Do Credit Market Shocks affect the Real Economy? Quasi-Experimental Evidence from the Great Recession and `Normal' Economic Times*

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Abstract

This paper uses comprehensive data on bank lending and establishment-level outcomes from 1997-2011 to test whether changes in small business bank lending affect the real economy. The shift-share style research design predicts county-level lending shocks using variation in pre-existing bank market shares and estimated bank supply-shifts. Counties with negative predicted supply shocks experienced declines in small business loan originations throughout the entire period, indicating that it is costly for these businesses to find new lenders. Using confidential microdata from the Longitudinal Business Database, we find the predicted lending shocks led to statistically significant, but economically small, declines in both small firm and overall employment during the Great Recession, but did not affect employment during the 1997-2007 period. Overall, this paper's evidence fails to support the hypothesis that the small business lending channel is an important determinant of economic activity.

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It is nearly conventional wisdom that banks play a special role in the economy. Specifically, it is widely believed that small and medium-sized businesses do not have ready substitutes for bank credit so that their influence on the economy is determined by bank finance (e.g., Brunner and Meltzer 1963; Bernanke 1983). Further, it is thought that their health can be an important determinant of macroeconomic fluctuations (Bernanke and Gertler 1995; Peek and Rosengren 2000; Ashcraft 2005).

This paper gauges the credit channel's empirical importance by separately measuring the economic consequences of shocks to small business credit during the 2007-2009 recession and during 'normal' economic times (i.e., 1997-2007). It is *ex ante* unclear whether credit channel effects should be larger in normal economic times when alternative sources of financing are likely to be more plentiful or during the Great Recession when the United States government and the Federal Reserve Board were aggressively trying to inject liquidity into financial markets.[1]

Our identification strategy leverages the substantial heterogeneity across banks in their year-to-year variation in small business lending along with geographic variation in bank market shares. Further, we isolate the portion of changes in bank lending that can be attributed to supply factors by purging each bank's national change in lending of its exposure to local markets. In the case of the Great Recession years, we predict the change in county-level small business lending over the 2007-2009 period using interactions of banks' pre-crisis county market shares and their national change in lending. Between 2007 and 2009, for example, Citigroup reduced small business lending by 84%, while U.S. Bancorp's small business lending declined by just 3%. The essence of our approach is to ask whether counties with more Citigroup branches than U.S. Bancorp branches before the crisis experienced sharper declines in their economies over the 2007-2010 period. There is sufficient variation in banks' market shares across counties in the same state that our results are based on within-state comparisons.

Using comprehensive data on both bank lending and real outcomes, the paper has three primary findings. First, counties with banks that cut lending had declines in small business loan

[1] This paper is an updated version of Greenstone and Mas (2012). The substantive difference between versions is that we primarily use restricted-use LBD data, whereas Greenstone and Mas (2012) primarily relied on NETS data, which is compiled by Walls and Associates using Dun and Bradstreet's proprietary Market Identifier files. Smaller changes include incorporating a symmetric growth measure, weighting the sample by each county's employment count in 2006 instead of the number of establishments in 2006, and extending the sample back to 1997.

originations over the 2008-2009 period. For example, a one standard deviation reduction in predicted lending in 2009 is associated with a 17% reduction in total county-level small business loan originations from 2009 through 2010. In the short term, at least, it appears that the costs to switching lenders are meaningful for small firms.

Second, this same predicted negative shock in lending depresses 2009-2010 employment growth rates for small standalone firms (single-unit establishments with fewer than 20 employees) by 0.4 percentage points. Under a series of polar assumptions described below, our estimates suggest these lending shocks can account for just 3 percent of the overall decline in small business employment in this period and 5 percent of the total decline in employment. Of course, we cannot reject that the employment effects would have been larger in the absence of the extraordinary interventions undertaken by the Federal Reserve and the U.S. Government to aid banks.

Third, there is a significant relationship between predicted lending shocks and bank loans to small businesses during the 1997-2007 period, but these predicted shocks are not associated with changes in economic activity. This finding runs counter to much of the previous literature, because it suggests that, at least with this identification strategy, the credit channel is not empirically important in normal times. While small businesses appear to have access to alternatives sources of credit in these periods, we cannot directly observe whether these alternatives are more costly.

This paper adds to the literature in several ways. First, our study is nationally representative, which allows us to consider the aggregate implications of our estimates without external validity concerns. Previous papers studying the U.S. have focused on subsamples defined by particular sets of firms, episodes, or regions (e.g., Chodorow-Reich 2014; Peek and Rosengren 2000; Ashcraft 2005).[2,3] Second, while other papers have also focused on small firms, which are more likely to be affected by bank supply decisions (e.g., Duygan-Bump et al. 2014), our paper

[2] Chodorow-Reich examines disruptions in the syndicated loan market following the collapse of Lehman Brothers in 2008, and finds that borrowers of weaker banks faced restrictions in credit supply, which translated into greater cuts in employment at these firms. Its sample is comprised of 2,040 firms that have an average employment size in 2008 of 2985 employees (median 620). Ashcraft examines the closing of healthy banks by the FDIC in some Texas counties in the late 1980s and early 1990s. Peek and Rosengren find that shocks to the balance sheets of Japanese banks operating in the U.S. from economic conditions in Japan affected construction activity in U.S. markets where the Japanese banks operated.

[3] Outside of the U.S., Bentolila et al. (2015) and Hochfellner et al. (2015) use administrative data to estimate the impact of credit supply shocks on employment in Spain and Germany, respectively

additionally measures the impacts on overall county-level employment. Consequently, our estimates incorporate establishment entry, exit, and expansion/shrinkage, as well as any multiplier-style effects or indirect effects via competitor responses without relying on assumption-dependent theoretical models. We believe this is unique for a national study of the U.S. Third, we utilize a research design that allows us to control for confounding demand factors that may have affected employment growth.

Our paper also contributes to the literature on the causes of the Great Recession and the subsequent slow recovery. The range of explanations for this deep decline and slow pace of recovery include reduced aggregate demand (Mian and Sufi 2014), uncertainty (Baker, Bloom, and Davis 2015; Bloom et al. 2014), and structural factors (Charles, Hurst, and Notowidigdo 2012). The list of explanations certainly also includes the tightening of bank lending standards and, at a high level, this theory is supported by the disproportionate employment losses incurred by small firms that are more reliant on bank lending than other firms (Charnes and Krueger 2011; CBO 2012; Fort et al. 2013). Based on this observation, some policymakers (e.g., Bernanke 2010; Krueger 2010) suggested that fractured credit markets played a major role in overall employment declines. Indeed, restoring access to credit was a key feature of the policy response following the financial crisis.[4]

The remainder of the paper is organized as follows. Section II provides some brief background on the financial crisis and the role of small businesses in the U.S. economy. Section III describes the data sources. Section IV explains the research design and how it is implemented. Section V outlines the econometric models and presents the results. Section VI interprets the findings, and Section VII concludes.

[4] Speaking in July 2010 at the Federal Reserve Meeting Series, "Addressing the Financing Needs of Small Businesses," Chairman Ben Bernanke stated that "making credit accessible to sound small businesses is crucial to our economic recovery and so should be front and center among our current policy challenges," and that "the formation and growth of small businesses depends critically on access to credit, unfortunately, those businesses report that credit conditions remain very difficult."

II. Background

The heart of the theory that banks are critical suppliers of credit for small businesses is the idea that it is costly for lenders to obtain information about these firms. Direct measurement of these costs cannot be measured with available data sets, but the existing evidence is often supportive of this possibility. For example using data from the Survey of Small Business Finances through 2003, Brevoort, Holmes, and Wolken (2010) estimate the median distance between firms and their suppliers of credit to be just 3 miles. Further, they find that only 14.5 percent of small firms borrowed from an institution that was more than 30 miles from their headquarters.[5]

A number of empirical studies have investigated the benefits of long-term lending relationships as a way to overcome information asymmetries in the lending market (e.g., Cole 1998, Berger and Udell 1995, Hoshi et al. 1990, Petersen and Rajan 1994). Berger et al. (2002) argue that firms that borrow from large banks tend to be more credit rationed, suggesting that firms that are cut off from credit from larger banks (as in our study) may not be able to obtain credit elsewhere. Nguyen (2015) provides direct evidence of this by showing that branch closings that follow mergers between large banks lead to prolonged declines in local small business lending, indicating that borrowers who lose access to credit have difficulty obtaining credit from other (bank) lenders. In the macroeconomics literature, credit market frictions have been suggested as a channel for the transmission of monetary policy, specifically through the effect of interest rates on the external finance premium, which arises through imperfections in credit markets (Bernanke and Gertler 1995).

There is evidence that the lending channel for small businesses may have been compromised during the Great Recession.[6] Most relevant to our context is that the liquidity crisis translated into less available credit across the economy, and the decline in commercial bank lending was especially severe for small business loans. According to data from the Federal Reserve Survey of Senior Loan Officers, the net percentage of loan officers reporting tightening standards for

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[5] Amel and Brevoort (2005) use survey data from the National Federation of Independent Business Research Foundation to show the median distance over which small firms search for credit is only 4.3 miles. Using data from a single large commercial bank, Agarwal and Hauswald (2010) find a similar median distance between the lending branch and the firm (2.6 miles), and argue this is because geographic proximity facilitates the acquisition of "soft" information. Using data from the Community Reinvestment Act, Laderman (2008) finds that only about 10 percent of small business lending is from banks with no branch in the local market.

[6] The mechanisms behind the unraveling of the financial system in 2008 are complex, and have been analyzed in depth by Brunnermeier (2009) and Shleifer and Vishny (2011) among others.

medium and large firms was 64 percent in the first quarter of 2009 as compared to zero percent in the first quarter of 2007. Data from banks reporting under the Community Reinvestment Act show loan originations to small businesses fell by 52% between 2007 and 2010.[7] A similar pattern is seen in the survey of members of the National Federation of Independent Business: loan availability began to decline in the beginning of 2007, did not reach its nadir until 2009, and has been on a slow recovery since then (Dunkelberg and Wade 2012).

A number of papers explore the underlying mechanisms for this decline in lending and conclude that it was, in large part, "supply-driven." Ivashina and Scharfstein (2010), for example, document that new loans to large borrowers fell by 79% between the second quarter of 2007 and the fourth quarter of 2008. They argue that an important mechanism behind this decline was banks' reduced access to short-term debt following the failure of Lehman, coupled with a drawdown of credit lines by their borrowers.[8] This severe contraction in small business lending may have led to significant real economic effects given both the importance of small businesses in the U.S. economy (in 2007, firms with less than 100 employees represented approximately 36% of employment and 20% of net job creation in the U.S.[9,10]) and their dependence on local bank credit.

III. Data Sources

Our analysis is conducted with what we believe to be the most comprehensive data ever assembled to investigate the role of bank lending in the real economy in the U.S.. The predicted lending shock is constructed using Community Reinvestment Act (CRA) disclosure data from

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[7] Appendix Figure 1 plots the log of constant dollar loan originations to small businesses—defined here as businesses with gross revenues of less than $1 million—from banks reporting under the Community Reinvestment Act. It is apparent that the 2008 financial crisis led to an enormous decline in originations to small businesses. Appendix Figure 2 shows a kernel density plot of the change in log (nominal) small business loan originations between 2007 and 2009 across counties, weighted by the number of establishments in each county in 2006. It reveals substantial geographic dispersion in the decline in small business lending, though the pervasive nature of the recession is also evident in the fact that almost all establishments were in counties that experienced some decline.

[8] See also Huang and Stephens (2014), Berrospide and Edge (2010), Goetz and Gozzi (2010), and Almeida et al. (2012).

[9] Calculated using Census Business Dynamic Statistics.

[10] We note, however, that there is considerable debate regarding the importance of small firms for net job creation. For example, using different datasets on firm employment dynamics Neumark, Wall and Zhang (2011) and Haltiwanger, Jarmin, and Miranda (2013) both find evidence supporting an inverse relationship between net growth rates and firm size, but the latter study notes that it is really new businesses, rather than small businesses, that disproportionately contribute to net job creation.

the Federal Financial Institutions Examination Council (FFIEC). The CRA requires banks above a certain asset threshold to report small business lending each year and by Census tract. The asset threshold was $1.033 billion in 2007 and is adjusted with CPI.[11] We estimate that, in 2007, CRA eligible banks accounted for approximately 86% of all loans under $1 million.[12,13]

The FFIEC provides data by bank, county, and year. Two definitions of small business lending are available: the total dollar amount of small business loan originations, defined as loans under $1 million (≈30% of total originations in 2007), and the dollar amount of small business loan originations to businesses with $1 million or less in annual gross revenue (≈13% of total originations in 2007). As our focus is on small firms, we use the second measure throughout the paper. These data are available from 1997 through 2010.

