The Impact of Bank Health on the Investment of Its Corporate Borrowers

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Abstract

This paper investigates how changes in a bank's health impact real investment of its existing borrowers. Using a sample of U.S. firm bank relationships, I find that firms reduce investment by around 10% in response to a one standard deviation increase in the loan nonperformance of their primary bank. This effect is only present during active borrowing relationships, and is not driven by reverse causality or changes in region or industry specific investment opportunities. The effect weakens in the decade following the mid-90s but returns post 2006, suggesting that U.S. have not become less bank dependent.

1 Introduction

This paper seeks to answer the question of how important bank health is to US firm investment, who is most impacted by it, and when it matters. The massive contraction in bank lending in the wake of the recent financial crisis has generated much attention on the question of the importance of bank health and its importance to individual corporations. The attention being paid to this question, and the importance placed on it, is somewhat new. The prominence of "transaction oriented" banking and the rapid consolidation of banks in the last decade changed how banking in the U.S. was perceived considerably.[1] The financial crisis generated a significant re-evaluation of this belief, but the overall question about the real effects remains elusive. This research provides important evidence on how bank switching costs impact real outcomes and sheds light on the foundational question of when and why it matters. Moreover, it provides some evidence as to why many academics and industry analysts had declared the U.S. to be a post-banking economy only a few years ago.

This paper exploits panel data identification techniques to examine the relative impact of bank distress on firm investment over a 20 year period by constructing a panel of borrower-lender relationships from 1988 to 2007. Isolating the cross sectional variation attributable to individual banks and unrelated to the identified borrowers, I find that an increase in the bank's non-performing loan ratio is associated with a significant decrease in future investment by its existing borrowers. Specifically, a one standard deviation increase in the non-performing loan ratio (an increase of around 2% of total loans) corresponds to a reduction in annual investment of firms which borrow from that bank by 8 to 10% in the

[1] For instance, Ashcraft (2005) begins his AER paper on the effects of two bank failures in the late 80s with a caveat, questioning whether, given the reduced bank dependency of the US economy, bank failures even still matter. He therefore makes an appeal to the importance of historical understanding as the primary draw of the paper.

following year. This effect is measured relative to other “bank dependent” firms in the cross section, and is specific to the banking relationship itself. The results of this paper directly establish investment distortion as one of the costs of banking relationships and highlight the importance of *individual* bank stability in the financing of corporate investment.

This impact is identified through cross sectional differences in bank health and firm investment, linked through the ex-ante establishment of banking relationships. As such, I hold aggregate macroeconomic conditions fixed and trace the components of bank distress from factors unrelated to the firm, and which I argue are plausibly orthogonal to the firm's investment opportunity set. This approach is unique in that it measures the relative impact on investment directly, and the results are not specific to any one period or event. Rather, they represent the average impact over a 20 year period.

This study provides extensive cross-sectional evidence as to which firms are most affected by these banking frictions and therefore which types of firms are more heavily reliant on their existing banking relationships. Young firms and firms with a high fraction of bank debt are especially sensitive to changes in the condition of their existing lenders, as are firms with low asset tangibility. This effect is also significantly larger for firms with outstanding non-investment grade public debt, suggesting that bank capital is not interchangeable with risky public debt, and providing new evidence for bank capital as complement to rather than a substitute for high yield bonds.

The time series evidence afforded by the 20-year panel sheds light on the economic conditions in which these banking frictions are most likely to be visible, and helps to demonstrate why many scholars and industry experts hypothesized that banking relationships were no longer important. Breaking this effect down over time reveals that most of the statistical significance is clustered in the early 1990s, though it is not specific to any one event. While the effect becomes larger and more significant at the beginning of the recent financial crisis, it is remarkably insignificant during most of the early 2000s, even around the recessionary period of 2001. This period saw a smaller, but still significant uptick in bank loan nonperformance, and the residual cross sectional variation is substantial. I propose some potential explanations for this diminished significance, and show evidence that the effect may be even larger during the 2007-2008 financial crisis. Overall, it appears that macro conditions in the early 2000s, either by policy or chance, obscured the still critical role of healthy banking relationships in the U.S. economy.

A number of previous studies have examined how shocks in the banking sector impact borrowing, capital allocation, and valuation among bank dependent firms. Much of the existing literature has centered on event study methodologies surrounding either large bank failures, large financial crises or both. Examples include work by Slovin, Sushka, and Polonchek (1993), Cosimano and McDonald (1998), Kang and Stulz (2000), Bae, Kang, and Lim (2002), Ongena, Smith, and Michalsen (2003), and Chava and Purnanandam (2010). Additionally, Chava and Purnanandam (2010) also examine the impact on certain real variable outcomes on bank dependent firms as compared to non-bank dependent firms during the Russian financial crisis. Similar to this, studies such as Paravisini (2008) , Khwaja and Mian (2008), Paravisini, Rappoport, Schnabl, and Wolfenzon (2011), Lin and Paravisini (Forthcoming), Peek and Rosengren (2000), Klein, Peek, and Rosengren (2002), and Gibson (1995) have examined these effects in or emanating from countries outside the U.S., and but all of them rely on a single large scale event for identification.

The primary identification in nearly all of these studies is through a difference-in-differences specification around a single crisis event. Evidence on investment impacts is often mixed, finding only weak subsample evidence of investment effects in some periods and stronger ones in others. The existence of these crisis events are quite useful in their ability to identify the source of variation in bank condition, but they leave open the possibility that the results are event specific. More importantly, we would like to know if these bank dependencies are more general frictions or if they only matter in times of economic crisis.

This question is more general, but also consequently harder to identify. To do this, I create a large sample of banking relationships as a function of their outstanding loans, and I track shocks to these banks by matching them to their regulatory filings. Using data on the banks themselves, I identify shocks resulting from sources outside the firm. Since the firm's borrowing makes up a tiny portion of the bank's loan portfolio, and the effect is primarily driven by changes in non-commercial lending, I eliminate direct reverse causality. I then extensively test and compare various specifications to argue that these cross-sectional shocks are not related to the investment opportunities of the firm. This study examines nearly two decades worth of information spanning 3 recessions, two banking crises and a diverse set of economic regimes. This is the first study to attempt to comprehensively document how and when these individual banking relationships matter to corporate investment and to do so in a setting, the United States, where bank relationships were assumed to have been been largely supplanted by market based financing and transaction oriented banking models.

The paper is organized as follows. Section 2 describes the identification strategy, details how the isolated sources of variation map to existing economic theories, and discusses the ultimate economic interpretation of the results. I further describe the tests which help validate the identification strategy as part of the panel data. Section 3 presents the data and the details of the sample construction. Section 4 presents evidence for the proposed link between bank financing and investment. Section 5 presents and discusses the main investment results. Section 6 examines the cross-sectional and time-series differences. Section 7 concludes.

2 Identification Strategy

The identification strategy I use in this paper is analogous to the difference-in-differences strategy used in many previous studies, though the exact method employed is slightly different. In a diff-in-diff estimation, the econometrician identifies an event at a particular time which affects one group of firms more than another. For instance, we might wish to examine an event which occurs in year t which impacts a group of banks A more than another group of banks B. We would then compare the change in investment, from time $t-1$ to time t, of firms which borrow from banks in group A at $t-1$ with those that borrow from banks in group B. If we can argue that the event did not *differentially* impact the investment opportunities of either group of firms, then we can argue that the estimated difference in mean investment was a result of the difference in exposure to the event itself.

Using a database of loans, I trace out the primary banking relationship of a large sample of U.S. firms over varying lengths of time. For each firm in my sample, I match a bank during the period after the relationship was initiated. Using a firm-bank fixed-effects specification, I measure changes in the non-performing loan ratio of each bank for each relationship period. Since I observe each firm in the sample, I can establish that this change in nonperforming loans (NPL) does not result from defaults at any of the firms. Using year fixed effects, I isolate the remaining cross-sectional difference between firms attached to banks with negative shocks to NPL to those attached to banks without shocks to NPL. As such, I'm comparing one firm as a function of its existing banking relationship to another firm as a function of its existing banking relationship. I will argue that these relative changes, holding aggregate macroeconomic condition fixed, emanate from sources which are uncorrelated with the residual changes in the investment opportunities of the firm. The nature of the panel allows me to perform several indirect tests of the validity of this assumption, most of which are not possible in a single period diff-in-diff specification.

My identification strategy hinges on the the fact that I can uniquely identify a firm-bank relationship on a one-to-one basis through time. Using geographic and industry specific time-effects allows me to further isolate the variation and identify the effect from variation that comes solely from differences within a certain region or industry at a particular time. I establish that the measured effect is present only when there is an active lending relationship, and is not present for the firm-bank pair before the relationship begins or after it is concluded. I also demonstrate that the effect is primarily driven by changes in the non-commercial lending arm of the bank. This provides assurance that unidentified defaults from the firm are not driving the result and that persistent poor selection by the commercial lending division is not driving the result.

