Do Credit Market Shocks Affect the Real Economy? Quasi-experimental Evidence from the Great Recession and "Normal" Economic Times

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Source: *American Economic Journal: Economic Policy* , February 2020, Vol. 12, No. 1 (February 2020), pp. 200-225

Published by: American Economic Association

Stable URL: https://www.jstor.org/stable/10.2307/26888552

American Economic Journal: Economic Policy 2020, 12(1): 200–225
https://doi.org/10.1257/pol.20160005

Do Credit Market Shocks Affect the Real Economy? Quasi-experimental Evidence from the Great Recession and "Normal" Economic Times†

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Using comprehensive data on bank lending and establishment-level outcomes from 1997–2010, this paper finds that small business lending is an unimportant determinant of small business and overall economic activity. A shift-share style research design is implemented to predict county-level lending shocks using variation in preexisting bank market shares and bank supply shifts. Counties with negative predicted lending shocks experienced declines in small business loan originations, indicating that it is costly to switch lenders. However, small business loan originations have an economically insignificant and generally statistically insignificant impact on both small firm and overall employment during the Great Recession and normal times. (*JEL* E32, E44, E52, G21, G32, L25)

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

This paper gauges the bank lending channel's empirical importance by separately measuring the economic consequences of shocks to small business credit during

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† Go to https://doi.org/10.1257/pol.20160005 to visit the article page for additional materials and author disclosure statement(s) or to comment in the online discussion forum.

the 2007–2009 recession and during "normal" economic times (i.e., 1997–2007). It is ex ante unclear whether bank lending 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 US government and the Federal Reserve Board aggressively injected liquidity into financial markets.

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. 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. For example, Citigroup reduced small business lending by 84 percent between 2007 and 2009, while US Bancorp's small business lending declined by just 3 percent. The essence of our approach is to ask whether counties with more Citigroup branches than US 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.

We use comprehensive data on both bank lending and real outcomes to show three primary findings. First, small firms that operated in markets served by unhealthy banks experienced substantial declines in loan originations during the Great Recession. For example, a one standard deviation reduction in predicted lending in 2009 is associated with a 17 percent, or $11 million on average, reduction in total county-level small business loan originations from 2009 through 2010, while a comparable lending shock in 2008 is associated with a 24 percent reduction from 2008 through 2009. Thus, during the Great Recession, the data strongly reject the null hypothesis that small firms are able to costlessly switch between bank lenders.

Second, the effects of these predicted negative lending shocks on the employment growth rate among small stand-alone firms (i.e., single-unit establishments with fewer than 20 employees) and county-level employment during the Great Recession is economically close to zero and generally statistically insignificant. Put another way, the elasticities of these employment measures with respect to small business loan originations are all less than 0.025. Furthermore, we estimate that, as an upper bound, these lending shocks can account for just 6 percent of the overall decline in small business employment (or 92,000 jobs) in this period and 4 percent of the total decline in employment (or 360,000 jobs). 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 US government to aid banks.

Finally, we show there is also a significant relationship between predicted lending shocks and bank loans to small businesses during the 1997–2007 period. Thus, we can also reject the null hypothesis that small firms are able to costlessly switch between lenders during "normal" economic times. As during the Great Recession, however, these predicted shocks are not associated with changes in small business or overall county-level employment. The elasticities of these measures of employment with respect to small business loan originations are both qualitatively zero, economically and statistically.

Overall, these findings contradict the conventional wisdom that the bank lending channel is an important determinant of small business and county-level economic activity. Specifically, we can rule out economically meaningful effects in normal times as well as during the Great Recession. It is worth noting, however, that our measures of bank supply shocks only affect lending from banks to small businesses; it seems plausible that small firms may have access to other sources of credit that are not prohibitively expensive when bank lending opportunities decline.

This paper 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 2016; Bloom et al. 2018), and structural factors (Charles, Hurst, and Notowidigdo 2016). 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 are other firms (Krueger and Charnes 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.[1]

Our paper also makes several contributions to the literature on the bank lending channel. 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 United States have focused on subsamples defined by particular sets of firms, episodes, or regions (e.g., Chodorow-Reich 2014, Peek and Rosengren 2000, Ashcraft 2005). Second, while other papers have focused on small firms, which are more likely to be affected by bank supply decisions (e.g., Duygan-Bump, Levkov, and Montoriol-Garriga 2015), our paper 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 United States. Third, we utilize a research design that allows us to control for confounding demand factors that may have affected employment growth. Finally, our conclusion that the bank lending channel does not have a meaningful impact on the real economy stands in contrast to much of the existing literature and, at the least, suggests that caution is warranted when advocating for policies to increase banks' credit supply to small businesses.

I. Background

The theory that banks are critical suppliers of credit for small businesses centers around the idea that it is costly for lenders to obtain information about these firms. Direct measurement of these costs cannot be measured with available datasets, but

[1] 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."

the existing evidence is often supportive of this possibility (e.g., Nguyen 2019). 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; Hoshi, Kashyap, and Scharfstein 1990; Petersen and Rajan 1994). 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).

In the wake of the US housing market crash of 2008, 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 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 (i.e., businesses with less than $1 million in gross revenues) fell by 52 percent between 2007 and 2010 and that the Great Recession was geographically pervasive (see online Appendix Figures 1 and 2). 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, reached its nadir in 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, documents that new loans to large borrowers fell by 79 percent 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. This severe contraction in small business lending may have led to significant real economic effects given both the importance of small businesses in the US economy (using Census Business Dynamic Statistics, we calculate that, in 2007, firms with less than 100 employees represented approximately 36 percent of employment and 20 percent of net job creation in the United States[2]) and their dependence on local bank credit.