To calculate changes in a bank's lending over time without including changes due to acquisitions, we employ the standard correction (e.g., Bernanke and Lown 1991), which is to identify acquisitions over every pair of years and treat the acquired and acquiring bank as a single entity over that span. Following this procedure, we roll banks up to the holding company level.[14] This leaves us with 654 bank holding companies that are in the data for at least one year over the 1997-2010 period. While these are a relatively small fraction of all banks, they are the largest banks nationally and thus account for a large share of all lending.

To study establishment-level dynamics, we use confidential microdata from the near universe of establishments in the U.S. Census Longitudinal Business Database (LBD). A key advantage of using these microdata is that we can compute growth rates over a given period based on establishments' sizes at the beginning of that period.[15] Specifically, for a given size category i

[11] Before 2005, the asset threshold was $250 million.

[12] We use FDIC Call Report data from 2007 to compute the fraction of all loan balances held by banks below the asset threshold. This is an inexact estimate since loan balances in the FDIC Call Reports are a stock measure, while CRA originations are a flow.

[13] FDIC Call Reports are not designed to study regional lending because the balance sheet data are only available nationally at the bank-level. For small community banks, however, it may not be a bad approximation to assign the location of the bank' headquarters as the market in which the bank lends (something that would clearly not work for, say, Bank of America). As discussed in Section IV, we use these data to better understand the implications of excluding the smaller banks that do not meet CRA reporting thresholds from our analysis. We find no evidence that small banks change lending balances in response to lending shocks of larger banks, and we conclude that our analysis is not greatly affected by the exclusion of these smaller banks.

[14] We use the FDIC institution directory to identify acquisitions and the FDIC Call reports to link banks to their holding companies.

[15] An example may help to clarify this approach to calculating the growth rate. Consider calculating the growth rates of establishments with 20 or fewer employees, and with 21 to 150 employees. Suppose that an establishment had 100 employees in 2007, shrank to 10 employees in 2008, and then increased to 15 employees in 2009. This

(e.g., establishments with fewer than 20 employees), we define employment growth between *t-1* and *t* in a given county as:

(1) *Employment growth rate*$_{it}$ = *[jobs created by new establishments*$_{it}$ *- jobs lost from closing establishments*$_{it}$ + *employment in continuing establishments*$_{it}$ *- employment in continuing establishments*$_{i,t-1}$*] / [0.5*employment*$_{it-1}$ *+ 0.5*employment*$_{it}$*].*

Subscript *i* denotes establishments that are in size category *i* at the end of period *t-1* so that we are only measuring the change in employment for establishments that were in the relevant size class in the base period, as well as new establishments. Note that the above growth measure is symmetric, ranging between -2 and 2 (as we use the average of *t-1* and *t* employment in the denominator), and is a second-order approximation to ln differences.

Similarly, we compute the establishment growth rate as:

(2) *Establishment growth rate*$_{it}$ = *[new establishments*$_{it}$ – *closing establishments*$_{it}$*] / [0.5*establishments*$_{it-1}$ *+ 0.5*establishments*$_{it}$*].*

We use the LBD microdata to compute these measures. In addition, we also use a special extract of the NETS database, which is compiled by Walls and Associates using Dun and Bradstreet's Market Identifier files.[16] From these microdata, we construct employment and establishment growth rates for all small standalone firms (single-unit establishments with fewer than 20 employees) in each county and year, as well as for establishments that are part of multi-state firms (defined as operating in at least three states). Estimates using the NETS database are primarily used to assess robustness.

County-level outcomes are constructed from the County Business Patterns (CBP) and the Quarterly Census of Employment and Wages (QCEW). The CBP are derived from the Census Business Registrar, while the QCEW are derived by the Bureau of Labor Statistics from state unemployment insurance records. Since the CBP and QCEW have very limited information on firm size, we use them exclusively for county-level analyses.[17]

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establishment would contribute to the 2007-8 growth rate for the 21 to 150 employee category and to the 2008-9 growth rate for the 20 or fewer category.

[16] See Walls (2007) for an in-depth description of these data.

[17] The County Business Patterns has information on the number of establishments by size-category of firm, but this breakdown is inadequate for our purposes. The QCEW does not break down the data by firm size.

Finally, our main estimating equations also include county-level controls derived from Census data, the QCEW, and county debt-to-income ratios from the Federal Reserve Bank of New York.[18]

IV. Research Design

Our research design is based on the observation that some banks cut small business lending more than others following the crisis, and that bank market shares vary substantially across local areas. Table 1 shows the percent change in the nominal dollar amount of small business lending between 2007 and 2009 according to FFIEC CRA disclosures. While small business lending declined by 48% nationally over this period, the table reveals considerable differences across individual banks.

Our identification strategy exploits heterogeneity in counties' exposure to these banks (as measured by their pre-shock market shares) under the testable assumption that firms can only incompletely substitute for a reduction in the supply of credit from their bank. Accordingly, a supply shock to a subset of banks in a given region will affect aggregate lending in that area. We test this assumption empirically, but the numerous papers cited above provide evidence of such frictions.

This section is comprised of three subsections. The first reviews the standard Bartik (1991) or shift-share style approach to develop instrumental variables for local economic shocks that are exogenous to local conditions.[19] We derive the conditions under which the standard approach is valid in our setting, and conclude that the necessary assumptions are unlikely to hold. The second subsection therefore sets out a modified version of the shift-share procedure that solves the primary identification problem associated with the standard approach. Of course, this approach is not assumption-free either, and we describe a series of specification checks that are designed to examine the credibility of these assumptions. The third subsection summarizes the instrumental variable that results from the modified shift-share approach and presents evidence on its validity.

[18] County-level debt-to-income ratios are posted on Amir Sufi's website at http://faculty.chicagobooth.edu/amir.sufi/data.html.
[19] See also Blanchard and Katz (1992), Card (2001), Autor and Duggan (2003), and Notowidigdo (2013) for other applications of this approach.

A. The Standard Shift-Share Approach

This section describes the standard approach to applying a shift-share style identification strategy to estimate the impacts of bank supply shocks. Consider the following estimating equation of interest:

$$(3)\ \ \Delta\ln(Y_i) = \gamma X_i + \beta\ln(Q_i) + \varepsilon_i,$$

where i indicates a county and Y_i is a county-level outcome variable (e.g., employment). The outcome is a function of covariates, X_i, and log loan originations in county i, $\ln(Q_i)$. Estimation of Equation (3) is unlikely to produce unbiased estimates of β, because firms in depressed (booming) areas will both reduce (increase) employment and demand less (more) credit—that is, local economic conditions are an unobserved determinant of the dependent variable that is correlated with loan originations. The challenge is that loan volume is an equilibrium outcome of supply and demand factors, and estimation is susceptible to confounding supply shocks with demand ones.

This is a garden-style identification problem and a tried and true solution is to identify an instrumental variable that isolates the variation in loan originations that is orthogonal to local demand conditions. The following equation specifies a Bartik-style solution by defining an instrumental variable, Z_i:

$$(4)\ Z_i = \sum_j (ms_{ij} * (\Delta\ln(Q_j) - \Delta\ln(Q)),$$

where county i's value of the instrument is the product of (i) the beginning of period market share of each bank j in that county (ms_{ij}) and (ii) the difference between each bank's national change in lending and the economy-wide change in lending, summed over all banks in the county. The exclusion restriction for this instrumental variables identification strategy requires that $COV(Z_i\varepsilon_i) = 0$. In other words, the national change in lending for counties' banks, relative to the national average, does not covary with unobserved factors that determine the county-level outcomes.

To understand the implications of this assumption, we rewrite $\Delta\ln(Q_j)$ as:

$$\Delta\ln\,(Q_j) = \textstyle\sum_i \left(ms^{ij}\,D_i\right) + S_j,$$

where ms^{ij} is the share of bank j's lending in county i. In this expression each bank j's national change in lending is expressed as the weighted sum of its exposure to county-level demand shifters (D_i) in each county where it operates, and a bank-level supply shifter (S_j). (The implications of bank-specific demand shocks are discussed below.) It is now possible to write the Bartik instrument as:

$$Z_i = \sum_j ms_{ij}\left(\sum_i \left(ms^{ij}D_i\right) + S_j - \Delta\ln(Q)\right)$$

$$Z_i = \sum_j ms_{ij}\sum_i ms^{ij}D_i + \sum_j ms_{ij}S_j - \Delta\ln(Q).$$

The validity of the exclusion restriction can now be considered in terms of the separate components of Z_i. The first term is the average exposure of county i's banks to demand shocks in the counties where the banks operate. The second term is the average supply shock faced by the banks that operate in county i. The identifying assumption, then, is that both of these terms are uncorrelated with ε_i. With respect to the former, this condition implies, for example, that counties with worse than average labor market shocks do not have disproportionate representation of banks with greater than average exposure to negative demand-side shocks nationally.

It is immediately evident, however, that since Z_i is a function of D_i, local demand shocks enter directly into the instrument, undermining its validity. For example, banks that operate in markets that were especially hard-hit by the recession (e.g., Florida, Nevada, and Arizona) may make fewer loans than banks with branches in other areas simply because of the larger negative demand shocks in these areas. The traditional approach to addressing this problem is to exclude the own state or county when constructing the instrument. However, this is unlikely to be useful in this setting, because there are regional economies of scale in banking and there is spatial correlation in demand shocks.

B. A Modified Shift-Share Approach

As a solution to this problem, we employ a modified shift-share approach that is purged of county-level demand shocks. The presence of branches of multiple bank holding companies in each county provides an opportunity to purge the common county, or demand, effects from banks' national changes in lending. Specifically, we estimate an equation that decomposes the contribution of the change in equilibrium credit to county and bank components:

$$(5)\ \Delta\ln\left(Q_{ij}\right) = d_i + s_j + e_{ij},$$

where the outcome variable is the log change in small business lending by bank j in county i between two years. We weight the equation by each bank's base period lending in county i so that an observation's influence is proportional to its lending in that year. The county fixed effects, d_i, measure the variation in banks' changes in lending that is common across banks in the same county. Consequently, these county fixed effects capture the local demand for credit.

The parameters of interest are those associated with the vector of bank fixed effects, s_j. They are estimates of changes in bank j's supply of credit that are purged of their differential exposure to county-level variation in demand for small business loans.[20] We estimate the s_j's for every pair of consecutive years beginning in 1997. Additionally, the bank fixed effects, s_j, are re-centered so that their (bank asset size weighted) mean is zero.

Our modified shift-share (Bartik) approach replaces the change in aggregate bank lending, $\Delta\ln\left(Q_j\right)$, in the construction of Z_i in Equation (4) with these estimated bank-specific supply shocks.[21] The resulting instrumental variable is a county-level measure of the predicted lending supply shock:

$$(6)\ Z_i = \sum_j (ms_{ij} * \hat{s}_j),$$

[20] In the Appendix we outline a simple model of local credit supply and demand and show that when county-level credit supply is perfectly elastic, the estimated bank fixed-effect for bank *j* is the average supply response of bank *j* minus the average supply response of all banks present in counties where *j* operates, weighted by a (possibly) county-specific demand elasticitiy.

[21] Khwaja and Mian (2008) use a similar methodology to purge firm-specific credit demand shocks for matched bank-firm lending data from Pakistan. See also Amiti and Weinstein (2013), who apply the Khwaja-Mian methodology to Japanese data to show that supply-side financial shocks can have large impacts on firm investment.

where $\hat{s}_j$ is the estimated bank fixed effect from fitting Equation (5) for changes in small business lending (as measured by CRA loans) between consecutive years and ms_{ij} is bank j's market share in county i in the first of the consecutive years. We standardize the county-level predicted shock, Z_i, using its mean and standard deviation, and weighting by county-level lending in the base year. As with the $\hat{s}_j$'s, we compute the predicted lending shock across every pair of consecutive years beginning in 1997.

The identifying assumption is now weaker then when using the unadjusted shift-share approach, and requires only that counties exposed to banks with above- or below- average supply shocks relative to county averages not have systematically above- or below-average shocks to outcomes.[22] This assumption would be violated if banks with negative supply shocks were more likely to be located in areas that were hard-hit by the recession; this might be the case if, for example, managerial skill in choosing branch locations is correlated with skill in choosing investments for the bank's portfolios.

