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# **Essays in Empirical Macroeconomics**

by

John Alexander Nelson Mondragon

A dissertation submitted in partial satisfaction of the

requirements for the degree of

Doctor of Philosophy

in

Economics

in the

Graduate Division

of the

University of California, Berkeley

Committee in charge:

Professor Yuriy Gorodnichenko, Chair

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Spring 2015

# **Essays in Empirical Macroeconomics**

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John Alexander Nelson Mondragon

## Abstract

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Doctor of Philosophy in Economics

University of California, Berkeley

Professor Yuriy Gorodnichenko, Chair

This dissertation provides evidence on the effects of changes in the supply of credit to households during the 2000s on employment and other outcomes of interest during the 2000s. In the first chapter I study the effects of contractions in household credit supply during the financial crisis of 2007-2009. I exploit a county's exposure to the collapse of a large and previously healthy lender as a natural experiment. I show that exposure to this shock appears to be uncorrelated with other important shocks at the time. Reduced form estimates suggest that this shock had large effects on the flow of credit, housing and non-housing expenditures, and employment. Using exposure to this shock as an instrument gives an estimated elasticity of employment with respect to household credit of about 0.3, caused by declines in both housing and non-housing demand. In the second chapter I study the size of the credit supply shock using non-parametric methods. I identify lender-specific supply-side shocks, which I then aggregate into a simple measure of credit supply shocks to counties. I provide conditions under which this measured shock can be used to quantify the importance of supply shocks to credit in both the cross-section and, in a partial equilibrium sense, the aggregate. Combining this measure with various estimates of the elasticity of employment with respect to the measure, I calculate that shocks to household credit can be responsible for 30 to 60% of the decline in employment from 2007 to 2010.

To My Parents

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Any remaining mistakes and limitations are my own.

# Chapter 1

## Household Credit and Real Effects of the Financial Crisis

### 1.1 Introduction

U.S. employment fell by over 7% from 2007 to 2010, but there is substantial uncertainty about what caused this decline. One potentially important shock to the economy over this period was the contraction in the supply of credit to households caused by distress in the financial sector. Insofar as household demand for output responded to changes in the supply of credit and output was affected by changes in household demand, then shocks to the supply of credit to households may have affected output and employment. The size of the household credit market suggests this channel has the potential to be very large. In 2006 households extracted almost \$900 billion of home equity with this quantity falling to essentially zero during the crisis.<sup>1</sup> However, there is relatively little evidence on how strongly household expenditures and employment responded to disruptions in household credit during the recession. I use a natural experiment to study the effects of contractions in household credit supply across counties on various outcomes of interest. I find that these shocks caused large declines in both housing and non-housing expenditures and that this resulted in significant declines in employment with losses concentrated in residential construction and non-tradables. My estimates suggest that a 10% decline in the flow of household credit due to supply shocks would cause a 3% decline in total employment. Given that household credit declined by about 40% over this period this suggests that shocks to the supply of household credit were potentially critical drivers of the recession.

I rely on variation across across U.S. counties to estimate quantities of interest. To ensure that this variation is exogenous, I exploit the collapse of Wachovia, a large and healthy lender before the crisis. In mid-2006, Wachovia purchased the mortgage lender

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<sup>1</sup>Estimates provided by Jim Kennedy of the mortgage system presented in “Estimates of Home Mortgage Originations, Repayments, and Debt On One-to-Four Family Residences,” Alan Greenspan and Kames Kennedy, Federal Reserve Board FEDS working paper no. 2005-41.

Golden West Financial to expand its market share in the West and to take advantage of Golden West's expertise in non-traditional mortgages. Golden West was a very large lender heavily concentrated in areas experiencing a collapse in house prices and specializing in loans with a high default risk. 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 resulted in Wachovia's distressed sale to Wells Fargo in December 2008.

I show that this distress resulted in Wachovia significantly contracting its supply of credit to households. Using detailed mortgage application data that allow me to observe the flow of household credit from lenders to counties, I show that during Wachovia's distress and even after its sale, Wachovia significantly contracted household access to credit 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. Similarly, applicants who did receive loans received smaller loans relative to other lenders in the area. Before and after the crisis, Wachovia's origination 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 for variation from a single lender to be informative about aggregate credit supply shocks it must be the case that there are frictions that limit the elasticity of substitution across lenders. Thus, if lender A contracts credit by more than lender B, then counties more dependent on lender A will suffer a larger credit contraction than counties more dependent on lender B. My estimates are functions of these frictions and so are informative about their relative strength in this period. But while limited substitution across lenders is well-documented in the firm credit market, there is less evidence on this elasticity in the household credit market. I provide direct evidence that these frictions are important in the household credit market. I find that Wachovia has a significantly higher market share in census tracts nearer to a Wachovia branch, even when comparing census tracts all within five miles of a branch. Consistent with this spatial pattern of market share, census tracts less than a mile away from a Wachovia branch had significantly lower mortgage growth from 2007 to 2010 relative to census tracts three to four miles away. Prior to the recession, these census tracts had similar trends in mortgage growth no significant selective placement of Wachovia branches across census tracts. This shows that spatial frictions were one of several potential frictions limiting the substitution of households away from the Wachovia shock.

Within counties in Wachovia's traditional markets, I find that an increase in Wachovia's market share of home purchase and refinance credit resulted in large and robust declines in household credit from 2007 to 2010. This reflects a direct decline in lending from Wachovia as well as indirect local equilibrium effects such as the resulting changes in house prices and income. Exposure to Wachovia in the household credit market also led to declines in retail expenditures, the number of house sales, and, to a lesser extent, declines in house prices. Residential construction employment responded most strongly to Wachovia exposure, followed by non-tradable employment, such as restaurant services. There was no significant

effect on tradable employment, consistent with these effects being driven by declines in household demand.

Distinguishing between the firm and household credit supply channels is critical. I first document that Wachovia's fall in lending to households was exceptional relative to other lenders in the market, while in the firm market Wachovia's contraction was strikingly average. This suggests that exposure to Wachovia in the cross-section will primarily reflect variation in the supply of household credit. I check this relationship using cross-sectional measures of Wachovia's importance in the firm credit market. While exposure to Wachovia in the firm and household credit markets is very correlated, there is useful variation between the two. Using this variation, I find that counties where more firms borrowed from Wachovia did have larger declines in firm credit even after exposure to Wachovia in the household credit market is taken into account. But Wachovia's role in the firm credit market had no effect on household credit, consumption, or any type of employment, except possibly in the nonresidential construction market. Together, these results indicate that the real effects of exposure to Wachovia were the result of Wachovia's role in the household credit market.

I also show that exposure to Wachovia is not correlated with common alternative explanations for the decline in employment. In particular, exposure to Wachovia is not standing in for household leverage, house price growth before the crisis, exposure to Golden West, subprime lending, trade shocks, and labor demand shocks from the real estate, finance, and construction sectors. In further support of the exclusion restriction, Wachovia's market share was very stable across the 2000s and therefore unlikely to reflect selection based on house price or household debt dynamics. To summarize, exposure to Wachovia appears to be a shock to household credit that was orthogonal to other local factors.

Using a county's exposure to Wachovia as an instrument for household credit supply gives the elasticity of employment to supply-driven changes in household credit 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. Given that household credit declined by over 40% during the Great Recession, the household credit channel's contribution to employment losses is potentially very large. This is important for a number of reasons. First, it suggests that household demand was an important driver of the recession so that policies to sustain or replace the fall in household demand would have been useful. Second, these results indicate that a large reason household demand fell was due to disruptions in the financial sector caused by the financial crisis. So to the extent that policy might have avoided or alleviated the distress in the financial sector and subsequent contraction in household credit supply a substantial portion of the fall in household demand might have been prevented.

## 1.2 Literature Review

Much of the focus on understanding the Great Recession has emphasized the collapse in house prices and household net worth, which are thought to have caused a fall in household

demand and so reduced employment. Mian and Sufi (2014) and Mian et al. (2013) provide seminal empirical evidence on the effects of changes in net worth on household consumption and employment. Mian et al. (2013) show that changes in household net worth induce large changes in household expenditures. Mian and Sufi (2014) show that these changes in household net worth also induce changes in employment and that the decline in net worth can explain as much as 40% of the observed decline in employment. Gropp et al. (2014) also emphasize the local decline in house prices, but focus on its effects on the supply of household credit. They argue that the collapse in house prices caused local lenders to reduce the supply of credit even to renters, who did not experience a decline in wealth. This body of work is distinct from my work as the effects that I identify are not caused by changes in local observables, but instead reflect shocks from financial intermediaries that are orthogonal to local conditions.

Another strain of literature emphasizes that the financial crisis reduced the supply of credit to firms who then reduced investment and employment. Ivashina and Scharfstein (2010) document that banks did contract their supply of credit significantly during the crisis. Almeida et al. (2009) and Campello et al. (2010) show that contractions in credit to firms affected firm outcomes including investment and employment. Chodorow-Reich (2014) and Greenstone et al. (2012) provide additional estimates of the effect of credit supply shocks on firm outcomes during the crisis in addition to providing partial equilibrium estimates of the size of the firm credit channel. Chodorow-Reich (2014) relies on variation from the syndicated loan market and finds large effects with as much as 40% of the observed decline in employment being attributable to declines in lending. Greenstone et al. (2012) uses variation from the small business credit market and finds very small effects. They calculate an upper bound of the contribution of credit shocks to small businesses responsible for at most 12% of the observed employment decline.

Much of the theoretical modeling of the Great Recession uses a decline in the supply of credit to households as a primary shock in generating outcomes reminiscent of the Great Recession. This is driven in part by the emphasis in the empirical importance of household leverage and the fact that interest rates have fallen to zero. This second fact is highly indicative of a reduced form “discount” factor shock, often explicitly modeled as contraction in credit supply to consumers. Along these lines, Eggertsson and Krugman (2012) model a reduction in aggregate demand caused by changes in indebted agents’ borrowing constraints. Guerrieri and Lorenzoni (2011) use a non-linear model and so emphasize that changes in borrowing constraints affect precautionary motives that can induce large declines in real output. Midrigan and Philippon (2011) highlight the role of liquidity shocks to households in a model that is explicitly cross-sectional. The previous models rely on nominal rigidities in order to generate declines in real output, Huo and Ríos-Rull (2013) and Kaplan and Menzio (2013) show that demand shocks combined with search frictions in goods markets can also result in real output declines where ? also consider a fall in demand resulting from a decline in household credit supply. Together, these papers argue that shocks to household credit supply can generate significant shortfalls in household demand, which in turn can result in declines in real output.

In spite of this emphasis in the theoretical literature, there has been relatively little empirical evidence on how strongly employment and household expenditures responded to supply-driven declines in household credit. Recent empirical work on the contraction in credit supply to households in the Great Recession includes Dagher and Kazimov (2012) and Ramcharan et al. (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.

Because the implication of this work is that household consumption depends significantly on the provision (or the expected provision) of household credit, my work is also related to the large literature on the determinants of consumption. More narrowly, Cooper (2013) finds that changes in household wealth increase household consumption primarily through a change in the value of collateral and that those households affected increase consumption significantly. Hurst and Stafford (2004) find that households likely to be liquidity constrained are more likely to refinance and extract equity, when they do, they spend significant fractions of the extracted equity on current consumption. Recent work by Violante et al. (2014) finds that the fraction of hand-to-mouth consumers, whose consumption is very sensitive to changes in the supply of credit, is significant once the share of wealthy hand-to-mouth households is taken into account. Baker (2013) finds that households likely to be nearer to borrower constraints have a significantly higher marginal propensity to consume and that the share of constrained households was large enough to significantly deepen the severity of the recession.

In addition to providing exogenous variation in credit supply to households, the Wachovia natural experiment contributes to our understanding of the effects of bank failures and the policy responses to bank failures. Ashcraft (2005) studies cases when the FDIC forced the failure of relatively healthy banks and finds that they have significant and long-lasting effects on local outcomes, although there the focus is on the provision of firm credit. Giannetti and Simonov (2013) study policy responses to fails, specifically capital injections, and find that large policy responses can have beneficial effects while capital injects that are too small might not increase the supply of credit and may even contribute to poorer lending standards. This is related to my work as 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.

### 1.3 Econometric Framework

In this section I build a simple econometric framework to guide my empirical work. 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  $\epsilon_i$ , which I assume is a scalar for simplicity. The reduced form solution for the price of credit in  $i$  is a function of supply-side factors in the area  $S_i$ , demand factors  $D_i$ , and other shocks so that  $r_i = R(S_i, D_i, v_i^r)$ , where  $v^r$  contains other factors. Finally, the prices of credit in other areas affect employment in  $i$  through spillovers, which are summarized by the functions  $g_i$  so that

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

For simplicity, I assume all functions are log linear where  $\hat{X}$  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  $v$

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

I call the elasticity  $\beta^{ES}$  the local direct effect of supply-side shocks to credit. This elasticity tells us the total effect, including local general equilibrium effects, of the local supply-side shock to credit on the outcome of interest at the area. This quantity is of direct interest as it tells us the direct, first order effects that supply-side shocks have on the outcome of interest. A very small elasticity would require a very large shock for the aggregate direct contribution to be large and so would likely mean that this channel is unimportant. Additionally, it is a useful moment for the calibration of structural models. The ideal approach to learning about  $\beta^{ES}$  would be to use data on  $\hat{E}_i$  and  $\hat{S}_i$  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  $\hat{S}_i$ . Consider the reduced form solution for the quantity of credit, which is a function of supply shocks, demand shocks, and other factors

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

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

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

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  $\tilde{v}_i = \sum_{j \neq i} \beta_{ij}^{ES} \hat{S}_j + v_i$ . Then a valid instrument is a variable  $Z_i$  that is correlated with changes in supply-side

shocks to credit  $Cov(Z_i, \hat{S}_i) \neq 0$ , and that satisfies the exclusion restrictions  $Cov(Z_i, \hat{D}_i) = Cov(Z_i, \tilde{v}_i) = Cov(Z_i, v_i^L) = 0$ . Under these assumptions, it is simple to see that

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

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  $\hat{L}^*$  is the decline in observed credit due to contractions in credit supply then  $\frac{\beta^{ES}}{\beta^{LS}} \times \hat{L}^* = \hat{E}^*$ , 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  $\hat{\gamma}$  converges in probability to  $\beta^{ES}/\beta^{LS}$  under standard assumptions. In this chapter I work to identify a valid instrument and recover this ratio of elasticities.

## 1.4 Data, Sample, and Summary Statistics

I rely on U.S. counties as my primary unit of observation. In addition to conforming with much of the prior literature, a wealth of reliable data are collected at the county level, which allows for a broad set of controls. 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. However, counties are not necessarily natural economic units of observation so that I also check my central results by aggregating to the Economics Research Service commuting zone level or restricting the sample to only large counties. These exercises are actually very similar and result in very similar results.

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.<sup>2</sup> See Avery et al. (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 et al. (2012) estimate the HMDA data cover between 77% and 95% of all mortgage originations from 2000 to 2006. I rely on the flow of non-refinance mortgages as my primary measure of household credit. While only a part of total household credit, mortgages are by far the largest component of most households' liabilities. 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*. These data include various characteristics of the loan and applicant including the loan amount, applicant income,

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<sup>2</sup><http://www.ffiec.gov/hmda/hmdaproducts.htm>.



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 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.<sup>3</sup> 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.<sup>4</sup> The CRA reporting thresholds are different from those in the HMDA and so do not cover the same set of lenders, although there is significant overlap. The data report measures of lending to small businesses at the county or census tract level depending on the definition used. The first measure defines small business credit as loans to businesses with revenue less than \$1 million. The second measure defines small business loans as any loan for less than \$1 million to a business. Greenstone et al. (2012) estimate that CRA-lenders originated 86% of all loans under \$1 million dollars and that the second definition covers about twice as much (30%) of total small business originations as the revenue-based measure. Because the coverage of the market is broader with the loan-size definition, I use this to measure firm credit flows.

I measure employment with the County Business Patterns (CBP) dataset, which contains annual observations on employment and payrolls by 4-digit NAICS identifier and size constructed from various administrative data from the universe of firms in the Census Bureau’s Business Register.<sup>5</sup> To separate firms into tradable, non-tradable, and construction industries I follow the classification of 4-digit NAICS codes in Mian and Sufi (2014). 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. 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 Zillow’s measure of sales volume.<sup>6</sup> 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.<sup>7</sup> The behavior of this index is broadly similar to the Case-Shiller index but is available at a finer level of geography.

To measure non-housing expenditures I rely on the Nielsen Retail Scanner database.<sup>8</sup> The data report sales at a weekly frequency from over 40,000 stores with county-level identi-

<sup>3</sup>I am very grateful to Robert Avery for making this file available to me.

<sup>4</sup><http://www.ffiec.gov/cra/craproducts.htm>.

<sup>5</sup><https://www.census.gov/econ/cbp/>

<sup>6</sup>See <http://www.zillow.com/research/data/>.

<sup>7</sup>See <http://www.zillow.com/research/zhvi-methodology-6032/> for more details of the Zillow methodology and Dorsey et al. (2010) for a discussion of different house price indexes.

<sup>8</sup>For more details see <http://research.chicagobooth.edu/nielsen/>.

fiers. The coverage of products is concentrated in non-durables, especially food, with broad coverage of counties (see Beraja et al. (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.<sup>9</sup> I also use gross income data from the IRS to measure county income.

**Summary Statistics and Sample** Figure 1.1 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. These categorizations are applied by the reporting institution when filing the HMDA report in accordance with HMDA guidelines. While home mortgage credit appears to recover in 2009, this is driven entirely by refinancing. Refinancing can reflect household's desire for liquidity as well as opportunistic pre-payment in order to take advantage of lower interest rates (Hurst and Stafford (2004)). Home purchase and home improvement originations continue to decline to about 40% and 30% of their 2005 level respectively. Figure 1.2 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 began to recover in early 2010.

Tables 1.1 and 1.2 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 the same 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 exposure to Wachovia is very low so that variation is not informative. I report summary statistics for both the entire set of counties and the subsample to demonstrate 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. All statistics are weighted by population 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. Construction declined by about 20% and tradables by 16%. Non-tradables were far less responsive, but still declined by 3% nationally and 2% in the subsample. Among these three categories,

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<sup>9</sup>See Lee and Van der Klaauw (2010) for more details.

non-tradables are the largest with about 19% of the employment share within counties, but all three categories account for only 44% of a county's total employment on average (most employment is not categorized).

There are large declines in home purchase credit and small business credit with both falling by about 46%. Refinancing credit (the total amount refinanced) actually grew by about 20% over the same period. Home purchase credit flows grew by over 60% from 2002 to 2006. 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 over 40% in both samples. 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 1.32 nationally and 1.14 in the subsample. House prices grew by about 40% in 2002 to 2006, and then declined by 20% nationally and 15% in the subsample from 2007 to 2010. Wachovia had about a 2% market share in home purchase mortgages and 3% of refinancing loans in the subsample and about 1% and 2% nationally. 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. Cross-correlations between observables are also very similar.

## 1.5 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 1.3). Wachovia's market share was also very persistent within these traditional markets. 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.

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 et al. (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 capitalization fell by \$1 billion, a little over 1% (Figure 1.4). 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 (Creswell (2006)). GWF was famously a portfolio lender and so retained all of its loans on its balance sheet, about \$125 billion in assets. It quickly became apparent that the performance of the loans acquired from GWF was particularly poor. In the fourth quarter of 2006, the first time Wachovia reported GWF's earnings with its own, Wachovia announced a decline of \$100 million in non-performing loans from its legacy operations but an addition of \$700 million in nonperforming loans from GWF (Cole (2007)). In addition, Wachovia's investment bank was suffering from poorly performing positions in the credit default swap market. Throughout the third and fourth quarters of 2007, Wachovia reported losses of roughly \$2.4 billion on asset-backed securities and loans while increasing its reserves for loan losses to \$1.5 billion (Dash and Werdigier (2007)). The jump in the loan-loss provision largely reflected "increased loss expectations for the portion of the Pick-a-Pay portfolio" according to the chief risk officer, and induced Wachovia to raise over \$3 billion dollars of equity in December 2007 to "strengthen" the company (Cole (2007)). In the fourth quarter of 2007 Wachovia's earnings declined 98% from the year before, from \$2.5 billion to just \$51 million.

