other periods tended to be less than 0.5 percent[18]

# 4 The Nonlocal Lending Shock

Differences in lender supply affected counties differently, because of variation in the extent to which different counties relied on different lenders pre-recession. I define the county-level credit supply instrument, the Nonlocal Lending Shock, as the weighted average of lender shocks over 2007-2010, with the weights being prerecession (2005-2007) mortgage origination market shares:

$$Nonlocal\ Lending\ Shock_i = \sum_B Share_{i,b}\phi_b \tag{2}$$

[17]In particular, I exclude loan sales to FNMA, GNMA, FHLMC, FAMC, and to affiliates of the lender

[18]source: Fannie Mae 10-K 2013, p. 143

The nonlocal lending shock measures the exposure of a county in the Great Recession to lender shocks.[19] One concern is that the prerecession shares are not random. For example, lenders preferring to lend to subprime borrowers might have a stronger presence in counties with a high share of subprime borrowers. I show in the Online Appendix that as long as I can observe the local characteristics driving the matching between counties and lenders, I can isolate variation in market shares that is 'as good as random.' To that end, I employ a detailed set of prerecession county covariates that explain about 60 percent of the cross-sectional variation in mortgage credit changes over 2007-2010. I also control for region and state fixed effects to guard against the possibility that different regions had different types of lenders.

Figure 9 is a map of the nonlocal lending shock, after controlling for a detailed set of county covariates. The map appears balanced with no apparent trends by region. I now describe the rich set of county-level controls I employ. A standard model of mortgage lending, such as the one presented in Mian and Sufi (2009), posits that lending changes at the local level are determined by changes in expectations of income growth (Adelino et al. 2016), house prices, or supply conditions at the local level. While changes in these factors are not directly observable, I control for a rich set of local characteristics related to them, and which together explain 60 percent of the cross-sectional variation in credit changes at the county level during the Great Recession. They are summarized in Table 3.

Controls for borrower characteristics include 2006 median income from the IRS, FICO scores, the fraction of households with FICO score under 620, debt-to-income ratio, the fraction of the population that is white and the fraction that is college-educated. Previous studies have found that these characteristics help explain labor and credit market outcomes during the Great Recession.[20] I also control for local loan characteristics: the fraction of mortgages issued between 2003-2006 that were nonconventional, GSE-securitized, and owner-occupied loans (Haughwout et al. 2011; Chinco and Mayer 2016; Bhutta 2015).[21] Because

[19] The sum is taken over all large, non-failed multimarket lenders $B$ as described in Section 2

[20] For the fraction of subprime borrowers, see: Keys et al. (2010),Demyanyk and Hemert (2011), Dell′Ariccia et al. (2012), Gerardi et al. (2008), and Mian and Sufi (2009). For household debt to income, see: Mian and Sufi (2014), and Carroll and Kimball (1996). For demographics: Elsby et al. (2010).

[21] Nonconventional loans are those insured by the Federal Housing Administration (FHA) or backed by guarantees from the U.S. Department of Veterans Affairs (VA), the Farm Service Agency (FSA), or the Rural Housing Service (RHS)

these types of mortgages were relatively resilient, counties entering the recession with lenders specializing in these loans might have experienced, all else equal, more robust credit growth. For example, the share of mortgages categorized as nonconventional, for example, rose from about 10% in 2005-2007 to about 45% 2008-2010 (Bhutta and Ringo (2014)), particularly due to increases in FHA lending.

I also control for recent trends and competitiveness in local markets: the growth in the number of lenders operating in each county from 2004-2006 using data from HMDA. As a measure of local competitiveness, I measure Herfindahl-Hirschman indices in 2006 for each county, which are standard measures of market concentration, also obtained from HMDA. I control directly for the severity of the housing boom by including the growth in home prices over 2003-2006. Finally, I also control for the 2006 shares of construction and tradable employment, and for prerecession levels of employment and mortgage originations.

# 5 Empirical Framework

I now discuss results based on the following instrumental variables specification:

$$\Delta Outcome_i^j = \theta X_i + \beta \widehat{\Delta Credit}_i + f_s + \epsilon_i \tag{3}$$

$$\Delta Credit_i = \delta X_i + \rho Nonlocal\ Lending\ Shock_i + f_s + v_i \tag{4}$$

where changes over 2007-2010 in different outcomes (house sales, house prices, employment) indexed by $j$ are considered each in turn, and $f_s$ are fixed effects that could be at the region, division, or state level – I report results for each. Descriptions of the regression controls $X$ and outcomes are provided in Table 3. The Nonlocal Lending Shock is the credit supply instrument defined according to equation 2. All of the outcome variables are expressed as percent changes over 2007-2010. For employment categories and the home price index, changes are taken between 2007Q4 and 2010Q4. For mortgage credit and home sales, which are flows, changes are taken between the average flow over 2008-2010 with respect to the value in 2007.[22]

[22] Using 2005-2007 as the base period produces very similar results.

Out of close to 3,200 counties, I have full data on the largest 876 counties, which account for 86% of total employment. I drop states having 3 or fewer counties, to ensure having at least a few observations for each state for the specifications that use state fixed effects, for a total of 859 remaining counties. I weight observations by the number of employed workers in 2006, though results are essentially unchanged in the absence of weighting if the sample is restricted (for example) to the 500 largest counties, which account for 76% of total employment. I guard against outliers by dropping 1% of observations on each tail for each of the outcomes I examine.[23] I cluster standard errors at the state level, to allow for correlated shocks within-states, due to for example, state-specific institutional arrangements and spatial correlation.

## 5.1 First Stage Results

The nonlocal lending shock contains significant independent explanatory power over local changes in mortgage credit in the Great Recession, consistent with the existence of switching costs across lenders. A 10 percent reduction in the nonlocal lending shock is associated with a decline of 4.8 percent in the value of mortgage credit extended over 2007-2010, conditional on all other observable characteristics of the localities. This is shown in column 1 of Table 7, which reports results from regressing local mortgage credit growth on the nonlocal lending shock as well as a full set of county controls.

The coefficient on the nonlocal lending shock remains highly significant and retains similar values when fixed effects are included at the region (Column 2), division (Column 3), or state (Column 4) levels.[24] Prerecession characteristics of the localities explain a sizeable portion of the variation in mortgage credit growth during the Recession (60 percent), but to the extent that I am missing important local characteristics, the inclusion of regional fixed effects serves as a way of gauging whether those unobserved factors matter. The coefficient estimate on the nonlocal lending shock remains stable when including region fixed effects (the coefficient

[23] For example, I drop house price growth outliers from the house price regression, but I don't drop those counties from the private employment growth regression (unless they are also outliers in that variable). The only exception is growth in house sales for which I winsorize 5% of observations.

[24] The Census defines 4 regions of the US (Northeast, South, Midwest, and West) and 9 divisions (New England, Middle Atlantic, East North Central, West North Central, South Atlantic, East South Central, West South Central, Mountain, and Pacific).

estimate increases from .48 to .54), division (.43) or state fixed effects(.28), all within the 95 percent confidence interval in the model without fixed effects.

The instrument adds considerable independent explanatory power to the models of credit changes. The partial R-squared associated with the nonlocal lending shock is about 10 percent in the model without fixed effects, and ranges from 4 percent in the model with state fixed effects to 14 percent in the model with region fixed effects. The F statistic associated with the hypothesis that the coefficient on the nonlocal lending shock is zero conditional on all other observables is above 20 in all specifications except in state fixed effects model, in which the F statistic is 9.2, just shy of 10, a commonly used rule of thumb to indicate weak instrument problems (Stock and Yogo (2002)). Even within-states there is enough variation to explain variation in credit changes across counties.

## 5.2 Effects of Supply Reductions on Residential Markets

Supply-driven, exogenous declines in mortgage credit are statistically associated with declines in home sales, home prices, and increases in delinquency rates as well as foreclosure rates. This is evidence of the negative effects of declines in mortgage supply on the health of local housing markets. The mechanism is that reductions in mortgage supply reduce the ability of households to buy homes and to refinance.

Declines in credit supply are associated with declines in home sales. Table 9, which includes region fixed effects, shows that a 10 percent reduction in mortgage credit (when instrumented using the nonlocal lending shock) is associated with a 10 percent decline in the issuance of new residential permits in the model with region fixed effects. This is evidence that households were unable to offset the reduction in credit availability originating from nonlocal sources by borrowing from private sources or from lenders other than their traditional, prerecession lenders.

