The Employment Effects of Credit Market Disruptions: Firm-level Evidence from the 2008-09 Financial Crisis

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Abstract

This paper investigates the effect of bank lending frictions on employment outcomes. I construct a new dataset that combines information on banking relationships and employment at two thousand nonfinancial firms during the 2008-09 crisis. The paper first verifies empirically the importance of banking relationships, which imply a cost to borrowers that switch lenders. I then use the dispersion in lender health following the Lehman crisis as a source of exogenous variation in the availability of credit to borrowers. I find that credit matters. Firms that had pre-crisis relationships with less healthy lenders had a lower likelihood of obtaining a loan following the Lehman bankruptcy, paid a higher interest rate if they did borrow, and reduced employment by more compared to pre-crisis clients of healthier lenders. Consistent with frictions deriving from asymmetric information, the effects vary by firm type. Lender health has an economically and statistically significant effect on employment at small and medium firms, but the data cannot reject the hypothesis of no effect at the largest or most transparent firms. Abstracting from general equilibrium effects, I find that the withdrawal of credit accounts for between one-third and one-half of the employment decline at small and medium firms in the sample in the year following the Lehman bankruptcy.

1. Introduction

Does the health of banks on Wall Street affect economic outcomes on Main Street? In the wake of the 2008-09 financial crisis, this question has generated substantial interest among politicians, the popular press, and the public at large. The renewed interest partly reflects the deeply unpopular government support for financial institutions, which policymakers defended by arguing that not providing such support would have dire implications for jobs in sectors far removed from banking (see e.g. Bernanke 2008). Notwithstanding the policy interventions, bank lending to nonfinancial firms in the United States contracted significantly during the crisis, and the economy experienced the sharpest employment decline in sixty years.

This paper investigates the link between credit market frictions and employment. I construct a new dataset that merges the Dealscan syndicated loan database, which contains the borrowing history of both public and private firms that have accessed the syndicated loan market, with confidential employment data from the Bureau of Labor Statistics Longitudinal Database. The merged dataset contains information on employment outcomes and banking relationships at two thousand nonfinancial firms, ranging in size from fewer than fifty employees to more than ten thousand. I then compare employment outcomes at firms that had borrowed before the crisis from relatively healthy financial institutions with otherwise similar firms that had borrowed from lenders more adversely affected during the crisis.

The paper's methodological approach relies on two key facts established below. First, bank-borrower relationships are sticky. This means that firms that borrowed *before* the crisis from banks that did less lending *during* the crisis would have greater difficulty obtaining bank financing than firms that had borrowed from healthier lenders. Second, the origins of the 2008 crisis lay outside of the corporate loan sector, implying that the cross-sectional variation in banks' willingness to make corporate loans was plausibly orthogonal to the characteristics of each banks' pre-crisis borrowers.

To further clarify the logic of the exercise, consider the examples of U.S. Bankcorp and Credit Suisse, both active lenders in the syndicated market in the United States. During the financial crisis, Credit Suisse suffered large losses from exposure to mortgage-backed securities, and its stock price declined by 60 percent during 2007-08. U.S. Bankcorp had relatively little exposure to mortgage-backed securities, experienced one of the smallest stock price declines among major banks during the crisis, and in July 2009 became among the first banks to fully repay its TARP commitment to the U.S. Treasury. In the nine months between the bankruptcy of Lehman Brothers and the end of the recession, Credit Suisse reduced its lending in the syndicated market by about 79 percent relative to before the crisis, whereas U.S. Bankcorp reduced lending by only 14 percent. As a result, firms with pre-crisis lending

syndicates where U.S. Bankcorp had a lead role were nearly four times as likely to receive a loan during the crisis as firms with syndicates where Credit Suisse had a lead role.

To establish the causal effect of credit supply in the general case, I first document the importance of borrower-lender relationships in the syndicated loan market. In a syndicated loan, the "lead arrangers" set up the loan deal with the borrower and provide most of the financing, and recruit other "participant" lenders to provide the remainder of the funds. Among borrowers that have previously accessed the syndicated market, the empirical stickiness in borrower-lender relationships exceeds by a factor of seven what one would predict based only on lenders' market shares, indicating frictions to switching lenders.

I next discuss measures of bank health in order to isolate the effect of credit supply. Unfortunately, banks' internal cost of funds are not directly observable. Instead, I measure the relative health of a firm's lenders using the amount of lending to other borrowers during the crisis by the firm's pre-crisis syndicate. The validity of this measure relies on unobserved components of borrower demand not varying in the cross-section of lenders. Three pieces of evidence lend support to this assumption. First, the sample appears well-balanced on observable characteristics, including industry and county employment of the borrowers. Second, using the Khwaja and Mian (2008) within-firm estimator, I show that the estimated effect of lender health on the likelihood of borrowing does not change in specifications with and without borrower fixed effects. This result directly addresses the concern of unobserved borrower characteristics within the subsample of firms that obtain a new loan during the crisis. Third, placebo regressions corresponding to employment changes near the end of the last business cycle expansion and during the 2001 recession indicate that employment at firms that had pre-crisis relationships with less healthy lenders did not differ systematically from employment at other firms during the placebo periods.

I also report results that instrument for banks' overall lending. The instrument set exploits the fact that the financial crisis originated outside of the nonfinancial corporate sector. The first instrument follows Ivashina and Scharfstein (2010) in measuring exposure to Lehman Brothers through the syndicated market. The second captures exposure to mortgage backed securities through the loading of each bank's stock return on the ABX index. The third contains selected items from banks' balance sheet and income statement items chosen to avoid concerns of reverse causality coming through the corporate loan portfolio. Each of the proposed instruments has predictive power for the change in lending by the bank. In these specifications, the identification assumption becomes that less healthy banks as measured by the particular instrument did not also lend to corporate borrowers drawn from a different distribution of borrower health.

With the bank health measures in hand, I turn to the consequences of the sharp contrac-

tion in credit supply following the collapse of Lehman Brothers. Pre-crisis clients of banks in worse financial condition had a fifty percent lower likelihood of receiving a new loan or a positive modification in the nine months following the Lehman failure. Moreover, among banks that did obtain a loan, interest spreads increased more. These findings resemble those of Ivashina and Scharfstein (2010) and Santos (2011) using similar data, but differ from those papers by focusing on borrower outcomes rather than bank outcomes, thereby establishing that borrowers of weaker banks could not simply switch to healthier banks during the crisis.

I then present evidence that the effects in the loan market translated into effects on real outcomes for the borrowers. In the year following the Lehman Bankruptcy, employment at pre-crisis clients of lenders at the 10th percentile of bank health fell by roughly four to five percentage points more than at clients of lenders at the 90th percentile. The estimated magnitude changes little whether or not the bank's overall change in lending is instrumented using the measures described above. Moreover, the instruments separately yield quantitatively similar results despite relatively weak cross-correlations within the instrument set.

The data also suggest that the importance of the credit supply channel varied by firm type. A partial equilibrium aggregation exercise indicates that the credit channel can explain between one-third and one-half of the employment decline at small and medium-sized firms in the sample (fewer than one thousand employees) in the year following Lehman. In contrast, the data cannot reject that the relative availability of bank credit supply had no effect on relative employment outcomes at the largest firms, or at firms with access to the bond market. The finding of differential effects at large and small firms can serve as a specification check for the validity of the research design. It also may help to explain why total private sector employment in the United States declined by 30 percent more at small firms than large firms in the year following the Lehman bankruptcy.

The employment shortfall at firms that had borrowed from less healthy lenders can inform an estimate of the aggregate effects of the financial frictions under certain conditions. First, standard external validity concerns arise regarding the representativeness of the small and medium firms in the sample. Second, in general equilibrium, some demand shifts from the more credit-constrained to the less constrained firms, inducing an increase in labor demand at the less constrained firms. In the opposite direction, the reduction in aggregate expenditure caused by the financial crisis lowers labor demand at the less constrained firms. The data do not inform on the direction of the general equilibrium effects. Instead, in an online appendix I calibrate a general equilibrium model which suggests that under plausible conditions they may be quantitatively small. In that case, the partial equilibrium aggregation exercise also gives an estimate of the aggregate effect of the financial frictions.

The paper relates to a number of strands of literature in both macroeconomics and

finance. Bernanke (1983) renewed interest in the concept of a credit channel that translates shocks to lending institutions into outcomes in the real economy through the destruction of bank-specific intermediary capital. A "small versus large" literature has looked for evidence of the credit channel using the insight that due to greater asymmetric information or smaller buffer savings, smaller, less transparent borrowers should exhibit greater sensitivity to credit supply constraints (Gertler and Gilchrist 1994; Duygan-Bump et al. 2011). In parallel, a "natural experiment" literature has studied shocks that induce variation in the cross-section of credit availability.[1]

The natural experiment papers robustly find contractions in lending at affected banks. However, firms facing a withdrawal of credit from one financing source may be able to substitute with financing from an alternative source (Becker and Ivashina Forthcoming; Adrian et al. 2013). Data limitations have made it difficult to show that the shocks affect real borrower economic outcomes. Two exceptions, Gan (2007) and Almeida et al. (2012), find contractions in investment at affected borrowers, but, in violation of the logic of the small versus large literature, necessarily restrict attention to firms that have regulatory filings with borrower-level information.[2] Conversely, Peek and Rosengren (2000) and Ashcraft (2005) find that local areas affected by banking distress have reduced economic activity, but cannot trace the effects to the firm level.[3] A key innovation of the current paper is to build a dataset that contains financial information and employment outcomes at a wide range of U.S. firms. The finding of heterogeneous treatment effects then provides a bridge between the small versus large and natural experiment literatures.[4] To my knowledge this is also the first natural experiment paper to examine effects on employment, a variable of key economic and popular interest.[5]

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[1]Slovin et al. (1993) study the share price response of borrowers of Continental Illinois Bank around its failure and rescue in 1984. Peek and Rosengren (1997), Peek and Rosengren (2000), Gan (2007), and Amiti and Weinstein (2011) explore consequences of the bursting of the Japanese real estate bubble. Khwaja and Mian (2008), Chava and Purnanandam (2011), and Lin and Paravisini (2013) use variation relating to the response to the 1998 Pakistani nuclear tests, the 1998 Russian crisis, and the 2002 WorldCom bankruptcy, respectively. Papers that use variation generated by the 2007-09 crisis include Albertazzi and Marchetti (2011), Aiyar (2012), De Haas and Van Horen (2012), Almeida et al. (2012), Ivashina and Scharfstein (2010), and Santos (2011).

[2]Slovin et al. (1993) and Lin and Paravisini (2013) link lending supply shocks to borrower stock price and financial outcomes. They face the same data restrictions that exclude non-public firms or firms not in the Compustat database. Amiti and Weinstein (2011) examine the effect on exports at Japanese firms, but only include firms listed on a stock exchange. A parallel corporate finance literature that studies the investment sensitivity of firm cash flows has had the same firm-size limitations (Fazzari et al. 1988; Kaplan and Zingales 1997).

[3]Benmelech et al. (2011) also use the Peek and Rosengren experiment but analyze the effect on MSA-level unemployment rates.

[4]In a very different institutional setting, Khwaja and Mian (2008) find a greater ability for larger borrowers to substitute financing following the Pakistani nuclear shock.

[5]In contemporaneous work, Greenstone and Mas (2012) show that the withdrawal of lending to small

The outline of the paper is as follows. Section 2 reviews the theoretical reasons for the formation of banking relationships, explains the key features of the syndicated loan market, and presents empirical evidence for the presence of frictions to switching lenders. Section 3 describes the data sources and the construction of the merged loan-employment dataset. Section 4 details the identification strategy, provides relevant background on the 2008-09 crisis, and describes the measures of bank health. Sections 5 and 6 report the paper's key empirical results, demonstrating the effect of lending supply on loan market outcomes and employment outcomes, respectively. Section 7 relates the firm-level results to an estimate of the total decline in employment in the sample due to financial frictions. Section 8 concludes.

2.   Relationship lending

The econometric approach in this paper requires that borrowers and lenders form relationships. Otherwise, pre-crisis clients of banks that restricted lending during the crisis could costlessly switch to borrowing from less constrained banks, with no reason to expect differential outcomes at pre-crisis borrowers of different banks. These relationships may result from adverse selection in the market for borrowers that switch lenders (Sharpe 1990); a signaling equilibrium where lending to the same borrower helps to overcome the moral hazard problem inherent to lead lenders recruiting participants (Sufi 2007b; Holmstrom and Tirole 1997); or a decline in ex-ante (due diligence) or ex-post (costly state verification) monitoring costs for repeat borrowers (Williamson 1987; Montoriol-Garriga and Wang).[6]

The theories just listed share a common assumption of asymmetric information. This generates testable hypotheses for the heterogeneity of treatment effects. First, the benefit to using a previous lender, or conversely the lemons cost to switching lenders, should decline with the transparency of the borrower. Second, if the per-dollar monitoring cost falls with the size of the loan, then the cost of asymmetric information falls with borrower size. Thus, the health of the relationship lenders should matter more to less transparent, smaller borrowers.

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firms has a significant effect on county employment during the Great Recession.

[6]In the Holmstrom and Tirole (1997) model, borrowers without enough "skin in the game" can choose a technology with lower probability of success in exchange for capturing private benefit. The borrower's incentive compatibility constraint puts a limit on the fraction of output that can be claimed by lenders, yielding a maximum incentive compatible interest rate. In the costly state verification model, lenders trade off the extra income from charging a higher interest rate with the increased probability that the borrower will be unable to repay all of the principal and interest and will declare bankruptcy. Deadweight loss in bankruptcy implies a maximum interest rate the lender is willing to charge. In that case, even borrowers with positive loan demand at an interest rate above their lender's internal cost of funds may find themselves unable to borrow, and the likelihood of getting rationed out of the market rises for clients of banks with larger increases in their internal cost of funds. These models therefore have the implication that some of the loan market adjustment may occur on the extensive margin of access to the market at all.

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2.1. The syndicated loan market

In a typical syndicated loan deal, the lead arranger comes to preliminary agreement on the terms of the loan with the borrower, performs the due diligence, and recruits other participant lenders to provide some of the financing. Both the lead lender and the participants sign the loan contract with the borrower. The modal deal contains one lead arranger and one participant; however, about one out of three deals contain at least five participant lenders, and one-quarter of loans contain multiple lead arrangers. Besides handling the borrower relationship, the lead arranger also retains a larger share of the loan than participants, with the lead share usually fifty to one hundred percent larger than each participant's share. Traditional deposit-taking banks and investment banks act as lead arrangers, while participants also include hedge funds and pension funds.

The syndicated loan market accounts for nearly half of all commercial and industrial lending in the United States, and two-thirds of lending with a maturity greater than 365 days.[7] The market serves both publicly-traded and private firms, with about half of the firms private. The median borrower in my sample (described in further detail in section 3) had sales of about $500 million (in 2005 dollars), and had 620 employees in 2008. However, the 10th percentiles of sales and employment were $60 million and 77 employees. For comparison, 71 percent of private sector employees in the United States work at firms with at least 50 employees.

