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 at least a 3.6% decline in employment from 2007 to 2010.

*Email: jnm@econ.berkeley.edu. Address: 530 Evans Hall #3880, Berkeley, CA 94720-3880. Phone: 510-684-3264. For the most recent version of this paper please check https://sites.google.com/site/johnnelsonmondragon/. I am deeply indebted to my advisor Yuriy Gorodnichenko, as well as Olivier Coibion, Amir Kermani, Christina Romer, and David Romer for their patience, guidance, and support. I also thank Gabriel Chodorow-Reich, Pierre-Olivier Gourinchas, Erik Johnson, Joshua Hausman, Kari Heerman, Martha Olney, Joshua Miller, Demian Pouzo, David Sraer, Johannes Wieland, and especially Mu-Jeung Yang, as well as seminar participants at Berkeley, the University of Washington, and the Federal Reserve Bank of Richmond. The National Science Foundation and the Federal Reserve Bank of Richmond both provided valuable support while this research was conducted. All errors are my own.
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.\footnote{Mian 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 Lademan [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.} There is also evidence that the financial crisis reduced the supply of credit to firms, which then cut investment and employment.\footnote{See 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 and Mas [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.} But much of the theoretical modelling 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.\footnote{Eggertsson 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 Philippin [2011] highlight the role of liquidity shocks to households. The previous models rely on nominal rigidities in order to generate declines in real output. Hao and Rios-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.} 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.\footnote{Recent 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.} 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.
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.  

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.  

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.

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As 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.

One 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 and Mas [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%. 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 significant declines in nondurable expenditures, the stock of auto debt, 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.

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.
finance, and construction sectors. In further support of the exclusion restriction, Wachovia’s market 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 and Mas [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

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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], Giammetti 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.
whether or not a lender is expanding or contracting credit to borrowers.

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. 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. 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 often reported in other applications can be biased downward. I construct a lower bound to the aggregate direct contribution to correct for measurement error. As mentioned above, supply shocks to household credit caused a decline in employment of at least 3.6% within

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For 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].

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I 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. This allows me to get rid of measurement error in a way that will always understate the actual effect.
the estimation sample of counties in the South and East (60% of the observed decline), and 4.5% nationally (64% of the observed decline). For comparison, Greenstone and Mas [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 $r_i = R(S_i, D_i, ε_i)$,
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

\[
E_i = E_i(R(S_i, D_i, v^r_i)), \quad g_i((R(S_j, D_j, v^r_j))_{j \neq i}), \quad \epsilon_i).
\]

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

\[
\hat{E}_i = \beta^{ES} \hat{S}_i + \sum_{j \neq i} \beta^{ES}_{ij} \hat{S}_j + v_i.
\]

I call the elasticity \( \beta^{ES} \) 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

\[
\bar{E} = \sum_i \omega_i \hat{E}_i = \sum_i \omega_i (\beta^{ES} \bar{S}_i + \sum_{j \neq i} \beta^{ES}_{ij} \bar{S}_j + v_i).
\]

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

\[
\text{aggregate general equilibrium contribution} = \sum_i \omega_i (\beta^{ES} \bar{S}_i + \sum_{j \neq i} \beta^{ES}_{ij} \bar{S}_j).
\]

This quantity answers the question of how much shifts in household credit supply affected aggregate employment. To calculate this quantity requires knowledge of \( \beta^{ES} \), the spillover effects \( \beta^{ES}_{ij} \), 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.\(^\text{12}\)

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

\(^\text{12}\)See Stumpner [2013] for recent work estimating the role of trade linkages across areas, one potential spillover effect, in the Great Recession.
to household credit

\[
\text{aggregate direct contribution} \equiv \beta^{ES} \sum_i \omega_i \overline{S}_i.
\]

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 \(\beta^{ES}\) 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 \(\beta^{ES}\) would be to use data on \(\overline{E}_i\) and \(\overline{S}_i\) and then estimate \(\beta^{ES}\), but the true shocks are not observed. Here I show that I can still recover useful information about \(\beta^{ES}\) so long as I observe another variable that is related to \(\overline{S}_i\). Consider the reduced form solution for the quantity of credit, which is a function of supply shocks, demand shocks, and other factors

\[
\overline{L}_i = \beta^{LS} \overline{S}_i + \beta^{LD} \overline{D}_i + v_i^L.
\]

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

\[
\overline{E}_i = \gamma \overline{L}_i + e_i
\]
as a way to relate supply-side shocks to employment. Direct estimates of $\gamma$, 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 $\tilde{v}_i = \sum_{j \neq i} \beta_{ij}^{ES} \tilde{S}_j + v_i$. Then a valid instrument is a variable $Z_i$ that is correlated with changes in supply-side shocks to credit $\text{Cov}(Z_i, \tilde{S}_i) \neq 0$, and that satisfies the exclusion restrictions $\text{Cov}(Z_i, \tilde{D}_i) = \text{Cov}(Z_i, \tilde{v}_i) = \text{Cov}(Z_i, v^L_i) = 0$. Under these assumptions, it is simple to see that

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

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 $\tilde{L}^*$ is the decline in observed credit due to contractions in credit supply then $\frac{\beta^{ES}}{\beta^{LS}} \times \tilde{L}^* = \tilde{E}^*$, 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 $\tilde{\gamma}$ converges in probability to $\frac{\beta^{ES}}{\beta^{LS}}$ 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 $\frac{\beta^{ES}}{\beta^{LS}}$ 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 $\pi$ and noise. Then I have the following system of equations

$$s_i = \pi \tilde{S}_i + v_{si},$$

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

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

But it is straightforward to see that OLS gives the following, where $\sigma^2_s$ is the variance of the noise and $\sigma^2_S$ is the variance of the true shock

$$\frac{\text{Cov}(\widehat{E}_i, s_i)}{\text{Var}(s_i)} = \beta^{ES} + \frac{\text{Cov}(\widehat{S}_i, \tilde{v}_i)}{\sigma^2_S}.$$ 

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 $\text{Cov}(\widehat{S}_i, Z_i) \neq 0$, but that now satisfies the appropriate exclusion restrictions $\text{Cov}(Z_i, \tilde{v}_i) = \text{Cov}(Z_i, v_{si}) = 0$. With these assumptions it is straightforward to see that

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

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

$$\frac{\beta^{ES}}{\pi} \sum_i \omega_i (\frac{\pi \widehat{S}_i + v_{si}}{\hat{s}_{si}}) = \beta^{ES} \sum_i \omega_i \widehat{S}_i + \frac{\beta^{ES}}{\pi} \sum_i \omega_i v_{si} = \beta^{ES} \sum_i \omega_i \widehat{S}_i = \text{aggregate direct contribution}.$$ 

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 $\beta^{ES}/\beta^{LS}$, 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 $\beta^{ES}/\pi$. 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

\(^{13}\)While 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. 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. 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. 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 and Mas [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. To separate firms into tradable, non-tradable, and construction industries I follow the classification of 4-digit NAICS codes in Mian and Sufi [2014]. 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 credit, mortgages are by far the largest component of most households’ liabilities. 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. The public data are available at an annual frequency from 1991 to 2013.