Declines in mortgage credit are also associated with declines in home prices. A 10 percent reduction in mortgage credit is associated with a 5.3 percent decline in home prices.[25] This

[25]This is consistent with other articles finding that supply-driven changes in credit have real effects on home prices, such as Favara and Imbs (2015), Mian and Sufi (2011), Adelino et al. (2012), Favara and Imbs (2015), DiMaggio and Kermani (2016), Anenberg et al. (2016), Vojtech et al. (2016), Passmore and Sherlund (2016), and Kung (2015).

association operates through the extensive marging. Measuring changes in mortgage credit using declines in the number of loans, rather than in the dollar value, yields nearly identical results.[26]

Delinquency rates and foreclosure rates also increased more in counties with weaker mortgage lenders. Table 9 shows that a 10 percent decline in mortgage credit is associated with a 1.3 and 0.8 percentage point increase in delinquency and foreclosure rates. This is evidence of the contractionary effects of reductions in mortgage supply on the health of local housing markets. The fall in home prices induced by the credit shock would make it more likely for households to go underwater.

In the Online Appendix I present results for the each dependent variable with no fixed effects, region, division, or state fixed effects. The main conclusions are essentially the same. The point estimates are very similar, though standard errors tend to rise in specifications with state fixed effects. For example, a 10 percent reduction in mortgage credit is associated with a 4.7, 5.3, 6.2, and 4.1 percent declines in home prices in the models without, region, division, and state fixed effects respectively. The similarity in the point estimates lends support to the identification strategy by ameliorating concerns about unobserved local factors.

## 5.3 Effects of Supply Reductions on Employment

Declines in mortgage credit are also associated with declines in employment. The job losses explained by the mortgage shock are concentrated in construction and financial services, a category of employment where over a third of workers are real estate intermediaries. The mechanism is that reductions in supply caused declines in housing demand, which negatively affected employment in industries reliant on housing demand. As evidence for this, I find that the construction losses are stronger in areas where housing supply is more elastic, that is, in areas where construction responds more to changes in housing demand. Overall, a 10 percent reduction in mortgage credit (for nonlocal reasons) is associated with a 1 percent decline in total private employment. Using the in-sample variation of the nonlocal lending shock, I estimate that about 10 to 15 percent of the employment losses in the Great Recession

[26]The coefficient estimate in the model with region fixed effects when using declines in the dollar value of mortgages is .536 while it is .544 when using declines in the number of loans.

can be explained by declines in mortgage supply.

Weak mortgage supply contributed to job losses in the construction sector, likely through at least two related channels: weak demand for new residential and commercial construction. The HMDA data only covers residential mortgages. However, to the extent that activity in both markets was tightly correlated, the credit shock would affect demand for both types of construction. Table 10 shows that a 10 percent decline in mortgage credit originating from nonlocal sources is associated with a 4.3 percent decline in construction employment for the model with region fixed effects, with results being similar for the other specifications. The mechanism is that declines in mortgage supply reduce housing demand, which is associated with lower employment in construction.

The employment losses in construction were, for a given decline in instrumented credit, more severe in areas where housing supply is more elastic.[27] That is, in areas where construction responds more strongly to changes in housing demand, the employment effects of a given credit decline were stronger. To see this, I focus on the sample of counties for which the Saiz (2010) measure of the elasticity of housing supply is available.[28] Table 11 reports results for changes in home prices and construction employment for the model with region fixed effects. Column 3 shows that the same relative decrease (increase) in credit is associated with lower (higher) construction employment in areas with higher housing supply elasticities – the coefficient estimate is positive for the interaction of credit changes and housing supply elasiticity. This is evidence for the mechanism that reductions in mortgage supply curtailed housing demand, and contributed to employment losses in construction.

Declines in mortgage supply are also associated with job losses in finance. This is also evidence that reductions in supply hurt housing demand, and therefore demand for housing intermediaries. Table 10 shows that a 10 percent reduction in mortgage credit is associated with a 3.9 percent decline in employment in financial sevices in the model without fixed effects, with the point estimates in the models with region fixed effects similar in magnitude.

[27] I add the interaction of credit changes and the housing supply elasticity to the regression model with region fixed effects. For the two endogenous regressors (credit changes and the interaction of credit changes and the housing supply elasticitiy), I use two instruments – the nonlocal lending shock, and the interaction of the nonlocal lending shock with the housing supply elasticity.

[28] Saiz (2010) estimates housing supply elasticity as a nonlinear combination of data on physical and regulatory building constraints, and population levels in 2000 at the metro area level.

Via the effects on construction and financial employment, declines in mortgage credit are associated with losses in total private employment. In the model with region fixed effects (Table 10), a 10 percent reduction in mortgage credit originating from nonlocal sources is associated with a significant 1.0 percent decline in total private employment. The models with other types of fixed effects have similar point estimates, as reported in the Online Appendix, though confidence intervals are wider, especially when division or state fixed effects are used. In the specification with state fixed effects, for example, a 10 percent reduction in instrumented mortgage credit is associated with a 0.8 percent decline in employment, with the associated 95 percent confidence interval ranging from -3.1 to 1.6 percent.

Declines in mortgage supply are only weakly associated with declines in employment in other, broader employment categories – 'other employment' (total private excluding construction and finance) and nontradable employment, which mostly consists of local retail and food, as shown in Table 10. That is, the reduction in home spending induced by the decline in mortgage supply is not associated with employment declines in other and nontradable employment. This suggests that the local elasticity of substitution between consumption and housing is relatively small (Boldrin et al. 2012).

The elasticity estimates of other and nontradable employment also contrast with their OLS counterparts, which are about three times larger and strongly significant, with t-statistics ranging from 3 to 8 across specifications, as shown in Table 8. The results suggest that the OLS coefficients are biased upwards because of reverse causality. The fact that elasticity estimates are significant in areas directly related to the mortgage market (home sales, home prices, construction, and finance) but not in areas only indirectly related, suggests that the credit shock is carefully identified.

## 5.4 Aggregate Implications

Using a partial equilibrium aggregation exercise, I find that reductions in mortgage supply could explain about 10 to 15 percent of the employment losses in the Great Recession. I use the in-sample variation in the nonlocal lending shock to estimate the total contribution of reductions in mortgage supply to employment declines. First, I choose a county that I will assume did not receive a credit shock, 'county zero'. The counterfactual experiment is to

answer the question, what would employment have been in other counties in 2010 if they had received the credit shock of county zero, instead of their own? The change in credit supply would induce changes in mortgage credit as well as changes in employment, via the elasticity estimates. For this section, I use the point estimates in the total private employment model with region fixed effects.

In particular, I assume that the county in the 99th percentile ('county zero') by the nonlocal lending shock did not experience a shift in supply in the Great Recession. Using county zero as the baseline is a way of circumventing the issue that shifts in the level of mortgage supply cannot be recovered from the cross-section. The validity of the assumption will determine whether the estimates in this section represent an under or over-estimate. For example, if the top counties by credit supply also received a downward shift in supply, the estimates here would be an under-estimate. The collapse in the originate-to-distribute lending model, as well as disruptions in wholesale funding markets, suggest the assumption is conservative.[29]

To begin, define the counterfactual employment change in county $i$, $\Delta Emp_i^{cf}$, as the predicted employment change had county $i$ experienced the nonlocal lending shock of county zero ($NLS_0$), rather than its own ($NLS_i$), after conditioning on all other observables $X_i$:

$$\begin{aligned}\Delta Emp_i^{cf} =& E[\Delta Emp_i | NLS_i = NLS_0, X_i] \\ & \Delta\widehat{Emp_i} + \beta(\Delta\widehat{Credit}_i(NLS_0) - \widehat{Credit}_i(NLS_i)) \\ & \Delta\widehat{Emp_i} + \beta\rho(NLS_0 - NLS_i)\end{aligned}$$

where $\Delta\widehat{Emp_i}$ denotes the fitted value from the private employment regression model with region fixed effects, $\beta$ is the estimated elasticity of employment with respect to mortgage supply, and $\rho$ is the coefficient on the nonlocal lending shock in the first stage regression. I then recover the end-period levels of employment corresponding to both the counterfactual and fitted changes in employment, using the initial-period employment level: $Emp_{i,2010Q4}^{cf} = Emp_{i,2007Q4}(1 + \Delta Emp_i^{cf})$ and $\widehat{Emp}_{i,2010Q4} = Emp_{i,2007Q4}(1 + \Delta\widehat{Emp_i})$. Then, the total jobs