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

Following the failure of WaMu on September 26, 2008, Wachovia experienced a silent bank run where it lost \$5 billion of primarily uninsured deposits in a single day. As a result, the

Federal Deposit Insurance Corporation (FDIC) organized the sale of Wachovia's operations to CitiGroup to avoid Wachovia's failure. However, Wachovia announced on October 3 that Wells Fargo would be purchasing the bank without government assistance. This was to take advantage of a more advantageous deal and the sale was completed in December. Following the merger, Wells Fargo continued to trim Wachovia's operations, although layoffs were moderate due to the relatively small geographic overlap between the two banks. Wells Fargo discontinued the Wachovia brand in 2011.

## 1.6 Loan-Level Evidence that Wachovia Contracted Credit to Households

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

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

$$\text{Prob(Originated)}_{it} = \alpha_{ct} + \beta_t \text{Wachovia}_i + \gamma_t X_{it} + e_{it}. \quad (1.1)$$

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.<sup>10</sup> 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.

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<sup>10</sup>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.

Figure 1.5 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 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 1.6 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 1.6 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.

## 1.7 Spatial Frictions

A critical determinant of how much Wachovia's contraction may affect real outcomes is the strength of frictions to substitution in the household credit market. If households are able to substitute across lenders easily, then the collapse of a single lender is unlikely to affect local outcomes. Substantial work in firm credit has relied on "soft" information as an explanation for sticky lending relationships. The importance of these frictions and relationship lending in firm finance has been well-documented by Berger and Udell (1995) and Petersen and

Rajan (1994), among others. The presence of these frictions also underlie the cross-sectional approaches to identifying the effects of firm credit supply shocks in Peek and Rosengren (2000) and Greenstone et al. (2012). There is less evidence, however, on the importance of these frictions in the household credit market, but Agarwal et al. (2011) find evidence for the use of soft information in home equity lending.

However, important work has shown that household behavior in the household credit market may result in limited substitution across lenders. Informational limitations can induce suboptimal shopping (Woodward and Hall (2012)) and a misunderstanding of loan terms (Bucks and Pence (2008) and Gerardi et al. (2010)). This “confusion” can reduce the price elasticity of borrower demand and so the elasticity of substitution across lenders (Chioveanu and Zhou (2013)). To the extent that borrower search is limited, it might suggest that physical distance might be an important determinant of which lender a borrower chooses. The literature on small business lending has emphasized the importance of spacial distance in determining terms between lenders and commercial borrowers (Petersen and Rajan (1994) and Petersen and Rajan (2002)). But little is known about the importance of distance and spatial frictions in the household credit market.

I test for the presence of these frictions with respect to distance from Wachovia. Specifically, I estimate a local zero-degree polynomial regression of the relationship between Wachovia’s market share in 2007 in a census tract and that census tract’s distance to the nearest Wachovia branch

$$\text{Wachovia's Market Share}_i = f(d_i) + e_i.$$

I limit the sample to census tracts within 4 miles of a Wachovia branch and demean each of the observations by the commuting zone average for all included census tracts. I weight the observations with an Epanechnikov kernel and set the bandwidth by rule of thumb. Figure 1.7 shows that Wachovia’s market share is strongly declining in distance. The difference within a commuting zone for moving a census tract from right next to a Wachovia branch to about 4 miles away reduces Wachovia’s market share by about 0.4 percentage point for home purchase loans and 0.5 percentage point for refinancing loans. Wachovia’s average market share in these census tracts is about 2% in the home purchase market and 5% in the refinance market so that the spatial frictions induce economically significant differences across census tracts.

Given this evidence for spatial frictions and the significant declines in credit access from the loan-level regressions, it is important to check if census tracts near a Wachovia branch had observably different trends in household credit growth during the boom or bust. Figure 1.8 estimates similar regressions but replaces market share with the growth rate demeaned by the commuting zone average for all census tracts within four miles of a Wachovia branch. The top two panels show that neither home purchase nor refinance loan growth had a significant relationship with distance from a Wachovia branch in the boom period. While there are some non-zero estimates the relationships do not resemble the market share estimates at all and the standard errors are very large. However, the bottom two panels show that during the crisis census tracts closer to a Wachovia branch, and so more likely to depend

on Wachovia, experienced deeper declines in home purchase and refinancing loan growth, although the confidence intervals for refinancing loan growth are wide enough to not reject the inclusion of zero. The relationship between loan growth and distance from 2007-2010 is strikingly different from the pre-crisis estimates, suggesting that Wachovia's contraction was not completely undone by substitution to other lenders.

These results at the census tract level show that distance from a Wachovia branch is an important determinant of the Wachovia's market share and that this same distance resulted in census tracts that depended on Wachovia experiencing larger declines in household credit during Wachovia's collapse. However, census tracts are unlikely to be useful for shedding light on any potential employment effects since these demand effects might spillover to neighboring census tracts. So going forward I aggregate my results to the county level.

## 1.8 Exclusion Restriction, First Stage, and Reduced Form

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

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

While access to credit at Wachovia contracted in 2008 and 2009 and census tracts exposed to Wachovia had lower loan growth, it still remains to show: (1) that exposure to Wachovia is likely to satisfy 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 of originations in household credit lending in the 2005 and 2006 HMDA data as my instrumental variable

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

There is a difficulty in constructing this market share. The HMDA data report not only mortgage originations, but mortgage purchases (when a second institution purchases the loan from the originating institution). Due to the inability to distinguish between these loans the HMDA aggregates suffer from a well-known problem of double-counting (see Scheessele (1998)). This suggests that one should not include purchases when calculating market shares since originators will have a deflated market share. However, Stanton et al. (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 not critical. This is especially true when weighed against the risk of missing important lender-county connections in the second part of my paper. Here I use only originations to compute market share. But all my reduced form results are robust to using originations and purchases to construct market share. I also provide non-parametric specifications to provide another quantification of the economic significance.

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 et al. (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 correction of Conley (1999).

The loan-level results showed that Wachovia was an average lender until the crisis, suggesting that Wachovia was expanding credit relative to other lenders prior to the crisis. But counties exposed to Wachovia might be different for some other reason, which would be potentially problematic. As a first check, Table 1.3 regresses exposure to Wachovia on a set of observables predictive of distress in local credit or employment markets. Overall, exposure to Wachovia is not strongly associated with indicators of the house price boom, subprime lending, or other important observables. There is a slight association with growth in nontradable employment growth and the level of mortgage leverage (not robust to state fixed effects). Critically, population growth, the share of employment in construction, growth in home purchase credit, and growth in construction employment are not predictive of exposure to Wachovia. The correlation with the share of HUD-regulated lenders is negative and robust across specifications, suggesting that exposure to Wachovia is not indicative of subprime lending. Overall, the joint tests of the regression significance indicate only mild statistical significance for the regressions without state fixed effects. In other words, most of the variation in exposure to Wachovia across counties is not associated with indicators of the economic boom and bust.

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 would be concerned about the exclusion restriction if exposure to Wachovia is strongly correlated with household credit growth in any direction well before the onset of the crisis. To check this, I estimate the following repeated cross-section regression of household credit, house price, and non-tradable employment relative to the base year of 2007 on exposure to Wachovia. I choose 2007 as the base year because that is the year right before Wachovia appears to begin contracting credit to households

$$\widehat{\text{Outcome}}_{i,t,2007} = \alpha_t + \beta_t \text{Wachovia Exposure}_i + \epsilon_{it}.$$

Figure 1.9 shows that counties exposed to Wachovia generally had no pre-crisis trends in any of these observables. While there is some slight credit growth in counties exposed to

Wachovia in the pre-crisis period, the relationship is small and the standard errors are very large. After 2007, however, counties exposed to Wachovia experienced larger declines in household credit growth that persist to 2011. Trends in non-tradable employment are also striking with essentially no pre-crisis trend in employment for counties exposed to Wachovia. But beginning in 2009 counties dependent on Wachovia began to experience larger declines in non-tradable employment. Finally, counties exposed to Wachovia also did not have a significant trend in relative growth in house prices leading up to the crisis. But again in 2009, after the initial decline in household credit, house prices began to fall, although the differences across counties are not statistically significant at standard levels of precision. Overall, the timing and direction of the Wachovia effects are consistent with a contraction in credit from Wachovia due to the crisis.

These relationships provide a stark contrast to the trends associated with household mortgage leverage in 2006. Household credit growth was booming in counties with high leverage until 2006, when growth turned severely negative and remained so until 2009. Non-tradable employment was also growing faster in counties with high leverage until 2008 and 2009. Finally, leveraged counties also saw house prices growing faster until 2006, when growth turned negative and remained so until about 2012. These correlations show the boom-bust pattern discussed by DiMaggio and Kermani (2014) and Kermani (2012), which are very different from the patterns associated with Wachovia.

**Effect of Exposure to Wachovia on Household Credit** Table 1.4 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 1.9, weak instrument issues are unlikely to be an issue. 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. The estimate suggests a one percentage point increase in exposure to Wachovia decreases home mortgages by over 2% from 2007-2010. This effect captures the direct decline in loans from Wachovia and any general equilibrium effects incident to the credit contraction (for example, the contraction in credit also causes a decline in income and house prices). The size of this effect is large. In addition to multipliers arising from declines in demand and subsequent falls in income, any effects on house price expectations are likely to significantly lower mortgage lending across all lenders, not just Wachovia. However, alternative measures of exposure such as deposit share and share of only originations gives smaller effects (from .5% to 1.7%) with similar statistical significance. The scaling issue must be kept in mind when interpreting these results. Column two introduces several important controls with no significant change in the coefficient and column three includes state fixed effects. State fixed effects lead to a relatively large decline in the point estimate and in precision, although the estimates hold up favorably relative to other observables such as leverage that have

been discussed in the literature. Columns four, five, and six repeat the specifications for growth in the total amount of refinancing credit. Refinancing growth seems to have a very large association with exposure to Wachovia, with a one percentage point increase in the exposure measure decreasing refinancing growth anywhere from 2 to over 3%. However, these estimates are very imprecise, even without state fixed effects. This suggests that it is important to isolate the different types of refinancing (cash-outs vs. interest rests). Figure 1.10 plots household credit growth against Wachovia exposure and visually confirms the relationship provided by the regressions. The left panel simply plots the association with a regression line while the right panel removes state trends. Consistent with the regressions, the relationship is stronger using interstate variation, but both panels show a significant and negative association between exposure to Wachovia and home purchase credit growth.

To partially address the scaling issue I also report semiparametric estimates of the relationship between exposure to Wachovia and credit growth in Figure 1.11. Because I cannot weight observations by their size I limit the sample to only those counties with at least 100,000 residents. The line reports the kernel-weighted estimate of the growth in home purchase credit (left panel) and refinance credit (right panel) as a function of exposure to Wachovia. Consistent with the regressions and scatter plots, exposure to Wachovia has a strongly negative relationship with home purchase credit growth. Moving from the most exposed counties to the least suggests a difference in household credit growth close to 10%, not far from the WLS estimates. The association with refinancing growth is also strongly negative with the implied difference approaching 20%. However, there is also much more dispersion in refinancing credit growth again emphasizing the need to eventually isolate refinancing that results in equity extraction. Together, these results show that counties dependent on Wachovia experienced significantly lower growth in household credit.

**Effect of Exposure to Wachovia on Expenditures and Employment** Before moving to the 2SLS estimates of the effect of household credit on employment, it is important to explore whether or not exposure to Wachovia had a discernible effect on household expenditures and employment in the reduced form and that these are likely resulting from effects on the supply of household credit.

The decline in household credit from Wachovia could have affected household demand in several ways. First, declines in home equity lines of credit (HELOCs) or cash-out refinancing loans will directly reduce household liquidity and expenditures (see Hurst and Stafford (2004) and Cooper (2013)). Second, as households are denied mortgages they are less likely to purchase a home. In addition, any consumption (often durables and home services) complementary to a home purchase will be foregone, although this is potentially countered by any substitution away from housing. Table 1.5 shows that exposure to Wachovia affected retail expenditures, house price growth, and housing sales. I measure expenditures using the Nielsen Retail Scanner data. Expenditure growth from 2007-2010 declined by about .8% in response to a one percentage point increase in Wachovia exposure with the effect very robust to controls, state fixed effects, and alternative weights. Table 1.5 shows that a

one percentage point increase in Wachovia exposure caused at least a 1.7% decline in house sales. This effect is also robust to controlling for mortgage leverage, state fixed effects, or alternative weights. Finally, there is also evidence in Table 1.5 that house prices decline from 2007-2010, although the effects are quite small and imprecise. The relationship becomes significantly larger when extending the horizon to 2007-2012, although state fixed effects again reduce the precision non-trivially. Exposure to Wachovia lowers house prices from 2007-2010 by about .3%, but the effect on house prices almost triples to 1.3% when I extend the horizon to 2012. This long lag suggests significant stickiness/momentum in home prices, potentially due to homeowners delaying sales in response to the weakening price level (see Genesove and Mayer (2001)). Overall, these results suggest exposure to Wachovia resulted in large declines in retail expenditures and home sales with some evidence of declines in house prices.

The decline in retail expenditures might cause a decline in local non-tradable employment if employment is determined by demand to some extent. Table 1.6 shows the effect of exposure to Wachovia on non-tradable employment and payrolls. Columns one and two show that losses in local non-tradable employment are higher in counties exposed to Wachovia, from 0.4 to 0.7% decline depending on the inclusion of state fixed effects. Columns three and four show that payrolls also decline as a result with a similar and possibly larger effect. All of these results are economically large and statistically fairly robust. These results suggest exposure to Wachovia did have effects on local real outcomes in the sectors likely to be affected.

It is also possible that the decline in house sales, house prices, and potential declines in home investment could affect local construction employment, particularly residential construction employment. Table 1.7 shows the results of exposure to Wachovia on total construction employment. Column one shows that exposure to Wachovia seems to cause a decline in overall construction employment, but this effect is not robust to state fixed effects (column two). Columns three and four do suggest that exposure to Wachovia resulted in payroll losses, but the difference between the two columns is very large. Table 1.8 limits the outcome to growth in only non-residential construction sectors. But here we see that once we control for state fixed effects there is no association between exposure to Wachovia and non-residential construction growth. Table 1.9 limits the outcome to growth only in residential construction employment and payrolls. Here we see that large losses in residential construction employment are associated with Wachovia exposure, although there is still a large difference between the estimates with and without state fixed effects. However, the lack of a response in non-residential construction indicates again that Wachovia is affecting outcomes through household credit market exposure.

If these effects are driven by declines in household demand, then it would be surprising to see any significant effects on tradables, which would depend far less on local demand. Consistent with this, Table 1.10 shows that while tradable employment has a negative relationship with Wachovia exposure the estimate is highly insignificant and exposure to Wachovia explains very little of the variation (0.002 r-squared). Additionally, the effect on payrolls even appears to be positive once state fixed effects are included.

It is possible that the losses in construction and non-tradables are undone by expansions

in sectors that are less affected by the shock. Table 1.11 and Table 1.12 present results for employment excluding all tradables and for total employment. We see that even including these sectors there are large declines in employment, although not as large as the losses in residential construction and non-tradables. As a further robustness check, I present non-parametric estimates of the relationship between both nontradable and tradable employment in Figure 1.12. These estimates show that both types of employment have a strong, negative relationship with exposure to Wachovia and that this relationship is reasonably linear.

While exposure to Wachovia caused losses in employment consistent with a fall in household demand, an important alternative to rule out is that the effects from Wachovia are actually due to contractions in firm credit, not household credit. While this would be interesting, it would be problematic for the interpretation of my results as illuminating the household credit channel. Ex ante, this is unlikely due to the size of Wachovia's home mortgage lending relative to its firm lending: the flow of 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 1.13 plots the flow of household and small business originations for Wachovia and the market in general. The right panel plots the stock of household and commercial loans reported on commercial bank call reports for both groups. Both graphs show that the decline in lending from Wachovia was primarily concentrated in the household lending market while Wachovia's lending to firms generally tracked the market trends.

Table 1.13 reports the relationship between household and firm credit and exposure to Wachovia in these two markets using the HMDA and CRA data. Column one includes a fixed effect for whether or not a county is in the top half of exposure to Wachovia in the small business credit market. The coefficient on exposure to Wachovia in the household credit market is essentially unchanged and the coefficient on exposure in the firm credit market is economically small and statistically insignificant. Column two replaces the continuous measure of exposure to Wachovia in the household credit market with a discrete measure identical to the CRA measure of high Wachovia exposure. Exposure to Wachovia in the firm credit market enters with the wrong sign, positively, while exposure in the household credit market enters negatively. These results suggest, surprisingly, that exposure to Wachovia in the firm credit market has essentially no effect on declines in household credit growth.

Columns three, four, and five replace growth in household credit with growth in small business credit. In contrast to the first two columns, exposure to Wachovia in the firm credit is negatively associated with growth in firm credit (exposure in the household credit market also enters negatively). The precision of the estimates is never high, but the results are very different from those for household credit growth. Column three simply shows that high exposure to Wachovia indicates a large, but imprecisely estimated, decline in firm credit growth. Column four includes continuous exposure to Wachovia in the household credit market. This reduced the sign on high exposure to Wachovia in the firm credit market but the estimate is still economically very large. Exposure to Wachovia in the household

credit market also has a negative effect on firm credit growth. Finally, column five uses both discrete measures of exposure to Wachovia. Again, exposure to Wachovia in both markets indicates large declines in firm credit growth. Surprisingly, the coefficient on household credit exposure is about two percentage points large than the coefficient on firm credit exposure. Overall, these regressions show that exposure to Wachovia in the firm credit market does predict declines in firm credit, but there seems to be no relationship between exposure to Wachovia in the firm credit market and household credit growth.

These results, especially column one of Table 1.13 emphasize that exposure to Wachovia in the household credit market largely reflects shocks to the household credit market. To further support this conclusion Table 1.14 reports regressions of nontradable and total employment growth on measures of exposure to Wachovia in both markets. Column one controls for above-median exposure to Wachovia in the CRA data, which has no economic or statistical significance on nontradable employment growth. Additionally, there is no change on the coefficient for exposure to Wachovia in the household credit market. Column two uses both discrete measures. While both coefficients are statistically imprecise, only the coefficient on exposure in the household credit market is economically significant with exposure in the firm credit market indicating only .1% lower employment growth as opposed to 2% lower. The results on total employment provide the same takeaways. While exposure to Wachovia in the firm credit market predicts lower firm credit growth, this exposure has essentially no relationship with the losses in employment associated with exposure to Wachovia.

Another possibility is that what I am labeling household credit is actually functioning as firm credit. Adelino et al. (2013) document that small firms, essentially entrepreneurs, rely on household credit, especially refinancing, to start and operate their firms. Consistent with this they find that instrumented growth in house prices leads to growth in the number of of very small, fewer than 4 employees, establishments and essentially no effects on larger firms. Interestingly, they do not find that the subsequent decline in house prices has similar employment effects. To check if my results are being driven entirely by growth in small establishments I regress establishment growth for different size categories on exposure to Wachovia in the household credit market in Table 1.15. I find that establishments of all sizes respond to Wachovia exposure in a similar way, with some evidence that growth in large establishments is more responsive. In general, there is no evidence that the results above are driven only by growth in small establishments, which would suggest a firm-side story.

**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  $\beta^{ES}/\beta^{LS}$ , the ratio of employment and credit quantities elasticities with respect to the shock to credit (see Section 1.3).

Table 1.16 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 1.2%. However, the 2SLS estimates in Table 1.16 show the OLS coefficient is biased downward with the estimate in column two about twice as large. The first-stage diagnostics are quite good across all specifications (which was apparent in the reduced form results earlier), which suggests Wachovia is unlikely to be a weak instrument. The elasticity of 0.3 means a 10% decline in household credit driven by supply shocks would cause a 3% decline in total county employment. Column three includes state fixed effects with no significant change in the estimated coefficient. Column four performs an additional robustness check by using only counties with at least 100,000 residents. This addresses the concern that employment and credit dynamics in small counties might not represent real changes. All of these estimates are strikingly similar.

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.

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

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

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

Additionally, many types of credit such as small consumer loans and home equity line of credit loans are not recorded in the HMDA data, but it is likely that Wachovia also contracted in these markets. This reinforces my interpretation of these results as the elasticity of employment with respect to a measure of household credit, and not just home mortgage credit. However there are two potential concerns. First, it is possible that Wachovia's market share in these other types of credit is very different, so that Wachovia's home mortgage market share is a poor indicator of exposure to these credit contractions. I find that Wachovia's market shares across counties in the three types of credit that I do measure are very correlated (0.72 to 0.85). Moreover, regressing the share of home improvement and refinance

loans on the share of home purchase loans gives highly significant coefficients of 0.88 and 1.25 respectively. While different from one, these coefficients suggest Wachovia's market shares across credit types are relatively similar.