The identifying variation in the model provides and important window into bank switching costs. Since I am comparing a firm attached to bank A against a firm attached to bank B at any given point in time, I am implicitly measuring the extent to which capital from one bank is fungible with another. Early work by Diamond (1984), Fama (1985), and Bernanke (1983) emphasize the role of banks in resolving asymmetric information problems, while Rajan (1992), Petersen and Rajan (1995), and Diamond and Rajan (2001) further demonstrate how these relationships can also impose ex-post costs on the borrower once the bank gains an informational advantage over its competitors. This highlights the difference *between* banks rather than simply the difference between bank and non-bank capital. This identification strategy also complements evidence on the price and non-price "bank effects" on loans found by studies such as Hubbard, Kuttner, and Palia (2002), Santos and Winton (2009), and Murfin (2012) . Since the bank condition also impacts real investment, this implies that these banks exert some monopoly power and suggest that that the loan effects are indeed the result of pricing power by the individual lenders.

3 Data and Sample Construction

3.1 Data Sources

To examine the link between firm investment and the health of its main bank, I first have to identify which bank is the firm's primary lender during any given period. For firms in the United States, this is difficult to observe directly since firms do not ordinarily disclose their primary banking relationship and, unlike Japan, no institutions exist to formalize this relationship. Ideally, I would want a self-reported list, from each firm, of both the bank they consider to be their primary lender in each year and the total amount of outstanding debt held with each lender. Since I cannot observe this information directly, I use available data on major new loans to construct an estimate of the strength and duration of these relationships.

To do this, I combine data across three different databases. The first is the Dealscan database of commercial loans, provided by the Loan Pricing Corporation. This database lists loans to various corporations from 1988 to early 2007 as well as various details about the credit facilities. While this database does not contain all loans made, it does record the majority of all major new loans made to public firms in the United States. According to Carey and Nini (2007), Dealscan has information on 50-75% of all U.S. commercial loan volume into the early 1990s, with coverage increasing to 80 - 90% from 1992-2002. The database contains information such as the dollar amount of the loan, the maturity and interest rate spreads on each loan, and select information about the type, purpose, and contractual terms contained in each loan. Data is available from 1988 to early 2007, and the database becomes progressively more comprehensive during later periods.

Using the name, ticker symbol, dates of operation, and observed loan sizes, I match the borrowers of each loan to CRSP-Compustat. I then use the name, location, dates of operation, and loan size information to match the lenders to the quarterly Call Reports data provided for all chartered U.S. commercial banks. This data comes from the required quarterly submissions of financial status for all chartered banks that fall under the jurisdiction of the Comptroller of Currency or the Office of Thrift, and it is made available by the Federal Reserve Bank of Chicago.

3.2 Panel Construction

The matching procedure establishes a one-to-many link between each firm and lender for a given credit facility. A firm may, and often does, have multiple credit facilities from the same bank within the same month or year. Each firm also often borrows from multiple banks and has outstanding loans from multiple different creditors at any given time. Using the effective start date and maturity of each loan facility allows me to create an overlapping timeline of lending relationships for each individual company to which a bank can be successfully matched.

For each firm-year, I take the total estimated outstanding loan amount and determine which lender holds the largest proportion in each year. This requires estimating the total share of the outstanding loan balances for each period held by each matched bank and calculating the largest lender for that period.

[Figure 1 about here.]

For example, assume a firm with no outstanding loans takes on Loan 1 during year t for $1 million, which matures during year t+3, prior to the end of the year. For simplicity, we will assume that each loan is paid in full only at maturity. At the end of year t, t+1, and t+2, the firm is estimated to be holding $1 million from this loan. If Loan 2 is then initiated during year t+2 for $4 million, the estimated total outstanding loan amount for the firm at t+2 is $5 million. If Loan 1 is funded entirely by Bank A, then Bank A is recorded as holding 100% of all outstanding loans at the end of year t and t+1, making Bank A the firm's primary bank. If Bank B holds 75% of Loan 2, then Bank B is recorded as holding $3 million of all outstanding bank debt at t+3 (60%) and replaces Bank A as the new largest lender for subsequent periods.

The process detailed above yields a panel of firms with an approximation of their outstanding loan balance in each year. This panel is unfortunately unable to distinguish, except in a few specific circumstances, if any of the outstanding loans were terminated early or refinanced by a new loan. This procedure may overestimate the strength of a given lending relationship where this is the case. To deal with this, I apply some basic screens to each observation. In particular, I require that the total estimated outstanding bank loans cannot exceed the total debt liabilities of the firm as recorded by Compustat, and the estimated total amount pledged to a particular firm cannot exceed the capital of each bank. While these screens do not fully eliminate this type of error, classifying a banking relationship as stronger than it actually is should bias against my hypothesis.

I am unable to match a lender wherever a firm's primary lender is a foreign bank or a non-bank financial institution, and the firm is not included in the sample. This yields a number of periods where the firm's largest lender is not identifiable, even though it has outstanding loans from one or more commercial banks. When the largest lender cannot be successfully matched to the call report data, I retain the second largest debt holder that can be successfully matched. This produces a rough estimate of each firm's primary banking relationship in each year, in terms of which bank holds the largest fraction of the firm's institutional debt. As this matching process is quite coarse, I do not place any preconditions on the structure of the debt that identifies the relationship. The issued debt which establishes the relationship may be a long term amortizing loan or a revolving multiyear facility, though most relationships are established by a mix of both. Since the actual holdings are estimated with a fair amount of noise, the purpose of the matching criteria is not to explicitly classify the full exposure of each bank to each firm, but simply to identify which bank is most active in providing debt capital to the firm at a certain time.

This procedure generates a single time-series for each firm where the status of the largest recordable loan holder is matched to the firm. The resulting panel, with unique observations for a given borrower at any point in time, is matched to the firm and bank data provided by Compustat and the Call Reports. To avoid measuring effects which influenced the initiation of the banking relationship, I remove all observations where the bank initiated a new primary banking relationship in the past year, since bank health is measured as of the beginning of the year. Thus, for all firm-bank pairs in the panel, the firm has had an established lending relationship with that bank for at least a year. I also remove all companies who were in default on any rated credit obligations or reported themselves as non-current with existing borrowing obligations, public or private, during any period.

3.3 Summary Statistics

Variables are constructed from Compustat, Dealscan, Call Report information, or a combination of the three. To remove the presence of extreme outliers, I trim investment, cash flow, Q, and Altman's Z-score at the 1% and 99% levels. To verify the proper bank and borrower identification, the estimated outstanding loan balance must be less than the total outstanding debt of the firm as recorded by Compustat. I omit all financial and real estate firms as well as all regulated utilities. I also require at least 2 observations for each firm-bank pair so that no observation is uniquely identified by fixed effects.

[Table 1 about here.]

The final sample consists of approximately 3,500 observations containing 878 unique firms and 95 unique banks, spanning from 1987 to 2008. Summary statistics are reported in Table 1. Firms in the sample tend to be larger than the average Compustat firm but similar along most other dimensions. Importantly, the sample is also diverse in terms of industry and geography. Firms in the sample have headquarters located in 49 different states and represent all 28 of the remaining 30 Fama-French, excluding financials and utilities.

The commercial banks in the sample are quite large, with a mean bank size of over $100 billion in assets. The average bank size has also changed quite radically over the 20 year sample. Prior to 1990, the top three banks in the sample recorded between $80 to $160 billion in total assets. With the wide spread consolidation in the banking industry over the last 10 years, the largest three banks have well over $1 trillion in assets in 2008. The average primary banking relationship lasts around 4.5 years.

4 Bank Health and Firm Investment

4.1 Determinants of Bank Health

Existing banking literature largely relies on three measures of bank health. Broadly speaking, these are equity capital ratios, the nonperformance of existing loans, and the bank's debt rating.[2] From a theoretical perspective, each of these commonly used measures proxies for the ability of a bank to make new loans from existing funds or new capital. Since a bank must raise funds it lends out from depositors or security investors, anything that significantly impairs this process will prevent the bank from functioning efficiently. Regulatory restrictions force banks to maintain certain levels of non-deposit capital, causing

[2] Since this study is focusing on individual banks, many of which are not publicly held, the use of debt ratings is beyond the scope of this paper.

distressed banks to restrict lending or raise new equity capital.

Since non-deposit capital is uninsured, its issuance is subject to adverse selection problems which make it proportionally more expensive (Stein, 1998). Outright bank failure, where the bank becomes completely unable to recapitalize and collapses, represents the most severe case of this problem. Barring an unexpected run on deposits, nonperforming loans are usually the most important determinant of this, since they must be reserved for, reducing loanable funds. Thus, nonperforming loan ratios proxy for the ability of banks to continue future operation in the face of capital constraints.

Before testing the net effect of bank health on firm investment, I first need to determine whether these bank health indicators are correlated with tightened lending in the banks in my sample. Specifically, I wish to test whether changes in the capitalization and nonperforming loan ratio affect the net amount of new lending made in the subsequent year. To test this, I regress the 1-year log change in commercial and industrial loans of each matched bank against the capitalization ratio, the total non-performing loan ratio and a group of control variables. The specification also contains bank and year fixed effects.

For robustness, I also reduce the sample to contain just the set of bank observations for which I also have a matching borrowing firm-year observation in the full bank-borrower sample. Since I am identifying the change in bank lending behavior directly from bank variables, I do not have the additional identification that I obtain in later tests on the matched sample. As a result, these results should be interpreted with caution. However, consistent results from this test will at least confirm the relationship between lending and the non-performing loan ratio over the sample period.