2.2. Evidence of banking relationships in the syndicated market

Whether borrowers use their previous lenders when accessing the market for a new loan provides a direct test of the presence of banking relationships.[8] Table 1 reports this likeli-

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[7]Beginning in November 2012, the Federal Reserve Survey of Terms of Business Lending began reporting separately on loans made under participation or syndication. By value, and averaging over the November 2012 and February 2013 releases, these loans constituted 40.8 percent of all commercial and industrial lending during the survey's reference week; 53.3 percent of lending with maturity of 31 to 365 days; 65.6 percent of lending with maturity greater than 365 days; and 56.0 percent of lending of any maturity by large domestic banks. See http://www.federalreserve.gov/releases/e2/default.htm.

[8]Previous work has documented a number of features of the market that indicate the presence of asymmetric information. Dennis and Mullineaux (2000) and Sufi (2007b) show that the number of participants in the syndicate increases with the transparency of the borrower. They interpret this finding as indicating that lead arrangers retain a larger portion of loans to less transparent borrowers as a signal of the quality of their private information about the borrower. Sufi (2007b) also shows that lead arrangers tend to recruit participants that have formed part of a previous syndicate for the borrower, again consistent with the existence of private information about borrowers that banks can learn over time. Bharath et al. (2007) explore the persistence of banking relationships in a regression framework.

hood. In column 1, the table reports results from the regression

$$Lead_{b,i} = \alpha_b + \gamma_1 \left[Previous\ lead_{b,i}\right] + \gamma_2 \left[Previous\ participant_{b,i}\right] \tag{1}$$
$$+ \gamma_3 \left[Previous\ lead_{b,i}\ X\ Public\ (Unrated)\right] + \gamma_4 \left[Previous\ lead_{b,i}\ X\ Rated\right] + \epsilon_{b,i},$$

where $Lead_{b,i} = 1$ if bank b serves as the lead bank for borrower i, and $Previous\ lead_{b,i} = 1$ if bank b served as the lead bank for i's previous loan. The estimated value of γ_1 is 0.71. In words, even after controlling for a bank's average market share (α_b), a bank that served as the prior lead lender of a private borrower (the omitted category) has a seventy-one percentage point greater likelihood of serving as the new lead lender. Both γ_3 and γ_4 are negative, indicating a lower repeat borrowing propensity among publicly-traded borrowers without and with a credit rating, respectively. The higher repeat-borrowing propensity among privately-held borrowers fits with the stickiness deriving from asymmetric information. Finally, $\gamma_2 > 0$, suggesting that previous participants also have a higher likelihood of becoming the lead lender. The small magnitude of γ_2 reflects the fact that it corresponds to the unconditional likelihood that a previous participant becomes the lead lender; the likelihood conditional on the lead lender disappearing is much larger.

Although equation (1) controls for overall lender market share, it does not account for the possibility that some banks may concentrate in lending to particular types of firms. If so, the repeat borrowing propensity could reflect bank specialization rather than true state dependence. Column 2 therefore adds a large number of fixed effects that effectively control for a lender's market share separately by borrower industry, state, year, incorporation status, riskiness, and size. The inclusion of the fixed effects adds remarkably little explanatory power to the regression, and has only minor effect on the estimated γs. Repeat borrowing appears to reflect the formation of relationships rather than bank specialization.

Finally, columns 3 and 4 repeat the exercise for participant banks rather than lead banks. The results suggest persistence among participants as well, with a previous participant having a roughly fifty percentage point greater likelihood of serving as a participant on a new loan.

3. Data

A principal innovation of this paper is to link datasets of loans and employment in order to observe employment outcomes of borrowers of different banks.

The loan market data come from the Thomson Reuters Dealscan database. Dealscan collects loan-level information on syndicated loans from SEC filings, company statements,

Table 1: Banking relationship regressions

	Lender chosen as lead		Lender chosen as participant	
	(1)	(2)	(3)	(4)
Explanatory variables:				
Previous lead	0.71**	0.67**	0.022**	−0.023**
	(0.011)	(0.012)	(0.0040)	(0.0045)
Previous participant	0.029**	0.020**	0.50**	0.46**
	(0.0014)	(0.0015)	(0.011)	(0.011)
Previous lead X Public (Unrated)	−0.052**	−0.043*		
	(0.016)	(0.017)		
Previous lead X Public (Rated)	−0.058**	−0.086**		
	(0.014)	(0.016)		
Previous participant X Public (Unrated)			0.039*	0.033+
			(0.018)	(0.018)
Previous participant X Public (Rated)			0.012	−0.038*
			(0.014)	(0.015)
Lender FE	Yes	Yes	Yes	Yes
2-digit SIC X lender FE	No	Yes	No	Yes
State X lender FE	No	Yes	No	Yes
Year X lender FE	No	Yes	No	Yes
Public/private X lender FE	No	Yes	No	Yes
All in drawn quartile X lender FE	No	Yes	No	Yes
Sales quartile X lender FE	No	Yes	No	Yes
R^2	0.480	0.504	0.285	0.334
Borrower clusters	3,253	3,253	3,253	3,253
Observations	349,008	349,008	349,008	349,008

The dependent variable is an indicator for whether the lender serves in the role indicated in the table header. For each loan in which the borrower has previous accessed the syndicated market, the dataset contains one observation for each potential lender, where a potential lender is a lender active in the syndicated loan market in that year. The variables Previous lead and Previous participant equal one if the lender served as the lead or as a participant on the borrower's previous loan, respectively. The sample covers 2001 to June 2009 and excludes loans to borrowers in finance, insurance, or real estate, and for which the purpose of the loan is not working capital or general corporate purposes. Estimation is via OLS. Standard errors in parentheses and clustered by borrower. +,*,** indicate significance at the 0.1, 0.05, 0.01 levels respectively.

and media reports, and attempts to process the universe of such loans.[9] The data include

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[9]Public companies must report any new bank loan to the SEC by filing a form 8-K or as an attachment to their quarterly or annual filing. Thomson Reuters publishes a quarterly set of "League Tables" using the Dealscan data, which rank lenders according to their level of activity in the syndicated market over the prior period. The public ranking of lenders gives banks an incentive to report to Dealscan loans that Dealscan might otherwise miss. For about ten percent of loans Dealscan reports a single lead lender and zero participants. In some of these cases, Dealscan does not observe which lenders serve as syndicate participants. In others, the loan is an add-on to a previous loan facility. Loans with a single lead arranger and zero participants are if anything slightly larger than other loans in the dataset.

the identities of the borrower and lenders present at origination, the terms of the loan, and the purpose of the loan (working capital, leverage buyout, etc.). The sample I use begins from all loans made to non-FIRE U.S. businesses with the primary purpose of the loan listed as "working capital" or "corporate purposes."[10] To restrict the sample to firms likely to have an active relationship with a lender during the crisis, I keep only borrowers with either at least one loan signed in or after 2004, or with a loan open in October 2007 or later.[11]

To obtain lender financial information, I merge the Dealscan lenders at the holding company level with data from the Federal Reserve FR Y-9C Consolidated Financial Statements for Bank Holding Companies (for lenders where the highest level parent is either a domestic financial holding company or a domestic bank holding company), and with data from Bankscope (for foreign holding companies and investment banks). I merge the lenders with data from CRSP to obtain stock price information. To keep the sample reasonably well balanced, I eliminate loans where the lead lender made only a small number of loans during the sample period.[12] This last restriction reduces the sample size by about five percent.

Employment data come from the confidential Bureau of Labor Statistics Longitudinal Database (LDB). The LDB builds from unemployment insurance records at state workforce agencies. The database follows establishments longitudinally and contains monthly employment and quarterly payroll (wages and salaries) at every private sector establishment in the United States. An establishment is a single physical place of work. Within each quarter, the LDB reports the employer identification number (EIN) of the tax-filing unit to which the establishment belongs. I refer to a group of establishments reporting under the same EIN as a firm.[13] Employment corresponds to the total number of employees on payroll

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[10]Firms with loans for other purposes may appear in the sample if they also have loans with a primary purpose of working capital or corporate purposes. The paper's results are robust to excluding firms with any leveraged buyout or M&A activity between 2005 and 2008.

[11]Dealscan contains 11,740 unique U.S. borrowers that either obtained a syndicated loan between 2004 and August 2008 or obtained a loan prior to 2004 that matured after October 2007. Of these, about two-thirds (7,885) report the primary purpose of the loan as "working capital" or "corporate purposes." (The next most common purpose is for a corporate takeover.) Eliminating borrowers in finance or real estate (FIRE), defined as SIC codes 6011-6799, further reduces the sample to 6,569. Finally, I remove borrowers with missing industry, state, or public/private status and winsorize the top one percent by sales, leaving a sample of 4,791 borrowers.

[12]For purposes of constructing their league tables, Thomson Reuters identifies one or more lead arrangers for each loan based on the descriptive role of each lender, and I adopt their classification.

[13]This definition of a firm derives from tax purposes and reporting conventions, and does not always correspond to the economic definition of control, nor to the scope of activities controlled by the loan recipient in the Dealscan data. If, however, shared tax liability maps into shared internal capital markets, then EIN is the correct ownership level for matching firms to their borrowing history in Dealscan. Otherwise, failure to identify all EINs with common ownership would lead to measurement error in the growth rates derived from the LDB, raising the standard errors in the regressions reported in section 6. More problematic, some states allow establishments that use professional payroll firms to report the EIN of the payroll firm rather than the establishment's owner, and I have to drop these firms from the sample. I hand check any firms

in the pay period containing the 12th day of the third month of the quarter, consistent with the standard U.S. statistical agency definition.

The LDB does not share a common identifier with the Dealscan data. For about one-quarter of the sample, I use a linking table between Dealscan and Compustat as described in Chava and Roberts (2008), and then use the EIN reported in Compustat to link to the LDB.[14] Another twelve percent of the firms in the Dealscan sample have exact matches in the LDB along the dimensions of firm name, city, and zip code. In the remaining cases, I perform a "fuzzy merge" using geographic and industry identifiers along with a bigram string comparator score of the firm name as reported in each dataset.[15] The final merged sample contains just over 2000 firms, or roughly half of the original Dealscan dataset.[16]

Panel A of table 2 gives summary statistics for the full sample of borrowers, the sample limited to loans with at least one lead lender among the forty-three most active, and the merged Dealscan-LDB sample. Each borrower appears in the sample exactly once, and the summary statistics correspond to the borrower's last pre-crisis loan. Both the sample limited to loans with the most active lenders and the merged Dealscan-LDB sample look quite similar to the full sample along the observable dimensions of loan size and borrowers' sales.[17] The sample (unweighted) average of the symmetric growth rate (defined in section 6) of employment from 2008:3-2009:3 is -9 percent. Aggregate employment in the sample declined by 5.8 percent (not shown), almost exactly equal to the 5.7 percent employment decline in the entire U.S. private sector.

The industry distribution of the firms broadly reflects that of the whole private sector,

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with a symmetric growth rate (defined below) greater in absolute value than 0.9. In many cases the extreme growth rates result from the use of multiple EINs by a single controlling economic unit. I either combine the EINs into a single new firm or drop the firm from the sample if I cannot identify all of the relevant EINs.

[14]The matching file is available on request from Michael Roberts. I conducted an assessment of each match using the information on firm sales reported in Dealscan as well as industry and geographic identifiers, resulting in a slightly different set of matches than in the Roberts file.

[15]A bigram string comparator computes the fraction of consecutive character matches between two strings. I implement the fuzzy merge using the Stata ado file reclink written by Michael Blasnik. I also perform a manual review of all of the matches to ensure accuracy.

[16]The unmatched firms fall into several categories. The merged sample does not include any firms with headquarters in eight states that did not grant access to their microdata. Some firms closed or merged prior to the financial crisis, and do not appear in the LDB with the same ownership structure as in Dealscan. A few firms that moved their headquarters across states or changed their name in the interval between their last Dealscan loan and the financial crisis may also be missing. As discussed in footnote 13, the merged sample also omits firms with establishments that use a professional employer organization to handle their payroll. Finally, firms that operate under multiple names may have generated too low a bigram string score to qualify as a match. The summary statistics presented in the next paragraph suggest that on balance the match attrition caused by these factors had limited effect on the composition of the sample.

[17]The difference between the samples described as "All lenders" and "Top 43 lenders" also reflects the removal of less than five percent of firms that are excluded from the probit regressions reported in section 5. These firms are in industries in which no firm in the sample obtained a loan or positive modification during the crisis.

Table 2: Sample summary statistics

	N	Mean	SD	p10	p50	p90
			Panel A: Firm variables			
Loan size (millions of 2005 dollars)						
All lenders	4,791	287	530	23	119	693
Top 43 lenders	4,391	302	542	26	129	720
Merged Dealscan-LDB	2,040	305	544	27	131	703
Sales at close (millions of 2005 dollars)						
All lenders	3,954	1,836	4,059	53	433	4,661
Top 43 lenders	3,623	1,928	4,149	60	478	4,869
Merged Dealscan-LDB	1,721	2,024	4,310	68	551	4,813
Employment growth rate, 2008:3-2009:3	2,040	-0.09	0.23	-0.29	-0.06	0.08
2008 employment level	2,040	2,985	9,993	77	620	6,128
			Panel B: Bank variables			
%Δ number of loans	43	-52.4	29.3	-87.4	-58.8	-7.5
Lehman cosyndication exposure (%)	42	1.15	1.20	0.34	0.72	1.91
ABX exposure	40	1.16	0.46	0.71	1.07	1.77
2007-08 trading revenue/assets (%)	42	-0.08	0.62	-0.72	0.01	0.39
2007-08 real estate net charge-offs/assets (%)	21	0.24	0.23	0.04	0.18	0.49
2007 deposits/assets (%)	43	42.2	25.4	3.0	47.4	68.2

Notes: The sample includes non-FIRE U.S. borrowers that obtained a loan for working capital or corporate purposes and with valid state, industry, and public/private status identifiers, excluding the top 1% by sales. Statistics for loan size and borrower sales correspond to information as of the last loan obtained by each borrower prior to September 2008. Rows indicated by Top 43 lenders include only borrowers whose last pre-crisis loan included one of the most active 43 lenders as a lead arranger. Rows indicated by Merged Dealscan-LDB and the employment statistics include only borrowers matched to the LDB. The variable %Δ loans equals the change in the annualized number of loans made by the bank between the periods October 2005 to June 2007 and October 2008 to June 2009. The variable Lehman cosyndication exposure equals the fraction of the bank's syndication portfolio where Lehman Brothers had a lead role in the loan deal. The variable ABX exposure equals the loading of the bank's stock return on the ABX AAA 2006-H1 index between October 2007 and December 2007.

with a Spearman rank correlation of 0.49 between employment shares (in NAICS 3-digit industries) in the sample and the whole population. Employment in the sample over-weights most heavily in retail trade and manufacturing, and under-weights in health care and administrative and support services.[18] Even among the smallest firms (fewer than 250 employees), the size distribution roughly resembles that of all private sector firms between 50 and 249 employees, with a Spearman correlation of 0.37.

The merged Dealscan-LDB sample contains roughly twice as many firms as would merging

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[18]The under-weighting of administrative and support services (NAICS 561) partly reflects the sample construction. The category includes professional employer organizations (NAICS 561330) and in many states the establishments for which they report payroll, the latter which I drop from the sample for the reasons described in footnote 13.