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

**Heterogeneous Effects** The literature on curvature of the consumption function suggests household leverage and income might affect the response to household credit supply shocks (see Baker (2013) and Carroll and Kimball (1996)). Columns one and two of Table 1.17 show that the elasticity in low-income counties is significantly larger than that recovered in high-income counties. Columns three and four show that high-leverage counties have a larger response than counties with less leverage. I find the estimated effect is slightly larger in low-income counties and there is essentially no difference between high- and low-leverage counties. While the difference in income is suggestive, further work with the ability to recover more precise estimates is necessary.

## 1.9 Conclusion

In this chapter 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 chapter I construct a measure of the broader shock to household credit and perform the simple accounting exercise to try and quantify the size of the supply shock to household credit.



Figure 1.1: 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.

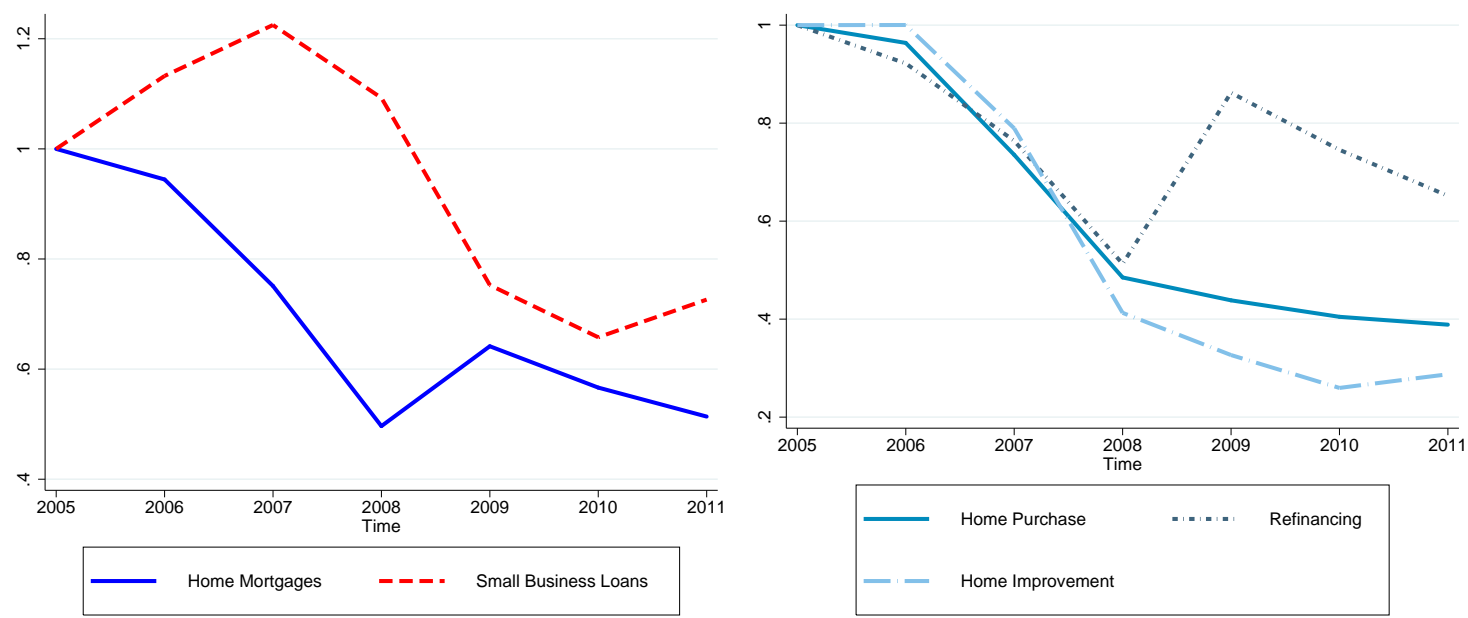


Figure 1.2: Nonfarm Employment and House Prices Normalized to January 2005 Level

*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.

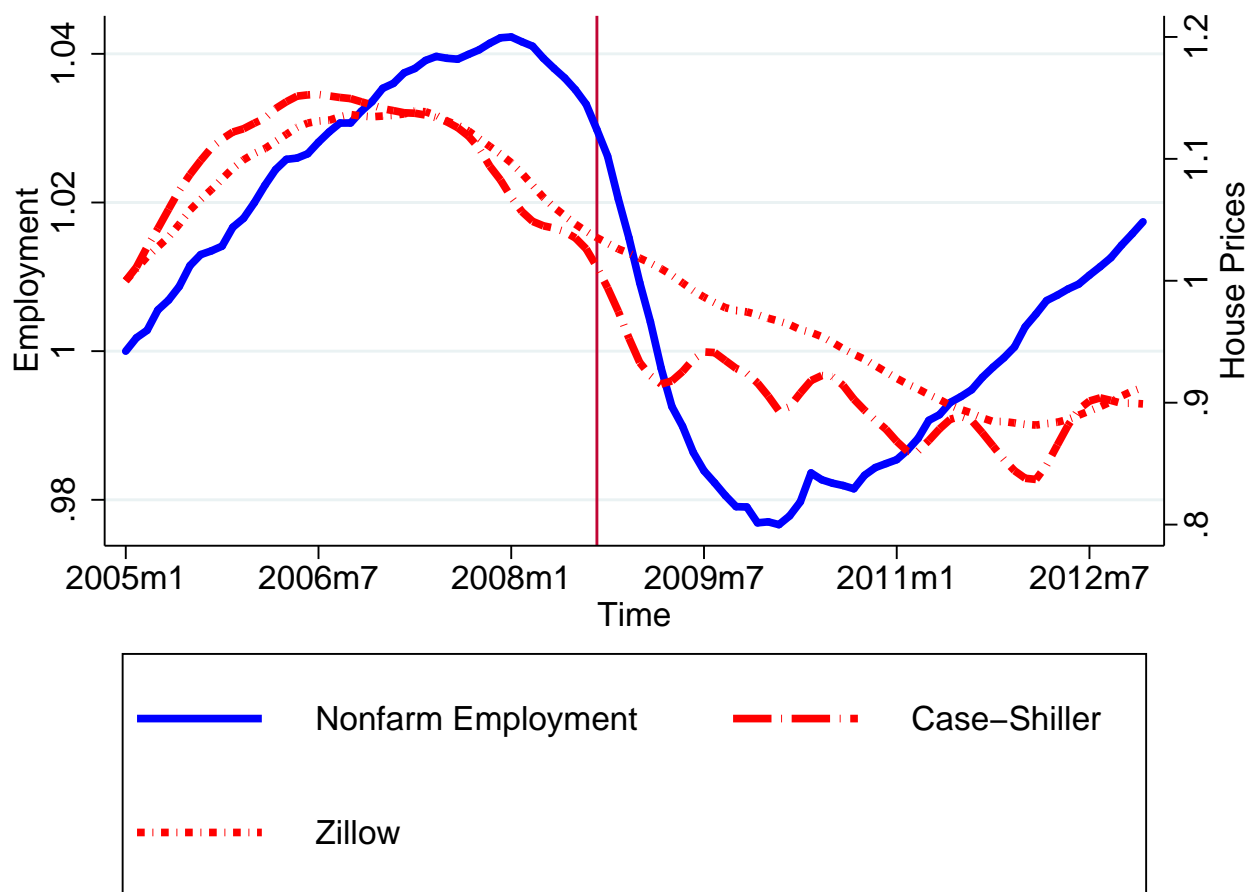


Figure 1.3: Distribution of Average of Wachovia Share of Home Mortgage Lending in 2005-2006

*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.

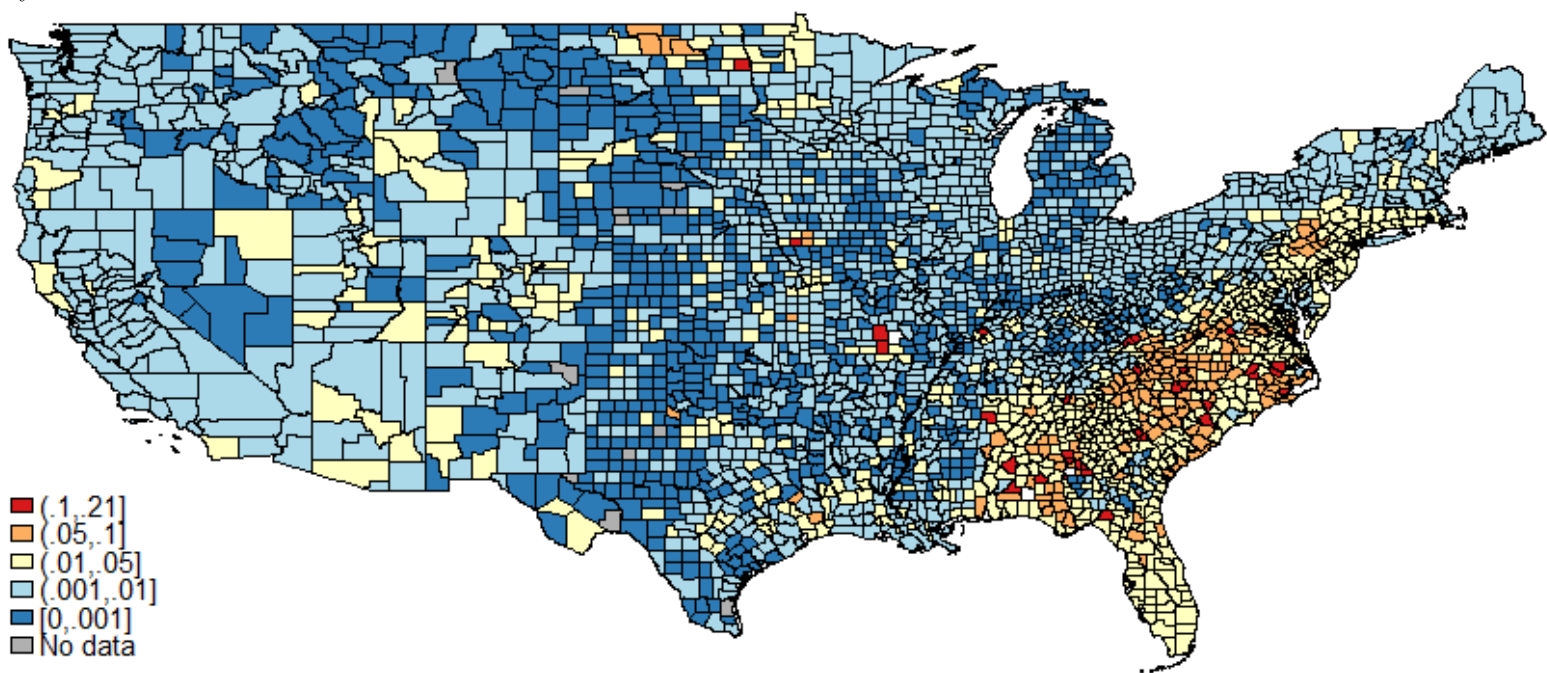


Figure 1.4: Wachovia's Collapse

*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.

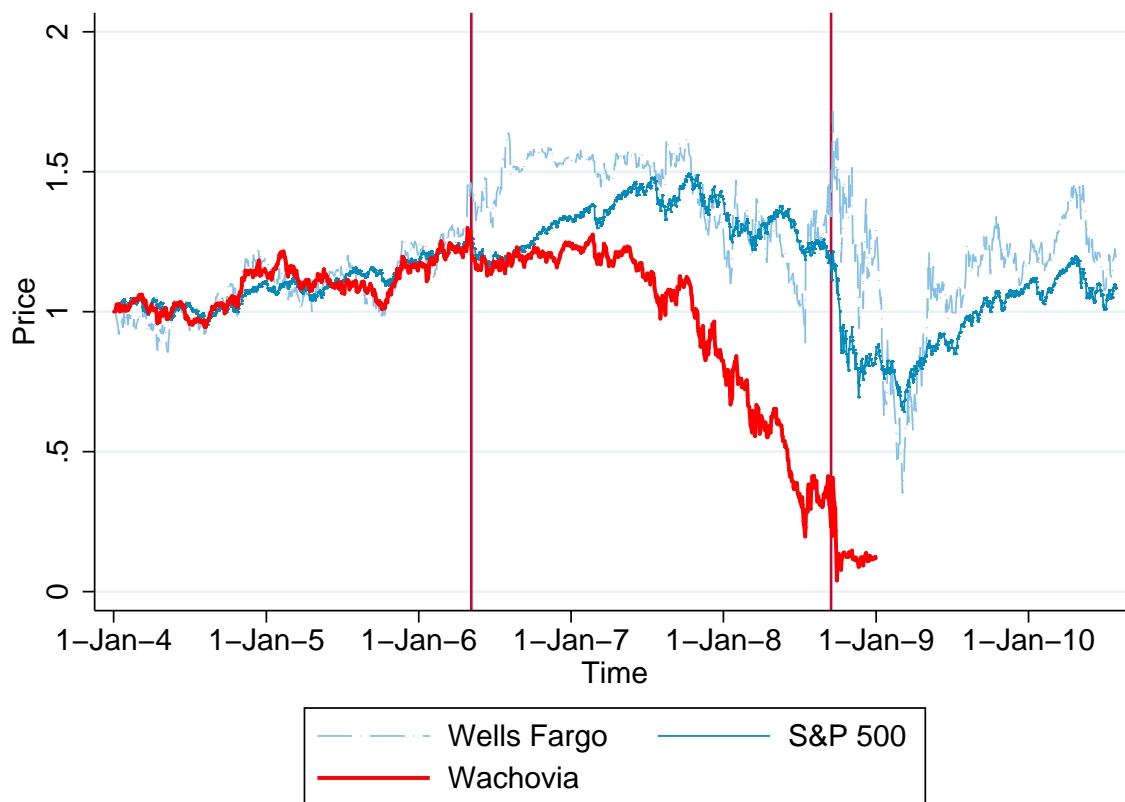


Figure 1.5: Wachovia and Origination Probabilities: Difference Within County and Income Group

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

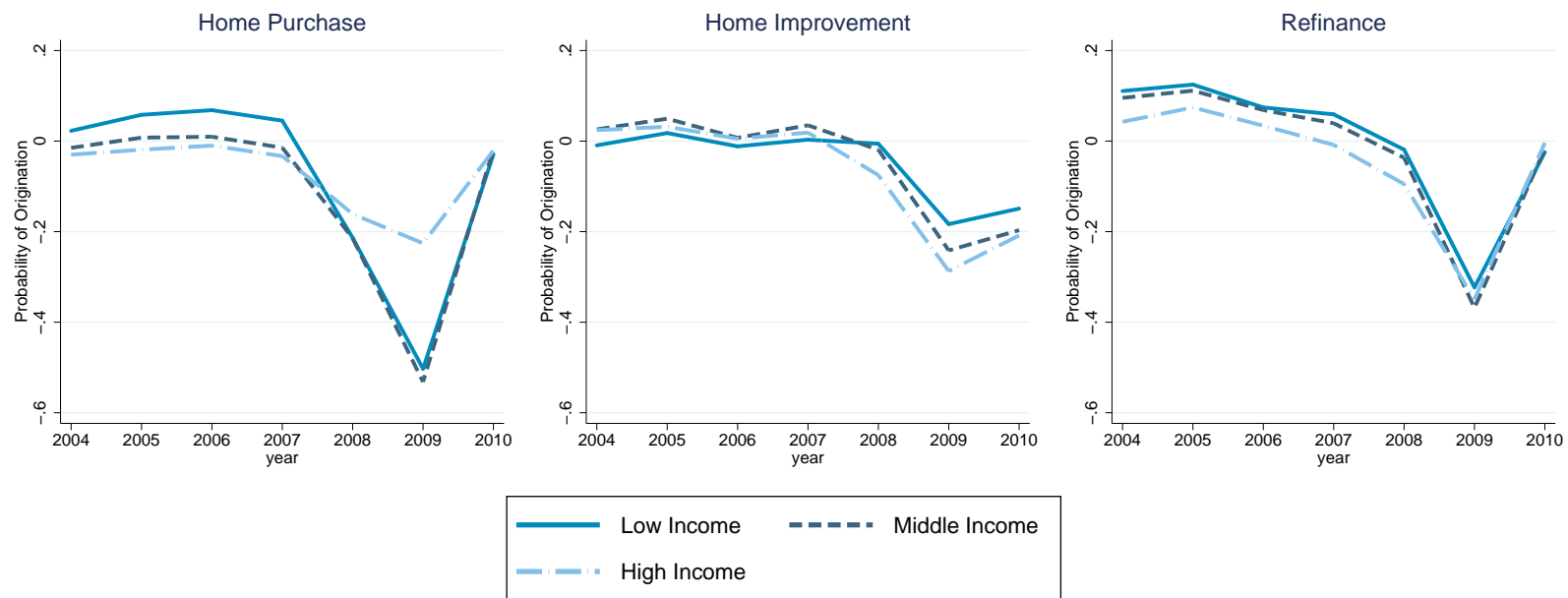


Figure 1.6: Wachovia and Log LTI and Log Income on Originated Home Purchase Loans:

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

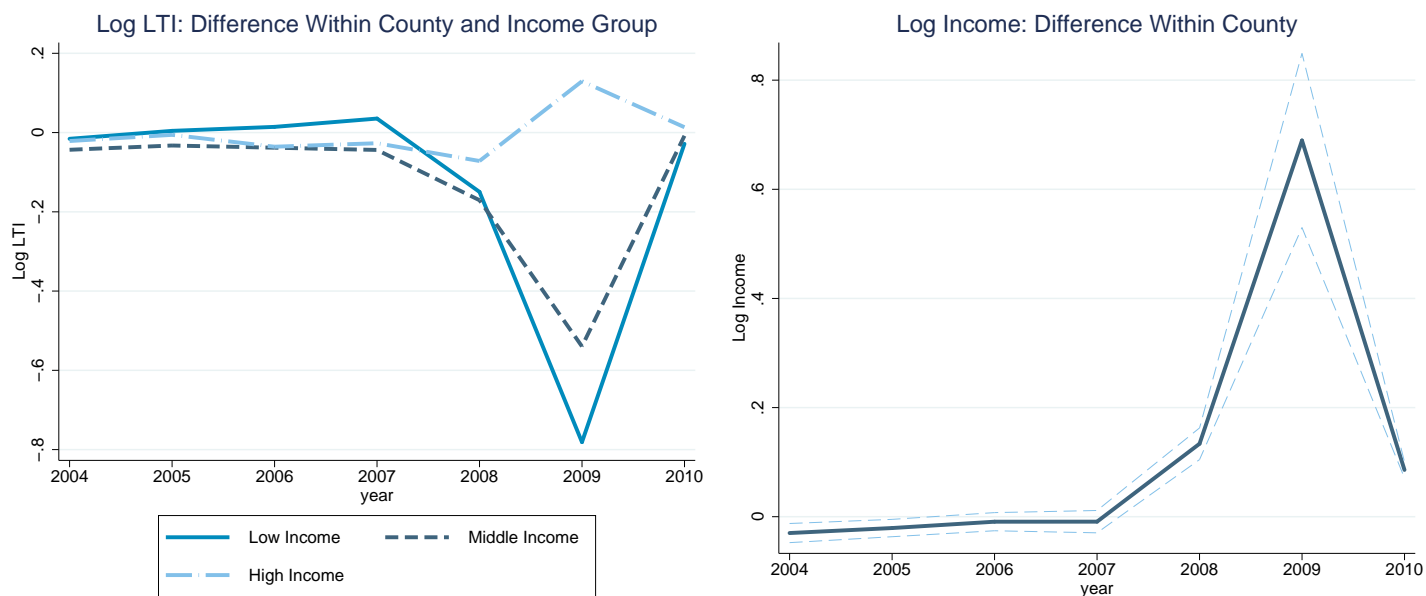


Figure 1.7: Wachovia's Share of Census Tract Originations in 2007 and Distance from Nearest Wachovia Branch

*Note:* These figures plot local polynomial smoothed estimates of Wachovia's market share in a census tract as a function of that tracts distance to the nearest Wachovia branch:  $\text{Wachovia's Market Share}_i = f(d_i) + e_i$  where  $d_i$  is the distance in miles of the census tract centroid to the nearest Wachovia branch. I limit the sample to all census tracts within four miles of a Wachovia branch and in a commuting zone. Market share is demeaned by the commuting zone average and I report Epanechnikov-kernel 95% confidence intervals reported. The estimates show that Wachovia's market share is significantly higher in census tracts closer to Wachovia branches for both home purchase and refinancing loans, indicating the presence of spatial frictions. Household credit data are from HMDA with market shares constructed using the count of originated loans. Similar results hold if including purchase loans. Branch location data are from the FDIC and census tract locations are from the Census. The number of observations is about 12,500.