[29]For similar approaches, see Chodorow-Reich (2014) and Mian and Sufi (2014).

explained by variation in the nonlocal lending shock is given by:

$$Total\ jobs\ lost\ explained\ by\ lending\ shock = \sum_{i:NLS_i<NLS_0} [Emp^{cf}_{i,2010Q4} - \widehat{Emp}_{i,2010Q4}] \quad (5)$$

The fraction of jobs lost that is explained by the lending shock is given by:

$$\frac{\sum_{i:NLS_i<NLS_0}[Emp^{cf}_{i,2010Q4} - \widehat{Emp}_{i,2010Q4}]}{\sum_i [Emp_{i,2010Q4} - Emp_{i,2007Q4}]} \quad (6)$$

The exercise indicates that the decline in mortgage supply can explain between 10 and 15 percent of the employment losses in the Great Recession, depending on the assumption made about 'county zero'. When the county in the 99th (95th) percentile is used, reductions in mortgage supply account for 15 (10) percent of the employment losses.

## 5.5 Robustnes

I test for the validity and interpretation of the main results of the paper along several dimensions. Non-random prerecession county-lender matching would be a concern, for example, if subprime lenders moved to subprime counties during the boom years, and those counties are systematically associated with employment declines in the Great Recession. In that case, the prerecession market shares would be endogenous in the sense that they would reveal underlying characteristics of localities that could be systematically associated with local shocks affecting home prices or employment in the recession. This concern is largely addressed by the high persistence in market shares pre-recession. 2000 market shares explain 72 percent of the variation in 2007 market shares (Figure 10). Moreover, I form the credit supply instrument using 2000-2002 market shares (as opposed to 2005-2007 shares as in the baseline – see Figure 11)). The elasticity estimates are very similar, though noisier, with the first stage F statistic also smaller (Table 12). I also show in the Online Appendix that controlling for the fraction of subprime borrowers is another way of addressing this particular concern, as long as the prerecession matching between counties and lenders is linear and is driven by local factors I can observe.

A related concern is that, for historical reasons, regions that performed poorly in the

recession were more likely to contain lenders that cut supply, without their being any causal associations. To check for this, I show that the elasticity estimates are highly consistent when controlling for region, division, state, or no fixed effects. These tables are in the Online Appendix.

I also run 'placebo' tests on the first and second stage equations. First, I regress yearly changes between mortgage credit (2000-2013) at the county-level on the nonlocal lending shock and all the county controls in the baseline case. Figure 12 plots the coefficient estimates and associated 95 percent confidence intervals on a year-by-year basis. The mortgage credit shock helps explain credit changes over 2007-2008 and 2008-2009 only, and not during any of the prerecession years. Second, I repeat the main 2SLS elasticity estimates, but now using left-hand side variables (e.g. employment changes) measured over the labor bust period of 2000-2003 and the housing boom years 2003-2006. Table 13 shows that the elasticity estimates during these earlier periods are not significant.

I also test for instrument validity by splitting the nonlocal lending shock by 'large' and 'small' lenders. The lending shock is the weighted average (by prerecession market shares) of lender shocks in the Great Recession. I partition the lending shock by market share. One instrument is based on large lenders – Bank of America, Wells Fargo, Chase, Citi, Suntrust, and PNC – and the other is based on the remaining lenders in the sample. Doing so splits market shares 53 to 47. The assumption of instrument validity can be tested in an over-identified system based on the correlation between the second-stage residuals and the instruments, which should be close to zero under the null of instrument validity. Table 14 reports results from using both instruments.[30] The instruments pass Hansen's J-test of over-identifying restrictions across models. The point estimates are also very similar to the baseline results reported in the paper.

In the baseline results of the paper, I did not include institutions that filed for bankruptcy (and were not acquired by another lender), because the portion of lending changes that is nonlocal cannot be plausibly isolated for these lenders, since lending for these institutions fell by 100% everywhere (there is no variation). This is a conservative choice. The inclusion

[30]The J test p-values for the null hypothesis that both instruments are valid are .97 and .42 for the delinquency and foreclosure rates models, respectively.

of these lenders might lead to biased elasticity estimates. On the other hand, their exclusion likely decreases the statistical power of the estimation approach. I add to the sample the ten largest multimarket lenders who failed over 2005-2010.[31] Table **??** shows that the addition of the failed institutions does increase the explanatory power of the credit supply instrument (first stage F statistics increase by about 10). The estimated elasticities are essentially the same as in the main results reported in the paper.

I allow for potentially nonlinear prerecession matching between counties and lenders. Across models, the most important prerecession characteristics of localities include the runup in home prices over 2003-2006, 2006 debt to income, and the fraction of borrowers in a county with FICO scores less than 620. In the Online Appendix I report the main regressions of the paper (with region fixed effects), this time including as additional explanatory variables the squared and cubed prerecession versions of these three variables. This controls for the extent to which counties and lenders might have been matched along these local characteristics. The results are essentially identical, ameliorating concern about nonlinear matching.

Finally, I show that the results in the paper are robust to controlling for realized declines in small business lending over 2007-2010, which I obtained from the Community Reinvestment Act dataset. I average the flow of new business originations over 2008-2010, and compute percent changes with respect to 2007. The CRA table in the Online Appendix shows that controlling for the change in small business lending does not affect the main results of the paper. This implies that the mortgage credit shock discussed in this paper is carefully identified, and pertains specifically to changes in the availability of mortgage credit.

# 6 Conclusion

One of the leading narratives of the Great Recession is the credit crunch view – disruptions in financial markets limited the supply of new credit, which reduced the spending capacity of households and firms, and lowered aggregate demand and employment, as discussed in prominent models of the Great Recession (Eggertsson and Krugman 2012; Guerrieri and

[31] American Home Mortgage, New Century Financial, IndyMac, Fremont Investment, WMC Mortgage, Lehman, Ameriquest, Option One, First Magnus, and Taylor, Bean, & Whitaker Mortgage.

Lorenzoni 2017; Midrigan and Philippon 2016). The gap in the literature is that there is little empirical evidence quantifying this channel, particularly for reductions in household credit supply. This paper fills that gap by attempting to quantify the extent to which employment losses in the Great Recession can be attributed to declines in credit supply for home purchase mortgages, the largest category of household credit.

To do so, I construct a measure of mortgage credit supply at the county-level that is plausibly exogenous to contemporaneous local shocks affecting employment and home prices. The credit supply instrument is based on two sources of variation: differences in the extent to which lenders cut supply in the Great Recession for nonlocal reasons, and variation in the intensity of county-lender relations coming into the recession.

The paper finds that reductions in mortgage supply mattered for employment in the Great Recession. A 10 percent reduction in mortgage credit that is supply-driven and plausibly exogenous to local shocks is associated with a 1 percent decline in total private employment. The job losses are concentrated in construction and financial employment, where about a third of employees are real estate intermediaries. Overall, reductions in mortgage supply could explain about 10 to 15 percent of the employment losses in the Great Recession, or 750 thousand to 1.1 million of the jobs lost.

