

Household Credit and Employment in the Great Recession

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Abstract

How much did the contraction in the supply of credit to households contribute to the decline in employment during the Great Recession? To answer this question I provide new estimates of: (1) the elasticity of employment with respect to household credit; and (2) the size of the supply shock to household credit. I exploit a county's exposure to the collapse of a large and previously healthy lender as a natural experiment. This gives an estimated elasticity of employment with respect to household credit of 0.3, caused by declines in both housing and non-housing demand. To estimate the size of the credit supply shock I use non-parametric methods to identify lender-specific supply-side shocks, which I then aggregate into a simple measure of credit supply shocks to counties. Combining this measure with estimates of the elasticity of employment with respect to the measure, I calculate that shocks to household credit were responsible for 30 to 60% of the observed decline in employment from 2007 to 2010.

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1 Introduction

U.S. employment fell by over 7% during the Great Recession, but there is substantial uncertainty about the causes of this decline. A leading account argues that the collapse in house prices and household net worth caused a fall in household demand, which then lowered employment.¹ There is also evidence that the financial crisis reduced the supply of credit to firms, which then cut investment and employment.² But much of the theoretical modeling of the Great Recession argues that the primary shock was a decline in the supply of credit to households, which then caused a collapse in demand and employment.³ In spite of this emphasis in the theoretical literature, there has been relatively little empirical evidence on: (1) how strongly employment responded to supply-driven declines in household credit; and (2) the size of the supply-side shock to household credit.⁴ I address these two questions in this paper.

First, I estimate the elasticity of employment with respect to declines in household credit caused by credit supply shocks. I rely on exogenous variation in credit supply across counties due to the collapse of Wachovia, a large and healthy lender before the crisis. I find that contractions in household credit supply caused declines in both housing and non-housing demand. This resulted in significant declines in employment, with losses concentrated in construction and non-tradables. I estimate that a 10% decline in household credit due to supply shocks would cause a 3% decline in total employment. This result shows that shocks to household credit did have important real effects on household demand and employment, as well as providing a well-defined moment that is useful

¹Mian and Sufi [2014] and Mian, Rao and Sufi [2013] provide evidence on the effects of changes in net worth on household demand and employment. Gropp, Krainer and Laderman [2014] also emphasize the local decline in house prices, but focus on its effects on local credit supply. This is distinct from my work as the effects that I identify are not caused by local shocks, but instead reflect shocks from financial intermediaries.

²See Ivashina and Scharfstein [2010] for evidence that banks were contracting their supply of credit during the crisis. Almeida, Campello, Laranjeira and Weisbenner [2009] and Campello, Graham and Harvey [2010] show that contractions in credit to firms affected firm outcomes including investment and employment. Chodorow-Reich [2014] and Greenstone, Mas and Nguyen [2012] estimate the effect of credit shocks on firm outcomes during the crisis in addition to providing partial equilibrium estimates of the size of the firm credit channel.

³Eggertsson and Krugman [2012] model the reduction in aggregate demand that can accompany changes in indebted agents' borrowing constraints. Guerrieri and Lorenzoni [2011] emphasize that changes in borrowing constraints also affect precautionary motives and so lead to large declines in real output. Midrigan and Philippon [2011] highlight the role of liquidity shocks to households. The previous models rely on nominal rigidities in order to generate declines in real output. Huo and Ríos-Rull [2013] show that shocks to financial intermediation combined with search frictions in goods markets can also result in demand shocks causing real output declines.

⁴Recent empirical work on the contraction in credit supply to households in the Great Recession includes Dagher and Kazimov [2012] and Ramcharan, Van den Heuvel and Verani [2012]. However, these papers do not provide evidence on the employment effects or attempt to estimate the size of the supply-side shock itself. Kermani [2012] and DiMaggio and Kermani [2014] are closely related to my work as they provide evidence on the employment effects of increases in the supply of household credit in the years prior to the recession.

in the calibration of structural models. However, this elasticity alone cannot quantify the size of the supply shock to household credit or the fall in employment due to this shock. While learning about the effects of supply shocks to household credit requires only a valid instrumental variable, estimating the magnitude of credit supply shocks to households requires additional structure. The second part of the paper, therefore, adds the minimal structure necessary to provide a quantitative account of the household credit channel.

To estimate the size of the shock, I non-parametrically identify lender-specific supply shocks to household credit using data on lender-county credit flows. My measured shock to a county is then the weighted sum of the lender shocks to an area. I discuss the structure that validates this approach in more detail below. With this shock and the elasticity of employment with respect to this measure, it is straightforward to calculate the direct contribution of the household credit channel to total employment losses. I find that shocks to household credit caused at least a 3.6 percentage point decline in employment, which is about 60% of the observed decline within the estimation sample.⁵

Throughout this paper, I rely on the cross-section of counties to estimate parameters of interest and perform the aggregation. The central requirement for this approach to be informative is that the credit supply shock to a county is a function of the county-specific set of lenders. Thus, if lender A contracts credit by more than lender B, then counties dependent on lender A will suffer a larger credit contraction than counties dependent on lender B. The size of this effect on outcomes of interest depends on the presence of frictions that limit the elasticity of substitution across lenders. So my estimates also shed light on the importance of these frictions in the household credit market.⁶ Given this approach, I will be able to consistently estimate the elasticity of interest so long as I can isolate variation in household credit supply that is not correlated with other factors affecting local employment.

⁵As I explain more fully in Section 2, the elasticity necessary for this accounting (the elasticity of employment with respect to the measured shock) is not the elasticity I estimate in the first part of the paper (the elasticity of employment with respect to household credit quantities). However, I show that it is straightforward to recover this second elasticity using exposure to Wachovia as an instrumental variable.

⁶One potential source of frictions is soft information, which gives rise to sticky lending relationships. The importance of these frictions and relationship lending in firm finance has been well-documented by Berger and Udell [1995] and Petersen and Rajan [1994], among others. The presence of these frictions also underlie the cross-sectional approaches to identifying the effects of firm credit supply shocks in Peek and Rosengren [2000] and Greenstone et al. [2012]. There is less evidence on the importance of these frictions in the household credit market, but Agarwal, Ambrose, Chomsisengphet and Liu [2011] find evidence for the use of soft information in home equity lending.

This exclusion restriction is satisfied by using exposure to Wachovia as a natural experiment. In mid-2006, Wachovia purchased the mortgage lender Golden West Financial to expand its market share in the West and to take advantage of Golden West's expertise in non-traditional mortgages. Wachovia rapidly began experiencing large losses on Golden West's portfolio of high-risk mortgages. Along with the market-wide collapse in liquidity, the losses from Golden West likely caused Wachovia's distressed sale to Wells Fargo (December 2008). Using detailed mortgage application data that allow me to observe the flow of household credit from lenders to counties, I show that Wachovia's distress resulted in a credit contraction across its traditional areas of operation, the South and the East. High-income applicants to Wachovia were 20 percentage points less likely to get a loan relative to similar applicants at non-Wachovia lenders within the same county. Wachovia's declines in origination probabilities for low- and middle-income applicants were over twice as large. Before and after the crisis, Wachovia's behavior was indistinguishable from the average lender, consistent with these origination patterns being caused by the crisis.

The decline in credit from Wachovia was large, but potentially irrelevant if borrowers could easily access credit at another lender. Within Wachovia's traditional markets, I find that an increase in initial exposure to Wachovia of one percentage point resulted in a decline in household credit from 2007 to 2010 of about 2.4%.⁷ This reflects a direct decline in lending from Wachovia and indirect local equilibrium effects such as the resulting changes in house prices and income. Exposure to Wachovia also led to declines in nondurable expenditures, house prices, and the volume of house sales. Construction employment responded most strongly to Wachovia exposure, followed by non-tradables, but there was no significant effect on tradable employment. Even after controlling for changes in construction employment, exposure to Wachovia had large effects on non-tradable employment, indicating the contraction in credit affected non-housing demand directly.

To check for omitted variables bias, I show that exposure to Wachovia is not correlated with common alternative explanations for the decline in employment. In particular, my estimates are robust to controlling for changes in firm credit, household leverage, the house price boom, exposure to Golden West, subprime lending, trade shocks, and labor demand shocks from the real estate, finance, and construction sectors. In further support of the exclusion restriction, Wachovia's market

⁷I measure household credit growth as growth in the flow of non-refinance mortgages and exposure to Wachovia is the average of Wachovia's market share in non-refinance mortgage flows from 2005-2006. I discuss this measure in more detail in Sections 3 and 4.

share was very stable across the 2000s and therefore unlikely to reflect selection based on house price or household debt dynamics. Exposure to Wachovia is largely uncorrelated with any pre-crisis trends in house prices, employment, and household credit. To summarize, exposure to Wachovia appears to be a shock to household credit supply that was orthogonal to other local factors.⁸

Using a county's exposure to Wachovia as an instrument for household credit supply gives the employment elasticity of 0.3 reported above. To the best of my knowledge, these are the first estimates of the effects of supply shocks to household credit on employment during the Great Recession. This elasticity is similar to that from DiMaggio and Kermani [2014] (about 0.2), which is estimated using data from the pre-crisis period and with alternative variation. Given that household credit declined by over 40% during the Great Recession, the household credit channel's contribution to employment is potentially very large. However, the observed decline in household credit likely reflects shifts in both credit demand and credit supply. So, quantitatively accounting for the contribution of the household credit channel to employment losses requires a measure of the supply-side shock to credit.

In the second part of this paper, I construct and use such a measure. Relying on insights from index theory, I assume a flexible functional form for the price of household credit in a county. Doing so implies the functional form for the supply shock to household credit: the true supply shock to a county is the weighted average of the passed-through lender-specific cost shocks in that county. Constructing a measure of the shock then reduces to measuring these lender cost shocks. I build on the approach of Greenstone et al. [2012], and exploit the fact that many lenders operate in multiple counties, and that the households in counties borrow from multiple lenders.⁹ This allows me to use growth in lender-county credit flows to estimate a fixed effect for each lender and each county. A lender fixed effect is then the average change in credit flows for that lender across all borrower relationships, conditional on all other fixed effects. I expect this statistic to reflect whether or not a lender is expanding or contracting credit to borrowers.

⁸In addition to providing exogenous variation in credit supply to households, the Wachovia natural experiment also contributes to our understanding of the effects of bank failures and the policy responses to bank failures (Ashcraft [2005], Giannetti and Simonov [2013]). Even though Wachovia was purchased by a relatively well-capitalized bank in an organized sale, the purchase did not prevent a deep contraction in credit that had real effects on the local economy.

⁹Related strategies have also been employed by Amiti and Weinstein [2013], Chodorow-Reich [2014], and Niepmann and Schmidt-Eisenlohr [2013]. The intellectual precursor to this strategy within this literature is Khwaja and Mian [2008] in that they used borrower-lender variation to control for demand shocks, although they do not use it to construct supply-side shocks.

Using a simple model of credit markets with monopolistic competition, I provide a structural interpretation of these fixed effects. This also allows me to lay out and check identification conditions that ensure a fixed effect is a measure of the lender’s cost shock and, critically, is purified of demand shocks. The estimated fixed effects allow me to measure any cost shock to lenders so long as it affects equilibrium credit quantities. In other words, the fixed effect measure of cost shocks is agnostic about the source of the cost shock. Alternative approaches rely on a proxy for a specific cost shock (for example, liquidity measures or exposure to Lehman Brothers) and so will only recover cost shocks related to that variable.¹⁰ In these cases, it is generally unknown whether or not that specific proxy captures the majority of cost shocks affecting lenders. Therefore, the fixed effects provide a more complete measure of the cost shocks affecting each lender, which is critical for the purpose of accounting.

I construct the household credit supply shock to a county by weighting the lender fixed effects with each lender’s market share in the county and then taking the sum. I show that the measured shock constructed in this way will only reflect the true supply-side shock to household credit plus measurement error. Thus, I have a measure of the shock to household credit that can be used for quantitative accounting given certain assumptions. I define the aggregate direct contribution of the household credit channel as the weighted sum of measured shocks across counties multiplied by the elasticity of employment with respect to the measured shock. This quantity gives the aggregate effect of credit supply shocks conditional on shutting down spillovers across counties. Because credit supply shocks are not randomly distributed across counties and because I am using a noisy measure of the true shock, direct estimates of the shock’s effect will likely be inconsistent. I correct for these issues by again using exposure to Wachovia as an instrumental variable. Consistent with results above, my estimates show that shocks to household credit had significant effects on employment, and that direct estimates might be significantly biased downward. I construct a lower bound to the aggregate direct contribution to correct for measurement error.¹¹ As mentioned above, supply shocks to household credit caused a decline in employment of at least 3.6% within the estimation sample of counties in the South and East (60% of the observed decline), and 4.5% nationally (64%

¹⁰For example, papers using proxies for specific types of shocks are Cornett, McNutt, Strahan and Tehranian [2011], Dagher and Kazimov [2012], Goetz and Gozzi [n.d.], and Ivashina and Scharfstein [2010].

¹¹I ensure the calculation is a lower bound to the aggregate direct contribution by subtracting a quantity, the average value of the shock in a specific subsample, from each area’s shock. Under certain assumptions, this allows me to get rid of measurement error in a way that will always understate the actual effect.

of the observed decline). For comparison, Greenstone et al. [2012] calculate an upper bound of 16% for employment losses due to shocks to small business credit. However, Chodorow-Reich [2014] uses variation from the syndicated loan market and suggests that credit shocks to firms can account for more than 40% of the observed decline in employment. While all of these calculations are partial equilibrium, together they suggest that the financial crisis was a fundamental driver of the Great Recession, with shocks to household credit being particularly powerful.

In the next section I lay out the accounting and estimation exercises in more detail. Section 3 describes the data and sample I use. Section 4 implements the instrumental variable estimation where I instrument for growth in household credit. Section 5 describes and constructs the measured shock, estimates the elasticity of employment with respect to this shock, and then performs the accounting exercises. Section 6 offers concluding remarks.

2 Accounting and Econometric Framework

In this section I build a simple accounting and econometric framework to guide my empirical work. I first define the accounting quantity of interest and identify the objects necessary to perform the ideal accounting: the true shock to household credit and the elasticity of the outcome with respect to this shock. I then discuss how, only using credit quantities and an instrumental variable, we can recover an elasticity that is informative about the true elasticity of interest and that can be used to discipline structural models. However, the recovered elasticity is not enough to construct the accounting object of interest. I then show that with a measure of the shock to credit and an instrumental variable (in practice, the same instrumental variable), I can calculate the accounting quantity of interest.

The economy is composed of I areas indexed by i . Each area has some outcome of interest E_i , which I take to be employment. Employment is given by a function E of the price of household credit in the area r_i , the prices of credit in other areas r_j , and all other factors ϵ_i , which I assume is a scalar for simplicity. The reduced form solution for the price of credit in i is a function of supply-side factors in the area S_i , demand factors D_i , and other shocks so that $r_i = R(S_i, D_i, v_i^r)$, where v^r contains other factors. Finally, the prices of credit in other areas affect employment in i

through spillovers, which are summarized by the functions g_i so that

$$E_i = E(\underbrace{R(S_i, D_i, v_i^r)}_{\text{price in } i}, \underbrace{g_i(\{R(S_j, D_j, v_j^r)\}_{j \neq i})}_{\text{prices in other areas}}, \epsilon_i).$$

For simplicity, I assume all functions are log linear where \widehat{X} signifies the log deviation of variable X and β^{ZX} is the elasticity of Z with respect to X . I place all terms unrelated to supply-side shocks in the residual v

$$\widehat{E}_i = \underbrace{\beta^{ES}}_{\text{local direct effect}} \widehat{S}_i + \sum_{j \neq i} \beta_{ij}^{ES} \widehat{S}_j + v_i.$$

I call the elasticity β^{ES} the local direct effect of supply-side shocks to credit. This elasticity tells us the total effect, that means including local general equilibrium effects, of the local supply-side shock to credit on the outcome of interest at the area.

The log deviation of total employment in the economy is the weighted sum of log deviations for each area, where weights reflect each area's share of total employment

$$\widehat{E} = \sum_i \omega_i \widehat{E}_i = \sum_i \omega_i (\beta^{ES} \widehat{S}_i + \sum_{j \neq i} \beta_{ij}^{ES} \widehat{S}_j + v_i).$$

The aggregate general equilibrium contribution of supply-side shocks to household credit on employment is then the weighted sum of all terms relating to supply-side shocks to credit

$$\text{aggregate general equilibrium contribution} \equiv \sum_i \omega_i (\beta^{ES} \widehat{S}_i + \sum_{j \neq i} \beta_{ij}^{ES} \widehat{S}_j).$$

This quantity answers the question of how much shifts in household credit supply affected aggregate employment. To calculate this quantity requires knowledge of β^{ES} , the spillover effects β_{ij}^{ES} , and the distribution of shocks. It is generally infeasible to recover the spillover effects because of the high dimension of the estimation problem ($I - 1$ by I parameters when we often only have I or, in a panel, I by T observations) and uncertainty about the spillover structure.¹²

A less ambitious, but still informative, quantity is the aggregate direct contribution of the shocks

¹²See Stumpner [2013] for recent work estimating the role of trade linkages across areas, one potential spillover effect, in the Great Recession.

to household credit

$$\text{aggregate direct contribution} \equiv \beta^{ES} \sum_i \omega_i \widehat{S}_i.$$

Measuring the aggregate direct contribution provides a reasonable starting point for understanding the scale of the aggregate general equilibrium contribution. Critically, this quantity is partial equilibrium in the sense that aggregate price effects (like declines in the safe interest rate) or reallocation across areas will not be captured. For example, as unemployment in California increases in response to a shock, one of the “spillovers” is that aggregate unemployment increases. The increase in the aggregate unemployment rate induces policy-makers to reduce the Federal Funds Rate. This policy response affects all counties and likely undoes some of the effects of the shock to California. However, the aggregate direct contribution provides a well-defined quantity that a structural model can target in calibration. Additionally, theory might suggest that spillovers will be very small or, as in the example above, go in the opposite direction, so that a small aggregate direct contribution indicates a small aggregate general equilibrium contribution. The correct interpretation of the aggregate direct contribution will depend on the model and what can be appropriately called a spillover, but it clearly contains useful information.

Learning about the Elasticity of Interest Setting aside the true shock and the accounting quantities, even knowing something about β^{ES} is helpful: A very small elasticity would require a very large shock for the aggregate direct contribution to be large. The ideal approach to learning about β^{ES} would be to use data on \widehat{E}_i and \widehat{S}_i and then estimate β^{ES} , but the true shocks are not observed. Here I show that I can still recover useful information about β^{ES} so long as I observe another variable that is related to \widehat{S}_i . Consider the reduced form solution for the quantity of credit, which is a function of supply shocks, demand shocks, and other factors

$$\widehat{L}_i = \beta^{LS} \widehat{S}_i + \beta^{LD} \widehat{D}_i + v_i^L.$$

This relationship is helpful because it relates credit quantities, which we are likely to observe, to the true shock \widehat{S}_i , which is unobserved. I can now use the estimation equation

$$\widehat{E}_i = \gamma \widehat{L}_i + e_i$$

as a way to relate supply-side shocks to employment. Direct estimates of γ , for example using OLS, will include the effects of demand shocks as well as any other correlation between the residuals v^L and v . Therefore I require an instrumental variable. Let $\tilde{v}_i = \sum_{j \neq i} \beta_{ij}^{ES} \widehat{S}_j + v_i$. Then a valid instrument is a variable Z_i that is correlated with changes in supply-side shocks to credit $Cov(Z_i, \widehat{S}_i) \neq 0$, and that satisfies the exclusion restrictions $Cov(Z_i, \widehat{D}_i) = Cov(Z_i, \tilde{v}_i) = Cov(Z_i, v_i^L) = 0$. Under these assumptions, it is simple to see that

$$\frac{Cov(\widehat{E}_i, Z_i)}{Cov(\widehat{L}_i, Z_i)} = \frac{\beta^{ES}}{\beta^{LS}}.$$

This quantity is the ratio of the elasticity of employment with respect to credit supply shocks to the elasticity of credit quantities with respect to credit supply shocks. In addition to telling us about the responsiveness of employment relative to the responsiveness of credit quantities, the ratio is useful for the calibration of structural models or simple hypothetical exercises. For example, if we believe \widehat{L}^* is the decline in observed credit due to contractions in credit supply then $\frac{\beta^{ES}}{\beta^{LS}} \times \widehat{L}^* = \widehat{E}^*$, or the direct percentage change in employment resulting from contractions in credit supply to households. Of course, the left-hand side implies I use the IV estimator so that $\widehat{\gamma}$ converges in probability to β^{ES}/β^{LS} under standard assumptions. The first part of this paper then works to identify a valid instrument and recover this ratio of elasticities.

Measuring the Aggregate Direct Contribution While β^{ES}/β^{LS} is informative, it does not tell us about the aggregate direct contribution of credit supply shocks to employment losses. To make progress, it is necessary to construct a measure of the true shock such that the measured shock will only reflect the true supply-side shock and noise. Credit quantities and even prices are not useful here because they reflect factors, such as demand shocks, that cannot credibly be thought of as noise. Assume that I have an observed variable s_i that is linearly related to the true credit shock by the parameter π and noise. Then I have the following system of equations

$$\begin{aligned} s_i &= \pi \widehat{S}_i + v_{si}, \\ \widehat{E}_i &= \beta^{ES} \widehat{S}_i + \tilde{v}_i. \end{aligned}$$

where $Cov(v_{si}, \tilde{v}_i) = 0$ is the measurement error or noise assumption. Given that s_i is observed by assumption, one could attempt to recover some measure of β^{ES} by estimating the following equation

$$\widehat{E}_i = \gamma_s s_i + e_{2i}.$$

But it is straightforward to see that OLS gives the following, where σ_s^2 is the variance of the noise and σ_S^2 is the variance of the true shock

$$\frac{Cov(\widehat{E}_i, s_i)}{Var(s_i)} = \left(\beta^{ES} + \underbrace{\frac{Cov(\widehat{S}_i, \tilde{v}_i)}{\sigma_S^2}}_{\text{omitted variable bias}} \right) / \left(\pi + \underbrace{\frac{\sigma_s^2}{\pi \sigma_S^2}}_{\text{attenuation bias}} \right).$$

We have two potential issues. First, the true shock might be correlated with other factors affecting employment. Second, the fact that the measured shock is only a noisy measure of the true shock attenuates the estimate.