II. 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 United States. The predicted lending shock is constructed using Community Reinvestment Act (CRA) disclosure data from the Federal Financial Institutions Examination Council (FFIEC). The CRA requires banks above a certain asset

[2] We note that while it is a widespread belief amongst policymakers that small businesses (and, thus, small business lending) are an important engine of economic growth, there is considerable debate in the academic literature regarding the importance of small firms for net job creation—see e.g., Neumark, Wall, and Zhang (2011) and Haltiwanger, Jarmin, and Miranda (2013).

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. We estimate that, in 2007, CRA eligible banks accounted for approximately 86 percent of all loans under \$1 million.[3]

The FFIEC provides data by bank, county, and year. As a measure of small business lending, we use the dollar amount of small business loan originations to businesses with \$1 million or less in annual gross revenue ($\approx$13 percent of total originations in 2007). 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 aggregate banks to the holding company level. We use the FDIC institution directory to identify acquisitions and the FDIC call reports to link banks to their holding companies. 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 nonpublic microdata from the near universe of establishments in the US Census Longitudinal Business Database (LBD) (US Census Bureau 2011). 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.[4] Specifically, for a given size category i, we define employment growth between $t-1$ and t in a given county as

$$(1) \qquad \textit{Employment growth rate}_{it}$$

$$= \big[\textit{jobs created by new establishments}_{it}$$

$$- \textit{jobs lost from closing establishments}_{it}$$

$$+ \textit{employment in continuing establishments}_{it}$$

$$- \textit{employment in continuing establishments}_{i,t-1}\big]/$$

$$\big[0.5 \times \textit{employment}_{it-1} + 0.5 \times \textit{employment}_{it}\big].$$

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

[3] 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.

[4] An example may help to clarify this measure. 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 establishment would contribute to the 2007–2008 growth rate for the 21 to 150 employee category and to the 2008–2009 growth rate for the 20 or fewer category.

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 natural log differences.

Similarly, we compute the establishment growth rate as

$$(2) \qquad \textit{Establishment growth rate}_{it} = \frac{[\textit{new establishments}_{it} - \textit{closing establishments}_{it}]}{[0.5 \times \textit{establishments}_{it-1} + 0.5 \times \textit{establishments}_{it}]}.$$

We use the LBD microdata to compute these measures. Additionally, we use a special extract of the NETS database, which is compiled by Walls and Associates (Walls 2007) using Dun and Bradstreet's Market Identifier files, to construct employment and establishment growth rates for all small stand-alone 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). The NETS database is 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). Since the CBP and QCEW have very limited information on firm size, we use them exclusively for county-level analyses.

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, courtesy of Amir Sufi.

III. Research Design

A. *Design Specifics*

Our research design, which is in the spirit of the "Bartik instrument," 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.[5] In the context of lending shocks, Peek and Rosengren (2000) is the most direct antecedent to our approach. 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 percent nationally over this period, the table reveals considerable differences across individual banks.

Our identification strategy exploits heterogeneity in counties' exposure to different banks (as measured by their pre-shock market shares) under the testable assumption that firms can only incompletely substitute to other banks in response to a reduction in credit supply from their current lender. Accordingly, a supply shock to a subset of banks in a given region will affect aggregate bank lending in that area.

[5] See Bowen and Finegan (1969), Bartik (1991), Blanchard and Katz (1992), Card (2001), Autor and Duggan (2003), and Notowidigdo (2011) for applications of this approach.

Table 1—Changes in Lending between 2007–2009 for Selected Large Bank Holding Companies

	Percent change in small business lending (1)	Percentile (2)	Percentile net of county fixed effects (3)
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		

Notes: Column 1 is the percent change in lending to firms with less than \$1 million 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.

We test this assumption empirically, but the numerous papers cited above provide evidence of such frictions.

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 first estimate an equation that decomposes the contribution of the change in equilibrium credit into county and bank components:

$$\Delta \ln(Q_{ij}) = d_i + s_j + e_{ij}, \tag{3}$$

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, the parameters s_j are estimates of changes in bank j's supply of credit that are purged of banks' differential exposure to county-level variation in credit demand.[6,7] The s_js are estimated for every pair of consecutive years beginning in 1997 and are re-centered so that their (bank asset size weighted) mean is zero.

[6] In the online 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 elasticity.

[7] Khwaja and Mian (2008) uses a similar methodology to purge firm-specific credit demand shocks for matched bank-firm lending data from Pakistan. See also Amiti and Weinstein (2018).

For each pair of consecutive years, we use the estimated bank fixed effects, $\hat{s}_j$, to calculate a county-level predicted lending shock as follows:

$$Z_i = \sum_j \left(ms_{ij} \times \hat{s}_j \right), \tag{4}$$

where ms_{ij} is bank j's market share in county i in the first of the consecutive years. For the estimation, we standardize the county-level predicted shock, Z_i, using its mean and standard deviation, and weight by county-level lending in the base year.

B. *Probing the Validity of the Research Design*

The validity of the research design rests on the assumption that counties exposed to banks with above- or below-average lending supply shocks, relative to county averages, not have systematically above- or below-average shocks to outcomes.[8] This subsection brings together a wide variety of evidence to explore the validity of this assumption and describes approaches that we use to further probe it in subsequent sections.