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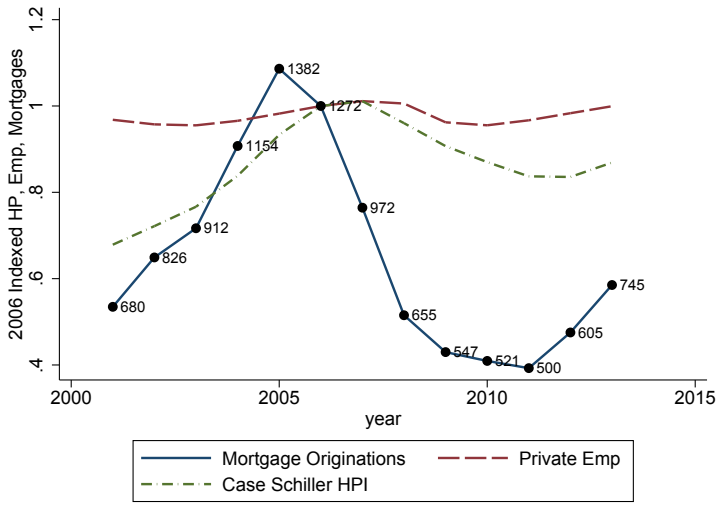
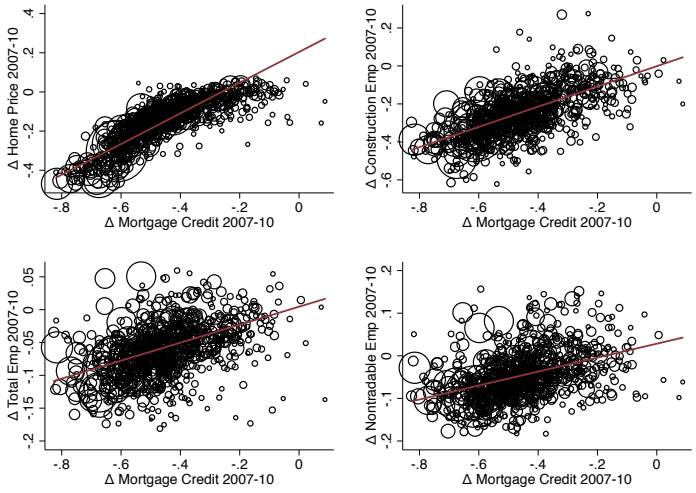
## Figure 1: National Trends in Employment, House Prices, and Mortgage Originations



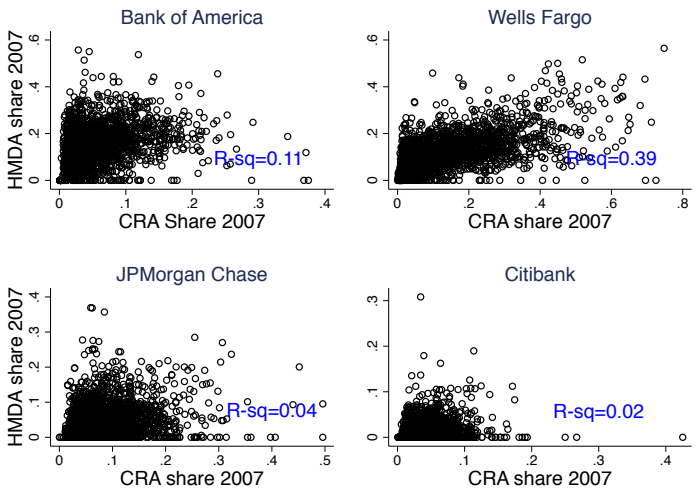

Mortgage originations are defined as the dollar amount of originations for 1-4 residential loans for home purchase and improvement, and are obtained from HMDA. The dollar value is in trillions of dollars.

## Figure 2: Association between Changes in Mortgage Credit and Local Outcomes Across U.S. Counties in the Great Recession



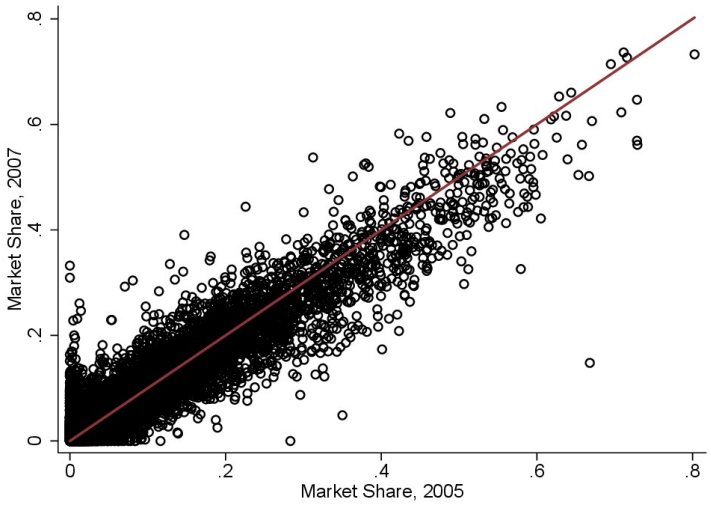

Data for largest 850 counties. Each county is weighted by the number of employed workers in 2006. Variables measure percent changes over 2008-2010 with respect to a prerecession period. For employment and home prices (stocks), changes are taken between 2007q4 and 2010q4. For mortgage credit (flow), the change is taken between the average flow over 2008-2010 and the value in 2007.

## Figure 3: County-Lender Market Shares in HMDA and CRA



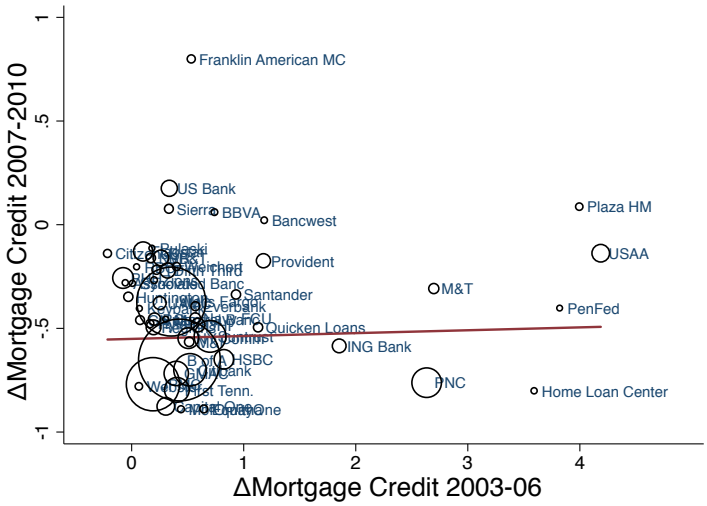

Mortgage market shares and small business loan market shares obtained from HMDA and CRA, respectively.

## Figure 4: Persistent Market Shares



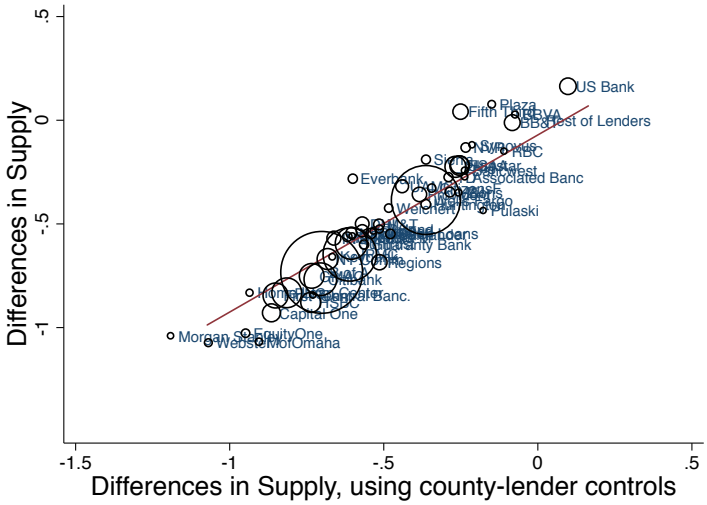

This plot shows the relation between county-lender mortgage market shares in 2005 (x-axis) and 2007 (y-axis). The lenders in the sample are large multimarket lenders located in at least 100 counties and with originations in excess of $1 billion in 2007. Counties in the sample had at least 3 lenders in 2005. Institutions that failed in 2005-2007 are dropped from the sample, as described in the text.

## Figure 5: Credit changes 2007-2010 versus changes in 2003-2006



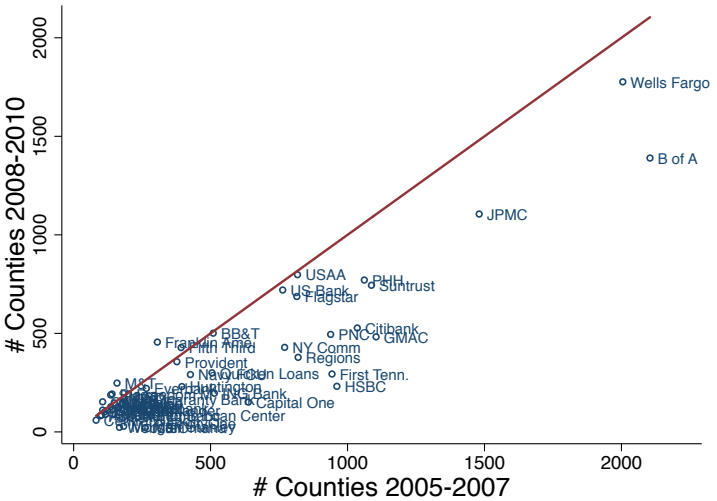

The figure plots changes in mortgage credit over 2007-2010 versus changes in mortgage credit over 2003-2006. The lenders in the sample are large multimarket lenders located in at least 100 counties and with originations in excess of $1 billion in 2007 who did not fail.