[Table 2 about here.]

The results, presented in Table 2, confirm the relationship between bank lending behavior and the fraction of non-performing loans at the beginning of the year. Column (1) reports the results for all sample banks from 1986 to 2009 during all years of operation. The coefficient on the nonperforming loan ratio is negative and significant, indicating that an increase in the fraction of nonperforming loans is associated with a subsequent decrease in net lending over the next year. The significant negative coefficient on assets is possibly attributable to large changes in size due to merger activity, which are not properly accounted for in the base lending estimate. Column (2) excludes all periods in which the bank acquired another bank, which may have substantially changed its lending base. Reassuringly, the coefficient on assets becomes indistinguishable from zero in this specification, and the effect of non-performing loans modestly increases in magnitude. The results in Column (3) restricts the sample solely to the periods for which I have a matching firm-bank relationship. The coefficient on nonperforming loans remains negative and significant over this smaller sample, and the results are generally consistent with bank behavior across the entire sample. However, the capitalization ratio is insignificant in the full sample and actually marginally negative when I restrict the sample to bank observations which I can match to an active borrower. This is somewhat surprising and likely due to the across bank heterogeneity in regulatory requirements throughout the sample period.

Since this analysis examines a 20 year panel, changes in U.S. regulatory capital requirements make it difficult to measure overall health from reported capital requirements. These requirements have changed radically during this period and have been imposed unequally for different classes and sizes of banks. For estimates around a particular financial shock, within a short time horizon where regulatory requirements do not significantly change over the period or across banks, changes in a single measure of capital adequacy can provide means for reasonable inference. Over the course of my sample the calculation and required level of regulatory capital ratios have changed significantly across different U.S. banks. For instance, certain large, money center banks have periodically had their required ratios reduced in the last 8 years, which would make cross-sectional comparison through time invalid for both large and small banks. Similarly, changes in risk weighting and the accounting for treasury holdings in the early 1990s temporarily allowed more liquid banks to manipulate their risk-adjusted capital ratios upward.[3]

The information contained in nonperforming loan ratios is less ambiguous. Nonperforming loans result from the bank's existing business, and an increase in this ratio always moves a bank closer to its existing capital limits. There is also much less discretion in reporting or transactional manipulation than in other measures of bank performance. For these reasons, nonperforming loans are often the most significant determinant of bank failure and net bank lending in existing studies. This is reinforced in a recent paper by Huang (2009), which finds a strong correlation between nonperforming loan ratios and the recall of revolving credit lines during the current financial crisis.[4]

One important benefit of being able to use the nonperforming loan ratio is that this bank level measure responds only to defaults and non-payments by its existing borrowers. In forming the sample, I remove all firms who are reported as non-current on their existing debt payments. In the unlikely event that a large portion of the outstanding loan to each firm is declared to be non-accruing without being disclosed by the firm, this should still have little direct impact on the bank level measure of nonperforming loans since each individual firm makes up only a small portion of the banks total loan portfolio. In an unreported test, I also verify this question directly by separating out the commercial loan nonaccruals from other loans, and I find similar results. This helps directly address the reverse causality

[3]Tests of the specified regressions do not significantly load on any of the commonly used capital adequacy ratios imposed subsequent to Basel I, Basel II, and the FDICA, many of which are not available in the earlier part of the sample. The inclusion of these ratios does not materially change the results of the test.

[4]See also Peek and Rosengren (2000), Paravisini (2008), Wheelock and Wilson (2000), Campello (2002), Ashcraft (2005), Lown and Peristiani (1996), and Hubbard, Kuttner, and Palia (2002), among others.

problem inherent in many studies of this type.

4.2 Bank lending and Firm Debt

Having verified that my measure of bank health is related to the lending activities of the banks in my sample, I now examine whether this shift in lending behavior also affects the borrowing behavior of the matched firms. For this test, I turn to the matched sample described in section 3.2. In this model, I employ separate fixed effects for each firm-bank pair, and year fixed-effects. The model identifies differences in net debt issuances as a function of changes in the health of the matched bank in the preceding period. The model is as follows.

$$\Delta Debt_{i,t} = \alpha + \beta_1 Nonperforming_{j,t-1} + \beta_2 X_{i,t-1} + C_{(i,j)} + T_t + \epsilon_{(i,j),t} \tag{1}$$

Change in debt, for Firm i, is measured either as the difference, scaled by assets, in Period t debt from Period t-1 debt or as the difference in *log* debt over the same period. $Nonperforming$ represents the nonperforming loan ratio of Firm i's Primary Lender j at the beginning of each period, and $X_{i,t-1}$ represents a vector of additional lagged firm-specific control variables. $C_{(i,j)}$ represents firm-bank fixed effects for each firm-bank pair (i, j), and T_t represents individual year fixed-effects. The results are presented in Table 3.

[Table 3 about here.]

Columns 1 and 2 estimate the model with net debt scaled by assets. The coefficient on $Nonperforming$ is negative and significant, implying that an increase in the nonperforming loan ratio of the identified primary bank, holding macroeconomic conditions fixed, leads to a decrease in net debt issuance over the subsequent period. Specifically, an increase in the nonperforming loan ratio of from 1 to 2% is associated with a net debt issuance that is 1 to

1.15% of assets lower. Results are similar when expressed as a change in log debt. The mean change in any given year is around 5%, so the results, while not large in absolute terms, do at least suggest a meaningful average effect.

They also help confirm that the matching process produces meaningful results. Within the specified time period, relative differences in bank health, specific to Bank j, are related to the borrowing behavior of the matching Firm i. I further address the identification of this effect in the following section.

5 Bank Health and Firm Investment

5.1 Main Results

Having established the relationship between bank health and borrowing activity within my sample, I now estimate whether there is any direct impact on firm investment. While relative bank health has been shown to have an impact on borrowing behavior and lending terms to firms in the U.S., there has been little direct evidence to suggest that it influences firm investment. Establishing whether or not this is true is especially important for U.S. firms, who are assumed to be less bank oriented than firms in other countries such as Japan, and presumably have greater access to alternative forms of financing.

My first test examines the impact of the main bank's nonperforming loan ratio on the annual investment level of the firm, holding all economy wide macroeconomic variation fixed. The model specification is as follows:

$$
\begin{aligned}
Investment_{i,t} = \alpha + \beta_1 Nonperforming_{j,t-1} + \\
\beta_2 TobinsQ_{i,t-1} + \beta_3 CashFlow_{i,t} + \\
\beta_4 X_{i,t-1} + C_{(i,j)} + T_t + \epsilon_{(i,j),t}
\end{aligned} \tag{2}
$$

Investment is firm capital expenditures divided by lagged PPE. *TobinsQ* is the market value of equity plus the book value of debt minus deferred taxes divided by book value of assets. *CashFlow* is the net income before extraordinary items plus depreciation and amortization divided by lagged PPE. *Nonperforming* is the ratio of nonperforming loans (loans not accruing plus loans 90+ days late) to total loans of the primary lender. $X_{i,t-1}$ represents a vector of additional lagged control variables. $C_{(i,j)}$ and T_t represent firm-bank and year effects, respectively. In all specifications, *TobinsQ* and *CashFlow* are treated as control variables. I treat the relationship between cash flow and investment as part of the statistical model, but I place no causal interpretation on the relationship as indicative of financing constraints. Identification of the effect is determined by the coefficient on *Nonperforming*. The inclusion of firm-bank fixed effects identifies the effect of relative changes in the *Nonperforming* on the variation in firm investment.

The year fixed-effects in this specification are particularly important to the interpretation of the model. By controlling for time, I am isolating the variation in *Nonperforming* that is due to the difference in bank performance within each year. Results from the model therefore capture the effect of changes in bank health *relative* to overall bank health, keeping fixed the health of other institutions. This both controls for macroeconomic trends and serves to identify the *lender specific* impact of bank distress suggested by the theory.

[Table 4 about here.]

Results are presented in Table 4. Column (1) contains the coefficient estimates for the baseline model. Controlling for both firm-bank and year effects, a one standard deviation increase in the nonperforming loan ratio of 2% decreases the average investment by around the same proportion, or 10% of average investment. For comparison, the total nonperforming loan ratio across all U.S. banks increased from 1.63% in September of 2008 to 3.75% in 2009. This represents a 2.12% increase for the average bank in this period. During the same period, the nonperforming loan ratio of Citibank, the largest U.S. commercial bank, increased by nearly twice as much.

These results can be qualitatively compared to the results generated by Gibson (1995). He finds that firm investment is 30% lower for firms with the weakest banks (the worst credit rating) relative to firms with the highest rated banks, with the highest rated banks constituting roughly half of the firm observations. A direct comparison is somewhat difficult, as the firms in his sample tended to have lower growth opportunities and with an average investment of around half that in my sample. As such, a given percentage drop in investment may be more economically meaningful for the firms firms in his sample. However, it is worth noting that the estimated effect in my sample is similar in magnitude. While firms in Japan may be more bank reliant, bank health is still similarly important in determining firm investment in the U.S.