First, Table 2 reports summary statistics of county characteristics based on whether the county's value of the predicted lending shock is above or below the median. Columns 1 and 2 are the raw means with no adjustments, and column 4 is the within-state difference after purging 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 (below median) predicted lending shocks have different characteristics compared to counties with better (above median) predicted shocks. This is not surprising given the spatial patterns seen in Figure 1. Column 4 shows that, when looking within states, counties with above- and below-median predicted shocks look more similar although some statistically significant differences remain. These differences imply that the exogeneity assumption is more likely to be valid conditional on these variables. Consequently, our main analysis will emphasize specifications that include state fixed effects and that control for a large set of county characteristics interacted with year dummies.

Second, we calculate the correlation between county i's fixed effect (d_i in equation (3)) and the market-share-weighted average fixed effect of banks located in that county (i.e., the market-share weighted average of the s_js in equation (3)), 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, and is consistent with the assumption that the cumulative supply response of banks in a county is uncorrelated with local economic shocks.

Third, Table 3 further assesses the degree to which unhealthy banks may have nonrandomly 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

[8] Borusyak, Hull, and Jaravel (2018) establishes conditions under which identification is achieved through quasi-random assignment of shocks. In the Goldsmith-Pinkham, Sorkin, and Swift (2018) framework, the identification assumption is exogeneity of shares.

Table 2—County Characteristics

	Above median in predicted lending shock (1)	Below median in predicted lending shock (2)	p-value on difference (3)	Above median − below median in predicted lending shock (within-state) (4)	p-value on within-state difference (5)
Employment growth 2002–2006 [$N = 3{,}117$]	0.042	0.042	0.970	0.005	0.351
	[0.114]	[0.132]		(0.005)	
Wage growth 2002–2006 [$N = 3{,}117$]	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)	
Percent change total bank lending 2002–2006 [$N = 3{,}138$]	0.022	0.108	0.001	−0.039	0.168
	[0.609]	[0.772]		(0.028)	
log median per capita income 2006 [$N = 3{,}140$]	10.585	10.580	0.624	0.012	0.172
	[0.220]	[0.270]		(0.009)	
Poverty rate 2006 [$N = 3{,}141$]	15.363	15.507	0.555	−0.064	0.767
	[5.950]	[6.728]		(0.217)	
Construction share 2006 [$N = 3{,}115$]	0.066	0.064	0.484	0.002	0.335
	[0.043]	[0.051]		(0.002)	
Manufacturing share 2006 [$N = 3{,}073$]	0.180	0.149	0.000	0.006	0.208
	[0.129]	[0.136]		(0.005)	
ln(density in 2006) [$N = 3{,}113$]	−10.860	−11.190	0.000	0.309	0.000
	[1.567]	[1.929]		(0.055)	
ln(population in 2006) [$N = 3{,}114$]	10.347	10.098	0.000	0.340	0.000
	[1.303]	[1.670]		(0.052)	
Debt-to-income ratio 2006 [$N = 2{,}219$]	1.540	1.651	0.000	−0.090	0.000
	[0.529]	[0.689]		(0.025)	

Notes: Standard deviations are 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.

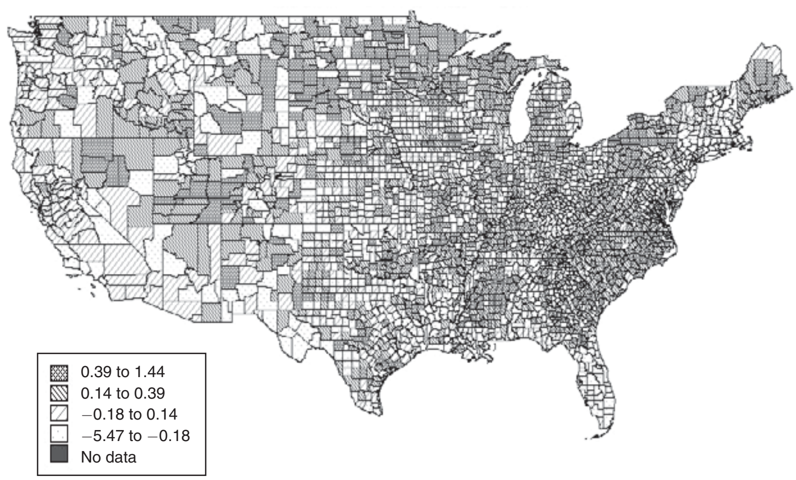

Figure 1. Adjusted Predicted Credit Shock

TABLE 3—TESTING FOR SPATIAL SORTING IN BANK LENDING SHOCKS

	County-level data (1)	Bank-level data (2)
	Dependent variable: Fixed effect of the bank with the largest market share in the county	Dependent variable: Bank fixed effect
Constant	0.527	0.618
	(0.039)	(0.031)
Fixed effect of the bank with the second largest market share in the county	−0.056	
	(0.060)	
Average competitor bank fixed effect in counties where the bank operates		0.078
		(0.111)
Observations	3,121	654
R^2	0.0042	0.001

Notes: Robust standard errors are 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.

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.