## Figure 6: Estimated Differences in Lender Supply



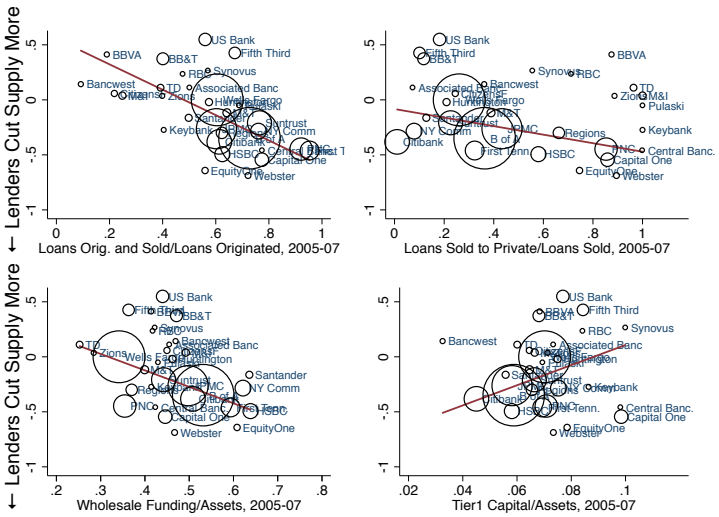

The variables on the y-axis and x-axis measures differences in the extent to which lenders cut supply 2007-2010, after controlling for variation in their exposure to local economies. The variable on the x-axis also controls for county-lender observables: income, subprime, owner-occupier, and race. Observations in the graph are weighted by the dollar value of prerecession originations.

## Figure 7: Number of Counties 2008-10 vs 2005-2007



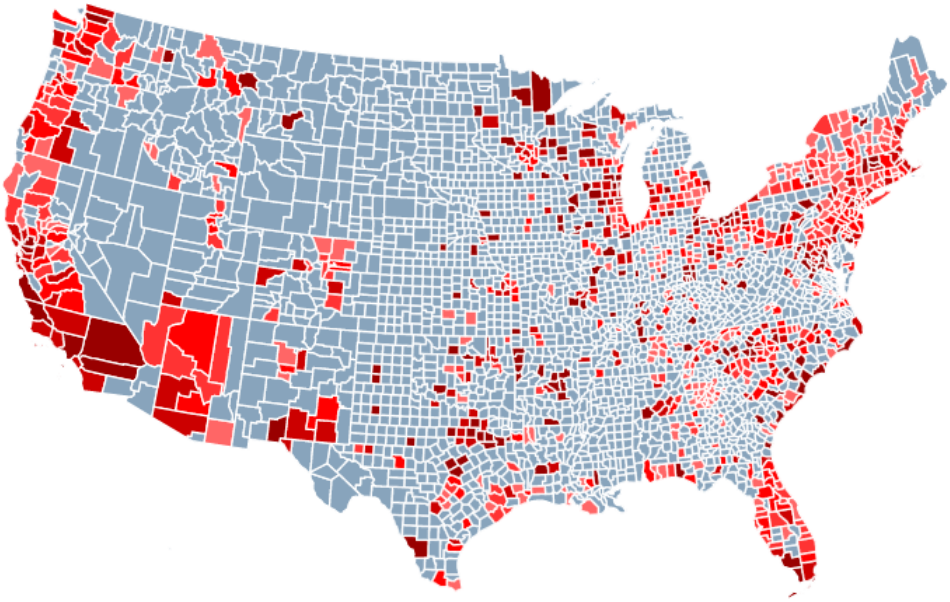

The figure plots the number of counties each lender was operating (with purchase mortgages exceeding 1 million dollars) in 2008-2010 versus 2005-2007. The lenders in the sample are large multimarket lenders located in at least 100 counties and with originations in excess of $1 billion in 2007 who did not fail.

## Figure 8: Funding Fragility and Lender Supply



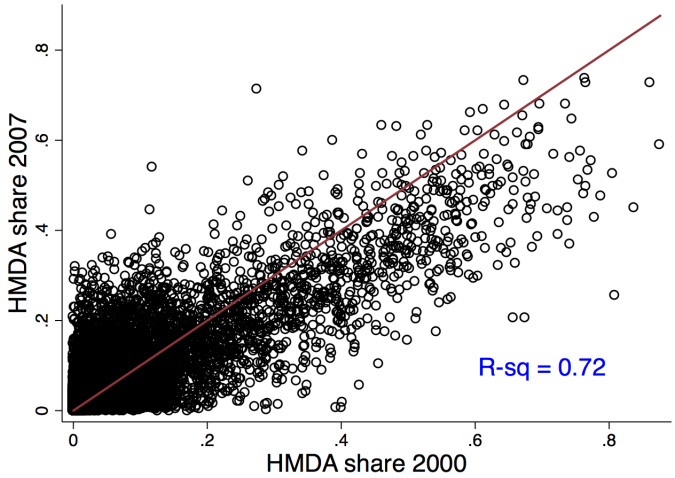

The variable on the y-axis measures differences in the extent to which lenders cut supply 2007-2010, after controlling for variation in their exposure to local economies. Variables on the x-axis are different measures of funding fragility over 2005-2007: ratio of mortgages originated and sold to total mortgages originated (top left); loans sold to private investors to total sales (top right); wholesale funding to assets (bottom left); and Tier 1 capital to assets (bottom right). Observations weighted by mortgage originations in 2007. The banks in the sample are large multimarket lenders located in at least 100 counties and with originations in excess of $1 billion in 2007.

## Figure 9: Nonlocal Lending Shock



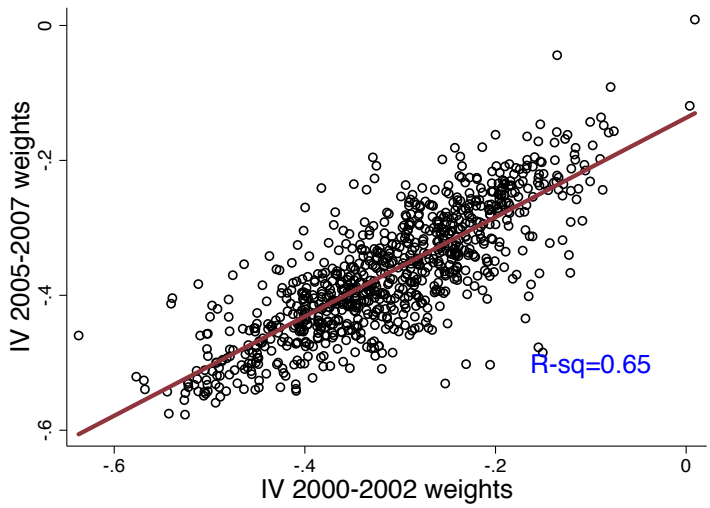
The Nonlocal Lending Shock measures the exposure of counties to lender shocks (see text for details). This shows the residual variation in the nonlocal lending shock after controlling for local prerecession characteristics. The map sorts the lending shock into quintiles for the largest 856 counties. Darker shades of red indicate stronger supply.

## Figure 10: Persistent Market Shares




This plot shows the relation between county-lender mortgage market shares in 2000 (x-axis) and 2007 (y-axis). The lenders in the sample are large multimarket lenders located in at least 100 counties and with originations in excess of $1 billion in 2007.

## Figure 11: Nonlocal Lending Shock using 2000-2002 Market Shares




The credit supply IV (the nonlocal lending shock) measures the exposure of counties to lender shocks (see text for details). The baseline measure is on the y-axis, based on 2005-2007 market shares. The x-axis is the IV constructed using 2000-2002 market shares.