An important consideration is that, unlike other studies which directly examine cash flow sensitivities, my tests primarily include cash flow as a control. As such, I do not filter out periods of negative cash flows in my estimation. In any given year, between 15-20% of companies in the Compustat universe have a negative net cash flow, defined as earnings before extraordinary items plus depreciation and amortization. In my sample, 11% of the firm-year observations contain a negative cash flow.

As documented by Allayannis and Mozumdar (2004), negative cash flow realizations do not lead to systematic disinvestment by firms. The theoretical and empirical relationship between cash flow and investment is therefore different when firms experience negative versus positive cash flows. Practically speaking, the slope of the cash flow coefficient has a kink at zero, below which the coefficient is close to zero. As a consequence, the inclusion of these observations generates an average cash flow effect that is around half to one-third of what is typically reported by studies which either explicitly or implicitly screen out these observations. To deal with this, I also include an indicator variable and an interaction term for periods of negative cash flow. This allows the cash flow coefficient to change when cash flow realizations are negative. In specification (3), as a robustness check, I include only firm-year observations in which the cash flow was positive.

The interaction between cash flow and nonperforming is positive in specification (3). This provides some evidence that firms with more internal cash flow are less dependent on the stability of their bank. The fact that it is almost indistinguishable from zero in the full sample also suggests that this relationship does not hold when cash flows are negative. Since investment is not materially sensitive to negative cash flow realizations, it is reasonable to assume that firms in these situations face a very different investment decision and are not proportionally more bank reliant when their cash flows are "more" negative.

I include additional controls such as Altman Z-score, asset size, and dummies for whether the firm is rated and if that rating is BB+ or less in specification (4). In specification (5), I include the contemporaneous non-performing loan ratio as a robustness check to identify whether the relationship is being driven by contemporaneous economic conditions. The interpretation of the main effect becomes somewhat more complicated in this specification, but the dominance of the lagged parameter indicates that the effect on investment is specific to the beginning of year bank condition. This gives additional evidence that the bank condition is directly affecting the behavior of the firm.

5.2 Additional Controls

While the year fixed effects specification holds economy wide macroeconomic constant, it does not necessarily capture time variation within the economy that may affect one particular group over another. Specifically, if there is a common economic component which is specific to groupings of firm-bank pairs, then the existing time effects would not fully control for the omitted variable. One possibility is that local economic shocks may affect both banks and firms located in a specific region. Overall bank health is often related to local economic conditions, especially in real estate markets, which may be correlated with a decrease in regional demand, and lower investment opportunities for regional firms. Another possibility is that certain banks may be over-exposed to certain industries, and factors affecting that industry would disproportionately affect the investment opportunities of the majority of their matched borrowers.

[Table 5 about here.]

To address these concerns and help establish the validity of my identification strategy, I re-estimate the model with more stringent category specific time effects. To control for time varying regional economic conditions, I replace the year-effects with region-year effects. This specification includes a separate year dummy for each of the 9 major census divisions in which the firm's headquarters are located. The results are presented in Column (1) of Table 5. To address the possibility of unobserved industry effects, I implement industry-year fixed effects in a similar manner. Using 30 Fama-French industries, I include a separate effect for each industry in each year. These results are presented in Column (2). In both cases, despite a significant reduction in identifying variation, the effect remains significant. More importantly, the coefficient estimate remains largely unchanged from the main regression.

While these classifications cannot completely account for unobserved economic specialization in lending, the industry-year controls should substantially reduce the coefficient if the effect is primarily driven by endogenous correlation. For this not to be the case, the common economic component must be almost entirely driven by unobserved differences within each region or industry at each point in time. If the null hypothesis that bank health has no direct effect on firm investment is true, removing the across country and across industry variation from the regression should significantly reduce the magnitude of the coefficient. Unreported Hausman tests of the single coefficient model also fail to reject the baseline specification as inconsistent compared to the more robust specifications.

The robustness of the effect to alternative specifications also illustrates the relative independence of the matching bank's health after controlling for the fixed-effects. The addition of firm level controls and within category time effects does little to alter the point estimate of the main coefficient. This indicates that the de-meaned effect is strikingly orthogonal to most firm level measures which are thought to determine investment. This allows reasonably good identification in a relatively restrictive model, even though the matching process is somewhat noisy.

5.3 Measurement Error in Q

Since Tobin's Q, as a proxy for the firm's marginal q, is a control variable for the differential impact on investment, it is useful to consider the extent to which the isolated variation in NPL ratios is correlated with Q. Tobin's Q is generally considered to be a mismeasured proxy for marginal q, and the extent to which the proxy fails to fully capture the variation in investment opportunities can cause spurious loanings on the other regressors. Erickson and Whited (2000, 2002) suggest a two step GMM estimation using higher order moments to more precisely identify the mismeasured regressor. The general specification is as follows:

$$y_i = z_i\alpha + \chi_i\beta + u_i$$

$$x_i = \gamma + \chi_i + \epsilon_i$$

Where χ is the mismeasured regressor, in this case marginal q, and z is a vector of perfectly measured regressors. This estimation requires that the mismeasured regressor be non-normally distributed, that $\beta \neq 0$, and a further requirement that the errors be i.i.d. In applying this estimator to firm investment, econometricians usually assume that there is some cross-sectional correlation in the error term due to time varying changes in the investment opportunity set of all firms. In this model, I assume that this cross-sectional correlation is swept out by the year fixed effect and that the remaining variation is reasonably close to i.i.d.

[Table 6 about here.]

The model accounts for the firm-bank fixed effects by first de-meaning the levels for each variable. The results of this specification are presented in Table 6. Model GMM3 in column (1) is estimated using the skewness, model GMM4 in column (2) is estimated using skewness and kurtosis, and model GMM5 in column (3) is estimated using higher order moments up to 5. In each case the error-ajusted effect of q increases while the impact of cash flow diminishes, suggesting a significant attenuation bias in Tobin's Q. The effect of nonperforming loans however, remains effectively unchanged. The fact that this effect not merely survives but is marginally strengthened provides important evidence that the identifying variation in NPL is uncorrelated with the investment opportunities of the firm.

5.4 Out of Sample Comparison

While the existing test rules out the possibility of national, regional, or industry level economic drivers, performance deterioration in the current bank may proxy for some different, unobserved deterioration in economic conditions experienced by the firm. While the effect remains very robust when controlling for endogenous changes within industry and region, it remains possible that the banks exhibit lending behaviors that expose them to similar risks that are unrelated to region or industry. If this is true, the performance of that particular bank's loan portfolio would always move with the investment behavior of the firm, regardless of whether the firm relied on the bank for capital.

To address this issue, I appeal to the following argument. If the health of a firm's bank matters directly though a financing channel, there should be an observable effect when the firm has an active borrowing relationship with the bank. Prior to initiating a borrowing relationship with the bank and after termination of the borrowing relationship, there should be no sensitivity to bank performance, as the firm is no longer reliant on that particular bank to supply new capital. Alternatively, if the relationship is driven by joint exposure to common economic factors, the relationship should persist regardless of whether the firm is currently reliant on that particular bank for capital.

To test this, I take each unique firm-bank combination within the sample and create a separate time-series for each pair. Each firm-bank pair contains the observations from the sample, all available observations prior to the first loan made from the bank to the firm, and all available observations subsequent to the expiration of the last loan made. For the new panel, I create indicator variables that take on a value of 1 for periods prior to, during, and after the active firm-bank relationship window. I then include one or more interaction variables which interact the status of the banking relationship with the nonperforming loan ratio.

The specification is as follows:

$$Inv_{i,t} = \alpha + \beta_1 Nonperforming_{j,t-1} + \beta_2 X_{i,t} + \tag{3}$$
$$\beta_3 I_{active}[0,1]_{i,j,t} + \beta_4 I * Nonperforming_{j,t-1} + C_{i,j} + T_t + \epsilon_{i,j,t}$$

So long as banks and firms don't radically alter their exposure to external economic factors when they initiate or sever a relationship, a bank specific effect should be significant only when the relationship is active. In the context of the model β_4 should be significantly less than zero while β_1 should be close to zero. Alternatively, a common economic shock should generate a negative relationship in all periods, producing a β_1 similar to the previous specification and no loading on β_4. While the decision to switch banks is not exogenous, I argue that a quite radical shift in economic exposure is necessary during each firm's switching event for the relationship to not continue to hold in some form. The results, presented in Table 7, should therefore provide a reasonable test of whether the effect is specific to the firm-bank relationship.

[Table 7 about here.]

Column (1) estimates the main regression on the panel in the pre and post relationship periods, *excluding* the main sample period. Consistent with the idea that the effect is solely a function of the existence of a lending relationship, the coefficient on nonperforming loans is not significantly different from zero. Column (2) estimates the baseline effect for the entire sample, with an additional interaction term taking on a positive value during the active sample period. This interaction model gives an estimate of the additional effect that presents itself when there is a banking relationship. The coefficient is negative and significant, while the baseline coefficient remains indistinguishable from zero. The model in column (3) jointly estimates a separate effect in each period, before, during, and after a lending relationship is in place. In all time periods where there is no lending relationship, the coefficient is indistinguishable from zero.