Fourth, we examine whether our measure of the regional lending shock is correlated with federal policy responses to the recession. To do this, we relate American Recovery and Reinvestment Act of 2009 expenditures and transfer payments by county with our measure of the predicted lending shock. The correlation is approximately zero.[9]

Fifth, we explore whether our estimates are sensitive to adjustment for a rich set of covariates. Perhaps the most important of these are state-by-year fixed effects, which mean the effect of the predicted lending shocks rely on comparisons within state by year cells. Figure 1 is a map of the United States where counties' shading reflects the quartile of their predicted lending shock.[10] While the regional correlation is evident, the key takeaway is that there is substantial within-state variation in the value of the predicted shock, indicating that it is possible to rely on within-state by

[9] Recovery Act expenditures are computed by Propublica from data scraped from the recovery.gov website, which listed all Recovery Act contracts. Transfer payments are from the Bureau of Economic Analysis. See the online Appendix for details. Other policy responses include the Toxic Assets Relief Program and measures by the Federal Reserve. It is possible that these responses encouraged banks to resume lending. However, the estimated shocks include any effects of the policy response that operated through banks, which were obviously not entirely successful given the large estimated decline in lending. The estimates are the effects of the shock after any offsetting response due to policy.

[10] Here, we use a version of the predicted lending shock that is computed for the entire 2007–2009 period—that is, 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.

year comparisons. We also show our results are robust to a wide set of county characteristics that are known to be predictors of the severity of the economic downturn.

Finally, we estimate all models on a sample of small (nonfranchise) establishments that are part of larger multi-unit and multi-state firms. These establishments' 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 establishments located in the same county. As shown below, we fail to find a significant relationship between the instrument and these establishments' outcomes across a wide variety of outcomes.

IV. 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 examine the effect of the predicted lending shocks on economic outcomes during the Great Recession and normal economic times (i.e., 1997–2007).

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

We begin with a graphical analysis where we divide counties into a top quartile, middle 50 percent, 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.

Figure 2 plots the $\phi_{t,k}$ from estimating

$$(5)\quad \ln(l_{it}) = p_{i,<25} + p_{i,>75} + \phi_{t,<25}p_{i,<25} + \phi_{t,25-75}p_{i,25-75} + \phi_{t,>75}p_{i,>75} + \epsilon_{it},$$

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 twenty-fifth percentile according to the value of its 2007–2009 predicted lending shock, $p_{i,25-75}$ is an indicator for whether the county lies in the middle 50 percent, and $p_{i,>75}$ is an indicator for being above the seventy-fifth percentile. Unless otherwise specified, all models in the paper are weighted by the county's 2006 employment count to account for heteroscedasticity due to differences in the variability of the dependent variable associated with the size of counties' economies.[11] The estimated $\phi_{t,k}$ 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.

Figure 2 shows small business loan originations fell for all three groups of counties in 2008 and 2009, but the drop was far more pronounced for the counties in the middle and bottom quartiles for the predicted 2007–2009 shock.

[11] Plotting the residuals from estimating equation (5) shows that smaller counties systematically have greater unexplained variability in the dependent variable.

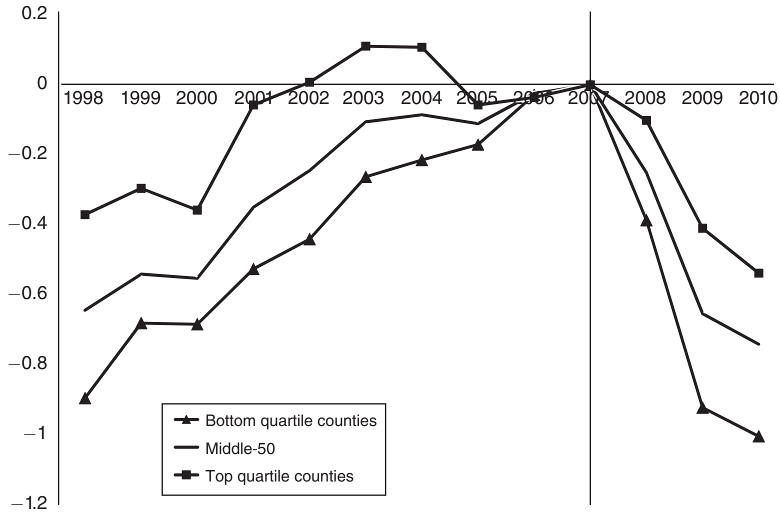

Figure 2. ln(Loan Originations) Relative to 2007

Notes: The figure is based on estimation of equation (5). Quartiles are based on the value of 2007–2009 predicted lending shock. See the text for further details.

Figure 3 is the regression-adjusted version of Figure 2 and is based on estimation of

$$\ln(l_{it}) = \delta_{st} + \beta_t X_{it} + \tau_{t,<25} p_{i,<25} + \tau_{t,25-75} p_{i,25-75} + \varepsilon_{it}, \tag{6}$$

where $p_{i,<25}$ is an indicator for whether the county is below the twenty-fifth 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 percent. 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 (log per capita income, construction share, manufacturing share, log population, log population density, and debt-to-income ratio) whose effects are allowed to vary by year. The state-by-year fixed effects mean that comparisons between the groups of counties are made within state for each year. 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 3, the line with triangle markers 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 that there is a strong relationship between predicted lending and loan originations even after these regression adjustments. Further, as predicted, the decline in lending is largest for the bottom quartile counties. Although there are differences in the level of loan originations between the three groups, the regression adjustment removes most of the difference in preexisting trends, especially during the 2000–2007 period.