Although this modified shift-share approach requires weaker assumptions than the standard one, it still requires the assumption that county demand shocks do not have a bank specific component. Suppose, instead, that the correct model for bank j's loan originations in county i is:

$$\Delta \ln (Q_{ij}) = d_i + d_j + s_j + e_{ij},$$

meaning that there is a bank-specific demand shock that cannot be separately identified from the supply shock; this would be the case if, for example, banks specialize in lending to certain industries, and these industries perform differentially from others. In the presence of these bank-specific demand shocks, our modified shift-share approach requires the assumption that they are uncorrelated with shocks to county outcomes.

We leverage the rich data available to conduct a number of specification checks to assess whether our estimates are driven by supply, rather than demand shocks. First, we estimate all models on a sample of small (non-franchise) establishments that are part of larger multi-unit and multi-state firms. These establishments provide a placebo test because their potential sources of credit presumably extend beyond the banks located in their county. Thus, they serve as a useful check for whether our specification adequately adjusts for confounding factors that affect all

[22] See Appendix for the derivation of this condition.

establishments located in the same county; evidence of a relationship between the instrument and local economic conditions would undermine the validity of the modified shift-share approach.[23] We fail to find a significant relationship between the instrument and these establishments' outcomes across a wide variety of outcomes.

Second, the below estimates of the impacts of the lending supply shocks are unaffected by the inclusion of a rich set of county characteristics that are known to be predictors of the severity of the economic downturn in a local area. This is not very surprising since the next subsection shows that, within states, the observed determinants of the outcomes are generally well-balanced among counties with above and below median values of the instrument.

Third, we calculate the correlation between county i's fixed effect (d_i in Equation 5) and the market-share-weighted average fixed effect of banks located in that county (i.e., the market-share weighted average of the s_j's in Equation 5), and find it is negative and close to zero (-0.07). This suggests that banks with negative shocks are not systematically sorted into areas with negative shocks. This finding supports the validity of the instrumental variable, and is consistent with the assumption that the cumulative supply response of banks in a county is uncorrelated with local economic shocks.

Fourth, we show below that the estimated supply responses of individual banks in a county are largely uncorrelated with one another. This argues against the possibility that there is an unobserved factor that is both correlated with local economic shocks and also attracted banks that eventually cut supply.

C. Summarizing the Instrumental Variable and Initial Evidence on its Validity

Figure 1 is a map of the United States where counties' shading reflects the quartile of their predicted lending shock. Here, we use a version of the predicted lending shock that is computed for the entire 2007-2009 period. That is, for this map, instead of estimating Equation (5) for consecutive years, we estimate the bank fixed effects, s_j, using the log change in small business lending by bank j in county i over the entire 2007-2009 period. The regional correlation is

[23] There may, of course, be indirect effects of local lending shocks on establishments who are not reliant on local banks. For example, the shock may enable them to take market share from establishments who are affected, or there may be a multiplier from the shock that negatively affects all firms in the area. Such indirect effects would complicate the interpretation of these intra-county comparisons.

evident, though not always in obvious ways. Florida and Massachusetts, for example, appear to have experienced worse-than-average shocks, while Georgia and Tennessee fared better. The figure also shows substantial within-state variation in the value of the predicted shock, thereby allowing for within-state comparisons.

Table 2 reports summary statistics of county characteristics based on whether the county is above or below median in terms its predicted lending shock. Columns (1) and (2) are the raw means with no adjustments, and Column (4) is the within-state difference after purging out state fixed effects. Columns (3) and (5) report the p-values from tests on equality of means.

Columns (1) and (2) show that counties with worse predicted lending shocks (i.e., below median) have different characteristics compared to counties with better (i.e., above median) predicted shocks. This is not surprising given the spatial patterns seen in Figure 1. These columns compare areas that are far-removed from one another, and, since different regions of the country were likely differentially affected by the economic downturn for a variety of reasons (such as exposure to the construction or manufacturing sectors) that are not directly related to the supply of bank credit, a richer model that controls for regional effects is desirable. Column (4) shows that, when looking within states, counties with above- and below-median predicted shocks look much more similar. The only characteristics for which there remain significant differences are population, population density, and the debt-to-income ratio. Consequently, our main analysis will emphasize specifications that include state fixed effects and that control for county population and debt-to-income ratio, as well as a larger set of county characteristics interacted with year dummies.

We also investigate whether banks with larger cuts in lending systematically sort into particular types of counties. The presence of such sorting might indicate that banks are able to observe something about a county's future prospects that is both unrelated to the predicted supply shocks and unobservable to the econometrician. Evidence of such sorting would undermine the validity of our research design.

Table 3 assesses the degree to which unhealthy banks non-randomly sorted into certain counties. In Column (1) we regress the fixed effect of the bank with the largest market share in a county against the fixed effect of the bank with the second largest market share in the same county. We do not find a significant correlation between the two. In Column (2) we take a more systematic approach by regressing bank j's fixed effect against the average fixed effect of other banks in markets where j operates, weighted by j's lending in each county. This specification also shows no significant relationship between the lending change of a bank and the lending changes of other banks in the same market. These spatial patterns are consistent with the presence of unhealthy banks in particular counties being due to "the luck of the draw", rather than to a systematic sorting of banks into certain counties as a function of their lending policy over the 2007-2009 period.

V. Econometric Models and Results

This section details the exact econometric specifications that we fit and the resulting estimates. The first subsection reports on the relationship between the predicted lending shocks and measured loan originations. The second and third subsections describe the approach for, and results of, estimating the effect of the predicted lending shocks on economic outcomes during the Great Recession and normal economic times (i.e., 1997-2007). In the context of Section IV, this section reports on the reduced form relationships between the modified instrument and the endogenous variable (i.e., observed loan originations) and county-level economic outcomes.

A. The Relationship Between the Predicted Lending Shock and Actual Loan Originations

This section provides evidence that our predicted lending shock measure is predictive of realized county-level loan originations. We begin with a graphical analysis where we divide counties into a top quartile, middle 50%, and bottom quartile according to the value of their 2007-2009 predicted lending shock. The bottom quartile consists of those counties who experienced the largest negative supply shock. We then estimate the following model:

$$(7)\ \ln(l_{it}) = \delta_{st} + \beta_t X_{it} + \tau_{t,<25}\, p_{i,<25} + \tau_{t,25-75} p_{i,25-75} + \varepsilon_{it}\ ,$$

where l_{it} denotes small business loan originations in county i and year t, $p_{i,<25}$ is an indicator for whether the county is below the 25th percentile according to the value of its 2007-2009 predicted lending shock, and $p_{i,25-75}$ is an indicator for whether the county lies in the middle 50%. The effects of these shocks are all allowed to vary by year, including pre-shock years, in order to investigate trends. The model includes a full set of state-by-year fixed effects, δ_{st}, and 2006 county characteristics whose effects are allowed to vary by year.[24] The state-by-year fixed effects mean that comparisons between the groups of counties are made within-state for each year. Finally, we weight the sample by each county's 2006 employment count.[25]

It is worth noting that the use of loan originations as the dependent variable is similar in spirit to using changes in total loans outstanding as an outcome variable. In this respect, since the outcome is a flow variable, the subsequent models are interpretable as first-differences. Thus, these are relatively rich models where the covariates predict changes, rather than levels, of the outcome variable.

The coefficients of interest are the $\tau_{t,k}$, which capture the annual within-state difference in loan originations between the counties with top quartile values of the predicted lending shock and counties in the bottom and middle two quartiles, respectively. In Figure 2, the line with triangle data points plots the coefficients associated with the bottom quartile and year interactions (i.e., $\tau_{t,<25}$), while the line with square data points plots the coefficients from the middle quartiles and year interactions (i.e., $\tau_{t,25-75}$).

The figure confirms there is a strong first-stage relationship even after these regression adjustments.[26] Although there are differences in the level of loan originations between the three groups, the regression adjustment removes most of the difference in pre-existing trends, especially during the 2000-2007 period.

In the subsequent analysis, we primarily rely on the continuous version of the predicted lending shock. In these models, we focus on the 2008 and 2009 shocks separately (i.e., we estimate Equation 5 over consecutive pairs of years, rather than for the entire 2007-2009 period), and estimate versions of the following model:

[24] The controls are log per capita income, construction share, manufacturing share, log population, log population density, and debt-to-income ratio.
[25] Unless otherwise specified, all models are weighted by the county's 2006 employment count.
[26] Appendix Figure 3 shows the corresponding unadjusted estimates. It plots the $\phi_{t,k}$ from estimating $\ln(l_{it}) = p_{i,<25} + p_{i,>75} + \phi_{t,<25}p_{i,<25} + \phi_{t,25-75}p_{i,25-75} + \phi_{t,>75}p_{i,>75} + \epsilon_{it}$.. These represent the annual means of small business loan originations for each group, relative to the 2007 value, which is constrained to be equal across all groups.

$$(8) \qquad \ln(l_{it}) = \delta_{st} + \beta_t X_{it} + \gamma_8 p_{i,2008} + \gamma_9 p_{i,2009} + \theta_{8,8}(\upsilon_{2008} \times p_{i,2008})$$

$$+\theta_{8,9}(\upsilon_{2009} \times p_{i,2008}) + \theta_{8,10}(\upsilon_{2010} \times p_{i,2008})$$

$$+\theta_{9,9}(\upsilon_{2009} \times p_{i,2009}) + \theta_{9,10}(\upsilon_{2010} \times p_{i,2009}) + \varepsilon_{it},$$

where $p_{i,\tau}$ is the predicted lending shock in county i in year τ, and υ_t are year dummies. The predicted shock main effects control for differences in county-level annual loan originations as a function of the 2008 and 2009 predicted supply shocks.[27] For ease of interpretation, the $p_{i,\tau}$'s are standardized to have a mean of zero and standard deviation of one. We report standard errors clustered at the county level to account for serial correlation.[28]

The parameters of interest are the $\theta's$. They are the coefficients on the interactions of the 2008 predicted lending shock with year indicators for 2008, 2009, and 2010, and on the interactions of the 2009 predicted lending shock with year indicators for 2009 and 2010. The $\theta's$ measure the impact of the lending shocks on loan originations in the year of the shock and all subsequent years, relative to the rate of loan originations in the years before the shock and in other counties. Thus, this is a difference-in-differences style estimator.

We emphasize linear combinations of the estimated coefficients that are both easier to interpret and useful for summarizing the magnitudes. Specifically, we report the cumulative effect of the 2008 shock over the 2008-2010 period, and (the larger) 2009 shock over the 2009-2010 period. We define ϕ_8 and ϕ_9 to be the cumulative effect of a county having a one standard deviation increase in its 2008 and 2009 predicted lending shocks:

$$\phi_8 = \theta_{8,8} + \theta_{8,9} + \theta_{8,10}$$

$$\phi_9 = \theta_{9,9} + \theta_{9,10}.$$

For example, ϕ_9 is the cumulative effect over the 2009-2010 period of a county that is +1 standard deviation in the 2009 distribution of predicted lending shocks on log loan originations.

[27] We have also estimated models that include county fixed effects, which is another way to control for differences in annual loan originations across counties. This alternative approach produced almost identical results. Due to the strong similarity of the results, we emphasize the more parsimonious specification going forward.

[28] We have also experimented with clustering by state, but this did not have a notable effect on the standard errors.

The results in Table 4 confirm a robust and statistically significant relationship between the predicted lending shock and loan originations.[29] Column (1) presents estimates from the specification that controls for state-by-year fixed effects. Column (2) adds the interaction of 2006 values of county covariates and year dummies. Column (3) adds to those covariates the interaction of year and the county's debt-to-income ratio in 2006. We add debt-to-income in a separate specification since it is not available for all counties. The point estimates in Column (2) imply that a county with a one standard deviation decline in predicted lending in 2008 experiences a large and persistent decline in loan originations of approximately 7.4% in 2008, 7.7% in 2009, and 8.9% in 2010.[30] The estimate for ϕ_9 suggests that a county with a one standard deviation decline in the predicted lending shock in 2009 is predicted to have a 17% reduction in loan originations over 2009-2010 relative to pre-crisis levels, as compared to the mean county.