An instrumental variable again provides a solution to these consistency issues. Assume we have a variable Z_i still correlated with the true shock $Cov(\widehat{S}_i, Z_i) \neq 0$, but that now satisfies the appropriate exclusion restrictions $Cov(Z_i, \tilde{v}_i) = Cov(Z_i, v_{si}) = 0$. With these assumptions it is straightforward to see that

$$\frac{Cov(\widehat{E}_i, Z_i)}{Cov(s_i, Z_i)} = \frac{\beta^{ES}}{\pi}.$$

But notice that instead of recovering β^{ES} , we recover β^{ES} normalized by π . This is the correct coefficient to calculate the aggregate direct contribution under the assumption that the weighted average of measurement errors is zero

$$\frac{\beta^{ES}}{\pi} \sum_i \omega_i \underbrace{(\pi \widehat{S}_i + v_{si})}_{\equiv s_i} = \beta^{ES} \sum_i \omega_i \widehat{S}_i + \frac{\beta^{ES}}{\pi} \underbrace{\sum_i \omega_i v_{si}}_{\text{assumed zero}} = \beta^{ES} \sum_i \omega_i \widehat{S}_i = \text{aggregate direct contribution.}$$

This result is striking: with a valid instrumental variable and if the weighted sum of measurement errors is mean zero, I am able to recover the aggregate direct contribution of the household credit channel.

However, it is optimistic to expect that I can construct a measure such that the weighted

average of measurement errors is zero. I show in appendix A.1 that even if the weighted average of measurement error is non-zero, I can construct quantities that are plausible lower bounds to the true aggregate direct contribution. Essentially, I can aggregate the difference between the measured shock and a subsample mean of the measured shock. If the subsample mean of the true shock has the same sign as the aggregate direct contribution, then the resulting sum will converge to a lower bound to the aggregate direct contribution.

Summary First, using only data on credit quantities and a valid instrumental variable I can recover β^{ES}/β^{LS} , which tells us about the responsiveness of employment relative to credit quantities. However, this elasticity does not quantify the household credit channel’s contribution to employment losses. With a measure of the supply-side shock to credit and an instrument I can recover the elasticity β^{ES}/π . This can be combined with the measured shock to recover the aggregate direct contribution of the household credit channel.

3 Data, Sample, and Summary Statistics

I rely on U.S. counties as my primary unit of observation. A wealth of reliable data are collected at the county level allowing for a wide set of controls. This and the large number of counties also helps recover more precise estimates than state-level data.¹³ I also check my central results by aggregating to the Economics Research Service commuting zone.

The data I use to measure household credit are from the Home Mortgage Disclosure Act (HMDA), an application-level database constructed by the Federal Financial Institutions Examination Council (FFIEC) from disclosure reports submitted by mortgage lenders.¹⁴ I rely on the flow of non-refinance mortgages as my measure of household credit. While only a part of total household

¹³While there are a large number of zipcodes, which would help precision, many of the important controls are not available at the zip level and many zip-level measures suffer from significant measurement error. Additionally, zipcodes can be so small that a household’s zipcodes of employment, residence, and consumption are often all different, which introduces substantial noise.

¹⁴<http://www.ffiec.gov/hmda/hmdaproducts.htm>. See Avery, Brevoort and Canner [2007] for a useful discussion. The reporting requirements include location of operation as well as asset and origination thresholds that have varied across years. Dell’Ariccia, Igan and Laeven [2012] estimate the HMDA data cover between 77% and 95% of all mortgage originations from 2000 to 2006.

credit, mortgages are by far the largest component of most households' liabilities.¹⁵ These data include various characteristics of the loan and applicant including the loan amount, applicant income, origination decision (for example, denied, approved but not originated, originated), census tract of the property for which the loan will be used, and an identifier for the originator or purchaser of the loan.¹⁶ The public data are available at an annual frequency from 1991 to 2013.

I rely on data from the Community Reinvestment Act (CRA) to measure and control for firm credit. These data are also compiled by the FFIEC from disclosure reports.¹⁷ The CRA reporting thresholds are different from those in the HMDA and so do not cover the same set of lenders, although there is significant overlap. The data report measures of lending to small businesses at the county or census tract level depending on the definition used. The first measure defines small business credit as loans to businesses with revenue less than \$1 million. The second measure defines small business loans as any loan for less than \$1 million to a business. Greenstone et al. [2012] estimate that CRA-lenders originated 86% of all loans under \$1 million dollars and that the second definition covers about twice as much (30%) of total small business originations as the revenue-based measure. Because the coverage of the market is broader with the loan-size definition, I use this to measure firm credit flows.

I measure employment with the County Business Patterns (CBP) dataset, which contains annual observations on employment and payrolls by 4-digit NAICS identifier and size constructed from various administrative data from the universe of firms in the Census Bureau's Business Register.¹⁸ To separate firms into tradable, non-tradable, and construction industries I follow the classification of 4-digit NAICS codes in Mian and Sufi [2014].¹⁹ Employment results are essentially identical using county-level data from the Quarterly Census of Employment and Wages (QCEW).

I use the Zillow Home Value Index for single-family residences to measure house prices as well as

¹⁵In 2008 housing debt was roughly \$10 trillion dollars while non-housing debt was about \$2.6 trillion. See the New York Federal Reserve Bank's *Quarterly Report on Household Debt and Credit*. While the share of refinance originations varies, refinancing loans often account for around 50% of all originations.

¹⁶The HMDA identifier does not necessarily reflect the ultimate entity operating an institution (for example, bank holding company) nor does it track mergers and acquisitions of lenders. I adjust for parents and acquisitions using the "Avery" file. I am very grateful to Robert Avery for making this file available.

¹⁷<http://www.ffiec.gov/cra/craproducts.htm>.

¹⁸<https://www.census.gov/econ/cbp/>

¹⁹Mian and Sufi classify an industry as tradable if it meets either some minimum of tradable revenue per worker or a gross trade value minimum. Non-tradable industries are narrowly classified as those industries involved in retail and restaurant services while construction is any industry related to "construction, real estate, or land-development." Most employment is not classified.

Zillow’s measure of sales volume.²⁰ The behavior of this index is broadly similar to the Case-Shiller index but is available in more counties.

To measure non-housing expenditures I rely on the Nielsen Retail Scanner database.²¹ The data report sales at a weekly frequency from over 40,000 stores with county-level identifiers. The coverage of products is concentrated in non-durables, especially food, with broad coverage of counties (see Beraja, Hurst and Ospina [2014]). However, the coverage of stores and products evolves over time. So to measure consumption growth I calculate quarterly growth rates in total expenditures using only the set of stores present in the county in both quarters. These rates can then be cumulated into changes at a longer horizon.

Additional data on debt stocks at the county level come from the county aggregates of the Federal Reserve Bank of New York-Equifax Consumer Credit Panel (CCP). These data are constructed from consumer credit reports and provide annual snapshots of credit card, mortgage, and auto debt balances and delinquency rates for about 2000 counties.²² I also use gross income data constructed by the IRS from tax returns to measure county income.

Summary Statistics and Sample Figure 1 plots the flow of credit originations and purchases in the home mortgage and small business loan markets for all counties normalized to be one in 2005. In the right panel the flow of home mortgage credit is broken out by the type of loan: refinancing, home purchase, or home improvement.²³ While home mortgage credit appears to recover in 2009, this is driven entirely by refinancing.²⁴ Home purchase and home improvement originations continue to decline to about 40% and 30% of their 2005 level respectively. Figure 2 plots total nonfarm employment and the Case-Shiller and Zillow national house price indexes, also normalized to be one in January 2005. By the peak of the financial crisis (late 2008) house prices had fallen roughly half of the distance they would ultimately fall while employment was just beginning its sharpest decline. House prices continued to fall until roughly 2012 while employment

²⁰See <http://www.zillow.com/research/data/>. This index is based on raw sales data on non-foreclosure arms-length sales. These raw data are then used to estimate a hedonic model in order to approximate an ideal home price index. See <http://www.zillow.com/research/zhvi-methodology-6032/> for more details of the Zillow methodology and Dorsey, Hu, Mayer and Wang [2010] for a discussion of different house price indexes.

²¹For more details see <http://research.chicagobooth.edu/nielsen/>.

²²See Lee and Van der Klaauw [2010] for more details.

²³These categorizations are applied by the reporting institution when filing the HMDA report in accordance with HMDA guidelines.

²⁴Refinancing can reflect household’s desire for liquidity as well as opportunistic pre-payment in order to take advantage of lower interest rates. See Hurst and Stafford [2004].

began to recover in early 2010.

Tables 1 and 2 present summary statistics for the total sample of counties with population greater than 50,000 containing CCP controls (excluding Hawaii and Alaska) and for the subsample of these counties in the East and South. The East and South compose the primary sample for the empirical analysis because these were Wachovia's traditional regions of activity. Outside of this region the instrument has little explanatory power. I report summary statistics for both the entire set of counties and the subsample to understand the comparability of the two samples. The population restriction is used to reduce noise from very small counties. The subsample accounts for over half (478) of all eligible counties and about 48% of all employment in 2006.

Total employment from 2007-2010 fell an average of 7% nationally and 6% in the subsample with tradable and construction employment experiencing larger declines. Non-tradables were far less responsive, but still declined by 3% nationally and 1% in the subsample. Among these three categories, non-tradables are the largest with 19% of the employment share within counties, but all three only account for about 44% of a county's total employment on average (most employment is not categorized).

There are large declines in household credit (42% nationally, 43% subsample) and small business credit (44%, 48%). Household credit flows grew by almost twice as much from 2002-2005 (74%, 75%). This growth in mortgage flows led to an increase in household leverage, measured as the ratio of mortgage debt per capita to the per capita gross income, of about 30% in both samples.²⁵ In 2006 this leverage ratio was roughly 1.06 nationally and 0.98 in the subsample. Nationally, house prices grew by about 33% in 2002 to 2005 and by 35% in the subsample, and then declined by 12% nationally and 11% in the subsample from 2007 to 2010. Broadly, the subsample had similar trends in important observables as the nation in general. This suggests that extrapolating estimates recovered from the subsample is a reasonable exercise.

4 Wachovia and the “Deal from Hell”

Analyst: *Okay. Ken I need to ask this question because I am getting it a lot from clients, I mean knowing what you know now about the mortgage market and the impact [...] on your stock price, would you still do the Golden West deal?*

²⁵Note that this is not the average household leverage ratio, but the ratio of average debt to average income.

Kennedy Thompson (CEO of Wachovia): *I think we're going to be happy that we did this deal long term. [...] because of the experience that we're having in the West as we use the branches that we acquired and I think on the mortgage side this product is...this Pick-a-Pay product is going to be very attractive when yield curves go back to normal and as the housing market comes out of recovery. So yes we're going through a little pain with it now but I think a year out, 18 months out, two years out we are going to be very happy that we did this deal.*

-Transcript of Wachovia's 2007 second quarter earnings call

Born as Wachovia National Bank in Salem, North Carolina in 1879, Wachovia Corporation eventually became the fourth-largest U.S. bank by assets in 2007. In 2007, Wachovia held about 6.6% of all bank deposits and over \$260 billion dollars of consumer loans, about 87% of which were secured by real estate. Wachovia was a national lender with wholesale operations in every state. But due to its consistent pattern of expansion into neighboring markets, the bank tended to have a significantly larger market share in the East and South (average of 2% and median of 1.5%, see Figure 3). Wachovia's market share was also very persistent within these traditional markets.²⁶

However, in May 2006 Wachovia acquired the nation's second-largest thrift Golden West Financial (GWF), operating as World Savings Bank (WSB), for \$25.5 billion. Wachovia was reportedly interested in expanding its footprint in the West where GWF had 123 branches and \$32 billion of deposits, as well as exploiting GWF's expertise in non-traditional loan products (Berman, Moltenkamp and Bauerlein [2006]). At the time of the purchase, Wachovia's CEO Kennedy Thompson described the deal as a "dream come true" (Creswell [2006]) and said that Wachovia was "merging with a crown jewel" (AP [2006]). GWF had been named a "Most Admired" company in mortgage services by Forbes magazine in 2006, in part because of its famous option-ARM (adjustable rate mortgage) loan branded as the "Pick-a-Pay." This loan allowed a borrower to choose her monthly payment from a menu of options, the smallest of which might mean the loan was negatively amortizing. The interest rate could also reset from a typically low "teaser" rate in response to various triggers, which would then adjust the menu of payments.

Following the announcement of the purchase, Wachovia's stock market value fell by \$1 billion, a little over 1% (Figure 4). Analysts worried that Wachovia had overpaid for a GWF portfolio of high-risk loans that was exposed to declining house prices in California, Arizona, and Florida

²⁶Wachovia's average share of lending from 2002-2003 is highly predictive of Wachovia's share of lending in 2005-2006 with an R-squared of 72% and a coefficient slightly greater than one.

(Creswell [2006]). GWF was famously a portfolio lender and so retained all of its loans on its balance sheet, about \$125 billion in assets. It quickly became apparent that the performance of the loans acquired from GWF was particularly poor. In the fourth quarter of 2006, the first time Wachovia reported GWF's earnings with its own, Wachovia announced a decline of \$100 million in non-performing loans from its legacy operations but an addition of \$700 million in nonperforming loans from GWF (Cole [2007]). In addition, Wachovia's investment bank was suffering from poorly performing positions in the credit default swap market. Throughout the third and fourth quarters of 2007, Wachovia reported losses of roughly \$2.4 billion on asset-backed securities and loans while increasing its reserves for loan losses to \$1.5 billion (Dash and Werdigier [2007]). The jump in the loan-loss provision largely reflected "increased loss expectations for the portion of the Pick-a-Pay portfolio" according to the chief risk officer, and induced Wachovia to raise over \$3 billion dollars of equity in December 2007 to "strengthen" the company (Cole [2007]). In the fourth quarter of 2007 Wachovia's earnings declined 98% from the year before, from \$2.5 billion to just \$51 million.

Conditions at Wachovia deteriorated quickly in 2008. The bank took a \$0.7 billion loss in the first quarter and reacted by stripping their chairman/CEO Kennedy Thompson of his chairmanship and cutting the dividend (White [2008]). In the second quarter Wachovia reported a "stunning" \$9 billion dollar loss, fired Thompson, announced the elimination of about 10,000 positions (6,000 terminations and 4,000 unfilled vacancies), cut its dividend for the second consecutive quarter, announced a capital raise of \$7 billion, and set aside a total of \$5.6 billion for loan losses. Additionally, Wachovia ceased offering "Pick-a-Pay" mortgages and completely shuttered its wholesale mortgage arm. By July, observers were referring to the GWF purchase as a "deal from hell" (Moore [2008]).

Following the failure of WaMu on September 26, 2008, Wachovia experienced a silent bank run where it lost \$5 billion of primarily uninsured deposits in a single day. As a result, the Federal Deposit Insurance Corporation (FDIC) organized the sale of Wachovia's operations to CitiGroup to avoid Wachovia's failure. However, Wachovia announced on October 3 that Wells Fargo would be purchasing the bank without government assistance. This was to take advantage of a more advantageous deal and the sale was completed in December. Following the merger, Wells Fargo continued to trim Wachovia's operations, although layoffs were moderate due to the relatively small geographic overlap between the two banks. Wells Fargo discontinued the Wachovia brand in 2011.

Evidence that Wachovia Contracted Credit to Households While the preceding narrative suggests Wachovia was a distressed institution, it does not necessarily follow that this distress was different from other lenders, that this distress translated into a contraction in access to credit at Wachovia, or that this contraction was economically important. I now turn to these considerations.

That Wachovia’s lending behavior was exceptional is readily apparent from the loan-level data in HMDA. To show this, I first bin an application as a high-, middle-, and low-income application depending on whether or not it fell into the top, middle, or bottom third of the income distribution of applications in the county. Within each income bin, I regress the probability a loan i in county c was originated on a full set of county fixed effects, a dummy for whether or not the loan was submitted to Wachovia (excluding GWF), and controls using OLS

$$\text{Prob(Originated)}_{it} = \alpha_{ct} + \beta_t \text{Wachovia}_i + \gamma_t X_{it} + e_{it}. \quad (1)$$

I limit the sample to applications in the South and East, exclude all loans with a loan-to-income ratio greater than eight or income less than five thousand, and drop counties with fewer than 2000 applications. This leaves me with about 300-800 counties depending on the year. I also divide applications according to the type of loan: home purchase, home improvement, and refinance. My controls are included to adjust for differences in the composition of applicants between lenders and include the log loan-to-income ratio, log income, race, lien status, regulator of the lender, sex of the applicant, and whether or not the loan is for a property that will be occupied by the owner. Results are robust to not including any controls. Because the entire set of controls are only available from 2004 onward and because I am no longer able to identify Wachovia after 2010, I only use these years.²⁷ The coefficients β_t can then be interpreted as the within-county, within-income group difference in origination probability between an application filed at Wachovia and an application filed at the average non-Wachovia lender, conditional on the observables.

Figure 5 reports the β_t ’s and shows that Wachovia was an average lender within the county up to the crisis, but then significantly contracted credit access across all loan categories and income groups. Low- and middle-income applicants saw their probability of origination decline by 50

²⁷The HMDA data suggest that in 2010, Wells Fargo was filing a number of loan originations under the Wachovia brand that would have been filed under the Wells Fargo name without the merger. Given that Wells Fargo absorbed Wachovia at the end of 2008, it would be surprising if there were still large differences in origination practices by 2010 so that this is not problematic.

percentage points in 2009 while even high-income applicants were 20 percentage points less likely to get a loan. Similarly, applicants for home improvement loans generally saw a decline of 20 percentage points and refinance loan applicants were over 30 percentage points less likely to get a loan from Wachovia. The county-clustered confidence intervals on these estimates are not reported for legibility, but are very tight. Origination probabilities for home purchase and refinance loans return to normal by 2010 while home improvement origination rates remain lower, likely reflecting Wells Fargo's tighter standards for home improvement loans.

I also examine the intensive margin of credit for loans that were originated. I run the same type of regression, but now with the log of the loan-to-income (LTI) ratio on originated loans as the outcome. I restrict the sample to home purchase loans as here the LTI will primarily reflect down-payment requirements and lending standards. In contrast, LTIs on refinance originations are difficult to interpret due to the inability to distinguish between "cash-out" and "rate-and-term" refinance loans. Similarly, lower home improvement LTIs are consistent with both an increase in the supply of small loans for consumption and a tightening in lending standards. The left panel of Figure 6 plots the estimated coefficients and shows that Wachovia originations to low- and middle-income applicants are significantly less leveraged in 2008 and 2009. Wachovia's loans to low-income applicants had LTIs almost 80% lower than originations at non-Wachovia lenders, and almost 60% for middle-income applicants. Interestingly, LTIs for high-income originations at Wachovia actually increased by a little less than 20%, suggesting Wachovia was actively substituting to borrowers likely to be better credit risks. Much of the changes in LTI are explained by Wachovia excluding low-income borrowers from credit. The right panel of Figure 6 plots the coefficients from putting log income on the left-hand side (here with county-clustered 95% confidence intervals) and combining all income groups. Beginning in 2008, home purchase loans originated by Wachovia have an income over 10% higher than originations at the average non-Wachovia lender. This difference increases to almost 70% in 2009 and almost disappears by 2010. Together these results suggest that Wachovia was contracting credit across the board, but that low- and middle-income applicants were far less likely to get credit from Wachovia.

Exclusion Restriction, First Stage, and Reduced Form Recall that the aim is to have an instrumental variable that will allow me to estimate the following system and retrieve $\hat{\gamma} = \beta^{ES} / \beta^{LS}$

$$\begin{aligned}\widehat{L}_i &= \sigma Z_i + e_{1i}, \\ \widehat{E}_i &= \gamma \widehat{L}_i + e_{2i}.\end{aligned}$$

While access to credit at Wachovia contracted in 2008 and 2009, it still remains to show: (1) that exposure to Wachovia satisfies the exclusion restriction (is not correlated with e_{si}); and (2) that exposure to Wachovia provides a strong first stage ($\sigma \neq 0$), or that areas where Wachovia was important experienced lower growth in household credit. I use the average of Wachovia's overall market share in non-refinance lending in the 2005 and 2006 HMDA data as my instrumental variable

$$\text{Wachovia Exposure}_i = (\text{Wachovia Share}_{i,2005} + \text{Wachovia Share}_{i,2006})/2.$$

There is a difficulty in constructing this market share. The HMDA data report not only mortgage originations, but mortgage purchases (when a second institution purchases the loan from the originating institution). Due to the inability to distinguish between these loans the HMDA aggregates suffer from a well-known problem of double-counting (see Scheessele [1998]). This suggests that one should not include purchases when calculating market shares since originators will have a deflated market share. However, Stanton, Walden and Wallace [2014] document that purchases allow one to trace the important wholesale and correspondent relationships that funded much of the mortgage market in this period. Ignoring purchases could then cause me to miss the links between lenders and counties that are critical for my exercise in the second part. The risk of artificially deflating exposure to Wachovia is that it will inflate my resulting reduced form estimates. Since my interest is not in the reduced form, per se, but instead in using exposure as an instrument (where the scaling error will cancel) the danger of rescaling is unimportant. This is especially true when weighed against the risk of missing important lender-county connections in the second part of my paper. Therefore, to be consistent, I use both originations and purchases when computing market shares. But all my reduced form results are robust to using only originations to construct market share. The coefficients on exposure to Wachovia are rescaled by about two thirds, but with

very similar statistical significance, although market share calculated incorporating purchases does tend to have slightly better statistical performance. This is consistent with Wachovia only using originations to supply credit within this sample while many other lenders in these areas did appear to originate and then sell their loans.

The loan-level results showed that Wachovia was an average lender until the crisis, suggesting that Wachovia was not causing a boom in credit prior to the crisis. But counties exposed to Wachovia might be different for some other reason, which would be problematic. As a first check, Table 3 presents bivariate regressions between Wachovia exposure and important observables.²⁸ These correlations suggest exposure to Wachovia was not strongly associated with the housing boom, subprime lending, or other important observables. Potentially problematic are the positive correlations between Wachovia exposure and household mortgage leverage in 2006 (column three) and the share of construction employment in 2005 (column five). As Mian and Sufi [2011] demonstrate, household leverage is negatively associated with the house price decline after 2006 and declines in employment, and we know the construction sector suffered a large contraction during the crisis. However, Wachovia exposure is negatively correlated (insignificant) with growth in household leverage from 2002-2005 (column four), population growth (column eight), and house price growth (column seven), all of which are also indicative of the housing boom and bust. Wachovia's share is also negatively (insignificant) associated with the share of lending regulated by the Department of Housing and Urban Development (HUD, column six). I use this variable as a proxy for subprime lending since much of the subprime market was driven by mortgage brokers regulated by HUD (see Engel and McCoy [2011]). These correlations indicate that counties exposed to Wachovia were not more likely to experience the housing boom and collapse.