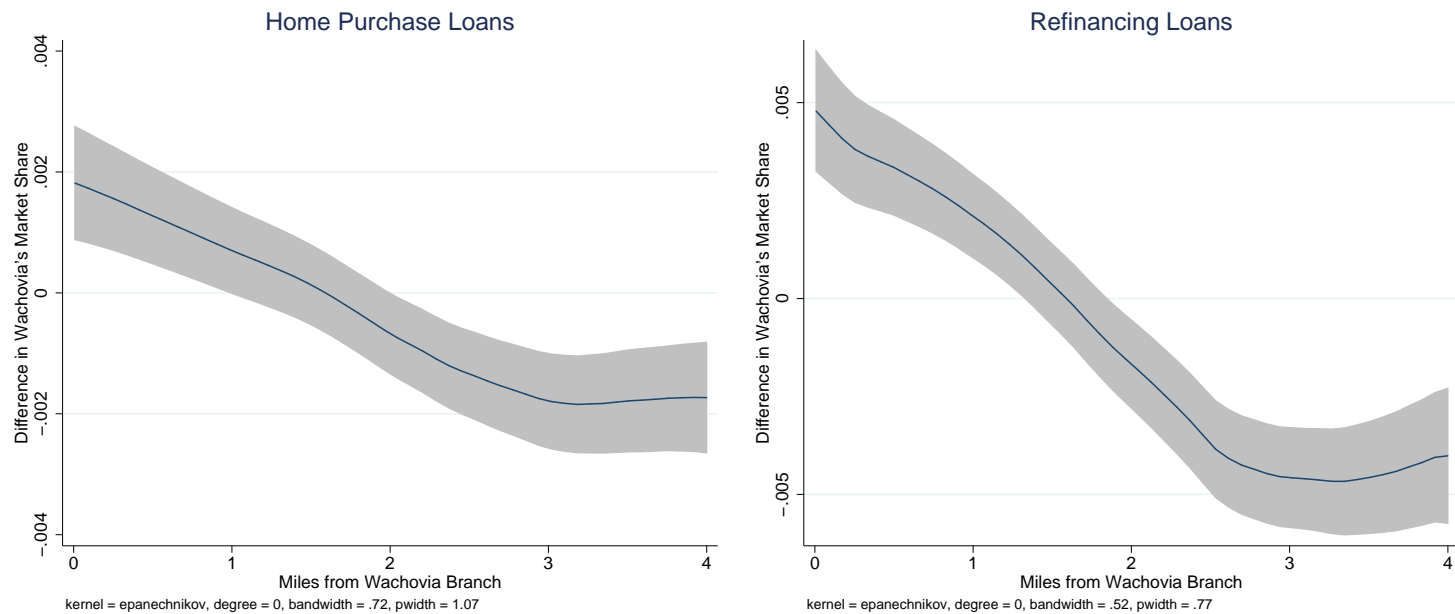


Figure 1.8: Census Tract Household Credit Growth and Distance from Nearest Wachovia Branch

*Note:* These figures plot local polynomial smoothed estimates of the growth in the number of loan originations in a census tract as a function of that tract's distance to the nearest Wachovia branch:  $\hat{L}_i = f(d_i) + e_i$  where  $d_i$  is the distance in miles of the census tract to the nearest Wachovia branch. I limit the sample to all census tracts within four miles of a Wachovia branch. Loan growth is demeaned by the commuting zone average and Epanechnikov-kernel 95% confidence intervals reported. The estimates show that from 2002-2006 there is no clear relationship between loan growth and proximity to Wachovia. From 2007 to 2010 census tracts close to Wachovia experienced less growth relative to census tracts three to four miles for both home purchase and refinancing loans. The differences between census tracts within a mile of Wachovia and census tracts four to five miles away from a Wachovia are economically significant: about two percentage points for home purchase loans and one percentage point for refinance loans. These results again indicate the presence of spatial frictions that limit substitution from Wachovia to other lenders. Household credit data are from HMDA with market shares constructed using the count of originated loans. Similar results hold if including purchase loans. Branch location data are from the FDIC and census tract locations are from the Census. The number of observations from 2002-2006 for home purchase loans is 8,838 and for refinancing loans 9,141, where the difference comes from dropping outliers. In 2007-2010 the number of observations is about 8,692 for home purchase loans and 8,892 for refinancing loans.

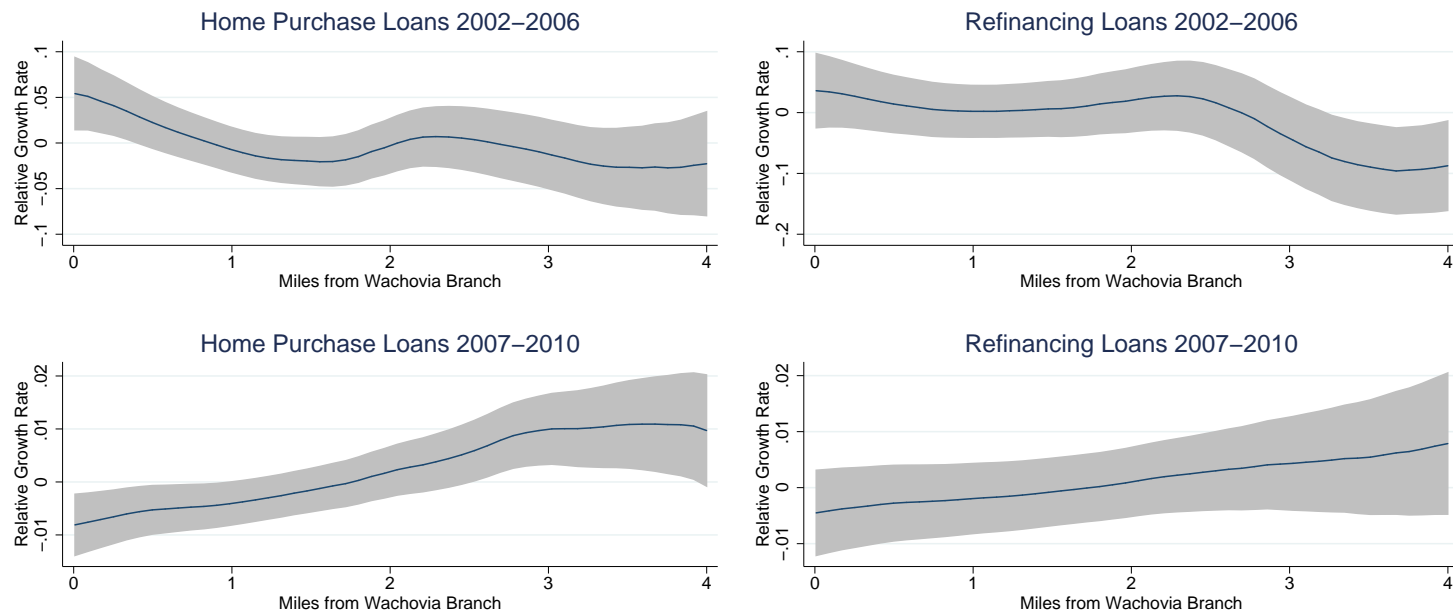




Figure 1.9: Exposure to Wachovia and Trends in Household Credit, Non-tradable Employment, and House Prices

*Note:* This figure plots coefficients from repeated cross-sectional regressions of home purchase credit, nontradable employment, and house prices all relative to 2007 on exposure to Wachovia:  $\text{Outcome}_{it,2007} = \alpha_t + \beta_t \text{Wachovia Exposure}_i + \epsilon_{it}$ . Wachovia exposure is measured as Wachovia's average market share in home purchase and refinance credit from 2005-2006. There is little evidence that exposure to Wachovia is associated with a trend in any of these outcomes prior to 2007. In 2008 home purchase growth begins to fall sharply in counties with more exposure to Wachovia while nontradable employment and house price growth begin to fall sharply in 2009. See the text for details on the data sources. The sample is limited to counties in the South and East with at least 50,000 residents and CCP data. State-clustered 95% confidence intervals are reported.

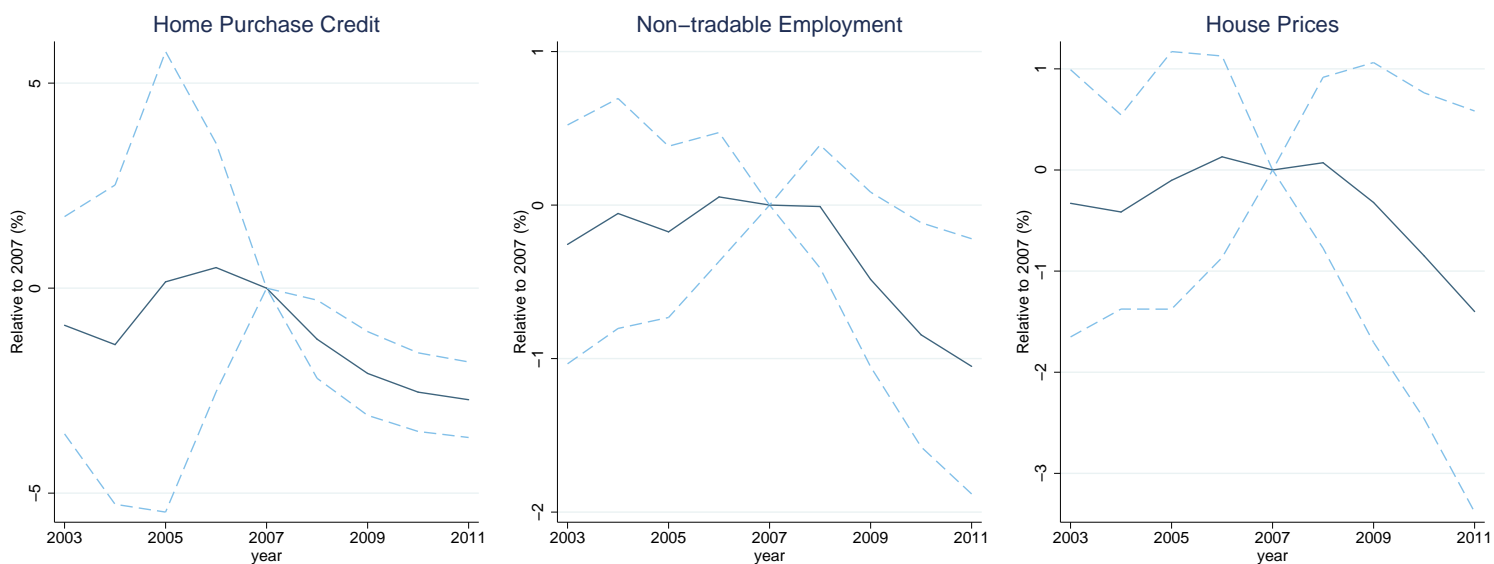


Figure 1.10: Effect of Exposure to Wachovia on Household Credit Growth

*Note:* This figure plots Wachovia exposure, measured as Wachovia's average market share of home purchase and refinance mortgages from 2005-2006 against the county-level growth rate of home purchase mortgages from 2007-2010. The right figure removes state averages from credit growth. 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. Observations are weighted with a counties population in 2006.

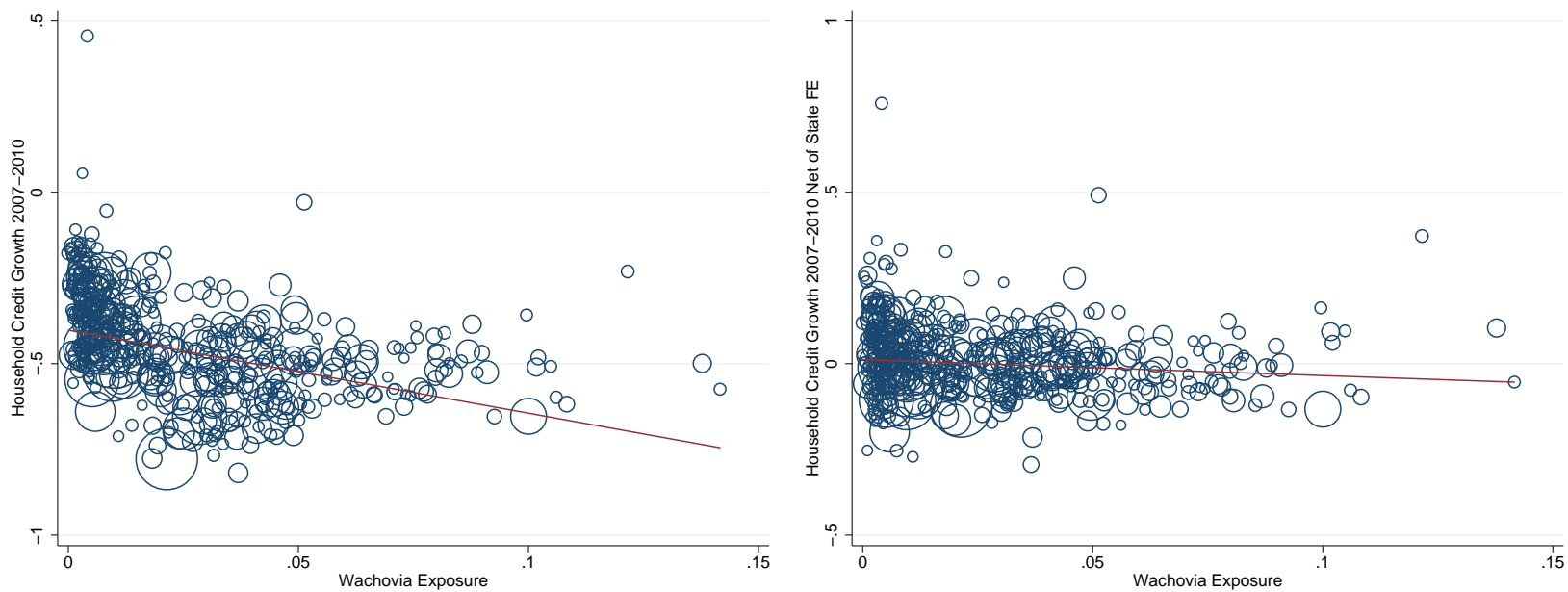


Figure 1.11: Wachovia and Household Credit Growth 2007-2010 in Large Counties, Semiparametric

*Note:* This figure plots the nonparametric relationship between Wachovia exposure and credit growth net of state fixed effects and mortgage leverage. Wachovia exposure is measured as Wachovia's average market share of non-refinance mortgages from 2005-2006. The left figure plots exposure and the county-level growth rate of home purchase mortgages and the right figure plots exposure against refinance mortgages from 2007-2010. Counties with more exposure to Wachovia tended to have significantly lower growth in both types of credit and the relationship appears to be very close to linear. The sample is limited to counties in the South and East with at least 100,000 residents and CCP data. All data are from HMDA.

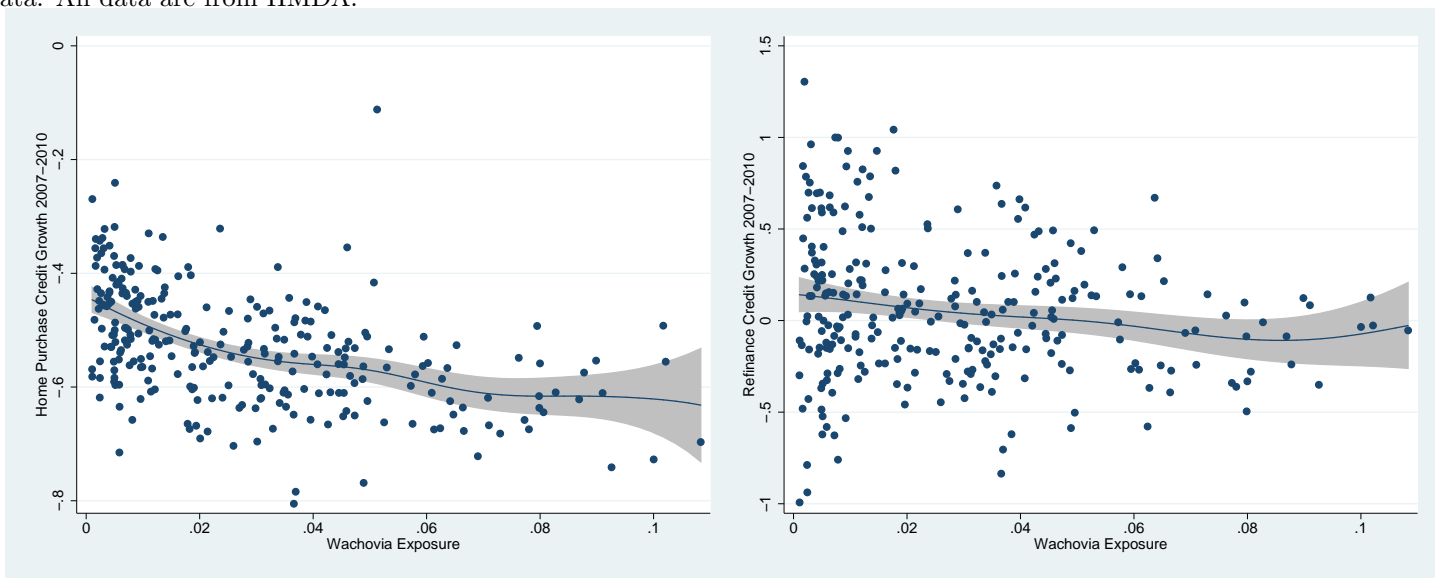


Figure 1.12: Wachovia and Employment Growth 2007-2010 in Large Counties, Semiparametric

*Note:* This figure plots the nonparametric relationship between Wachovia exposure and employment growth net of state fixed effects and mortgage leverage. Wachovia exposure is measured as Wachovia's average market share of non-refinance mortgages from 2005-2006. The left figure plots exposure and the county-level growth rate of home purchase mortgages and the right figure plots exposure against refinance mortgages from 2007-2010. Counties with more exposure to Wachovia tended to have significantly lower growth in both types of credit and the relationship appears to be very close to linear. The sample is limited to counties in the South and East with at least 100,000 residents and CCP data. All data are from HMDA.