One particular concern in this analysis is that the estimation of the termination of a banking relationship is quite noisy. The primary problem I face is that, since Dealscan does not contain the entire universe of loans made to these companies, I cannot determine completely whether the borrower is maintaining an active borrowing relationship and the loans were simply not recorded by LPC. There is no clear way to distinguish between a true cessation of the relationship and simply not being able to observe a new loan. However, such sample error should bias the tests against this finding.

The validity of these out-of-sample comparisons rest on the assumption that the banks in the sample are not radically altering their exposure to the industries these firms represent during what I measure as active and inactive lending periods. While I cannot directly observe the industry concentration of each bank from the Call Report data, I can examine a rough estimate of the industry concentration of each bank in each period through the observed lending activity in the Dealscan database. As previously mentioned, Dealscan does not list all of the lending activity of each bank, but it does record a significant percentage of the total dollar volume of lending. Taking the loans recorded in Dealscan, I can estimate the fraction of each bank's aggregate lending volume attributable to the firm's primary industry for each measurement period.

[Figure 3 about here.]

For each firm-bank pair, in both the active and inactive relationship periods, I calculate the total lending volume to all companies in Dealscan with the same Fama-French industry classification as the firm. I then divide the amount lent to each industry by the total lending volume during the same period. The average of these ratios for each period represent the average industry lending concentration of the bank to the industry of the firm when the lending relationship is active and when it is inactive. To reduce noise artificially created by small sample issues, I require that at least two loans be made to each industry in the representative periods. This ensures that the difference in average industry lending activity of each bank is determined by multiple sample loans. As a further robustness check, I examine a cutoff of at least 10 loans to a given industry in each lending period. The results are represented graphically in Figure 3. Means tests reveals no consistent statistical difference in the industry concentration of each bank across the sample periods. Though time variation may exist in industry lending activity by banks, as documented by previous studies, this variation is small and does not appear to significantly skew the selection of firm-bank relationships in the sample. This gives reassurance that the banks in this sample are fairly well diversified in their commercial portfolios and that the sample firms do not represent a weighted selection of concentrated lending activity.

5.5 Reverse Causality, Selection, and the Source of Nonperformance

Identification in this model is achieved through changes in the nonperforming loan ratio subsequent to the establishment of a main bank relationship. The model implicitly assumes that the evolution of the bank's relative nonperformance is driven by changes in the bank's portfolio which are largely unrelated to the firm. This assumption would be violated if this change in performance is driven by common unobserved selection characteristics in the commercial lending portfolio. Essentially, one may worry that the commercial lending arm of a given bank simply makes bad decisions across the board, which result in bad loans and

and systematically underperforming firms. This could be thought of as a kind of indirect reverse causality resulting from the common actions of the commercial lending division at each bank.

The breakdown of information at the bank level allows me to test this question directly. The Call Reports contain some additional classifying information on the type of loan in each bank lending portfolio for the majority of banks in my sample. Using this breakout information, I can identify total nonperforming loan amounts attributable to commercial and industrial loans and to non-commercial and industrial loans. If the result is due to common variation in the selection of commercial lending, the effect should be driven entirely by changes in the C&I nonperformance and not in the rest of the portfolio.

I construct the nonperforming C&I loan fraction as the total C&I loans not accruing and 90+ days late scaled by the total value of the loan portfolio. I can then construct a similar measure of the non-C&I NPL using the non-performance of the real estate, personal, agricultural, and other loans in the portfolio. I then run the main test specification using only the nonperformance in each loan group in isolation and then using C&I NPL as an additional regressor, allowing C&I nonperformance to have a separate, additional impact. The results of this test are shown in Table 8. As shown in specification (1), the primary results hold even when focusing solely on nonperformance in non-corporate lending. Specification (2) reports the results of the main test with an additional impact allowed for by the change in C&I nonperformance. Note that the primary effect of loan non-performance dominates the regression, and the inclusion of C&I nonperformance has no additional explanatory power for investment. Specification (3) reports the same model as specification (2), but reports two separate coefficients for C&I and non C&I loans for the purpose of comparison.

[Table 8 about here.]

This result verifies several important assumptions. First, it helps verify that the results are not being driven by unexplained reverse causality. Second, while shocks to NPL ratio are important for all loan groups, the primary source of identifying variation is coming through shocks to the bank's non-commercial loan portfolios. Lastly, it demonstrates that the effect is primarily related to the direct impact on bank operations and not to the revelation of new information about its past commercial lending activities. Defaults in the commercial lending division of these banks do not have any additional impact that might be related to information. This stands somewhat in contrast to the results of Murfin (2012), who argues that portfolio defaults cause the tightening of non-priced loan terms due in part to the revelation of information by the default. While this information effect may exist, its impact on the cost and availability of investment capital appears to be unimportant in the context of my sample.

5.6 Bank Location and Distance

The physical location of the bank, the location of the firm, and the distance between them provide a useful testing ground for the determinants of bank dependence. In addition to providing robustness checks for correlated economic conditions, the distance between the headquarters of the firm and the headquarters of the bank should be related to the relative importance of bank health and bank financing.

[Figure 2 about here.]

As illustrated in Figure 2, there is wide variation in location across the sample for both borrowers and lenders. In addition to being geographically diverse themselves, there is also wide variation in the distance between the bank and the firm. Borrowers may do business with banks located anywhere from a few miles to over 5000 miles away. The median estimated distance between borrower and lender in the sample is 500 miles, and over three-fourths of the firms in the sample have a borrowing relationship with a bank headquartered in a different state.

Firms borrowing from local banks face similar regional economic conditions. Those banks are likely to have retail customers whose deposits and purchasing behavior are correlated with the local economy. They are also likely to cater to commercial customers in similar industries and with similar customer bases. Firms borrowing from non-local banks do not face these same issues, but they may face greater difficulty when trying to negotiate new loans.

When banks contract their credit supply, they are likely to do so to those customers who are more difficult to monitor. Therefore, if the link between investment and bank health is related to credit availability, we would expect to see the relationship become more negative with greater distances between the firm and the bank.

To test this question, I split the sample into two groups based upon the geographic location of the firm relative to the bank. I gather the city, state, and zip code of each firm headquarters from Compustat and each bank headquarters from their Call Reports. I first divide the sample into firms borrowing from banks located within the same state and firms which borrow from banks in a different state. Second, I divide across firms borrowing from banks within and across the 9 U.S. census divisions. Lastly, I create a physical distance measure by calculating the number of miles between the zip code of the firm and bank. The median distance between firm and bank is roughly 500 miles, so I split the sample between firms who are more than 500 miles away from their bank and those firms who are less than 500 miles away from their bank.

Ideally, I would like to isolate the operating area of each firm and not just the location of the main headquarters. The location of the headquarters is an inexact measure of the geographic reach of the firm and the bank. However, even for firms with a national reach, the location of the firm headquarters still represents the firm's strongest administrative presence.

The results of this specification are presented in Table 9. Bank sensitivity does not vary significantly between firms using local banks and firms using non-local banks. In fact, the magnitude of the effect is slightly higher for firms located across state and regional boundaries. This implies that the measured effect is not being generated by regional correlation in economic conditions.

[Table 9 about here.]

The coefficient is actually significantly larger at the 10% level for firms located more than 500 miles away. This suggests that greater distance may make firms more likely to cut investment. It also suggests that these firms may face greater informational frictions in their borrowing.

Since firms should optimally weigh the information costs of greater distance when choosing their lender, a univariate increase in distance does not necessarily imply greater information costs. However, it is reasonable to assume that firms who are located closer to their lenders do have an informational advantage during times of distress. Additionally, firms who borrow from more distant lenders may be unable to borrow locally because there is not enough local banking capital satisfy their needs. This would also make them more dependent on their existing bank. I will explore these explanations further in section 6.2.

6 The Determinants of Investment-Bank Health Sensitivity

6.1 Cross-Sectional Characteristics

Having established the average investment response to lender health, I now turn to the question of the cross-sectional determinants of this effect. The importance of bank financing varies considerably from firm to firm. Theory implies that the importance of bank capital to a firm is heavily dependent on the degree of asymmetric information, the relative importance of agency considerations, and by extension, the firm's inability to raise external financing from arm's-length transactions. Given this, the impact of bank health should covary with empirical proxies for these determinants.

The reliance on bank capital can be thought of as a specific type of financial constraint that only binds in certain states of the world. It is generally thought that strong banking relationships smooth financial frictions and make firms less reliant on internal cash flows[5]. Firms that rely heavily on bank borrowing make a tradeoff between this benefit and the hold-up costs generated in states of the world where the firm needs to obtain outside financing. The firms most affected by lender health are those which have less access to other forms of financing and those for which asymmetric information make it more difficult to secure new sources of financing.

To test this, I split the sample into groups based on four classification schemes, which serve as proxies for these frictions. For each classification, I rank the firms based on their status as of the year the relationship was initiated so that the time-series for each firm is contained within a single group. First, I split the sample into three groups based on the debt

[5] See Hoshi, Kashyap, and Scharfstein (1991)

rating of the firm at the time the firm-bank relationship was initiated. The three groups represent firms with a debt rating of BBB- or better, firms with a debt rating of BB+ or worse, and firms without a debt rating. Second, I split the sample into groups based on the top and bottom third of the sample based on firm age, asset tangibility, and the ratio of bank debt to property, plant and equipment. Firm age is defined as the number of years the firm has reported in Compustat up to that point, and asset tangibility is the ratio of net property, plant, and equipment to total assets.