This impression is largely confirmed by the joint regression results presented in Table 4. Mortgage leverage continues to be positively associated with Wachovia's exposure and there is a slight positive association with the share of construction share. Now the correlation with population growth is significantly negative. There is still no significant association with house price growth,

²⁸Unless otherwise noted, my estimates are clustered at the state level. To adjust for the relatively small number of clusters, I report p-values and confidence intervals computed with the pairs bootstrap with 1,000 replications. All results are robust to using the wild cluster bootstrap of Cameron, Gelbach and Miller [2008], but Kline and Santos [2012] show that the wild bootstrap can have worse performance under misspecification. The pairs bootstrap, because it is nonparametric, is more robust and so I rely on it here. My results also hold when using the spatial correlation HAC correction of Conley [1999].

the share of HUD-regulated lenders, or non-tradable growth in pre-crisis employment and the correlation with mortgage leverage is not significant when including house price growth.

Another way to check the Wachovia’s validity as an instrument is to determine if the timing of its county-level effects are consistent with that implied by the narrative and application-level evidence. For example, I should be concerned about the exclusion restriction if exposure to Wachovia has effects on household credit before the onset of the crisis. So I estimate the following repeated cross-section regression of annual household credit, house price, and non-tradable employment growth on exposure to Wachovia:

$$\widehat{E}_{it} = \alpha_t + \beta_t \text{Wachovia Exposure}_i + \epsilon_{it}.$$

Figure 7 shows that counties exposed to Wachovia generally had no pre-crisis trends in any of these observables. While there is a burst of credit growth in counties exposed to Wachovia in 2004-2005, all other years before the crisis show no significant relationship. But counties exposed to Wachovia began to experience negative growth in household credit during 2008 and 2009. Trends in non-tradable employment are even clearer with coefficients near zero until 2008 and 2009, when they turn significantly negative. Finally, counties exposed to Wachovia had slightly negative relative growth in house prices leading up to the crisis, although this relationship is insignificant. Then house prices began to fall in 2009 and 2010, staying well below zero until 2013 (not shown). Overall, the timing and direction of the Wachovia effects are consistent with a contraction in credit from Wachovia due to the crisis.

Effect of Exposure to Wachovia on Household Credit Table 5 gives the results from regressing household credit growth from 2007 to 2010 on exposure to Wachovia. The standard first stage diagnostics in column one are very good with a large F-statistic and R-squared. Together with the effects evident in Figure 7, weak instrument issues are unlikely to be an issue.²⁹ The estimate suggests a one percentage point increase in exposure to Wachovia decreases home mortgages by about 2.5% from 2007-2010. This effect captures the direct decline in loans from Wachovia and

²⁹As Olea and Pflueger [2013] show, the traditional first stage diagnostics are generally invalid in the presence of clustered residuals. When I compute the critical values for their test (the robust F-statistic here is identical to the efficient F-statistic they suggest when just identified) I can reject their “worst-case” scenario at at least the 10% level.

any general equilibrium effects incident to the credit contraction (for example, the contraction in credit also causes a decline in income and house prices). The size of this effect is large. In addition to multipliers arising from declines in demand and subsequent falls in income, any effects on house price expectations are likely to significantly lower mortgage lending across all lenders, not just Wachovia. However, alternative measures of exposure such as deposit share and share of only originations gives smaller effects (from .5% to 1.7%) with similar statistical significance. Column two introduces several important controls with no significant change in the coefficient. Column three employs a quantile regression to check for the significance of outliers, again with no significant difference in the Wachovia coefficient. Column four includes regional fixed effects and column five includes state fixed effects. Region dummies have no effect on the Wachovia coefficient, but with state fixed effects we see the expected sign but with a smaller point estimate and less precision. Columns six and seven use weighted least squares with county population in 2006 as weights and finds very similar results and with much better precision in the regression with state fixed effects. Figure 8 plots household credit growth against Wachovia exposure and visually confirms the relationship. The distribution of mortgage flows is relatively wide and appears symmetric when Wachovia's share is very small, suggesting counties with low exposure are equally likely to have high or low growth. But as Wachovia's share increases, growth declines and the distribution appears to tighten.

Effect of Exposure to Wachovia on Expenditures and Employment Before moving to the 2SLS estimates of the effect of household credit on employment, it is important to demonstrate that exposure to Wachovia had a discernible effect on household demand and employment in the reduced form and that these are likely resulting from effects on the supply of household credit.

The decline in household credit from Wachovia could have affected household demand in several ways. First, declines in home equity lines of credit (HELOCs) or cash-out refinancing loans will directly reduce household liquidity and expenditures (see Hurst and Stafford [2004] and Cooper [2013]). Second, as households are denied mortgages they are less likely to purchase a home. In addition, any consumption (often durables and home services) complementary to a home purchase will be foregone, although this is potentially countered by any substitution away from housing. Table 6 shows that exposure to Wachovia affected retail expenditures, measured with the Nielsen

Retail Scanner data. These expenditures declined by about .9% in response to a one percentage point increase in Wachovia exposure with the effect very robust to controls, state fixed effects, and weighting by population. Table 7 shows that a one percentage point increase in Wachovia exposure caused a 2.5% decline in house sales. This effect is also robust to controlling for mortgage leverage, state fixed effects, or weighting by population in 2006. There is also evidence in Table 8 that house prices decline from 2007-2010, although the effects are larger when extending the horizon to 2007-2012. Exposure to Wachovia lowers house prices from 2007-2010 by about .6%, but the effect on house prices almost triples to 1.5% when I extend the horizon to 2012. This long lag suggests significant stickiness/momentum in home prices, potentially due to homeowners delaying sales in response to the weakening price level (see Genesove and Mayer [2001]). Overall, the effects on house prices are relatively imprecise, especially when controlling for state fixed effects.

The decline in retail expenditures might cause a decline in local non-tradable employment if employment is at all demand determined. Table 9 shows the effect of exposure to Wachovia on non-tradable employment and payrolls. Columns one and two show that losses in local non-tradables are higher, about 0.7% decline, in counties exposed to Wachovia, both within and across states. Columns three and four show that payrolls also decline as a result with a similar but slightly larger effect. One important possibility to rule out is that the employment losses are purely local, and that neighboring counties might experience growth in employment as workers and business activity leaves the distressed county. To check for this I aggregate the data to the commuting zone level in columns five and six and find strikingly similar estimates to those in columns one and two. This suggests that much of the losses are in fact absorbed by the counties experiencing the shock.

It is also possible that the decline in house sales, house prices, and potential declines in home investment could affect local construction employment, particularly residential construction employment. Table 10 shows the results of exposure to Wachovia on construction employment. Exposure to Wachovia seems to cause a decline in construction employment, but this effect is not robust to state fixed effects. However, columns three and four show that exposure to Wachovia caused large declines in residential construction while non-residential construction had essentially no response. This is again consistent with exposure to Wachovia affecting local outcomes through household credit.

If these effects are driven by declines in household demand, then it would be surprising to see

any effect on tradables, which would depend far less on local demand. Consistent with this, Table 11 shows that while tradable employment has a negative relationship with Wachovia exposure the estimate is highly insignificant and exposure to Wachovia explains very little of the variation (0.002 r-squared).

It is possible that the losses in construction and non-tradables are undone by expansions in sectors that are less affected by the shock. Columns three and four regress employment excluding tradables on exposure to Wachovia. We see that even including these sectors there are large declines in employment, although less strong than those in residential construction and non-tradables. Total employment, in columns five and six, also falls as expected since tradables do not expand in response to the shock.

While exposure to Wachovia caused losses in employment consistent with a fall in household demand, an important alternative is that the effects from Wachovia are actually due to contractions in firm credit, not household credit. While this would be interesting, it would be problematic for the interpretation of my results as illuminating the household credit channel. Ex ante, this is unlikely due to the size of Wachovia's home mortgage lending relative to its firm lending. Across counties Wachovia's home mortgage lending was roughly five times the size of its small business lending. While flow measures of total firm credit are unavailable, the stock of Wachovia's household lending on its call report was about four times the size of its total commercial and industrial loan stock. Aggregate trends also suggest that Wachovia's distress primarily resulted in a contraction in household credit. The left panel of Figure 9 plots the flow of household and small business originations for Wachovia and the market in general. The right panel plots the stock of household and commercial loans reported on commercial bank call reports for both groups. Both graphs show that the decline in lending from Wachovia was primarily concentrated in the household lending market while Wachovia's lending to firms generally tracked the market trends.

Table 12 checks this possibility in several ways. Column one reports the baseline estimate. Column two includes a third order polynomial of the growth in small business originations from 2007 to 2010. The resulting estimate of Wachovia's effect is likely to be conservative as a decline in household credit may cause a decline in firm credit. Even so, there is remarkably little change in the Wachovia coefficient. This indicates the shock to Wachovia was not likely to be operating through shocks to small business credit. However, this should not be taken as evidence that

small businesses credit was irrelevant since Greenstone et al. [2012] do find effects on employment, although the effects are quantitatively small. It is also possible to check if the results are operating through exposure to Wachovia in the home credit market or the small business credit market. Wachovia's market share is highly correlated across, which makes estimates of the two highly noisy and unstable, although generally more weight is put on exposure to Wachovia in the HMDA market. An alternative way to check this concern is to simply estimate dummies for above-median exposure to Wachovia in both the HMDA and CRA data. Column three includes a dummy for exposure to Wachovia in the HMDA data, which enters negatively and significantly. Column four includes a dummy of high exposure to Wachovia in the CRA data, which enters insignificantly and has only a slight effect on the HMDA market dummy. Together with the pattern of employment losses, these results suggest exposure to Wachovia affected local outcomes primarily through its effects household credit.

As an additional check that exposure to Wachovia is operating through household credit, I control for industry-specific labor demand shocks. Column five includes the county's 2006 share of employment in the finance and insurance (NAICS 52), real estate (53), and construction (23) industries. Employment in all of these industries declined nationally over this period. I also include the share of employment in tradables to control for exposure to trade shocks. While these variables have some explanatory power, evidenced by the improved R-squared, the coefficient on Wachovia exposure is essentially unchanged.

Instrumental Variable Estimates Exposure to Wachovia caused declines in household credit and employment. The effects are distinct from the boom and bust in house prices, changes in small business credit, trade shocks, and industry-specific declines in labor demand. Given these results I use exposure to Wachovia as an instrument for household credit growth to recover β^{ES}/β^{LS} , the ratio of employment and credit quantities elasticities with respect to the shock to credit (see Section 2).

Table 13 presents the OLS and 2SLS estimates for the effects of household credit growth on total employment growth from 2007-2010. I use total employment as my baseline outcome in order to capture any substitution effects that might be obscured by looking only at non-tradable employment. If I interpret the OLS coefficient as causal then it implies a 10% reduction in home

mortgages will cause a decline in employment of about 1.5%. However, the 2SLS estimates in Table 13 show the OLS coefficient is biased downward with the estimate in column two about twice as large. The first-stage diagnostics are very good across all specifications, which suggests Wachovia is unlikely to be a weak instrument. The elasticity of 0.3 says that a 10% decline in household credit driven by supply shocks would cause a 3% decline in total county employment. Column three includes the controls for industry shares and leverage fixed effects with no significant change in the estimated coefficient. Column four performs an additional robustness check by using exposure to Wachovia measured in 2002-2003, at the start of the housing boom, instead of 2005-2006. This addresses the concern that Wachovia's market share at the peak of the housing boom reflected Wachovia's selection based on house price dynamics. The estimate using early exposure as an instrument gives essentially the same elasticity, although the first stage relationship is weaker, which is sensible since it should be somewhat noisier than actual exposure. Finally, to check if spillovers across counties are confounding the estimated elasticity, I aggregate the county-level data to Census commuting zones and repeat the estimation in column five and get a very similar estimate.

It is striking that the 2SLS estimate is so much larger than the OLS estimate. Intuition about omitted variable bias would normally lead us to expect a smaller 2SLS estimate. However, there are multiple factors that could reverse this logic. Changes in demand for mortgage credit might be very volatile and only slightly related to broader employment growth. For example, a decline in mortgage demand might occur because households are substituting a home purchase for nondurable consumption. This type of shock might have neutral or even expansionary effects on total employment.³⁰

Additionally, many types of credit such as small consumer loans and home equity line of credit loans are not recorded in the HMDA data, but it is likely that Wachovia also contracted in these

³⁰In detail, let σ_X^2 be the variance of variable X . I allow $Cov(\tilde{v}_i, \widehat{D}_i) \neq 0$, or that demand shocks to credit can be correlated with factors affecting the outcome of interest. We see the OLS estimate of $\widehat{E}_i = \gamma \widehat{L}_i + e_i$ would give (asymptotically)

$$\text{plim } \hat{\gamma}^{OLS} = \frac{Cov(\widehat{E}_i, \widehat{L}_i)}{Var(\widehat{L}_i)} = \frac{\beta^{ES} \beta^{LS} \sigma_S^2 + \beta^{ED} \beta^{LD} \sigma_D^2 + \beta^{E\tilde{v}} \beta^{LD} Cov(\tilde{v}_i, \widehat{D}_i)}{(\beta^{LS})^2 \sigma_S^2 + (\beta^{LD})^2 \sigma_D^2 + (\beta^{L\tilde{v}})^2 \sigma_{\tilde{v}}^2}$$

Standard omitted variable bias logic ($Cov(\tilde{v}_i, \widehat{D}_i) > 0$) suggests the estimate could be inflated. However, it is entirely possible that the terms in the denominator are so large as to render $\hat{\gamma}^{OLS} < \beta^{ES} / \beta^{LS}$. For example, this could occur if demand shocks in the mortgage market are relatively unimportant for total employment (β^{ED} small), but demand shocks are very volatile (σ_D^2 large).

markets. This reinforces my interpretation of these results as the elasticity of employment with respect to a measure of household credit, and not just home mortgage credit. However there are two potential concerns. First, it is possible that Wachovia's market share in these other types of credit is very different, so that Wachovia's home mortgage market share is a poor indicator of exposure to these credit contractions. I find that Wachovia's market shares across counties in the three types of credit that I do measure are very correlated (0.72 to 0.85). Moreover, regressing the share of home improvement and refinance loans on the share of home purchase loans gives highly significant coefficients of 0.88 and 1.25 respectively. While different from one, these coefficients suggest Wachovia's market shares across credit types are relatively similar.

To the best of my knowledge, these are the first estimates of the effect of supply shocks to household credit on employment during the Great Recession. The nearest comparison is to DiMaggio and Kermani [2014], who estimate the effects of expansions in household credit on non-tradable employment during the boom years. Using changes in lending regulations to instrument household credit at the county level, they recover an elasticity of about 0.2. This is somewhat smaller, but given the confidence intervals, very close to my estimates. This suggests the employment effects of credit contractions were similar in the boom and the bust.

Heterogeneous Effects The literature on curvature of the consumption function suggests household leverage and income might affect the response to household credit supply shocks (see Baker [2013] and Carroll and Kimball [1996]). Columns one and two of Table 14 show that the elasticity in low-income counties is significantly larger than that recovered in high-income counties. Columns three and four show that high-leverage counties have a larger response than counties with less leverage. I try to proxy for the ability of household to substitute across lenders with the share of HUD-regulated lenders. Many of these lenders depended on the securitization market for funding and became distressed as this market collapsed (see Engel and McCoy [2011]). Columns five and six divide the sample by share of HUD-regulated lending above and below the median. I find the estimated effect is about twice as large in counties with more HUD-regulated lending. Intuitively, as households are unable to substitute lending due to other lenders also being distressed the effect of the credit shock becomes larger. While this difference is suggestive, further work with the ability to recover more precise estimates is necessary.

Summary I provided evidence that household demand and employment responded strongly to supply-side contractions in credit during the Great Recession. This lays an empirical foundation for models using shocks to household credit to explain the Great Recession as well as an empirical moment for use in calibrations. But the elasticity of 0.3 alone does not imply that the supply shock to household credit was an important contributor to employment losses. This depends on both the elasticity and the size of the shock. While I can account for effect of the shock from Wachovia, this does not quantify the size of shocks from other lenders. In the next section I construct a measure of the broader shock to household credit and perform the simple accounting exercise outlined in Section 2.

5 Constructing and Using a Measure of Credit Shocks

The task in this section is to construct a measure of the supply shock to household credit. I first lay out a simple equilibrium model of credit across areas/borrowers and lenders with monopolistic competition. This structure implies that the true supply shock to credit markets is simply the weighted average of lender-specific shocks, where the weights reflect the lender's market share in that area. For example, in areas that depend only on Wachovia the true shock will only be the shock from Wachovia, while in areas borrowing from more lenders the true shock will aggregate the shock from each lender in proportion to the lender's market share. The challenge then becomes approximating these lender-specific shocks. Using the model, I provide identification conditions under which I can recover a measure of each lender's cost shock from data on lender-borrower credit flows (in practice, the HMDA data). So I estimate a lender fixed effect where I control for demand shocks with borrower fixed effects. The lender fixed effects are then measures of how much that lender was contracting or expanding access to credit. Using these fixed effects, I construct a measure of the true shock. I can then recover the elasticity of employment with respect to this measure using an instrumental variable (see Section 2). Together, this elasticity and shock allow me to estimate the size of household credit channel's effect on aggregate employment.

5.1 A Simple Model of Household Credit

Each area is a borrower that has demand for credit from each lender operating in the area. Lenders solve a standard monopolist’s problem. The assumption of monopolistic competition is central as it implies that variation in the health of an individual lender will have some impact on the cost incurred by borrowers. This should be taken as a reduced form way of capturing the various frictions that limit substitution across lenders. Evidence from Section 4 suggests this is a reasonable assumption, but to the extent that this assumption is incorrect, I should find no effects. I log-linearize this model around an arbitrary equilibrium point to arrive at simple expressions that can be linked directly to observables in the data. While simple, this model is sufficient to describe how I identify the structural supply-side shocks from data on credit flows.

Credit Demand The quantity of credit demanded from a lender j by an area i is L_{ij} and is given by the function L^D . I assume each of these is a differentiable, invertible Marshallian demand function that take prices r_{ij} and demand shifters d_{ij} (for example, wealth or tastes) as inputs. The function is allowed to differ across areas and lenders

$$L_{ij} = L_{ij}^D(r_{ij}, d_{ij}). \quad (2)$$

The price r can be thought of as any cost incurred by the borrower that is set by the lender (this could be down-payment requirements, probability of denial, and so on) and can easily be extended to be non-scalar.³¹

Credit Supply Lenders are monopolists who solve separable, static problems in each area.³² I express the problem in terms of the inverse demand functions $r_{ij} = r_{ij}(L_{ij})$ implied by 2. The monopolist faces a cost function $\sigma_{ij}(L_{ij}, c_{ij})$ that is differentiable and takes the level of lending and

³¹Notice that this function has the simplification that the local “aggregate” price of credit, that is the price index for credit in the area, enters as a “demand” effect. That is, any changes in the price of credit unrelated to lender j ’s price are forced into the d term. This is not important for the work here since I only want to identify the lender-specific variation.

³²Assuming the problems are separable across areas is a simplifying assumption. One could instead imagine that the lender has an additional constraint that limits the equalization of marginal revenue across areas. Allowing for these effects would substantially complicate the estimation by introducing non-linearities, but it could offer efficiency gains. However, without knowing the relevant factors or nature of the cross-area constraint, taking this step seems more likely to introduce misspecification.

cost-shifters c_{ij} as inputs. This gives the standard monopolist's problem

$$\max_{L_{ij}} r_{ij}(L_{ij})L_{ij} - \sigma_{ij}(L_{ij}, c_{ij}). \quad (3)$$

Letting the price elasticity of demand be $\epsilon_{ij}^{Lr}(L_{ij})$, we have the standard solution for the monopolist's price

$$r_{ij} = \sigma'_{ij}(L_{ij}, c_{ij}) / \left(1 + \frac{1}{\epsilon_{ij}^{Lr}(L_{ij})} \right). \quad (4)$$

As is standard, the cost of borrowing is a function of marginal cost and the price elasticity of demand.³³

Equilibrium It is useful to log-linearize the equations 4 and 2 around an arbitrary equilibrium point. This gives us (ignoring approximation error)

$$\widehat{r}_{ij} = \epsilon_{ij}^{rc} \widehat{c}_{ij} + \epsilon_{ij}^{rL} \widehat{L}_{ij}, \quad (5)$$

and from 2 we have

$$\widehat{L}_{ij} = \epsilon_{ij}^{Lr} \widehat{r}_{ij} + \epsilon_{ij}^{Ld} \widehat{d}_{ij}, \quad (6)$$

where ϵ signifies the appropriate elasticity. These two structural equations pin down the pair of credit quantity \widehat{L}_{ij} and price \widehat{r}_{ij} .

We can rearrange them to get reduced form equations in terms of the demand and cost shifters

$$\begin{aligned} \widehat{r}_{ij} &= \frac{\epsilon_{ij}^{rc}}{1 - \epsilon_{ij}^{rL} \epsilon_{ij}^{Lr}} \widehat{c}_{ij} + \frac{\epsilon_{ij}^{rL} \epsilon_{ij}^{Ld}}{1 - \epsilon_{ij}^{rL} \epsilon_{ij}^{Lr}} \widehat{d}_{ij} \\ &\equiv \beta_{ij}^r \widehat{c}_{ij} + \zeta_{ij}^r \widehat{d}_{ij}, \end{aligned} \quad (7)$$

and

$$\begin{aligned} \widehat{L}_{ij} &= \frac{\epsilon_{ij}^{Lr} \epsilon_{ij}^{rc}}{1 - \epsilon_{ij}^{rL} \epsilon_{ij}^{Lr}} \widehat{c}_{ij} + \frac{\epsilon_{ij}^{Ld}}{1 - \epsilon_{ij}^{rL} \epsilon_{ij}^{Lr}} \widehat{d}_{ij} \\ &\equiv \beta_{ij}^L \widehat{c}_{ij} + \zeta_{ij}^L \widehat{d}_{ij}. \end{aligned} \quad (8)$$

³³I find it helpful to write this and other elasticities in terms their natural signs, and not the absolute value of the elasticity as is sometimes the convention.

These are familiar expressions, but it is useful to review several coefficients. The coefficient β_{ij}^r in 7 gives the pass-through from changes in lender costs to borrower costs or prices. This coefficient multiplied by changes in lender cost gives the total change in the cost of borrowing from a specific lender, so that this is the supply-side shock to credit from a lender j to a borrower i . The coefficient β_{ij}^L , which multiplies cost shocks in the quantity equation 8, is closely related as it is the product of the pass-through parameter and the price elasticity of demand ($\beta_{ij}^L = \epsilon_{ij}^{Lr} \beta_{ij}^r$). Intuitively, the change in quantity borrowed reflects both the change in the price of credit and the effect of increased costs on quantities borrowed.