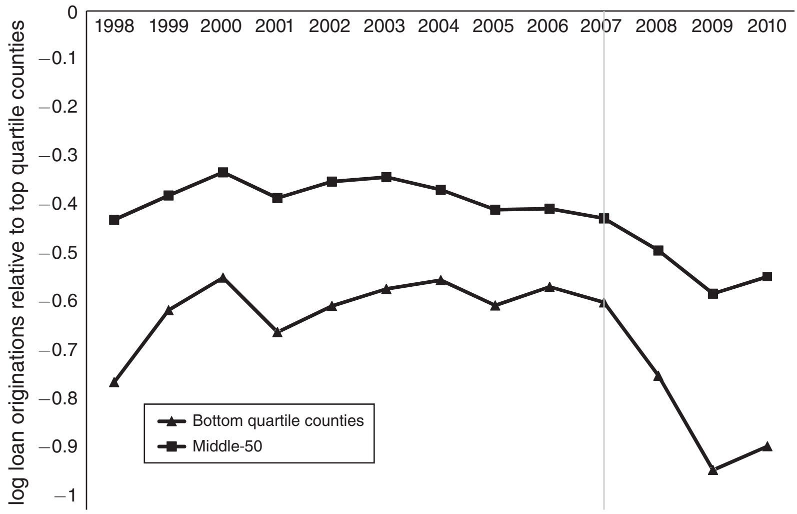

Figure 3. Regression-Adjusted Difference in ln(Loan Originations) between Top Quartile and Lower Quartile Counties

Notes: The figure is based on estimation of equation (6). Quartiles are based on the value of 2007–2009 predicted lending shock. See the text for further details.

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 (3) over consecutive pairs of years, rather than for the entire 2007–2009 period), and estimate versions of the following model:

$$\text{(7)} \quad \ln(l_{it}) = \delta_{st} + \beta_t X_{it} + \gamma_8 p_{i,2008} + \gamma_9 p_{i,2009} + \theta_{8,8}(\upsilon_{2008} \times p_{i,2008}) + \theta_{8,9}(\upsilon_{2009} \times p_{i,2008}) + \theta_{8,10}(\upsilon_{2010} \times p_{i,2008}) + \theta_{9,9}(\upsilon_{2009} \times p_{i,2009}) + \theta_{9,10}(\upsilon_{2010} \times p_{i,2009}) + \varepsilon_{it},$$

where $p_{i,\tau}$ is the standardized 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. We have also estimated models that include county fixed effects, which is another way to control for differences in annual loan originations across counties. Due to the strong similarity of the results, we emphasize the more parsimonious specification going forward. We report standard errors clustered at the county level to account for serial correlation.[12]

[12] We have also experimented with clustering by state. This increases the standard errors in some instances and reduces them in others, but the changes are not appreciable in magnitude.

The parameters of interest are the θs. 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 θs 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.

These linear combinations are presented in Table 4. Online Appendix Table 1 presents the corresponding γ estimates. 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, as suggested by Mian and Sufi (2014). We add debt-to-income in a separate specification, since it is not available for all counties.

The results in Table 4 confirm a robust and statistically significant relationship between the predicted lending shock and loan originations. 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 0.24 log points, or \$24 million, in 2008–2010. 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 0.17 log point, or \$11 million, reduction in loan originations over 2009–2010 relative to pre-crisis levels, as compared to the mean county. It is noteworthy that the estimates are largely unaffected by the choice of control variables.

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 small business bank lending in that area.[13]

[13] In online 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

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	43,358	42,224	30,884
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 (7) where the dependent variable is log loan originations. Standard errors clustered on county are in parentheses. An observation is a county-by-year cell. Shocks refer to predicted loan originations as specified in equation (4). 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. Specifications are weighted by 2006 county-level employment. See the text for further details.

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 by fitting versions of equation (7). Here 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 subsection since loan originations are an approximation to the preferred, but unobserved, outcome of changes in the outstanding value of loans to small businesses. As before, we emphasize estimates of the cumulative effects of the 2008 and 2009 predicted lending shocks.[14] Coefficients on all main effects and interaction terms are reported in the online Appendix.

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. Firms may, of course, still borrow from nonbank sources, which is one reason the effect of a bank lending shock may not have real effects.

[14] Our estimates for the effect of the lending shock on employment are robust to using the 2007–2009 shock to lending, as opposed to separately estimating the 2008 and 2009 shocks. We take the latter approach in order to estimate this effect during non-Great Recession periods.

TABLE 5—EFFECT OF PREDICTED LENDING SHOCK ON EMPLOYMENT AND ESTABLISHMENT GROWTH RATES FOR SMALL STAND-ALONE FIRMS

	Employment growth rate			Establishment growth rate		
	(1)	(2)	(3)	(4)	(5)	(6)
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)
Observations	43,540	42,420	30,842	43,540	42,420	30,842
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 (7). The dependent variable in columns 1–3 is the employment growth rate for small stand-alone firms calculated according to equation (1). The dependent variable in columns 4–6 is the establishment growth rate for small stand-alone firms calculated according to equation (2). Small stand-alone firms are defined to be single-unit establishments with fewer than 20 employees. Standard errors clustered on county are in parentheses. An observation is a county-by-year cell. Shocks refer to predicted loan originations as specified in equation (4). 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. Specifications are weighted by 2006 county-level employment. See the text for further details. Table 5 shows cumulative effects. Coefficients on all interaction terms are reported in online Appendix Table 3.