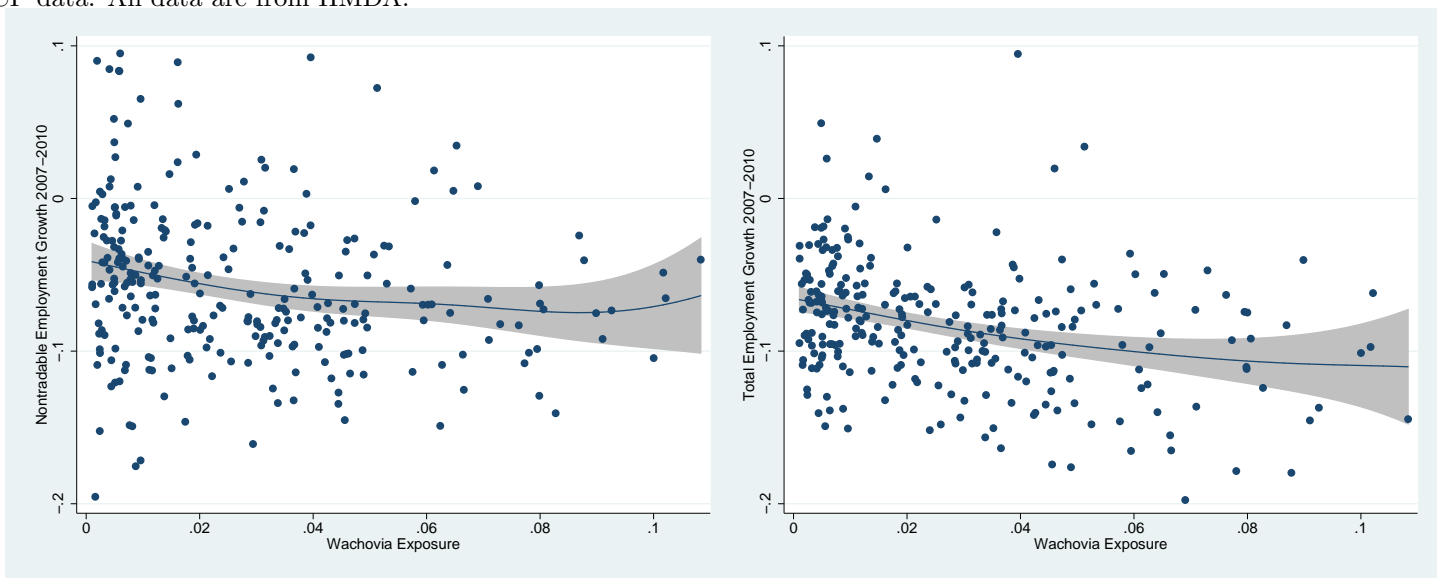


Figure 1.13: Household and Firm Credit from Wachovia and Market

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

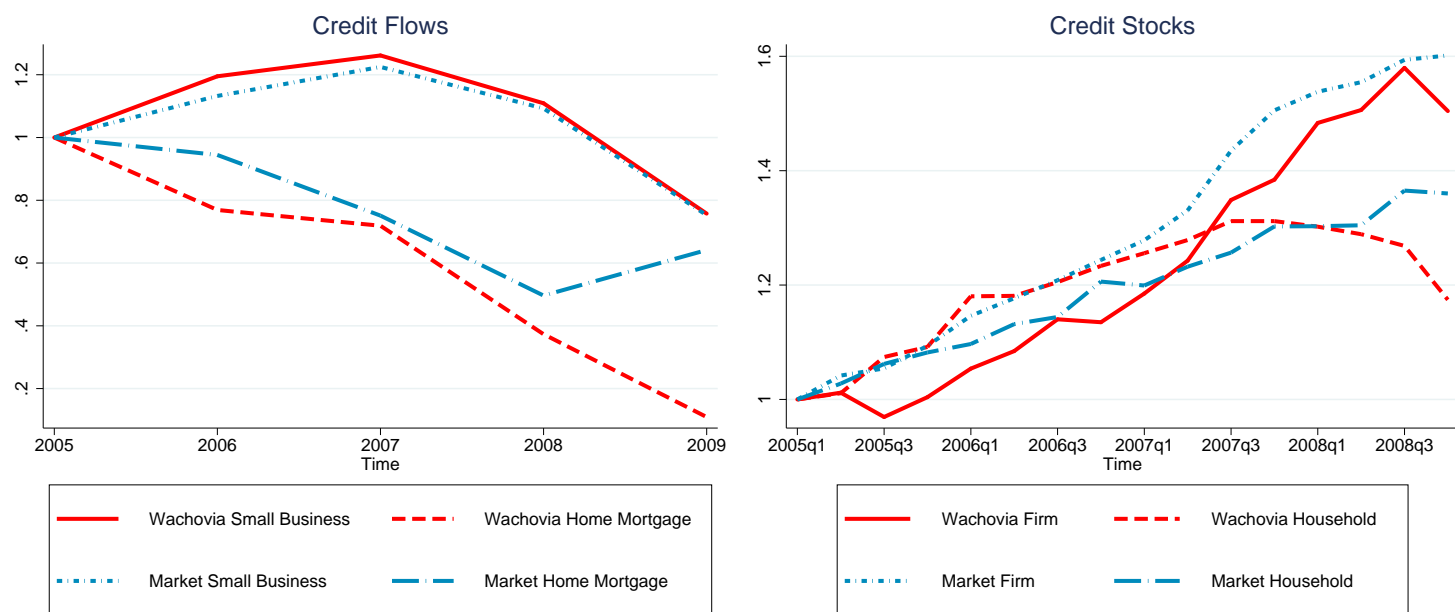


Table 1.1: Summary Statistics

*Note:* This table gives summary statistics for all 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. Estimates are weighted using county population in 2006.

	mean	sd	count
Total Employment 2007-2010	-0.07	0.05	779
Construction Employment 2007-2010	-0.20	0.13	779
Non-tradable Employment 2007-2010	-0.03	0.06	779
Tradable Employment 2007-2010	-0.16	0.13	779
Construction Share 2006	0.12	0.04	779
Non-tradable Share 2006	0.17	0.04	779
Tradable Share 2006	0.10	0.06	779
Home Purchase Credit 2007-2010	-0.46	0.12	779
Refinance Credit 2007-2010	0.19	0.57	771
Small Business Credit 2007-2010	-0.46	0.13	779
Home Purchase Credit 2002-2006	0.62	0.52	779
Refinance Credit 2002-2006	-0.16	0.50	779
Mortgage Leverage 2002-2006	0.44	0.21	779
Mortgage Leverage 2006	1.32	0.49	779
House Prices 2002-2006	0.45	0.30	579
House Prices 2007-2010	-0.18	0.14	604
Wachovia Home Purchase Share 2002-2006	0.01	0.02	779
Wachovia Refinance Share 2002-2006	0.02	0.02	779
Wachovia Average Share 2002-2006	0.02	0.02	779

Table 1.2: Summary Statistics - East and South

*Note:* This table gives summary statistics for counties in the East and South 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. Estimates are weighted using county population in 2006.

	mean	sd	count
Total Employment 2007-2010	-0.06	0.05	478
Construction Employment 2007-2010	-0.17	0.13	478
Non-tradable Employment 2007-2010	-0.02	0.06	478
Tradable Employment 2007-2010	-0.16	0.14	478
Construction Share 2006	0.11	0.04	478
Non-tradable Share 2006	0.18	0.04	478
Tradable Share 2006	0.09	0.06	478
Home Purchase Credit 2007-2010	-0.46	0.14	478
Refinance Credit 2007-2010	0.21	0.58	477
Small Business Credit 2007-2010	-0.50	0.11	478
Home Purchase Credit 2002-2006	0.67	0.53	478
Refinance Credit 2002-2006	-0.13	0.51	478
Mortgage Leverage 2002-2006	0.41	0.20	478
Mortgage Leverage 2006	1.14	0.35	478
House Prices 2002-2006	0.44	0.27	342
House Prices 2007-2010	-0.14	0.13	360
Wachovia Home Purchase Share 2002-2006	0.02	0.02	478
Wachovia Refinance Share 2002-2006	0.03	0.03	478
Wachovia Average Share 2002-2006	0.03	0.02	478

Table 1.3: Joint Correlation Between Wachovia Exposure and Observables

*Note:* This table reports OLS estimates of Wachovia exposure jointly regressed on various county observables:  $\text{Wachovia Exposure}_i = \alpha + \sum_j \beta_j X_{ji} + \epsilon_i$ . The regressions show there is no systematic relationship between exposure to Wachovia and many of the pre-crisis observables linked to the housing boom and bust. I measure exposure with Wachovia's average market share in 2005-2006 of home purchase and refinance mortgages within the county. The only robust relationship is the negative association with the share of lending done by HUD-regulated lenders. See the paper for data sources and definitions. 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. Estimates are weighted using county population in 2006.

	(1) $\beta$ / p/(CI)	(2) $\beta$ / p/(CI)	(3) $\beta$ / p/(CI)	(4) $\beta$ / p/(CI)
Non-tradable Employment 2002-2006	0.026 0.020 (0.004, 0.049)	0.011 0.246 (-0.009, 0.030)	0.037 0.034 (0.003, 0.070)	0.010 0.114 (-0.002, 0.022)
Construction Employment 2002-2006	-0.000 0.208 (-0.004, 0.003)	0.003 0.022 (0.001, 0.006)	-0.000 0.340 (-0.004, 0.004)	0.002 0.086 (-0.001, 0.005)
Mortgage Leverage 2006	0.012 0.004 (0.004, 0.020)	-0.004 0.784 (-0.025, 0.017)	0.011 0.086 (-0.001, 0.023)	-0.007 0.714 (-0.037, 0.023)
Construction Share 2006	0.017 0.746 (-0.072, 0.106)	-0.074 0.178 (-0.200, 0.052)	-0.030 0.982 (-0.131, 0.072)	-0.110 0.134 (-0.252, 0.033)
HUD-regulated Share 2002-2006	-0.088 0.002 (-0.143, -0.032)	-0.032 0.002 (-0.143, -0.032)	-0.119 0.004 (-0.185, -0.054)	-0.058 0.004 (-0.185, -0.054)
Population 2002-2006	-0.013 0.956 (-0.065, 0.038)	-0.010 0.956 (-0.065, 0.038)	-0.003 0.174 (-0.075, 0.068)	0.021 0.174 (-0.075, 0.068)
Home Purchase Credit 2002-2006	-0.001 0.620 (-0.006, 0.004)	-0.002 0.328 (-0.005, 0.001)	0.001 0.554 (-0.006, 0.008)	-0.005 0.024 (-0.009, -0.001)
House Prices 2002-2006			-0.005 0.916 (-0.023, -0.023)	0.015 0.778 (-0.040, -0.040)
FE	—	State	—	State
N	478	478	342	342
Clusters	25	25	23	23
R2	0.072	0.680	0.083	0.698
F-stat	4.070	9.690	1.864	14.506

Table 1.4: Effect of Exposure to Wachovia on Home Purchase and Refinance Credit Growth 2007-2010

*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:  $\widehat{L}_i = \alpha + \beta \text{Wachovia Exposure}_i + \theta X_i + \epsilon_i$ . I measure exposure with Wachovia's average market share in 2005-2006 to home purchase and refinance mortgages within the county. Exposure to Wachovia had a large and robust effect on both types of household credit growth across counties. The 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 and to a lesser extent state fixed effects. 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. Estimates are weighted using county population in 2006.

	(1) Purchase $\beta$ /p/(CI)	(2) Purchase $\beta$ /p/(CI)	(3) Purchase $\beta$ /p/(CI)	(4) Refinance $\beta$ /p/(CI)	(5) Refinance $\beta$ /p/(CI)	(6) Refinance $\beta$ /p/(CI)
Wachovia Average Share	-2.357 0.000 (-2.925, -1.789)	-2.168 0.000 (-2.741, -1.594)	-1.621 0.230 (-5.175, 1.933)	-3.732 0.048 (-7.361, -0.104)	-2.066 0.320 (-5.467, 1.335)	-2.615 0.738 (-10.470, 5.240)
Mortgage Leverage 2006		-0.092 0.078 (-0.192, 0.008)	-0.026 0.896 (-0.130, 0.077)		-0.577 0.004 (-0.983, -0.170)	0.151 0.898 (-0.465, 0.767)
Construction Share 2006		-0.131 0.994 (-0.588, 0.327)	0.014 0.226 (-0.291, 0.318)		-0.708 0.846 (-2.646, 1.230)	-0.590 0.736 (-2.660, 1.481)
HUD-regulated Share 2006		-0.401 0.006 (-0.693, -0.108)	-0.458 0.000 (-0.691, -0.224)		0.490 0.956 (-1.116, 2.096)	-1.859 0.020 (-3.324, -0.395)
Nontradable Employment 2002-2006		-0.200 0.026 (-0.370, -0.030)	0.013 0.418 (-0.116, 0.142)		-0.188 0.790 (-1.246, 0.869)	0.504 0.002 (0.168, 0.839)
FE	-	-	State	-	-	State
N	478	478	478	477	477	477
Clusters	25	25	25	25	25	25
R2	0.154	0.311	0.626	0.021	0.147	0.603
Robust F-stat	32.667	12.586	13.127	0.952	1.655	5.988



Table 1.5: Effect of Wachovia Exposure on Retail Expenditures, House Sales, and House Prices 2007-2010

*Note:* This table reports regression point estimates, p-values, and 95% confidence intervals of the relationship between exposure to Wachovia and retail expenditure, house sales, and house price growth at the county level:  $\text{Outcome}_i = \alpha + \beta \text{Wachovia Exposure}_i + \theta X_i + \epsilon_i$ . I measure exposure with Wachovia's average market share in 2005-2006 to home purchase and refinance mortgages within the county. Counties exposed to Wachovia experienced larger declines in retail sales and house sales growth and this result is very robust to using only within state variation. House prices also appear to decline in response to Wachovia exposure, but only when then the horizon is extended to 2012. 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. Estimates are weighted using county population in 2006.

	(1) Retail $\beta$ / p/(CI)	(2) Retail $\beta$ / p/(CI)	(3) Home Sales $\beta$ / p/(CI)	(4) Home Sales $\beta$ / p/(CI)	(5) HPI 2007-2010 $\beta$ / p/(CI)	(6) HPI 2007-2012 $\beta$ / p/(CI)	(7) HPI 2007-2012 $\beta$ / p/(CI)
Wachovia Average Share	-0.747 0.000 (-1.052, -0.443)	-0.983 0.000 (-1.390, -0.576)	-1.877 0.000 (-2.754, -0.999)	-2.143 0.000 (-3.589, -0.697)	-0.373 0.272 (-0.989, 0.243)	-1.209 0.006 (-2.164, -0.434)	-1.299 0.382 (-4.471, 1.873)
Mortgage Leverage 2006	-0.041 0.000 (-0.063, -0.018)	-0.029 0.326 (-0.084, 0.026)	0.167 0.044 (0.010, 0.323)	0.067 0.642 (-0.133, 0.266)	-0.252 0.000 (-0.332, -0.173)	-0.253 0.002 (-0.200, -0.067)	-0.134 0.074 (-0.279, 0.012)
FE	—	State	—	State	—	—	State
N	471	471	356	356	360	360	360
Clusters	25	25	23	23	23	23	23
R2	0.085	0.211	0.126	0.494	0.435	0.397	0.706
F-stat	18.382	15.020	6.227	9.071	7.107	11.366	6.596

Table 1.6: Effect of Wachovia Exposure on Nontradable Employment and Payroll 2007-2010

*Note:* This table reports regression point estimates, p-values, and 95% confidence intervals of the relationship between exposure to Wachovia and nontradable employment and payroll growth at the county level:  $\hat{E}_i = \alpha + \beta \text{Wachovia Exposure}_i + \theta X_i + \epsilon_i$ . Unless otherwise noted the outcomes are growth rates from 2007 to 2010. I measure exposure with Wachovia's market share in 2005-2006 of home purchase and refinance mortgages within the county. Counties exposed to Wachovia experienced larger declines in nontradable employment and payroll growth. This result is robust to state fixed effects, although some precision is lost. See the text for variable construction. 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. Estimates are weighted using county population in 2006.

	(1) Employment $\beta$ / p/(CI)	(2) Employment $\beta$ / p/(CI)	(3) Payrolls $\beta$ / p/(CI)	(4) Payrolls $\beta$ / p/(CI)
Wachovia Exposure	-0.696 0.000 (-1.030, -0.361)	-0.437 0.138 (-1.069, 0.196)	-0.684 0.004 (-1.166, -0.203)	-0.546 0.042 (-1.073, -0.020)
Mortgage Leverage 2006	-0.032 0.006 (-0.049, -0.016)	0.015 0.264 (-0.014, 0.045)	-0.040 0.006 (-0.070, -0.010)	-0.036 0.436 (-0.116, 0.043)
FE	—	State	—	State
N	478	478	478	478
Clusters	25	25	25	25
R2	0.110	0.312	0.046	0.124
F-stat	7.828	3.640	9.854	4.572

Table 1.7: Effect of Wachovia Exposure on Construction Employment and Payroll 2007-2010

*Note:* This table reports regression point estimates, p-values, and 95% confidence intervals of the relationship between exposure to Wachovia and construction employment and payroll growth at the county level:  $\hat{E}_i = \alpha + \beta \text{Wachovia Exposure}_i + \theta X_i + \epsilon_i$ . Unless otherwise noted the outcomes are growth rates from 2007 to 2010. I measure exposure with Wachovia's market share in 2005-2006 of home purchase and refinance mortgages within the county. Counties exposed to Wachovia experienced appear to experience larger declines in construction employment and payroll growth, but the estimates are reduced significantly when including state fixed effects. See the text for variable construction. 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. Estimates are weighted using county population in 2006.

	(1) Employment $\beta$ / p/(CI)	(2) Employment $\beta$ / p/(CI)	(3) Payrolls $\beta$ / p/(CI)	(4) Payrolls $\beta$ / p/(CI)
Wachovia Exposure	-1.509 0.000 (-2.019, -0.999)	-0.372 0.696 (-2.011, 1.267)	-2.086 0.004 (-1.166, -0.203)	-0.531 0.042 (-1.073, -0.020)
Mortgage Leverage 2006	-0.170 0.000 (-0.220, -0.120)	-0.076 0.006 (-0.124, -0.028)	-0.186 0.006 (-0.070, -0.010)	-0.085 0.436 (-0.116, 0.043)
FE	—	State	—	State
N	478	478	478	478
Clusters	25	25	25	25
R2	0.292	0.475	0.254	0.476
F-stat	17.530	6.837	19.085	7.999

Table 1.8: Effect of Wachovia Exposure on Nonresidential Construction Employment and Payroll 2007-2010

*Note:* This table reports regression point estimates, p-values, and 95% confidence intervals of the relationship between exposure to Wachovia and nonresidential construction employment and payroll growth at the county level:  $\hat{E}_i = \alpha + \beta \text{Wachovia Exposure}_i + \theta X_i + \epsilon_i$ . Unless otherwise noted the outcomes are growth rates from 2007 to 2010. I measure exposure with Wachovia's market share in 2005-2006 of home purchase and refinance mortgages within the county. This table shows that once state fixed effects are taken into account there is essentially no relationship between employment and payroll growth in nonresidential construction and exposure to Wachovia. 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. Estimates are weighted using county population in 2006.

	(1) Employment $\beta$ /p/(CI)	(2) Employment $\beta$ /p/(CI)	(3) Payrolls $\beta$ /p/(CI)	(4) Payrolls $\beta$ /p/(CI)
Wachovia Exposure	-1.446 0.000 (-2.044, -0.848)	-0.078 0.180 (-2.875, 2.718)	-2.197 0.000 (-3.030, -1.365)	-0.149 0.276 (-3.758, 3.460)
Mortgage Leverage 2006	-0.142 0.000 (-0.206, -0.078)	-0.037 0.160 (-0.085, 0.012)	-0.138 0.004 (-0.234, -0.043)	0.009 0.624 (-0.044, 0.062)
FE	—	State	—	State
N	474	474	473	473
Clusters	25	25	25	25
R <sup>2</sup>	0.183	0.414	0.118	0.346
F-stat	10.942	2.270	12.782	0.064

Table 1.9: Effect of Wachovia Exposure on Residential Construction Employment and Payroll 2007-2010

*Note:* This table reports regression point estimates, p-values, and 95% confidence intervals of the relationship between exposure to Wachovia and nonresidential construction employment and payroll growth at the county level:  $\hat{E}_i = \alpha + \beta \text{Wachovia Exposure}_i + \theta X_i + \epsilon_i$ . Unless otherwise noted the outcomes are growth rates from 2007 to 2010. I measure exposure with Wachovia's market share in 2005-2006 of home purchase and refinance mortgages within the county. This table shows that once state fixed effects are taken into account there is essentially no relationship between employment and payroll growth in nonresidential construction and exposure to Wachovia. 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. Estimates are weighted using county population in 2006.

	(1) Employment $\beta$ / p/(CI)	(2) Employment $\beta$ / p/(CI)	(3) Payrolls $\beta$ / p/(CI)	(4) Payrolls $\beta$ / p/(CI)
Wachovia Exposure	-1.797 0.000 (-2.273, -1.321)	-0.697 0.020 (-1.283, -0.110)	-2.295 0.000 (-2.860, -1.730)	-0.811 0.240 (-2.202, 0.580)
Mortgage Leverage 2006	-0.177 0.000 (-0.221, -0.134)	-0.090 0.014 (-0.156, -0.025)	-0.208 0.000 (-0.256, -0.160)	-0.128 0.004 (-0.200, -0.056)
FE	—	State	—	State
N	477	477	478	478
Clusters	25	25	25	25
R2	0.409	0.588	0.405	0.572
F-stat	21.480	8.013	27.695	8.166

Table 1.10: Effect of Wachovia Exposure on Tradables Employment and Payroll 2007-2010

*Note:* This table reports regression point estimates, p-values, and 95% confidence intervals of the relationship between exposure to Wachovia and nonresidential construction employment and payroll growth at the county level:  $\hat{E}_i = \alpha + \beta \text{Wachovia Exposure}_i + \theta X_i + \epsilon_i$ . Unless otherwise noted the outcomes are growth rates from 2007 to 2010. I measure exposure with Wachovia's market share in 2005-2006 of home purchase and refinance mortgages within the county. This table shows that once state fixed effects are taken into account there is essentially no relationship between employment and payroll growth in nonresidential construction and exposure to Wachovia. 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. Estimates are weighted using county population in 2006.