Overall, these estimates provide evidence that there are important frictions in the small business lending market. When firms lose access to credit from their bank, it appears that there are meaningful costs that prevent them from immediately switching to other banks, thus leading to a decline in aggregate lending in that area.[31,32]

B. The Relationship Between the Predicted Lending Shocks and Economic Activity During the Great Recession

Having established a strong relationship between predicted and actual loan originations, we turn to examining the effects of these predicted shocks on measures of economic activity using the same specifications. Before describing these results, we note that the dependent variables are all measured in growth rates or log differences. Thus, the controls in the statistical models can be interpreted as controls for growth rates. This is not a change in focus from the previous

[29] Appendix Table 1 presents the corresponding γ estimates.
[30] More precisely, these are log points.
[31] In Appendix Table 2, we use FDIC Call Report data to test whether non-CRA banks are offsetting the effects of lower lending from the larger banks. For small banks, defined as those that are not subject to CRA disclosure, we assign banks to the county where they are headquartered. We then estimate whether loan balances of small banks are affected by the predicted lending supply shocks of larger banks in that county. We find virtually no evidence that small banks change lending balances in response to the lending shocks of larger banks. Thus, the omission of small banks from the analysis is not likely to be a major problem for our analysis.
[32] They may still borrow from non-bank sources, which is one reason the effect of a bank lending shock may not have real effects. We discuss this at the end of Section V.

subsection since loan originations are an approximation to the preferred, but unobserved, outcome of changes in the outstanding value of loans to small businesses.

1. *Small Standalone Firms.* Table 5 provides estimates for the effects of the supply shocks on the growth rates of small standalone firms, which are defined to be single-unit establishments with fewer than 20 employees. We estimate Equation (8) where the dependent variable is either the employment or establishment growth rate for small standalones.

The estimates for ϕ_8 and ϕ_9 (recall these represent the total effect of the predicted lending shocks occurring in 2008 and 2009, respectively) show the 2008 effect (ϕ_8) is close to zero and statistically insignificant, while a one standard deviation in the 2009 shock corresponds to a 0.3 percent cumulative reduction in the employment growth rate over 2009-2010 without county covariates (Column 1), and a 0.4 percent reduction with covariates (Column 2). Adding the debt-to-income control in Column (3) does not change the estimated coefficient relative to the estimate in Column (2), suggesting that the predicted lending shock is not picking up the effects of deleveraging as emphasized in Mian and Sufi (2014). As a basis for comparison, this set of firms experienced a 10% decline in employment between the end of 2007 and the end of 2010.[33]

In Columns (4)-(6) we report the results from estimating the same models, but using the small standalone growth rate as the dependent variable, following the Equation (2) definition of the growth rate and versions of the Equation (8) specification. These estimates range from marginally significant to insignificant across the specifications, though the standard errors are quite large. In the NETS data reported in Appendix Table 3 we find smaller but more precise (and statistically significant) effects on business births and deaths. Therefore, while there is some suggestive evidence that credit affects the formation and destruction of small businesses, the imprecise estimate in the LBD dataset prevents us from making a firm conclusion.

2. *Small Establishments in Multi-Unit Firms*. As a specification check, Table 6 examines a set of establishments that should not be as sensitive to local lending shocks: namely, small (non-franchise) establishments that are part of larger multi-unit firms. These establishments are less likely to be affected by the lending conditions in a particular county since multi-unit firms tend to have broader geographic coverage. We find, across all specifications, that the estimated effect

[33] This figure is based on data from the Census Business Dynamics Statistics.

of lending shocks for employment growth rates are insignificant. Moreover, the estimates on the interactions of the predicted lending shocks and year dummies are jointly insignificant.

A caveat is that we were not able to verify in the LBD data whether the multi-unit firm was geographically diversified or concentrated in a single county. In the latter case we might still pick up some effect of the credit shock. As an additional check, we therefore present estimates from the NETS database where we limit the sample to small establishments of multi-unit firms that operate in at least three states. This sample of firms should have very limited, if any, exposure to changes in the supply of lending from banks in a particular county. These estimates range from slightly negative and insignificant to negative and borderline significant.[34] We conclude from this analysis that the credit shock variables are not picking up differential business cycle effects across regions.

3. *County-Level Economic Outcomes*. Table 7 explores the relationship between the predicted small business lending shock and county-level employment and establishment growth. These estimates provide an opportunity to gauge the full county-level effect of credit supply shocks beyond the category of small firms, including any general equilibrium effects. We use employment growth from the CBP and the QCEW. These datasets are designed to measure the same thing, but they are not perfectly correlated, and the literature does not offer guidance on which is better suited for analyses of county employment. To reduce measurement error in these analyses, we therefore use the average of the growth rates from these datasets for each county and year.

In the specification with state fixed effects and baseline controls (Column 2), the cumulative effect of one standard deviation in the 2009 shock is a statistically significant 0.34 percentage points.[35] It is not surprising that this coefficient is smaller than the small establishment sample as we expect that a credit shock will have a larger impact on smaller firms. Note, also, that the interactions between the predicted lending shocks and year dummies are only marginally significant in the joint test (p-value = 0.18). As in the case for the small establishment sample,

[34] One reason why we see a negative response is that chain establishments might be indirectly affected by the local lending shock. For example, these firms' products may be substitutes for those sold by the small standalones. In this case, the decline in credit for the standalones may allow the chain establishments to expand their operations if a standalone's inability to replace old equipment or expand its operations results in more business for establishments that are part of multi-state firms.

[35] It is not possible to obtain reliable estimates of population changes over these years, so it is unclear whether the shocks affected outmigration or employment to population ratios.

the 2008 shock has an employment effect that is close to zero. We do not find a significant impact of the lending shocks on total county establishments from the 2008 shock, but there is a significant effect from the 2009 shock. The magnitude is smaller than the one estimated using small establishments in Table 5, but it is much more precisely estimated.

C. The Relationship Between the Predicted Lending Shocks and Economic Activity During "Normal" Economic Times

Up to now, we have considered the effects of the credit shocks that occurred over the 2007-2009 period. However, the methodology we use to construct the predicted lending shock can also be used to assess how shocks affected the real economy during less volatile times. To this end, we extend the analysis to include shocks dating back to 2000 and employ a model that incorporates all shocks simultaneously.

We estimate a model that constrains the effect of the predicted lending shock to be the same for all years, but allows for a shift in 2008 and 2009. For loan originations, the estimating equation is:

$$\text{(9)} \qquad \ln(l_{it}) = \theta_1 p_{it} + \theta_2 p_{it-1} + \theta_3(\nu_{2008} \times p_{it}) + \theta_4(\nu_{2009} \times p_{it-1})$$
$$+\theta_5(\nu_{2009} \times p_{it}) + \theta_6(\nu_{2010} \times p_{it-1}) + \beta X_{it} + \lambda_i + \delta_{st} + \varepsilon_{it},$$

where ν_t is a dummy for year *t*, and the lending shocks for county *i* in year *t*, p_{it}, are calculated as in Equation (6). This specification assumes that a shock has an effect over two periods, in *t* and *t+1*. In addition to reporting the estimated θ parameters, we also report the total effect of the 2008 shock $(\theta_1 + \theta_2 + \theta_3 + \theta_4)$, the total effect of the 2009 shock $(\theta_1 + \theta_2 + \theta_5 + \theta_6)$, and the excess effect of the 2008 and 2009 shocks, which are $(\theta_3 + \theta_4)$ and $(\theta_5 + \theta_6)$, respectively. We estimate the model separately for small establishment and total county employment growth rates.

Table 8 presents these estimates. Column (1) shows there is a strong relationship between predicted small business lending and actual small business lending in all years of the sample. This means we can use the predicted lending shocks to test the relationship between lending and employment in non-crisis years. The interaction terms show that there is a much larger and more precise effect of predicted lending on actual lending over the 2008-2009 period. We believe this asymmetry is an inherent feature of the shift-share approach, as it is easier to predict where lending will decline than where it will grow.

Column (2) presents estimates from Equation (9) with the LBD small standalone employment growth outcome. The shock terms for the pre-2008 period are small and insignificant. The estimated impact of the 2009 shock, however, is similar in magnitude to the estimate in Table 5: one standard deviation in the predicted lending shock is associated with a 0.4 percent reduction in the small establishment growth rate over the 2009-2010 period. The difference in effects between 2009 and the earlier years is significant, as shown by the coefficient on the "Excess effect of the 2009 shock." As in Table 5, there is no relationship between the 2008 predicted lending shocks and small standalone employment outcome.

Column (3) shows the predicted lending shocks have no significant effect on county-level outcomes in non-crisis years. The effects of the 2008 and 2009 shocks are close to zero and insignificant.

We also present visual evidence of the year-by-year differences in the effect of predicted lending shocks on outcomes. To do this, we estimate the following model:

$$(10) \quad \ln(l_{it}) = \omega_{1t}p_t + \omega_{2t}p_{t-1} + \omega_{3t}p_{t+1} + \delta_{st} + \beta X_{it} + v_t + \lambda_i + \varepsilon_{it}.$$

This model includes the interactions of the year *t* shock, the lagged *t-1* shock, and the lead *t+1* shock with calendar year dummies. Therefore, it allows the effect of a shock to persist over two periods and to differ by calendar year. We include the lead term as a specification check since a shock in year *t+1* should not affect lending in year *t*.

Figure 3 plots the effect of the shock originating in each year. For each year *t,* we plot the sum of ω_{1t} and ω_{2t+1}, which is the effect of a one standard deviation lending shock that occurred in year *t* on lending in years *t* and *t+1*. The dotted lines show the 95 percent confidence interval. As seen in Table 8, the relationship between predicted lending and actual lending is highly significant in all years, but displays a counter-cyclical pattern with a point estimate that is almost

5 times larger in 2009 than in 2004.[36] As previously discussed, we believe this pattern derives mechanically from the construction of the predicted shock.

Figure 4 is analogous to Figure 3 except the outcome variable is now small standalone employment growth. The figure visually confirms that the 2009 shock has a larger effect on small business employment relative to any other year.

We conclude that while the effect of small business lending shocks on employment is relatively small during the 2008 financial crisis, it is larger than in earlier years. The lack of a relationship during the pre-crisis period may be because economic activity is responsive to changes in total credit availability, not just credit from banks. Alternative sources of credit for small businesses, such as home equity loans, were also restricted during the 2007-2009 period, which could be why the effect during the 2008 financial crisis is larger. It is also worth noting that credit was largely booming during the pre-crisis period. As such, our results may reflect that the real effects of negative credit supply shocks are asymmetric relative to those of positive credit supply shocks.

VI. Interpretation

It is natural to ask how much of the employment loss during the Great Recession can be attributed to the reduction in bank credit supply to small businesses. However, it is not straightforward to apply estimates of the cross-sectional effects of these supply shocks to time-series variation in aggregate small business lending. Such an approach requires taking a stand on the share of the change in small business lending nationally that was due to supply shifts, rather than to demand shifts, and on the magnitude of the general equilibrium effects of these shocks.

As an alternative, we conduct the following simple and transparent bounding exercise. We obtain an upper bound estimate of the aggregate effects by assuming that the entire reduction in small business lending between 2007 and 2009 was driven by the credit supply decisions of banks. Clearly this is a polar assumption and will overestimate the effect of reduced credit supply since some of the observed reduction in lending was due to lower demand for credit as a result of the

[36] The estimated coefficient (standard error) for the average of the lead terms of the predicted credit supply shock, ω_{1t}, is -0.0010 (0.0007), indicating that future credit supply shocks do not significantly affect loan originations. The lack of significance of the lead terms supports the validity of our specification.

recession and a more elevated risk of business default. However, we still believe this to be a useful exercise for the purpose of assessing the magnitudes of our estimates.

The first step is to note that CRA-disclosed small business lending declined by 22% in 2008 and 33% in 2009. Assuming that these represent supply shifts, we can apply these shifts to our estimates to assess the magnitude of the aggregate impact of the 2007-2009 lending shocks on small business employment growth and county-level economic activity.

The second step is to estimate a two stage least squares (2SLS) model where employment is the dependent variable, and the regressors of interest are contemporaneous and lagged log loan originations. The instruments for these regressors are the interactions of the 2008 lending shock with 2008, 2009 and 2010 dummies, and the interaction of the 2009 shock with 2009 and 2010 dummy. The model also includes all main effects, state-by-year fixed effects, and the standard set of county-level control variables interacted with year dummies. Thus, the first-stages are versions of Equation (8) where the dependent variables are contemporaneous and lagged log loan originations. These models are estimated on data from 1997 through 2010.