Dealscan with Compustat after applying my sample filters. Crucially, while the Dealscan-LDB sample has about the same number of large (1000 or more employee) firms as a Dealscan-Compustat sample, it has more than five times as many small and medium firms.[19] Still, even among firms with more than 50 employees, the Dealscan-LDB sample over-weights large firms relative to the distribution in the whole private sector, where firms with between 50 and 1000 employees vastly outnumber those with more than 1000.[20]

4. Identification

4.1. Theory

To determine the effect of credit availability on employment, one needs to isolate a measure of loan supply.

Formally, write the growth rate $g_{i,s}^y$ of employment y at firm i that had a pre-crisis relationship with lending syndicate s as a function of a vector of (omitting time subscripts) loan characteristics $L_{i,s}$; observable characteristics of the firm X_i; unobservable characteristics U_i; and an unobserved idiosyncratic component uncorrelated with X_i or U_i:

$$g_{i,s}^y \;\; = \;\; f\left(L_{i,s}, X_i, U_i, \epsilon_i\right). \tag{2}$$

For the moment, suppose $L_{i,s}$ consists only of an indicator variable for whether the firm receives a loan, and that this depends on firm characteristics, the internal cost of funds at the pre-crisis lending syndicate R_s, and an idiosyncratic disturbance η_i uncorrelated with ϵ_i and U_i:

$$L_{i,s} = h\left(R_s, X_i, U_i, \eta_i\right). \tag{3}$$

Under the assumptions (i) $U_i \perp\!\!\!\perp R_s$ (where $\perp\!\!\!\perp$ denotes statistical independence) and (ii) separability of $f\left(.\right)$ between its first two and second two arguments, equations (2) and (3) could be estimated using the generalized method of moments, with the moment condition $E\left[\left\{g_{i,s}^y - f\left(L_{i,s}, X_i, 0, 0\right)\right\} R_s\right] = 0$. In economic terms, assumption (i) states that the health of banks must be uncorrelated with the unobserved characteristics of their borrowers that affect either loan market or employment outcomes. I will sometimes refer to this assumption below as "as good as random" matching of banks and borrowers conditional on observables.

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[19]See footnote 50 for a description of the Dealscan-Compustat sample.

[20]The sample contains roughly twice as many firms in the 50-999 as in the 1000+ size class. In 2008 in the whole private sector, that ratio was roughly 24:1.

With these assumptions, the system (2)-(3) would identify the causal effect of the extensive margin loan market outcome on employment.

Three complications prevent direct adoption of the setup described above. First, R_s is not directly observed. Instead, one needs an observable measure of loan supply M_s such that $Corr\left(M_s, R_s\right) \neq 0$ and $U_i \perp\!\!\!\perp M_s$. Second, in practice modifications of existing loans may have as important an effect on the availability of credit as the signing of a new loan, and as described below the data appear to systematically under-report that outcome. Third, employment may depend not only on success in obtaining a loan, but also on the interest spread, size, length, covenants, and other characteristics of the loan obtained, as well as on expectations of future credit availability if firms face costs to adjusting their labor input. In other words, $h\left(\boldsymbol{\cdot}\right)$ is a vector-valued function, and the system (2)-(3) is under-identified for determining the effect of any particular component of $L_{i,s}$ on employment. These considerations do not however invalidate study of the reduced-form impact of lender health on employment. Substituting the arguments of (3) into (2), and replacing R_s with the observed measure M_s:

$$g_{i,s}^y = g\left(M_s, X_i, U_i, \epsilon_i, \eta_i\right). \tag{4}$$

Consistent estimation of $\frac{\partial g}{\partial M_s}$ follows from the orthogonality condition $U_i \perp\!\!\!\perp M_s$.

4.2. Origins of the 2008-09 crisis

The 2008-09 financial crisis began outside of the corporate loan sector. This makes the period particularly amenable to a study of the effects of loan supply precisely because it enhances the plausibility of bank health shocks orthogonal to the corporate loan portfolio.

The first signs of distress in financial markets came in June 2007, with the rescue by the investment bank Bear Stearns of a subsidiary hedge fund that had invested heavily in subprime mortgages. A month and a half later, the French bank BNP Paribas announced the freezing of three investment funds based on an inability to value the funds' subprime assets. The announcement sparked a rise in the interest rate at which banks lend to each other in the interbank market (see Figure 1).[21] Concerns mounted as a wave of bank writedowns on their subprime portfolios ensued. The panic reached a brief crescendo in March 2008, when the withdrawal of short-term financing to Bear Stearns forced its sale to J.P.Morgan.

Financial conditions stabilized somewhat over the summer, but then deteriorated sharply in September 2008. On September 10 Lehman Brothers reported a $3.9 billion loss for the

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[21]The Treasury-Eurodollar ("TED") spread became a widely watched indicator of financial distress during the crisis. Gertler and Kiyotaki (2011) provide theoretical justification for why stress in the interbank market matters in an economy with lending relationships of the type described in section 2.

third quarter of its fiscal year.[22] Five days later, unable to find a buyer and unable to obtain short-term financing, Lehman Brothers filed for bankruptcy. The cost of interbank lending spiked immediately. A cascade of market and policy events followed, including an $85 billion loan from the New York Federal Reserve Bank to the insurer AIG; the announcement by the money market fund Reserve Management Corporation that its net asset value had fallen below par, prompting widespread withdrawals from other money market funds; the forced sales of the investment bank Merrill Lynch and the commercial bank Wachovia; and direct capital injections by the federal government into major financial institutions through the TARP, to name a few. The stress in the interbank lending market began to ameliorate during the fall and winter of 2008, but remained elevated until the summer of 2009.

Figure 1: Stress in the interbank lending market

(3 month Treasury–Eurodollar spread, basis points)

Source: Federal Reserve Board of Governors (H.15 Release).

The major institutional failures during the crisis - the Bear Stearns and BNP Paribas funds, Bear Stearns itself, Lehman Brothers, and AIG - all resulted from exposure to real estate and mortgage securities and funding structure. The economics literature has studied the cross-section of bank health outcomes more systematically. I am not aware of any

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[22]The collapse of Lehman Brothers provides a good example of how distress in the financial sector originated outside of corporate lending. The weakness on the asset side of Lehman's balance sheet traced in part to an active decision by the bank's management at the beginning of 2006 to expand its own account investment in commercial real estate assets, compounded by a decision one year later to adopt a "counter-cyclical growth strategy" of increasing market share while other banks sought to reduce their real estate exposure (Valukas 2010, p. 59-80). On the liability side, Lehman relied heavily on short-term wholesale financing, leaving the firm vulnerable to a "repo run" (Gorton and Metrick 2012).

paper that has implicated the performance of the corporate loan portfolio. Instead, the literature has highlighted exposure to specific failing institutions (Ivashina and Scharfstein 2010); exposure to the real estate market and toxic assets (Santos 2011; Erel et al. 2011); and liability structure (Fahlenbrach et al. 2012; Ivashina and Scharfstein 2010; Gorton and Metrick 2012).

As banks absorbed asset writedowns and reduced funding availability, the internal cost of funds would rise. In standard models, the bank's first order condition for new lending equates the (properly discounted) expected return on the loan with the shadow value of the marginal dollar – the internal cost of funds. This relationship between new lending and the internal cost of funds provides the link between the financial market distress and the reduction in lending. Indeed, Cornett et al. (2011) conduct a comprehensive analysis of bank outcomes using regulatory data on a large set of commercial banks from the FDIC Call reports, and find that variation in bank balance sheets and funding sources well-explain the cross-sectional distribution of loan origination during the crisis.

4.3. Measuring loan supply

How then to measure the health of different banks? A broad measure uses the quantity of lending at each bank to proxy for the shadow price. Specifically, to measure credit availability to borrower i during the crisis, I use lending during the crisis by i's pre-crisis syndicate to all other borrowers.[23] Since lenders retain a larger share of loans in which they have a lead role, I weight each loan by the share retained by the lender.[24] If a bank's total lending reflects its internal cost of funds, then this measure will satisfy the condition $Corr\left(M_s, R_s\right) \neq 0$,

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[23] For mergers that occur prior to the onset of the financial crisis, I treat borrowers of the acquired company as if they had borrowed from the acquirer. This amounts to assuming that the loan desks of the acquiring and target banks had become fully integrated. For the mergers that occurred during the crisis, I keep the acquiring and target lenders separate. However, in measuring the crisis lending of each firm, I recode as borrowing from the target if a pre-crisis client of the target obtains a crisis loan from the acquirer. For example, Wachovia and Wells Fargo are separate lenders, but a firm that borrowed from Wachovia before the crisis and from Wells Fargo during the crisis gets recoded as having borrowed from Wachovia during the crisis. This treatment allows the data to determine the benefits to borrowers of a "shotgun marriage."

[24] In most cases Dealscan does not report the actual loan shares, so I instead use as weights the average share retained by lead lenders and participants in deals with the same syndicate structure. For example, in deals with one lead arranger and one participant on average the lead arranger retains 60 percent of the loan and the participant the rest, so I use 0.6 and 0.4 as weights in all deals with one lead arranger and one participant. The absence of actual shares also explains the use of the number of loans rather than the dollar value, as the fat right tail in the distribution of loan size means that the dollar value may compound measurement error stemming from the need to impute loan shares. At the bank level, the correlation coefficient between the two measures equals 0.87 (0.91 when weighted by the number of pre-crisis borrowers). All of the results in the paper would look similar using the dollar value of lending instead. However, the dollar value correlates less strongly with the other health variables described below, consistent with the dollar value containing greater measurement error.

making it relevant for loan outcomes of borrower i. It will satisfy the exclusion restriction $Corr\left(M_s, U_i\right) = 0$ if the unobserved characteristics of pre-crisis borrowers of syndicate s that influence loan outcomes are uncorrelated.

Formally, let $L_{b,j,t}$ equal one if bank b makes a loan to borrower j in period t, and $\alpha_{b,j,t}$ bank b's imputed share of the total commitment. First define:

$$\Delta L_{-i,b} \;\; = \;\; \frac{\sum_{j\neq i} \alpha_{b,j,crisis} L_{b,j,crisis}}{0.5 \sum_{j\neq i} \alpha_{b,j,normal} L_{b,j,normal}}, \tag{5}$$

where *crisis* equals the nine month period from October 2008 to June 2009, and *normal* equals the eighteen month period containing October 2005 to June 2006 and October 2006 to June 2007. In words, $\Delta L_{-i,b}$ measures the quantity of loans made by bank b to all borrowers other than firm i relative to before the crisis. $\Delta L_{-i,b}$ uses all loan packages in which b participates as either a lead lender or participant, but gives greater weight to packages where it served as a lead lender.

The measure of loan supply to each borrower uses a weighted average over all members of the last pre-crisis loan syndicate. Let a tilde denote this measure:

$$\Delta \tilde{L}_{i,s} \;\; = \;\; \sum_{b \in s} \alpha_{b,i,last} \Delta L_{-i,b}. \tag{6}$$

This measure gives a simple, transparent, and consistent way of classifying banks. It circumvents the difficulty of determining the correct level of ownership for ascribing bank health indicators, since it applies directly to the relevant lending entity. And, it should have a tight relationship with the unobserved internal cost of funds R_s since it relates directly to the loan portfolio. However, it relies on the relatively strong identification condition that the *cross-sectional* variation in bank lending reflects only supply factors or observed characteristics of the borrowers – in other words, that unobserved characteristics of borrowers that affect loan demand are not correlated at the lender level. The origins of the 2008-09 described above, and the fact that previous literature has explained the cross-sectional variation in bank health from factors outside of the corporate loan portfolio, make this identification condition at least plausible. Empirical support comes from the balancing of the sample on observed borrower characteristics reported in section 4.4, and an exercise exploring unobserved characteristics reported in section 4.5. Nonetheless, in what follows I also instrument for this measure using indicators of lender health that partially relax this identification condition. With these measures, the identification assumption becomes that less healthy banks as measured by the particular instrument did not also lend to corporate

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borrowers drawn from a different distribution of borrower health.[25]

The first proposed instrument follows Ivashina and Scharfstein (2010) in constructing exposure to Lehman Brothers through the fraction of a bank's syndication portfolio where Lehman Brothers had a lead role (*Lehman exposure*). Ivashina and Scharfstein show that this measure correlates negatively with new lending. They argue that firms that had credit lines where Lehman had a lead role drew down their lines by more as a precautionary measure following the disappearance of their main lender, and this led to a draining of liquidity from the other syndicate members.

The next measure captures banks' exposure to toxic mortgage-backed securities (*ABX exposure*). For the foreign-owned or investment banks in the sample that do not file standardized FR Y-9C reports to the Federal Reserve, it is essentially impossible to obtain the exposure directly from the balance sheet on a consistent basis. Instead, I infer banks' exposure from the correlation of their daily stock return with the return on the ABX AAA 2006-H1 index. This index follows the price of residential mortgage-backed securities issued during the second half of 2005 and with a AAA rating at issuance. The loading of a bank's stock return on the ABX index thus gives a measure of the bank's exposure to the underlying components or similar securities. The AAA index includes securities that banks would have viewed as completely safe upon acquisition. Indeed, the index remained roughly at par until the fall of 2007, but then fell by ten percent in October and November of that year. By 2009 the index had fallen another third; however, I compute the loadings only over the period October 2007 to December 2007 to avoid reverse causality if movements in the ABX sometimes reflected fire-sale of securities by distressed banks around the period of the Bear Stearns and Lehman Brothers collapses.[26]

Finally, I measure lender health using a number of bank balance sheet and income statement variables not directly affected by the corporate loan portfolio (*Bank statement items*). These include trading account losses (where much of the subprime writedowns occurred), real estate charge-offs, and the ratio of deposits to liabilities (a proxy for funding stability).

For each measure, I construct the weighted average over the members of the borrower's

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[25]I use these other indicators as instruments for $\Delta \tilde{L}_{i,b}$, rather than inserting them directly into equation (4), to facilitate comparison of magnitudes. In most cases the instrumental variables estimates will exceed the OLS. In general, loan demand can reflect either a healthy firm wanting to expand or an unhealthy firm needing to cushion a fall in revenue. This suggests ambiguity in the direction of any possible bias with OLS. Moreover, if the loan share imputation procedure introduces measurement error into $\Delta \tilde{L}_{i,b}$, then the instrumental variables estimates may correct the attenuation bias in the OLS results. This would also cause the instrumental variables estimates to exceed the OLS.

[26]The results in the paper are robust to using a longer window, to using the partial loading after conditioning on the three Fama-French factors, to using a later vintage of the ABX, or to using the loading on the index of the lowest rated BBB- securities. I am grateful to Stijn Van Nieuwerburgh for the suggestion of using the correlation with the ABX to measure exposure to toxic assets.

last pre-crisis syndicate, defined by the last loan obtained with a start date before September 2008. The weights are the (imputed) loan commitment shares. Panel B of table 2 reports summary statistics for the bank change in lending and each of the proposed instruments. The full sample contains 43 banks; the Lehman exposure measure excludes Lehman Brothers; the MBS exposure excludes three banks without publicly-traded equity; the trading revenue excludes Bear Stearns because it covers the period through the end of 2008; and the real estate charge-offs omit banks not regulated by the Federal Reserve or FDIC.