## Figure 12: Regressing Yearly Mortgage Credit Changes on Nonlocal Lending Shock

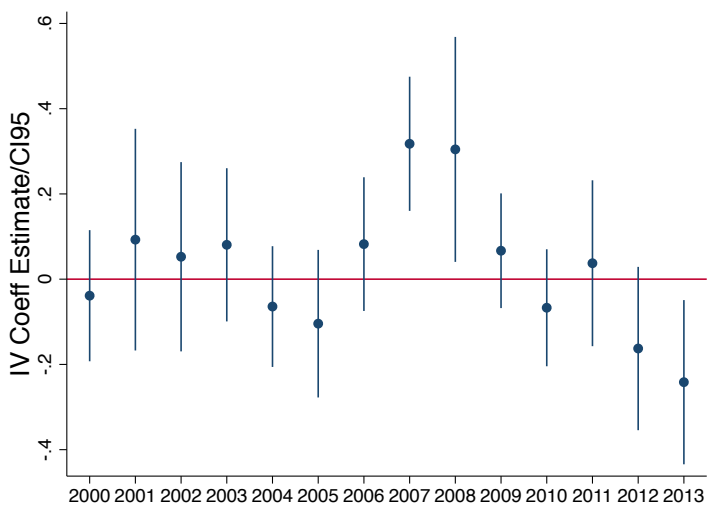



Figure shows coefficient estimates ($\rho_t$) and 95 percent confidence intervals when regressing yearly mortgage credit changes at the county-level on the nonlocal lending shock $NLS_i$ and the other controls used in the baseline specification: $\Delta Credit_{i,t} = \rho_t NLS_i + \gamma X_i + \upsilon_i$ for $t = 2000, 2001, ...2013$

## Table 1: County Summary Statistics

| *Great Recession Outcomes, 2007-2010 percent changes* | Mean | SD | p10 | Median | p90 |
|---|---|---|---|---|---|
| Δ Private Emp | -0.06 | 0.04 | -0.12 | -0.06 | -0.01 |
| Δ Construction Emp | -0.24 | 0.13 | -0.41 | -0.25 | -0.07 |
| Δ Finance Emp | -0.08 | 0.08 | -0.18 | -0.09 | 0.02 |
| Δ Other Emp | -0.04 | 0.04 | -0.10 | -0.05 | 0.01 |
| Δ Nontradable Emp | -0.04 | 0.06 | -0.11 | -0.05 | 0.03 |
| Δ Home Sales | -0.35 | 0.14 | -0.52 | -0.36 | -0.19 |
| Δ Home Prices | -0.15 | 0.12 | -0.32 | -0.13 | -0.01 |
| Δ Delinquency +90 Days | .041 | 0.027 | 0.019 | 0.033 | .104 |
| Δ Foreclosures | .016 | 0.014 | .006 | .011 | .029 |
| Δ Mortgage Credit | -0.44 | 0.15 | -0.63 | -0.46 | -0.24 |
| *Prerecession Characteristics, 2006 levels and 2003-2006 percent changes* | | | | | |
| Δ Home Prices | 0.25 | 0.18 | 0.06 | 0.20 | 0.51 |
| Δ Mortgage Credit | 0.43 | 0.36 | 0.03 | 0.39 | 0.92 |
| Debt to Income | 1.79 | 0.59 | 1.17 | 1.65 | 2.62 |
| Median Income (thousands) | $48 | $12 | $37 | $45 | $66 |
| Median FICO | 710 | 32 | 660 | 717 | 747 |
| % FICO <620 | 0.27 | 0.08 | 0.17 | 0.26 | 0.39 |
| % Owner-Occupied Loans | 0.85 | 0.08 | 0.75 | 0.87 | 0.92 |
| % Conventional Loans | 0.82 | 0.11 | 0.67 | 0.83 | 0.95 |
| % GSE-securitized Loans | 0.66 | 0.14 | 0.49 | 0.69 | 0.78 |
| Construction Share of Emp | 0.12 | 0.04 | 0.08 | 0.11 | 0.18 |
| Tradable Share of Emp | 0.13 | 0.08 | 0.04 | 0.12 | 0.23 |
| Δ #Lenders | -0.21 | 0.10 | -0.35 | -0.21 | -0.08 |
| % White Population | 0.86 | 0.13 | 0.69 | 0.90 | 0.98 |
| % Educ ≥ College | 0.15 | 0.05 | 0.08 | 0.13 | 0.22 |

For prerecession characteristics, level variables are measured in 2006 while percent changes are taken over 2003-2006 with 2003 as the base year. The change in delinquency and foreclosure rates is in percentage points. For stocks, changes are taken between 2010Q4 and 2007Q4. For flow variables, that change is taken between the average flow over 2008-2010 and the value in 2007.

## Table 2: Lender Summary Statistics

| | Mean | SD | p10 | Median | p90 | N |
|---|---|---|---|---|---|---|
| Δ Mortgage Credit 2007-2010 | -0.38 | 0.30 | -0.79 | -0.37 | 0.01 | 56 |
| #Counties 2007 | 487 | 475 | 121 | 279 | 1,117 | 56 |
| Mortgage Credit 2007 (billions) | $12.12 | $30.76 | $1.08 | $2.48 | $24.39 | 56 |
| Loan Sales/Loans Originated 2005-2007 | 0.68 | 0.25 | 0.36 | 0.70 | 0.99 | 56 |
| Loans Sold to Private Investors/Loans Sold 2005-2007 | 0.60 | 0.37 | 0.08 | 0.64 | 1.00 | 56 |
| Wholesale Funding/ Assets 2005-2007 | 0.44 | 0.11 | 0.34 | 0.44 | 0.61 | 31 |
| Tier 1 Capital/Assets 2005-2007 | 0.07 | 0.01 | 0.06 | 0.07 | 0.09 | 31 |

This table provides summary statistics for the lenders in the sample, which are large multimarket lenders located in at least 100 counties and with originations in excess of $1 billion in 2007.

## Table 3: Data Definitions

| *Variable* | *Definition* | *Source* |
|---|---|---|
| *Great Recession Outcomes, 2007-2010 percent changes* | | |
| Mortgage Credit | By county-year, the dollar amount of originations for 1-4 residential loans for home purchase and improvement. | HMDA |
| $\Delta$ Credit | Percent change in average mortgage credit over 2008-2010 with respect to 2005-2007 | HMDA |
| $\Delta$ Residential Permits | Percent change in average permits over 2008-2010 with respect to 2005-2007 | Census |
| $\Delta$ House Prices | Percent change in house prices from 2007Q4 to 2010Q4. | CoreLogic HPI |
| $\Delta$ Emp$^j$ | Percent change in employment category $j$ from 2007Q4 to 2010Q4 | QCEW |
| *Prerecession Characteristics, 2006 levels and 2003-2006 percent changes* | | |
| Household Income | Median | IRS |
| FICO score | Median | Equifax CCP |
| Subprime | Fraction of households in a county with FICO score less than 620) | Equifax CCP |
| Household Debt-to-Income | Ratio of debt to income, from the web appendix to | Mian and Sufi (2014) |
| White population | Fraction of population identified as white | Census |
| College population | Fraction of population with a college degree or more | Census |
| Nonconventional Loans | One minus the fraction of loans issued over 2003-2006 identified as conventional loans | McDash |
| GSE-securitized Loans | Fraction of loans issued over 2003-2006 insured by GNMA, FNMA, or FHLMC | McDash |
| Owner-Occupied Loans | Fraction of mortgages over 2003-2006 identified as owner-occupied | HMDA |
| Herfindahl Index | Sum of squared market shares across lenders in county | HMDA |
| $\Delta$ # Lenders | Growth in the number of lenders per county over 2003-2006 | HMDA |
| $\Delta$ House Prices | Growth in house prices over 2003Q4-2006Q4 | CoreLogic HPI |
| Construction | Construction share of employment | QCEW |
| Tradable | Tradable share of employment, where tradable employment is defined as in Mian and Sufi (2014) | CBP |
| Unemp Rate | Unemployment Rate | BLS LAU |
| Level Home Prices | Log level median house price | Census |
| Level Employment | Log level of employed workers | QCEW |
| Level Mortgage Credit | Log level of mortgage originations | HMDA |

This table provides definitions and sources for the data used throughout the paper. Unless otherwise specified, prerecession level variables are measured in 2006 while growth rates are taken over 2003-2006. Outcome variables are in percent changes over 2008-2010 with respect to a prerecession period. For stocks, changes are taken between 2010Q4 and 2007Q4. For flow variables, that change is taken between the average flow over 2008-2010 and the value over 2005-2007.