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I use the Zillow Home Value Index for single-family residences to measure house prices as well as credit, mortgages are by far the largest component of most households’ liabilities. 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. The public data are available at an annual frequency from 1991 to 2013.

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15 In 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.

16 The 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.


18 https://www.census.gov/econ/cbp/.

19 Mian 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.\textsuperscript{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.\textsuperscript{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.\textsuperscript{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.\textsuperscript{23} While home mortgage credit appears to recover in 2009, this is driven entirely by refinancing.\textsuperscript{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

\textsuperscript{20}See \url{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 \url{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.

\textsuperscript{21}For more details see \url{http://research.chicagobooth.edu/nielsen/}.

\textsuperscript{22}See Lee and Van der Klaauw [2010] for more details.

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

\textsuperscript{24}Refinancing 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.\footnote{Note that this is not the average household leverage ratio, but the ratio of average debt to average income.} 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?
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, Mollenkamp 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.26

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26 Wachovia’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.
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

\[
\text{Prob} (\text{Originated})_{it} = \alpha_{ct} + \beta_{t}\text{Wachovia}_{it} + \gamma_{t}X_{it} + \epsilon_{it}. \tag{1}
\]

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 \(\beta_{t}\) 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 \(\beta_{t}\)'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

\(^{27}\)The 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 $\gamma = \beta^{ES}/\beta^{LS}$

\[
\begin{align*}
\tilde{L}_i &= \sigma Z_i + e_{1i}, \\
\tilde{E}_i &= \gamma \tilde{L}_i + e_{2i}.
\end{align*}
\]

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 subtlety 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. 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.

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28 Unless 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].

21
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:

\[ \hat{E}_{it} = \alpha_t + \beta_t \text{Wachovia Exposure}_i + \epsilon_{it}. \]

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 not a concern.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

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29 As 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 quite 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. 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, although with state fixed effects we see the expected sign but with a smaller point estimate and less precision. Since within-state spillovers are likely to be more important, this is not surprising. 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 Demand 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. I also use this section to exclude other channels that could be confounding my results.

The decline in household credit from Wachovia could have affected household demand in several ways. First, as households are denied mortgages they are less likely to purchase a home. Second, any consumption (often durables and home services) complementary to a home purchase will also be foregone, although this is potentially countered by any substitution away from housing. Third, declines in home equity lines of credit (HELOC) or cash-out refinancing loans will directly reduce household liquidity (see Hurst and Stafford [2004]). Table 6 shows that exposure to Wachovia affected measures of both nondurable and durable demand. Nondurable consumption, measured with the Nielsen Retail Scanner data, declined about .9% in response to a one percentage point increase in Wachovia exposure. The stock of auto debt (column two), a rough proxy for auto
purchases, declines by a slightly smaller rate (.7%). Columns three and four report house price declines from 2007-2010 and from 2007-2012. Exposure to Wachovia lowers prices from 2007-2010 by about .6%, but this effect is relatively imprecise. 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]). Column five shows that a one percentage point increase in Wachovia exposure caused a 2.5% decline in sales, consistent with the historical positive correlation between house prices and transaction volume (Stein [1995], Leamer [2007]).

Table 7 considers employment for various sector definitions and suggests that declines in housing and non-housing demand were both important contributors to employment losses. Construction employment (column one) responded strongly (1.2%), suggesting a direct effect from housing demand. Tradable employment (column two) has the same sign, but is statistically indistinguishable from zero at standard levels. Employment growth excluding construction and tradables (column three) had a negative, but smaller (.5%) response to Wachovia. Some of this decline could be the result of spillovers from the decline in construction employment in addition to direct effects on household non-tradable demand. Column four controls for these spillovers by including a third-order polynomial of construction employment growth from 2007-2010 as a control. This is likely to give a conservative estimate or “over control” because declines in household credit likely caused declines in construction indirectly as economic activity fell. Exposure to Wachovia still causes a decline of about .4%, which is about 80% of the original effect. Construction employment is about 11% of employment on average and employment excluding tradables and construction is about 80% of total employment on average. Using the estimates from column one and four with these shares and ignoring spillovers indicates that the decline in demand unrelated to construction had an effect on total employment about two to three times as large as the effect on construction. Collecting these effects across all sectors, column five shows that increasing the exposure to Wachovia by one percentage point resulted in about a decline of -0.7% in total employment.

One potentially important channel by which this shock to household credit could have affected employment is through a decline in house prices. Mian and Sufi [2014] emphasize the effect that declining house prices had on household demand through a decline in the value of collateral and wealth effects. Alternatively, Adelino, Schoar and Severino [2013] and Schmalz, Sraer and Thesmar
both emphasize the role of housing as collateral for firm financing of employment and firm creation. In Table 6, I show that house prices had not fallen very much by 2010, so that observed declines in collateral value are unlikely to be driving employment here. However, to test the strength of this channel, column six of Table 7 includes a third-order polynomial of house price growth from 2007-2010. The coefficient declines (from -.7 to -.49), but roughly half of this difference is due to the difference in sample (house prices are only available for 360 counties). This result and the timing of the decline in house prices suggest that the declining value of collateral was not the primary mechanism by which the Wachovia shock affected employment.

Table 8 shows exposure to Wachovia had a significant and robust effect on total employment growth over this period. Column one includes the share of lending by GWF, the lender purchased by Wachovia, which has essentially no effect on the Wachovia coefficient or on employment growth. Column two controls for household leverage linearly includes fixed effects by each quartile of household leverage, but there is still a large effect on total employment.

To check that exposure to Wachovia is operating through household credit, I control for industry-specific labor demand shocks. Column three 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 well within the range of previous estimates.

A final 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 capturing 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.

As a robustness check, in column four of Table 8 I include a third-order polynomial of small business credit growth 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 operating through shocks to small business credit. However, this should not be taken as evidence that small businesses credit was irrelevant since Greenstone and Mas [2012] do find effects on employment, although the effects are quantitatively small.

**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 $\frac{\beta^{ES}}{\beta^{LS}}$, the ratio of employment and credit quantities elasticities with respect to the shock to credit (see Section 2).

Table 9 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 9 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. Additionally, many types of credit such as bank cards and home equity line of credit loans are not recorded in the HMDA data, but contractions in these credit types are likely correlated with the Wachovia instrument (i.e. Wachovia likely contracted its other lending along with its mortgage lending). This reinforces my interpretation of these results as the elasticity of employment with respect to household credit, and not just home mortgage credit. The only potential concern is that Wachovia’s behavior in these additional credit markets is not representative of the behavior more broadly, but without additional data I cannot test this concern. However, given Wachovia’s average behavior in the HMDA data and its place as one of the top lenders in the country, its behavior is both interesting on its own and unusual to be extremely atypical.