Firm age and asset tangibility proxy for the relative transparency of the firm, with older firms and firms with more tangible assets having a greater degree of transparency. The ratio of bank debt to PPE indicates the overall reliance on bank capital and also proxies for the available collateral of each firm.[6]

[Table 10 about here.]

The results are presented in Table 10. Firms with a bond rating of BBB- or better are considered to have investment grade debt. Bond ratings below BBB- are considered to below investment grade, or junk. As expected, firms with investment grade debt do not show a marked sensitivity to their bank's health as compared to unrated firms. Surprisingly, the bank coefficient for the junk rated firms is 2.5 times as large as the average coefficient for the full sample. This suggests that, while these firms may have access to public debt markets, they are also heavily bank dependent.

These results parallel the findings of Rauh and Sufi (2009), who show that low credit quality firms are more likely to have a tiered capital structure that includes covenant heavy bank debt and "rely on tightly monitored secured bank debt for liquidity." They find evidence that firms with low credit quality lose access to arm's-length short term program debt, such

[6] Chava and Purnanandam (2010), for instance, employ the same measure as a proxy for available collateral.

as commercial paper, and are forced to rely on secured bank debt for liquidity. These firms are therefore more likely to have their short term investment influenced by bank decisions, since they are the firms over which banks exert the most control.

The effect is much stronger for firms with low asset tangibility and a large bank debt ratio, suggesting that more opaque firms and firms with less available collateral face greater problems when their current bank is in trouble. As expected, older firms appear to be slightly less sensitive to bank debt, though the difference is not statistically significant.

6.2 Interpretation and the Elasticity of Investment to Credit

The impact of bank health on the investment policy of the U.S. firms in this study is still surprisingly meaningful given the size and scope of these firms and the rough comparisons to countries with far more bank centric systems of corporate finance. To give further interpretation to these results and to further explore the channels through which this effect operates, I employ an instrumental variables model to estimate the elasticity of investment to credit as a function of relative bank health.

To estimate these values, I rely on the fixed-effects identification of the primary regressions. Since the fixed effects specifications are filtering out the common credit demand component related to bank health, I can use the NPL ratio as an instrument for the relative net borrowing of the firm during the period. This isolates the variation in net borrowing that is due solely to relative changes in the health of its primary lender, and excludes the common credit demand component from the borrowing equation. I can then estimate impact of the contraction in credit availability on firm investment using 2-stage least squares. The first and second stage regressions are as follows:

$$\Delta Debt_{i,t} = \beta_0 + \beta_1 Nonperforming_{j,t-1} + \beta_2 X_{i,t-1} + C_{(i,j)} + T_t + \epsilon_{(i,j),t} \tag{4}$$

$$Investment_{i,t} = \gamma_0 + \gamma_1 \Delta \widehat{Debt}_{i,t} + \gamma_2 X_{i,t-1} + C_{(i,j)} + T_t + \epsilon_{(i,j),t} \tag{5}$$

where $\Delta Debt_{i,t}$ is the total net borrowing of firm i in period t scaled by beginning of period PPE, and *Investment* is the capital expenditures of firm i in period t scaled by beginning of period PPE.

[Table 11 about here.]

The results of the first and second stage IV regression are presented in Table 11. For each specification each specification, I also estimate the elasticity of investment to credit $\frac{\partial y}{\partial x} \cdot \frac{x}{y}$, where y is the dependent variable *Investment* and x is the net debt issuance predicted by the main banks relative NPL ratio. The elasticities are estimated both at the mean of the independent variables of each subsample and the mean of each variable in the full sample.

For the full sample, annual investment decreases by 1.3% for every of 10 % decrease in net new borrowing, represented by an estimated elasticity of 0.13. Given this represents the impact on one-year investment from the credit reduction attributable to a single U.S. lender, this impact is surprisingly large. For comparison, Paravisini, Rappoport, Schnabl, and Wolfenzon (2011) also estimate a similar effect of bank credit contractions on real economic activity, in their case firm exports in Peru. They estimate the elasticity of firm exports to bank specific credit shocks in Peru and find an elasticity of 0.23.

The estimated elasticity for firms located more than 500 miles from their main bank is substantially higher than more local firms, with an estimated elasticity of 0.201 to 0.109. While the impact on of bank health on net borrowing is larger for more distant firms, the primary driver of the differential impact on investment appears to be a greater sensitivity of investment to the resulting contraction in borrowing. This suggests that banks are not simply cutting credit to the most distant firms during periods of distress. Rather, these firms appear to be more reliant on credit to finance their investment activities.

Conversely, the elasticity of investment appears to be roughly the same for firms in the sample of junk rated firms, unrated firms, and the full sample. The differential impact on investment reported in Table 10 appears here to be driven by a significantly larger impact on net borrowing. This result suggests that distressed banks are more likely to cut credit to their riskier commercial borrowers or alter the terms of lending in ways which which are more costly to riskier firms.

6.3 Investment-Bank Sensitivity Over Time

Prior to the financial crisis of 2007-2008, many researchers had suggested that bank relationships were becoming much less important in corporate finance. The prominence of "transaction oriented" banking and the rapid consolidation of banks in the last decade changed how banking relationships were perceived. In light of recent events, this conjecture demands further investigation. To address this, I include two time trend variables in the model. First, I include a simple time variable, taking on a value of "1" in 1987 and increasing by 1 each year, interacted with the nonperforming loan ratio. Second, I divide the sample into seven three year time periods and separately estimate the impact of nonperforming loans for each period. The results are presented in Table 12.

[Table 12 about here.]

Under the assumption that bank dependence has been steadily decreasing over time, the interaction of the time trend with the nonperforming loan ratio gives a linear estimate of the change over time. Under this very restrictive assumption, the effect would have diminished to around zero by the year 2000. The period estimation suggests that the effect was measurably significant between 1988 and 1996 and became completely insignificant until the most recent period. Given that the early 1990s represent the last major period of widespread bank distress in the U.S., it is unsurprising that the effect should be most concentrated then since most of the independent sample variation is concentrated there. Consistent with conjectures about the diminished importance of banking relationships, the effect appears to have diminished after then. However, I cannot verify whether this is true since the lack of variation during this period drastically reduces the power of the test.

The point estimate for the most recent period is quite large, even larger than that of the estimated effect in the early 1990s. The standard error is also quite large, so the estimate can only be considered marginally significant. However, combined with recent evidence on the same effect found by studies such as Huang (2009), it does point to a widespread increase in the importance of bank health. This and other studies provide an increasing body of evidence that the stability of banking relationships remains a vital part of efficient corporate investment.

The time variation in specification (3) is graphically illustrated in Figure 4(a). Post 2000, prior to the recent financial crisis, it is difficult to find the same effect as in earlier periods. This effect may simply be due to a lack of identifying variation, so it is difficult to say whether the economic effect is present or simply not identifiable. To get a sense of the identifying variation, I plot the cross sectional mean and standard deviation of the nonperforming loan ratio of the sample banks over the entire period. As illustrated in Figure 4(b), there is a significant decline in the cross sectional variation after the early 1990s. The banking sector as a whole appears to have become more stable. While the recession of the early 2000s witnessed an increase in cross sectional variation, it does not appear to produce an identifiable impact on firm investment.

One possible explanation is that non-bank capital sources were able to more efficiently fill the gap in the post-2000 period. The extreme freeze in the commercial paper markets at the start of the crisis period is often seen as a major contributor to the decline in corporate investment. Figure 4(c) plots the cross-sectional variation in NPL against the 3-month commercial paper rate over the same period. Commercial paper rates do appear to have fallen more precipitously during the 2001-2002 recession period than in the 1990s recessionary period. Additionally, Figure 4(d) documents a steady, marked increase in the average equity capitalization of banks from the early 1990s to the present. It is therefore possible that tighter scrutiny by regulators and increased overall capital requirements helped dampen the impact of bank distress during this period. It is also worth noting however, that the standard deviation of bank capitalization, even relative to the mean, had increased from the early 2000s. Capital requirements during the period appear to have become stricter overall, but regulators also afforded more leeway for certain banks, especially the largest ones which were perceived to be the most stable.

Both industry analysts and academics were not without reason in believing that banking relationships had become less important by the early 2000s. While economic downturns in this period did hurt the banking sector, it appeared as though the spill over effects were far less important than in previous decades. It is understandable why this belief was so persistent, as both policy and the changing structure of the industry may very well have dampened this bank channel during the last decade. What was perhaps unforeseen was the increasing exposure to new economic forces which economists did not yet understand.

7 Conclusion

In this paper, I provide evidence of the impact of bank health on the investment expenditure of their existing borrowers. I find that firms with established borrowing relationships show a significant decline in investment when the nonperforming loan ratio of their primary bank increases. This relationship is specific to the firm-bank relationship, and is not driven by unobserved changes in regional or industry-specific factors. These effects represent a capital constraint over and above the macroeconomic effects of overall credit tightening, and provide new insight into the tradeoffs inherent in bank financing.