## Table 4: Mortgage Market Shares are Highly Persistent Year-on-Year

| | Dependent variable: Market Share 2007 | | | |
|---|---|---|---|---|
| | | Top FICO quartile | Bottom FICO quartile | |
| Market Share 2005 | .907*** | .916*** | .911*** | .906*** |
| | (.001) | (.002) | (.003) | (.002) |
| County FE | No | No | No | Yes |
| N | 27,624 | 6,906 | 6,912 | 27,624 |
| R-squared | 0.91 | 0.92 | 0.89 | 0.91 |

The dependent (explanatory) variable is the 2007 (2005) mortgage origination market shares (in dollars) by county-lender. The lenders in the sample are large multimarket lenders located in at least 100 counties and with originations in excess of $1 billion in 2007. Counties in the sample had at least 3 lenders in 2005.

## Table 5: Funding Fragility and Differences in Lender Supply

| | Dependent variable: $\phi^b$ | | | |
|---|---|---|---|---|
| | (1) Coef./SE | (2) Coef./SE | (3) Coef./SE | (4) Coef./SE |
| Wholesale Debt/Assets 2005-2007 | -0.356*** | -0.267*** | -0.220** | -0.424*** |
| | (0.071) | (0.074) | (0.084) | (0.124) |
| Loan Sales/Originations 2005-2007 | -0.491*** | -0.537*** | -0.646*** | -0.662*** |
| | (0.123) | (0.114) | (0.146) | (0.134) |
| Private Loan Sales/Originations 2005-2007 | -0.380*** | -0.452*** | -0.528*** | -0.559*** |
| | (0.113) | (0.108) | (0.125) | (0.128) |
| Tier1 Capital 2005-2007 | | 0.276** | 0.330** | 0.336** |
| | | (0.113) | (0.121) | (0.145) |
| $\Delta$ Mortgage Credit 2003-2006 | | | 0.126 | 0.061 |
| | | | (0.108) | (0.127) |
| Weighted | Yes | Yes | Yes | No |
| N | 31 | 31 | 31 | 31 |
| R-squared | 0.72 | 0.77 | 0.79 | 0.68 |
| Adj R-squared | 0.69 | 0.74 | 0.74 | 0.62 |

The dependent variable measures differences in the extent to which lenders cut supply 2007-2010, after controlling for variation in their exposure to local economies (lender fixed effects coefficients estimated in equation 1; more negative coefficients indicate weaker supply). The explanatory variables measure the extent to which banks relied on fragile funding sources over 2005-2007, and credit growth over 2003-2006. Standard errors are in parenthesis. Banks in the sample were located in at least 100 counties and issued mortgages in excess of $1 billion in 2007. All variables are standardized.

## Table 6: Lender rankings before and after controlling for variation in local market exposure.

| Lender | Δ Originations, 2007-2010 | Rank | Rank, Net of County FE | Originations, $billions 2007 |
|---|---|---|---|---|
| US Bank | 17% | 2 | 2 | 7,449 |
| Bank of the West | 1% | 6 | 15 | 1,260 |
| Flagstar | -13% | 7 | 13 | 10,368 |
| Pulaski Mtg Co. | -15% | 9 | 27 | 1,029 |
| Citizens Financial | -15% | 10 | 20 | 1,876 |
| BB&T | -15% | 11 | 7 | 6,836 |
| Provident Funding | -18% | 13 | 23 | 5,644 |
| Toronto Dominion | -21% | 16 | 17 | 2,211 |
| Fifth Third | -21% | 17 | 4 | 6,412 |
| M&T | -34% | 24 | 29 | 3,093 |
| Wells Fargo | -37% | 27 | 24 | 129,800 |
| Everbank | -37% | 28 | 18 | 2,307 |
| UAMC | -38% | 29 | 19 | 4,522 |
| Navy FCU | -43% | 30 | 33 | 3,300 |
| Quicken Loans | -45% | 33 | 34 | 2,838 |
| Pulte | -45% | 34 | 31 | 4,047 |
| Freedom Mtg | -47% | 36 | 37 | 2,463 |
| DHI | -48% | 38 | 28 | 5,086 |
| Regions | -49% | 39 | 44 | 6,305 |
| Suntrust | -53% | 40 | 39 | 27,855 |
| NY Community | -54% | 41 | 43 | 12,011 |
| M&I | -55% | 42 | 21 | 2,546 |
| HSBC | -65% | 44 | 52 | 10,888 |
| Bank of America | -65% | 45 | 45 | 182,100 |
| Citibank | -69% | 46 | 47 | 29,109 |
| Ally Financial | -71% | 47 | 46 | 16,627 |
| PNC | -76% | 49 | 49 | 24,105 |
| JPMorgan Chase | -76% | 50 | 41 | 77,644 |
| First Tennessee | -79% | 52 | 51 | 17,049 |
| Capital One | -86% | 53 | 53 | 8,817 |
| Median across Lenders | -37% | | | $2,477 |
| Standard Deviation | 30% | | | $30,656 |

Rank (Column 2) is out of the 56 lenders in the sample, which are lenders with over $1 billion in mortgage originations and with loans exceeding $1 million in at least 100 counties, in 2007. Rank Net of County FE (Column 3) shows the ranking after controling for variation in local exposure. Lenders in the sample were located in at least 100 counties and issued mortgages in excess of $1 billion in 2007.

## Table 7: The Explanatory Power of the Nonlocal Lending Shock

| | Dependent variable: Δ Mortgage Credit 2007-2010 | | | |
|---|---|---|---|---|
| | *No FE* Coef./SE | *Region FE* Coef./SE | *Division FE* Coef./SE | *State FE* Coef./SE |
| Nonlocal Lending Shock | 0.475*** | 0.539*** | 0.425*** | 0.271*** |
| | (0.10) | (0.09) | (0.08) | (0.09) |
| All other controls | Yes | Yes | Yes | Yes |
| Adj. R-squared | 0.60 | 0.67 | 0.71 | 0.80 |
| R-squared | 0.59 | 0.66 | 0.70 | 0.78 |
| First stage F stat | 21.52 | 34.42 | 26.68 | 9.24 |
| Observations | 859 | 859 | 859 | 859 |

This table shows the effects of changes in mortgage credit, when instrumented using the nonlocal lending shock, on changes in local outcomes for the largest 850 U.S. counties. The nonlocal lending shock measures the exposure of counties to lender shocks (see text for details). Observations weighted by the number of employed workers in 2006. The dependent variable is winsorized 1 percent in each tail. Standard errors clustered by state. *, **, and *** indicate significance at the 0.1, 0.05, and 0.01 levels, respectively.

## Table 8: OLS Associations

| | Dependent variables: | | | | |
|---|---|---|---|---|---|
| | Δ Permits Coef./SE | Δ Constr. Emp Coef./SE | Δ Total Emp Coef./SE | Δ Other Emp Coef./SE | Δ Nontr. Emp Coef./SE |
| Δ Mortgage Credit 2007-2010 | 0.881*** | 0.367*** | 0.136*** | 0.113*** | 0.084*** |
| | (0.14) | (0.06) | (0.02) | (0.02) | (0.03) |
| All other controls | Yes | Yes | Yes | Yes | Yes |
| R-squared | 0.51 | 0.66 | 0.47 | 0.35 | 0.38 |
| Adj. R-squared | 0.50 | 0.65 | 0.45 | 0.33 | 0.36 |
| Observations | 837 | 822 | 842 | 843 | 843 |

This table shows that the OLS association between changes in mortgage credit and changes in local employment and home prices over 2007-2010, when controlling for all other county covariates. All regressions include region fixed effects and all other observed characteristics of localities used in the other tables in the paper. Observations weighted by the number of employed workers in 2006. Standard errors clustered by state. *, **, and *** indicate significance at the 0.1, 0.05, and 0.01 levels, respectively.