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 Di Maggio and Kermani [2014], who estimate the effects of expansions in household credit on non-tradable credit.

\[ \text{plim } \hat{\gamma}^{\text{OLS}} = \frac{\text{Cov}(\tilde{E}_i, \tilde{L}_i)}{\text{Var}(\tilde{L}_i)} = \frac{\beta^{ES} \beta^{LS} \sigma^2 + \beta^{KD} \beta^{LD} \sigma^2 + \beta^{EE} \beta^{LD} \text{Cov}(\tilde{v}_i, \tilde{D}_i)}{(\beta^{LS})^2 \sigma^2 + (\beta^{LD})^2 \sigma^2 + \beta^{LL} \sigma^2} \]

Standard omitted variable bias logic (\( \text{Cov}(\tilde{v}_i, \tilde{D}_i) > 0 \)) suggests the estimate could be inflated. However, it is entirely possible that the terms in the denominator are so large as to render \( \hat{\gamma}^{\text{OLS}} < \beta^{ES} / \beta^{LS} \). For example, this could occur if demand shocks in the mortgage market are relatively unimportant for total employment (\( \beta^{ED} \) small), but demand shocks are very volatile (\( \sigma^2_D \) large).
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. My baseline estimate includes all employment (notably construction) and is unweighted, but when I restrict the outcome to non-tradable employment and weight the regression by population in 2006 I recover an estimate of 0.22. That is, the two elasticities are effectively identical, which suggests the employment effects of credit contractions were similar in both the expansion and contraction in credit supply. I delay discussing how my estimates relate to those found in structural models until the end of the next section, when I discuss the aggregate implications and the elasticities together.

**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 10 show that the elasticity in low-income counties is essentially identical to the elasticity in high-income counties. Columns three and four show that high-leverage counties have a larger response than counties with less leverage, although the difference is not statistically significant. Baker [2013] suggests credit constraints or liquidity are the relevant factors, and not leverage per se. I try to proxy for this with the ability of households to substitute into non-distressed lenders if their primary lender becomes distressed. One indicator for the health of household credit markets is 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 almost 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

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

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.\textsuperscript{31}

**Credit Supply** Lenders are monopolists who solve separable, static problems in each area.\textsuperscript{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 cost-shifters $c_{ij}$ as inputs. This gives the standard monopolist’s problem

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

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

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

\textsuperscript{31}Notice 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.

\textsuperscript{32}Assuming 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.
As is standard, the cost of borrowing is a function of marginal cost and the price elasticity of demand.\textsuperscript{33}

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

\[
\hat{r}_{ij} = \epsilon_{ij}^{rc} \hat{C}_{ij} + \epsilon_{ij}^{L} \hat{L}_{ij},
\]

and from 2 we have

\[
\hat{L}_{ij} = \epsilon_{ij}^{Lr} \hat{r}_{ij} + \epsilon_{ij}^{Ld} \hat{d}_{ij},
\]

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

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

\[
\hat{r}_{ij} = \frac{\epsilon_{ij}^{rc}}{1 - \epsilon_{ij}^{rL} \epsilon_{ij}^{Lr}} \hat{C}_{ij} + \frac{\epsilon_{ij}^{Ld} \epsilon_{ij}^{Lr}}{1 - \epsilon_{ij}^{rL} \epsilon_{ij}^{Lr}} \hat{d}_{ij}
\equiv \beta_{ij}^{r} \hat{C}_{ij} + \zeta_{ij}^{r} \hat{d}_{ij},
\]

and

\[
\hat{L}_{ij} = \frac{\epsilon_{ij}^{Lr} \epsilon_{ij}^{rc}}{1 - \epsilon_{ij}^{rL} \epsilon_{ij}^{Lr}} \hat{C}_{ij} + \frac{\epsilon_{ij}^{Ld}}{1 - \epsilon_{ij}^{rL} \epsilon_{ij}^{Lr}} \hat{d}_{ij}
\equiv \beta_{ij}^{L} \hat{C}_{ij} + \zeta_{ij}^{L} \hat{d}_{ij}.
\]

These are familiar expressions, but it is useful to review several coefficients. The coefficient \(\beta_{ij}^{r}\) 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 \(\beta_{ij}^{L}\), 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

\textsuperscript{33}I 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.
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 $\hat{S}_i$ 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 imply some function $R_i$ that aggregates lender-area prices into an
area-specific price $r_i$

$$r_i = R_i\left(\{r_{ij}(c_{ij}, d_{ij})\}\right).$$ (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 as

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

Using 7 to substitute for each lender’s price, we can define the true supply-side shock to credit for

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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.
the area as the deviation in the area’s price due to the lender cost shocks

\[
\hat{S}_i \equiv \frac{1}{2} \sum_j (\omega_{ij,t} + \omega_{ij,t-1}) \beta_{ij}^r c_{ij}
\]  

(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: \( \{\beta_{ij}^r c_{ij}\} \). 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 and Mas [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 \( \Lambda_j \) (for a lender \( j \))

\[
\tilde{L}_{ij} = \alpha_i D_i + \rho_j \Lambda_j + e_{ij}.
\]

(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)**

\[
E_i(\zeta_{ij}^L \tilde{d}_{ij}) = 0, \forall j.
\]

(12)

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 $\rho$. Using equations 8, 12 and noticing that lender costs can be decomposed as $\tilde{c}_{ij} = C_j + e_{ij}$ we see that

$$
E_i(\tilde{L}_{ij}) = E_i(\beta_{ij}^L\tilde{c}_{ij} + \zeta_{ij}^L\tilde{d}_{ij})
= E_i(\beta_{ij}^L\tilde{c}_{ij})
= E_i(\beta_{ij}^L(C_j + e_{ij}^c))
= C_j E_i(\beta_{ij}^L) + E_i(\beta_{ij}^L e_{ij}^c).
$$

(13)

Under standard assumptions, OLS estimates of the lender fixed effect $\rho$ 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, $\rho$ does not contain any demand shocks $\tilde{d}$. Moreover, by construction we have

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

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 $\beta_{ij}^L\tilde{c}_{ij}$ with the estimated lender fixed effects to construct a measure of the
true shock

\[ s_i \begin{array}{c} \sim \omega \\ \text{measured shock} \end{array} = \frac{1}{2} \sum_j \left( \omega_{ij,t} + \omega_{ij,t-1} \right) \rho_j. \] (14)

What is the relationship between the true shock \( s_i \) and my measure \( s_i \)? Recall the measurement relationship from Section 2

\[ s_i = \pi \hat{S}_i + v_{si}. \]

I would like to know first, that \( \pi \) will not be zero, and second the mean of the residual. If \( \pi \) 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 \( s_i \) can be decomposed into \( \pi \hat{S}_i + v_{si} \), where \( \pi = E_j \left( E_i \left( \epsilon_{ij}^{Lr} \right) \right) \neq 0 \) so long as the price elasticity of demand \( \epsilon_{ij}^{Lr} \) 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.