The system above describes how supply-side and demand-side shocks manifest themselves in price and quantity changes between lenders and areas. I will use this simple structure to understand how to identify and construct supply-side shocks to credit for an area.

Area Shocks Here I show that if I assume a theoretically reasonable functional form for the “true” shock \widehat{S}_i to a borrower, then the central problem becomes one of approximating the inputs to this function. I then show that I can estimate a set of lender fixed effects and, under plausible identification assumptions, these will be valid proxies for the true inputs. The resulting measure is linearly related to the true shock so long as the price elasticity of demand is, on average, non-zero, which is implied by the model.

The demand functions 2 imply some function R_i that aggregates lender-area prices into an area-specific price r_i

$$r_i = R_i(\{r_{ij}(c_{ij}, d_{ij})\}). \quad (9)$$

Changes in lender-area costs c_{ij} affect borrowers through the price, so to construct the true credit shock to an area (that is, the change in price due to lender cost shocks) we must know the aggregator function R_i . However, the demand functions are generally unknown and can be difficult to specify credibly.

Instead I take advantage of a central insight from index number theory to construct a price index that is appropriate for a broad class of demand systems. Let $\omega_{ij,t}$ and $\omega_{ij,t-1}$ be the expenditure shares at the two points at which we are comparing prices. Then the Törnqvist index, a superlative

index, gives the percentage change in the price of credit to an area as³⁴

$$\widehat{r}_i = \frac{1}{2} \sum_j (\omega_{ij,t} + \omega_{ij,t-1}) \widehat{r}_{ij}.$$

Using 7 to substitute for each lender's price, we can define the true supply-side shock to credit for the area as the deviation in the area's price due to the lender cost shocks

$$\underbrace{\widehat{S}_i}_{\text{true shock}} \equiv \frac{1}{2} \sum_j (\omega_{ij,t} + \omega_{ij,t-1}) \beta_{ij}^r \widehat{c}_{ij} \quad (10)$$

Expression 10 gives a functional form for the credit supply shock that is consistent with a broad class of demand functions.

Identification of Inputs However, I do not observe the correct inputs to 10: $\{\beta_{ij}^r \widehat{c}_{ij}\}$. So I approximate these inputs with a lender-specific fixed effect and show that this provides me with a measure of the true shock. Specifically, Greenstone et al. [2012] propose regressing changes in credit quantities between areas i and lenders j on a full set of lender and area fixed effects, represented here as coefficients on the dummy variables D_i (for an area i) and Λ_j (for a lender j)

$$\widehat{L}_{ij} = \alpha_i D_i + \rho_j \Lambda_j + e_{ij}. \quad (11)$$

They then weight these fixed effects by the market share in a base period to construct supply-side shocks to small business credit, the intuition being that the lender fixed effects reflect changes in quantities driven by supply-side shocks. With the simple structure I have laid out, it is straightforward to see that I must assume that demand shocks are rendered mean zero for each lender:

Identification Assumption (unconditional)

$$E_i(\zeta_{ij}^L \widehat{d}_{ij}) = 0, \forall j. \quad (12)$$

³⁴Since Diewert [1978] and Diewert and Nakamura [1993] it has been understood that a superlative index has theoretically desirable qualities: Any superlative index is a second order approximation to any homothetic, twice-differentiable expenditure function.

This is also the identification assumption made by Chodorow-Reich [2014]. Because this assumption is unconditional it is stronger than necessary. In appendix A.2 I provide the conditional statement. In words, I require that the expected demand shocks be zero for each lender after being projected onto the set of borrower fixed effects. It is easiest to understand this condition by considering how it might be violated. For example, lenders might select into the same sub-market within each county, such as specialization in subprime lending markets across counties. If there is a shock affecting the demand of all subprime borrowers across markets then I will recover some of the decline in subprime demand in the lender's fixed effect. For the moment, I assume this condition holds, but I address these issues in the empirical implementation.

Under assumption 12, it is straightforward to determine what exactly is recovered by the fixed effect coefficient ρ . Using equations 8, 12 and noticing that lender costs can be decomposed as $\widehat{c}_{ij} = C_j + e_{ij}^c$ we see that

$$\begin{aligned}
E_i(\widehat{L}_{ij}) &= E_i(\beta_{ij}^L \widehat{c}_{ij} + \zeta_{ij}^L \widehat{d}_{ij}) \\
&= E_i(\beta_{ij}^L \widehat{c}_{ij}) \\
&= E_i(\beta_{ij}^L (C_j + e_{ij}^c)) \\
&= C_j E_i(\beta_{ij}^L) + E_i(\beta_{ij}^L e_{ij}^c). \tag{13}
\end{aligned}$$

Under standard assumptions, OLS estimates of the lender fixed effect ρ converge to this quantity. The first component is the lender's common cost shock and the average effect across areas that cost shocks had on quantities. Intuitively, if a lender tends to operate in areas where demand is very elastic then we will estimate a larger fixed effect (conditional on the pass-through decision). The second term is the average of the product of the same elasticity and the idiosyncratic component of lender-area cost shocks. If the distribution of the lender's cost shocks is such that very responsive areas were exposed to larger cost-shocks then we will also recover a larger lender effect. The central point is that the lender fixed effects will recover purely supply-side effects for each lender. That is, ρ does not contain any demand shocks \widehat{d} . Moreover, by construction we have

$$Cov_j(\rho_j, \beta_{ij}^r \widehat{c}_{ij}) \neq 0, \forall i$$

because the fixed effect recovers the common shock. This means I now have a proxy or measure for the true inputs.

To summarize, if we are able to make demand shocks conditionally mean zero for each lender, then we are able to recover a measure of the lender's average cost shock. This is a powerful insight since the method is non-parametric and therefore agnostic about the source of the shock.

Measured Shock and Measurement Error Assuming the identification condition holds, I replace the true inputs $\beta_{ij}^r \widehat{c}_{ij}$ with the estimated lender fixed effects to construct a measure of the true shock

$$\underbrace{s_i}_{\text{measured shock}} \equiv \frac{1}{2} \sum_j (\omega_{ij,t} + \omega_{ij,t-1}) \rho_j. \quad (14)$$

What is the relationship between the true shock 10 and my measure 14? Recall the measurement relationship from Section 2

$$s_i = \pi \widehat{S}_i + v_{si}.$$

I would like to know first, that π will not be zero, and second the mean of the residual. If π is zero then my measured shock will not be correlated with the true shock and so it will not be useful for accounting purposes. If the mean of the residual is non-zero then I will have to construct the lower bound to the aggregate direct contribution. The answer falls into two broad cases distinguished by a specific kind of selection between lenders and areas. Here I describe the case with no selection as the case with selection is unlikely to be quantitatively relevant.³⁵

Proposition One *The measured shock 14 can be decomposed into $\pi \widehat{S}_i + v_{si}$, where $\pi = E_j(E_i(\epsilon_{ij}^{Lr})) \neq 0$ so long as the price elasticity of demand (ϵ_{ij}^{Lr}) is non-zero. The measured shock will compress ($|\pi| < 1$), expand ($|\pi| > 1$), or match ($|\pi| = 1$) the true shock. In monopolistic competition it must be the case that $|\pi| \geq 1$. The expected measurement error depends on interactions between market share and structural elasticities and will not, in general, be zero.*

³⁵The significance of selection is effectively a question about the value of the covariances

$$\text{Cov}_i(\omega_{ij,t} + \omega_{ij,t-1}, \beta_{ij}^r e_{ij}^c).$$

This covariance will plausibly be overstated by

$$\text{Cov}_i(\omega_{ij,t} + \omega_{ij,t-1}, \widehat{L}_{ij})$$

which I estimate to be essentially zero for all lenders. I also estimate the covariance with changes in denial rates, a measure of price, and find zeros. This suggests this type of selection is unlikely to be quantitatively important.

See appendix A.3 for the proof, which involves only simple algebra and manipulation of identities. So long as the price elasticity of demand is non-zero the measured shock will be correlated with the true shock. The measurement error will only have mean zero in knife-edge cases and without knowing the joint distribution of elasticities, shocks, and weights I cannot sign the mean of the measurement error.

Summary The work above shows that I can construct a measured shock that is related to the true shock, but we can expect to misstate the size of the true shock. I show in appendix A.1 that even if the shock’s size is being misstated, I can still construct a lower bound to the aggregate direct contribution of the household credit channel. I now implement this procedure.

5.2 Empirical Results

I estimate the lender fixed effects using the growth rate of county-lender non-refinance mortgages calculated from HMDA data between the sum of lending in 2005-2006 and the sum in 2008-2009. Results are also robust to aggregating to the metropolitan or commuting zone level. I also restrict the sample to lenders operating in at least 30 counties, but alternative cutoffs also work. I drop extreme outliers, but my results are robust to winsorizing or not adjusting these observations. The average area-lender percentage change over this period is about -24% (median -35%) with a very wide distribution (10th percentile is -.86% and the 90th percentile is 58%).

These restrictions reduce the sample from almost 9,000 individual lenders (after aggregating to the top parent) to 360 lenders with over 67,000 lender-county observations. After exclusions, the median number of counties operated in is 64, 40 at the 25th percentile, and 115 at the 75th percentile. Despite dropping so many lenders the remaining sample covers 52% of all lending in 2005-2006 and 66% in 2008-2009. This large change in coverage is striking. Many of the smaller lenders and brokerages likely to drop from the sample are generally thought to have depended on the securitization market, so that their contraction is plausibly due to supply-side shocks (Engel and McCoy [2011]). To the extent that I ignore the contraction from these lenders, I will likely be underestimating the size of the supply shock.³⁶ The estimation sample also represents about 51% of all assets reported on call reports in 2006. Table 15 reports the regulator statistics for the entire

³⁶Some of this difference might also be due to the decline in “double-counting” of loans in HMDA as securitizations and purchase agreements fell. See Scheessele [1998], for a discussion of double-counting in the HMDA data.

sample of HMDA lenders and the estimation sample. While the percentage of bank lenders (OCC, FRB, or FDIC) is roughly similar (over 50%), we see that the restrictions significantly reduce the number of small lenders as indicated by the decline in share of FDIC- and NCUA-regulated lenders. The estimation sample has a roughly equivalent share of HUD-regulated lenders as the overall sample.

A key assumption is the conditional version of the identification condition 12, or that demand shocks are rendered mean-zero after being projected onto the space of borrower fixed effects. As an intuitive check of this condition I estimate 11 with two levels of fixed effects: state and county.³⁷ If the identification assumption does not hold then I would expect the lender fixed effects to vary substantially as I include better “controls” for borrower demand (as I replace state fixed effects with county fixed effects). Figure 10 plots the two sets of lender fixed effects against each other. Both distributions tend to be concentrated below zero and are fairly symmetric, suggesting most but not all lenders were contracting credit. The extremely tight correlation, with the scatter plot essentially lying along the 45-degree line, shows that adding lower-level fixed effects has almost no effect on the lender estimates. If selection into counties had been biasing the lender fixed effects down, for example, then this scatter plot would lie predominantly above the 45-degree line: controlling for the county-level demand shocks would absorb much of the negative effect. This suggests the identification assumption is plausible. This figure also highlights the extreme nature of Wachovia’s lending contraction as Wachovia is easily one of the 10 most negative fixed effects.

Measured Shock and Instrumental Variable Estimation Given that the identification assumption appears reasonable, I use the lender fixed effects conditional on county fixed effects to construct the measured shock 14. The theoretical expenditure share required by the price index for household credit is a conceptually and practically difficult quantity.³⁸ To simplify, I assume the true expenditure shares are given by the shares of household credit lending in the HMDA data: ω_{ijt} is equal to lender j ’s quantity of lending to area i divided by the total quantity of credit borrowed by area i at time t . I construct the weights for the same periods as the fixed effect estimates.

³⁷Estimating the fixed effects is actually non-trivial (see McCaffrey, Lockwood, Mihaly and Sass [2010]). I use the code procedure *reg2hdfe* provided by Guimaraes and Portugal [2010], which preserves the mean and estimates a full set of lender fixed effects.

³⁸Without knowing utility functions it is impossible to properly weight the various costs, both pecuniary and non-pecuniary (denial probability, down-payment, flexible vs. fixed interest rate, etc) that compose the price. Even if we knew the weights implied by the utility function, data on these additional prices are generally not available.

Figure 11 shows the distribution of the measured shock across all counties in the South and East subsample. Within this subsample the shock is always negative with an average of about -.12.

Recall from Section 2 that even if the measured shock reflects only the true shock and noise, it is still necessary to use an instrumental variable to get consistent estimates. This is because the true shock is unlikely to be randomly distributed across areas and because noise will attenuate my estimates. I showed that Wachovia was a valid instrumental variable for supply shocks to household credit, so I employ it here as an instrument for the measured shock. Intuitively, from Figure 10 and Section 4, I know that Wachovia had a very negative effect on household credit supply, so when I know a county is exposed to Wachovia I expect that the measured shock to also be lower (more contractionary). However, exposure to Wachovia is also useful because I documented that it likely satisfies the exclusion restriction, and so avoids omitted variable bias.

Table 16 shows that exposure to Wachovia is robustly associated with the measured shock. The point estimate is very stable across specifications and not driven by outliers (column three) or state-level shocks (column four). The validity of the instrument hinges on Wachovia only affecting the measured shock through Wachovia's own shock. This implies that I should find no association between Wachovia exposure and the measured shock excluding Wachovia's fixed effect from the measured shock. If I do find a negative relationship it would suggest that exposure to Wachovia is correlated with exposure to other lenders who are transmitting contractionary shocks. This would lead to concern about the validity of the exclusion restriction, although it would not necessarily invalidate it. Column six excludes Wachovia's fixed effect from the measured shock and show that there is essentially no relationship with exposure to Wachovia. This also tells me that exposure to Wachovia is not correlated with measurement error, again validating the instrument. Figure 12 visually confirms the strong negative relationship between exposure to Wachovia and the measured shock.

Given these strong diagnostics, I instrument for the measured shock with exposure to Wachovia to recover β^{ES}/π , the elasticity of interest normalized by the measurement coefficient. I report the results in Table 17.³⁹ First, the OLS estimate (unreported) suggests the measured shock has an effect on employment, but it is not significantly different from zero. The WLS estimate is much more precise and recovers a similar estimate: a one percentage point in the measured shock implies

³⁹I correct for the generated regressor with the clustered pairs bootstrap as before.

a decline in employment of about .6%. However, the instrumented estimates show that the direct estimate is significantly biased downward. Column two gives an estimate of 1.4%, while controlling for industry shocks and mortgage leverage in column four suggests the effect on employment is about 1.1%, still twice as large as the direct estimates. Recall that π , which is essentially the average price elasticity of demand faced by lenders, must be at least one since I have assumed monopolistic competition. This means that $\beta^{ES} \geq \beta^{ES}/\pi$ so that the true elasticity of employment with respect to credit supply shocks is at least one. Employment excluding tradables, reported in Column four gives almost the same estimate.

5.3 Aggregate Direct Contribution

With estimates of β^{ES}/π , Appendix A.1 shows that I can calculate a lower bound to the aggregate direct contribution of shocks to household credit with the following sum (also see Section 2)

$$\underline{\text{aggregate direct contribution}} = \frac{\beta^{ES}}{\pi} \sum_i \omega_i (s_i - \sum_{j:s_j \geq k^*} \tilde{\omega}_j s_j). \quad (15)$$

I sum the measured shock and then subtract the average of measured shocks greater than some number. I then multiply this sum by the instrumented estimate of β^{ES}/π . So long as the conditional average of the true shock has the same sign as the true aggregate direct contribution then the resulting sum will converge to a lower bound to the desired quantity.

Table 18 reports the estimated effects on total employment for the estimation sample and for the national sample of counties with sufficiently large population. The first column uses the coefficient from column three of Table 17, to arrive at the effect on total employment. The second column uses the coefficient for employment excluding tradables from column four of Table 17. I then weight each area by the share of total employment given by its employment excluding tradables. The resulting quantity is the implied effect on total employment arising from declines in employment, ignoring the tradable sector. For reference, the total national employment decline in the CBP data is about 7.2% while the total decline in the South and East is 6%.

The first row reports the aggregate direct contribution without subtracting any quantity, that is assuming the weighted sum of measurement error is mean zero. Ignoring bias in the measure indicates the shock to household credit was massive: it predicts more of a decline in employment

than actually observed. While it is theoretically possible that spillovers or other shocks undid much of this partial equilibrium effect, it is likely that the measured shock is overstating the true shock. The second row sets k^* equal to the 75th percentile of the measured shock within the subsample and then computes 15. The third row does the same within the national sample of counties. The first column shows that shocks to household credit account for at least 60 to 62% of the observed declines in employment (3.6% decline in the subsample and 4.5% nationally). The second column shows that limiting this calculation to effects on just non-tradable (in other words, excluding tradables) industries gives very similar magnitudes. If I am even more conservative and set k^* equal to the 66th percentile the lower bound falls to 52/53% in the first column.

These effects are large. Differential price movements might undo some of these effects through trade, but Stumpner [2013] shows that the trade channel amplified demand shocks similar to this one, which would mean the household credit channel would be responsible for even more employment losses. Aggregate price effects or policy responses might also undo some of these partial equilibrium effects, but the fact that the zero lower bound was binding for much of this period limits the extent to which the safe interest rate could undo these losses.

For comparison, Greenstone et al. [2012] assume all of the decline in small business credit was due to supply, which captures at most 16% of the decline in total employment. Chodorow-Reich [2014] performs a similar accounting exercise for the contribution of firm credit shocks on employment from 2007-2009 and calculates a lower bound for the aggregate direct contribution between 34 and 47% of 7%. Together with my calculation, these numbers suggest the crisis-induced contraction in credit was responsible for almost all of the decline in employment. In contrast to both of these accounts, Mian and Sufi [2014] suggest that over 50% of the decline in overall employment is due to the shock to household net worth. But this calculation is made by assuming most of the change in household net worth is exogenous, whereas much of this decline is likely the result of credit supply shocks. This suggests their calculation overstates the household net worth channel. This is a distinction between amplification and shock since both the household and firm credit channels induce changes in household net worth and so will result in amplification due to changes in household net worth. Hall [2012] provides a full general equilibrium accounting of the Great Recession and subsequent slump into what he terms the financial friction and household deleveraging channels. This distinction is essentially between financial frictions affecting firms and

those affecting households. Hall finds that, at impact, the household credit channel is responsible for roughly 30% of the decline on impact with this share generally declining. Given the relative size of my and Chodorow-Reich's calculations this suggests that either general equilibrium effects amplify the firm credit channel and dampen the household credit channel or that the decomposition of the data given by Hall's model is biased in some manner.

6 Conclusion

Whether or not the contraction in household credit supply was an important cause of the decline in employment during the Great Recession is critical to understanding the recession and developing policy responses to similar events. Using cross-sectional variation and exposure to the lender Wachovia as a novel instrument, I show that contractions in the supply of credit to households caused large declines in housing and non-housing demand. The fall in household demand caused large losses in construction and non-tradable employment, giving an elasticity of total employment with respect to household credit of 0.3. I then account for the aggregate effect of these shocks by constructing a measure of the shock. With a simple model of credit markets, I show how to identify lender-specific shocks to household credit using data on lender-borrower credit flows. I then use these lender-specific shocks to construct a measure of the shock to an area. I compute that contractions in the supply of household credit caused employment to fall by at least 3.6%, about 60% of the observed decline. While this calculation ignores the general equilibrium response of aggregate prices and policy, it suggests the recession would have not have been as severe if the supply of credit to households or household demand had been maintained.

There are numerous avenues for future research. It would be informative to use the quantities I recover here to calibrate a model that is explicitly cross-sectional. This would allow us to understand how the partial equilibrium aggregate quantity and cross-sectional elasticity recovered here discipline the aggregate general equilibrium quantities in which we are ultimately interested. Further work on the state-dependence of the response to household credit shocks is also critical to understand when financial shocks have large macroeconomic effects and when they do not. The policy response to Wachovia's stress ostensibly avoided its failure, but I document that there were still significant effects on the real economy even after the policy response. This suggests there might

be substantial room to better understand and improve the policy response to distressed financial institutions. It is also of first order importance to understand why there are significant frictions to substitution across lenders in the household credit market; without these frictions the failure of a particular institution would be largely irrelevant. Finally, it is striking that the losses in employment and credit seem to persist as a level effect through the end of the observation period. This indicates that there might be potential distortions in productivity or the presence of persistent demand effects related to household balance sheets.

A Appendix

A.1 Constructing a Lower Bound to the Aggregate Direct Contribution

The logic for constructing the lower bound is simple. Assume there is a subsample of the data such that the weighted average of the true shock has the same sign as the weighted average of the true shock across the entire sample. Also assume that measurement error has the same weighted mean in both samples. If I take the average of the difference between the measured shock and the average within a subsample, the measurement error will cancel. I will then be left with the average of the difference between the actual shock and the actual shock's average in the subsample. Since these have the same sign, I will have a smaller quantity (pushed towards zero).

First, I state a standard result about the convergence of weighted averages of random variables from Etemadi [2006]. Let w_i be a sequence of positive weights where $W_n = \sum_{i=1}^n w_i \rightarrow \infty$,

$$\sup_{n \geq 1} \frac{nw_n}{W_n} < \infty \quad \text{and} \quad \sup_{n \geq 2} \sum_{i=1}^{n-1} \left(\frac{i|w_{i+1} - w_i|}{W_n} \right) < \infty. \quad (16)$$

Then if $\{X_i\}$ is a sequence of random variables

$$\frac{1}{n} \sum_{i=1}^n X_i \xrightarrow{\text{plim}} X_0 \Rightarrow \frac{1}{W_n} \sum_{i=1}^n w_i X_i \xrightarrow{\text{plim}} X_0.$$

With this result in hand it is straightforward to construct a lower bound to the true aggregate direct contribution.

Proposition Two *Assume (1) the true shock \widehat{S}_i is distributed with a non-zero mean, (2) the measurement error is distributed iid with mean μ_e , (3) sequences $\omega_i \equiv w_i/W_i$, $\tilde{\omega}_j \equiv \tilde{w}_j/\widetilde{W}_j$ both satisfy the conditions in 16, and (4) $\exists k^* \in \mathbb{R}$ such that*

$$\text{sgn}\left(\sum_i \omega_i \widehat{S}_i\right) = \text{sgn}\left(\sum_{j:s_j \geq k^*} \tilde{\omega}_j \widehat{S}_j\right). \quad (17)$$

Then the object

$$\underline{\text{aggregate direct contribution}} \equiv \frac{\beta^{ES}}{\pi} \sum_i \omega_i (s_i - \sum_{j:s_j \geq k^*} \tilde{\omega}_j s_j) \quad (18)$$

converges in probability to a lower bound to the true aggregate direct contribution in that the limit

of the weighted difference is greater than the aggregate direct contribution if the aggregate direct contribution is less than zero. The opposite is true if the true aggregate direct contribution is greater than zero.