## Table 9: Housing Elasticities with respect to Mortgage Supply

| | Dependent variables 2007-2010: | | | |
|---|---|---|---|---|
| | Δ Permits Coef./SE | Δ Home Price Coef./SE | Δ Delinq. Rate Coef./SE | Δ Foreclosure Rate Coef./SE |
| Instrumented | | | | |
| Δ Credit 2007-10 | 1.064*** | 0.531*** | -0.128*** | -0.081*** |
| | (0.23) | (0.12) | (0.03) | (0.03) |
| All other controls | Yes | Yes | Yes | Yes |
| Region fixed effects | Yes | Yes | Yes | Yes |
| R-squared | 0.51 | 0.84 | 0.80 | 0.64 |
| First stage F stat | 34.84 | 35.02 | 31.18 | 30.81 |
| Observations | 837 | 843 | 847 | 845 |

This table shows the effects of changes in mortgage credit, when instrumented using the nonlocal lending shock, on changes in local outcomes for the largest 850 U.S. counties. The nonlocal lending shock measures the exposure of counties to lender shocks (see text for details). All regressions include region fixed effects and all other observed characteristics of localities used in the other tables in the paper. Observations weighted by the number of employed workers in 2006. The dependent variable is winsorized 1 percent in each tail. Standard errors clustered by state. *, **, and *** indicate significance at the 0.1, 0.05, and 0.01 levels, respectively.

## Table 10: Employment Elasticities with respect to Mortgage Supply

| | Dependent variables 2007-2010: | | | | |
|---|---|---|---|---|---|
| | Δ Constr. Emp Coef./SE | Δ Fin Emp Coef./SE | Δ Total Emp Coef./SE | Δ Other Emp Coef./SE | Δ Nontr. Emp Coef./SE |
| Instrumented | | | | | |
| Δ Credit 2007-10 | 0.428*** | 0.396*** | 0.104** | 0.040 | 0.047 |
| | (0.10) | (0.06) | (0.05) | (0.05) | (0.09) |
| All other controls | Yes | Yes | Yes | Yes | Yes |
| State fixed effects | Yes | Yes | Yes | Yes | Yes |
| R-squared | 0.66 | 0.17 | 0.47 | 0.33 | 0.38 |
| First stage F stat | 35.34 | 34.40 | 33.90 | 34.13 | 34.16 |
| Observations | 822 | 843 | 842 | 843 | 843 |

This table shows the effects of changes in mortgage credit, when instrumented using the nonlocal lending shock, on changes in local outcomes for the largest 850 U.S. counties. The nonlocal lending shock measures the exposure of counties to lender shocks (see text for details). All regressions include region fixed effects and all other observed characteristics of localities used in the other tables in the paper. Observations weighted by the number of employed workers in 2006. The dependent variable is winsorized 1 percent in each tail. Standard errors clustered by state. *, **, and *** indicate significance at the 0.1, 0.05, and 0.01 levels, respectively.

## Table 11: Elasticity of Construction Employment with Housing Supply Interaction

| | Dependent variables 2007-2010: | | | |
|---|---|---|---|---|
| | Δ Permits Coef./SE | Δ Permits Coef./SE | Δ Constr. Emp Coef./SE | Δ Constr. Emp Coef./SE |
| $\Delta\widehat{Credit}$ 2007-10 | 1.127*** | 1.158*** | 0.412*** | 0.420*** |
| | (0.26) | (0.25) | (0.09) | (0.09) |
| $\Delta\widehat{Credit}$ 2007-10 × Elasticity | | 0.315** | | 0.089* |
| | | (0.14) | | (0.05) |
| All other controls | Yes | Yes | Yes | Yes |
| R-squared | 0.57 | 0.57 | 0.69 | 0.70 |
| First stage F stat | 33.83 | 15.84 | 33.93 | 16.66 |
| Observations | 478 | 478 | 491 | 491 |

This table shows the effects of changes in mortgage credit, when instrumented using the nonlocal lending shock, on changes in local outcomes for the largest 850 U.S. counties. The nonlocal lending shock measures the exposure of counties to lender shocks (see text for details). All regressions include region fixed effects and all other observed characteristics of localities used in the other tables in the paper. Observations weighted by the number of employed workers in 2006. The dependent variable is winsorized 1 percent in each tail. Standard errors clustered by state. *, **, and *** indicate significance at the 0.1, 0.05, and 0.01 levels, respectively.

## Table 12: Elasticities with IV constructed using 2000-2002 Shares

| | Dependent variables 2007-2010: | | | | |
|---|---|---|---|---|---|
| | Δ Permits Coef./SE | Δ Constr. Emp Coef./SE | Δ Total Emp Coef./SE | Δ Other Emp Coef./SE | Δ Nontr. Emp Coef./SE |
| Instrumented | | | | | |
| Δ Credit 2007-10 | 0.892*** | 0.277* | 0.071 | 0.009 | -0.050 |
| | (0.28) | (0.16) | (0.05) | (0.06) | (0.13) |
| All other controls | Yes | Yes | Yes | Yes | Yes |
| R-squared | 0.51 | 0.65 | 0.46 | 0.31 | 0.35 |
| First stage F stat | 19.54 | 20.04 | 19.96 | 20.07 | 19.62 |
| Observations | 837.00 | 822.00 | 842.00 | 843.00 | 843.00 |

This table shows the effects of changes in mortgage credit over 2007-2010, when instrumented using the nonlocal lending shock based on 2000-2002 market shares (as opposed to the baseline measure which uses 2005-2007 shares). All regressions include region fixed effects and all other observed characteristics of localities used in the other tables in the paper. The nonlocal lending shock measures the exposure of counties to lender shocks (see text for details). Each observation is weighted by the number of employed workers in 2006. Standard errors clustered by state. *, **, and *** indicate significance at the 0.1, 0.05, and 0.01 levels, respectively.

## Table 13: 'Placebo' Regressions over 2000-2003 and 2003-2006

| | Δ Total 00-03 Coef./SE | Δ Total 03-06 Coef./SE | Δ Constr 00-03 Coef./SE | Δ Constr 03-06 Coef./SE |
|---|---|---|---|---|
| Instrumented | | | | |
| Δ Credit 2007-10 | 0.064 | -0.082 | 0.202 | -0.196 |
| | (0.06) | (0.08) | (0.17) | (0.14) |
| All other controls | Yes | Yes | Yes | Yes |
| Observations | 806 | 838 | 805 | 822 |

This table reports results from a 'placebo' regression. The dependent variables are in percent change over 2000-2003 and 2003-2006. All regressions include region fixed effects and all other observed characteristics of localities used in the other tables in the paper. Each observation is weighted by the number of employed workers in 2006. Standard errors clustered by state. *, **, and *** indicate significance at the 0.1, 0.05, and 0.01 levels, respectively.

## Table 14: Over Identification Tests with Large and Small Lender Instruments

| | Dependent variables 2007-2010: | | | | |
|---|---|---|---|---|---|
| | Δ Permits Coef./SE | Δ Constr. Emp Coef./SE | Δ Total Emp Coef./SE | Δ Other Emp Coef./SE | Δ Nontr. Emp Coef./SE |
| Instrumented | | | | | |
| Δ Credit 2007-10 | 1.077*** | 0.418*** | 0.096** | 0.040 | 0.057 |
| | (0.23) | (0.10) | (0.04) | (0.05) | (0.09) |
| All other controls | Yes | Yes | Yes | Yes | Yes |
| R-squared | 0.51 | 0.66 | 0.46 | 0.33 | 0.38 |
| First stage F stat | 19.07 | 19.53 | 18.64 | 18.82 | 18.84 |
| Hansen's J test, p-value | 0.76 | 0.70 | 0.55 | 0.98 | 0.60 |
| Observations | 837 | 822 | 842 | 843 | 843 |

This table shows the effects of changes in mortgage credit over 2007-2010, when instrumented using two instruments, one based on BoA, WF, JMPC, Citi, Suntrust, PNC, and the other on the remaining lenders. Table reports the J test p-values of the null hypothesis that both instruments are valid. All regressions include region fixed effects and all other observed characteristics of localities used in the other tables in the paper. Observations weighted by the number of employed workers in 2006. Standard errors clustered by state. *, **, and *** indicate significance at the 0.1, 0.05, and 0.01 levels, respectively.