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

\[^{35}\text{The significance of selection is effectively a question about the value of the covariances} \]

\[ \text{Cov}_i \left( \omega_{ij,t} + \omega_{ij,t-1}, \beta_{ij}^r e_{ij} \right). \]

This covariance will plausibly be overstated by

\[ \text{Cov}_i \left( \omega_{ij,t} + \omega_{ij,t-1}, \tilde{L}_{ij} \right) \]

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.
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.\footnote{Some 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.} The estimation sample also represents about 51% of all assets reported on call reports in 2006. Table 11 reports the regulator statistics for the entire 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.\textsuperscript{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.\textsuperscript{38} To simplify, I assume the true expenditure shares are given by the shares of household credit lending in the HMDA data: $\omega_{ijt}$ 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. 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

\textsuperscript{37}To speed estimation of the fixed effects I use the code \texttt{reg2hdfe} provided by Guimaraes and Portugal [2010], but results are identical using OLS.

\textsuperscript{38}Without 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.
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 12 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. Columns five and six exclude 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 \( \beta^{ES}/\pi \), the elasticity of interest normalized by the measurement coefficient. I report the results in Table 13.\(^{39}\) First, the OLS estimate suggests the measured shock has an effect on employment, but it is not significantly different from zero. An increase of one percentage point in the measured shock implies a decline in employment of about .6%. However, the instrumented estimates show that the OLS 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 OLS estimate. Recall that \( \pi \), 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 exactly the same estimate.

\(^{39}\) I correct for the generated regressor with the clustered pairs bootstrap as before.
Table 14 provides 2SLS estimates for the effect of credit supply shocks on additional outcomes over 2007-2010 unless noted. We see that nondurables and auto debt both respond by more than employment, with household credit flows the most responsive. House prices respond with a slight lag while housing sales are very sensitive to my measure. Population flows (column seven) barely respond, which is consistent with the evidence from Yagan [2013], which shows that mobility provides very little insurance to income and employment shocks.

It is informative to compare these estimates with those derived by structural models. Midrigan and Philippon [2011] build a cross-sectional model of household liquidity and credit. They use a 20% shock to the liquidity constraint to generate a 46% change in leverage (i.e. an elasticity of roughly 2.3). Given that their model implies an elasticity of total employment to leverage of 0.137, this implies an elasticity of employment to the shock of $0.137 \times 2.3 = 0.3$, much smaller than the lower bound of one implied by my estimates. This suggests their model severely understates the responsiveness of employment to household credit shocks. A potential source for this difference is that the elasticity of employment to leverage they use is not necessarily the causal relationship, although the direction of bias is not obvious. They also compute that consumption in their model is about 1.5 times as responsive as employment. Strikingly, the ratio of the effect of my measured shock on non-durable consumption from Nielsen (1.72) to the effect on employment (1.14) is also about 1.51. Thus, while my estimates do not agree with the absolute value of their elasticities, the relative responsiveness is matched almost exactly.

Comparing my estimates to elasticities in aggregate models necessarily come with important caveats. Guerrieri and Lorenzoni [2011] build a heterogeneous agent model with incomplete markets and credit constraints. The flexible price version featuring a cost to intermediation and durable consumption implies an elasticity of output with respect to a temporary change in intermediation cost (at the peak) of roughly 0.5. But with fixed prices this elasticity jumps to about 1.4. Adjusting for the fact that output fell by about 70% as much as employment during the Great Recession this suggests an employment elasticity of almost two, well above the minimum of one implied by my estimates. Interestingly, their model features very little response of nondurable consumption to the credit shock, which is inconsistent with my estimates that nondurable expenditures were

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40 While Midrigan and Philippon [2011] distinguish between shocks to liquidity constraints and shocks to credit constraints, I am unable to make this distinction with my data.
strongly affected by household credit shocks. This suggests that either spillovers not recovered by my estimates completely undo any aggregate effects on nondurable consumption or that the dependence of nondurable expenditures on household credit is not captured by the model.

5.3 Aggregate Direct Contribution

With estimates of $\beta^{ES}/\pi$, 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)

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

(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 $\beta^{ES}/\pi$. 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 15 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 13, to arrive at the effect on total employment. The second column uses the coefficient for employment excluding tradables from column seven of Table 14. 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 and Mas [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 \( W_n = \sum_{i=1}^{n} w_i \to \infty \),

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

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

\[
\frac{1}{n} \sum_{i=1}^{n} X_i \to X_0 \Rightarrow \frac{1}{W_n} \sum_{i=1}^{n} w_i X_i \to X_0.
\]

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

\[
\text{sgn} \left( \sum_{i} \omega_i \tilde{S}_i \right) = \text{sgn} \left( \sum_{j: s_j \geq k^*} \tilde{\omega}_j \tilde{S}_j \right). \tag{17}
\]

Then the object

\[
\text{aggregate direct contribution} = \frac{\beta E S}{\pi} \sum_{i} \omega_i (s_i - \sum_{j: s_j \geq k^*} \tilde{\omega}_j s_j) \tag{18}
\]

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

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

Then I can construct the following object

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

$$ = \beta^{ES} \sum_{i} \omega_i \hat{S}_i - \beta^{ES} \sum_{j: s_j \geq k^*} \tilde{\omega}_j \hat{S}_j + \beta^{ES} \left( \frac{1}{\pi} \sum_{i} \omega_i v_{si} - \sum_{j: s_j \geq k^*} \tilde{\omega}_j v_{sj} \right) $$

$$ \rightarrow \text{plim} \text{ aggregate direct contribution} - \beta^{ES} \sum_{j: s_j \geq k^*} \tilde{\omega}_j \hat{S}_i. $$

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

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

where $S$ is $N \times J$, $D$ is $N \times I$, and $\alpha$ and $\rho$ 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

$$ \rho = (S'(1 - P_D)S)^{-1}S'(1 - P_D)\hat{L}. $$
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. $\Gamma$ and $B$ are the matrices of structural parameters $\gamma$ and $\beta$ 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

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

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

$$\hat{S}_i = \frac{1}{2} \sum_j (\omega_{ij,t} + \omega_{ij,t-1})\beta_{ij}^L(C_j + e^c_{ij}). \quad (23)$$

The measured shock to the same area $i$ is

$$s_i = \frac{1}{2} \sum \omega_{ij,t} + \omega_{ij,t-1}E_i(\beta_{ij}^L)C_j$$

$$= \frac{1}{2} \sum (\omega_{ij,t} + \omega_{ij,t-1})E_i(\epsilon_{ij}^c \beta_{ij}^r)C_j$$