Assumption (1) ensures the true aggregate direct contribution is always interesting (i.e. signed). Given Assumptions (2) and (3) it follows that for any number k such that the set $s_j : s_j \geq k$ is non-empty

$$\text{plim} \sum_i \omega_i v_{si} = \text{plim} \sum_{j:s_j \geq k} \tilde{\omega}_j v_{sj} = \mu_e. \quad (19)$$

Then I can construct the following object

$$\begin{aligned} \underline{\text{aggregate direct contribution}} &= \frac{\beta^{ES}}{\pi} \sum_i \omega_i (s_i - \sum_{j:s_j \geq k^*} \tilde{\omega}_j s_j) \\ &= \beta^{ES} \sum_i \omega_i \widehat{S}_i - \beta^{ES} \sum_{j:s_j \geq k^*} \tilde{\omega}_j \widehat{S}_j + \frac{\beta^{ES}}{\pi} \left(\sum_i \omega_i v_{si} - \sum_{j:s_j \geq k^*} \tilde{\omega}_j v_{sj} \right) \\ &\xrightarrow{\text{plim}} \text{aggregate direct contribution} - \beta^{ES} \sum_{j:s_j \geq k^*} \tilde{\omega}_j \widehat{S}_i. \end{aligned}$$

Assumption (4) ensures the terms on the right-hand side have the same sign, so it must be the case that if the aggregate direct contribution is positive then the object constructed will always be less than the actual aggregate direct contribution, and if the aggregate direct contribution is negative then this relationship is reversed. Therefore, the object calculated here will understate the aggregate direct contribution of shocks in the limit.

A.2 Identification Conditions

Let N be the number of observations, then in matrix notation model 11 becomes

$$\widehat{L} = D\alpha + S\rho + e,$$

where S is $N \times J$, D is $N \times I$, and α and ρ are $I \times 1$ and $J \times 1$ respectively. Let $P_D \equiv D(D'D)^{-1}D'$ (the projection matrix to the space of borrower dummies). Then we have the following standard partitioned regression expression for the coefficients on the lender dummies

$$\rho = (S'(1 - P_D)S)^{-1}S'(1 - P_D)\widehat{L}.$$

Let A be the $I \times 1$ vector of common demand shocks for each area, e^a the $J \times I$ matrix of lender-borrower demand shocks, C the $J \times 1$ vector of common cost shocks for each lender, and e^c be the $J \times I$ matrix of lender-borrower cost shocks. Γ and B are the matrices of structural parameters γ and β multiplying the demand and supply shocks with the same dimensions as e^a and e^c respectively. Finally, let $\mathbf{1}$ be an $N \times 1$ vector of ones. Then the matrix representation of 8 is the following where \circ is the Hadamard/Schur product

$$\widehat{L} = S\Gamma A + S(\Gamma \circ e^a)D'\mathbf{1} + DBC + S(B \circ e^c)D'\mathbf{1}. \quad (20)$$

Then the numerator of our lender estimates becomes

$$S'(1 - P_D)\Delta L = S'(1 - P_D)(S\Gamma A + S(\Gamma \circ e^a)D'\mathbf{1} + DBC + S(B \circ e^c)D'\mathbf{1}).$$

In order for the lender fixed effects to be completely purged of demand shocks I require the following conditions hold (in expectation)

$$S'(1 - P_D)S\Gamma A = 0, \quad (21)$$

$$S'(1 - P_D)S(\Gamma \circ e^a)D'\mathbf{1} = 0. \quad (22)$$

A.3 Decomposition of Measured Shock

No selection between lenders and areas means that there will be zero covariance between (1) area-lender weights and the idiosyncratic shocks, (2) area-lender weights and the structural elasticities, and (3) idiosyncratic shocks and structural elasticities. The true shock to an area i is simply

$$\widehat{S}_i = \frac{1}{2} \sum_j (\omega_{ij,t} + \omega_{ij,t-1}) \beta_{ij}^r (C_j + e_{ij}^c). \quad (23)$$

The measured shock to the same area i is

$$\begin{aligned} s_i &= \frac{1}{2} \sum_j (\omega_{ij,t} + \omega_{ij,t-1}) E_i(\beta_{ij}^L) C_j \\ &= \frac{1}{2} \sum_j (\omega_{ij,t} + \omega_{ij,t-1}) E_i(\epsilon_{ij}^{Lr} \beta_{ij}^r) C_j \end{aligned}$$

$$\begin{aligned}
&= \frac{1}{2} \sum_j (\omega_{ij,t} + \omega_{ij,t-1}) (E_i(\epsilon_{ij}^{Lr}) E_i(\beta_{ij}^r) + Cov_i(\epsilon_{ij}^{Lr}, \beta_{ij}^r)) C_j \\
&= \frac{1}{2} \sum_j (\omega_{ij,t} + \omega_{ij,t-1}) \underbrace{[(E_j(E_i(\epsilon_{ij}^{Lr})) + \widehat{E_i(\epsilon_{ij}^{Lr}))} (\beta_{ij}^r + \widehat{E_i(\beta_{ij}^r))} + Cov_i(\epsilon_{ij}^{Lr}, \beta_{ij}^r)]}_{\equiv E_i(\epsilon_{ij}^{Lr})} C_j \\
&= \underbrace{E_j(E_i(\epsilon_{ij}^{Lr}))}_{\equiv \pi} \frac{1}{2} \sum_j (\omega_{ij,t} + \omega_{ij,t-1}) \beta_{ij}^r C_j + \\
&\quad \underbrace{\frac{1}{2} \sum_j (\omega_{ij,t} + \omega_{ij,t-1}) [\widehat{E_i(\epsilon_{ij}^{Lr})} \beta_{ij}^r + \widehat{E_i(\epsilon_{ij}^{Lr})} \widehat{E_i(\beta_{ij}^r)} + E_j(E_i(\epsilon_{ij}^{Lr})) \widehat{E_i(\beta_{ij}^r)} + Cov_i(\epsilon_{ij}^{Lr}, \beta_{ij}^r)]}_{\equiv \psi_i} C_j \\
&= \pi \widehat{S}_i - \pi \frac{1}{2} \sum_j (\omega_{ij,t} + \omega_{ij,t-1}) \beta_{ij}^r e_{ij}^c + \psi_i \\
&\equiv \pi \widehat{S}_i + v_{si}.
\end{aligned}$$

The parameter π is simply the average across lenders of the average across areas of the demand elasticity faced by lenders, which we fully expect to be negative and, importantly, non-zero. This is exactly the relationship posited. The key questions then are about the sign and magnitude of the expected measurement error: are $E_i(v_{si})$ (and $\sum_i \omega_i v_{si}$) zero, positive, or negative?

In general it is infeasible to sign or quantify the terms composing v_{si} without knowing the joint distribution of these variables. Holding other elasticities constant, the pass-through parameter is declining as the price elasticity of demand becomes more negative. This means the unweighted average of $\widehat{E_i(\epsilon_{ij}^{Lr})} E_i(\beta_{ij}^r)$ is likely negative, but I take the weighted average where the weights reflect market share. The correlations between market share and the demand elasticity and market share and pass-through are theoretically ambiguous.⁴⁰ Similarly, the sign of $Cov_i(\epsilon_{ij}^{Lr}, \beta_{ij}^r)$ depends on the size of the other structural elasticities as well as the relationship with market share. Thus, it is not only possible but likely that the average, weighted or not, measurement error is nonzero.

⁴⁰Assumptions about competition and the shape of demand can matter. Dornbusch [1987] and Marquez [1994] provide models of where pass-through is increasing in market share. Feenstra, Gagnon and Knetter [1996] shows this relationship can be strongly non-linear. Weyl and Fabinger [2013] provide an excellent discussion of pass-through and welfare under various arrangements.

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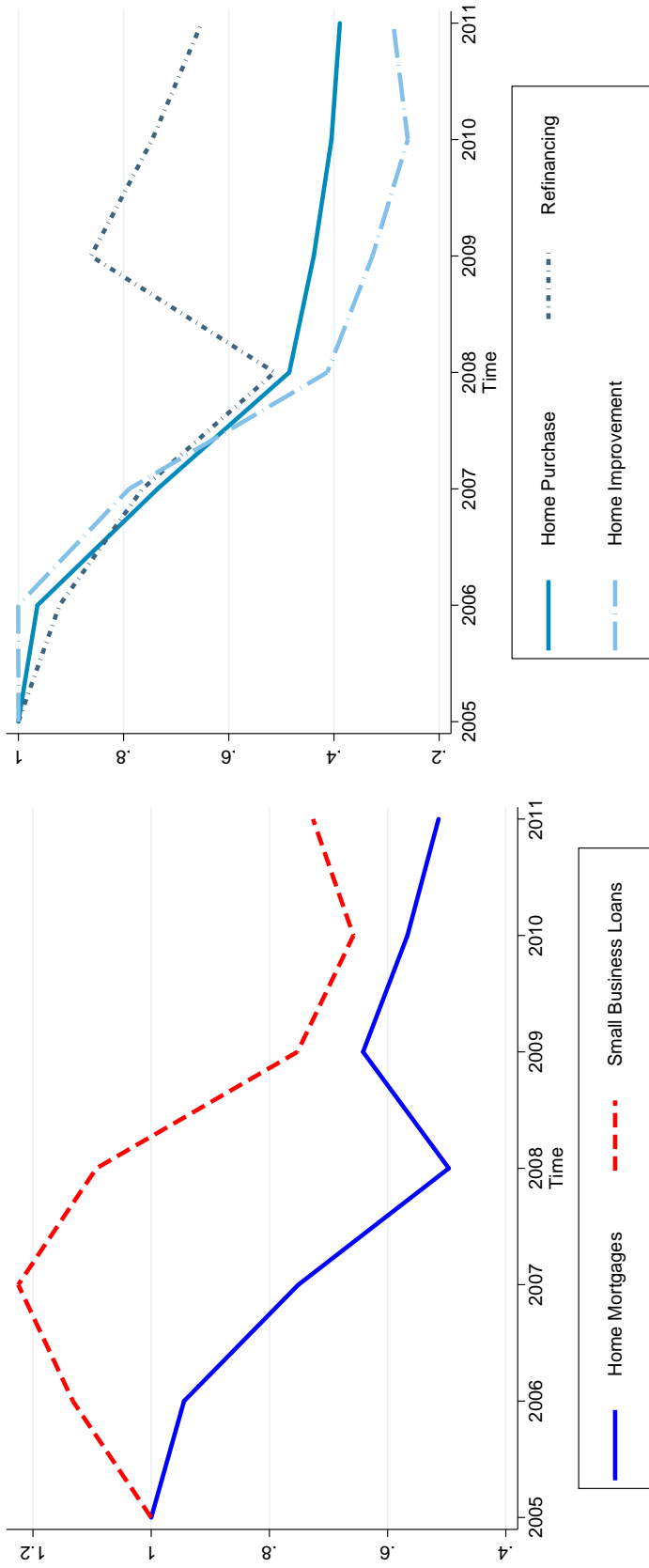
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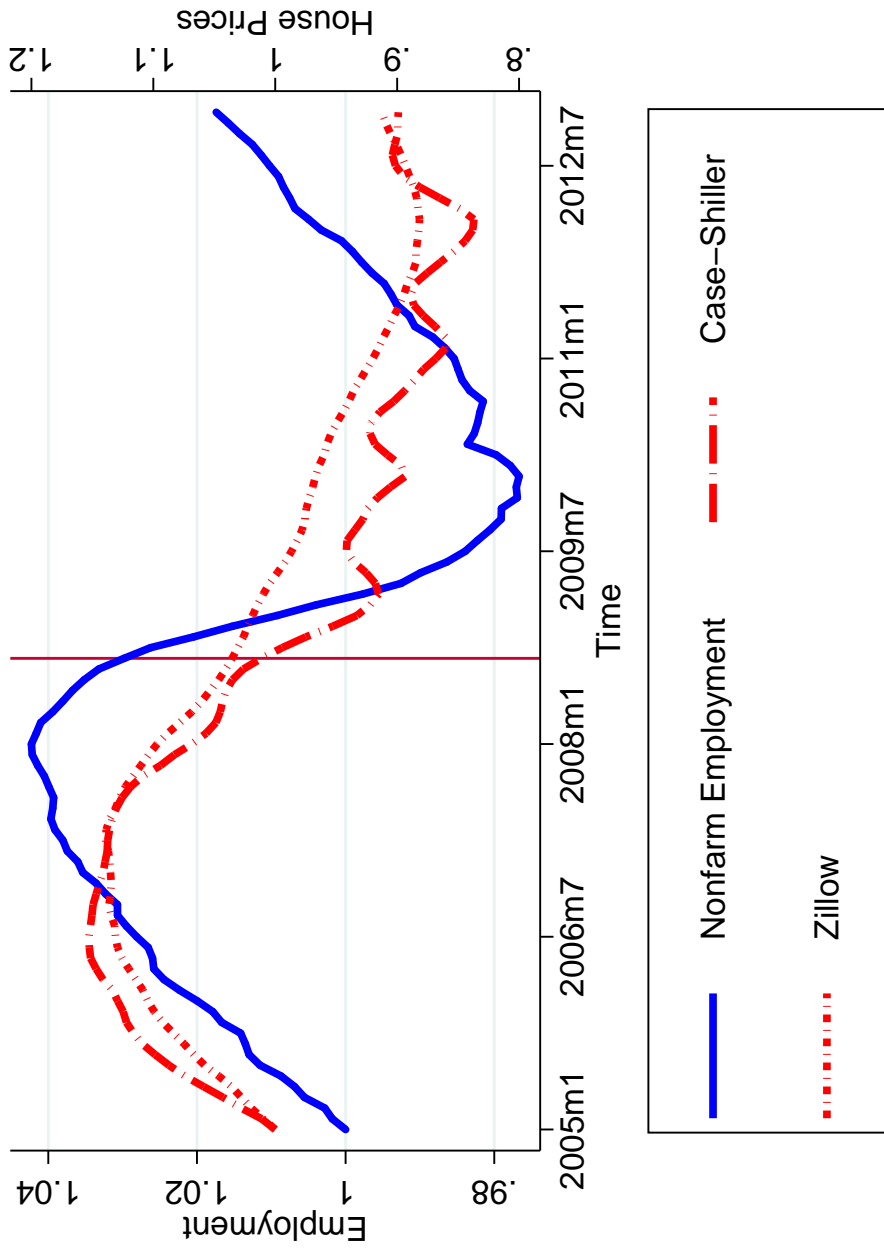
B Figures

Figure 1: Aggregate Credit Flows Normalized to 2005 Level



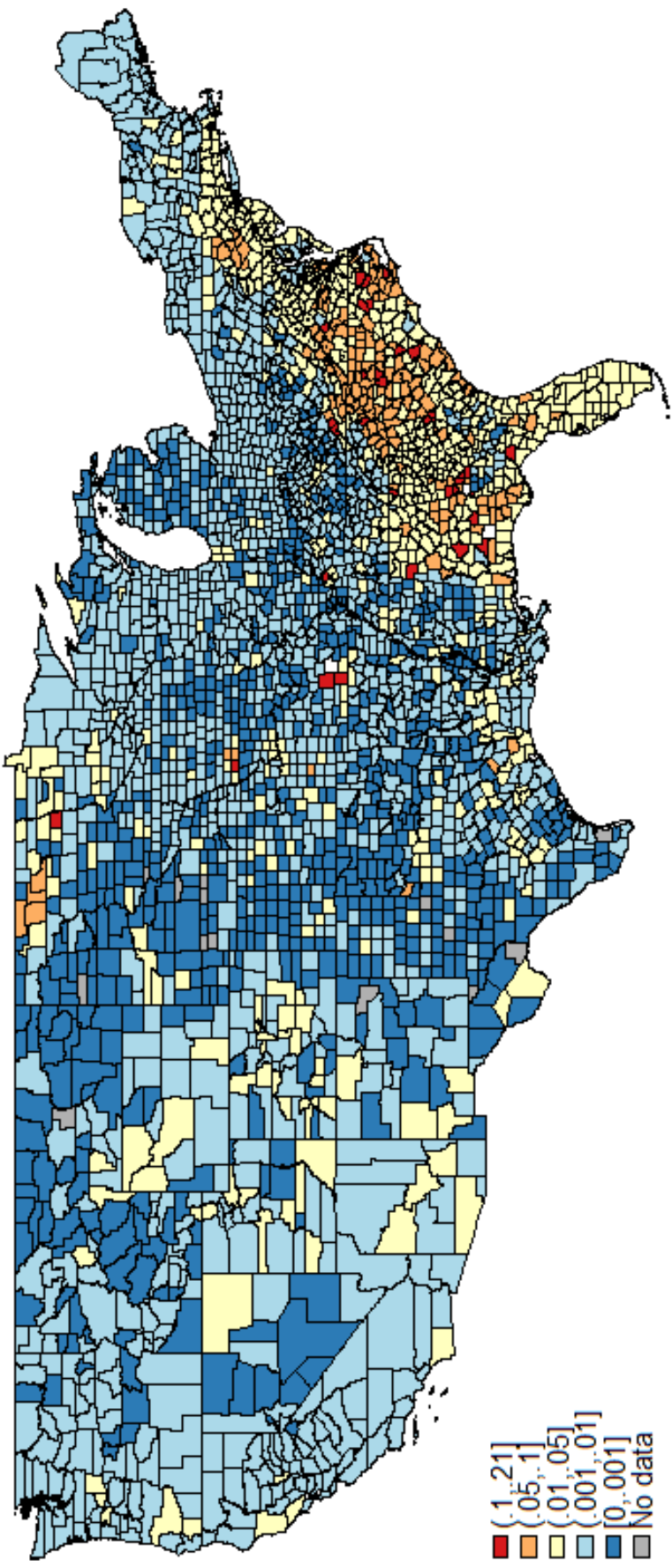
Note: The left panel plots total flows of home mortgage and small business loans normalized to be one in 2005. We see that the mortgage market started to decline in 2006, but growth in small business credit market did not become negative until 2008. The right panel separates home mortgage lending into home purchase, home improvement, and refinancing loans, all normalized to one in 2005. Both home purchase and home improvement mortgages decline through 2010 or 2011 while refinancing loans spike in 2009 and 2010 when the Federal Funds Rate drops to zero. Home improvement loans were roughly 3% of the total home mortgage market (by origination value) in 2005 while home purchase loans were about 48%. Mortgage calculations are from the HMDA data and small business loan calculations are from the CRA data.

Figure 2: Nonfarm Employment and House Prices Normalized to January 2005 Level



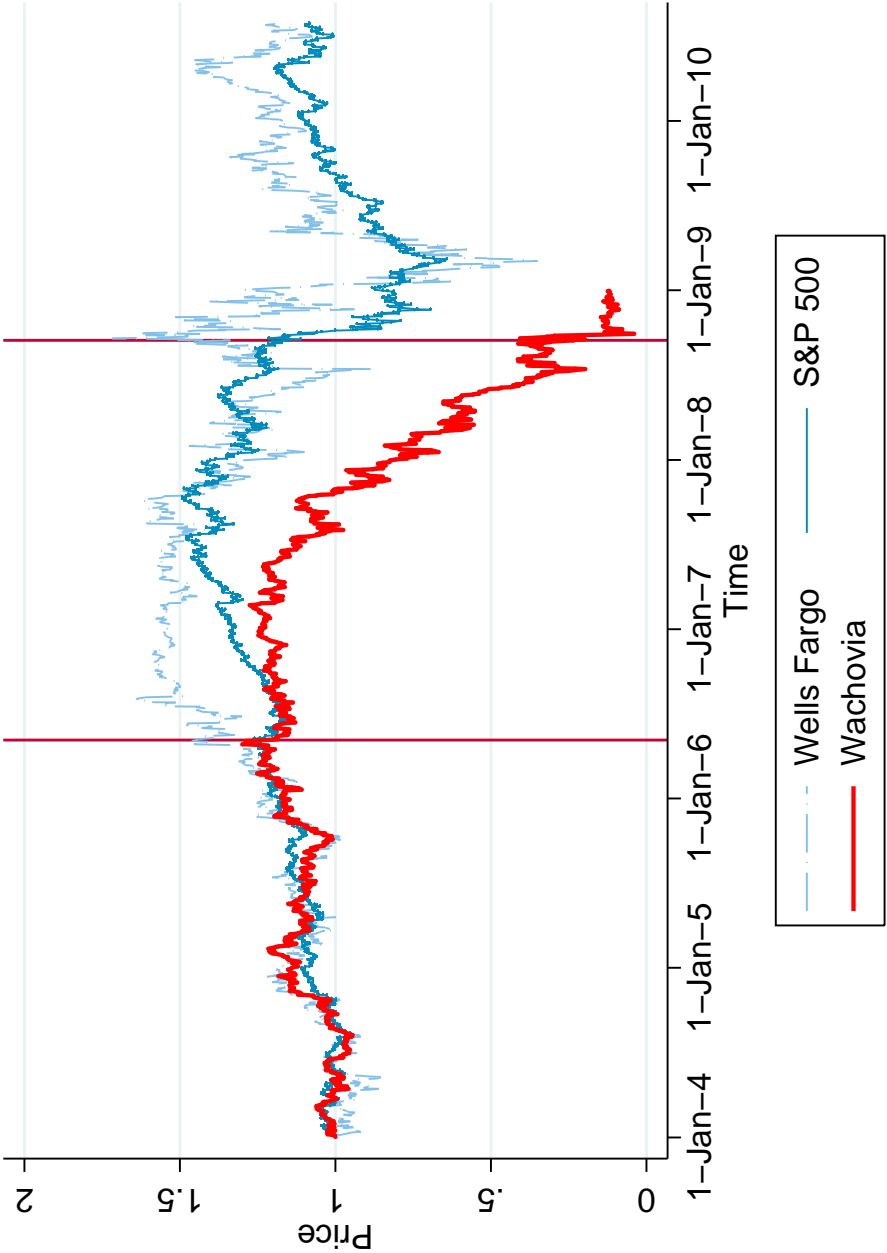
Note: This figure plots total non-farm employment from the BLS (left axis) and the Case-Shiller and Zillow national house price indexes (right axis) at monthly frequency, all normalized to be one in January 2005. We see that house prices stopped growing by mid-2006 and began to decline in 2007, falling steeply from 2008 and 2009. By contrast, employment began its decline in early 2008 and then accelerated at about the third quarter. The red line indicates the September 2008, the beginning of the peak crisis period.