Table 3 presents correlations of each of the three proposed instruments, at the bank level, with the change in lending. The table reports regressions weighted by the number of pre-crisis borrowers of each bank, and the right hand side variables have been normalized to have unit variance. Column 1 replicates the finding in Ivashina and Scharfstein (2010) that banks that had participated in a higher fraction of syndicates where Lehman had a lead lending role reduced new lending by more. Column 2 indicates a strong relationship with the loading of the bank's stock return on the ABX AAA index covering the latter half of 2005. Finally, column 3 indicates that the bank statement items also predict the change in lending.[27] Importantly, the three proposed instruments do not correlate strongly with each other, with a (weighted) correlation coefficient of 0.29 between the Lehman exposure and the ABX exposure, 0.53 between the Lehman exposure and the fitted values of the bank statement items, and 0.34 between the ABX exposure and the bank statement items.[28]

4.4. Observed characteristics of borrowers

The identification assumption requires orthogonality between bank health and *unobserved* firm characteristics that affect loan demand or employment. The empirical exercises below therefore control for a rich set of observed nonfinancial borrower characteristics. In particular: industry fixed effects remove the possibility of spurious results due to banks that specialize in particular industries doing poorly on the measures described in section (4.3);

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[27]In results not shown, I also find an absence of evidence of reverse causality between corporate loan portfolios and the overall health of the bank. I can split bank net income into net charge-offs on commercial and industrial (C&I) loans and other net income only for the subset of banks in the sample regulated by the Federal Reserve or FDIC. A regression of the change in lending on only C&I charge-offs yields an insignificant coefficient and of the "wrong" sign. Similarly, a bivariate regression of the bank's stock return on C&I charge-offs yields an insignificant coefficient and an R^2 of 0.05, while adding net income excluding C&I charge-offs to the regression produces a highly significant coefficient (t-statistic of 5) and raises the R^2 to 0.61. Finally, recognizing that stock markets are forward-looking, a regression of the 2007-08 stock return on C&I charge-offs in 2009 also yields a coefficient insignificantly different from zero.

[28]A previous version of this paper also considered the change in lending by each bank to borrowers accessing the syndicated market for the first time. The strength of the correlation of this measure with the overall change in lending produced results extremely close to the OLS results reported below. This version does not report these results to conserve space, but they are available upon request.

Table 3: Determinants of bank lending

Explanatory variables:	Change in lending during the crisis		
	(1)	(2)	(3)
Lehman cosyndication exposure	-0.14^{**}		
	(0.049)		
ABX exposure		-0.11^{*}	
		(0.041)	
2007-08 trading revenue/assets			0.046
			(0.040)
Real estate charge-offs flag			0.012
			(0.050)
2007-08 real estate net charge-offs/assets			-0.092^{+}
			(0.051)
2007 Bank Deposits/Assets			0.19^{**}
			(0.059)
Joint test p-value	0.008	0.013	0.002
R^2	0.16	0.15	0.35
Observations	42	40	42

Notes: The dependent variable is the change in the annualized number of loans made by the bank between the periods October 2005 to June 2007 and October 2008 to June 2009, with each loan scaled by the importance of the lender in the loan syndicate as described in section 4.3 of the text. Observations weighted by number of pre-crisis borrowers. The explanatory variables have been normalized to have unit variance. +,*,** indicate significance at the 0.1, 0.05, 0.01 levels respectively.

state fixed effects and the total employment change in a borrower's county control for spatial clustering of banks and borrowers; loan-year fixed effects remove any confound if the timing of firms' borrowing is endogenous as in Mian and Santos (2011); the interest spread over the base rate (usually LIBOR) charged on the last pre-crisis loan proxies for ex-ante borrower riskiness; borrower's sales, fixed effects separating borrowers into non-public and without access to the bond market, public and without access to the bond market, and borrowers with access to the bond market,[29] and indicator variables for whether the borrower had used multiple lead lenders pre-crisis and whether the last pre-crisis loan had multiple lead arrangers all proxy for both transparency and access to outside funds; and indicator variables for whether the last pre-crisis loan was a credit line or term loan and whether the borrower had any loan reported in Dealscan coming due during the crisis proxy for loan demand.

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[29]I classify firms as having access to the bond market if they have a credit rating from either Moody's or Standard and Poor's as reported in Dealscan, or if they have ever issued public debt. For the latter, I merge Dealscan with the Mergent FISD bond database, using a procedure similar to that described in section 3 to merge Dealscan with the Bureau of Labor Statistics Longitudinal Database.

Each of these variables may influence borrower outcomes regardless of pre-crisis relationship, making one motivation for including them a reduction in the residual variance of the dependent variable. As well, stability of the coefficient of interest with and without control variables helps to address the concern that borrowers of different pre-crisis banks also differed along unobserved dimensions. To that end, the main tables below also report bivariate specifications for comparison.

Table 4 displays summary statistics for the control variables after splitting borrowers into four quantiles of the change in lending to other borrowers measure. The first two rows show the average employment change in a borrower's industry and county, respectively, using the national Quarterly Census of Employment and Wages. Clients of lenders in the top (healthiest) quartile belonged to industries that experienced an average employment decline of 8.9 percent in the year following the Lehman bankruptcy, compared to an average decline of 8.6 percent in the industries of clients of lenders in the bottom quartile. Clients of lenders in the top and bottom quartile operated in counties that had essentially identical average employment changes.[30]

The fact that lenders identified as healthier did not lend before the crisis to firms in counties or industries that had systematically better crisis employment outcomes indicates that bank specialization by industry or geography cannot explain the borrower outcomes even in a bivariate OLS context. Importantly, the absence of differences between firms with relationships with healthy and unhealthy lenders masks significant variation in employment outcomes across counties and industries. Instead, it reflects strong balancing of the sample along these observable dimensions. A similar pattern emerges for the remaining covariates described above, with the differences across lender quantiles small in magnitude.

4.5. Unobserved characteristics of borrowers

An exercise using the sample of borrowers that obtained a loan during the crisis can help to address whether unobserved characteristics of borrowers correlate at the lender level. The first step of this exercise asks whether lenders that reduced overall lending by more also reduced lending by more to the *same borrower* as compared to other lenders. Following Khwaja and Mian (2008), column 1 of table 5 implements this test by regressing the change

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[30]The county-level measure uses the change in employment in each county in which a firm operates establishments, averaged to the firm level using establishment employment shares as weights. The changes reported in table 4 weight the percent employment change in each industry (county) by the number of firms in the quantile operating in that industry (county). The unconditional average employment decline by industry using the industry weights is 8.5 percent, while the decline by county using county weights is 5.6 percent. The difference reflects the distribution of firms across industries and geography, and crucially the fact that the weights do not account for the total employment in each industry or county.

Table 4: Balancing of covariates in the sample

	Quantile of lender health				Memo: standard
	1	2	3	4	deviation
Mean employment change in:					
Borrower's industry, 2008:3-2009:3	-0.086	-0.081	-0.085	-0.089	0.083
Borrower's county, 2008:3-2009:3	-0.056	-0.056	-0.056	-0.056	0.009
Share with bond market access	0.455	0.540	0.458	0.236	0.494
Share private, no bond market access	0.418	0.331	0.363	0.525	0.492
Share public, no bond market access	0.127	0.129	0.179	0.239	0.374
Mean all in drawn spread	266	155	156	199	133
Median sales at close ($2005 billions)	0.366	0.837	0.701	0.285	4.146
Mean year of last pre-crisis loan	2005.83	2005.98	2006.03	2006.05	1.50
Share with loan due during crisis	0.193	0.188	0.183	0.205	0.394

Notes: The table splits the sample into four quantiles based on the change in the annualized number of loans made by the borrower's last pre-crisis syndicate between the periods October 2005 to June 2007 and October 2008 to June 2009. Employment change by borrower industry computed at the 4-digit SIC level using 6-digit NAICS employment levels from the Quarterly Census of Wages and Employment and an SIC-NAICS concordance table available from the Bureau of Labor Statistics. Employment change by borrower county computed by averaging the employment change in all counties in which a firm operates establishments using establishment employment shares as weights. The last column reports the standard deviation of the variable summarized in each row.

in lending in a borrower-lender pair on the loan supply measure and a full set of borrower fixed effects. The fixed effects then absorb any borrower characteristics that might influence loan outcomes. Inclusion of borrower fixed effects necessitates that every borrower have multiple lenders. The sample therefore includes one observation for each lead lender and participant in the pre-crisis syndicate. The dependent variable equals the log change in the dollar value of lending by that syndicate member from the pre-crisis loan to the crisis:

$$[\ln{(1 + \alpha_{b,i,crisis}V_{i,crisis})} - \ln{(\alpha_{b,i,last}V_{i,last})}] = \delta[\Delta L_{-i,b}] + FE_i + \nu_{i,b},$$

where $\alpha_{b,i,t}$ denotes bank b's imputed share of the loan to borrower i in period t; $V_{i,t}$ denotes the dollar value of the loan; $t \in \{crisis, last\,pre-crisis\,loan\}$; $\Delta L_{-i,b}$ denotes the change in lending by bank b to all other borrowers; and $\nu_{i,b}$ an idiosyncratic error term.[31] Column 1 of table 5 reports the results. The positive coefficient indicates that a firm that had both healthy and unhealthy members in its pre-crisis syndicate borrowed more during the crisis from the healthier member.

Under certain assumptions, the difference in the point estimates between regressions

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[31]The addition of 1 in the first log term accounts for the fact that some pre-crisis syndicate members do not appear in the crisis syndicate, in which case $\alpha_{b,i,crisis} = 0$. Other suitable growth rate measures that can handle exit, such as a symmetric growth rate or the conventional percent change, yield similar results.

Table 5: Testing for unobserved characteristics of borrowers

	Δ log(lending in borrower-lender pair)	
	(1)	(2)
Explanatory variables:		
%Δ loans to other borrowers ($\Delta\tilde{L}_i$)	1.05**	1.07**
	(0.33)	(0.32)
1-digit SIC, loan year FE	No	Yes
Bond market access/Public/Private FE	No	Yes
Additional Dealscan controls	No	Yes
Borrower FE	Yes	No
R^2	0.423	0.088
Borrowers	432	432
Banks	43	43
Observations	2,005	2,005

Notes: The sample contains only borrowers that signed a new loan between October 2008 and June 2009. The sample contains one observation per member of the borrower's last pre-crisis syndicate. The dependent variable is the log change in the dollar amount of lending from that lender to the borrower. The variable $\Delta\tilde{L}_i$ equals the change in the annualized number of loans made by the bank between the periods October 2005 to June 2007 and October 2008 to June 2009, and has been normalized to have unit variance. Estimation is via OLS. Standard errors in parentheses and clustered by the pre-crisis lender (column 1) or twoway-clustered on pre-crisis lender and borrower (column 2). +,*,** indicate significance at the 0.1, 0.05, 0.01 levels respectively.

including and excluding the fixed effects captures the amount of bias induced by not-as-good-as-random matching of borrowers and lenders.[32] To facilitate this comparison, the second column of table 5 reports the corresponding specification without the borrower fixed effects but with the full set of controls discussed in section 4.4. Intuitively, the difference between the right hand side variables in the specifications reported in columns 1 and 2 captures exactly the unobserved characteristics of the borrowers. If the unobserved characteristics were correlated with the lending measure, then the point estimate would change to reflect the omitted variables. Instead, the point estimate is essentially identical across columns. This provides direct validation of as-good-as-random matching within the set of firms that obtained a loan during the crisis.

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[32]Specifically, the true model of bank lending must be additively separable over bank health and firm characteristics, *id est*, of the form $[\ln(1 + \alpha_{b,i,crisis}V_{i,crisis}) - \ln(\alpha_{b,i,last}V_{i,last})] = \delta[\Delta L_{-i,b}] + \gamma X_i + e_{i,b}$, where X_i contains firm characteristics that affect loan demand (see Khwaja and Mian 2008, 1421-1423).

5. Loan market outcomes

This section presents results for the effect of the banking relationship on loan outcomes. Heuristically, these results correspond to the first-stage of an instrumental variables design, where the second stage outcome is employment. For the measure of credit availability to be relevant to firm employment, it should also predict outcomes in the loan market during the financial crisis.

5.1. Timing

Before discussing the loan market results, I comment briefly on the timing. Figure 2 shows the dollar value of new lending by the forty-three most active banks in the syndicated market. The market expanded rapidly during the mid-2000s, but began to contract during the fourth quarter of 2007.[33] New lending troughed in the fourth quarter of 2008, coincident with the peak of stress in the interbank market. Lending started its rebound somewhat after the interbank market stabilized, with a slow recovery in volume beginning at the end of 2009.[34]

The narrative timeline, the timepath of interbank lending spreads in figure 1, and the timing of the trough in new bank lending all point to a division in the crisis between the periods before and after the Lehman bankruptcy. The acuity of both financial distress and employment losses following the Lehman bankruptcy suggest that financial frictions may have had especially great influence on employment outcomes during that period. Indeed, of the 8.8 million private sector jobs lost in the United States from the peak in January 2008

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[33]The initial fall in lending does not seem to reflect expectations of a large decline in real activity; for example, in June 2008 members of the Federal Reserve Open Market Committee forecast that the unemployment rate would remain roughly unchanged (at around 5.5 percent) over the coming year.

[34]A perceived build-up of liquid assets at nonfinancial corporations generated much attention following the crisis, with the possible implication that borrowing constraints could not matter in an environment where corporations were simultaneously accumulating large amounts of cash or cash equivalents. Two facts bear mentioning in this regard. First, the Federal Reserve Financial Accounts of the United States (formerly know as the Flow of Funds), which publishes the data, revised away the large increase in aggregate cash holdings initially reported as having begun during the second half of 2009. The June 2013 release reports $1.66 trillion in liquid assets (code FL104001005Q) at nonfinancial corporations in the fourth quarter of 2011, down from $2.23 trillion as initially reported in the March 2012 release. The small increase in liquid assets during the early part of the recovery did little more than offset their decline during the recession; deflated by the GDP price index, liquid assets in the fourth quarter of 2011 remained below their pre-crisis peak. Second, I have performed unpublished tabulations of liquid asset holdings by firm size for manufacturing firms using the Census Bureau Quarterly Financial Reports, and for a representative sample of all firms using the IRS Statistics of Income. (Notably, these datasets provide the source data for the Flow of Funds estimates.) In both datasets, the rebound in holdings of liquid assets during 2009 occurred almost entirely at firms in the largest size classes, consistent with small and medium firms still facing liquidity constraints during the period under study.