The 2SLS estimates are reported in Table 9. [37] All entries can be interpreted as elasticities since the outcomes are expressed as growth rates or natural log differences and the endogenous variables are the natural log of the loan origination rate. Column (1) reports the estimates for employment growth of small establishments in the LBD. As this is a bounding exercise, we are less concerned with statistical significance than with magnitudes and confidence bands. Nevertheless, we find that employment growth has a marginally significant positive relationship with contemporaneous shocks and an insignificant relationship with lagged shocks. The elasticity of employment growth with respect to current loan originations is 0.009 (se = 0.008), while the elasticity with respect to lagged originations is -0.003 (se = 0.007). The first stage Angrist-Pischke F-statistics are well above conventional thresholds.

The 2SLS estimates imply that the national changes in small business lending (22% in 2008 and 33% in 2009) resulted in 0.3 percentage points (=0.22*0.009+0.33*0.009+0.22*-0.003+0.33*-0.003) lower small business employment at the end of 2010 due to the reduction in small business lending. Thus, this upper bound estimate accounts for only 3 percent of the 10 percent decline in small business employment between the end of 2007 and 2010 for firms with fewer

[37] Appendix Table 4 reports the corresponding OLS models.

than 20 employees. The same exercise using the values at the upper end of the 95% confidence interval for the time *t* and *t-1* lending effects results in a reduction of small business employment of 1.2 percentage points, or an upper-bound of 12 percent of the overall change over the period.[38] However, this estimate is the combination of two extreme assumptions and the true contribution is very likely much lower.

Column (2) reports on aggregate county employment. A similar calculation on the effect of total employment shows a modest cumulative upper bound effect of 0.3 percentage points from 2008-2009. Thus, the point estimates suggest that the reduction in small business lending accounted for a decline in total employment of up to 5 percent of the 6 percent decline in total employment between the end of 2007 and the end of 2010. Using the values at the upper end of the 95% confidence interval implies the decline in small business lending contributed to no more than 15% of the overall reduction in total employment. Here, too, it is apparent that the decline in small business loans was not a primary contributor to the employment decline during the Great Recession.

VII. Conclusion

This paper has used a new identification strategy and what we believe to be the most comprehensive data set assembled to investigate the role of bank lending on the real economy. We develop a new measure of local credit supply shocks for small businesses that is based on (i) the pre-existing market shares of the banks that serve a county and (ii) the national change in each bank's lending that is attributable to supply factors (e.g., due to differences in the crisis' effect on their balance sheets).

We first apply this identification strategy to the Great Recession period. The analysis finds that the 2008 and 2009 measures of local credit supply shocks are associated with sharp declines in total county-level small business loan originations. We find that a one standard deviation reduction in the 2009 measures of local credit supply shocks is associated with a 17% reduction in total county-level small business loan originations from the end of 2008 through the end of

[38] Calculating the 95% confidence interval associated with the 0.005 point estimate requires calculating the standard error of the linear combination of coefficients on ln(loan originations)(t) and ln(loan originations)(t-1). This, in turn, requires knowing the covariance between these regression coefficients. As the LBD data are restricted access, we are unable to calculate this directly, and therefore use the covariance calculated from the CBP/QCEW estimates.

2010, indicating that, at least in the near term, it is costly for small businesses to switch lenders. With respect to impacts on the real economy, we find that a one standard deviation reduction in the 2009 predicted lending shock depresses 2010 levels of small establishment employment growth by 0.4 percentage points. Under a series of polar assumptions, these estimates suggest these lending shocks can account for just 3 percent of the overall decline in small business employment in this period and 5 percent of the total decline in employment.

We also apply this same identification strategy to the 1997-2007 period. Again, we find that there is a significant relationship between predicted lending shocks and bank loans to small businesses. However, these predicted lending shocks are not associated with changes in economic activity during this period. This finding suggests that, at least when using the variation from this paper's identification strategy, the credit channel is not empirically important in normal times.

These results are informative, although unlikely to be dispositive, about a series of policy issues. The banking industry is heavily regulated and, as the extraordinary response to the recent financial crisis demonstrates, governments are willing to extend significant aid to banks during periods of financial stress. In the United States, these policies included capital injections through the Toxic Assets Relief Program, nearly costless loans from the Federal Reserve to banks, and stress tests. It is possible, and indeed perhaps likely, that the credit shocks and resulting impacts on the broader economy would have been more severe in their absence.

We close with two final observations. First, if the limited impact of credit shocks on the real economy during the "normal" period of 1997-2007 reflects that borrowers are usually able to adjust to a restriction in bank credit without impacting the real economy, it may be appropriate to take these findings as evidence that proposals for new programs that aim to increase banks' credit supply for small businesses are unnecessary, at least if the goal is to affect county-level employment. Second, as this paper does not present evidence on the moral hazard consequences of government interventions in the banking market, it does not provide a complete picture of their welfare impacts.

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Figure 1

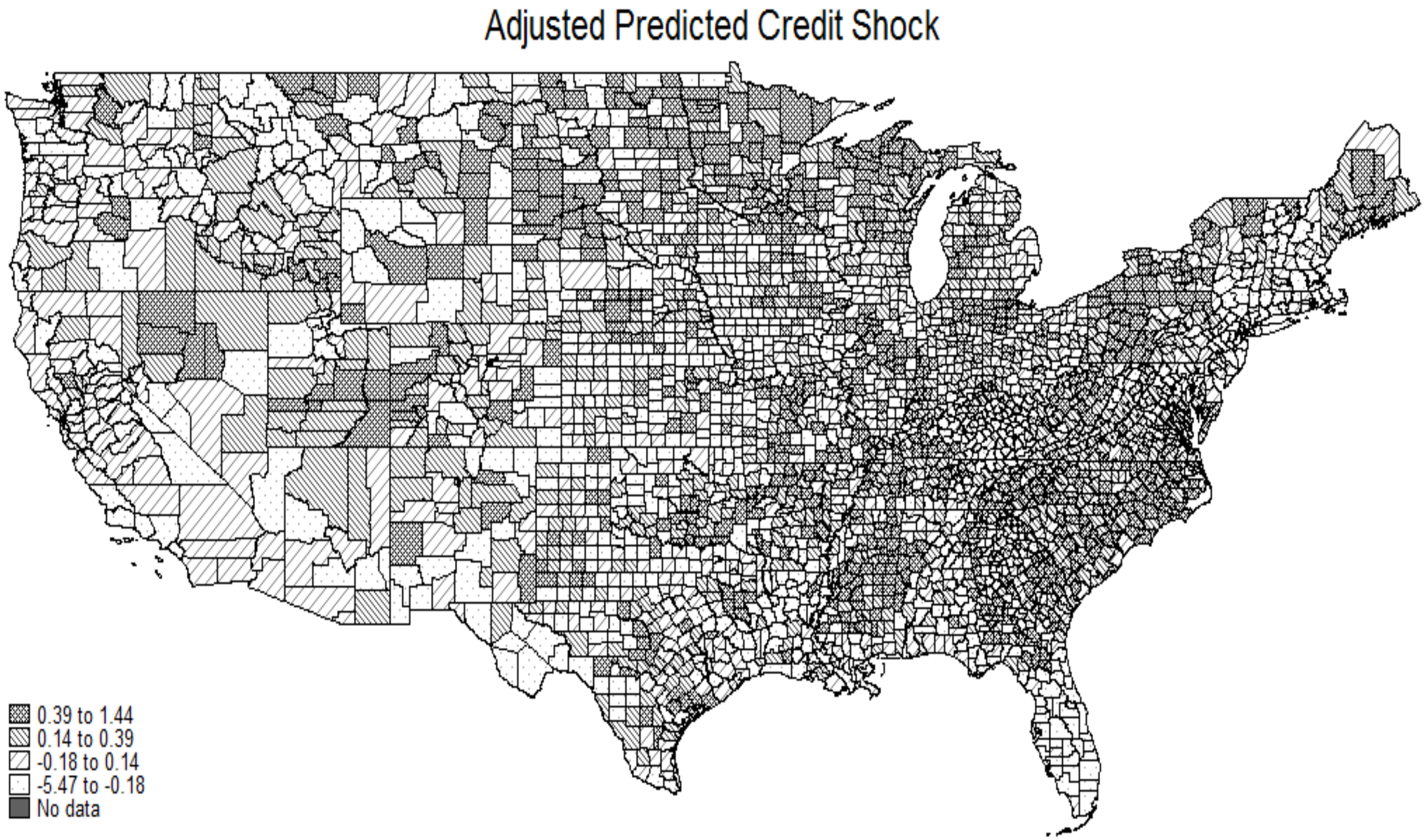
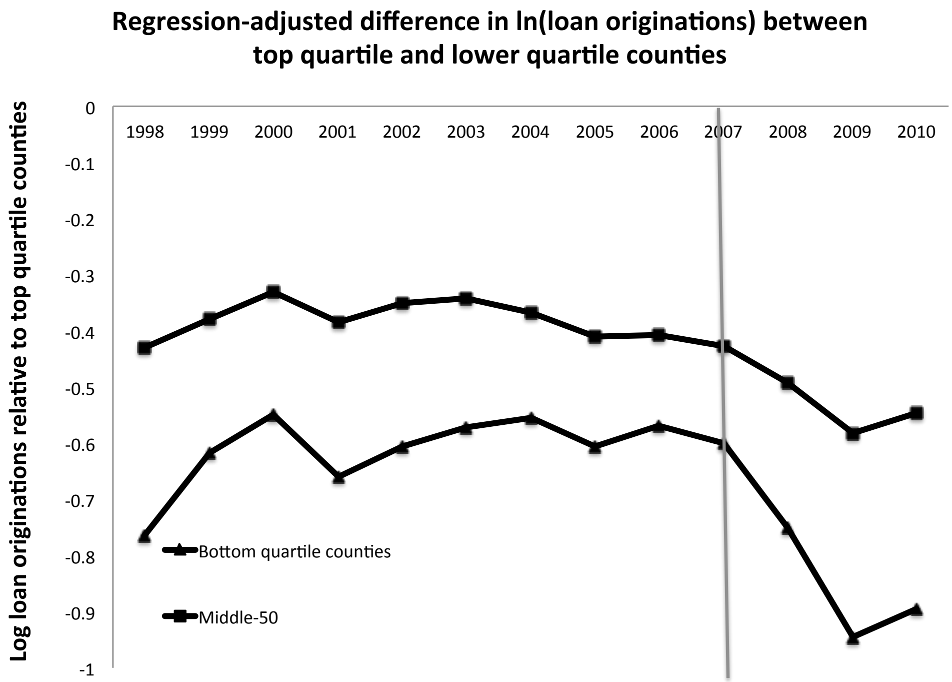
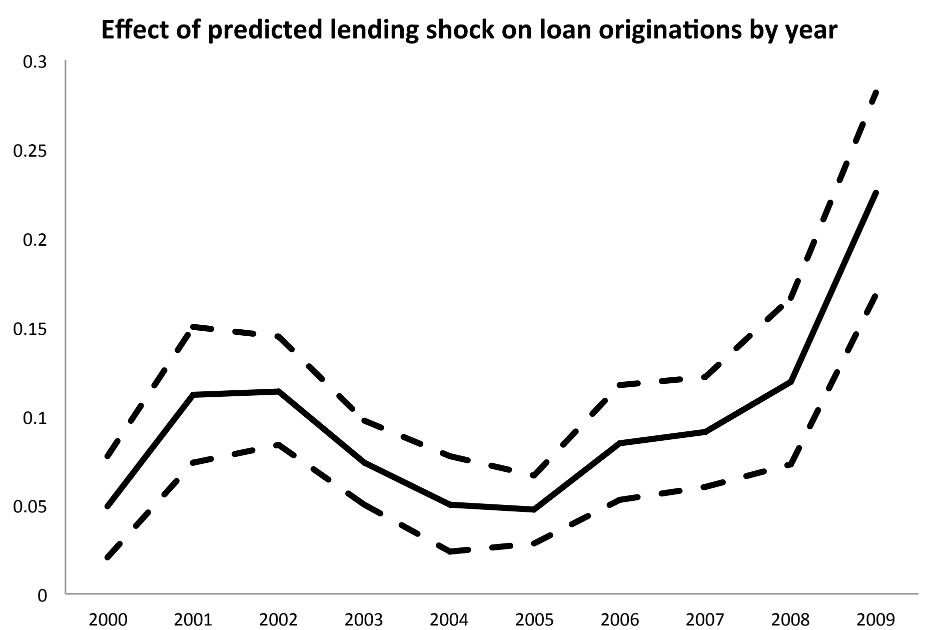

Adjusted Predicted Credit Shock
0.39 to 1.44
0.14 to 0.39
-0.18 to 0.14
-5.47 to -0.18
No data