	(1) Employment $\beta$ /p/(CI)	(2) Employment $\beta$ /p/(CI)	(3) Payrolls $\beta$ /p/(CI)	(4) Payrolls $\beta$ /p/(CI)
Wachovia Average Share	-0.317 0.464 (-0.948, 0.315)	-0.865 0.450 (-3.499, 1.770)	-0.939 0.094 (-2.065, 0.187)	0.642 0.856 (-3.721, 5.005)
Mortgage Leverage 2006	-0.011 0.676 (-0.081, 0.060)	0.001 0.082 (-0.091, 0.094)	-0.124 0.026 (-0.236, -0.012)	-0.032 0.850 (-0.211, 0.146)
FE	—	State	—	State
N	478	478	450	450
Clusters	25	25	25	25
R <sup>2</sup>	0.004	0.129	0.031	0.110
F-stat	0.324	1.126	2.693	0.226

Table 1.11: Effect of Wachovia Exposure on Employment and Payroll Excluding Tradables 2007-2010

*Note:* This table reports regression point estimates, p-values, and 95% confidence intervals of the relationship between exposure to Wachovia and nonresidential construction employment and payroll growth at the county level:  $\hat{E}_i = \alpha + \beta \text{Wachovia Exposure}_i + \theta X_i + \epsilon_i$ . Unless otherwise noted the outcomes are growth rates from 2007 to 2010. I measure exposure with Wachovia's market share in 2005-2006 of home purchase and refinance mortgages within the county. This table shows that once state fixed effects are taken into account there is essentially no relationship between employment and payroll growth in nonresidential construction and exposure to Wachovia. 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. Estimates are weighted using county population in 2006.

	(1) Employment $\beta$ / p/(CI)	(2) Employment $\beta$ / p/(CI)	(3) Payrolls $\beta$ / p/(CI)	(4) Payrolls $\beta$ / p/(CI)
Wachovia Average Share	-0.631 0.000 (-0.880, -0.382)	-0.295 0.104 (-0.642, 0.051)	-1.041 0.000 (-1.366, -0.715)	-0.354 0.534 (-1.244, 0.537)
Mortgage Leverage 2006	-0.045 0.000 (-0.062, -0.028)	-0.003 0.534 (-0.032, 0.026)	-0.061 0.000 (-0.088, -0.033)	-0.023 0.106 (-0.050, 0.004)
FE	—	State	—	State
N	478	478	478	478
Clusters	25	25	25	25
R <sup>2</sup>	0.201	0.350	0.161	0.293
F-stat	16.380	1.784	32.580	2.028

Table 1.12: Effect of Wachovia Exposure on Total Employment and Payroll 2007-2010

*Note:* This table reports regression point estimates, p-values, and 95% confidence intervals of the relationship between exposure to Wachovia and nonresidential construction employment and payroll growth at the county level:  $\hat{E}_i = \alpha + \beta \text{Wachovia Exposure}_i + \theta X_i + \epsilon_i$ . Unless otherwise noted the outcomes are growth rates from 2007 to 2010. I measure exposure with Wachovia's market share in 2005-2006 of home purchase and refinance mortgages within the county. This table shows that once state fixed effects are taken into account there is essentially no relationship between employment and payroll growth in nonresidential construction and exposure to Wachovia. 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. Estimates are weighted using county population in 2006.

	(1) Employment $\beta$ / p/(CI)	(2) Employment $\beta$ / p/(CI)	(3) Payrolls $\beta$ / p/(CI)	(4) Payrolls $\beta$ / p/(CI)
Wachovia Average Share	-0.669 0.000 (-0.896, -0.441)	-0.334 0.204 (-0.838, 0.169)	-1.003 0.000 (-1.344, -0.662)	-0.288 0.598 (-1.183, 0.608)
Mortgage Leverage 2006	-0.037 0.000 (-0.054, -0.019)	0.001 0.232 (-0.020, 0.022)	-0.063 0.002 (-0.092, -0.035)	-0.024 0.056 (-0.049, 0.001)
FE	—	State	—	State
N	478	478	478	478
Clusters	25	25	25	25
R <sup>2</sup>	0.193	0.351	0.160	0.291
F-stat	13.123	2.110	28.899	2.561



Table 1.13: Wachovia Exposure in the Household and Firm Credit Market 2007-2010

*Note:* This table reports regression point estimates, p-values, and 95% confidence intervals of the relationship between types of exposure to Wachovia and types of credit growth at the county level:  $\hat{L}_i = \alpha + \beta \text{Wachovia Exposure}_i + \theta X_i + \epsilon_i$ . This table shows that exposure to Wachovia in the small business credit market has no predictive power of declines in household credit. Exposure to Wachovia in both household and firm credit causes declines in firm credit. Wachovia Exposure is measured as Wachovia's market share in the home purchase and refinance markets. High Wachovia (CRA) and High Wachovia (HMDA) are indicators for whether or not a county is in the top half of exposure. Alternative specifications give similar results. All specifications include state fixed effects. Unless otherwise noted the outcomes are growth rates from 2007 to 2010. I measure exposure in several ways. 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. Estimates are weighted using county population in 2006.

	(1) Home Purchase $\beta$ / p/(CI)	(2) Home Purchase $\beta$ / p/(CI)	(3) Firm Credit $\beta$ / p/(CI)	(4) Firm Credit $\beta$ / p/(CI)	(5) Firm Credit $\beta$ / p/(CI)
Wachovia Exposure	-1.667 0.204 (-4.675, 1.342)			-1.173 0.140 (-2.751, 0.406)	
Mortgage Leverage 2006	-0.060 0.228 (-0.170, 0.050)	-0.066 0.074 (-0.142, 0.009)	-0.076 0.508 (-0.375, 0.182)	-0.087 0.048 (-0.172, -0.001)	-0.089 0.004 (-0.156, -0.022)
High Wachovia (CRA)	-0.002 0.184 (-0.063, 0.058)	0.023 0.412 (-0.022, 0.068)	-0.097 0.508 (-0.375, 0.182)	-0.064 0.312 (-0.203, 0.075)	-0.050 0.382 (-0.166, 0.066)
High Wachovia (HMDA)		-0.110 0.022 (-0.191, -0.029)			-0.071 0.120 (-0.162, 0.019)
FE	State	State	State	State	State
N	484	486	480	478	480
Clusters	25	25	25	25	25
R2	0.605	0.619	0.544	0.555	0.562
F-stat	3.433	18.089	5.604	5.466	5.790

Table 1.14: Which Type of Credit Matters for Employment Growth 2007-2010

*Note:* This table reports point estimates, p-values, and 95% confidence intervals of the relationship between types of exposure to Wachovia and employment growth at the county level:  $\hat{E}_i = \alpha + \beta \text{Wachovia Exposure}_i + \theta X_i + \epsilon_i$ . Unless otherwise noted the outcomes are growth rates from 2007 to 2010. I measure Wachovia Exposure with Wachovia's market share in 2005-2006 of home purchase and refinance mortgages within the county. Firm exposure is Wachovia's share of the small business lending market. High Wachovia (CRA) and High Wachovia HMDA are indicators for a county having above-median exposure to Wachovia in that market. The sample is restricted to all counties in the South and East with CCP controls and at least 50,000 residents. Pair bootstrap-t used to construct symmetric p-values and 95% confidence intervals clustered at the state level. Estimates are weighted using county population in 2006.

	(1) Nontradables $\beta$ / p/(CI)	(2) Nontradables $\beta$ / p/(CI)	(3) Total $\beta$ / p/(CI)	(4) Total $\beta$ / p/(CI)
Wachovia Average Share	-0.419 0.152 (-1.017, 0.179)		-0.264 0.466 (-0.981, 0.454)	
Mortgage Leverage 2006	0.016 0.220 (-0.009, 0.041)	0.017 0.150 (-0.008, 0.042)	0.003 0.564 (-0.018, 0.023)	0.003 0.548 (-0.017, 0.022)
High Wachovia (CRA)	-0.002 0.330 (-0.023, 0.020)	-0.001 0.204 (-0.019, 0.017)	-0.007 0.640 (-0.024, 0.011)	-0.002 0.426 (-0.023, 0.019)
High Wachovia (HMDA)		-0.020 0.246 (-0.062, 0.022)		-0.020 0.120 (-0.046, 0.007)
FE	State	State	State	State
N	478	478	478	478
Clusters	25	25	25	25
R2	0.312	0.311	0.352	0.356
F-stat	2.797	2.933	1.858	2.338

Table 1.15: Wachovia Exposure and Establishment Growth by Size 2007-2010

*Note:* This table reports point estimates, p-values, and 95% confidence intervals of the relationship between exposure to Wachovia and establishment growth in different size categories at the county level:  $\hat{E}_i = \alpha + \beta \text{Wachovia Exposure}_i + \theta X_i + \epsilon_i$ . Unless otherwise noted the outcomes are growth rates from 2007 to 2010. I measure Wachovia Exposure with Wachovia's market share in 2005-2006 of home purchase and refinance mortgages within the county. 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. Estimates are weighted using county population in 2006.

	(1) 1-4 $\beta$ /p/(CI)	(2) 5-9 $\beta$ /p/(CI)	(3) 10-19 $\beta$ /p/(CI)	(4) 20-49 $\beta$ /p/(CI)	(5) 50+ $\beta$ /p/(CI)
Wachovia Average Share	-0.433 0.000 (-0.711, -0.156)	-0.527 0.000 (-0.820, -0.235)	-0.514 0.000 (-0.744, -0.285)	-0.703 0.000 (-0.958, -0.448)	-0.756 0.000 (-0.991, -0.520)
Mortgage Leverage 2006	-0.013 0.082 (-0.028, 0.002)	-0.037 0.000 (-0.053, -0.020)	-0.042 0.000 (-0.058, -0.026)	-0.046 0.000 (-0.065, -0.028)	-0.052 0.000 (-0.071, -0.034)
N	486	486	486	486	486
Clusters	25	25	25	25	25
R2	0.085	0.145	0.148	0.192	0.200
Robust F-stat	3.323	5.774	7.707	16.087	15.009

Table 1.16: Effect of Supply-driven Changes in Household Credit on Total Employment Growth 2007-2010

*Note:* This table reports point estimates, p-values, and 95% confidence intervals of the relationship between household credit growth and employment growth at the county level:  $\hat{E}_i = \alpha + \beta \hat{L}_i + \theta X_i + \epsilon_i$ . I report OLS and 2SLS estimates where I use exposure to Wachovia as an instrumental variable. Unless otherwise noted the outcomes are growth rates from 2007 to 2010. I measure exposure with Wachovia's market share in 2005-2006 of home purchase and refinance mortgages within the county. The sample is restricted to all counties in the South and East with CCP controls and at least 50,000 residents except when "Large" is indicated, when it is restricted to counties with at least 100,000 residents. Pair bootstrap-t used to construct symmetric p-values and 95% confidence intervals clustered at the state level. Estimates are weighted using county population in 2006.

	(1) OLS $\gamma$ /p/(CI)	(2) 2SLS $\gamma$ /p/(CI)	(3) 2SLS $\gamma$ /p/(CI)	(4) 2SLS-Large $\gamma$ /p/(CI)
Home Purchase Credit 2007-2010	0.129 0.000 (0.087, 0.172)	0.314 0.000 (0.211, 0.417)	0.254 0.090 (-0.068, 0.576)	0.323 0.104 (-0.114, 0.761)
FE	-	-	State	State
N	486	484	484	274
Clusters	25	25	25	25
Robust F-stat		29.799	3.523	4.268
Weak ID P-value		0.001	0.200	0.205

Table 1.17: Effect of Supply-driven Changes in Household Credit on Total Employment Growth 2007-2010, Heterogeneity

*Note:* This table reports point estimates, p-values, and 95% confidence intervals of the relationship between household credit growth and employment growth at the county level:  $\hat{E}_i = \alpha + \beta \hat{L}_i + \theta X_i + \epsilon_i$ . I report 2SLS estimates where I use exposure to Wachovia as an instrumental variable. Unless otherwise noted the outcomes are growth rates from 2007 to 2010. I measure exposure with Wachovia's market share in 2005-2006 of home purchase and refinance mortgages within the county. The sample is restricted to all counties in the South and East with CCP controls and at least 50,000 residents except when "Large" is indicated, when it is restricted to counties with at least 100,000 residents. Pair bootstrap-t used to construct symmetric p-values and 95% confidence intervals clustered at the state level. Estimates are weighted using county population in 2006

	(1) High Income $\beta$ / p/(CI)	(2) Low Income $\beta$ / p/(CI)	(3) High Leverage $\beta$ / p/(CI)	(4) Low Leverage $\beta$ / p/(CI)
Home Purchase Credit 2007-2010	0.289 0.000 (0.153, 0.425)	0.369 0.000 (0.150, 0.589)	0.324 0.036 (0.036, 0.613)	0.310 0.004 (0.135, 0.485)
N	243	241	243	241
Clusters	24	18	25	23
Robust F-stat	25.545	13.837	8.606	35.415
Weak ID P-value	0.005	0.003	0.015	0.006

## Chapter 2

# Accounting for Shocks to the Household Credit Market

### 2.1 Introduction

In the first chapter I provided evidence that shocks to household credit had large effects on household expenditures and employment. These estimates suggest that shocks to household credit could have played an important role in the employment losses following the financial crisis. However, a proper accounting requires not only the elasticity of employment with respect to these shocks, but also the size of the shock. In this chapter I provide an estimate of the size of the shock to the household credit market. 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 30 to 60% of the observed decline in employment within the estimation sample. As I explain more fully in Section 1.3, 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 chapter (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.

Relying on insights from index theory, I assume a flexible functional form for the price of household credit in a county. Doing so implies the functional form for the supply shock to household credit: the true supply shock to a county is the weighted average of the passed-through lender-specific cost shocks in that county. Constructing a measure of the shock then reduces to measuring these lender cost shocks. I build on the approach of Greenstone et al. (2012), and exploit the fact that many lenders operate in multiple counties, and

that the households in counties borrow from multiple lenders. Related strategies have also been employed by Amity 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. This approach allows me to use growth in lender-county credit flows to estimate a fixed effect for each lender and each county. A lender fixed effect is then the average change in credit flows for that lender across all borrower relationships, conditional on all other fixed effects. I expect this statistic to reflect whether or not a lender is expanding or contracting credit to borrowers.

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.<sup>1</sup> In these cases, it is generally unknown whether or not that specific proxy captures the majority of cost shocks affecting lenders. Therefore, the fixed effects provide a more complete measure of the cost shocks affecting each lender, which is critical for the purpose of accounting.

I construct the household credit supply shock to a county by weighting the lender fixed effects with each lender's market share in the county and then taking the sum. I show that the measured shock constructed in this way will only reflect the true supply-side shock to household credit plus measurement error. Thus, I have a measure of the shock to household credit that can be used for quantitative accounting given certain assumptions. I define the aggregate direct contribution of the household credit channel as the weighted sum of measured shocks across counties multiplied by the elasticity of employment with respect to the measured shock. This quantity gives the aggregate effect of credit supply shocks conditional on shutting down spillovers across counties. Because credit supply shocks are not randomly distributed across counties and because I am using a noisy measure of the true shock, direct estimates of the shock's effect will likely be inconsistent. I correct for these issues by again using exposure to Wachovia as an instrumental variable. Consistent with results above, my estimates show that shocks to household credit had significant effects on employment, and that direct estimates might be significantly biased downward. I construct a lower bound to the aggregate direct contribution to correct for measurement error. 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. Under certain assumptions, this allows me to get rid of measurement error in a way that will always understate the actual effect. Using instrumented estimates, supply shocks to

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<sup>1</sup>For example, papers using proxies for specific types of shocks are Cornett et al. (2011), Dagher and Kazimov (2012), Goetz and Gozzi (n.d.), and Ivashina and Scharfstein (2010).

household credit caused a decline in employment of at least 3.6% within the estimation sample of counties in the South and East (60% of the observed decline), and 4.5% nationally (64% of the observed decline). Using OLS estimates gives numbers roughly one-half the size. For comparison, Greenstone et al. (2012) calculate an upper bound of 16% for employment losses due to shocks to small business credit. However, Chodorow-Reich (2014) uses variation from the syndicated loan market and suggests that credit shocks to firms can account for more than 40% of the observed decline in employment. While all of these calculations are partial equilibrium, together they suggest that the financial crisis was a fundamental driver of the Great Recession, with shocks to household credit being particularly powerful.

In the next section I lay out the accounting and estimation exercises in more detail. Section 2.3 describes and constructs the measured shock, estimates the elasticity of employment with respect to this shock, and then performs the accounting exercises.

## 2.2 Measuring the Aggregate Direct Contribution

Recall the simple framework introduced in Chapter 1 where the economy is composed of  $I$  areas indexed by  $i$ . The log change in employment was given by the following equation

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

Given this, 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

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

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

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

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  $\beta_{ij}^{ES}$ , and the distribution of shocks. It is generally infeasible to recover the spillover effects because of the high dimension of the estimation problem ( $I - 1$  by  $I$  parameters when we often only have  $I$  or, in a panel,  $I$  by  $T$  observations) and uncertainty about the spillover structure.<sup>2</sup>

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<sup>2</sup>See Stumpner (2013) for recent work estimating the role of trade linkages across areas, one potential spillover effect, in the Great Recession.



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

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

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.

While  $\beta^{ES}/\beta^{LS}$  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

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

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

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

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

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

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

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

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

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

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

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 below 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. So 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.

## 2.3 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 1.3). Together, this elasticity and shock allow me to estimate the size of household credit channel's effect on aggregate employment.

## A Generic Model

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 1.5 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 lender-area prices  $r_{ij}$ , the area aggregate price  $r_i$ , and demand shifters  $d_{ij}$  (for example, wealth or tastes) as inputs. The function is allowed to differ across areas and lenders

$$L_{ij} = L_{ij}^D(r_{ij}, r_i, d_{ij}). \quad (2.1)$$

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.

**Credit Supply** Lenders are monopolists who solve separable, static problems in each area. 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 introducing this

dimension is likely to lead to misspecification. I express the problem in terms of the inverse demand functions  $r_{ij} = r_{ij}(L_{ij})$  implied by 2.1. The monopolist faces a linear cost function  $\sigma_{ij}(c_{ij})L_{ij}$  that is differentiable and takes the level of lending and cost-shifters  $c_{ij}$  as inputs. This gives the standard monopolist's problem

$$\max_{L_{ij}} r_{ij}(L_{ij})L_{ij} - \sigma_{ij}(c_{ij})L_{ij}. \quad (2.2)$$

Letting the price elasticity of demand be  $\epsilon_{ij}^{Lr}$  and assuming it is constant, we have the standard solution for the monopolist's price

$$r_{ij} = c_{ij} / \left( 1 + \frac{1}{\epsilon_{ij}^{Lr}} \right). \quad (2.3)$$

As is standard, the cost of borrowing is a markup over marginal cost determined by the price elasticity of demand.<sup>3</sup>

The demand functions 2.1 imply some function  $R_i$  that aggregates lender-area prices into an area-specific price  $r_i$

$$r_i = R_i(\{r_{ij}(c_{ij})\}). \quad (2.4)$$

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

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

Since 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.