Figure 2: Aggregate new lending from top 43 lenders

(Billions of 2005 dollars, seasonally–adjusted at quarterly rate)
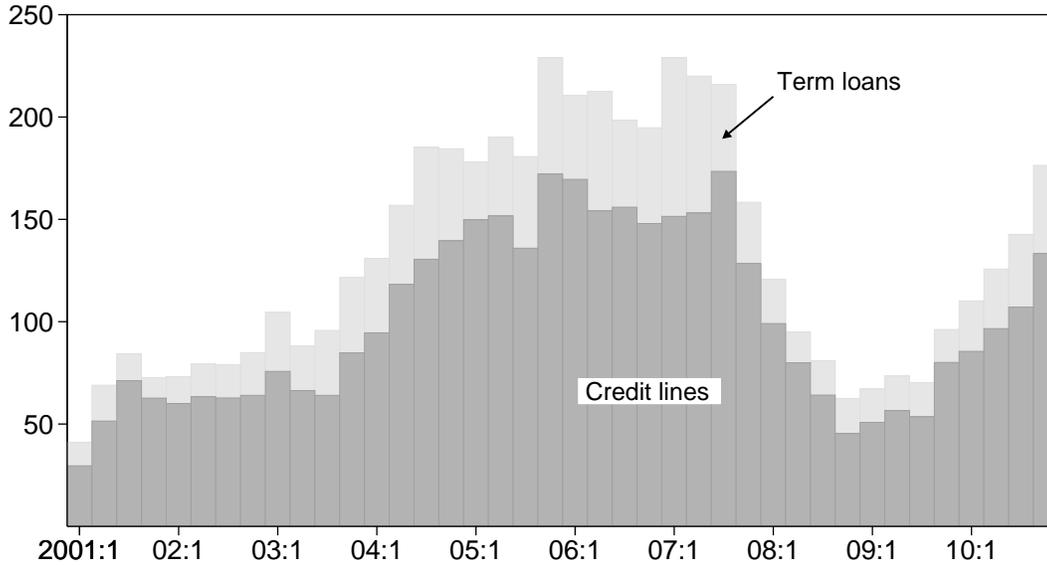
Notes: The figure shows the face value of new loans to non-FIRE borrowers for working capital or general corporate purposes in which one of the forty-three most active lenders had a lead role. Values seasonally-adjusted by author using Census-X12.

to the local trough in February 2010, fully half came in just the six month period ending in March 2009, and three-quarters in the year after the Lehman bankruptcy. Finally, the Lehman cosyndication exposure measure of bank health applies only to the period after the Lehman bankruptcy. The loan market results therefore focus on the period immediately following the Lehman bankruptcy, defined as October 2008 to June 2009.[35] The choice of June 2009 as a terminal month reflects both the timing of the U.S. recession, which ended that month, and the timing of the return to normalcy in the interbank lending market.

5.2. Loan market extensive margin results

The first outcome concerns whether the firm signed a new loan during the crisis period, or received a favorable modification of an existing loan. Most loans get renegotiated over the course of the contract (Roberts 2012). I define a favorable modification as either increasing the size of the loan, or extending the maturity or changing non-pricing terms (typically relaxing financial covenants) without reducing the loan size.[36] The loan modification margin

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[35]I set the post-Lehman period to begin in October 2008 because the loan date reported in Dealscan corresponds to the start of the loan facility, which may lag the signing of the loan agreement and will certainly lag the beginning of the loan processing.

[36]This definition implicitly assumes that a modification that doesn't change size or pricing constitutes a favorable change of terms to the borrower, such as the change in the definition of a covenant to avoid

may affect many more firms than those that actually wanted to sign a new loan during the crisis.[37] Unfortunately, no comprehensive dataset exists of loan modifications. The Dealscan loan modification module contains information on four times as many modifications of loans of publicly-traded firms as privately held firms during the crisis. This difference may reflect a reporting bias, since the publicly-traded firms must report loan modifications in their regulatory filings while private firms have no reporting obligation. If so, the findings reported below may understate the effect of lender health on this outcome.

Columns 1 and 2 of table 6 estimate the specification:

$$P\left(Borrow_{i,s} = 1\right) = G\left(\pi_0 + \pi_1 \Delta \tilde{L}_{i,s} + \gamma X_i + \eta_{i,s}\right), \qquad (7)$$

where $Borrow_{i,s}$ is an indicator variable for whether borrower i of pre-crisis syndicate s obtained a loan or favorable modification (from any lender) during the crisis period of October 2008-June 2009; $\Delta \tilde{L}_{i,s}$ is the measure of the change in the number of loans to other borrowers of the pre-crisis syndicate as defined in section 4.3; X_i contains the additional control variables defined in section 4.4; and $\eta_{i,s}$ is an error term. The estimation uses the full Dealscan sample (i.e. including firms not matched to the LDB) and is done via probit. The table reports the marginal coefficients scaled by 100 and after normalizing $\Delta \tilde{L}_{i,s}$ to have unit variance; hence the coefficient on $\Delta \tilde{L}_{i,s}$ has the interpretation of the percentage point increase in the likelihood of a positive loan market outcome from having a relationship with a bank one standard deviation above the mean. The variance-covariance matrix of $\eta_{i,s}$ allows for arbitrary correlation among borrowers with the same lead arranger.

The coefficient on $\Delta \tilde{L}_{i,s}$ indicates a strong, positive effect of loan supply on the likelihood of obtaining a loan during the crisis. Column 1 estimates the bivariate specification, and column 2 includes the full set of controls. With the full set of controls, a one standard deviation increase in loan growth from the members of the pre-crisis syndicate implies a two percentage point increase in the likelihood of signing a new loan or receiving a favorable modification. Changing the loan supply of a borrower at the 10th percentile of the distribution to that of a borrower at the 90th percentile would increase the likelihood by about five percentage points. The predicted likelihood at the mean of all other covariates for a borrower of a syndicate at the 10th percentile was 9.1 percent (not shown), yielding an increase of roughly fifty percent

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technical default. The definition classifies 61 percent of firms that receive a modification as receiving a favorable modification. Of the remainder, 28 percent received a reduction in their available commitment, and an additional 5 percent had an increase in pricing.

[37]In many cases, a renegotiation stems from the violation or imminent violation of a loan covenant, which gives the lender the option of waiving the violation or reducing the loan amount and increasing the interest rate. Indeed, Sufi (2007a) estimates that eight percent of publicly traded firms are in violation of a covenant in any quarter, a number which likely understates the level during a recession.

in the likelihood of obtaining a loan when going from the 10th percentile syndicate to the 90th.

Table 6: The effect of bank health on the likelihood of obtaining a loan

	Firm obtains a new loan or positive modification					
	Probit		$\Delta\tilde{L}_{i,s}$ instrumented using			
			Lehman exposure	ABX exposure	Bank statement items	All
	(1)	(2)	(3)	(4)	(5)	(6)
Explanatory variables:						
%Δ loans to other firms ($\Delta\tilde{L}_{i,s}$)	2.19**	2.00**	3.65**	2.33*	2.28**	2.32**
	(0.79)	(0.53)	(1.28)	(1.12)	(0.64)	(0.63)
2-digit SIC, state, loan year FE	No	Yes	Yes	Yes	Yes	Yes
Bond access/Public/Private FE	No	Yes	Yes	Yes	Yes	Yes
Additional Dealscan controls	No	Yes	Yes	Yes	Yes	Yes
First stage F-statistic			14.0	8.2	18.2	19.8
J-statistic p-value			.	.	.	0.206
$E[borrow]$	0.134	0.134	0.134	0.134	0.134	0.134
$E[\widehat{borrow}: \Delta\tilde{L}_{p_{90}} - \Delta\tilde{L}_{p_{10}}]$	0.052	0.048	0.087	0.055	0.054	0.055
Lead lender 1 clusters	43	43	43	40	43	40
Lead lender 2 clusters	43	43	43	40	43	40
Observations	4,391	4,391	4,391	4,354	4,391	4,354

Notes: The dependent variable is an indicator for whether the borrower signed a new loan or received a favorable modification to an existing loan between October 2008 and June 2009. The variable $\Delta\tilde{L}_{i,s}$ equals the change in the annualized number of loans made by the bank between the periods October 2005 to June 2007 and October 2008 to June 2009, and has been normalized to have unit variance. The variable Lehman cosyndication exposure equals the fraction of the bank's syndication portfolio where Lehman Brothers had a lead role in the loan deal. The variable ABX exposure equals the loading of the bank's stock return on the ABX AAA 2006-H1 index between October 2007 and December 2007. The balance sheet and income statement items include the ratio of deposits to assets at the end of 2007; the ratio of trading revenue over 2007-08 to assets; the ratio of net real estate charge-offs over 2007-08 to assets; and an indicator for reporting real estate charge-offs. The last column includes all of the instruments. For each firm, the bank-level measures are averaged over the members of the firm's last pre-crisis loan syndicate, with weights given according to each bank's role. In columns 1 and 2 estimation is via probit and the table reports marginal coefficients. In columns 3-6 $\Delta\tilde{L}_{i,s}$ is instrumented using the variable indicated in the column heading and estimation is via 2SLS. Borrower-level covariates are as of the last pre-crisis loan taken by each borrower. Variables sales and all in drawn contain imputed value of 0 if missing and regression contains an indicator variable for whether imputation occurs. Standard errors in parentheses and twoway clustered on the lead lenders in the borrower's last pre-crisis loan syndicate. +,*,** indicate significance at the 0.1, 0.05, 0.01 levels respectively.

The inclusion of the controls changes the point estimate little, but reduces the residual variance such that the standard error falls by a third. The coefficient on pre-crisis loan spread

is negative and significant, suggesting that riskier borrowers had greater difficulty obtaining a loan during the crisis. Similarly, the positive coefficient on sales implies that smaller borrowers had a lower likelihood of a positive loan market outcome. Finally, borrowers with a loan coming due during the crisis had a higher likelihood of obtaining a loan.[38]

Each of the next columns reports an instrumental variables specification. The magnitude of the bank health variable $\Delta \tilde{L}_{i,s}$ changes little using the ABX exposure or the bank statement items.[39] The magnitude rises using the Lehman cosyndication measure. The larger magnitude (but not the statistical significance) reflects the influence of borrowers from just four lenders that each have exposure to Lehman more than 70 percent higher than any other lender, with the fitted coefficient falling by nearly half excluding these borrowers.[40] Thus these three imperfectly correlated instrument sets all generate coefficients on the endogenous variable of a similar magnitude. The last column provides more formal statistical diagnostics, grouping all of the instruments together. The F-statistic in the last column is 19.8, above the Stock and Yogo (2005) criterion for 5 percent maximal relative bias. Moreover, the Hansen J-statistic cannot reject exogeneity of all of the instruments. The estimated coefficient in the last column is again very similar in magnitude to the OLS coefficient.

5.3. Loan market intensive margin results

Table 7 shows the effect of loan supply on the loan interest rate among borrowers that obtained a new loan during the crisis period. Following Santos (2011) and Hubbard et al. (2002), the sample contains crisis loan facilities matched to the last pre-crisis facility of a similar type.[41] Unlike the existing literature, the sample includes some borrowers who switched lenders during the crisis, since I am concerned with borrower outcomes rather than bank outcomes. The control variables mirror those described above, except that the much smaller sample size requires substituting 1 digit for 2 digit SIC industry fixed effects and removing the geography fixed effects.

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[38]Splitting the dependent variable into the signing of a new loan and a positive modification indicates predictive power of bank health on both dimensions. For example, the column 2 coefficient of 2.00 divides into roughly 60 percent from the signing of a new loan, and 40 percent from modifications, with each estimate on its own significant at the 1 percent level. Besides conserving space, combining the measures also reflects the finding of Roberts (2012) that Dealscan does not always properly classify a new contract as a new loan or as a modification of an existing loan.

[39]For the columns including the ABX measure, the regressions exclude borrowers of the three lead lenders without stock price information.

[40]The lenders are Goldman Sachs, Morgan Stanley, Bear Stearns, and Royal Bank of Canada. Excluding borrowers of these lenders, the fitted coefficient falls to 2.5, with a standard error of 1.1.

[41]Specifically, both loans in a matched pair must either be term loans or credit lines. If the borrower obtained multiple facilities of the same type as part of the last pre-crisis loan package, the match uses the facility closest in size and maturity.

Table 7: The effect of bank health on interest rate spreads

	Change in interest rate spread					
	OLS		$\Delta\tilde{L}_{i,s}$ instrumented using			
			Lehman exposure	ABX exposure	Bank statement items	All
	(1)	(2)	(3)	(4)	(5)	(6)
Explanatory variables:						
%Δ loans to other firms ($\Delta\tilde{L}_{i,s}$)	−14.6**	−12.2**	−23.1*	−20.0	−17.2*	−17.6**
	(5.26)	(4.15)	(11.2)	(13.3)	(7.63)	(6.68)
1-digit SIC, loan year FE	No	Yes	Yes	Yes	Yes	Yes
Bond access/Public/Private FE	No	Yes	Yes	Yes	Yes	Yes
Additional Dealscan controls	No	Yes	Yes	Yes	Yes	Yes
First stage F-statistic			60.5	7.8	14.3	14.5
J-statistic p-value	.	.	.	.	.	0.967
$E[\Delta Spread]$	130.6	130.6	130.6	130.7	130.6	130.7
$E[\widehat{Spread}: \Delta\tilde{L}_{p_{90}} - \Delta\tilde{L}_{p_{10}}]$	-39.7	-33.0	-62.8	-54.3	-46.6	-47.7
Lead lender 1 clusters	34	34	34	32	34	32
Lead lender 2 clusters	30	30	30	28	30	28
Observations	350	350	350	346	350	346

Notes: The dependent variable is the interest spread, in basis points, charged to a firm on a loan starting between October 2008 and June 2009, less the interest spread charged to the same firm on its last loan of the same type (credit line or term loan) obtained prior to September 15, 2008. The regressions exclude loan pairs with an increase of > 400 basis points. See the text for further details of the sample construction. The variable $\Delta\tilde{L}_{i,s}$ equals the change in the annualized number of loans made by the bank between the periods October 2005 to June 2007 and October 2008 to June 2009, and has been normalized to have unit variance. The variable Lehman cosyndication exposure equals the fraction of the bank's syndication portfolio where Lehman Brothers had a lead role in the loan deal. The variable ABX exposure equals the loading of the bank's stock return on the ABX AAA 2006-H1 index between October 2007 and December 2007. The balance sheet and income statement items include the ratio of deposits to assets at the end of 2007; the ratio of trading revenue over 2007-08 to assets; the ratio of net real estate charge-offs over 2007-08 to assets; and an indicator for reporting real estate charge-offs. For each firm, the bank-level measures are averaged over the members of the firm's last pre-crisis loan syndicate, with weights given according to each bank's role. Additional Dealscan controls: Multiple lead lenders indicator; Loan due during crisis indicator; Credit line indicator; Log sales at close; All in drawn spread; Credit line*all in drawn. Standard errors in parentheses and twoway clustered on the lead lenders in the borrower's last pre-crisis loan syndicate. +,*,** indicate significance at the 0.1, 0.05, 0.01 levels respectively.

The results indicate that pre-crisis borrowers of healthier banks received a lower interest rate if they borrowed during the crisis. Among all matched crisis borrowers, the average interest spread increased by about 130 basis points. In the OLS estimation, borrowers with pre-crisis relationships at the 10th percentile of crisis lending had increases of 33 basis points more than borrowers at the 90th percentile, and the coefficients are precisely estimated. As

with the extensive margin results, the additional controls have minor effect on the point estimate. The instrumental variables specifications suggest even larger effects on borrowing costs, with an interdecile difference of 48 basis points grouping all of the instruments together, but one cannot reject equality with the OLS estimates.