Figure 3: Distribution of Average of Wachovia Share of Home Mortgage Lending in 2005-2006



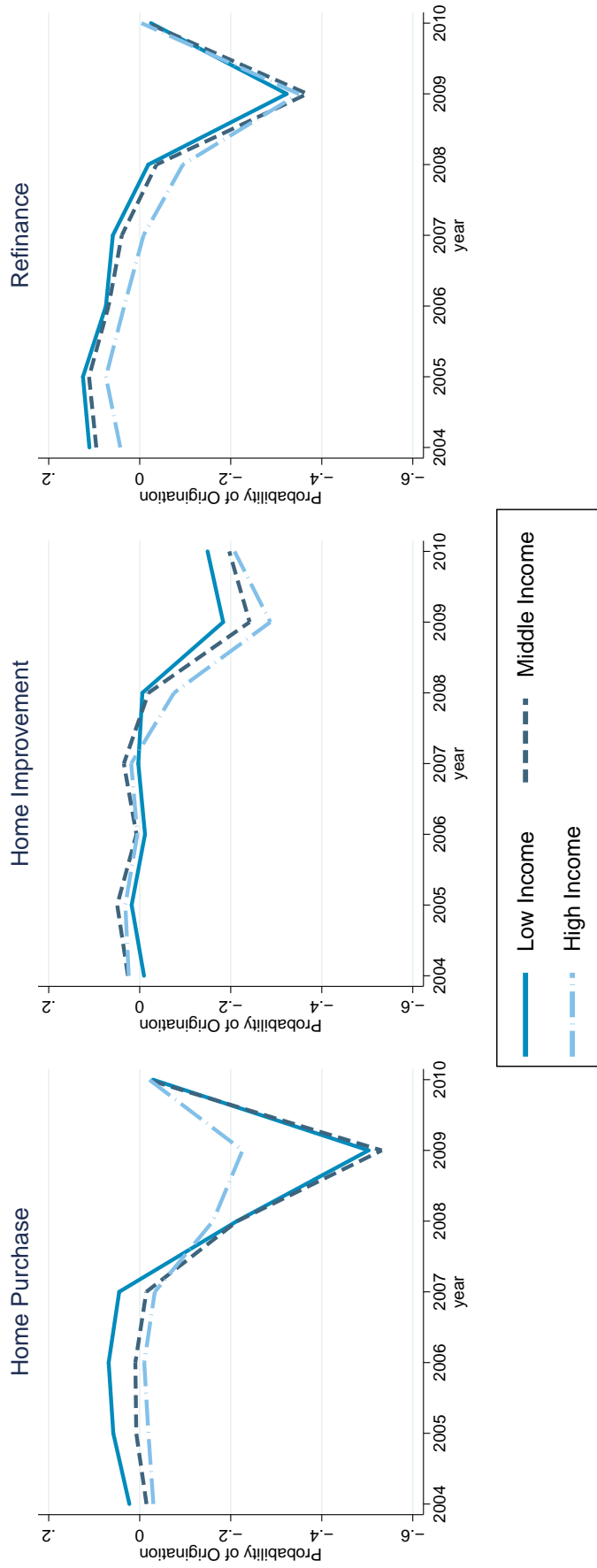
Note: This figure plots Wachovia's average market share of originated and purchased loans over 2005-2006 in the home mortgage market as measured in the HMDA data. It shows that Wachovia had a national presence, but that its market share tended to be fairly small everywhere but the East and South.

Figure 4: Collapse of Wachovia's Stock Price



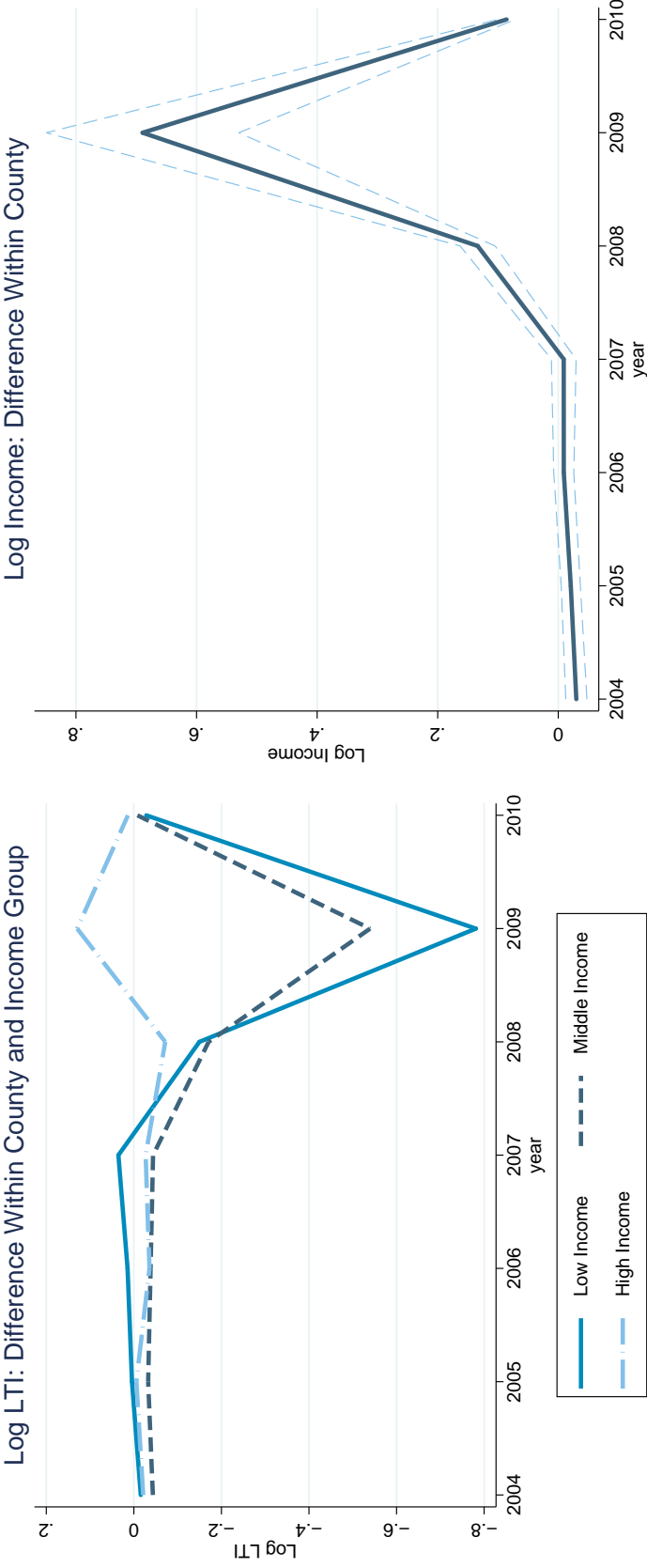
Note: This figure plots the stock prices for Wachovia and Wells Fargo and the unweighted S&P 500 index, all normalized to be one in 2004. The first red line marks Wachovia's purchase of GWF on May 7, 2006, and the second red line marks the failure of Lehman Brothers on September 15, 2008. All measures track each other relatively closely until 2006. Wachovia experienced a loss of \$1 billion dollars in market capitalization upon purchasing GWF and through 2007-2008 Wachovia's stock performed significantly worse than the broader market and Wells Fargo. In December 2008, Wells Fargo purchased Wachovia, and the trade of Wachovia stock halted. Data are daily averages from CRSP.

Figure 5: Wachovia and Origination Probabilities: Difference Within County and Income Group



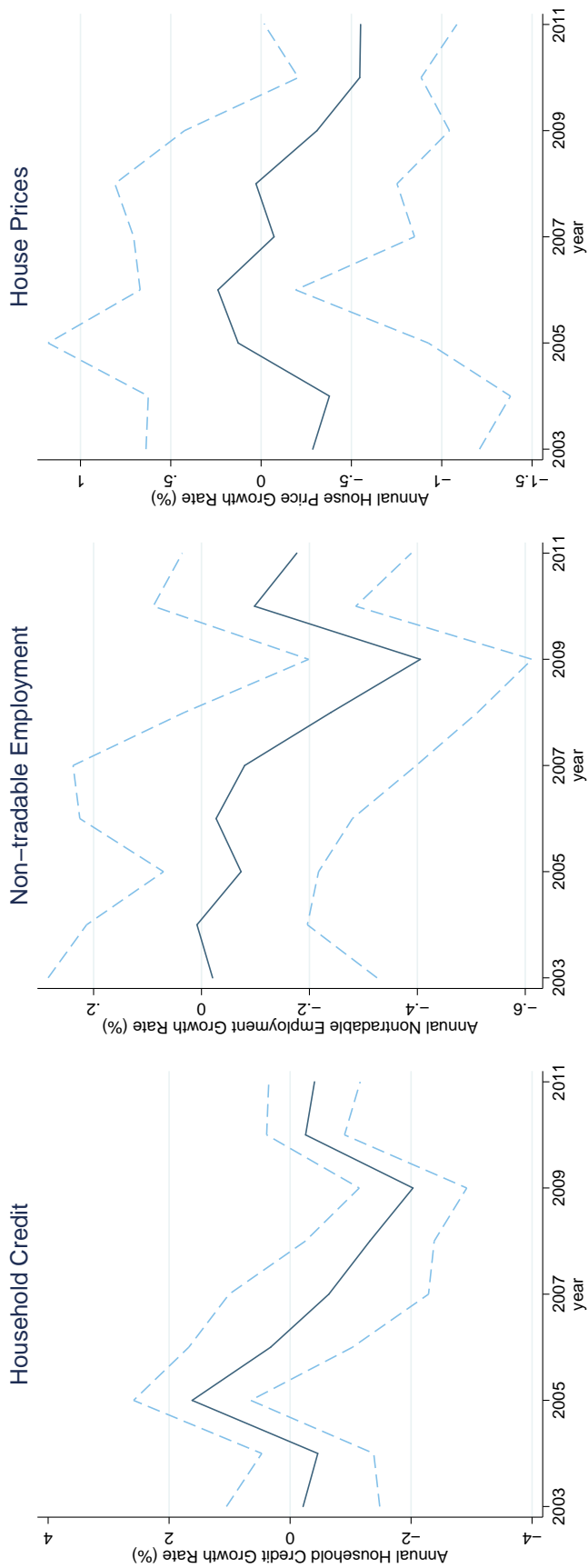
Note: These figures plot the within-county difference in origination probability for an application submitted to Wachovia relative to the average non-Wachovia lender for home purchase, home improvement, and refinance loans: $\text{Prob}(\text{Originated})_{it} = \alpha_{ct} + \beta_t \text{Wachovia}_i + \gamma_t X_{it} + e_{it}$. Each regression is run within the top, middle, and bottom third of incomes in each county. The figures show that leading up to the crisis Wachovia was an average lender in the county, but by 2008 and 2009 an applicant to Wachovia was much less likely to have an application originated. This trend is apparent in all types of loans and income groups. At the peak, this difference is over 50 percentage points for home purchase loans, 20 percentage points for home improvement loans, and 30 percentage points for refinance loans. Each regression is estimated with OLS and includes a full set of county fixed effects. Controls are the log LTI ratio, log income, an indicator for the applicant being black, first and second lien indicators, sex indicator, and an indicator for whether or not the property will be owner-occupied. I exclude all loans with an LTI greater than eight or a reported income less than five thousand. I limit the sample to all counties in the South and East with at least 2,000 valid applications. The number of observations varies from about two million to three-hundred thousand. All data are from from HMDA. Standard errors are not reported so that the figures are legible, but they are very tight.

Figure 6: Wachovia and Log LTI and Log Income on Originated Home Purchase Loans:



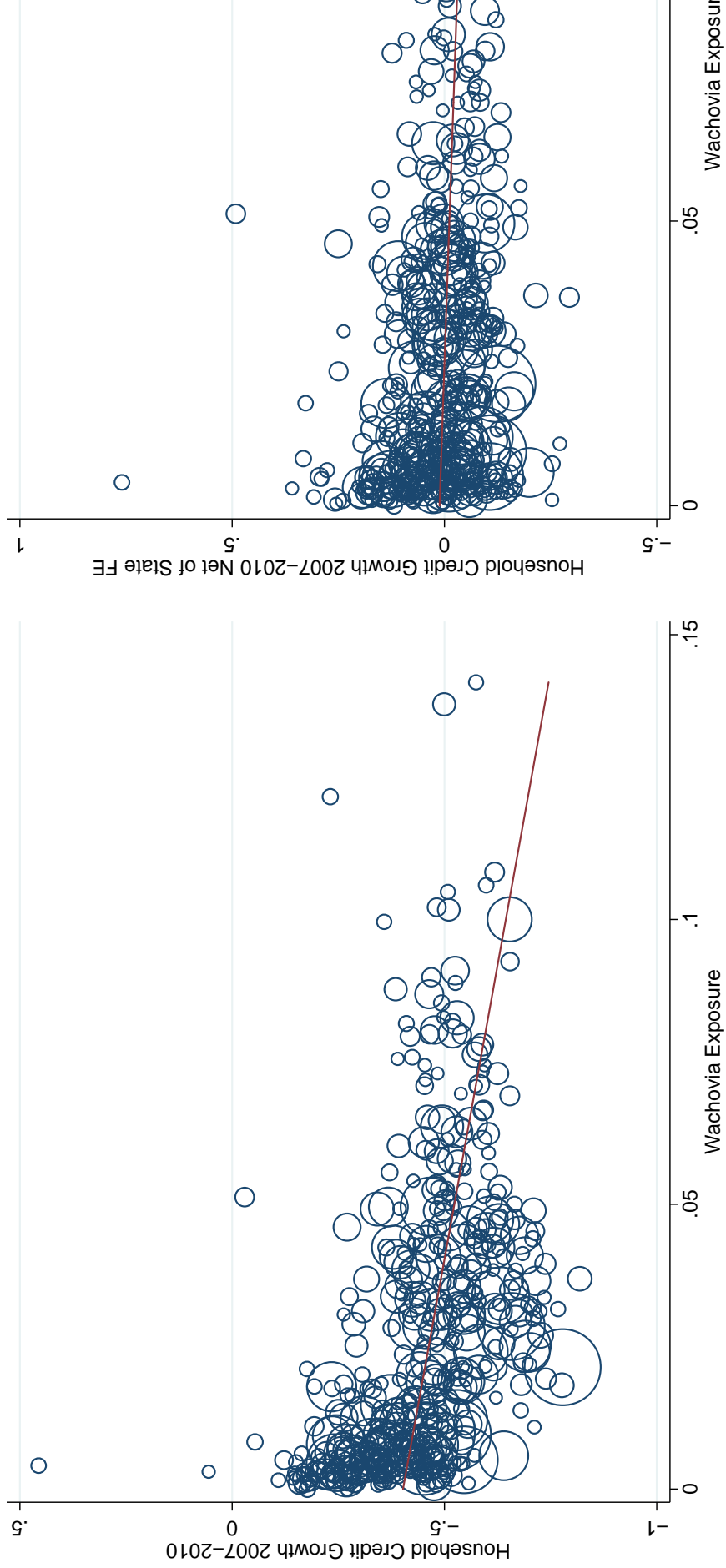
Note: The left figure plots the within-county, within-income group difference in log LTI for a home purchase loan originated by Wachovia relative to an origination by the average non-Wachovia lender: $\log(LTI)_{it} = \alpha_{ct} + \beta_t \text{Wachovia}_i + \gamma_t X_{it} + e_{it}$. Income groups are defined as the top, middle, and bottom third of incomes in each county. The right figure plots coefficients and county-clustered standard errors for regressions of log income for home purchase originations by Wachovia across all income groups. Leading up to the crisis Wachovia was essentially indistinguishable from the average lender in the county, but by 2008 and 2009 a loan originated by Wachovia had a significantly lower LTI ratio for low- and middle-income applicants while high-income applicants had a slightly higher LTI ratio. This difference disappears in 2010. Originated loans by Wachovia had average income leading up to the crisis, but in 2008 and 2009 the average income of a Wachovia origination increased by over 60%, indicating deep substitution to high-income borrowers. Controls include an indicator for the applicant being black, first and second lien indicators, sex indicator, and an indicator for whether or not the property will be owner-occupied. I exclude all loans with an LTI greater than eight, income less than five thousand, and limit the sample to all counties in the South and East with at least 2,000 valid applications. The number of observations varies from almost three million to three-hundred thousand depending on the specification.

Figure 7: Effect of Exposure to Wachovia on Annual Household Credit, Non-tradable Employment, and House Price Growth



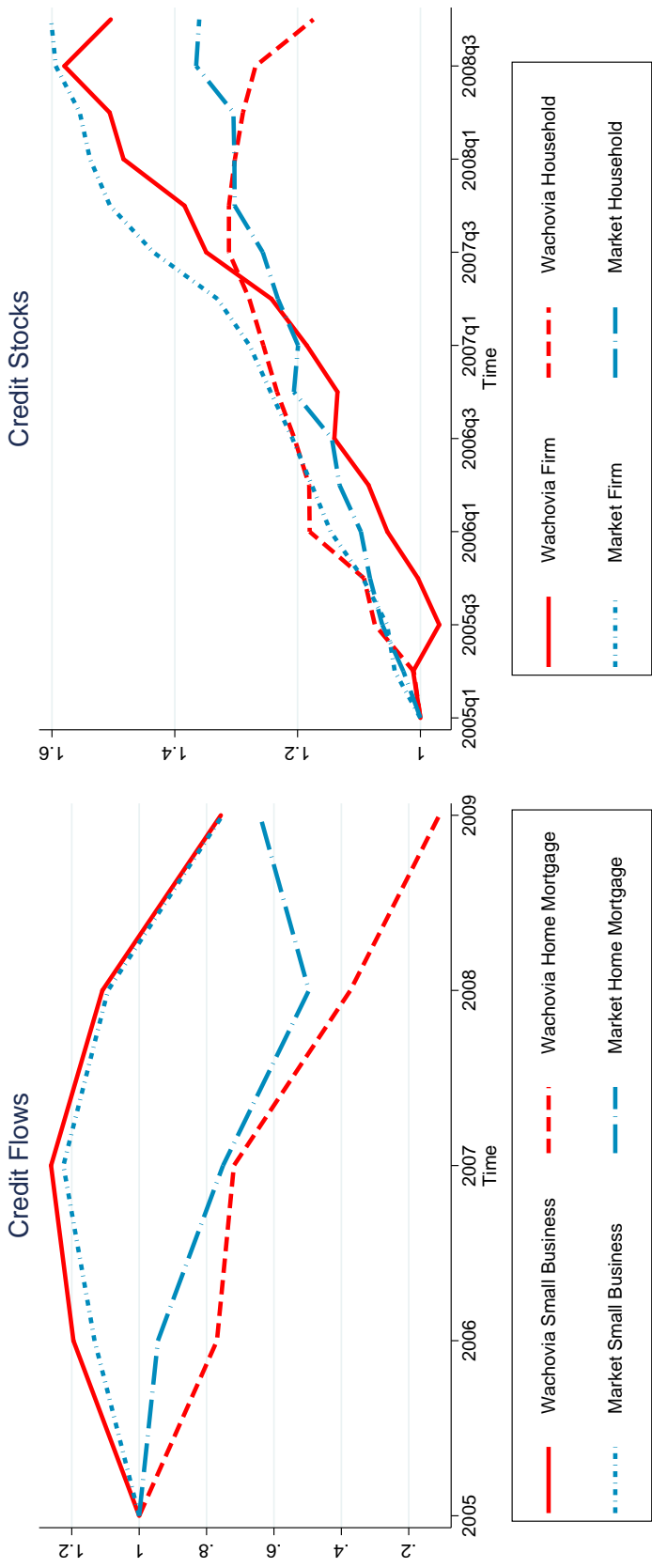
Note: This figure plots coefficients from repeated cross-sectional regressions of household credit growth, non-tradable employment growth, and house price growth on exposure to Wachovia: $\widehat{E}_{it} = \alpha_t + \beta_t \text{Wachovia Exposure}_i + \epsilon_{it}$. Wachovia exposure is measured as Wachovia's average market share of non-refinance mortgages from 2005-2006 rescaled by 100. For all but the 2004-2005 period there is no pre-crisis relationship between household credit growth and Wachovia, but beginning in 2008 the association turns very negative. Both non-tradable employment and house prices have essentially no trend leading up to the crisis and then their association with Wachovia turns negative. Household credit growth is measured as growth in non-refinance mortgages from HMDA, employment data are from the CBP and non-tradables are defined as in Mian and Sufi [2014], house price data are from Zillow. The sample is limited to counties in the South and East with at least 50,000 residents and CCP data. State-clustered 95% confidence intervals are reported.

Figure 8: Effect of Exposure to Wachovia on Household Credit Growth



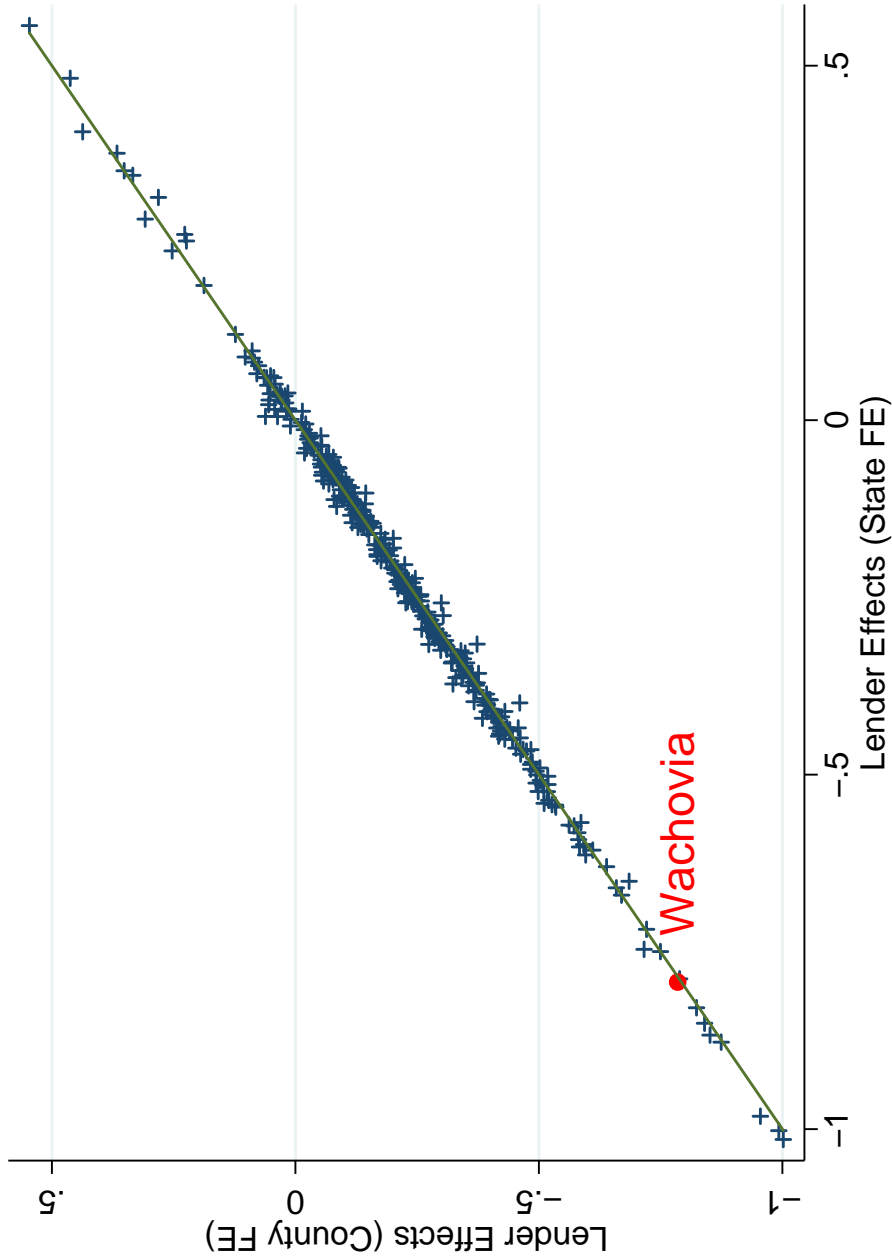
Note: This figure plots Wachovia exposure, measured as Wachovia's average market share of non-refinance mortgages from 2005-2006 against the county-level growth rate of non-refinance mortgages from 2007-2010. Counties with more exposure to Wachovia tended to have significantly lower mortgage growth, suggesting more negative shocks to credit supply in these counties. The bivariate regression line is plotted in red and has a strongly negative slope. The sample is limited to counties in the South and East with at least 50,000 residents and CCP data. All data are from HMDA.