Figure 2



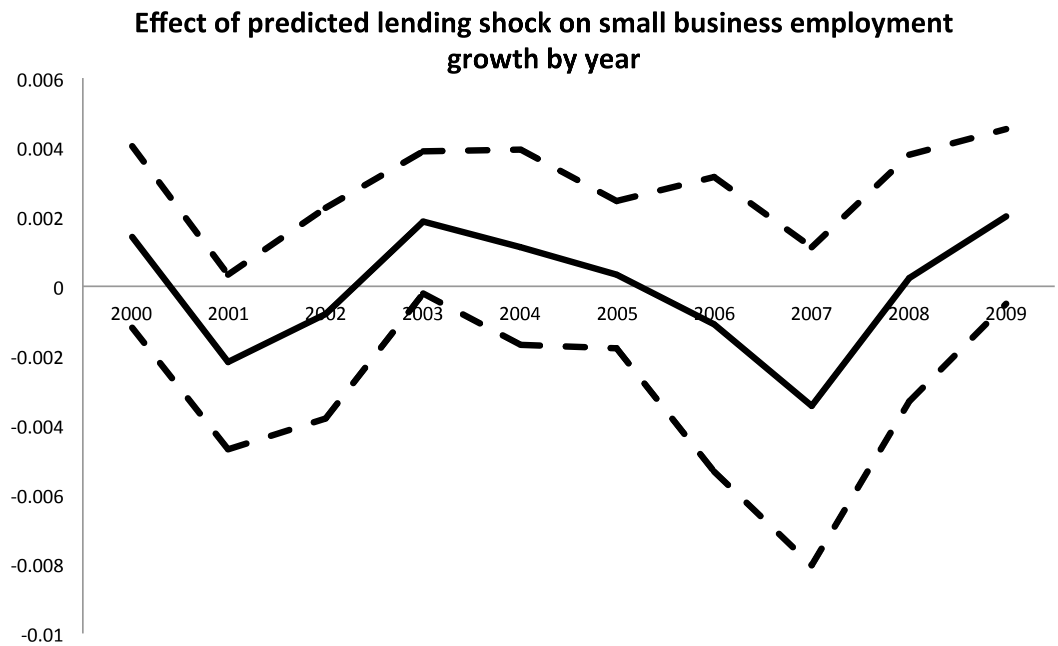
Notes: Figure is based on estimation of equation (7). Quartiles are based on the value of the 2007-2009 predicted lending shock. See text for further details.

Figure 3



Notes: Figure is based on estimation of equation (10) where the dependent variable is log small business loan originations. Y-axis shows effect of a one standard deviation change in predicted log lending on log loan originations in *t* and *t+1*. See text for further details.

Figure 4



Notes: Figure is based on estimation of equation (10) where the dependent variable is the employment growth rate for small standalone firms, defined as single unit establishments with fewer than 20 employees. Y-axis shows effect of a one standard deviation change in predicted log lending on small business employment growth in *t* and *t+1*. See text for further details.

Table 1: Changes in lending between 2007-2009 for selected large bank holding companies

	(1) Percent change in small business lending	(2) Percentile	(3) Percentile net of county fixed effects
Bank of New York Mellon	-89.9	2	3
JP Morgan Chase	-88.5	2	2
Citigroup	-83.6	4	6
Bank of America	-77.2	6	9
Wachovia	-57.0	18	22
Capital One Financial	-79.5	5	5
Suntrust Banks	-41.8	34	36
Regions Financial	-37.7	38	34
Wells Fargo	-33.1	44	59
HSBC	-31.9	45	71
BB&T	-19.5	60	58
PNC Financial	-33.2	43	44
U.S. Bancorp	-3.3	76	78
Median across all CRA reporting banks	-32		
All banks combined	-48		

Note: Column (1) is the percent change in lending to firms with less than $1m in gross revenue between 2007-2009 as reported in CRA disclosures published by the FFIEC. Column (2) is the percentile of the change in CRA lending across all holding companies that meet the criteria for CRA disclosure (a lower percentile is worse). Column (3) is the percentile in the change in CRA lending after partialing out county fixed effects.

Table 2: County characteristics

	(1) Above median in predicted lending shock	(2) Below median in predicted lending shock	(3) p-value on difference	(4) Above median - Below median in predicted lending shock (within-state)	(5) p-value on within-state difference
Employment growth 2002-2006 [n=3117]	0.042	0.042	0.970	0.005	0.351
	[0.114]	[0.132]		(0.005)	
Wage growth 2002-2006 [n=3117]	0.142	0.154	0.000	0.000	0.965
	[0.078]	[0.087]		(0.003)	
Home price appreciation 2002-2006 [n=571]	0.327	0.449	0.000	-0.014	0.321
	[0.020]	[0.203]		(0.014)	
% change total bank lending 2002-2006 [n=3138]	0.022	0.108	0.001	-0.039	0.168
	[0.609]	[0.772]		(0.028)	
Log median per capita income 2006 [n=3140]	10.585	10.580	0.624	0.012	0.172
	[0.220]	[0.270]		(0.009)	
Poverty rate 2006 [n=3141]	15.363	15.507	0.555	-0.064	0.767
	[5.950]	[6.728]		(0.217)	
Construction share 2006 [n=3115]	0.066	0.064	0.484	0.002	0.335
	[0.043]	[0.051]		(0.002)	
Manufacturing share 2006 [n=3073]	0.180	0.149	0.000	0.006	0.208
	[0.129]	[0.136]		(0.005)	
ln(density in 2006) [n=3113]	-10.860	-11.190	0.000	0.309	0.000
	[1.567]	[1.929]		(0.055)	
ln(population in 2006) [n=3114]	10.347	10.098	0.000	0.340	0.000
	[1.303]	[1.670]		(0.052)	
Debt-to-income ratio 2006 [n=2219]	1.540	1.651	0.000	-0.090	0.000
	[0.529]	[0.689]		(0.025)	

Notes: Standard deviations in brackets. Employment growth, wage growth, construction share, and manufacturing share are from the QCEW. Change in lending is from the FFIEC. Per capita income, poverty rates, population and density are from the Census. Home values are from Zillow. County debt-to-income ratios are from the Federal Reserve Bank of New York. Column 4 is obtained from a regression of the county characteristic on an indicator for above median with state fixed effects.

Table 3: Testing for spatial sorting in bank lending shocks

	(1)	(2)
	County-level data	Bank-level data
	Dependent variable: Fixed effect of the bank with the largest marketshare in the county	Dependent variable: Bank fixed effect
Constant	0.493	0.618
	(0.044)	(0.031)
Fixed-effect of the bank with the second largest marketshare in the county	-0.043	
	(0.069)	
Average competitor bank fixed-effect in counties where the bank operates		0.078
		(0.111)
Observations	2352	654
R-squared	0.0025	0.001

Notes: Robust standard errors in parentheses. Model 1 is an OLS regession of the fixed effect of the bank with the largest market share in the county on the fixed effect of the bank with the second highest market share, weighted by the number of establishments in the county in 2006. The bank fixed effects are estimated from a regression of the log change in small business lending by county and bank between 2007 and 2009 on county and bank holding company fixed effects weighted by 2007 lending. Model 2 is a regression of each bank holding company's fixed effect on the average bank fixed effect in counties where the bank operates. To compute the average bank fixed effect, we calculate the dollar weighted average bank fixed effect in every county excluding bank i, and then aggregate these averages to the bank holding company level weighting by the share of bank i's lending in the county.

Table 4: Relationship between predicted lending shock and ln(loan originations)

	(1)	(2)	(3)
2009 shock * 2010	0.0744	0.0812	0.0822
	(0.0089)	(0.0090)	(0.0096)
2009 shock * 2009	0.0809	0.0879	0.0882
	(0.0087)	(0.0088)	(0.0091)
2008 shock * 2010	0.0549	0.0893	0.0937
	(0.0108)	(0.0105)	(0.0117)
2008 shock * 2009	0.0496	0.0766	0.0815
	(0.0106)	(0.0111)	(0.0123)
2008 shock * 2008	0.0478	0.0735	0.0847
	(0.0092)	(0.0089)	(0.0093)
Cumulative effect of 2008 shock	0.1523	0.2394	0.2599
	(0.0258)	(0.0249)	(0.0273)
Cumulative effect of 2009 shock	0.1553	0.1691	0.1704
	(0.0164)	(0.0166)	(0.0175)
F-test of joint significance of shock interactions (p-value)	0.000	0.000	0.000
Observations	43358	42224	30884
State-by-year fixed effects	X	X	X
Baseline controls		X	X
Debt-to-income ratio			X

Notes: Entries are based on estimation of Equation (8) where the dependent variable is log loan originations. Standard errors clustered on county in parentheses. An observation is a county-by-year cell. Shocks refer to predicted loan originations as specified in Equation (6). Baseline controls are 2006 log density, log population, construction share, manufacturing share, and log per capita income. All controls are interacted with year dummies. All main effects are included. See the text for further details.

Table 5: Effect of predicted lending shock on employment and establishment growth rates for small standalone firms

	Employment growth rate			Establishment growth rate		
	(1)	(2)	(3)	(4)	(5)	(6)
2009 shock * 2010	0.0015	0.0018	0.0019	0.0081	0.0073	0.0073
	(0.0009)	(0.0009)	(0.0010)	(0.0046)	(0.0043)	(0.0046)
2009 shock * 2009	0.0011	0.0022	0.0023	0.0073	0.0063	0.0058
	(0.0010)	(0.0010)	(0.0012)	(0.0053)	(0.0050)	(0.0055)
2008 shock * 2010	-0.0003	-0.0019	-0.0018	0.0073	-0.0033	-0.0055
	(0.0011)	(0.0010)	(0.0010)	(0.0051)	(0.0046)	(0.0051)
2008 shock * 2009	-0.0011	0.0002	-0.0004	0.0079	-0.0061	-0.0101
	(0.0014)	(0.0012)	(0.0013)	(0.0064)	(0.0060)	(0.0067)
2008 shock * 2008	-0.0024	-0.0002	-0.0006	-0.0066	-0.0105	-0.0137
	(0.0009)	(0.0009)	(0.0009)	(0.0200)	(0.0202)	(0.0224)
Cumulative effect of 2008 Shock	-0.0038	-0.0019	-0.0029	0.0086	-0.0199	-0.0292
	(0.0028)	(0.0024)	(0.0025)	(0.0265)	(0.0260)	(0.0288)
Cumulative effect of 2009 Shock	0.0026	0.0040	0.0042	0.0154	0.0136	0.0131
	(0.0014)	(0.0015)	(0.0018)	(0.0096)	(0.0088)	(0.0098)
F-test of joint significance of shock interactions (p-value)	0.03	0.03	0.10	0.10	0.55	0.47
Observations	43540	42420	30842	43540	42420	30842
State-by-year fixed effects	X	X	X	X	X	X
Baseline controls		X	X		X	X
Debt-to-income ratio			X			X

Note: Entries are based on estimation of Equation (8). The dependent variable in Columns (1)-(3) is the employment growth rate for small standalone firms calculated according to Equation (1). The dependent variable in Columns (4)-(6) is the establishment growth rate for small standalone firms calculated according to Equation (2). Small standalone firms are defined to be single-unit establishments with fewer than 20 employees. Standard errors clustered on county in parentheses. An observation is a county-by-year cell. Shocks refer to predicted loan originations as specified in Equation (6). Baseline controls are 2006 log density, log population, construction share, manufacturing share, and log per capita income. All controls are interacted with year dummies. All main effects are included. See the text for further details.