6. Employment outcomes

A number of channels may link loan market outcomes to employment. For firms that use working capital to finance payroll or other inputs into production, the relevant measure of marginal cost in the pricing decision incorporates the interest cost of the borrowing (Chari et al. 1995). A higher price of borrowing therefore acts like a cost-push shock, which for a firm facing a downward-sloping product demand curve implies a lower quantity of output and lower labor demand. At the extreme, firms may decide to forgo any working capital and only finance production inputs out of retained earnings, or may face credit rationing. A firm that does not borrow at all because of the health of its lender would have to cut payroll to levels consistent with its cash holdings. More generally, any firm that uses the borrowing rate to discount future profits and that does not hire its labor on the spot market each period (due to adjustment costs or search costs or some other friction) will demand less labor as soon as the borrowing rate rises. Finally, firms that do not currently need to borrow but face uncertain future shocks may optimally decrease payroll as a precautionary motive if they believe their ability to secure a loan in the future has diminished due to the health of their lender.

6.1. Employment growth rate definition

As has become standard in the literature using establishment-level employment microdata (see e.g. Davis et al. 1996), define the growth of employment y at establishment e belonging to firm i between periods $t - k$ and t using the symmetric growth rate:

$$g_{e,i,t-k,t}^{y} = \frac{y_{e,i,t} - y_{e,i,t-k}}{0.5\left[y_{e,i,t} + y_{e,i,t-k}\right]}. \tag{8}$$

The growth rate definition in (8) is a second order approximation of the log difference growth rate around zero; it is bounded in the range $[-2, 2]$; and it can accommodate both entry and exit. The latter two features help particularly to limit the influence of outliers.

The analogous growth rate at the firm level simply aggregates all establishments belonging to the firm *in a given quarter*, which equivalently yields a weighted average of the

establishment-level growth rates:

$$
\begin{aligned}
g^y_{i,t-k,t} &= \frac{\sum_{e \in i_{t-k}} y_{e,i,t} - \sum_{e \in i_{t-k}} y_{e,i,t-k}}{0.5 \left[\sum_{e \in i_{t-k}} y_{e,i,t} + \sum_{e \in i_{t-k}} y_{e,i,t-k} \right]}, \\
&= \sum_{e \in i_{t-k}} \omega^y_{e,i,t-k,t} g^y_{e,i,t-k,t}
\end{aligned}
\tag{9}
$$

where

$$
\omega^y_{e,i,t-k,t} = \frac{y_{e,i,t} + y_{e,i,t-k}}{\sum_{e \in i_{t-k}} [y_{e,i,t} + y_{e,i,t-k}]}.
$$

Grouping establishments according to their ownership in a given quarter, rather than computing the growth rate based on the level of employment reported across all establishments of a firm in each quarter, removes the influence of mergers or divestments on employment change. That is, this grouping convention defines employment changes as occurring only when a job appears or disappears, and not when firm employment changes due only to changes in ownership.[42] The decision to group firms by their period $t - k$ owner rather than their period t owner stems from the design of the natural experiment. Firms received a "treatment" beginning in September 2008. A potential outcome for a firm is divestment of selected establishments. Since divested establishments received the same loan supply treatment as retained establishments, I group them according to their beginning of period ownership. In practice the distinction matters little to the results reported below.

6.2. Employment specifications

The empirical specification mirrors that in (7), where as before $\Delta \tilde{L}_{i,s}$ denotes the change in the number of loans made during the crisis to other borrowers of $i's$ pre-crisis syndicate (see section 4.3):

$$
g^y_{i,s,t-k,t} = \beta_0 + \beta_1 \Delta \tilde{L}_{i,s} + \gamma X_i + \epsilon_{i,s,t-k,t}.
\tag{10}
$$

The covariance matrix of $\epsilon_{i,s,t-k,t}$ again allows for arbitrary correlation across firms with the same lead lender.

As above, I present results for a bivariate specification, and with additional firm-level controls. Along with the variables described in section 4.4, the firm-level controls in X_i contain the following variables drawn from the LDB data: a lag of the dependent variable, computed over the two-year period just prior to the onset of any financial turmoil, 2005:2-2007:2; the

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[42]It also helps to mitigate against the problems caused by the possible use of multiple EINs reporting under the same ownership structure in the LDB data, since otherwise a change in reporting structure would reveal itself as large swings in employment as whole establishments changed EINs, even if no actual job creation or destruction occurred. See footnote 13 for further discussion.

average employment change in the counties where the firm operates, using establishment employment shares as weights; fixed effects separating the firms into three size bin classes of 1-249 employees (small), 250-999 (medium), and 1000+ (large); and fixed effects for three age bin classes corresponding to firm birth in the 2000s (young), 1990s (mid), and pre-1990 (old).[43] Each of these variables may affect labor demand independent of the pre-crisis banking relationship. Including them should therefore reduce the residual variance, while stability of $\hat{\beta}_1$ with and without the control variables provides support for the validity of the loan supply measure. In particular, the lag of the dependent variable helps to address the concern that some banks may have lent to borrowers on different long-term employment trajectories. The county employment change absorbs local demand shocks, while recent research has highlighted the role of young firms in job creation (Haltiwanger et al. 2013).

Economic theory predicts that less transparent firms and firms without access to alternative forms of financing would exhibit greater sensitivity to banking frictions. Indeed, this theory makes an unconditional prediction – firms with access to the bond market should perform better during banking crises than firms without access. While a proper test of this prediction would have to account for the fact that bond market access is not randomly assigned, it is nonetheless useful to check the raw correlation. Table 8 shows the result in the baseline sample. Firms without access to the bond market had employment growth rates about three percentage points lower than firms with access, and this difference is highly statistically significant.[44] Moreover, adding a number of firm covariates to the regression, including size, age, industry, and county employment change, slightly increases the point estimate. These findings are suggestive of banking frictions mattering during this period, and are also consistent with the results in Adrian et al. (2013) and Becker and Ivashina (Forthcoming) that firms with access did in fact substitute toward the bond market.

The financial frictions literature has also put special emphasis on smaller firms (Gertler and Gilchrist 1994), which may be more vulnerable to credit shocks due to lower transparency, non-convex monitoring costs, or fewer pledgable assets. These considerations motivate regressions including interactions for the three size bins:

$$
\begin{aligned}
g^y_{i,s,t-k,t} &= \beta_0 + \beta_{1,small}\left[\Delta\tilde{L}_{i,s} * Small\right] + \beta_{1,med}\left[\Delta\tilde{L}_{i,s} * Medium\right] + \beta_{1,large}\left[\Delta\tilde{L}_{i,s} * Large\right] \\
&+ \gamma X_i + \epsilon_{i,s,t-k,t};
\end{aligned} \tag{11}
$$

---

[43]Although the large size bin contains more than double the employment of each of the small and medium bins, the number of firms in each size bin in the merged Dealscan-LDB sample is roughly equal. I define size classes by the 2007:2 employment level to mitigate against mean reversion in employment. Firm age corresponds to the age of the oldest establishment belonging to the firm.

[44]As explained in footnote 29, I classify firms as having access to the bond market if they have a credit rating from either Moody's or Standard and Poor's, or if they have ever issued public debt.

Table 8: Bond market access and employment

	Employment growth rate 2008:3-2009:3	
	(1)	(2)
Explanatory variables:		
No bond market access	-2.65^{**}	-3.15^{**}
	(0.98)	(1.10)
2-digit SIC and state FE	No	Yes
Firm size bin FE	No	Yes
Firm age bin FE	No	Yes
Lagged employment growth	No	Yes
County employment growth	No	Yes
R^2	0.003	0.172
Observations	2,040	2,040

Notes: The dependent variable is the symmetric growth rate g_j^y of employment. Firms that do not have access to the bond market do not have a credit rating from either Moody's or Standard and Poors, and have never issued public debt. Firms divided into size bin classes of 1-250, 250-999, and 1000+, and age bins for birth in the 2000s, 1990s, or earlier. Eicker-White standard errors in parentheses. +,*,** indicate significance at the 0.1, 0.05, 0.01 levels respectively.

as well as allowing the treatment effect to differ by whether the firm has access to public debt markets:

$$
\begin{aligned}
g_{i,s,t-k,t}^y &= \beta_0 + \beta_{1,bond\,access}\left[\Delta\tilde{L}_{i,s} * bond\,market\,access\right] \\
&+ \beta_{1,no\,access}\left[\Delta\tilde{L}_{i,s} * no\,access\right] + \gamma X_i + \epsilon_{i,s,t-k,t}.
\end{aligned}
\tag{12}
$$

6.3. Main employment results

Table 9 shows results for the change in employment over the period 2008:3-2009:3. The one year period obviates the need to control for seasonality. Once again the table reports coefficients scaled by 100 and with $\Delta\tilde{L}_{i,s}$ normalized to have unit variance, so that the coefficients have the interpretation of the percentage point change in employment growth from moving one standard deviation in pre-crisis syndicate health.

In the OLS specification with the full set of controls, the credit supply measure has a large and statistically significant effect on employment. The magnitude falls slightly in the bivariate specification, but rises after instrumenting. In all columns the statistical significance reaches at least the five percent threshold.[45] In this subsample of Dealscan firms

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[45]Technically the loading on the ABX AAA index constitutes a generated regressor. The standard errors reported in the tables do not account for this to maintain comparability across columns. A bootstrap standard error based on a bootstrapped sample of trading days to compute pseudo-loadings and of firm

matched to their BLS counterparts, the first stage F-statistic with all of the instruments equals 23.1, again above the Stock and Yogo criterion for 5 percent maximal bias, and the J-statistic again cannot reject exogeneity of the instrument set. Of note, the county employment change enters with a coefficient close to one, suggesting that local demand conditions had an important effect on firm outcomes. However, this control variable by itself does not affect the lender health coefficient, which reflects the geographic balancing of firms with relationships with healthier and less healthy lenders.

The implied economic magnitude is substantial. Borrowing from the 10th rather than the 90th percentile of lenders results in an additional decline in employment of four percentage points in the OLS specification and 5.5 percentage points with all of the instruments. For comparison, the average firm-level employment decline in the sample equaled 9.2 percent, while employment in the total nonfarm private sector declined 5.7 percent over the period.[46]

Table 10 presents the results allowing for heterogeneous treatment effects. The table reports only results for the $\Delta \tilde{L}_{i,s}$ measure, but reduced form regressions of the size and bond access bins interacted with the instruments yield similar results. Column 1 indicates that the credit supply measure has a large and precisely estimated effect on employment at small and medium firms. In contrast, the data cannot reject no effect of the measure on employment at the largest firms, and the point estimate is about one-quarter of the size. The finding that banking relationships matter more to smaller borrowers provides strong evidence of asymmetric information in lending markets. The results also comport with those in Duygan-Bump et al. (2011), who find that employment during the recession fell by more in

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observations accounting for the clustering indicates no bias in the standard errors reported in the paper.

[46]The online appendix provides an alternative means of assessing economic significance by reporting second stage coefficients from 2SLS regressions, where the first stage consists of the extensive margin loan market outcome regressed on one of the lender health measures listed in the header to table 9, and the second stage the change in employment regressed on the fitted value of the loan market outcome. The results suggest an economically very large effect of credit availability on employment. For example, grouping the Lehman exposure, ABX exposure, and bank balance sheet and income statement items together as the instrument set, the second stage coefficient equals 42.0 (s.e. 23.4). The coefficient has the interpretation that a firm that did not receive a new loan or positive modification would have a decline in employment of 42 percent using a symmetric growth rate, which corresponds to a 53 percent decline relative to the initial level.

Section 4.1 identified two reasons to exercise caution in interpreting these results. Both would cause the exercise to overstate the true effect of losing access to credit. First, the single source of exogenous variation in loan outcomes – variation in lenders' internal cost of funds – and multiple endogenous variables (did the firm receive a loan, the interest rate conditional on receiving a loan, etc.) render the system under-identified. In other words, the estimated effect of not receiving a loan also encompasses the effect of other outcomes such as receiving a higher interest rate. Indeed, in this specification the J test of the exclusion restriction of the grouped instruments rejects exogeneity at the ten percent level. Second, and as discussed in section 5.2, the endogenous variable appears to potentially exclude a number of loan modifications to private borrowers. Such incomplete information would bias the first stage toward zero, leading to an overestimate of the second stage coefficient. Importantly, while these concerns implicate the consistency of the second stage coefficients of employment on loan market outcomes, they do not invalidate the estimates of employment directly on bank health measures reported in the rest of this section.

Table 9: The effect of lender credit supply on employment

	Employment growth rate 2008:3-2009:3					
	OLS		$\Delta\tilde{L}_{i,s}$ instrumented using			
			Lehman exposure	ABX exposure	Bank statement items	All
	(1)	(2)	(3)	(4)	(5)	(6)
Explanatory variables:						
%Δ loans to other firms ($\Delta\tilde{L}_{i,s}$)	1.17*	1.67**	2.49*	3.17*	2.13*	2.38**
	(0.58)	(0.61)	(1.00)	(1.35)	(0.88)	(0.77)
Lagged employment growth		0.0033	0.0039	0.0045	0.0036	0.0039
		(0.019)	(0.019)	(0.019)	(0.019)	(0.019)
Emp. change in firm's county		0.89*	0.85+	0.86+	0.87+	0.89+
		(0.43)	(0.46)	(0.48)	(0.45)	(0.46)
2-digit SIC, state, loan year FE	No	Yes	Yes	Yes	Yes	Yes
Firm size bin FE	No	Yes	Yes	Yes	Yes	Yes
Firm age bin FE	No	Yes	Yes	Yes	Yes	Yes
Bond access/Public/Private FE	No	Yes	Yes	Yes	Yes	Yes
Additional Dealscan controls	No	Yes	Yes	Yes	Yes	Yes
First stage F-statistic			15.5	8.5	18.5	23.1
J statistic p-value			.	.	.	0.190
$E[g_j^y]$	-0.092	-0.092	-0.092	-0.093	-0.092	-0.093
$E[\hat{g}_j^y$: $\Delta\tilde{L}_{p_{90}} - \Delta\tilde{L}_{p_{10}}]$	0.027	0.039	0.058	0.074	0.050	0.055
Lead lender 1 clusters	43	43	43	40	43	40
Lead lender 2 clusters	43	43	43	40	43	40
Observations	2,040	2,040	2,040	2,015	2,040	2,015

Notes: The dependent variable is the symmetric growth rate g_j^y of employment. The variable $\Delta\tilde{L}_{i,s}$ equals the change in the annualized number of loans made by the bank between the periods October 2005 to June 2007 and October 2008 to June 2009, and has been normalized to have unit variance. The variable Lehman cosyndication exposure equals the fraction of the bank's syndication portfolio where Lehman Brothers had a lead role in the loan deal. The variable ABX exposure equals the loading of the bank's stock return on the ABX AAA 2006-H1 index between October 2007 and December 2007. The balance sheet and income statement items include the ratio of deposits to assets at the end of 2007; the ratio of trading revenue over 2007-08 to assets; the ratio of net real estate charge-offs over 2007-08 to assets; and an indicator for report real estate charge-offs. For each firm, the bank-level measures are averaged over the members of the firm's last pre-crisis loan syndicate, with weights given according to each bank's role. In columns 1 and 2 estimation is via OLS. In columns 3-6 $\Delta\tilde{L}_{i,s}$ is instrumented using the variable indicated in the column heading. Borrower-level covariates are as of the last pre-crisis loan taken by each borrower. Firms divided into size bin classes of 1-250, 250-999, and 1000+, and age bins for birth in the 2000s, 1990s, or earlier. Variables sales and all in drawn contain imputed value of 0 if missing and regression contains an indicator variable for whether imputation occurs. Standard errors in parentheses and twoway clustered on the lead lenders in the borrower's last pre-crisis loan syndicate. +,*,** indicate significance at the 0.1, 0.05, 0.01 levels respectively.

financially-dependent industries, but only at small and mid-sized firms, and Krueger (2010), who finds a relatively greater increase in layoffs at small establishments beginning around late 2008.[47]

The concentration of loan supply effects at small and medium firms may also help to explain why employment fell more at these firms during the post-Lehman banking crisis period. Figure 3 shows quarterly employment changes during the recession and recovery by firm size class, using the published tabulations of the LDB (the Business Employment Dynamics).[48] Prior to the Lehman bankruptcy, employment at the largest firms fell slightly faster than at firms in the 50-249 and 250-999 size classes. The relationship reverses dramatically thereafter. During the heart of the banking crisis in the fourth quarter of 2008 and the first quarter of 2009, smaller firms reduced employment by much more than those in the largest size class. Indeed, a monotonic relationship between the severity of losses and firm size class obtains during this period. The differential then disappears beginning in the summer of 2009, coincident with the timing of the stabilization of interbank lending markets. Integrating over the entire two-year period of employment losses in the United States, about two-thirds of the decline in employment came at firms with fewer than 1000 employees.