Small Stand-alone Firms.—Table 5 provides estimates for the effects of the supply shocks on the growth rates of small stand-alone firms, which are defined to be single-unit establishments with fewer than 20 employees. We estimate equation (7) where the dependent variable is either the employment or establishment growth rate for small stand-alones. As a basis for comparison, this set of firms experienced a 5.3 percent decline in employment between the end of 2007 and the end of 2010 and the number of these firms decreased by 4.8 percent, based on Census Business Dynamics Statistics data.

The estimates in columns 1–3 indicate that the predicted lending shocks did not have a meaningful impact on employment growth among these firms. The column 2 estimates for the total effect of the one standard deviation predicted lending shocks are close to zero and statistically insignificant in 2008 (i.e., ϕ_8) and indicate a 0.4 percent cumulative reduction (or a decline of 44 small business jobs in the average country) in 2009 (i.e., ϕ_9). These estimates correspond to a very small elasticity of small stand-alone employment with respect to small business lending—it is effectively zero when using the 2008 lending shock and only 0.024 when using the 2009 lending shock.[15] Finally, we note that adding the debt-to-income control in column 3 does not qualitatively 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).[16]

[15] We calculate the elasticity for the 2009 lending shock by using the ratio of the column 2 estimates from Tables 4 and 5: $0.024 = 0.004/0.1691$.

[16] Column 4 of online Appendix Table 3 reports estimates from a specification that include the following additional variables as controls: population growth from 2000–2006, fraction college educated, fraction minority, female labor force participation, elderly share of the population, and share foreign born. The additional controls

Columns 4–6 report the results from estimating the same models, but using the small stand-alone firm establishment growth rate (defined in equation (2)) as the dependent variable. These estimates range from marginally significant to insignificant across the specifications, though the standard errors are quite large. In the NETS data reported in online Appendix Table 4, we find smaller but more precise (and statistically significant) effects on business births and deaths. There is, therefore, some evidence that credit affects the formation and destruction of small businesses.[17]

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 (nonfranchise) 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.[18] We find, across all specifications, that the estimated effect of lending shocks for employment growth rates are insignificant. Moreover, online Appendix Table 5 shows the estimates on the interactions of the predicted lending shocks and year dummies are jointly insignificant. We conclude from this analysis that the credit shock variables are not picking up differential business-cycle effects across regions.

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 limited exposure to changes in the supply of lending from banks in a particular county. These estimates range from slightly negative (i.e., a negative lending shock increases employment) and statistically insignificant at conventional levels to negative and borderline significant.

In principle, general equilibrium forces could explain these negative results among multi-unit firms operating in multiple states. For example, the predicted lending shocks may enable these firms to take market share from establishments who are more dependent on credit from the local credit market. Alternatively, there may be a multiplier from the shock that negatively affects all firms in the area. Such indirect effects complicate the interpretation of these intra-county comparisons as a placebo test and mean that they should be interpreted cautiously.

County-Level Economic Outcomes.—Table 7 explores the relationship between the predicted small business lending shock and county-level employment and establishment growth, which declined by 6.7 percent and 4.3 percent, respectively, for the average county between 2007 and 2010. These estimates provide an opportunity

slightly attenuate the estimates of ϕ_8 and ϕ_9, thereby reinforcing the conclusion from Table 5 of economically small effects. The addition of these controls brings the estimate of ϕ_9 out of statistical significance at conventional levels.

[17] Column 8 of online Appendix Table 3 shows that with additional controls, the LBD estimates for ϕ_8 move into statistical significance at conventional levels.

[18] A caveat is that a local lending shock could propagate to unaffected units of the firm, as found in Giroud and Mueller (2017), in which case these firms may be imperfect placebos.

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)
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)
Observations	43,503	42,406	30,842	40,184	39,142	28,678
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 (7) 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 are in parentheses. An observation is a county-by-year cell. Shocks refer to predicted loan originations as specified in equation (4). 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. Specifications are weighted by 2006 county-level employment. See the text for further details. Table 6 shows cumulative effects. Coefficients on all interaction terms are reported in online Appendix Table 5.

to gauge the full county-level effect of credit supply shocks beyond the category of small firms, including any general equilibrium effects.

In columns 1–3, the outcome is the average of the employment growth rates from the CBP and QCEW 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, or 168 jobs, in the average county, while the 2008 shock's magnitude is about one-third and is not statistically significant.[19] These estimates correspond to a very small elasticity of county-level employment with respect to small business bank lending: 0.005 for the 2008 lending shock and 0.020 for the 2009 shock. It is not surprising that these coefficients and elasticities are smaller than the ones from the small establishment sample as we expect that a small business lending credit shock will have a larger impact on smaller firms. The clear finding across the employment regressions is that the estimated employment effects are only statistically significant in about half the instances and economically small in all cases.[20]

As was the case for the small establishment sample, 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. Here, too, it is apparent that even the elasticity associated with the 2009 lending shock is quite small (0.016).

[19] 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.

[20] Note, also, that online Appendix Table 6 shows the interactions between the predicted lending shocks and year dummies are only marginally significant in the joint test (p-value $= 0.18$).

Table 7—Effect of Predicted Lending Shock on County Aggregate Outcomes

	Employment growth			Establishment growth		
	(1)	(2)	(3)	(4)	(5)	(6)
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)
Observations	42,947	41,973	30,830	42,947	41,973	30,830
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 (7) 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 are in parentheses. An observation is a county-by-year cell. Shocks refer to predicted loan originations as specified in equation (4). 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. Specifications are weighted by 2006 county-level employment. See the text for further details. Table 7 shows cumulative effects. Coefficients on all interaction terms are reported in online Appendix Table 6.