46
\[
\begin{align*}
&= \frac{1}{2} \sum_j (\omega_{ij,t} + \omega_{ij,t-1}) (E_i(\epsilon_i^{Lr}) E_i(\beta_{ij}^{s} + Cov_i(\epsilon_i^{Lr}, \beta_{ij}^{s}))) C_j \\
&= \frac{1}{2} \sum_j (\omega_{ij,t} + \omega_{ij,t-1}) [(E_j(E_i(\epsilon_i^{Lr}) + E_i(\epsilon_i^{Lr}))(\beta_{ij}^{s} + E_i(\beta_{ij}^{s}))) + Cov_i(\epsilon_i^{Lr}, \beta_{ij}^{s})] C_j \\
&= E_j(E_i(\epsilon_i^{Lr})) \frac{1}{2} \sum_j (\omega_{ij,t} + \omega_{ij,t-1}) \beta_{ij}^{s} C_j + \\
&\quad \frac{1}{2} \sum_j (\omega_{ij,t} + \omega_{ij,t-1}) [E_i(\epsilon_i^{Lr}) \beta_{ij}^{s} + E_i(\epsilon_i^{Lr}) E_i(\beta_{ij}^{s}) + E_j(E_i(\epsilon_i^{Lr}) E_i(\beta_{ij}^{s}) + Cov_i(\epsilon_i^{Lr}, \beta_{ij}^{s}))] C_j \\
&= \pi \hat{S}_i - \pi \frac{1}{2} \sum_j (\omega_{ij,t} + \omega_{ij,t-1}) \beta_{ij}^{s} \epsilon_{ij} + \psi_i \\
&\equiv \pi \hat{S}_i + v_{si}.
\end{align*}
\]

The parameter \(\pi\) 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 \(E_i(\epsilon_i^{Lr}) E_i(\beta_{ij}^{s})\) 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.\(^4\) Similarly, the sign of \(Cov_i(\epsilon_i^{Lr}, \beta_{ij}^{s})\) 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.

\(^4\)Assumptions 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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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: \( \text{Prob}(\text{Originated})_{it} = \alpha_{ct} + \beta \text{Wachovia}_i + \gamma_t X_{it} + e_{it} \). 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 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:

Log LTI: Difference Within County and Income Group

Log Income: Difference Within County

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) = α + βt Wachovia + γt Xit + eit. 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 county groups. Leading up to the crisis Wachovia was essentially indistinguishable from the average lender in the county, but by 2008 and 2009 the average income of a Wachovia originator 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: $\tilde{E}_{it} = \alpha_t + \beta_t \text{Wachovia Exposure}_i + \epsilon_{it}$. 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.
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

Credit Flows

Credit Stocks

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, $\tilde{L}_{ij} = \alpha_i D_i + \rho_j \Lambda_j + \epsilon_{ij}$, 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.
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 $\rho$ come from the regression $\bar{L}_{ij} = \alpha_i D_i + \rho_j A_{ij} + e_{ij}$. 

Figure 11: Distribution of Measured Shocks in the South and East
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 $\rho$ come from the regression $\overline{L}_{ij} = \alpha_i D_i + \rho_j \Lambda_j + e_{ij}$. See the text for more details.
C Tables
### Table 1: Summary Statistics - Total Sample

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*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

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*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: Correlation Between Wachovia Exposure and County Observables

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<td>0.421</td>
<td>0.770</td>
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Note: This table reports OLS estimates of Wachovia exposure regressed on various county observables: Wachovia Exposure \( \bar{y} = \alpha + \beta X_i + \epsilon_i \). 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

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N 478 342
Clusters 25 23
R2 0.124 0.112
F-stat 3.838 2.233

Note: This table reports OLS estimates of Wachovia exposure jointly regressed on various county observables: Wachovia Exposure, $i = \alpha + \sum_j \beta_j x_{ij} + \epsilon_i$. 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)

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FE: – Region, State
N: 478
Clusters: 25
R2: 0.120
F-stat: 28.187

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: $L_i = \alpha + \beta Wachovia Exposure_i + \theta X_i + \epsilon_i$. 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 Non-housing and Housing Demand

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<td>β /p/(CI)</td>
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<td>(-1.268, -0.438)</td>
<td>(-1.256, -0.155)</td>
<td>(-1.451, 0.153)</td>
<td>(-2.599, -0.329)</td>
<td>(-3.901, -1.017)</td>
</tr>
<tr>
<td>Mortgage Leverage 2006</td>
<td>-0.083</td>
<td>-0.075</td>
<td>-0.135</td>
<td>-0.157</td>
<td>-0.069</td>
</tr>
<tr>
<td></td>
<td>(-0.144, -0.021)</td>
<td>(-0.142, -0.009)</td>
<td>(-0.195, 0.052)</td>
<td>(-0.383, 0.068)</td>
<td>(-0.223, 0.085)</td>
</tr>
<tr>
<td>House Prices 2002-2005</td>
<td>-0.275</td>
<td>-0.305</td>
<td>0.280</td>
<td>0.468</td>
<td>0.196</td>
</tr>
<tr>
<td></td>
<td>(-0.253, 0.212)</td>
<td>(-0.757, 0.147)</td>
<td>(0.102, 0.458)</td>
<td>0.006</td>
<td>11.651</td>
</tr>
</tbody>
</table>

| N                      | 471       | 478       | 342       | 342       | 308       |
| Clusters               | 25        | 25        | 23        | 23        | 21        |
| R2                     | 0.090     | 0.162     | 0.542     | 0.501     | 0.199     |
| F-stat                 | 15.283    | 11.553    | 12.759    | 14.984    | 11.651    |

Note: This table reports OLS point estimates, p-values, and 95% confidence intervals of various measures of household housing and non-housing demand at the county level regressed on exposure to Wachovia: $Z_i = \alpha + \beta Wachovia Exposure_i + \theta X_i + \epsilon_i$. I measure exposure with Wachovia’s market share in 2005-2006 of non-refinance mortgages within the county. Exposure to Wachovia caused large declines across measures of both non-housing and housing goods. Column one measures nondurable expenditures using the growth in total retail sales as reported in the Nielsen Retail Scanner data. Column two reports the growth in the stock of auto debt from the CCP data. Columns three and four show the effect on house prices as reported by Zillow over a short and long horizon. Column five reports the growth rate in sales volume also from Zillow. Columns three through five control for pre-crisis house price growth from Zillow to improve precision. 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 7: Effect of Wachovia Exposure on Employment Growth by Sector 2007-2010