Column 2 of table 10 indicates that credit supply has an economically large effect on and significant explanatory power for the change in employment at firms without access to the bond market, but a much smaller effect on and no significant explanatory power for the change at firms with access. While these results may stem in part from the positive correlation between bond market access and transparency, they also likely reflect the substitution toward bond financing by firms with bond market access and diminished access to bank credit. Indeed, in the subsample of the firms in Dealscan that have ever accessed the bond market, a probit regression of whether the firm issued any public debt between October 2008 and June 2009 on the syndicate health measure $\Delta \tilde{L}_{i,s}$ and the standard set of controls yields a negative coefficient with a t-statistic of 2.6. That is, firms attached to weaker lenders did in fact compensate in part by issuing more public debt.[49]

The last column of table 10 investigates the relative importance of size and bond market access. The column reports coefficients from allowing the treatment effect to differ among four firm-type bins, with each bin the interaction of access to the bond market and firm size

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[47]Krueger uses unpublished JOLTS data that identify size class from establishment rather than firm size.

[48]The employment changes in Figure 3 and discussed in this paragraph use a dynamic sizing methodology. This means that a firm that begins the quarter with 1010 employees and ends the quarter with 980 employees will contribute to a decline in employment of 10 in the 1000+ class (going from 1010 to 1000) and of 20 (going from 1000 to 980) in the 250-999 class.

[49]Becker and Ivashina (Forthcoming) and Adrian et al. (2013) show that within a single firm bond issuance moves counter-cyclically while bank finance moves pro-cyclically. The result reported in the text provides evidence of financing substitution in the cross-section of firms as well.

Table 10: The effect of lender credit supply on employment with heterogeneous treatment effects

Explanatory variables:	Employment growth rate 2008:3-2009:3		
	(1)	(2)	(3)
$\Delta \tilde{L}_{i,s}$ * Large	0.54		
	(0.97)		
$\Delta \tilde{L}_{i,s}$ * Medium	1.84[+]		
	(0.97)		
$\Delta \tilde{L}_{i,s}$ * Small	2.16**		
	(0.79)		
$\Delta \tilde{L}_{i,s}$ * Bond market access		1.04	
		(1.00)	
$\Delta \tilde{L}_{i,s}$ * No access		2.01**	
		(0.60)	
$\Delta \tilde{L}_{i,s}$ * Bond access & large			0.23
			(1.15)
$\Delta \tilde{L}_{i,s}$ * Bond access & small/medium			1.47
			(1.06)
$\Delta \tilde{L}_{i,s}$ * No access & large			0.79
			(1.21)
$\Delta \tilde{L}_{i,s}$ * No access & small/medium			2.26**
			(0.58)
Lagged employment growth	Yes	Yes	Yes
Emp. change in firm's county	Yes	Yes	Yes
2-digit SIC, state, loan year FE	Yes	Yes	Yes
Firm size & age bin FE	Yes	Yes	Yes
Bond access/Public/Private FE	Yes	Yes	Yes
Additional Dealscan controls	Yes	Yes	Yes
Observations(Access & large)	483	483	483
Observations(Access & small/medium)	434	434	434
Observations(No access & large)	315	315	315
Observations(No access & small/medium)	808	808	808
Observations	2,040	2,040	2,040

Notes: The dependent variable is the symmetric growth rate g_j^y of employment. The variable $\Delta \tilde{L}_{i,s}$ equals the change in the annualized number of loans made by the bank between the periods October 2005 to June 2007 and October 2008 to June 2009, and has been normalized to have unit variance. Firms divided into size bin classes of 1-250, 250-999, and 1000+, and age bins for birth in the 2000s, 1990s, or earlier. Bond market access is equal to 1 if the firm has any bonds listed in the Mergent FISD database or if the firm has a credit rating. Additional Dealscan controls: Multiple lead lenders indicator; Loan due during crisis indicator; Credit line indicator; Log sales at close; All in drawn spread; Credit line*all in drawn. Standard errors in parentheses and twoway clustered on the lead lenders in the borrower's last pre-crisis loan syndicate. +,*,** indicate significance at the 0.1, 0.05, 0.01 levels respectively.

Figure 3: Employment losses by firm size

(Quarterly change in private sector employment, percent)
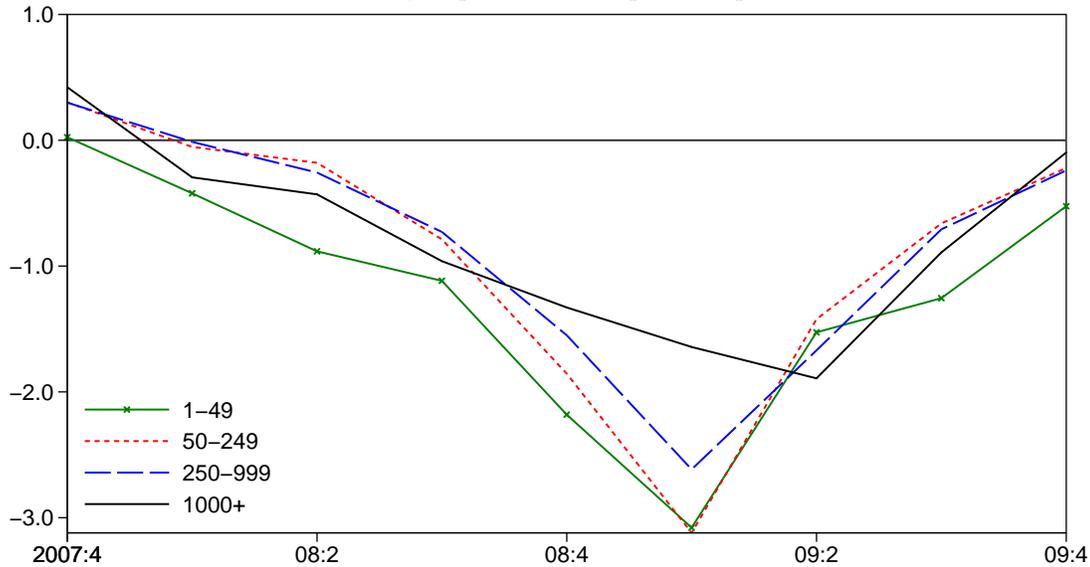
Source: Bureau of Labor Statistics (Business Employment Dynamics).
 Notes: The figure shows the percent change in employment by firm size class. The numerator is the change in employment using a dynamic sizing methodology. The denominator is the average level of employment during the two quarters. The BLS only reports employment levels for the first quarter of each year; the denominators for intervening quarters are linearly interpolated.

divided between large (>999 employees) and small/medium. As expected from the results just discussed, the point estimate of the treatment at large firms with bond market access is essentially zero, while the effect at small and medium firms without bond market access is economically large and highly statistically significant. While the magnitudes of the standard errors limit the potential inference, the coefficients at both large firms without bond market access and small and medium firms with access are larger than the coefficient at large firms with bond market access. It appears that both size and bond market access have separate effects on the importance of lender health to employment outcomes.

As a final point, the heterogeneous treatment results serve as a specification check for the validity of the loan supply measure. For unobserved borrower characteristics to explain the results, it would have to either be the case that only small and medium borrowers matched selectively with banks along the unobserved dimensions, while the largest borrowers associated with lenders randomly, or that the largest borrowers faced no frictions to switching lenders during the crisis.[50] Similarly, borrowers with bond market access would have to have

_____

[50]A potential caveat to the heterogeneity of treatment effects by size stems from the measurement error problem raised in footnote 13. Larger firms are more likely to split their reporting into multiple EINs, which could increase the measurement error in the dependent variable for this group of firms. To check this

matched differently from borrowers without such access.

6.4. Other time periods

The effect of credit supply on employment over two other periods can help shed light both on the mechanism at work and the course of the 2007-09 recession. The first period covers the beginning of the recession in December 2007 up to the Lehman bankruptcy. As shown in figure 2, the aggregate volume of new lending to firms began to decline in the fourth quarter of 2007, and troubles in the banking sector had begun to appear by that time as well. Still, the severity of the credit crunch, as measured by the volume of lending or the stress in the interbank market, remained well below what obtained following the Lehman bankruptcy. For that reason, one might expect that credit availability explains a smaller share of the decline in employment during this period.

The top panel of table 11 reports the results for the pre-Lehman period. For these specifications only, the loan supply measure reflects the change in lending during the pre-Lehman period relative to before the crisis, rather than the change in lending post-Lehman as used elsewhere in the paper.[51] The panel also omits results for the Lehman cosyndication exposure instrument and the bank balance sheet items, since both capture aspects of bank health that post-date the employment period. (The next subsection will report placebo regressions.) The results indicate an effect of credit supply on employment in the pre-Lehman period, but of about one-third of the magnitude relative to the post-Lehman results reported in columns 2 and 4 of table 9.

Whether credit frictions can have long-lasting effects on real outcomes has important implications for the ability of such frictions to explain events at business cycle frequencies. In the context of the 2007-09 recession and recovery, credit market conditions appear to have stabilized by the summer of 2009, but employment did not begin to recover until the beginning of 2010. The timing thus suggests that either something else held back hiring in the aggregate economy, affected firms still had difficulty obtaining credit despite the apparent

possibility, I re-ran the specification but using employment as reported in Compustat rather than the LDB. The merged Dealscan-Compustat sample contains many fewer firms in the small and medium size classes, so I combine those into a single class. I also restrict the sample to companies that end their fiscal year in December (about 71 percent of the Compustat firms), and compute the employment change over the smallest possible window that encompasses Sep-08 to Sep-09, namely Dec-07 to Dec-09. The resulting sample has 850 large ($>$999) firms and 238 small and medium firms. The results look quite similar to those in table 10, with a t-statistic of -0.85 for the effect of $\Delta \tilde{L}_{i,s}$ on the employment change at large firms, and a t-statistic of 3.63 for the effect at small and medium firms. Measurement error does not appear to explain the absence of loan supply effects at large firms.

[51]The R^2 of the two measures is 0.72. Since the matching of borrowers and lenders for the pre-Lehman period relies on loans obtained prior to December 2007, the sample omits borrowers who obtained their first loan after that date.

Table 11: The effect of lender credit supply on employment pre-Lehman and in the medium-run

	OLS	$\Delta\tilde{L}_{i,s}$ instrumented using			
		Lehman exposure	ABX exposure	Bank statement items	All
	(1)	(2)	(3)	(4)	(5)
		Panel A 2007:4-2008:3			
Explanatory variables:					
%Δ loans to other firms ($\Delta\tilde{L}_{i,s}$)	0.55[+]		1.26		
	(0.31)		(0.81)		
Lagged employment growth	0.052**		0.053**		
	(0.015)		(0.015)		
Emp. change in firm's county	0.59[+]		0.52		
	(0.32)		(0.32)		
First stage F-statistic			9.4		
Observations	1,895		1,872		
		Panel B 2008:3-2010:3			
Explanatory variables:					
%Δ loans to other firms ($\Delta\tilde{L}_{i,s}$)	1.94**	3.40**	5.18**	2.14*	2.67**
	(0.63)	(1.26)	(1.94)	(1.00)	(0.90)
Lagged employment growth	0.049**	0.051**	0.052**	0.050**	0.050**
	(0.018)	(0.018)	(0.017)	(0.017)	(0.017)
Emp. change in firm's county	−0.17	−0.21	−0.25	−0.17	−0.19
	(0.49)	(0.52)	(0.52)	(0.50)	(0.50)
First stage F-statistic		15.4	8.4	18.5	23.0
Observations	2,013	2,013	1,988	2,013	1,988
2-digit SIC, state, loan year FE	Yes	Yes	Yes	Yes	Yes
Firm size & age bin FE	Yes	Yes	Yes	Yes	Yes
Bond access/Public/Private FE	Yes	Yes	Yes	Yes	Yes
Additional Dealscan controls	Yes	Yes	Yes	Yes	Yes

Notes: The dependent variable is the symmetric growth rate g_j^y of employment. The variable $\Delta\tilde{L}_{i,s}$ equals the change in the annualized number of loans made by the bank between the periods December 2004 to August 2006 and December 2007 to August 2008 (panel A), or between the periods October 2005 to June 2007 and October 2008 to June 2009 (panel B), and has been normalized to have unit variance. Firms divided into size bin classes of 1-250, 250-999, and 1000+, and age bins for birth in the 2000s, 1990s, or earlier. Additional Dealscan controls: Multiple lead lenders indicator; Loan due during crisis indicator; Credit line indicator; Log sales at close; All in drawn spread; Credit line*all in drawn. Standard errors in parentheses and twoway clustered on the lead lenders in the borrower's last pre-crisis loan syndicate. +,*,** indicate significance at the 0.1, 0.05, 0.01 levels respectively.

calm, or propagation mechanisms prolonged the effect of the initial shock.

The bottom panel of table 11 offer suggestive evidence by extending the baseline period to 2008:3-10:3. Recalling that the OLS coefficient on loan supply for the 2008:3-09:3 period equaled 1.67, the coefficient of 1.94 in the first column indicates that firms forced to shed additional employment post-Lehman did not make up any of the difference in the year following. A similar result obtains for the instrumental variables specifications. Credit frictions appear to have long-lasting effects. In regressions not shown, I also find that these firms had no differential likelihood of obtaining a loan during the second year post-Lehman.[52]

A number of mechanisms could potentially explain the persistence of the effects. If firms use recessions to purge excess labor or "innovate to survive" under tighter financial constraints, then being forced to cut employment more during the crisis could have long-lasting effects, even if the borrowing constraint weakens subsequently. Likewise, restricting output due to financing constraints may cause customers to switch suppliers, which could persist beyond the period during which the constraint binds. Aghion et al. (2012) model a precautionary saving channel, in which affected borrowers reduce inputs in order to hoard liquidity in case of a future shock. Finally, excluding firms from the sample that went bankrupt reduces the coefficients in the 2008:3-10:3 period by roughly 20 percent.