Figure 9: Household and Firm Credit: Wachovia and the Market



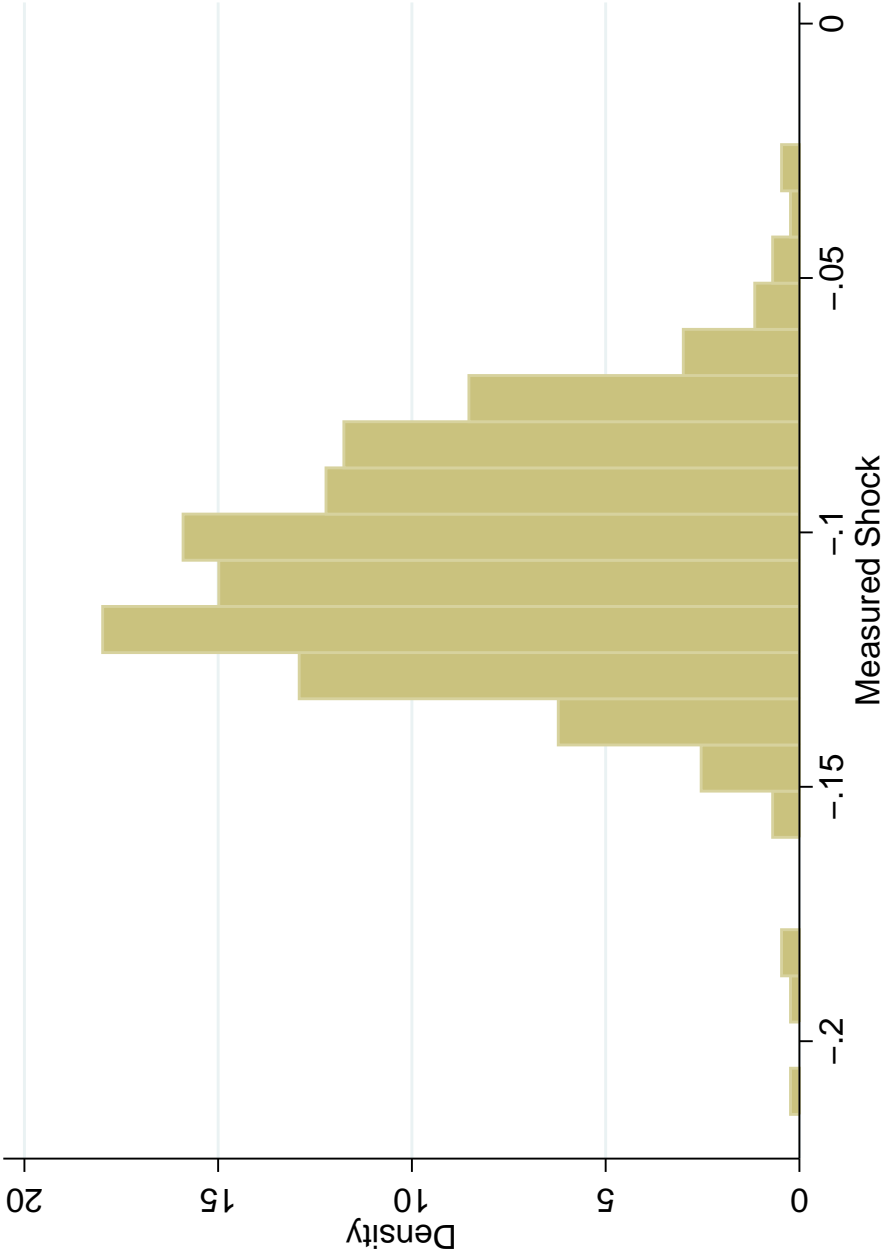
Note: This figure plots credit flows and credit stocks on commercial bank balance sheets for Wachovia and the broader market, all normalized to be one in 2005. The left panel shows that home mortgage originations and purchases from Wachovia declined significantly and differentially over the crisis. However, small business originations and purchases from Wachovia mirrored the aggregate trend almost exactly. The right panel plots the quarterly stock of household and firm credit on bank balance sheets calculated from all bank call reports. Wachovia's stock of firm loans continued to grow until the fourth quarter of 2008 while Wachovia's holdings of household credit began to decline by late 2007. Both figures suggest Wachovia's credit contraction was largely concentrated in household credit. I adjust Wachovia's stock of household credit to smooth the incorporation of GWF's stock of loans. Flow data are calculated from the HMDA and CRA data. Household lending is composed of loans to individuals and loans secured by real estate. Firm lending is composed of all commercial and industrial loans.

Figure 10: Checking the Identification Assumption: Stability of Lender Fixed Effects



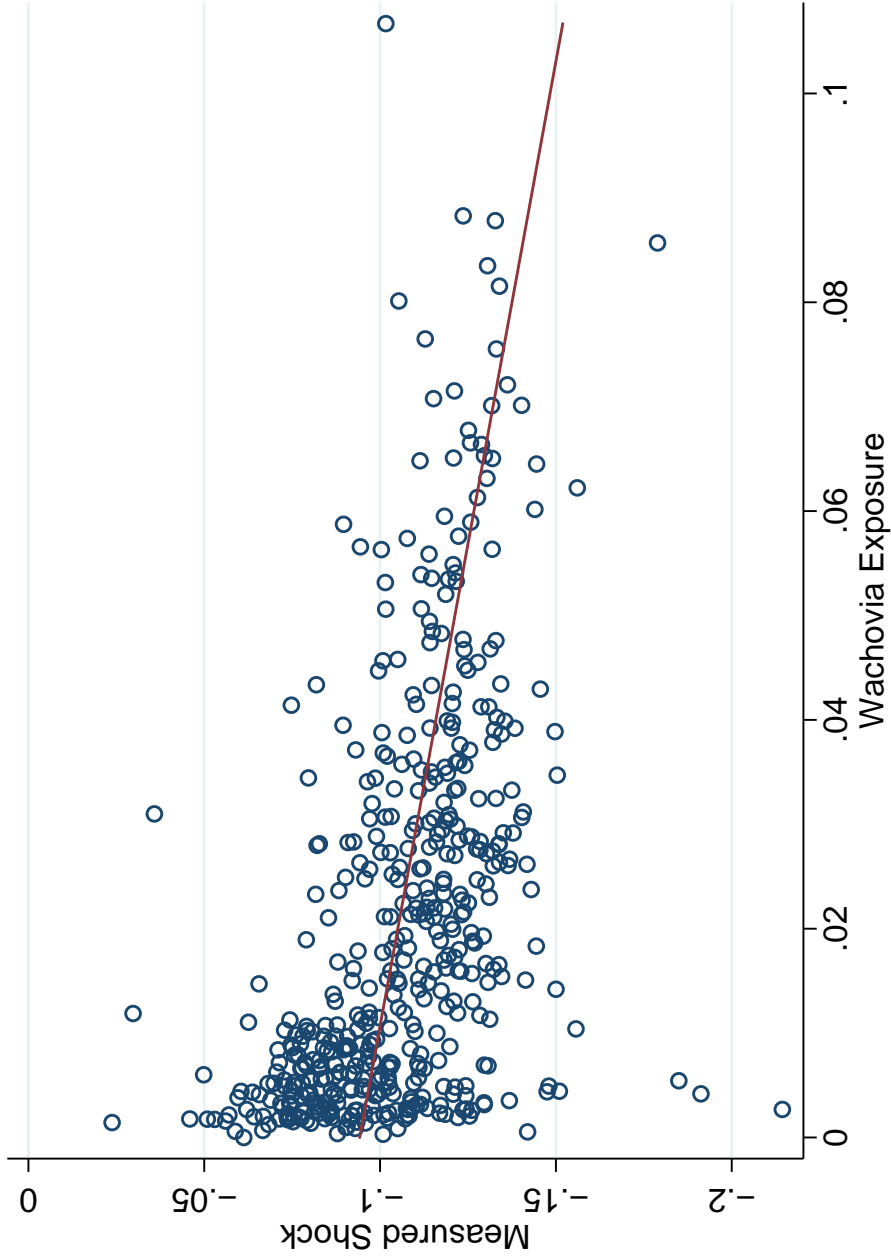
Note: This figure plots the lender fixed effects from estimating regression 11, $\hat{L}_{ij} = \alpha_i D_i + \rho_j A_j + e_{ij}$, on lender-county household credit growth with state fixed effects (horizontal axis) and county fixed effects (vertical axis) and the 45-degree line. If the identification assumption (equation 12) was inappropriate then I would expect differences in the estimates. For example, if the points were predominantly above the 45-degree line it would indicate that lender effects were biased down (more negative) when controlling for state shocks relative to county shocks. That the points lie along the 45-degree line suggests the identification assumption is appropriate. All data are from HMDA.

Figure 11: Distribution of Measured Shocks in the South and East



Note: This figure plots the distribution of the measured shock s_i within the subsample of counties in the East and South with at least 50,000 residents and CCP data. A more negative number indicates a more contractionary supply shock to household credit. The measured shock is defined as $s_i = \frac{1}{2} \sum_j (\omega_{ij,t} + \omega_{ij,t-1}) \rho_j$, where ρ come from the regression $\tilde{L}_{i,j} = \alpha_i D_i + \rho_j \Lambda_j + e_{i,j}$.

Figure 12: Effect of Exposure to Wachovia on the Measured Shock



Note: This figure plots Wachovia exposure, measured as Wachovia's average market share of non-refinance mortgages from 2005-2006, against the county's measured shock to household credit supply. Counties with more exposure to Wachovia tended to have significantly more negative (contractionary) measured shocks, showing that exposure to Wachovia is correlated with the measured shock. The bivariate regression line is also plotted in red and has a negative slope. The sample is limited to counties in the South and East with at least 50,000 residents and CCP observables. The measured shock is defined as $s_i = \frac{1}{2} \sum_j (\omega_{ij,t} + \omega_{ij,t-1}) \rho_j$, where ρ come from the regression $\tilde{L}_{ij} = \alpha_i D_i + \rho_j \Lambda_j + e_{ij}$. See the text for more details.

C Tables

Table 1: Summary Statistics - Total Sample

	mean	sd	count
Total Employment 2007-2010	-0.07	0.06	779
Construction Employment 2007-2010	-0.19	0.15	779
Nontradable Employment 2007-2010	-0.03	0.07	779
Tradable Employment 2007-2010	-0.15	0.16	779
Construction Share 2005	0.12	0.04	779
Nontradable Share 2005	0.19	0.04	779
Tradable Share 2005	0.13	0.08	779
Household Credit 2007-2010	-0.42	0.13	779
Small Business Credit 2007-2010	-0.44	0.14	779
Household Credit 2002-2005	0.74	0.55	779
Mortgage Leverage 2002-2005	0.31	0.17	779
Mortgage Leverage 2006	1.06	0.36	779
House Prices 2002-2005	0.33	0.26	579
House Prices 2007-2010	-0.12	0.13	604

Note: This table gives summary statistics for the entire sample of counties with at least 50,000 residents and CCP observables. All statistics with a year range are growth rates between those years. Construction, tradable, and non-tradable employment are classified according to Mian and Sufi [2014]. See the text for data sources.

Table 2: Summary Statistics - Subsample

	mean	sd	count
Total Employment 2007-2010	-0.06	0.06	478
Construction Employment 2007-2010	-0.17	0.16	478
Nontradable Employment 2007-2010	-0.01	0.07	478
Tradable Employment 2007-2010	-0.15	0.17	478
Construction Share 2005	0.12	0.04	478
Nontradable Share 2005	0.19	0.04	478
Tradable Share 2005	0.13	0.08	478
Household Credit 2007-2010	-0.43	0.14	478
Small Business Credit 2007-2010	-0.48	0.13	478
Household Credit 2002-2005	0.75	0.51	478
Mortgage Leverage 2002-2005	0.29	0.17	478
Mortgage Leverage 2006	0.98	0.31	478
House Prices 2002-2005	0.35	0.25	342
House Prices 2007-2010	-0.11	0.12	360

Note: This table gives summary statistics for the subsample of counties with at least 50,000 residents and CCP observables in the East and South . All statistics with a year range are growth rates between those years. Construction, tradable, and non-tradable employment are classified according to Mian and Sufi [2014]. See the text for data sources.

Table 3: Exclusion Restriction I: Relationship Between Wachovia Exposure and County Observables

	(1) β /p/(CI)	(2) β /p/(CI)	(3) β /p/(CI)	(4) β /p/(CI)	(5) β /p/(CI)	(6) β /p/(CI)	(7) β /p/(CI)	(8) β /p/(CI)	(9) β /p/(CI)
NT Employment 2002-2005	0.013 0.244 (-0.006, 0.032)								
Household Credit 2002-2005		0.001 0.826 (-0.003, 0.004)							
Mortgage Leverage 2006			0.011 0.000 (0.004, 0.017)						
Mortgage Leverage 2002-2005				-0.014 0.022 (-0.026, -0.001)					
Construction Share 2005					0.040 0.134 (-0.007, 0.087)				
HUD Share 2005						-0.051 0.010 (-0.090, -0.013)			
House Prices 2002-2005							-0.000 0.999 (-0.009, 0.009)		
Population 2002-2005								-0.017 0.514 (-0.051, 0.017)	
Log(Income per return 2006)									0.002 0.780 (-0.004, 0.009)
N	478	478	478	478	478	478	478	478	478
R2	0.003	0.001	0.034	0.015	0.007	0.030	0.000	0.002	0.001
F-stat	0.958	0.090	3.450	1.229	1.162	2.264	0.000	0.409	0.241

Note: This table reports OLS estimates of Wachovia exposure regressed on various county observables: Wachovia Exposure_{*i*} = $\alpha + \beta X_i + \epsilon_i$. I measure exposure with Wachovia's market share in 2005-2006 of non-refinance mortgages within the county. Exposure to Wachovia is not broadly associated with important observables related to the house price boom or household leverage. The first column reports 2002-2005 non-tradable employment from the CBP following the employment classifications of Mian and Sufi [2014]. Column two is 2002-2005 household credit growth from HMDA. Column three is the ratio of the mortgage stock per capita from the CCP to per capita adjusted gross income from the IRS and column four is growth in this ratio from 2002 to 2005. Column five is the share of employment in construction in 2005 from CBP. Column six is the share of lending done by HUD-regulated lenders in the HMDA data. Column seven is growth in the house price index from Zillow. Column eight is 2002-2005 population growth from the Census. Column nine is the log of gross income per capita from the IRS. The sample is restricted to all counties in the South and East with CCP controls and at least 50,000 residents. Pair bootstrap-t used to construct symmetric p-values and 95% confidence intervals clustered at the state level.

Table 4: Exclusion Restriction II: Joint Correlation Between Wachovia Exposure and Observables

	(1) β /p/(CI)	(2) β /p/(CI)
NT Employment 2002-2005	0.019 0.092 (-0.002, 0.041)	0.030 0.018 (0.003, 0.057)
Mortgage Leverage 2006	0.016 0.000 (0.009, 0.022)	0.013 0.010 (0.003, 0.023)
Construction Share 2005	0.029 0.490 (-0.027, 0.086)	0.009 0.440 (-0.064, 0.082)
HUD Share 2005	-0.079 0.002 (-0.120, -0.038)	-0.101 0.002 (-0.151, -0.051)
Population 2002-2005	-0.044 0.068 (-0.093, 0.005)	-0.042 0.198 (-0.105, 0.020)
House Prices 2002-2005		-0.003 0.018 (0.003, 0.057)
N	478	342
Clusters	25	23
R2	0.112	0.115
F-stat	3.128	2.541

Note: This table reports OLS estimates of Wachovia exposure jointly regressed on various county observables: $\text{Wachovia Exposure}_i = \alpha + \sum_j \beta_j X_{ji} + \epsilon_i$. I measure exposure with Wachovia's market share in 2005-2006 of non-refinance mortgages within the county. Exposure to Wachovia is positively correlated with mortgage leverage, share of employment in construction, and negatively correlated with population growth leading up to the crisis. The regressions have some joint significance. Non-tradable employment growth, and the share of employment in construction are from the CBP following the employment classifications of Mian and Sufi [2014]. Household credit growth is from HMDA. Mortgage leverage is the ratio of the mortgage stock per capita from the CCP to per return adjusted gross income from the IRS. The share of lending done by HUD-regulated lenders is from the HMDA data. House prices are from Zillow. Population growth is from the Census. The sample is restricted to all counties in the South and East with CCP controls and at least 50,000 residents. Pair bootstrap-t used to construct symmetric p-values and 95% confidence intervals clustered at the state level.

Table 5: Effect of Exposure to Wachovia on Household Credit Growth 2007-2010 (First Stage)

	(1) OLS β / p/(CI)	(2) OLS β / p/(CI)	(3) Quantile β / p/(CI)	(4) OLS β / p/(CI)	(5) OLS β / p/(CI)	(6) OLS β / p/(CI)	(7) WLS β / p/(CI)
Wachovia Exposure	-2.532 0.000	-2.465 0.000	-2.570 0.000	-2.409 0.000	-1.430 0.342	-2.689 0.000	-2.157 0.182
Mortgage Leverage 2006	(-3.247, -1.816)	(-3.107, -1.824)	(-3.577, -1.562)	(-3.268, -1.551)	(-5.044, 2.183)	(-3.539, -1.838)	(-5.596, 1.282)
		-0.064	-0.076	-0.063	-0.021	-0.061	-0.025
		0.478	0.738	0.540	0.840	0.214	0.826
Construction Share 2005	(-0.225, 0.097)	(-0.225, 0.097)	(-0.303, 0.151)	(-0.219, 0.093)	(-0.106, 0.064)	(-0.146, 0.024)	(-0.144, 0.094)
		-0.238	-0.242	-0.165	-0.136	-0.321	-0.086
		0.898	0.720	0.934	0.446	0.326	0.754
HUD Share 2005	(-1.015, 0.538)	(-1.015, 0.538)	(-1.083, 0.599)	(-0.807, 0.477)	(-0.429, 0.157)	(-0.847, 0.206)	(-0.375, 0.204)
		-0.815	-0.800	-0.834	-0.721	-0.830	-0.624
		0.000	0.002	0.000	0.002	0.000	0.006
		(-1.208, -0.423)	(-1.190, -0.410)	(-1.293, -0.376)	(-1.109, -0.333)	(-1.339, -0.321)	(-0.969, -0.280)
FE	-	-	-	Region	State	-	State
N	478	478	478	478	478	478	478
Clusters	25	25	25	25	25	25	25
R2	0.120	0.313	0.313	0.317	0.536	0.281	0.608
Robust F-stat	28.187	48.197		34.335	11.144	16.187	14.632

Note: This table reports point estimates, p-values, and 95% confidence intervals for household credit growth at the county level (measured as non-refinance mortgage growth) regressed on exposure to Wachovia: $\tilde{L}_i = \alpha + \beta \text{Wachovia Exposure}_i + \theta X_i + \epsilon_i$. I measure exposure with Wachovia's market share in 2005-2006 of non-refinance mortgages within the county. Exposure to Wachovia had a large and robust effect on household credit growth across counties. The fairly high R-squared suggests Wachovia is a reasonably strong instrument. The baseline estimate in column one shows that increasing exposure to Wachovia by one percentage point leads to a decrease in household credit of 2.4% over three years. This estimate is robust to controls, quantile regression to check for outliers (column three), region fixed effects (column four), and, to a lesser extent, state fixed effects (column five), although to a lesser extent. The sample is restricted to all counties in the South and East with CCP controls and at least 50,000 residents. Pair bootstrap-t used to construct symmetric p-values and 95% confidence intervals clustered at the state level.

Table 6: Effect of Wachovia Exposure on Retail Expenditures 2007-2010

	(1) OLS β / p/(CI)	(2) OLS β / p/(CI)	(3) WLS β / p/(CI)	(4) WLS β / p/(CI)
Wachovia Exposure	-0.853 0.000	-0.902 0.024	-0.948 0.000	-1.167 0.000
Mortgage Leverage 2006	(-1.268, -0.438) -0.083 0.018	(-1.677, -0.127) -0.062 0.014	(-1.309, -0.587) -0.043 0.000	(-1.523, -0.810) -0.032 0.254
	(-0.144, -0.021)	(-0.110, -0.014)	(-0.066, -0.021)	(-0.083, 0.019)
FE	-	State	-	State
N	471	471	471	471
Clusters	25	25	25	25
R2	0.090	0.229	0.076	0.209
F-stat	15.283	5.608	17.470	17.030

Note: This table reports regression point estimates, p-values, and 95% confidence intervals of retail expenditure growth at the county level regressed on exposure to Wachovia: $\text{Retail Sales}_i = \alpha + \beta \text{Wachovia Exposure}_i + \theta X_i + \epsilon_i$. I measure exposure with Wachovia's market share in 2005-2006 of non-refinance mortgages within the county. Counties exposed to Wachovia experienced larger declines in retail sales growth and this result is robust to state fixed effects and controlling for mortgage leverage. Retail expenditures growth is measured using total retail sales as reported in the Nielsen Retail Scanner data. The sample is restricted to all counties in the South and East with CCP controls and at least 50,000 residents. Pair bootstrap-t used to construct symmetric p-values and 95% confidence intervals clustered at the state level. WLS estimates are weighted using county population in 2006.