<table>
<thead>
<tr>
<th></th>
<th>(1) Construction</th>
<th>(2) Tradables</th>
<th>(3) No Trade/Construction</th>
<th>(4) No Trade/Construction</th>
<th>(5) Total</th>
<th>(6) Total</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>β /p/(CI)</td>
<td>β /p/(CI)</td>
<td>β /p/(CI)</td>
<td>β /p/(CI)</td>
<td>β /p/(CI)</td>
<td>β /p/(CI)</td>
</tr>
<tr>
<td>Wachovia Exposure</td>
<td>-1.196</td>
<td>-0.798</td>
<td>-0.463</td>
<td>-0.369</td>
<td>-0.708</td>
<td>-0.492</td>
</tr>
<tr>
<td></td>
<td>(0.004)</td>
<td>(0.250)</td>
<td>(0.042)</td>
<td>(0.054)</td>
<td>(0.014)</td>
<td>(0.094)</td>
</tr>
<tr>
<td></td>
<td>(-2.072, -0.320)</td>
<td>(-2.236, 0.640)</td>
<td>(-0.912, -0.014)</td>
<td>(-0.743, 0.005)</td>
<td>(-1.237, -0.178)</td>
<td>(-1.106, 0.121)</td>
</tr>
<tr>
<td>Mortgage Leverage 2006</td>
<td>-0.180</td>
<td>-0.046</td>
<td>-0.010</td>
<td>-0.001</td>
<td>-0.032</td>
<td>-0.003</td>
</tr>
<tr>
<td></td>
<td>(0.006)</td>
<td>(0.150)</td>
<td>(0.358)</td>
<td>(0.194)</td>
<td>(0.008)</td>
<td>(0.458)</td>
</tr>
<tr>
<td></td>
<td>(-0.269, -0.092)</td>
<td>(-0.108, 0.016)</td>
<td>(-0.028, 0.009)</td>
<td>(-0.024, 0.022)</td>
<td>(-0.057, -0.007)</td>
<td>(-0.033, 0.027)</td>
</tr>
<tr>
<td>Construction Employment Polynomial</td>
<td>–</td>
<td>–</td>
<td>–</td>
<td>Yes</td>
<td>–</td>
<td>–</td>
</tr>
<tr>
<td>House Price Polynomial</td>
<td>–</td>
<td>–</td>
<td>–</td>
<td>–</td>
<td>Yes</td>
<td>–</td>
</tr>
<tr>
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<td>478</td>
<td>478</td>
<td>478</td>
<td>478</td>
<td>478</td>
<td>360</td>
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<tr>
<td>Clusters</td>
<td>478</td>
<td>25</td>
<td>25</td>
<td>25</td>
<td>25</td>
<td>23</td>
</tr>
<tr>
<td>R2</td>
<td>0.172</td>
<td>0.019</td>
<td>0.031</td>
<td>0.048</td>
<td>0.111</td>
<td>0.176</td>
</tr>
<tr>
<td>F-stat</td>
<td>20.742</td>
<td>2.980</td>
<td>7.137</td>
<td>4.941</td>
<td>15.391</td>
<td>15.404</td>
</tr>
</tbody>
</table>

Note: This table reports OLS point estimates, p-values, and 95% confidence intervals of various measures of employment growth at the county level regressed on Wachovia exposure: \( \hat{E}_{ix} = \alpha + \beta \text{Wachovia Exposure}_{i} + \theta X_{i} + \epsilon_{i} \). The first four columns show that employment effects of Wachovia exposure were strongest in construction and in employment excluding tradables. Columns three and fourth show growth in all employment except tradables and construction where column four shows that even after controlling for changes in construction employment with a third-order polynomial there are still large effects on employment. Columns five and six show the effects on total employment where column six controls for changes in house prices from 2007-2010 with a third-order polynomial. Sector definitions come from Mian and Sufi [2014]. 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.
Table 8: Effect of Wachovia Exposure on Total Employment 2007-2010

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>$\beta$ /p/(CI)</td>
<td>$\beta$ /p/(CI)</td>
<td>$\beta$ /p/(CI)</td>
<td>$\beta$ /p/(CI)</td>
</tr>
<tr>
<td>Wachovia Exposure</td>
<td>-0.707</td>
<td>-0.653</td>
<td>-0.603</td>
<td>-0.620</td>
</tr>
<tr>
<td></td>
<td>0.016</td>
<td>0.030</td>
<td>0.048</td>
<td>0.070</td>
</tr>
<tr>
<td></td>
<td>(-1.261, -0.153)</td>
<td>(-1.245, -0.060)</td>
<td>(-1.195, -0.011)</td>
<td>(-1.306, 0.066)</td>
</tr>
<tr>
<td>GWF Share 2005-2006</td>
<td>0.091</td>
<td>0.130</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>(-3.291, 3.473)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Mortgage Leverage 2006</td>
<td>-0.033</td>
<td>-0.042</td>
<td>-0.024</td>
<td>0.024</td>
</tr>
<tr>
<td></td>
<td>0.034</td>
<td>0.002</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(-0.062, -0.003)</td>
<td>(-0.063, -0.020)</td>
<td>(-0.047, -0.002)</td>
<td></td>
</tr>
<tr>
<td>Leverage Fixed Effects</td>
<td>–</td>
<td>Yes</td>
<td>–</td>
<td>–</td>
</tr>
<tr>
<td>Industry Shares</td>
<td>–</td>
<td>–</td>
<td>Yes</td>
<td>–</td>
</tr>
<tr>
<td>Firm Credit Polynomial</td>
<td>–</td>
<td>–</td>
<td>–</td>
<td>Yes</td>
</tr>
<tr>
<td>N</td>
<td>478</td>
<td>478</td>
<td>478</td>
<td>478</td>
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<tr>
<td>Clusters</td>
<td>25</td>
<td>25</td>
<td>25</td>
<td>25</td>
</tr>
<tr>
<td>R2</td>
<td>0.111</td>
<td>0.119</td>
<td>0.157</td>
<td>0.138</td>
</tr>
</tbody>
</table>

Note: This table reports OLS point estimates, p-values, and 95% confidence intervals of total employment growth at the county level regressed on exposure to Wachovia: $\hat{E}_i = \alpha + \beta_{\text{Wachovia Exposure}} + \theta X_i + \epsilon_i$. The estimates show that exposure to Wachovia had a robust and negative effect on total employment growth. Column one includes the share of lending by GWF, which is insignificant. Column two controls for household leverage as fixed effects by quartile. Column three includes the shares of employment in finance, real estate, construction, and tradables sectors. Column four controls for 2007-2010 growth in small business credit with a third order polynomial. These results suggest firm credit is unlikely to be driving the effects from Wachovia exposure. Regressions limited to the subsample of counties in the South and East with at least 50,000 residents in 2006, and with CCP data. Pair bootstrap-t used to construct symmetric p-values and 95% confidence intervals clustered at the state level.
Table 9: Effect of Supply-driven Changes in Household Credit on Total Employment Growth 2007-2010

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>OLS 2SLS</td>
<td>2SLS 2SLS</td>
<td>2SLS 2SLS</td>
<td>2SLS (Wachovia 2002-2003)</td>
<td>2SLS (Commuting Zone)</td>
</tr>
<tr>
<td><strong>Household Credit 2007-2010</strong></td>
<td>0.147 0.000</td>
<td>0.325 0.020</td>
<td>0.294 0.120</td>
<td>0.320 0.080</td>
<td>0.259 0.000</td>
</tr>
<tr>
<td></td>
<td>(0.105, 0.189)</td>
<td>(0.040, 0.610)</td>
<td>(-0.067, 0.655)</td>
<td>(-0.039, 0.679)</td>
<td>(0.111, 0.406)</td>
</tr>
<tr>
<td><strong>Leverage Control</strong></td>
<td>-</td>
<td>-</td>
<td>Yes</td>
<td>Yes</td>
<td>-</td>
</tr>
<tr>
<td><strong>Industry Shares</strong></td>
<td>-</td>
<td>-</td>
<td>Yes</td>
<td>Yes</td>
<td>-</td>
</tr>
<tr>
<td><strong>N</strong></td>
<td>478</td>
<td>478</td>
<td>478</td>
<td>478</td>
<td>289</td>
</tr>
<tr>
<td><strong>Clusters</strong></td>
<td>25</td>
<td>25</td>
<td>25</td>
<td>25</td>
<td>24</td>
</tr>
<tr>
<td><strong>R2</strong></td>
<td>0.139</td>
<td>-0.066</td>
<td>0.056</td>
<td>0.003</td>
<td>-0.054</td>
</tr>
<tr>
<td><strong>Robust F-stat</strong></td>
<td>28.187</td>
<td>21.504</td>
<td>16.714</td>
<td>15.765</td>
<td></td>
</tr>
<tr>
<td><strong>Weak ID P-value</strong></td>
<td>0.002</td>
<td>0.002</td>
<td>0.011</td>
<td>0.003</td>
<td></td>
</tr>
</tbody>
</table>