6.5. Employment placebo regressions

The effect of the crisis bank health measures on employment during pre-crisis periods can serve as a specification check of the validity of the health measures. Table 12 reports results for two pre-crisis periods that use the same sample as that of table 9, and assign the same loan supply measures to each borrower. Thus the only difference in specification is the period covered by the dependent variable.[53]

The first period covers the end of the previous business cycle expansion up until the beginning of the turmoil in financial markets, 2005:2-07:2. A finding of a positive relationship in this period would raise the concern that banks with higher crisis lending had pre-crisis borrowers that had higher secular employment growth trends, while a negative finding could simply indicate that the borrowers had abnormal cyclical patterns. However, the top panel

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[52]The implications of the loan market outcomes are not obvious. Firms that could not obtain a loan during the year after Lehman may have had pent-up loan demand, suggesting a higher likelihood of obtaining a loan once markets stabilized. On the other hand, serial correlation in lender health could mean that these firms remained attached to weaker lenders.

[53]This statement requires two caveats. First, the placebo samples are slightly smaller, as a few of the borrowers either did not appear in the LDB during the placebo period or had an identification change that prevents linking them to the original sample. Second, the lagged dependent variable included in the regressions corresponds to the period from 3.25 to 1.25 years prior to the beginning of the placebo period, mirroring the time difference used in the main regressions.

shows that the loan supply measures have essentially no predictive power during this period.

Table 12: The effect of lender credit supply on employment in two placebo periods

	OLS	$\Delta\tilde{L}_{i,s}$ instrumented using			
		Lehman exposure	ABX exposure	Bank statement items	All
	(1)	(2)	(3)	(4)	(5)
			Panel A 2005:2-2007:2		
Explanatory variables:					
%Δ loans to other firms ($\Delta\tilde{L}_{i,s}$)	−0.19	−0.67	−1.57	1.63	0.92
	(0.74)	(1.63)	(1.72)	(1.24)	(1.15)
Lagged employment growth	0.028[+]	0.027[+]	0.028[+]	0.028[+]	0.028[+]
	(0.014)	(0.014)	(0.014)	(0.015)	(0.015)
Emp. change in firm's county	0.80	0.80	0.78	0.79	0.77
	(0.49)	(0.49)	(0.50)	(0.48)	(0.49)
First stage F-statistic		15.6	8.8	18.9	23.8
Observations	1,879	1,879	1,854	1,879	1,854
			Panel B 2001:3-2002:3		
Explanatory variables:					
%Δ loans to other firms ($\Delta\tilde{L}_{i,s}$)	−0.80	−0.74	1.30	−0.93	−0.72
	(0.59)	(1.44)	(1.89)	(0.93)	(0.85)
Lagged employment growth	0.024	0.024	0.024	0.024	0.024
	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)
Emp. change in firm's county	1.53**	1.53**	1.62**	1.53**	1.59**
	(0.51)	(0.50)	(0.51)	(0.51)	(0.50)
First stage F-statistic		16.5	7.7	17.8	26.3
Observations	1,675	1,675	1,653	1,675	1,653
2-digit SIC, state, loan year FE	Yes	Yes	Yes	Yes	Yes
Firm size & age bin FE	Yes	Yes	Yes	Yes	Yes
Bond access/Public/Private FE	Yes	Yes	Yes	Yes	Yes
Additional Dealscan controls	Yes	Yes	Yes	Yes	Yes

Notes: The dependent variable is the symmetric growth rate g_j^y of employment. All right hand side variables exactly equal those used to produce columns 2-6 of table 9, except the lagged employment growth rate which instead uses the period from 3.25 to 1.25 years prior to the beginning of the placebo period, and the county employment change which is contemporaneous to the placebo period. Standard errors in parentheses and twoway clustered on the lead lenders in the borrower's last pre-crisis loan syndicate. +,*,** indicate significance at the 0.1, 0.05, 0.01 levels respectively.

The second placebo period covers the end of the previous U.S. recession, from 2001:3-

02:3. The 2001-02 period has a number of superficial similarities to the 2008-09 period, including the gap between the period start and the NBER business cycle peak; the economic shock during the first month of the period (in the former case the events of September 11th); and the timing of the last month of the period as near but not yet at the local minimum in aggregate employment. On the other hand, the 2001 recession did not have any obvious origin in credit market events. The concern, raised in a different context in Mian and Santos (2011), that some banks lend to more countercyclical borrowers and that this tendency explains the results above, would predict a positive coefficient of 2008-09 lending on 2001-02 employment. Instead, the point estimates in four of the five columns of the bottom panel are negative, and none is statistically significant.

Firms attached to worse lenders and that had worse employment outcomes during 2008-09 do not appear different from other firms during pre-crisis periods.

7. Aggregate implications

With some additional assumptions, the firm-level results in section 6 can help to inform about the aggregate effect of the credit frictions during the 2008-09 period.

7.1. Aggregate effects in the sample

The first step involves estimating the total effect of bank lending frictions on employment in the sample. In the counterfactual exercise, every borrower faces a pre-crisis syndicate that changed its lending supply by the same amount as the most liberal syndicate. The estimate of effects in the sample depends on two assumptions:

A1 (partial equilibrium): the total employment effects equal the sum of the direct employment effects measured at each firm.

A2 (unconstrained at the top): the most liberal syndicate did not shift its lending supply function during the crisis.

The widespread distress in financial markets after the Lehman bankruptcy suggests that A2 may be quite conservative. If the syndicate identified as the most liberal also contracted its lending supply function, the estimates below will understate the true level of employment effects in the sample.

To begin, define the counterfactual growth rate if firm i in size-class c had borrowed from the τ'th percentile of lenders:

$$
\begin{aligned}
g^y_{i,s,t-k,t}(Q_\tau) &= E\left[g^y_{i,s,t-k,t} | \Delta \tilde{L}_{i,s} = \Delta \tilde{L}_{Q_\tau}\right] \\
&= \hat{g}^y_{i,s,t-k,t} + \hat{\beta}_{1,c}\left[\Delta \tilde{L}_{Q_\tau} - \Delta \tilde{L}_{i,s}\right].
\end{aligned}
\tag{13}
$$

$\Delta \tilde{L}_{Q_\tau}$ denotes the loan measure for the borrower at the τ'th percentile of the distribution, while $\hat{g}^y_{i,s,t-k,t}$ denotes the fitted value from the regression. Let T denote the mapping from symmetric growth rates to the end-period level, holding the initial level fixed:[54]

$$
T[x] = \frac{1 + 0.5x}{1 - 0.5x} y_{i,t-k}.
\tag{14}
$$

Using equation (14), define the counterfactual period t employment level at the τ'th percentile as $y_{i,t}(Q_\tau) = T\left[g^y_{i,s,t-k,t}(Q_\tau)\right]$, and similarly the fitted value employment level as $\hat{y}_{i,t} = T\left[\hat{g}^y_{i,b,t-k,t}\right]$.[55] Identifying the most liberal syndicate as that at the τ'th percentile of the distribution, the total sample employment losses due to frictions are then:

$$
\text{Total losses due to frictions} = \sum_{\Delta \tilde{L}_{i,s} \leq \Delta \tilde{L}_{Q_\tau}} \left[y_{i,t}(Q_\tau) - \hat{y}_{i,t}\right].
$$

The fraction of the sample net employment change due to frictions equals:

$$
\frac{\sum_{\Delta \tilde{L}_{i,s} \leq \Delta \tilde{L}_{Q_\tau}} \left[y_{i,t}(Q_\tau) - \hat{y}_{i,t}\right]}{\sum_i \left[y_{i,t-k} - y_{i,t}\right]}.
\tag{15}
$$

Table 13 reports the results. Since the estimated coefficient for large firms is quantitatively small and not significant, I impose that $\beta_{1,large} = 0$, and use the coefficients reported in column 1 of table 10 for the marginal effect at small and medium firms.

The table reports results using either the 90th or 95th percentile syndicate as the unconstrained lender. Total employment at firms in the sample with fewer than 1000 employees declined by seven percent from 2008:3 to 2009:3.[56] The exercise indicates that the shift in

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[54]That is, $T\left[g^y_{t-k,t}\right]$ solves:

$$
g^y_{t-k,t} = \frac{T\left[g^y_{t-k,t}\right] - y_{i,t-k}}{0.5\left[T\left[g^y_{t-k,t}\right] + y_{i,t-k}\right]}.
$$

[55]OLS imposes that $\sum_i g^y_{i,s,t-k,t} = \sum_i \hat{g}^y_{i,s,t-k,t}$. However, both the non-linearity of $T[.]$ and the non-degenerate size distribution of $y_{i,t-k}$ imply that $\sum_i y_{it} \neq \sum_i \hat{y}_{it}$ unless by chance.

[56]For comparison, summing over the four quarters 2008:4-2009:3 the employment declines at firms with between 50 and 999 employees in the quarter, and dividing by the employment level in 2008:3, yields a

Table 13: Total effect of credit availability at small and medium firms in the sample

	2008:3-2009:3 (Percent)
Total employment decline	7.0
Share of losses due to credit availability, $\tau = 90$	34.4
Share of losses due to credit availability, $\tau = 95$	47.3

Notes: The table reports the fraction of employment losses due to credit availability at small and medium firms, as described in the text. τ refers to the percentile of the lending syndicate identified as the most liberal syndicate.

loan supply can account for between one-third and one-half of these losses, depending on the percentile used to identify the most liberal syndicate. The loan supply shift to small and medium firms accounts for a much smaller fraction of job losses in the full sample, between 3.2 and 4.4 percent. While about two-thirds of the sample consists of firms with fewer than 1000 employees, the fat right tail of the employment size distribution means that these firms include just under ten percent of total employment in the sample.

7.2. Aggregate effects in the population

The first step in translating the results in table 10 into an estimate of the whole economy involves re-weighting. In the private sector, two-thirds of the aggregate employment decline occurred at firms with fewer than 1000 employees. Multiplying the estimates in table 10 by two-thirds, the shift in loan supply would explain between about one-fifth and one-third of the decline in employment in the year following the Lehman bankruptcy.

It is important to note that the preceding calculation ignores both external validity and general equilibrium concerns. The external validity concern relates to the representativeness of the small and medium firms in the sample relative to all firms with fewer than 1000 employees in the population. On the one hand, the sample excludes very small firms – recall that the tenth percentile firm by employment has 77 employees. In the population, about one-third of private sector employment occurs at firms at least that small. If these firms exhibit even greater sensitive to their lenders' health, then the estimated magnitude of effects in the sample could understate the average in the population. Counter to that, the sample by construction contains firms dependent on external financing. While the requirement that firms have a lending relationship by itself may not generate too much sample selection bias,[57] the criterion that the loan be syndicated does restrict the sample to those firms with the

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decline of 6.7 percent in the entire U.S. economy.

[57]For example, the 2003 Federal Reserve Survey of Small Business Finances (the most recent available) finds that more than eighty percent of firms with between 100 and 499 employees have a credit line (Mach and Wolken 2006).

largest bank dependence, and this selection may be most severe for smaller firms. In a similar vein, the firms in the sample may have depended on banks in better or worse health than the average firm in the population.[58]

General equilibrium effects pose a second challenge to extrapolating to the whole economy. In partial equilibrium, financially-constrained firms facing a downward-sloping product demand curve reduce production and raise prices. This results in lower labor input at constrained firms relative to unconstrained firms, consistent with the cross-sectional empirical evidence presented above. The aggregate effect of the frictions, however, also depends on whether the unconstrained firms adjust their labor input.

General equilibrium analysis suggests opposing channels that may lead unconstrained firms also to adjust their labor input. First, some labor shifts from constrained to unconstrained firms. Part of the reallocation of labor results from a shift in product demand, as relative prices at unconstrained firms fall or constrained firms ration their output. Further reallocation comes from the decline in employment at constrained firms causing a fall in the real product wage, which induces unconstrained firms to move down their labor demand curves. The magnitude of the labor reallocation depends on the substitutibility of both the goods produced at the different firms and the labor used in production. Second, the financial shock generates a reduction in aggregate expenditure. The fall in aggregate expenditure reduces labor demand at unconstrained firms.

An online appendix presents a formal general equilibrium model that illustrates these channels and fully characterizes the relationship between the empirically-estimated relative employment outcomes and the aggregate effects that obtain in general equilibrium. For plausible parameter values, the general equilibrium effects in the model either magnify the effects from the partial equilibrium exercise or have at most a modest attenuating effect. This result suggests that the magnitudes in table 10 may provide a reasonable benchmark for the aggregate effect of the frictions. Nonetheless, it has some sensitivity to the model's assumptions and parameter choices, indicating that general equilibrium effects provide a second source of uncertainty in moving from the estimated effects in the sample to the whole economy.

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[58]In particular, by construction the sample contains large banks. However, a simple tabulation of return on assets by 2007 bank size indicates similar returns across large and small banks.

8. Conclusion

This paper has shown that banking relationships matter. In particular, it has linked the health of a firm's lenders to its employment outcomes. The relationship appears economically important both at the level of the firm and for aggregate fluctuations. At the level of the firm, the predicted change in employment varies by as much as five percentage points depending on the health of its lenders. In the aggregate, these frictions can account for as much as one-third to one-half of the decline in employment at small and medium firms in the year following the collapse of Lehman Brothers.

These results have implications for explanations of the severity of the 2007-09 recession. In a series of papers, Mian and Sufi (2010; 2011; Mian et al. 2011) argue for the importance of what they term the "deleveraging-aggregate demand hypothesis," which explains the recession by a reduction in consumption demand by households trying to reduce debt burdens following the collapse of house prices (see also Eggertsson and Krugman 2012). The findings here provide direct evidence for a complementary channel that highlights the role of financial frictions in restricting the availability of credit to firms (Gilchrist and Zakrajsek 2012; Hall 2011; Stock and Watson 2012). The frictions channel can potentially explain the acceleration of the downturn following Lehman Brothers, as well as the added severity during that period at smaller firms. It may also provide a partial explanation for the unusual rise in unemployment relative to the fall in output (Daly and Hobjin 2010), if the lack of credit caused firms to purge excess labor more than they otherwise would.

If financial frictions can help to explain why the downturn accelerated in the fall of 2008, they face a challenge in explaining the persistence of the slump, since credit markets appear to have stabilized well before the aggregate economy returned to normal (Hall 2010). The result that at the level of the firm employment losses due to frictions do not appear to have dissipated at all two years later is intriguing in this regard. Better understanding of the mechanisms that generate such persistence at the microeconomic level could lead to improved macroeconomic insight and would provide one fruitful avenue for future research.

Finally, the paper has documented that the importance of banking relationships varies by firm type. Consistent with theories that emphasize asymmetric information about borrowers, the pre-crisis banking relationship appears to have essentially no effect on crisis outcomes for the largest and most transparent borrowers, and substantial effects for smaller borrowers and those without access to public debt markets. Data constraints have in the past limited analysis of credit frictions and cash-flow sensitivity to the effect at large, transparent firms. Future research should continue to look for ways to study the effects at smaller firms as well.

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