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

$$\text{(8)} \qquad \ln(l_{it}) = \theta_1 p_{it} + \theta_2 p_{it-1} + \theta_3(\nu_{2008} \times p_{it}) + \theta_4(\nu_{2009} \times p_{it-1}) + \theta_5(\nu_{2009} \times p_{it}) + \theta_6(\nu_{2010} \times p_{it-1}) + \beta X_{it} + \lambda_i + \delta_{st} + \varepsilon_{it},$$

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 (4). This specification assumes that a shock has an effect over two periods, in t and $t+1$. In addition to reporting the estimated effect in a normal year $(= \theta_1 + \theta_2)$ 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. The overall growth rates between 2007 and 2010 were −52 percent (loan originations), −5.3 percent (employment at small stand-alones), and −7.4 percent (all private employment).

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

Table 8—Effect of Predicted Lending Shock on Employment by Year

	log originations (1)	Small stand-alones (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	30,160	30,300	29,945

Notes: Entries are based on estimation of equation (8). Standard errors clustered on county are in parentheses. An observation is a county-by-year cell. Shocks refer to predicted lending shocks as calculated in equation (4). 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. Specifications are weighted by 2006 county-level employment. See text for further details.

years of the sample. This means we can use the predicted lending shocks to test the relationship between lending and employment in noncrisis years. It is also apparent that there is a much larger and more precise effect of predicted lending on actual lending over the 2008–2009 period. Further, the estimated effects of the 2008 and 2009 shocks are very similar to those in Table 4, with the differences due to minor differences between equations (7) and (8) that accommodate the estimation of the impacts of lending shocks in years outside the Great Recession period.[21]

Column 2 presents estimates from equation (8) with the LBD small stand-alone employment growth outcome, while the outcome in column 3 is county-level employment. A few findings stand out. First, the shock terms' impact on small stand-alone employment and total employment during "normal" economic times (i.e., outside the Great Recession) are all economically small and, in three out of

[21] 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.

four cases, statistically insignificant. It is apparent that the elasticities of these two measures of employment with respect to small business loan originations are both qualitatively zero, economically and statistically. Second, the 2009 lending shock had a larger impact on small stand-alone employment than comparable shocks in other years, although the magnitude of this difference is small. Third, as found in Tables 5 and 7, the impacts of the lending shocks on both categories of employment are very small and generally statistically insignificant during the Great Recession.

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:

$$\ln\left(l_{it}\right) = \omega_{1t} p_t + \omega_{2t} p_{t-1} + \omega_{3t} p_{t+1} + \delta_{st} + \beta X_{it} + \nu_t + \lambda_i + \varepsilon_{it}. \tag{9}$$

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 4 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.[22]

Figure 5 is analogous to Figure 4 except the outcome variable is now small stand-alone employment growth. The figure produces little evidence of impacts on employment in either "normal" or Great Recession years. Overall, the evidence points to the conclusion that the small business lending channel was not an economically important determinant of small business or overall economic activity at any point over the 1997–2010 period.

V. Aggregate Implications

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, but it is not straightforward to apply estimates of the cross-sectional effects of these supply shocks to time-series variation in aggregate small business bank 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

[22] 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.

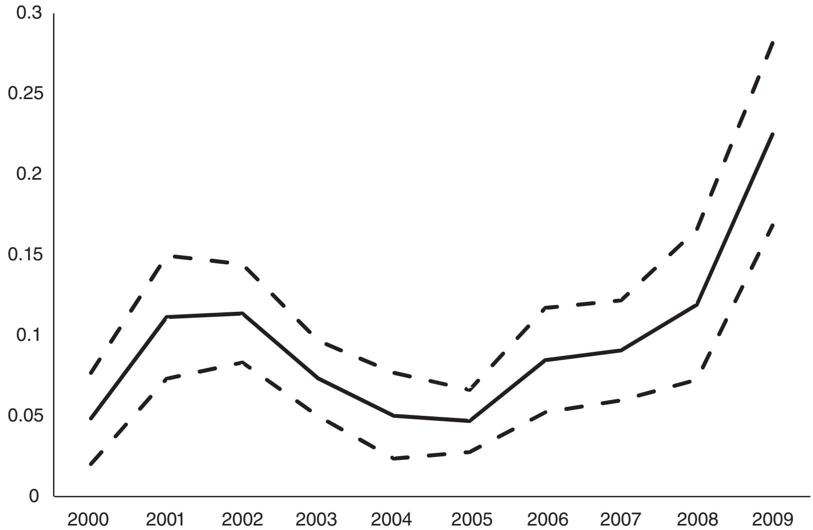

Figure 4. Effect of Predicted Lending Shock on Loan Originations by Year

Notes: The figure is based on estimation of equation (9) where the dependent variable is log small business loan originations. The *y*-axis shows the effect of a one standard deviation change in predicted log lending on log loan originations in t and $t + 1$. Dashed lines show 95 percent confidence intervals. See text for further details.