**Equilibrium** It is useful to log-linearize the equations 2.3 and 2.1 around an arbitrary equilibrium point. This gives us (ignoring approximation error)

$$\hat{r}_{ij} = \epsilon_{ij}^{rc} \hat{c}_{ij}, \quad (2.5)$$

and from 2.1 we have

$$\hat{L}_{ij} = \epsilon_{ij}^{Lr} \hat{r}_{ij} + \epsilon_i^L \sum_k \tilde{\omega}_{ik} \hat{r}_{ik} + \epsilon_{ij}^{Ld} \hat{d}_{ij}, \quad (2.6)$$

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<sup>3</sup>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 done.

where  $\epsilon$  signifies the appropriate elasticity. Because the price elasticity of demand and cost functions are constant with respect to the quantity demanded, I can simply substitute for price  $\hat{r}_{ij}$  into the expression for  $\hat{L}_{ij}$  to get

$$\hat{L}_{ij} = \epsilon_{ij}^{Lr} \epsilon_{ij}^{rc} \hat{c}_{ij} + \epsilon_i^L \sum_k \tilde{\omega}_{ik} \epsilon_{ik}^{rc} \hat{c}_{ik} + \epsilon_{ij}^{Ld} \hat{d}_{ij}. \quad (2.7)$$

This equation describes how supply-side and demand-side shocks manifest themselves in quantity changes between lenders and areas. Intuitively, a lender's cost shock will affect the quantity borrowed directly due to the substitution effect but also indirectly through its effect on the aggregate price. I can define the true supply-side shock to credit for the area as the deviation in the area's price due to the lender cost shocks

$$\underbrace{\hat{S}_i}_{\text{true shock}} \equiv \frac{1}{2} \sum_j \tilde{\omega}_{ij} \epsilon_{ij}^{rc} \hat{c}_{ij} \quad (2.8)$$

If supply and demand shocks are positively correlated and are negative then the expectation across areas of lender-area credit flows will give a lower bound estimate of the lender's common shock

$$E_i(\hat{L}_{ij}) = E_i(\epsilon_{ij}^{Lr} \epsilon_{ij}^{rc} \hat{c}_{ij} + \epsilon_i^L \hat{S}_i + \epsilon_{ij}^{Ld} \hat{d}_{ij}) \leq E_i(\epsilon_{ij}^{Lr} \epsilon_{ij}^{rc} \hat{c}_{ij}).$$

That is, I will be attributing too much of the decline in credit to the lender as I will also be including the local supply shock and the common demand shock. However, this and related quantities can still be useful for accounting purposes, even if the quantities themselves are not perfect measures of the supply shock coming from each lender.

To see this, assume for the moment that the additional shocks beyond the lender-specific shock are mean zero for each lender: **(unconditional)**

$$E_i(\epsilon_i^L \hat{S}_i + \epsilon_{ij}^{Ld} \hat{d}_{ij}) = 0, \forall j. \quad (2.9)$$

Because this assumption is unconditional and because it requires the shocks be zero it is stronger than necessary. In words, I require that the expected demand shocks be mean 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. Next, label

$$\rho_j = E_i(\epsilon_{ij}^{Lr} \epsilon_{ij}^{rc} \hat{c}_{ij}).$$

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). However, 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  $\hat{d}$ . By construction it is also the case that the estimate is correlated with the true input to the supply shock

$$Cov_j(\rho_j, \epsilon_{ij}^c \hat{c}_{ij}) \neq 0, \forall i.$$

I replace the true inputs  $\beta_{ij}^r \hat{c}_{ij}$  with the estimated lender fixed effects to construct a measure of the true shock

$$\underbrace{s_i}_{\text{measured shock}} \equiv \frac{1}{2} \sum_j (\omega_{ij,t} + \omega_{ij,t-1}) \rho_j. \quad (2.10)$$

What is the relationship between the true shock 2.8 and my measure 2.10? Recall the measurement relationship from Section 1.3

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

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.<sup>4</sup>

**Proposition One** *The measured shock 2.10 can be decomposed into  $\pi \hat{S}_i + v_{si}$ , where  $\pi = E_j(E_i(\epsilon_{ij}^{Lr})) \neq 0$  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.2 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

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<sup>4</sup>The significance of selection is effectively a question about the value of the covariances

$$Cov_i(\omega_{ij,t} + \omega_{ij,t-1}, \beta_{ij}^r \epsilon_{ij}^c).$$

This covariance will plausibly be overstated by

$$Cov_i(\omega_{ij,t} + \omega_{ij,t-1}, \hat{L}_{ij})$$

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

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.

The above discussion shows that, given some rather extreme assumptions about the mean of demand and supply shocks, we can recover a measure of the level of lender shocks. However, the assumptions about the means are too strong to be reasonable (and they preclude many interesting situations). In the next section I present the weakest conditions such that I can bound the aggregate direct contribution, a quantity of direct interest.

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

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

$$\sup_{n \geq 1} \frac{nw_n}{W_n} < \infty \quad \text{and} \quad \sup_{n \geq 2} \sum_{i=1}^{n-1} \left( \frac{i|w_{i+1} - w_i|}{W_n} \right) < \infty. \quad (2.11)$$

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

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

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

**Proposition Two** *Assume (1) the true shock  $\hat{S}_i$  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 2.11, and (4)  $\exists k^* \in \mathbb{R}$  such that*

$$\text{sgn}\left(\sum_i \omega_i \hat{S}_i\right) = \text{sgn}\left(\sum_{j:s_j \geq k^*} \tilde{\omega}_j \hat{S}_j\right). \quad (2.12)$$

*Then the object*

$$\underline{\text{aggregate direct contribution}} \equiv \frac{\beta^{ES}}{\pi} \sum_i \omega_i (s_i - \sum_{j:s_j \geq k^*} \tilde{\omega}_j s_j) \quad (2.13)$$

*converges in probability to a lower bound to the true aggregate direct contribution in that the limit of the weighted difference is greater than the aggregate direct contribution if the*

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

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

$$\text{plim} \sum_i \omega_i v_{si} = \text{plim} \sum_{j:s_j \geq k} \tilde{\omega}_j v_{sj} = \mu_e. \quad (2.14)$$

Then I can construct the following object

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

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

Given this work, the critical assumption is that the measurement error is distributed iid and so has a constant mean. Given this, one can make a simple assumption about the mean of the shock and then take a difference appropriately. Consider again the measured shock

$$\rho_j = E_i(\epsilon_{ij}^{Lr} \epsilon_{ij}^{rc} \hat{c}_{ij} + \epsilon_i^L \hat{S}_i + \epsilon_{ij}^{Ld} \hat{d}_{ij}).$$

If the common component of demand shocks  $\hat{d}_{ij}$  is correlated with the common supply-component (for example, if contractionary lenders are more likely to be in areas with contractionary demand shocks) then the error will not be iid. Instead, the measured shock will be more negative (contractionary) in places with negative shocks. The implication of this is that the mean of the error will vary conditional on the observed value of the measured shock, which would invalidate the approach to constructing the lower bound described above. So it is critical to estimate the lender shocks in such a way that helps ensure the errors are distributed iid. Because of the obvious concern of correlation between supply shocks and demand shocks, a central objective should be to avoid conflating these shocks with the lender-specific shock.

One approach to avoid this concern that can be examined analytically is to use the lender's shock net of the area average under the assumption that the common area-specific



components net to zero

$$\begin{aligned} E_i(\widehat{L}_{ij} - E_j(\widehat{L}_{ij})) &= E_i((\epsilon_{ij}^{Lr} \epsilon_{ij}^{rc} \widehat{C}_{ij} + \epsilon_i^L \widehat{S}_i + \epsilon_{ij}^{Ld} \widehat{d}_{ij}) - (E_j(\epsilon_{ij}^{Lr} \epsilon_{ij}^{rc} \widehat{C}_{ij}) + \epsilon_i^L \widehat{S}_i + E_j(\epsilon_{ij}^{Ld} \widehat{d}_{ij}))) \\ &= E_i((\epsilon_{ij}^{Lr} \epsilon_{ij}^{rc} \widehat{C}_{ij}) - E_j(\epsilon_{ij}^{Lr} \epsilon_{ij}^{rc} \widehat{C}_{ij}) + \epsilon_{ij}^{Ld} \widehat{d}_{ij} - E_j(\epsilon_{ij}^{Ld} \widehat{d}_{ij})) \\ &= E_i((\epsilon_{ij}^{Lr} \epsilon_{ij}^{rc} \widehat{C}_{ij}) - E_j(\epsilon_{ij}^{Lr} \epsilon_{ij}^{rc} \widehat{C}_{ij})) \end{aligned}$$

This estimate it will misstate the actual shock (understate it if the average shock is negative or overstate it if the average shock is positive). What it provides is an estimate of the lender's contraction relative to the average of other lenders in all areas where the lender operates. Thus, the weighted average of these estimates will result in ranking areas as having contractionary shocks if they are exposed, on average, to lenders who are more contractionary than other lenders. Here we can ask does this estimation approach obviously lead to a non-iid error structure if there is a correlation between demand and supply shocks. Interestingly, the error structure likely ends up being non-iid but in a predictable manner. To see this note that

$$Cov_i(\epsilon_i^L S_i, \sum_j \omega_{ij} E_i(E_j(\epsilon_{ij}^{Lr} \epsilon_{ij}^{rc} \widehat{C}_{ij}))) > 0.$$

That is, places with contractionary shocks are likely to have more contractionary lenders, on average. Notice that this is not necessarily the case since the contractionary shock could be arising to an exceptionally high weight. However, given that most weights are relatively low in my example, this is unlikely. In this case the error would actually be negatively correlated with the true shock and so compress the distribution. Under these conditions, the claim that the procedure above produces a lower bound would still be valid, although the bound created would be even more extreme.

Along these same lines, Greenstone et al. (2012) propose regressing changes in credit quantities between areas  $i$  and lenders  $j$  on a full set of lender and area fixed effects, represented here as coefficients on the dummy variables  $D_i$  (for an area  $i$ ) and  $\Lambda_j$  (for a lender  $j$ )

$$\widehat{L}_{ij} = \alpha_i D_i + \rho_j \Lambda_j + e_{ij}. \quad (2.15)$$

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. Amiti and Weinstein (2013) use essentially the same approach to identification and examine some of their largest lender fixed effects from Japanese credit markets in detail. The variety of shocks driving the fixed effects demonstrates the power of the methodology. Some shocks are related to the announcement by a Japanese regulator of illegal actions by several financial institutions. Several others were related to computer or "fat finger" errors, a memorable case being the trader who mistakenly switched the intended price (610,000) with the intended quantity (one) of his trade. The breadth of shocks recovered suggests the non-parametric structure of this approach is useful in constructing a general measure of supply-side shocks.

Unfortunately, it is impossible to express the solution to the normal equations solved when estimating Equation 2.15, see McCaffrey et al. (2010) and Mihaly et al. (2010) for more details and a discussion of estimation routines in this context. However, the condition necessary to impose is that, conditional on the common borrower fixed effect, the correlation between the measured shock's error and the true shock is at least not positive. Under this condition, which I will explore in later work through monte carlo experiments, it is always possible to construct a lower bound to the aggregate direct contribution. To summarize, we are able to recover a measure of the lender's average cost shock that is non-parametric and agnostic about the source of the shock.

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 that even if this is the case, I can still construct a lower bound to the aggregate direct contribution of the household credit channel. I now implement this procedure.

## 2.4 Estimates of Lender Shocks

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 give very similar results. I drop extreme outliers, but my results are robust to winsorizing or not adjusting these observations, although it makes some difference with respect to precision. 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.<sup>5</sup> The estimation sample also represents about 51% of all assets reported on call reports in 2006. Table 2.1 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

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<sup>5</sup>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.

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 2.9, or that the fixed effects do not reflect local demand or supply shocks. As an intuitive check of this condition I estimate 2.15 with two levels of fixed effects: state and county.<sup>6</sup> 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 2.4 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 one of the most negative fixed effects.

## 2.5 Measured Area 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 2.10. The theoretical expenditure share required by the price index for household credit is a conceptually and practically difficult quantity. 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. 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 2.2 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 1.3 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

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<sup>6</sup>To speed estimation of the fixed effects I use the code *reg2hdfe* provided by Guimaraes and Portugal (2010).

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 2.4 and Section 1.5, 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 2.2 shows that exposure to Wachovia is robustly associated with the measured shock. The point estimate is very stable across specifications and not driven by outliers (column three) or state-level shocks (column four). The validity of the instrument hinges on Wachovia only affecting the measured shock through Wachovia's own shock. This implies that I should find no association between Wachovia exposure and the measured shock excluding Wachovia's fixed effect from the measured shock. If I do find a negative relationship it would suggest that exposure to Wachovia is correlated with exposure to other lenders who are transmitting contractionary shocks. This would lead to concern about the validity of the exclusion restriction, although it would not necessarily invalidate it. Column six excludes Wachovia's fixed effect from the measured shock and show that there is essentially no relationship with exposure to Wachovia. This also tells me that exposure to Wachovia is not correlated with measurement error, again validating the instrument. Figure 2.3 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 2.3.<sup>7</sup> First, the OLS estimate (unreported) suggests the measured shock has an effect on employment, but it is not significantly different from zero. The WLS estimate is much more precise and recovers a similar estimate: a one percentage point in the measured shock implies a decline in employment of about .6%. However, the instrumented estimates show that the direct estimate is significantly biased downward. Column two gives an estimate of 1.4%, while controlling for industry shocks and mortgage leverage in column four suggests the effect on employment is about 1.1%, still twice as large as the direct estimates. Recall that  $\pi$ , 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 the same estimate.

## Aggregate Direct Contribution

With estimates of  $\beta^{ES}/\pi$ , I showed above that I can calculate a lower bound to the aggregate direct contribution of shocks to household credit with the following sum (also see Section

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<sup>7</sup>I correct for the generated regressor with the clustered pairs bootstrap as before.

1.3)

$$\underline{\text{aggregate direct contribution}} = \frac{\beta^{ES}}{\pi} \sum_i \omega_i (s_i - \sum_{j:s_j \geq k^*} \tilde{\omega}_j s_j). \quad (2.16)$$

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 2.4 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 2.3, to arrive at the effect on total employment. The second column uses the coefficient for employment excluding tradables from column four of Table 2.3. 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 2.16. The third row does the same within the national sample of counties. The first column shows that shocks to household credit account for at least 60 to 62% of the observed declines in employment (3.6% decline in the subsample and 4.5% nationally). The second column shows that limiting this calculation to effects on just non-tradable (in other words, excluding tradables) industries gives very similar magnitudes. If I am even more conservative and set  $k^*$  equal to the 66th percentile the lower bound falls to 52/53% in the first column.

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

For comparison, Greenstone et al. (2012) assume all of the decline in small business credit was due to supply, which captures at most 16% of the decline in total employment. Chodorow-Reich (2014) performs a similar accounting exercise for the contribution of firm credit shocks on employment from 2007-2009 and calculates a lower bound for the aggregate direct contribution between 34 and 47% of 7%. Together with my calculation, these numbers

suggest the crisis-induced contraction in credit was responsible for almost all of the decline in employment. In contrast to both of these accounts, Mian and Sufi (2014) suggest that over 50% of the decline in overall employment is due to the shock to household net worth. But this calculation is made by assuming most of the change in household net worth is exogenous, whereas much of this decline is likely the result of credit supply shocks. This suggests their calculation overstates the household net worth channel. This is a distinction between amplification and shock since both the household and firm credit channels induce changes in household net worth and so will result in amplification due to changes in household net worth. Hall (2012) provides a full general equilibrium accounting of the Great Recession and subsequent slump into what he terms the financial friction and household deleveraging channels. This distinction is essentially between financial frictions affecting firms and those affecting households. Hall finds that, at impact, the household credit channel is responsible for roughly 30% of the decline on impact with this share generally declining. Given the relative size of my and Chodorow-Reich's calculations this suggests that either general equilibrium effects amplify the firm credit channel and dampen the household credit channel or that the decomposition of the data given by Hall's model is biased in some manner.

## 2.6 Conclusion

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 30 to 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 and 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.

Figure 2.1: Checking the Stability of Lender Fixed Effects

*Note:* This figure plots the lender fixed effects from estimating regression 2.15,  $\hat{L}_{ij} = \alpha_i D_i + \rho_j \Lambda_j + e_{ij}$ , on lender-county household credit growth with state fixed effects (horizontal axis) and county fixed effects (vertical axis) and the 45-degree line. If the identification assumption (equation 2.9) was inappropriate then I would expect differences in the estimates. For example, if the points were predominantly above the 45-degree line it would indicate that lender effects were biased down (more negative) when controlling for state shocks relative to county shocks. That the points lie along the 45-degree line suggests the identification assumption is appropriate. All data are from HMDA.

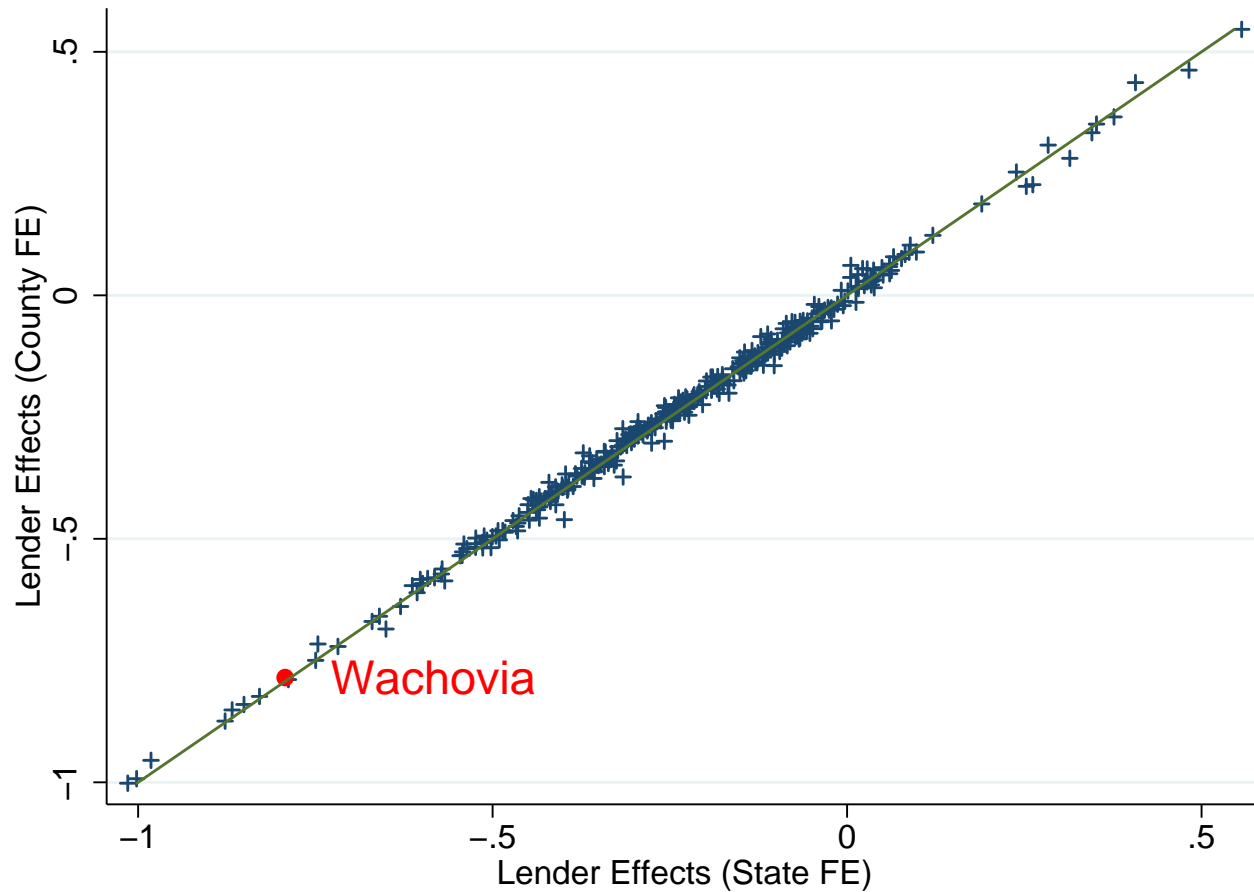




Figure 2.2: Distribution of Measured Shocks in the South and East

*Note:* This figure plots the 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  $\hat{L}_{ij} = \alpha_i D_i + \rho_j \Lambda_j + e_{ij}$ .