Table 6: Effect of predicted lending shock on employment growth rates for small establishments that are part of multi-unit firms

	LBD: Establishments that are Part of Multi-Unit Firms			NETS: Establishments that are Part of Multi-State Firms		
	(1)	(2)	(3)	(4)	(5)	(6)
2009 shock * 2010	0.0023	0.0015	0.0018			
	(0.0017)	(0.0014)	(0.0016)			
2009 shock * 2009	0.0011	0.0012	0.0012	0.0038	-0.0034	-0.0028
	(0.0011)	(0.0011)	(0.0012)	(0.0050)	(0.0035)	(0.0037)
2008 shock * 2010	0.0017	-0.0029	-0.0034			
	(0.0016)	(0.0015)	(0.0016)			
2008 shock * 2009	0.0020	0.0000	-0.0003	0.0151	-0.0005	-0.0015
	(0.0014)	(0.0013)	(0.0013)	(0.0039)	(0.0033)	(0.0036)
2008 shock * 2008	-0.0003	-0.0010	-0.0009	-0.0041	-0.0100	-0.0100
	(0.0015)	(0.0014)	(0.0015)	(0.0060)	(0.0057)	(0.0060)
Cumulative effect of 2008 shock	0.0034	-0.0039	-0.0047	0.0110	-0.0105	-0.0115
	(0.0034)	(0.0030)	(0.0031)	(0.0070)	(0.0063)	(0.0067)
Cumulative effect of 2009 shock	0.0035	0.0028	0.0030	0.0038	-0.0034	-0.0028
	(0.0023)	(0.0021)	(0.0024)	(0.0050)	(0.0035)	(0.0037)
F-test of joint significance of shock interactions (p-value)	0.18	0.26	0.29	0.00	0.25	0.29
Observations	43503	42406	30842	40184	39142	28678
State-by-year fixed effects	X	X	X	X	X	X
Baseline controls		X	X		X	X
Debt-to-income ratio			X			X

Notes: Entries are based on estimation of Equation (8) where the dependent variable is the employment growth rate for small establishments that are part of multi-unit firms. Small establishments are defined to be those with less than 20 employees. Columns (1)-(3) use the LBD data, which extends through 2010. Columns (4)-(6) use the NETS data, which extends only through 2009. Standard errors clustered on county in parentheses. An observation is a county-by-year cell. Shocks refer to predicted loan originations as specified in Equation (6). Baseline controls are 2006 log density, log population, construction share, manufacturing share, and log per capita income. All controls are interacted with year dummies. All main effects are included. See the text for further details.

Table 7: Effect of predicted lending shock on county aggregate outcomes

	Employment growth			Establishment growth		
	(1)	(2)	(3)	(4)	(5)	(6)
2009 shock * 2010	0.0006	0.0008	0.0007	0.0006	0.0012	0.0013
	(0.0009)	(0.0009)	(0.0010)	(0.0005)	(0.0004)	(0.0005)
2009 shock * 2009	0.0027	0.0026	0.0027	0.0007	0.0015	0.0016
	(0.0011)	(0.0011)	(0.0012)	(0.0005)	(0.0005)	(0.0006)
2008 shock * 2010	0.0017	0.0002	0.0004	-0.0012	-0.0013	-0.0015
	(0.0011)	(0.0010)	(0.0011)	(0.0007)	(0.0005)	(0.0006)
2008 shock * 2009	-0.0012	-0.0003	-0.0010	-0.0002	-0.0003	-0.0007
	(0.0014)	(0.0012)	(0.0013)	(0.0008)	(0.0007)	(0.0007)
2008 shock * 2008	-0.0002	0.0012	0.0007	-0.0002	0.0006	0.0004
	(0.0010)	(0.0009)	(0.0010)	(0.0005)	(0.0004)	(0.0005)
Cumulative effect of 2008 shock	0.0004	0.0012	0.0001	-0.0016	-0.0011	-0.0018
	(0.0028)	(0.0024)	(0.0026)	(0.0018)	(0.0014)	(0.0015)
Cumulative effect of 2009 shock	0.0033	0.0034	0.0034	0.0013	0.0027	0.0028
	(0.0017)	(0.0017)	(0.0019)	(0.0010)	(0.0009)	(0.0010)
F-test of joint significance of shock interactions (p-value)	0.017	0.176	0.313	0.111	0.001	0.010
Observations	42947	41973	30830	42947	41973	30830
State-by-year fixed effects	X	X	X	X	X	X
Baseline controls		X	X		X	X
Debt-to-income ratio			X			X

Notes: Entries are based on estimation of Equation (8) where the dependent variables are, respectively, county-level employment and establishment growth. We use the average of the growth rates from the CBP and QCEW. Standard errors clustered on county in parentheses. An observation is a county-by-year cell. Shocks refer to predicted loan originations as specified in Equation (6). Baseline controls are 2006 log density, log population, construction share, manufacturing share, and log per capita income. All controls are interacted with year dummies. All main effects are included. See the text for further details.

Table 8: Effect of predicted lending shock on small business employment by year

	Log originations (1)	Small Standalones (LBD) (2)	All Private employment (CBP/QCEW) (3)
Shock (t)	0.0492	-0.0006	0.0005
	(0.0030)	(0.0005)	(0.0004)
Shock (t-1)	0.0337	0.0002	0.0007
	(0.0031)	(0.0003)	(0.0003)
Shock (t) *2008	0.0285	0.0005	-0.0010
	(0.0083)	(0.0011)	(0.0010)
Shock (t-1) *2009	0.0410	0.0001	-0.0008
	(0.0109)	(0.0013)	(0.0008)
Shock (t) *2009	0.0465	0.0028	0.0009
	(0.0088)	(0.0011)	(0.0010)
Shock (t-1) *2010	0.0860	0.0010	-0.0008
	(0.0091)	(0.0009)	(0.0012)
Total effect of 2008 shock	0.1524	0.0002	-0.0006
	(0.0144)	(0.0018	(0.0014)
Total effect of 2009 shock	0.2154	0.0035	0.0013
	(0.0152)	(0.0015)	(0.0018)
Excess effect of the 2008 shock	0.0695	0.0006	-0.0018
	(0.0158)	(0.0021)	(0.0014)
Excess effect of the 2009 shock	0.1326	0.0039	0.0001
	(0.0160)	(0.0016)	(0.0018)
F-test for joint significance of interactions (p-value)	0.00	0.09	0.24
Observations	30160	30300	29945

Notes: Entries are based on estimation of Equation (9). Standard errors clustered on county in parentheses. An observation is a county-by-year cell. Shocks refer to predicted lending shocks as calculated in Equation (6). The total effect of the 2008 shock is: shock(t) + shock(t-1) + shock(t)*2008 + shock(t-1)*2009. The total effect of the 2009 shock is: shock(t) + shock(t-1) + shock(t)*2009 + shock(t-1)*2010. The excess effect of the 2008 shock is: shock(t)*2008 + shock(t-1)*2009. The excess effect of the 2009 shock is: shock(t)*2009 + shock(t-1)*2010. All models include baseline controls (2006 log density, log population, construction share, manufacturing share, and log per capita income) interacted with year dummies. All main effects are included. See text for further details.

Table 9: Two Stage Least Squares Models of the Relationship Between Economic Activity and Small Business Loan Originations

	LBD (1)	CBP/QCEW (2)
ln(loan originations) (t)	0.0089	0.0203
	(0.0078)	(0.0080)
ln(loan originations) (t-1)	-0.0032	-0.0143
	(0.0073)	(0.0077)
Point estimate: upper bound impact of 2008-2009 credit supply reduction on 2008-2010 employment growth	-0.003	-0.003
Upper 95% CI: upper bound impact of 2008-2009 credit supply reduction on 2008-2010 employment growth	-0.012	-0.009
Angrist Pischke First-Stage F-stat (t)	52.23	51.88
Angrist Pischke First-Stage F-stat (t-1)	77.00	77.40
Observations	39359	39001

Notes: Entries show two stage least squares estimates of the relationship between small business lending and employment. The dependent variable in Column (1) is small business employment growth. The dependent variable in Column (2) is county-level employment growth. All models include state-by-year fixed effects along with baseline controls (2006 log density, log population, construction share, manufacturing share, and log per capita income) interacted with year dummies. All main effects are included. The upper bound impact of the 2008-2009 credit supply reduction on 2008-2010 employment growth is obtained by assuming the entire decline in small business lending observed over this period was supply-driven. See text for further details.

Appendix

Assumptions Underlying Modified Instrument

Our estimating equation of interest is of the form:

$$\Delta\ln(Y_i) = \gamma X_i + \beta \ln(Q_i) + \varepsilon_i,$$

where i indicates a county and Y_i is a county-level outcome variable. The outcome is a function of covariates, X_i, and loan originations in county i, $\ln(Q_i)$.

A standard formulation of the Bartik instrument is:

$$(A1)\ Z_i = \sum_j (ms_{ij} * (\Delta\ln(Q_j) - \Delta\ln(Q)),$$

where county i's value of the instrument is the product of (i) the beginning of period market shares of banks in its county and (ii) the difference between each bank's national change in lending and the economy-wide change in lending, summed over all banks in the county.

Consider a simple model of credit demand and supply where credit supply of a bank to a county is perfectly elastic and there is county heterogeneity in the elasticity of credit demand. The change in the log "price" of credit for bank j in county i can be written as

$$\Delta\ln(p_{ij}) = -\varepsilon_{ij}^S,$$

where ε_{ij}^S is a bank/county specific supply shock. We rewrite $\Delta\ln(Q_j)$ as:

$$\Delta\ln(Q_j) = \sum_i (ms^{ij}\varepsilon_i^D - \beta_i ms^{ij}\Delta\ln(p_{ij}))$$

$$= \sum_i (ms^{ij}\varepsilon_i^D + \beta_i ms^{ij}\varepsilon_{ij}^S)$$

$$= \sum_i ms^{ij} D_i + S_j,$$

where $\beta_i > 0$ is the county-specific demand elasticity for credit and ms^{ij} is the share of bank *j's* lending in county i. Note that we assume there are county-specific demand shocks but not bank-specific demand shocks. The implications of bank-specific shocks are discussed below. Given this model, the Bartik instrument can be written as

$$Z_i = \sum_j ms_{ij}(\sum_i (ms^{ij}\varepsilon_i^D + \beta_i ms^{ij}\varepsilon_{ij}^S) - \Delta\ln(Q)),$$

$$= \sum_j ms_{ij}\sum_i ms^{ij}\,\varepsilon_i^D + \sum_j ms_{ij}\,S_j - \Delta\ln(Q),$$

The first term is the average exposure of banks in county i to demand shocks in places where the banks operate. The second term is the average supply shocks across all banks in county i. When employing the Bartik instrument it is necessary to assume that both of these terms are uncorrelated with ε_i. However, it is immediately evident that Z_i is a function of ε_i^D so local demand shocks enter directly and are a possible threat to validity.

We instead employ a shift-share approach that is purged of local demand shocks for lending as a partial solution to this problem. Specifically, we estimate an equation that decomposes the contribution of the change in equilibrium credit to county and bank components:

$$\Delta\ln(Q_{ij}) = d_i + s_j + e_{ij},$$

where the outcome variable is the log change in small business lending by bank *j* in county *i*. The vector d_i is a full set of county fixed effects and the parameters of interest are those associated with the vector of bank fixed effects, s_j. They are estimates of change in bank credit purged of banks' differential geographic exposure to small lending shocks. We complement the standard shift-share (Bartik) approach by replacing the change in aggregate bank lending, $\Delta\ln(Q_j)$, in the construction of Z_i in Equation A1 with these estimated bank-specific supply shocks.

To see what we are identifying in estimating this equation with respect to the simple supply and demand model specified above, note that:

$$\hat{s}_j = \frac{1}{I}\sum\nolimits_{i=1}^{I}(\Delta\ln(Q_{ij}) - \Delta\ln(Q_{i\sim}))$$

$$= \frac{1}{I}\sum\nolimits_{i=1}^{I}(\varepsilon_i^D + \beta_i\varepsilon_{ij}^S - \frac{1}{J}\sum\nolimits_{j=1}^{J}(\varepsilon_i^D + \beta_i\varepsilon_{ij}^S))$$

$$= \frac{1}{I}\sum\nolimits_{i=1}^{I}\beta_i({}_i\,\varepsilon_{ij}^S - \frac{1}{J}\sum\nolimits_{j=1}^{J}\varepsilon_{ij}^S)$$

The estimated bank effect identifies the average supply response for bank *j* relative to the average supply response of banks in counties where *j* operates, weighted by the (possibly heterogeneous) demand elasticity.