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: $\tilde{E}_i = \alpha + \gamma L_i + \beta X_i + \epsilon_i$. 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 fixed effects for household leverage and the shares of employment in finance, construction, real estate, and tradables. Sector definitions come from Mian and Sufi [2014]. Column four uses Wachovia exposure from 2002-2003 as an instrument and recovers essentially the same elasticity. Column five aggregates the data to census commuting zones and recovers a very similar estimate of the elasticity. 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.
Table 10: Heterogeneous Effects of Supply-driven Changes in Household Credit on Total Employment Growth 2007-2010

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>High Income</td>
<td>Low Income</td>
<td>High Leverage</td>
<td>Low Leverage</td>
<td>High HUD</td>
<td>Low HUD</td>
</tr>
<tr>
<td></td>
<td>$\gamma /p/(CI)$</td>
<td>$\gamma /p/(CI)$</td>
<td>$\gamma /p/(CI)$</td>
<td>$\gamma /p/(CI)$</td>
<td>$\gamma /p/(CI)$</td>
<td>$\gamma /p/(CI)$</td>
</tr>
<tr>
<td>Household Credit 2007-2010</td>
<td>0.310</td>
<td>0.334</td>
<td>0.429</td>
<td>0.314</td>
<td>0.432</td>
<td>0.228</td>
</tr>
<tr>
<td></td>
<td>(0.013, 0.608)</td>
<td>(0.113, 0.554)</td>
<td>(-0.187, 1.044)</td>
<td>(-0.038, 0.667)</td>
<td>(0.012, 0.851)</td>
<td>(0.079, 0.378)</td>
</tr>
<tr>
<td>N</td>
<td>239</td>
<td>239</td>
<td>239</td>
<td>239</td>
<td>239</td>
<td>239</td>
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<tr>
<td>Clusters</td>
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<td>25</td>
<td>23</td>
<td>22</td>
<td>23</td>
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<tr>
<td>R2</td>
<td>0.061</td>
<td>-0.166</td>
<td>-0.204</td>
<td>-0.206</td>
<td>-0.130</td>
<td>0.091</td>
</tr>
<tr>
<td>Robust F-stat</td>
<td>17.411</td>
<td>33.323</td>
<td>4.795</td>
<td>29.132</td>
<td>9.269</td>
<td>24.043</td>
</tr>
<tr>
<td>Weak ID P-value</td>
<td>0.003</td>
<td>0.006</td>
<td>0.074</td>
<td>0.003</td>
<td>0.005</td>
<td>0.016</td>
</tr>
</tbody>
</table>

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: $\bar{E}_i = \alpha + \gamma L_i + \theta X_i + \epsilon_i$. I instrument for household credit growth with exposure to Wachovia over 2005-2006. Columns one and two show the elasticity of employment is almost identical in low- and high-income counties. Columns three and four show the elasticity is larger, but imprecisely estimated, in high-leverage counties. Columns five and six report a much larger elasticity 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.
Table 11: Lender Regulator Proportions in Total HMDA Data and Estimation Sample

<table>
<thead>
<tr>
<th>Estimation Sample</th>
<th></th>
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</thead>
<tbody>
<tr>
<td>OCC</td>
<td>0.30</td>
</tr>
<tr>
<td>FRB</td>
<td>0.20</td>
</tr>
<tr>
<td>FDIC</td>
<td>0.14</td>
</tr>
<tr>
<td>OTS</td>
<td>0.08</td>
</tr>
<tr>
<td>NCUA</td>
<td>0.05</td>
</tr>
<tr>
<td>HUD</td>
<td>0.22</td>
</tr>
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</table>

<table>
<thead>
<tr>
<th>Total</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>OCC</td>
<td>0.14</td>
</tr>
<tr>
<td>FRB</td>
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</tr>
<tr>
<td>FDIC</td>
<td>0.32</td>
</tr>
<tr>
<td>OTS</td>
<td>0.07</td>
</tr>
<tr>
<td>NCUA</td>
<td>0.23</td>
</tr>
<tr>
<td>HUD</td>
<td>0.16</td>
</tr>
</tbody>
</table>

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

<table>
<thead>
<tr>
<th></th>
<th>(1) OLS</th>
<th>(2) OLS</th>
<th>(3) Quantile</th>
<th>(4) OLS</th>
<th>(5) OLS-No Wachovia</th>
<th>(6) OLS-No Wachovia</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>β /p/(CI)</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Wachovia Exposure</td>
<td>-0.541</td>
<td>-0.498</td>
<td>-0.538</td>
<td>-0.406</td>
<td>-0.143</td>
<td>-0.016</td>
</tr>
<tr>
<td></td>
<td>0.004</td>
<td>0.000</td>
<td>0.012</td>
<td>0.004</td>
<td>0.576</td>
<td>0.450</td>
</tr>
<tr>
<td></td>
<td>(-0.872, -0.210)</td>
<td>(-0.811, -0.184)</td>
<td>(-0.901, -0.176)</td>
<td>(-0.593, -0.219)</td>
<td>(-0.471, 0.186)</td>
<td>(-0.217, 0.186)</td>
</tr>
<tr>
<td>Mortgage Leverage 2006</td>
<td>-0.012</td>
<td>-0.011</td>
<td></td>
<td>0.400</td>
<td>0.768</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
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<td>0.009</td>
<td></td>
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<td></td>
<td></td>
<td></td>
<td>(-0.033, 0.009)</td>
<td>(-0.046, 0.024)</td>
<td></td>
</tr>
<tr>
<td>HUD Share 2005</td>
<td>-0.010</td>
<td>-0.007</td>
<td></td>
<td>0.584</td>
<td>0.198</td>
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<tr>
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<td></td>
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<td></td>
<td>-0.080</td>
<td>0.061</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>(-0.080, 0.061)</td>
<td>(-0.216, 0.201)</td>
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<tr>
<td>Construction Share 2005</td>
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<td>0.934</td>
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<td>-0.095</td>
<td>0.074</td>
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<tr>
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<td></td>
<td>(-0.095, 0.074)</td>
<td>(-0.222, 0.161)</td>
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</table>