The effect is strongest amongst firms that are more informationally opaque, have fewer tangible assets, and have less access to outside financing. These firms are likely to see the greatest benefits from the screening and monitoring activities provided by banks, but they are also more likely to be adversely impacted when their particular bank runs into problems. This is especially important in light of recent events, where the largest banks in the United States have become seriously distressed. These banks were once viewed as being among the most stable institutions in the financial industry, having a reach that far exceeds the major distressed banks of the 1990s.

The largest concern facing policy makers is whether these large-scale failures in the financial sector have led to a debilitating shortage of credit across the board, and what range of policies will be most effective in spurring new lending. However, the reliance of firms on existing banking relationships makes it more difficult for firms to find new sources of capital. This raises the additional concern that access to financing will constrain new investment in a substantive way, blunting the growth of an entire class of firms.

The findings of this paper suggest that the impact on firm investment is reasonably substantive, and has been for most of the last 25 years. This has important policy implications for both regulators and bank dependent firms. It also raises new questions about how banks determine their lending policy to both existing and new clients. The internal workings of bank lending policy are still somewhat of a mystery. On the firm side, it raises questions about how firms form new lending relationships, especially when the costs of switching are significant. Future research in this area will provide an important window into these channels.

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Figure 1: Sample Construction Illustration

The following figure illustrates the sample construction process. Each point on the timeline represents the end of the indicated year. In this example, Dealscan records two loans taken out by Compustat Firm 1. The first loan establishes the main firm-bank relationship between Firm 1 and Bank A, beginning as of the end of 1990. During 1992, a second loan is taken out, funded primarily by Bank B. As of 1992, Bank B now provides the majority of all bank capital to Firm 1. Since Loan 2 expires after the first loan, Bank B permanently supplants Bank A as the firm's primary lender in 1992.

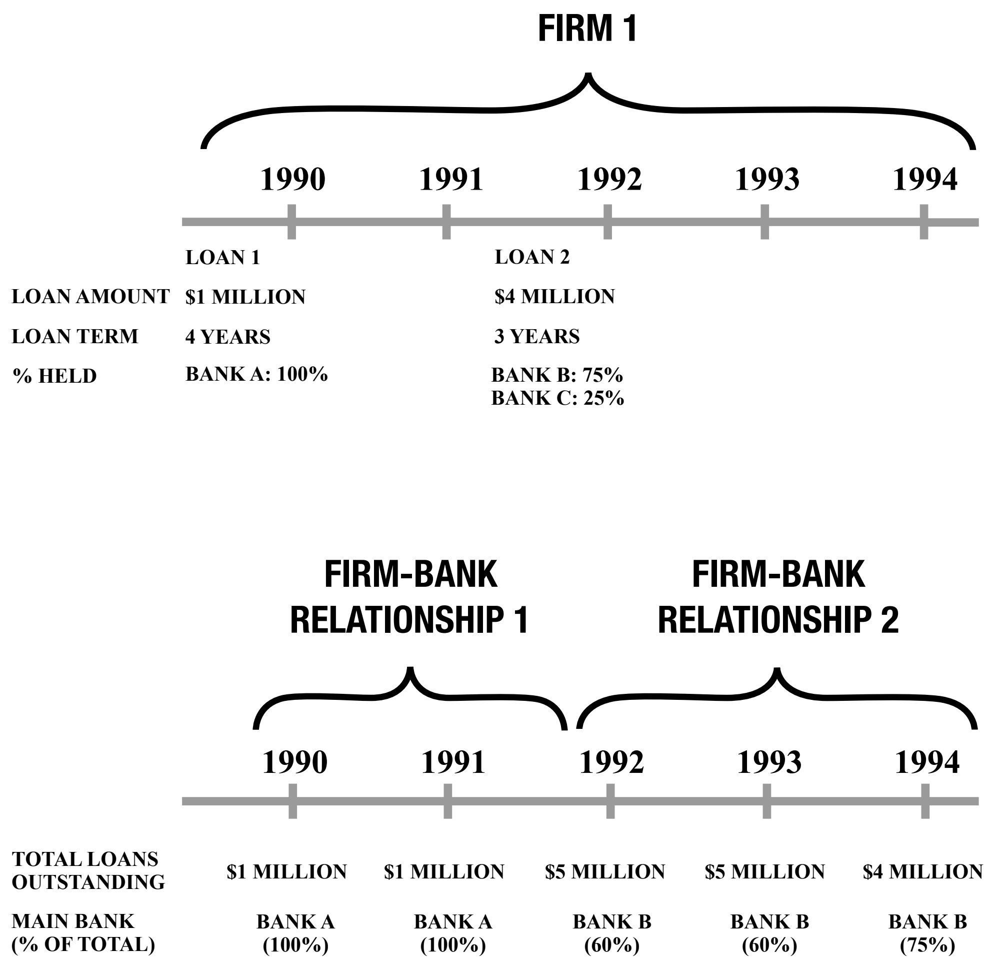
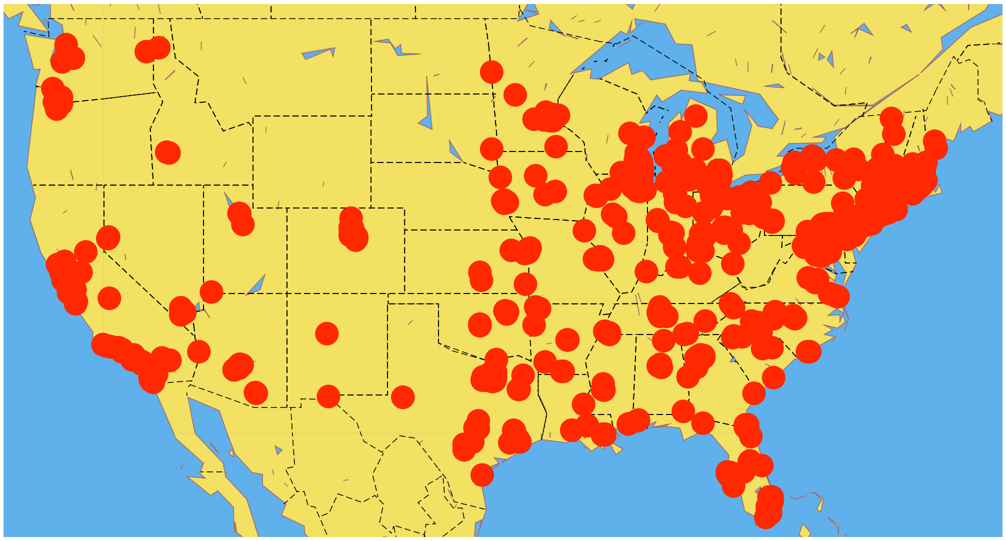
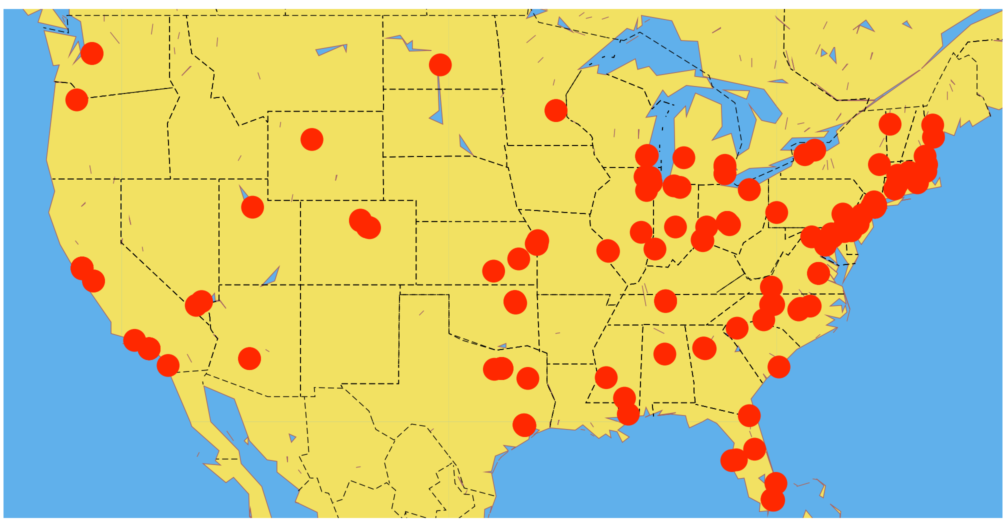


Figure 2: Geographic Location of Firms and Banks

The following two maps show the geographic location of the headquarters of each firm and bank in the sample for which a zip code is available in the continental United States. Each dot represents the location of one or more entities, placed by zip code, as recorded by either the Compustat tapes or Call Report data.