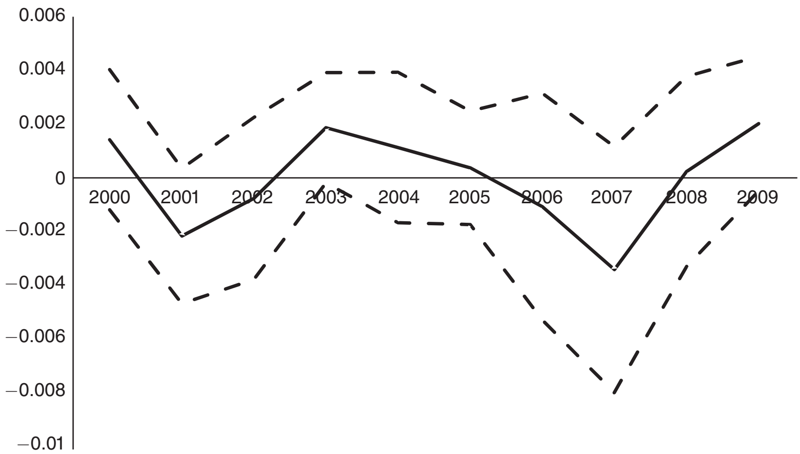

Figure 5. Effect of Predicted Lending Shock on Small Business Employment Growth by Year

Notes: The figure is based on estimation of equation (9) where the dependent variable is employment growth rate for small stand-alone firms, defined as single unit establishments with fewer than 20 employees. The *y*-axis shows the effect of a one standard deviation change in predicted log lending on small business employment growth in t and $t + 1$. Dashed lines show 95 percent confidence intervals. See text for further details.

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 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.

Extrapolating from treatment effects estimated at the local market level to national aggregates is always a delicate exercise and, to borrow the language of Kline and Moretti (2014), requires accounting for both the direct and indirect effects of treatment. A decline in local small business lending not only affects the county in which the shock hits, but potentially also affects surrounding counties via endogenous labor adjustment. While mindful of these concerns, we believe them to be limited in this particular context as our estimates suggest that even the direct local effects of our bank lending shocks are small.[23]

The first step in our procedure is to note that CRA-disclosed small business lending declined by 22 percent in 2008 and 33 percent 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 two-stage least squares (2SLS) models where small business employment and county-level employment growth rates are the dependent variables, 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 (9) 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.[24] 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 rates. Column 1 reports the estimates for employment growth at small establishments in the LBD. Importantly, the first-stage Angrist-Pischke F-statistics are well above conventional thresholds. As this is a bounding exercise, we are less concerned with statistical significance than with magnitudes and confidence bands. Neither contemporaneous nor lagged shocks have a statistically significant effect on small business employment growth. Further, they are economically very small; the elasticities are 0.009 (SE $=$ 0.008) and -0.003 (SE $=$ 0.007), respectively.

The 2SLS estimates imply that the national changes in small business lending (-22 percent in 2008 and -33 percent in 2009) resulted in 0.3 percentage points ($= 0.22 \times 0.009 + 0.33 \times 0.009 + 0.22 \times -0.003 + 0.33 \times -0.003$) 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 6 percent of the 5.3 percent decline in small business employment between the end of 2007 and 2010 for firms with fewer than 20 employees. Put another way, an upper bound estimate is that the

[23] Chodorow-Reich (2019) finds that for fiscal multipliers, estimates that use cross-sectional variation in spending are close to estimates of national multipliers.

[24] Online Appendix Table 7 reports the corresponding OLS models.

Table 9—Two-Stage Least Squares Models of the Relationship between Economic Activity and Small Business Loan Originations

	Small establishment employment growth (LBD) (1)	County-level employment (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 percent CI: upper bound impact of 2008–2009 credit supply reduction on 2008–2010 employment growth	−0.012	−0.009
Angrist-Pischke first-stage F-statistic (t)	52.23	51.88
Angrist-Pischke first-stage F-statistic ($t-1$)	77.00	77.40
Observations	39,359	39,001

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. Specifications are weighted by 2006 county-level employment. 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.

reduction in small business lending can only account for a reduction in 92,000 jobs out of the loss of 1.6 million small business jobs nationally.

Column 2 reports on aggregate county employment. Repeating the calculation from the previous paragraph suggests that the reduction in small business lending accounted for a decline in total employment of at most 4 percent of the 7.4 percent decline in total employment between the end of 2007 and the end of 2010. This upper bound estimate implies that small business lending shocks accounted for just 360,000 job losses out of the 8.8 million total decline in employment. These results indicate that the decline in small business loans was not a primary contributor to employment declines during the Great Recession.

VI. Conclusion

Applying a new identification strategy to what we believe is the most comprehensive dataset ever assembled to investigate the role of bank lending to small businesses on the real economy, we investigate the importance of the small business lending channel on employment and other measures of economic activity during the 1997–2010 period. We find that measures of local credit supply shocks are associated with sharp declines in total county-level small business loan originations during the Great Recession and during "normal" economic times (i.e., 1997–2007). This result indicates that, at least in the near term, it is costly for small businesses to switch bank lenders. With respect to impacts on the real economy, however, we find that small business loan originations have an economically, and generally statistically, insignificant impact on both small firm and overall employment during both the Great Recession *and* normal times.

Overall, this paper has applied a plausibly credible research design to comprehensive data and, with significant precision, found that the bank lending channel appears to be unimportant in the modern US economy. The absence of real economy impacts in response to the large lending shocks during the Great Recession is especially striking evidence against the presumption that the bank lending channel is a key determinant of economic activity. These results raise questions about the magnitude of benefits from policies that increase banks' credit supply, which naturally must be weighed against their costs, including the possibility that such programs increase banks' risk-taking.

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