The resulting instrument is:

$$Z_i = \sum\nolimits_{j=1}^{J} ms_{ij}(\tfrac{1}{I}\sum\nolimits_{i=1}^{I}\beta_i({}_i\,\varepsilon_{ij}^S - \tfrac{1}{J}\sum\nolimits_{j=1}^{J}\varepsilon_{ij}^S)).$$

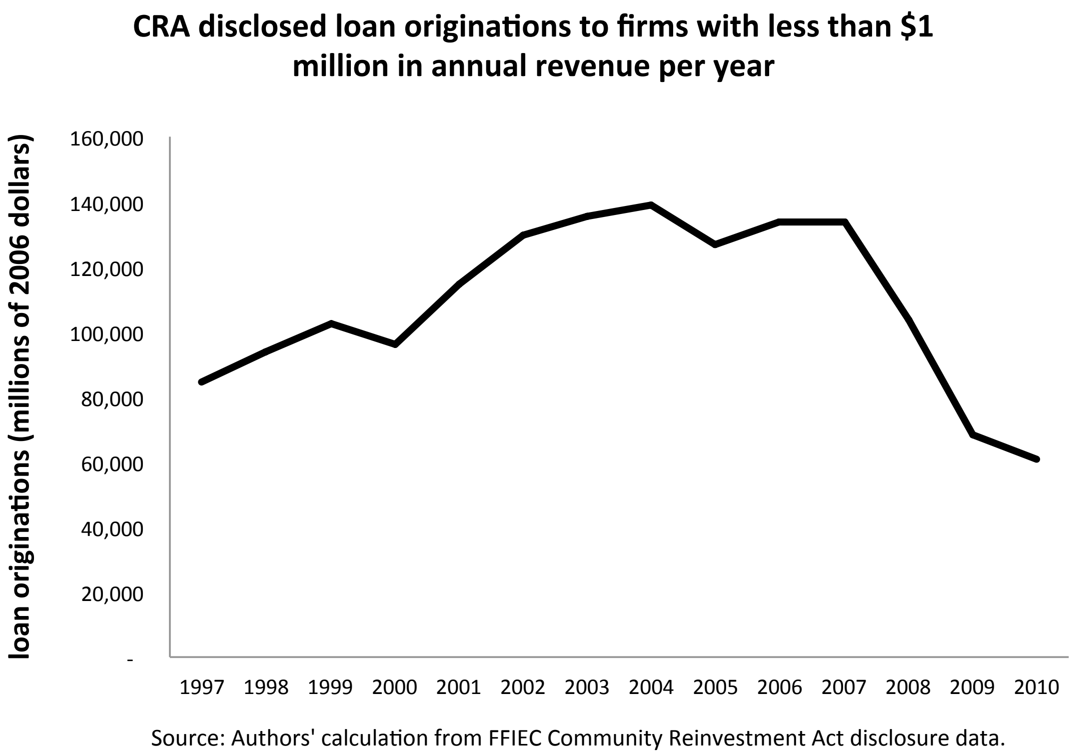
Under this approach, the required assumption is now weaker than when using the unadjusted approach, requiring that counties exposed to banks with above- or below-average supply shocks relative to county averages (weighted by demand elasticities) not have systematically above- or below-average shocks to outcomes. If demand elasticities are heterogeneous, we also require that the magnitude of the elasticity not be systematically related to outcomes.

We made the assumption that county demand shocks have no bank specific component. Suppose that credit demand is instead:

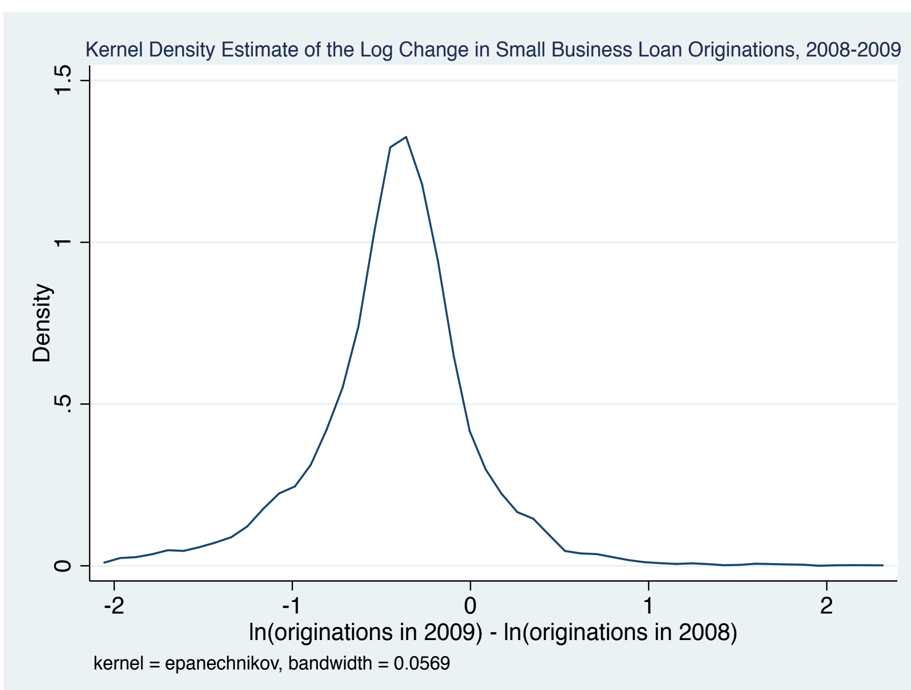
$$\Delta \ln(Q_{ij}) = \varepsilon_i^D + \varepsilon_j^D - \beta_i \Delta \ln(p_{ij}) = \varepsilon_i^D + \varepsilon_j^D + \beta_i \varepsilon_{ij}^S,$$

meaning that there is a bank specific demand shock that cannot be separately identified from the supply shock. This case would entail an added assumption for the validity of the instrument that banks that have a bigger demand shock are not more likely to be located in hard-hit areas. An example of how this might fail is if banks specialize in lending to certain industries, and if these industries decline relative to others, that will represent a national demand shock to the bank that might be correlated to county outcomes.

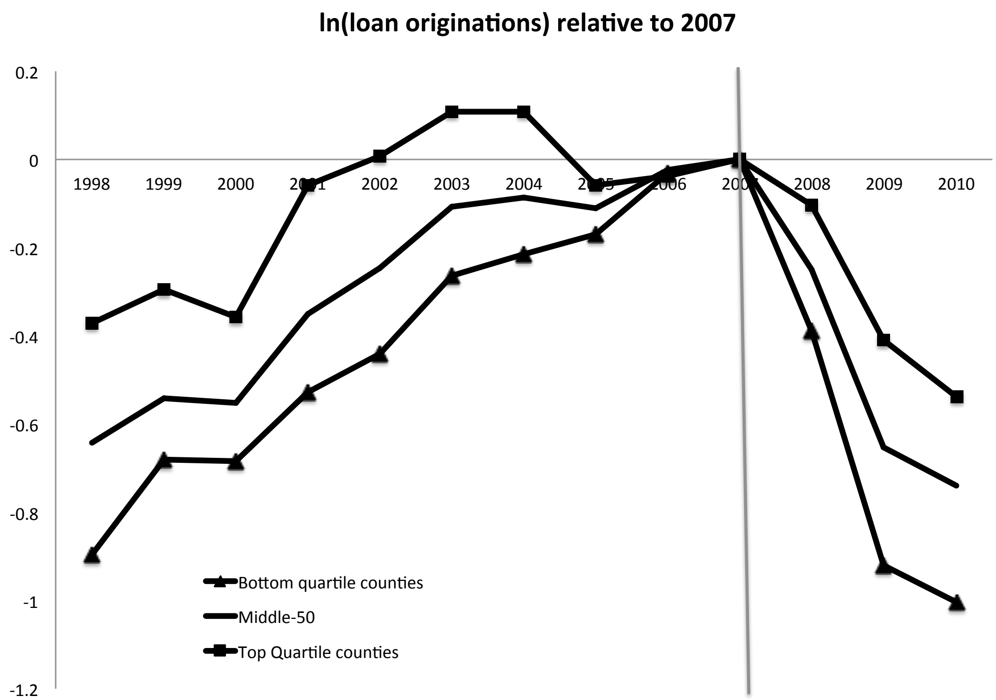
Appendix Figure 1



Appendix Figure 2



Appendix Figure 3



Notes: Figure shows small business loan originations in counties divided according to the value of their 2007-2009 predicted lending shock. See the text for further details.

Appendix Table 1: Main effects of the predicted lending shocks

	(1)	(2)	(3)
		Panel A: ln(loan originations)	
2008 shock	-0.3033	0.2754	0.2724
	(0.0753)	(0.0180)	(0.0199)
2009 shock	-0.1044	0.0128	0.0066
	(0.0612)	(0.0171)	(0.0185)
		Panel B: Small standalone firms (LBD)	
		Employment growth	
2008 shock	-0.0038	-0.0010	-0.0007
	(0.0012)	(0.0010)	(0.0010)
2009 shock	-0.0011	-0.0010	-0.0009
	(0.0008)	(0.0007)	(0.0008)
		Establishment growth	
2008 shock	-0.0094	0.0027	0.0049
	(0.0052)	(0.0047)	(0.0052)
2009 shock	-0.0082	-0.0069	-0.0068
	(0.0045)	(0.0042)	(0.0045)
		Panel C: County-level aggregates	
		Employment growth	
2008 shock	-0.0004	0.0005	0.0005
	(0.0006)	(0.0005)	(0.0006)
2009 shock	-0.0002	-0.0007	-0.0007
	(0.0005)	(0.0005)	(0.0005)
		Establishment growth	
2008 shock	-0.0010	0.0001	0.0001
	(0.0005)	(0.0005)	(0.0005)
2009 shock	-0.0006	-0.0007	-0.0007
	(0.0005)	(0.0004)	(0.0004)
State-by-year fixed effects	X	X	X
Baseline controls		X	X
Debt-to-income ratio			X

Notes: Entries are based on estimation of Equation (8). The dependent variables are, respectively, log small business loan originations, small standalone firm employment and establishment growth rates, and county-level aggregate employment and establishment growth rates. Standard errors clustered on county in parentheses. An observation is a county-by-year cell. Shocks refer to predicted loan originations as specified in Equation (6). Baseline controls are 2006 log density, log population, construction share, manufacturing share, and log per capita income. All controls are interacted with year dummies. All main effects are included. See the text for further details.

Appendix Table 2: Relationship between predicted lending shock and ln(loan originations) for non-CRA banks

	ln(loan originations)
2009 shock * 2009	0.026
	(0.026)
2008 shock * 2009	-0.024
	(0.030)
2008 shock * 2008	-0.055
	(0.031)
Observations	29284

Notes: This table tests whether areas with larger credit shocks experienced increased lending from banks not covered by the CRA. The unit of analysis is commercial banks that are below the CRA asset threshold. The dependent variable is small loan balances from FDIC Call Reports. Standard errors clustered on county in parentheses. See text for further details.

Appendix Table 3: Effect of predicted lending shock on employment and establishment growth rates for small establishments, NETS data

	Employment growth rate		Establishment growth rate	
	(1)	(2)	(3)	(4)
2009 shock * 2009	0.003	0.002	-0.005	0.003
	(0.001)	(0.001)	(0.004)	(0.001)
2008 shock * 2009	0.009	0.005	0.021	0.005
	(0.002)	(0.001)	(0.004)	(0.002)
2008 shock * 2008	-0.001	0.002	-0.005	0.002
	(0.001)	(0.001)	(0.002)	(0.001)
Cumulative effect of 2008 Shock	0.008	0.007	0.016	0.007
	(0.002)	(0.002)	(0.004)	(0.002)
Cumulative effect of 2009 Shock	0.003	0.002	-0.005	0.003
	(0.001)	(0.001)	(0.004)	(0.001)
F-test of joint significance of shock interactions (p-value)	0.00	0.00	0.00	0.00
Observations	40287	28678	40287	28678
State-by-year fixed effects	X	X	X	X
Baseline controls		X		X
Debt-to-income ratio		X		X

Note: Entries are based on estimation of Equation (8) where the dependent variable is, respectively, the employment or establishment growth rate for small establishments. Small establishments are defined to be those with less than 20 employees. These estimates use the NETS data, which only extends through 2009. Standard errors clustered on county in parentheses. An observation is a county-by-year cell. Shocks refer to predicted loan originations as specified in Equation (6). Baseline controls are 2006 log density, log population, construction share, manufacturing share, and log per capita income. All controls are interacted with year dummies. All main effects are included. See the text for further details.

Appendix Table 4: OLS Models of the Relationship Between Economic Activity and Small Business Loan Originations

	LBD	CBP/QCEW
	(1)	(2)
ln(loan originations) (t)	0.0007	0.0010
	(0.0010)	(0.0009)
ln(loan originations) (t-1)	-0.0026	-0.0028
	(0.0011)	(0.0008)
Observations	39359	39001

Notes: Entries show OLS estimates of the relationship between small business lending and employment. The dependent variable in Column (1) is small business employment growth. The dependent variable in Column (2) is county-level employment growth. All models include state-by-year fixed effects along with baseline controls (2006 log density, log population, construction share, manufacturing share, and log per capita income) interacted with year dummies. All main effects are included. See text for further details.