State Fixed Effects

<table>
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<th></th>
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<th></th>
<th>Yes</th>
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<tbody>
<tr>
<td>N</td>
<td>478</td>
<td>478</td>
<td>478</td>
<td>478</td>
<td>478</td>
<td>478</td>
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<td>25</td>
<td>25</td>
<td>25</td>
<td>25</td>
</tr>
<tr>
<td>R2</td>
<td>0.203</td>
<td>0.233</td>
<td>0.232</td>
<td>0.431</td>
<td>0.018</td>
<td>0.293</td>
</tr>
<tr>
<td>Robust F-stat</td>
<td>17.977</td>
<td>6.006</td>
<td>27.862</td>
<td>0.431</td>
<td>0.018</td>
<td>0.293</td>
</tr>
</tbody>
</table>

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 $s_i = \frac{1}{4} \sum_j (\omega_{ij,t} + \omega_{ij,t-1}) \rho_j$, and $\rho_j$ come from the regression $\hat{L}_{ij} = \alpha_i D_i + \rho_j \Lambda_j + \epsilon_{ij}$. 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 two columns exclude 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.
Table 13: Effect of Measured Shock on Employment 2007-2010

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>OLS</td>
<td>2SLS</td>
<td>2SLS</td>
<td>2SLS-No Tradables</td>
</tr>
<tr>
<td>( \gamma ) /p/(CI)</td>
<td>0.562</td>
<td>1.520</td>
<td>1.143</td>
<td>1.076</td>
</tr>
<tr>
<td>(CI)</td>
<td>(-0.320, 1.444)</td>
<td>(0.809, 2.230)</td>
<td>(0.484, 1.802)</td>
<td>(0.477, 1.674)</td>
</tr>
<tr>
<td>Leverage Fixed Effects</td>
<td>–</td>
<td>–</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Industry Shares</td>
<td>–</td>
<td>–</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>N</td>
<td>478</td>
<td>478</td>
<td>478</td>
<td>478</td>
</tr>
<tr>
<td>Clusters</td>
<td>25</td>
<td>25</td>
<td>25</td>
<td>25</td>
</tr>
<tr>
<td>R2</td>
<td>0.055</td>
<td>-0.104</td>
<td>0.064</td>
<td>0.029</td>
</tr>
<tr>
<td>Robust F-stat</td>
<td>17.977</td>
<td>23.515</td>
<td>23.515</td>
<td></td>
</tr>
</tbody>
</table>

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: \( \hat{E}_i = \alpha + \gamma s_i + \beta X_i + \epsilon_i \). 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 \( \rho \) come from the regression \( \hat{L}_{ij} = \alpha_i + D_i + \beta_j + \epsilon_{ij} \). 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.
Table 14: Effect of Measured Shock on Other Outcomes 2007-2010 (2SLS)

<table>
<thead>
<tr>
<th></th>
<th>(1) Nondurables</th>
<th>(2) Auto Debt</th>
<th>(3) Household Credit</th>
<th>(4) HPI</th>
<th>(5) HPI 2007-2012</th>
<th>(6) Sales</th>
<th>(7) Population</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Measured Shock</strong></td>
<td>1.725</td>
<td>1.428</td>
<td>4.142</td>
<td>1.135</td>
<td>2.701</td>
<td>4.696</td>
<td>0.089</td>
</tr>
<tr>
<td></td>
<td>(0.649, 2.801)</td>
<td>(0.312, 2.545)</td>
<td>(2.227, 6.057)</td>
<td>(0.284, 1.985)</td>
<td>(1.348, 4.055)</td>
<td>(1.885, 7.508)</td>
<td>(-0.004, 0.182)</td>
</tr>
<tr>
<td><strong>Mortgage Leverage 2006</strong></td>
<td>-0.060</td>
<td>-0.056</td>
<td>-0.081</td>
<td>-0.129</td>
<td>-0.141</td>
<td>-0.051</td>
<td>0.009</td>
</tr>
<tr>
<td></td>
<td>(0.002, 0.000)</td>
<td>(0.082, 0.000)</td>
<td>(0.000, 0.000)</td>
<td>(0.000, 0.000)</td>
<td>(0.638, 0.000)</td>
<td>(0.000, 0.000)</td>
<td>0.000</td>
</tr>
<tr>
<td><strong>House Prices 2002-2005</strong></td>
<td>-0.264</td>
<td>-0.279</td>
<td>0.325</td>
<td>-0.000</td>
<td>0.000</td>
<td>0.000</td>
<td>0.000</td>
</tr>
<tr>
<td></td>
<td>(-0.315, -0.214)</td>
<td>(-0.353, -0.205)</td>
<td>(0.241, 0.408)</td>
<td>0.394</td>
<td>0.000</td>
<td>0.000</td>
<td>(0.381, 0.407)</td>
</tr>
<tr>
<td><strong>Population 2002-2005</strong></td>
<td>0.394</td>
<td>0.000</td>
<td>0.000</td>
<td>0.000</td>
<td>0.000</td>
<td>0.000</td>
<td>0.000</td>
</tr>
</tbody>
</table>

N 471 478 478 342 342 308 478
Clusters 25 25 25 23 23 21 25
R2 0.026 0.015 -0.095 0.508 0.382 0.086 0.513

Note: This table reports 2SLS point estimates, p-values, and 95% confidence intervals for the elasticity of county outcomes with respect to the measured shock. 
\[ \hat{E}_i = \alpha + \gamma s_i + \beta X_i + \epsilon_i. \] 
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 \( \rho \) come from the regression \[ \hat{L}_{ij} = \alpha_i D_i + \rho_j \Lambda_j + \epsilon_{ij}. \] 
The estimates suggest large and robust effects of supply-side shocks to household credit except on population growth, on which there is small but statistically significant effect, except on population growth. Nondurables, household credit, and house sales are especially responsive. 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.
Table 15: Aggregate Direct Contribution of Shocks to Household Credit 2007-2010 (% Change)

<table>
<thead>
<tr>
<th></th>
<th>Total Employment</th>
<th>Employment Excluding Tradables</th>
</tr>
</thead>
<tbody>
<tr>
<td>No Adjustment - South and East</td>
<td>-11.8</td>
<td>-10.3</td>
</tr>
<tr>
<td>75th Percentile - South and East</td>
<td>-3.6</td>
<td>-3.1</td>
</tr>
<tr>
<td></td>
<td>-4.5</td>
<td>-3.9</td>
</tr>
<tr>
<td>75th Percentile - National</td>
<td>-3.1</td>
<td>-2.7</td>
</tr>
<tr>
<td>66th Percentile - South and East</td>
<td>-3.1</td>
<td>-2.7</td>
</tr>
<tr>
<td>66th Percentile - National</td>
<td>-3.8</td>
<td>-3.3</td>
</tr>
</tbody>
</table>

*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 direct contribution $= \frac{\beta^{ES}}{\pi} \sum_i \omega_i (s_i - \sum_j \omega_j \tilde{s}_j)$ for different choices of the cutoff $k^*$. See the text for details.