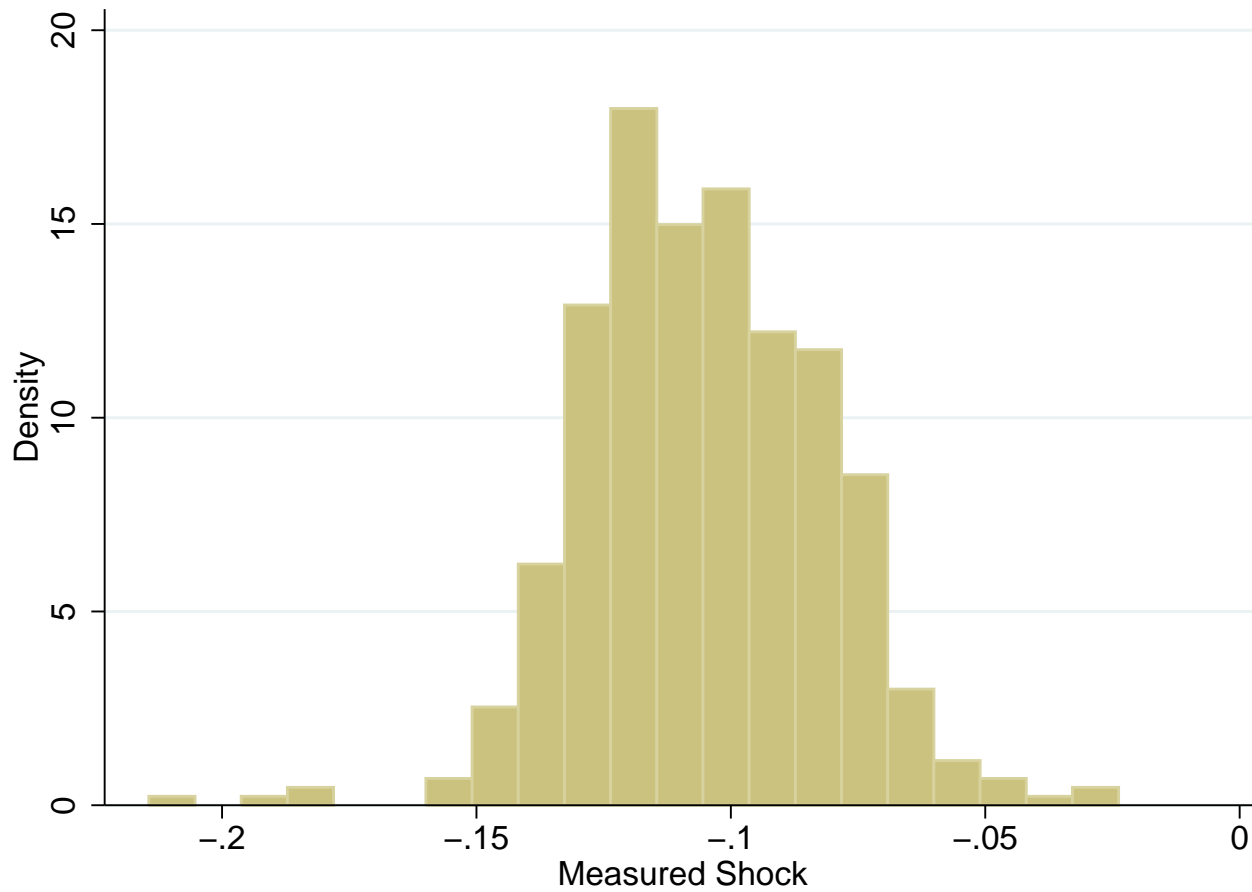


Figure 2.3: Effect of Exposure to Wachovia on the Measured Shock

*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

$$\hat{L}_{ij} = \alpha_i D_i + \rho_j \Lambda_j + e_{ij}.$$
 See the text for more details.

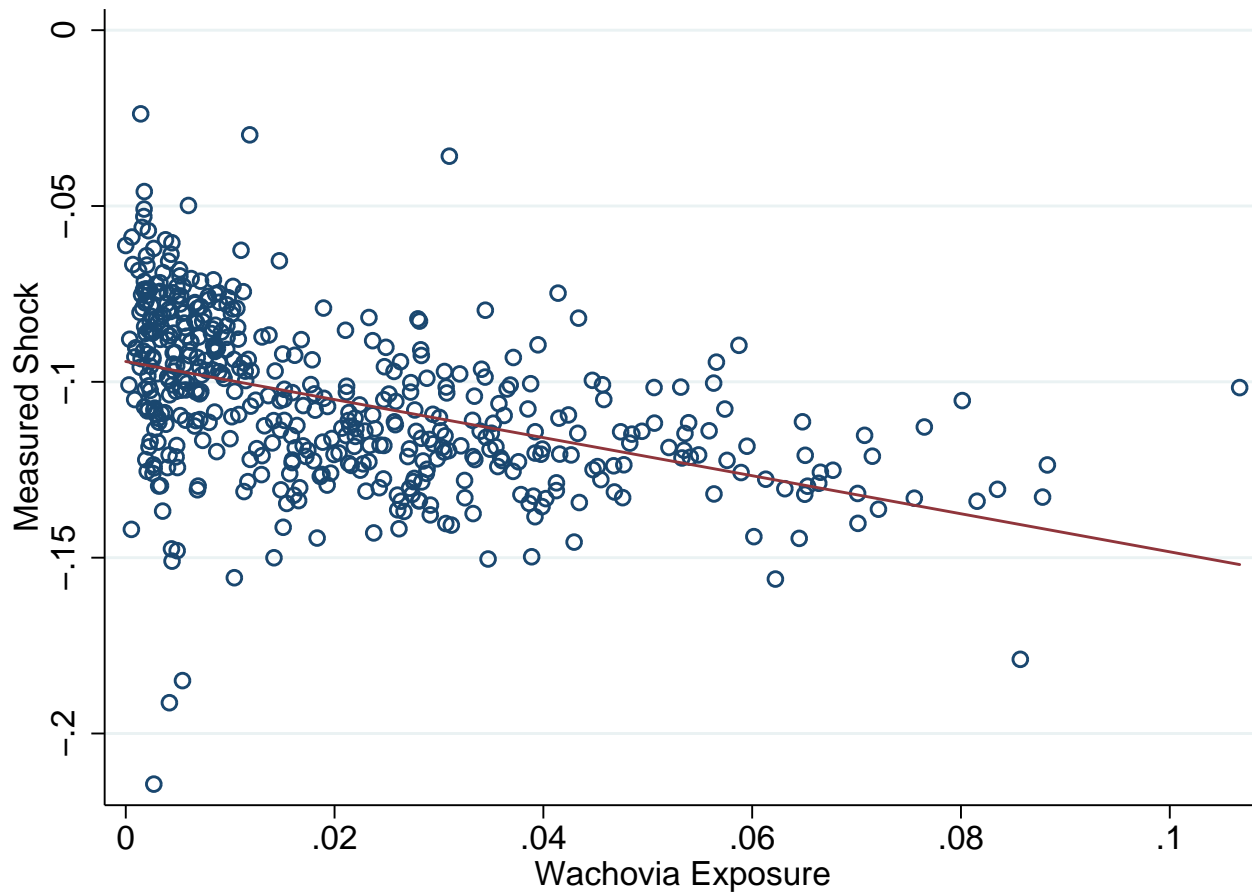


Figure 2.4: Simulated Measured and True Shocks

*Note:* This figure plots the measured shocks against the true shocks for a simulated example. The main point is that the difference between the two appears to be distributed iid. See the appendix for details.

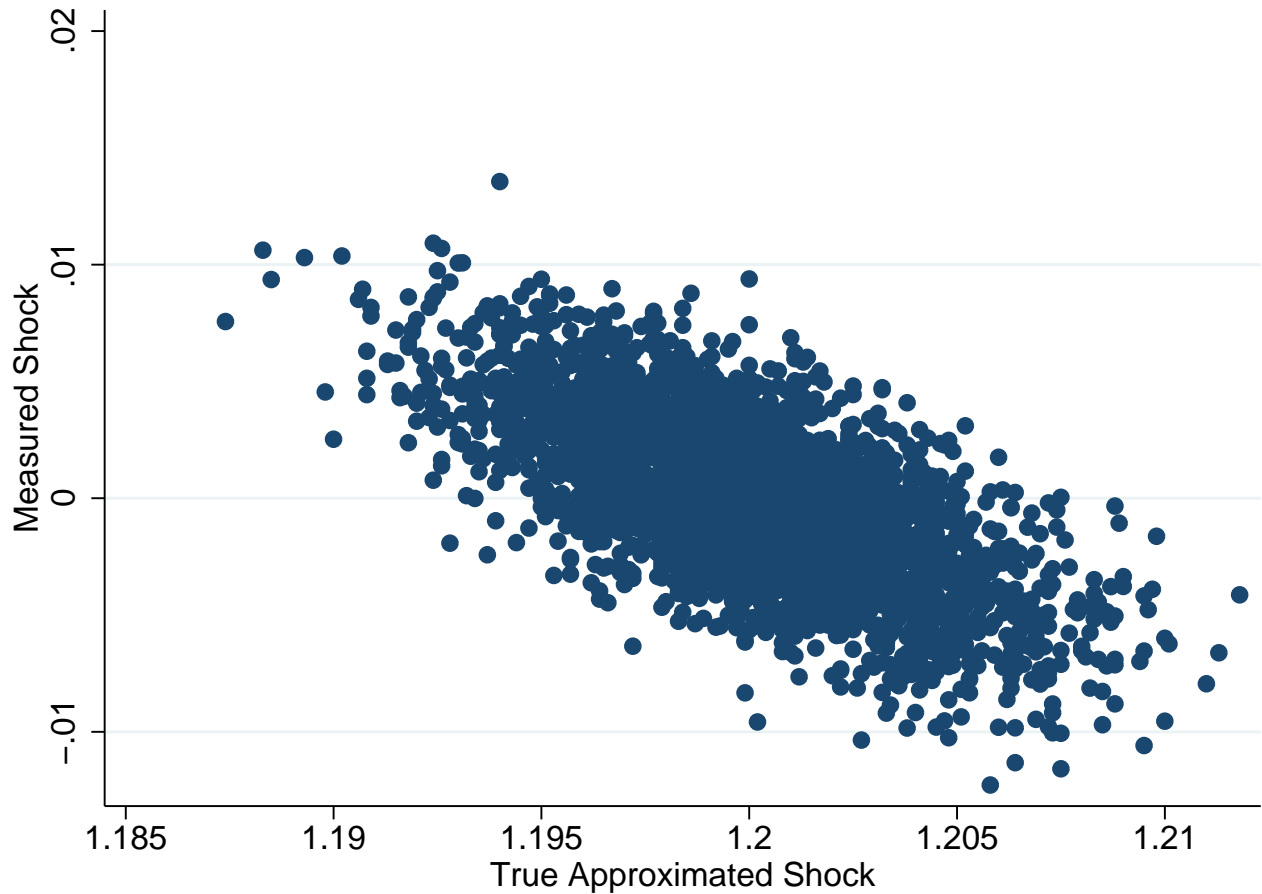


Table 2.1: Lender Regulator Proportions in Total HMDA Data and Estimation Sample

*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.

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

Table 2.2: Effect of Exposure to Wachovia on the Measured Shock (First Stage)

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

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

Table 2.3: Effect of Measured Shock on Employment 2007-2010

*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  $\hat{L}_{ij} = \alpha_i D_i + \rho_j \Lambda_j + e_{ij}$ . The instrumented estimates are about twice as large as the OLS estimate, suggesting significant attenuation bias when only using OLS. Columns three and four control for household leverage fixed effects and the shares of employment in finance, construction, real estate, and tradables. Column four limits the outcome variable to employment growth excluding tradables. Sector definitions come from Mian and Sufi (2014). The sample is limited to the subsample of counties in the South and East with at least 50,000 residents in 2006 with CCP data. Pair bootstrap-t used to construct symmetric p-values and 95% confidence intervals clustered at the state level and adjusted for the generated regressor. All regressions are weighted using county population in 2006.

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

Table 2.4: Aggregate Direct Contribution of Shocks to Household Credit 2007-2010 (% Change)

*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 estimated using 2SLS and the second column uses WLS estimates. 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. These calculations suggest shocks to household credit had significant effects on employment over this period. Explicitly, the numbers report

$\text{direct contribution} = \frac{\beta^{ES}}{\pi} \sum_i \omega_i (s_i - \sum_{j:s_j \geq k^*} \tilde{\omega}_j \hat{s}_j)$  for different choices of the cutoff  $k^*$ . See the text for details.

	2SLS	WLS
No Adjustment - South and East	-11.8	-6.8
75th Percentile - South and East	-3.6	-2.1
75th Percentile - National	-4.5	-2.6

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# Appendix A

## A.1 An Explicit Example and Simulation

Each area is populated by a single household with two partners. One partner decides how much to borrow overall and the second partner must shop across all of the monopolistic lenders in the area for credit. For simplicity, the problem is completely static so that credit is necessary to purchase a certain good.

Household problem

$$\max_{c,H} \log(c_{it}) + \gamma \log(H_{it}) \quad \text{s.t.} \quad (\text{A.1})$$

$$c_{it} + p_{it}H_{it} + r_{it}L_{it} \leq z_{it} + p_{it}H_{it-1}, \quad (\text{A.2})$$

$$L_{it} \leq p_{it}H_{it}. \quad (\text{A.3})$$

By appropriate choice of the parameter  $\gamma$  it can be assured that the borrowing constraint will be binding so that

$$L_{it} = p_{it}H_{it}.$$

I also assume that the supply of housing is constant so that  $H_{it} = \bar{H}$ . Later I relax this assumption. It is trivial to solve this problem for the price of housing

$$p_{it} = \frac{z_{it}\gamma}{\bar{H} + \gamma r_{it}} \frac{1}{r_{it}}. \quad (\text{A.4})$$

Given the assumption that households are always against the borrowing constraint this implies total loan demand is

$$L_{it} = \frac{z_{it}\gamma}{\bar{H} + \gamma r_{it}} \frac{1}{r_{it}} \bar{H}.$$

### Credit Demand

The household shopping partner must collect the required amount of loans from the set of lenders in the area where there is a utility benefit to using certain lenders and this is incurred

for each unit of credit. For example, the distance between the borrower and lender would result in type of problem:

$$\begin{aligned} \max_{L_{ijt} \geq 0} \quad & U(\{L_{ijt}\}) \quad \text{s.t.} \\ & \sum_j r_{ijt} L_{ijt} \leq r_{it} L_{it}, \end{aligned} \tag{A.5}$$

I assume utility over lenders takes the Armington CES form:

$$U(\{L_{ijt}\}) \equiv \left( \sum_j \alpha_{ijt}^{1/\eta_i} L_{ijt}^{\frac{\eta_i-1}{\eta_i}} \right)^{\frac{\eta_i}{\eta_i-1}}.$$

where the  $\alpha$  are demand shocks that sum to one and  $\eta$  is the elasticity of substitution.

This gives standard expressions for demand from a specific lender

$$L_{ij} = \alpha_{ij} \left( \frac{r_{ij}}{r_i} \right)^{1-\eta_i} L_i$$

and for the area-specific price index

$$r_i = \left( \sum_j \alpha_{ij} r_{ij}^{1-\eta_i} \right)^{\frac{1}{1-\eta_i}}.$$

The ratio of credit flows between a lender and area is then, once I substitute the expression for  $L_i$  and hold constant housing supply and prices, simply

$$\frac{L_{ijt}}{L_{ijt-1}} = \frac{\alpha_{ijt}}{\alpha_{ijt-1}} \left( \frac{r_{ijt}}{r_{ijt-1}} \frac{r_{it-1}}{r_{it}} \right)^{1-\eta_i} \frac{z_{it}}{z_{it-1}} \frac{r_{it-1}}{r_{it}}.$$

I can close the model simply by assuming the same monopolists problem as before where demand functions are now given by  $L_{ijt}$ . I assume that monopolist lenders face a linear cost

$$\max_{L_{ij}} r_{ij}(L_{ij}) L_{ij} - c_{ij} L_{ij}.$$

## Simulation

The shocks in the model above are to lender cost shocks  $c_{ij}$ , demand shocks  $\alpha_{ij}$ , and to area-specific demand shocks  $z_{it}$ . In order to simulate the model I need to also set the number of areas  $I$ , the number of lenders  $J$ , and the distribution of demand elasticities  $\eta_i$ 's. I assume the ratio of cost shocks  $c_{ijt}/C_{ijt-1}$  are drawn from normal  $N(\mu_c, \sigma_c^2)$ , which implies the levels of costs are themselves drawn from log normals. The demand shocks for each period are drawn from a uniform distribution and then rescaled to fit the unit interval. The ratio of

income shocks to the area are drawn from a normal distribution. When I set  $\eta$  to be constant across areas and equal to two, I draw it from a truncated normal with a lower bound (open) of at least. As a baseline exercise I set the correlations across all observables to be zero.

I set the parameters of all of these distributions to comparable to those of the data. I set  $I$  equal to 3000 (approximate number of counties) and  $J$  equal to 400 (approximate number of lenders). For simplicity, I assume each lender is operating in each area.

I simulate the data and then construct the approximate shock in the same manner discussed in the text. Figure X plots the measured shock against the true shock. The slope is reversed due to the measured shock relying on the elasticity of demand. While there is clearly dispersion in the measured shock, it is reassuring that the measurement error appears to be iid. Regressing the change in lending on the measured shock gives estimates that are highly significant, but the coefficient is essentially uninterpretable. In the simulation the supply shock is responsible for a decline in total lending of 16%. Applying the procedure outlined in the text where I correct the measured shock with the average of the measured shock in the top quartile of the distribution. I calculate that supply shocks were responsible for a decline of about -13%. Without correcting for the measurement error the implied decline was essentially zero. Because the error was iid I was able to use the difference across the distribution to understate the aggregate direct contribution. While this is comforting, clearly more work must be done on the robustness of this procedure to various assumptions regarding the correlations across fundamentals as well as differences in other structural parameters.

## A.2 Identification Conditions

Let  $N$  be the number of observations, then in matrix notation model 2.15 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 A.10 is the following where  $\circ$  is the Hadamard/Schur product

$$\hat{L} = STA + S(\Gamma \circ e^a)D'\mathbf{1} + DBC + S(B \circ e^c)D'\mathbf{1}. \quad (\text{A.6})$$

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)

$$S'(1 - P_D)S\Gamma A = 0, \quad (\text{A.7})$$

$$S'(1 - P_D)S(\Gamma \circ e^a)D'\mathbf{1} = 0. \quad (\text{A.8})$$

## Decomposition of Measured Shock

To decompose the measured shock I use a slightly different structure than that provided in the text. Assume the following reduced form equations in terms of the demand and cost shifters (essentially, I am ignoring the local supply shock and lumping it in with the “demand” shock

$$\begin{aligned} \hat{r}_{ij} &= \frac{\epsilon_{ij}^{rc}}{1 - \epsilon_{ij}^{rL}\epsilon_{ij}^{Lr}}\hat{c}_{ij} + \frac{\epsilon_{ij}^{rL}\epsilon_{ij}^{Ld}}{1 - \epsilon_{ij}^{rL}\epsilon_{ij}^{Lr}}\hat{d}_{ij} \\ &\equiv \beta_{ij}^r\hat{c}_{ij} + \zeta_{ij}^r\hat{d}_{ij}, \end{aligned} \quad (\text{A.9})$$

and

$$\begin{aligned} \hat{L}_{ij} &= \frac{\epsilon_{ij}^{Lr}\epsilon_{ij}^{rc}}{1 - \epsilon_{ij}^{rL}\epsilon_{ij}^{Lr}}\hat{c}_{ij} + \frac{\epsilon_{ij}^{Ld}}{1 - \epsilon_{ij}^{rL}\epsilon_{ij}^{Lr}}\hat{d}_{ij} \\ &\equiv \beta_{ij}^L\hat{c}_{ij} + \zeta_{ij}^L\hat{d}_{ij}. \end{aligned} \quad (\text{A.10})$$

These are familiar expressions, but it is useful to review several coefficients. The coefficient  $\beta_{ij}^r$  in A.9 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 A.10, is closely related as it is the product of the pass-through parameter and the price elasticity of demand ( $\beta_{ij}^L = \epsilon_{ij}^{Lr}\beta_{ij}^r$ ). Intuitively, the change in quantity borrowed reflects both the change in the price of credit and the effect of increased costs on quantities borrowed.

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

$$\hat{S}_i = \frac{1}{2} \sum_j (\omega_{ij,t} + \omega_{ij,t-1}) \beta_{ij}^r (C_j + e_{ij}^c). \quad (\text{A.11})$$



The measured shock to the same area  $i$  is

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

The parameter  $\pi$  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  $\widehat{E_i(\epsilon_{ij}^{Lr})} E_i(\beta_{ij}^r)$  is likely negative, but I take the weighted average where the weights reflect market share. The correlations between market share and the demand elasticity and market share and pass-through are theoretically ambiguous. 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 et al. (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. Similarly, the sign of  $Cov_i(\epsilon_{ij}^{Lr}, \beta_{ij}^r)$  depends on the size of the other structural elasticities as well as the relationship with market share. Thus, it is not only possible but likely that the average, weighted or not, measurement error is nonzero.