Table 7: Effect of Wachovia Exposure on House Sales 2007-2010

	(1) OLS β / p/(CI)	(2) OLS β / p/(CI)	(3) WLS β / p/(CI)	(4) WLS β / p/(CI)
Wachovia Exposure	-2.459 0.002 (-3.901, -1.017)	-1.823 0.006 (-2.984, -0.663)	-2.878 0.000 (-3.772, -1.985)	-2.817 0.016 (-4.523, -1.112)
Mortgage Leverage 2006	-0.069 0.450 (-0.223, 0.085)	-0.044 0.886 (-0.219, 0.132)	0.016 0.728 (-0.063, 0.094)	0.028 0.918 (-0.131, 0.187)
House Prices 2002-2005	0.280 0.006 (0.102, 0.458)	0.322 0.194 (-0.155, 0.798)	0.356 0.004 (0.162, 0.550)	0.321 0.004 (0.162, 0.550)
FE	-	State	-	State
N	308	308	308	308
Clusters	21	21	21	21
R2	0.199	0.443	0.341	0.502
F-stat	11.651	5.823	17.257	7.323

Note: This table reports regression point estimates, p-values, and 95% confidence intervals of house sales growth at the county level regressed on exposure to Wachovia: $\text{House Sales}_i = \alpha + \beta \text{Wachovia Exposure}_i + \theta X_i + \epsilon_i$. I measure exposure with Wachovia's market share in 2005-2006 of non-refinance mortgages within the county. Counties exposed to Wachovia experienced larger declines in house sales growth and this result is robust to state fixed effects and controlling for mortgage leverage and pre-crisis house price growth. House sales are measured using Zillow data. The sample is restricted to all counties in the South and East with CCP controls and at least 50,000 residents. Pair bootstrap-t used to construct symmetric p-values and 95% confidence intervals clustered at the state level. WLS estimates are weighted using county population in 2006.

Table 8: Effect of Wachovia Exposure on House Prices

	(1) OLS β / p/(CI)	(2) OLS β / p/(CI)	(3) OLS β / p/(CI)	(4) WLS β / p/(CI)	(5) WLS β / p/(CI)
Wachovia Exposure	-0.615 0.076 (-1.310, 0.080)	-1.464 0.008 (-2.599, -0.329)	-0.824 0.766 (-4.656, 3.008)	-1.340 0.000 (-2.010, -0.670)	-1.076 0.432 (-3.621, 1.469)
Mortgage Leverage 2006	-0.135 0.102 (-0.301, 0.030)	-0.157 0.252 (-0.383, 0.068)	-0.102 0.046 (-0.203, -0.000)	-0.133 0.004 (-0.219, -0.048)	-0.090 0.108 (-0.205, 0.024)
House Prices 2002-2005	-0.275 0.122 (-0.615, 0.065)	-0.305 0.196 (-0.757, 0.147)	-0.265 0.022 (-0.476, -0.054)	-0.320 0.000 (-0.408, -0.232)	-0.228 0.000 (-0.408, -0.232)
FE	-	-	State	-	State
N	342	342	342	342	342
Clusters	23	23	23	23	23
R2	0.542	0.501	0.710	0.542	0.734
F-stat	12.759	14.984	6.643	22.309	5.041

Note: This table reports regression point estimates, p-values, and 95% confidence intervals of house price growth at the county level regressed on exposure to Wachovia: $\widehat{HP}_i = \alpha + \beta \text{Wachovia Exposure}_i + \theta X_i + \epsilon_i$. I measure exposure with Wachovia's market share in 2005-2006 of non-refinance mortgages within the county. Counties exposed to Wachovia appear to have experienced larger declines in house prices. The results are fairly imprecise, although the point estimates are quite robust to the inclusion of state fixed effects. House prices are measured using Zillow data. The sample is restricted to all counties in the South and East with CCP controls and at least 50,000 residents. Pair bootstrap-t used to construct symmetric p-values and 95% confidence intervals clustered at the state level. WLS estimates are weighted using county population in 2006.

Table 9: Effect of Wachovia Exposure on Non-tradable Employment and Payrolls 2007-2010

	(1)	(2)	(3)	(4)	(5)	(6)
	Employment β / p/(CI)	Employment β / p/(CI)	Payroll β / p/(CI)	Payroll β / p/(CI)	CZ Employment β / p/(CI)	CZ Employment β / p/(CI)
Wachovia Exposure	-0.874 0.000	-0.662 0.180	-0.933 0.006	-0.850 0.006	-0.706 0.002	-0.615 0.028
Mortgage Leverage 2006	(-1.308, -0.439) -0.033 0.004	(-1.608, 0.285) 0.018 0.404	(-1.586, -0.281) -0.048 0.016	(-1.391, -0.308) -0.046 0.328	(-1.123, -0.289) -0.036 0.010	(-1.130, -0.101) 0.011 0.690
	(-0.053, -0.012)	(-0.021, 0.057)	(-0.086, -0.010)	(-0.141, 0.049)	(-0.060, -0.011)	(-0.018, 0.039)
FE	-	State	-	State	-	State
N	478	478	478	478	289	289
Clusters	25	25	25	25	24	24
R2	0.095	0.314	0.049	0.129	0.166	0.481
Robust F-stat	5.311	3.156	7.413	7.996	7.459	4.610

Note: This table reports regression point estimates, p-values, and 95% confidence intervals of non-tradable employment growth at the county level regressed on exposure to Wachovia: $\widehat{E}_i = \alpha + \beta \text{Wachovia Exposure}_i + \theta X_i + \epsilon_i$. I measure exposure with Wachovia's market share in 2005-2006 of non-refinance mortgages within the county. Non-tradable employment follows the definition in Mian and Sufi [2014] and is primarily retail and food services. Counties exposed to Wachovia experience larger declines in non-tradable employment and payrolls. The results are very robust to the inclusion of state fixed effects and controlling for mortgage leverage. The sample is restricted to all counties in the South and East with CCP controls and at least 50,000 residents. Pair bootstrap-t used to construct symmetric p-values and 95% confidence intervals clustered at the state level. All regressions are weighted using county population in 2006.

Table 10: Effect of Wachovia Exposure on Construction Employment 2007-2010

	(1) Construction β / p/(CI)	(2) Construction β / p/(CI)	(3) Only Residential β / p/(CI)	(4) Only Non-residential β / p/(CI)
Wachovia Exposure	-1.793 0.000	-0.648 0.410	-1.065 0.014	-0.156 0.306
Mortgage Leverage 2006	(-2.513, -1.074) -0.177 0.000	(-2.249, 0.952) -0.077 0.022	(-1.869, -0.262) -0.104 0.000	(-2.984, 2.672) -0.051 0.212
	(-0.232, -0.122)	(-0.139, -0.014)	(-0.161, -0.046)	(-0.135, 0.032)
FE	-	State	State	State
N	478	478	478	478
Clusters	25	25	25	25
R2	0.234	0.473	0.528	0.257
Robust F-stat	14.421	6.576	9.268	3.384

Note: This table reports regression point estimates, p-values, and 95% confidence intervals of construction employment growth at the county level regressed on exposure to Wachovia: $\bar{E}_i = \alpha + \beta \text{Wachovia Exposure}_i + \theta X_i + \epsilon_i$. I measure exposure with Wachovia's market share in 2005-2006 of non-refinance mortgages within the county. Total construction employment follows the definition in Mian and Sufi [2014]. Residential construction excludes 4-digit sectors clearly unrelated to residential construction, although results are also robust to only looking at residential construction itself. Counties exposed to Wachovia experience larger declines in total construction, although this result is not robust to state fixed effects. However, when looking only at residential construction exposure to Wachovia has a significant negative effect whereas non-residential construction does not appear to respond at all. The results are very robust to the inclusion of state fixed effects and controlling for mortgage leverage. The sample is restricted to all counties in the South and East with CCP controls and at least 50,000 residents. Pair bootstrap-t used to construct symmetric p-values and 95% confidence intervals clustered at the state level. All regressions are weighted using county population in 2006.

Table 11: Effect of Wachovia Exposure on Employment 2007-2010

	(1)	(2)	(3)	(4)	(5)	(6)
	Tradables	Tradables	No Tradables	No Tradables	Total	Total
	β / p/(CI)	β / p/(CI)	β / p/(CI)	β / p/(CI)	β / p/(CI)	β / p/(CI)
Wachovia Exposure	-0.356	-0.941	-0.788	-0.490	-0.830	-0.501
	0.514	0.460	0.000	0.068	0.000	0.182
Mortgage Leverage 2006	(-1.137, 0.425)	(-3.741, 1.859)	(-1.130, -0.446)	(-1.012, 0.032)	(-1.126, -0.533)	(-1.244, 0.242)
	-0.001	0.001	-0.045	-0.001	-0.038	0.002
	0.092	0.064	0.000	0.090	0.000	0.360
	(-0.077, 0.075)	(-0.078, 0.080)	(-0.065, -0.026)	(-0.041, 0.040)	(-0.057, -0.019)	(-0.033, 0.037)
FE	—	State	—	State	—	State
N	478	478	478	478	478	478
Clusters	25	25	25	25	25	25
R2	0.002	0.127	0.167	0.354	0.163	0.354
Robust F-stat	0.228	0.944	12.217	2.169	11.062	2.312

Note: This table reports regression point estimates, p-values, and 95% confidence intervals of several definitions of employment growth at the county level regressed on exposure to Wachovia: $\bar{E}_i = \alpha + \beta \text{Wachovia Exposure}_i + \theta X_i + \epsilon_i$. I measure exposure with Wachovia's market share in 2005-2006 of non-refinance mortgages within the county. Employment definitions follow those in Mian and Sufi [2014]. While the point estimates in columns one and two indicate that exposure to Wachovia caused losses in tradables, they estimates are extremely imprecise and there is very little variation actually being captured by Wachovia as measured by the r-squared. In contrast, employment excluding tradables and total employment both seem to respond significantly and negatively to exposure to Wachovia. These results are generally robust to including state fixed effects, although the result in total employment is fairly imprecise. The sample is restricted to all counties in the South and East with CCP controls and at least 50,000 residents. Pair bootstrap-t used to construct symmetric p-values and 95% confidence intervals clustered at the state level. All regressions are weighted using county population in 2006.

Table 12: Effect of Wachovia Exposure on Employment Excluding Tradables 2007-2010: Robustness Checks

	(1) β / p/(CI)	(2) β / p/(CI)	(3) β / p/(CI)	(4) β / p/(CI)	(5) β / p/(CI)
Wachovia Exposure	-0.788	-0.742			-0.727
	0.000	0.002			0.000
Mortgage Leverage 2006	(-1.130, -0.446)	(-1.110, -0.374)			(-1.049, -0.405)
	-0.045	-0.038		-0.028	-0.039
	0.000	0.000		0.006	0.000
High Wachovia Exposure (HMDA)	(-0.065, -0.026)	(-0.056, -0.020)		(-0.048, -0.009)	(-0.057, -0.020)
			-0.047	-0.033	
			0.000	0.000	
High Wachovia Exposure (CRA)			(-0.060, -0.033)	(-0.050, -0.017)	
				-0.008	
				0.436	
				(-0.024, 0.007)	
Firm Credit Polynomial	-	Yes	-	-	-
Industry Shares	-	-	-	-	Yes
N	478	478	478	478	478
Clusters	25	25	25	25	25
R2	0.167	0.206	0.212	0.240	0.201
Robust F-stat	12.217	22.969	20.128	13.346	5.637

Note: This table reports regression point estimates, p-values, and 95% confidence intervals of employment excluding tradables growth at the county level regressed on exposure to Wachovia while controlling for firm credit shocks in various ways. Columns one, two, and three measure include exposure to Wachovia linearly: $\widehat{E}_i = \alpha + \beta \text{Wachovia Exposure}_i + \theta X_i + \epsilon_i$. Column two includes a third-order polynomial of the change in small business credit originations from 2007-2010 as measured in the CRA data. Columns three and four include dummies for above median exposure to Wachovia in the respective markets. I measure exposure with Wachovia's market share in 2005-2006 of non-refinance mortgages within the county. Employment excluding tradables follows the definition in Mian and Sufi [2014]. Counties exposed to Wachovia experience larger declines in employment. This relationship is not driven by changes in small business credit or exposure to Wachovia in the small business credit market. The sample is restricted to all counties in the South and East with CCP controls and at least 50,000 residents. Pair bootstrap-t used to construct symmetric p-values and 95% confidence intervals clustered at the state level. All regressions are weighted using county population in 2006.

Table 13: Effect of Supply-driven Changes in Household Credit on Total Employment Growth 2007-2010

	(1) OLS γ /p/(CI)	(2) 2SLS γ /p/(CI)	(3) 2SLS γ /p/(CI)	(4) 2SLS Early γ /p/(CI)	(5) 2SLS CZ γ /p/(CI)
Household Credit 2007-2010	0.135 0.000 (0.089, 0.181)	0.388 0.000 (0.224, 0.552)	0.288 0.096 (-0.079, 0.656)	0.347 0.000 (0.223, 0.471)	0.300 0.000 (0.172, 0.428)
FE	-	-	Yes	-	-
N	478	478	478	478	289
Clusters	25	25	25	25	24
Robust F-stat		28.417	2.162	9.662	26.445
Weak ID P-value		0.001	0.253	0.005	0.001

Note: This table reports OLS and 2SLS point estimates, p-values, and 95% confidence intervals for the elasticity of county-level employment growth with respect to household credit: $\widehat{E}_i = \alpha + \gamma \widehat{L}_i + \beta X_i + \epsilon_i$. I instrument for household credit growth with exposure to Wachovia over 2005-2006, except in column four. Overall we see that growth in household credit has a large positive effect on employment growth. The second column instruments for household credit with exposure to Wachovia and doubles the OLS estimate from column one. Column three includes state fixed effects. Column four uses Wachovia exposure from 2002-2003 as an instrument. Column five aggregates the data to census commuting zones and recovers a very similar estimate of the elasticity as with the county-level data. Regressions limited to the subsample of counties in the South and East with at least 50,000 residents in 2006 with CCP data. Pair bootstrap-t used to construct symmetric p-values and 95% confidence intervals clustered at the state level. All regressions are weighted using population in 2006.

Table 14: Heterogeneous Effects of Supply-driven Changes in Household Credit on Total Employment Growth 2007-2010

	(1)	(2)	(3)	(4)	(5)	(6)
	High Income β / p/(CI)	Low Income β / p/(CI)	High Leverage β / p/(CI)	Low Leverage β / p/(CI)	High HUD β / p/(CI)	Low HUD β / p/(CI)
Household Credit 2007-2010	0.283 0.002 (0.144, 0.422)	0.562 0.040 (0.018, 1.107)	0.538 0.132 (-0.243, 1.318)	0.330 0.000 (0.118, 0.542)	0.455 0.000 (0.217, 0.694)	0.213 0.000 (0.105, 0.321)
N	239	239	239	239	239	239
Clusters	24	18	25	23	22	23
Robust F-stat	25.717	7.536	3.538	19.078	10.309	37.847
Weak ID P-value	0.003	0.013	0.106	0.007	0.008	0.011

Note: This table reports 2SLS point estimates, p-values, and 95% confidence intervals for the effect of household credit growth, measured as non-refinance mortgage growth, on total employment growth at the county-level within various subsamples to check for heterogeneous effects: $\widehat{E}_i = \alpha + \gamma \widehat{L}_i + \theta X_i + \epsilon_i$. I instrument for household credit growth with exposure to Wachovia over 2005-2006. Columns one and two show the elasticity of employment is significantly higher in low-income counties. Columns three and four show the elasticity is larger in high-leverage counties. Columns five and six show the elasticity is larger in counties with more lending by HUD-regulated lenders. This suggests that the ability of households to substitute to other lenders mitigates the effects of the credit supply shock. Regressions limited to the subsample of counties in the South and East with at least 50,000 residents in 2006 with CCP data. Pair bootstrap-t used to construct symmetric p-values and 95% confidence intervals clustered at the state level. All regressions are weighted using county population in 2006.

Table 15: Lender Regulator Proportions in Total HMDA Data and Estimation Sample

Estimation Sample	
OCC	0.30
FRB	0.20
FDIC	0.14
OTS	0.08
NCUA	0.05
HUD	0.22
Total	
OCC	0.14
FRB	0.08
FDIC	0.32
OTS	0.07
NCUA	0.23
HUD	0.16

Note: This table reports the fraction of lenders under each regulator at 2006 within the set of lenders (360) used to estimate the fixed effects and the entire HMDA sample (about 9,000 lenders). The estimation sample is restricted to lenders operating in at least 30 counties. OCC stands for the Office of the Comptroller of the Currency, FRB the Federal Reserve Bank, FDIC the Federal Deposit Insurance Corporation, OTS the Office of Thrift Supervision, NCUA the National Credit Union Association, and HUD the Department of Housing and Urban Development. As expected the estimation sample includes more national banks (OCC), which tend to be larger. Small lenders (FDIC and NCUA) are underrepresented. The data are from HMDA, see the text for more detail.

Table 16: Effect of Exposure to Wachovia on the Measured Shock (First Stage)

	(1)	(2)	(3)	(4)	(5)
	OLS	OLS	Quantile	OLS	OLS-No Wachovia
	β / p/(CI)	β / p/(CI)	β / p/(CI)	β / p/(CI)	β / p/(CI)
Wachovia Exposure	-0.677	-0.656	-0.538	-0.369	0.025
	0.000	0.000	0.012	0.156	0.270
	(-0.818, -0.535)	(-0.810, -0.501)	(-0.901, -0.176)	(-0.921, 0.183)	(-0.520, 0.569)
Mortgage Leverage 2006		-0.006	-0.011		
		0.414	0.768		
		(-0.018, 0.005)	(-0.046, 0.024)		
HUD Share 2005		-0.013	-0.007		
		0.462	0.198		
		(-0.074, 0.049)	(-0.216, 0.201)		
Construction Share 2005		-0.045	-0.030		
		0.386	0.934		
		(-0.119, 0.029)	(-0.222, 0.161)		
FE	-	-	-	State	State
N	478	478	478	478	478
Clusters	25	25	25	25	25
R2	0.230	0.246	0.232	0.429	0.286
Robust F-stat	28.389	9.993		7.022	0.031

Note: This table reports OLS and quantile point estimates, p-values, and 95% confidence intervals of the measured shock to household credit regressed on exposure to Wachovia over 2005-2006: $s_i = \alpha + \beta \text{Wachovia Exposure}_i + \epsilon_i$ where $s_i = \frac{1}{2} \sum_j (\omega_{ij,t} + \omega_{ij,t-1}) \rho_j$, and ρ_j come from the regression $\tilde{L}_{ij} = \alpha_i D_i + \rho_j \Lambda_j + \epsilon_{ij}$. The first four columns show that exposure to Wachovia had a significant and robust effect on the measured shock. The effect is robust to standard controls and is not driven by outliers. The last column excludes Wachovia from the measured shock and show that exposure to Wachovia then has no significant relationship with the measured shock. The sample is limited to the subsample of counties in the South and East with at least 50,000 residents in 2006 with CCP data. Pair bootstrap-t used to construct symmetric p-values and 95% confidence intervals clustered at the state level. All regressions are weighted using county population in 2006.

Table 17: Effect of Measured Shock on Employment 2007-2010

	(1) WLS γ /p/(CI)	(2) 2SLS γ /p/(CI)	(3) 2SLS γ /p/(CI)	(4) 2SLS-No Tradables γ /p/(CI)
Measured Shock	0.606 0.000 (0.305, 0.906)	1.408 0.000 (0.816, 2.000)	1.103 0.000 (0.598, 1.609)	1.131 0.000 (0.606, 1.656)
Leverage Fixed Effects	–	–	Yes	Yes
Industry Shares	–	–	Yes	Yes
N	478	478	478	478
Clusters	25	25	25	25
Robust F-stat		28.389	34.337	34.337

Note: This table reports OLS and 2SLS point estimates, p-values, and 95% confidence intervals for the elasticity of county-level employment with respect to the measured shock: $\widehat{E}_i = \alpha + \gamma s_i + \beta X_i + \epsilon_i$. I instrument for the measured shock with exposure to Wachovia over 2005-2006. The measured shock is defined as $s_i = \frac{1}{2} \sum_j (\omega_{ij,t} + \omega_{ij,t-1}) \rho_j$, where ρ come from the regression $\widehat{L}_{ij} = \alpha_i D_i + \rho_j \Lambda_j + e_{ij}$. The instrumented estimates are about twice as large as the OLS estimate, suggesting significant attenuation bias when only using OLS. Columns three and four control for household leverage fixed effects and the shares of employment in finance, construction, real estate, and tradables. Column four limits the outcome variable to employment growth excluding tradables. Sector definitions come from Mian and Sufi [2014]. The sample is limited to the subsample of counties in the South and East with at least 50,000 residents in 2006 with CCP data. Pair bootstrap-t used to construct symmetric p-values and 95% confidence intervals clustered at the state level and adjusted for the generated regressor. All regressions are weighted using county population in 2006.

Table 18: Aggregate Direct Contribution of Shocks to Household Credit 2007-2010 (% Change)

	Total Employment	Employment Excluding Tradables
No Adjustment - South and East	-11.8	-10.3
75th Percentile - South and East	-3.6	-3.1
75th Percentile - National	-4.5	-3.9
66th Percentile - South and East	-3.1	-2.7
66th Percentile - National	-3.8	-3.3

Note: This table reports calculations of the lower bound to the aggregate employment effect of shocks to household credit. The first column reports calculations using the effects of the measured shock on total employment and the second column uses only the effects on employment excluding tradables. The first row does not correct for measurement error in the shock and suggests the household credit channel was responsible for over 100% of the observed change in employment. The second row sets k^* equal to the 75th percentile of the measured shock and finds the household credit channel caused 57% of the employment decline within sample and 60% nationally. The last rows set k^* equal to the 66th percentile of the measured shock and give similarly large estimates. These calculations suggest shocks to household credit had significant effects on employment over this period. Explicitly, the numbers report $\text{direct contribution} = \frac{\beta^{ES}}{\pi} \sum_i \omega_i (s_i - \sum_{j:s_j \geq k^*} \tilde{\omega}_j \widehat{s}_j)$ for different choices of the cutoff k^* . See the text for details.