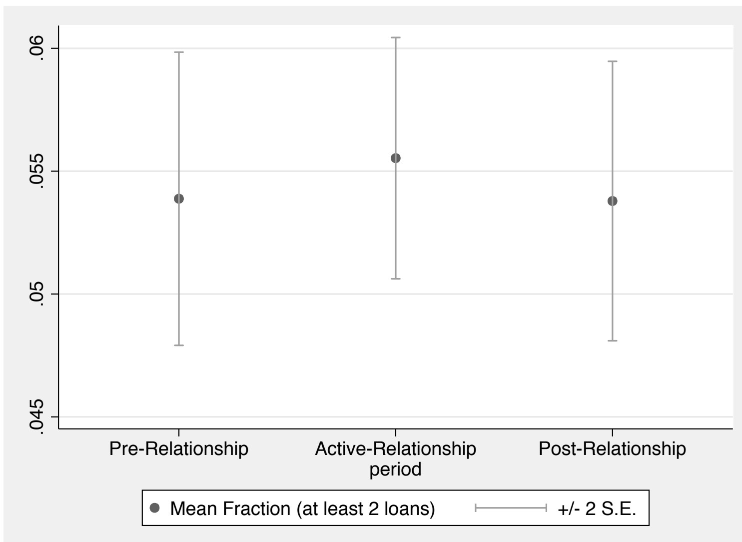
(a) Firm Locations



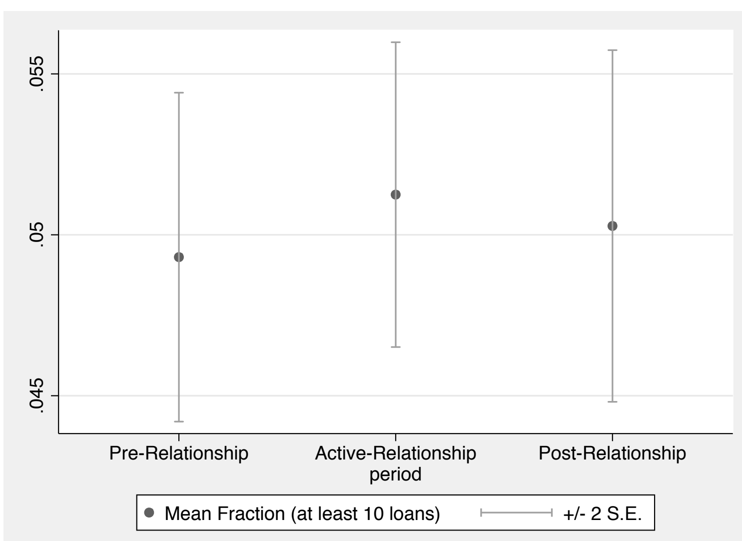
(b) Bank Locations

Figure 3: Industry Exposure Across Periods

The following figures show the average same-industry concentration for each bank-firm pair in each active and inactive lending period. Each period represents the average fraction of total lending volume by the bank to the Fama-French industry of the matched firm as estimated from the observed loans in Dealscan. The mean values are accompanied by ranges representing +/- 2 standard errors of the estimated group mean. These values are estimated requiring at that least 2 loans (or at least 10 loans) be recorded to each industry in the period.



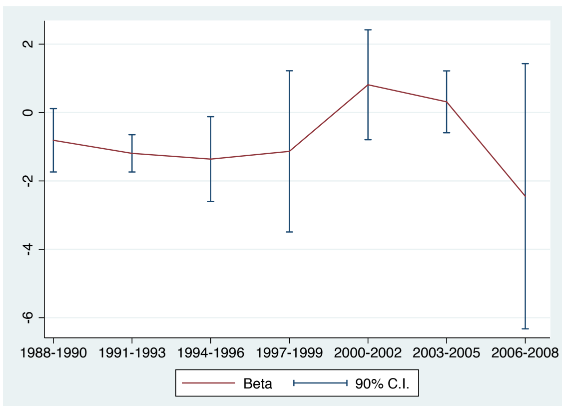
(a) # Loans per industry ≥ 2



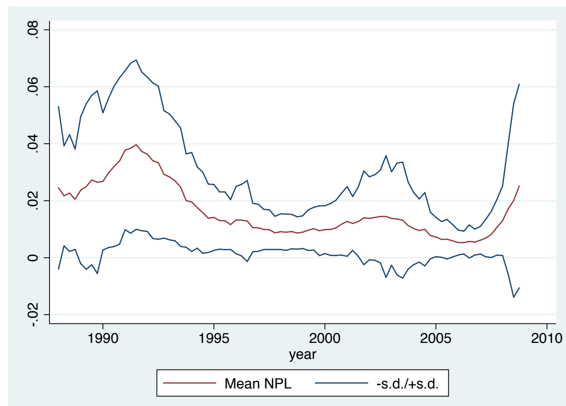
(b) # Loans per industry ≥ 10

Figure 4

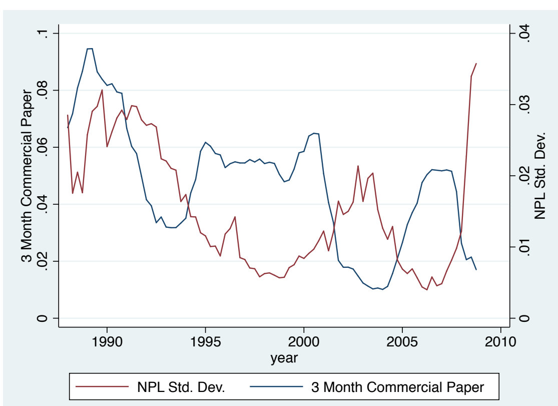
The following graphs show the impact of changes in the nonperforming loan ratio over time and the level of identifying variation during the period. Graph (a) shows the estimated impact of changes in the nonperforming loan ratio over 3 year windows along with the 90% confidence interval for each estimate. Graphs (b) and (d) show the cross-sectional mean and standard deviation of the nonperforming loan ratio and equity capitalization of the sample banks, respectively, over the estimated sample period and sub-periods. Graph (c) shows the 3-month commercial paper rate together with the cross-sectional standard deviation of the nonperforming loan ratio.



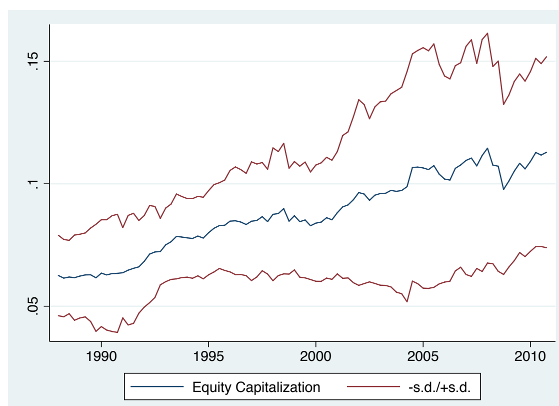
(a)



(b)



(c)



(d)

Table 1: Summary Statistics

This table presents summary statistics for the main sample. The sample contains all firm-year observations, from 1987 to 2008, for which I was able to successfully match the firm's largest or second largest lender to a commercial bank in the Call Reports. The sample was created by aggregating all loans recorded by LPC in the Dealscan database and estimating the largest loan holder for each firm in each year. Where I was unable to match the largest lender, I matched the second. I exclude the year in which the lending relationship is initiated and any observations where the estimated outstanding loan amount exceeds the total debt recorded in Compustat or where the bank's debt share exceeds the capital of the matched bank.

	Mean	SD	10th	25th	Median	75th	90th
Investment	0.223	0.190	0.069	0.113	0.179	0.266	0.408
Nonperforming	0.022	0.022	0.004	0.008	0.014	0.027	0.059
Tobins-Q	1.569	0.857	0.918	1.049	1.312	1.749	2.471
CashFlow	0.319	0.780	-0.017	0.128	0.259	0.457	0.814
Loan/Assets	0.185	0.162	0.038	0.079	0.148	0.249	0.362
Z-Score	1.691	1.215	0.472	1.098	1.725	2.367	3.031
Log(Firm Assets)	7.144	2.112	4.139	5.681	7.328	8.665	9.780
Log(Bank Assets)	11.778	1.711	9.283	10.648	12.035	13.256	13.835
Observations	3535						

Table 2: Bank Health and Commercial Lending

This table presents regression results for commercial lending model. The model is estimated using quarterly Call Report data. The dependent variable is the annual *log* change in Commercial and Industrial loans of the bank. Capitalization is the ratio of total equity to assets. Nonperforming is the ratio of nonperforming loans plus loans 90+ days past due divided by the total outstanding loans. Income is the 1-year net income of the bank divided by the beginning of period assets. Deposits/Assets is the total amount of ordinary deposits for each bank. Each model also contains bank and year fixed effects. Model 1 estimates the results for all matching banks in all available years between 1986 and 2009. Model 2 excludes all periods in which a particular bank underwent a major merger. Model 3 contains only bank-year observations for which I also have a matching borrower relationship and matching firm data in CRSP-Compustat. All estimates are Huber-White corrected for heteroskedasticity and clustered at the bank level.

	(1)	(2)	(3)
$\text{Capitalization}_{t-1}$	1.354	0.0756	-0.754*
	(1.078)	(0.288)	(0.419)
Log(Assets)_{t-1}	-0.130***	-0.00107	-0.0270
	(0.037)	(0.014)	(0.017)
$\text{Nonperforming}_{t-1}$	-2.594***	-3.144***	-1.883***
	(0.581)	(0.335)	(0.508)
Income_{t-1}	0.0363***	0.00803	2.265
	(0.014)	(0.012)	(1.460)
$\text{Deposits/Assets}_{t-1}$	0.0990	-0.0734	-0.128
	(0.268)	(0.096)	(0.101)
N	3482	3241	1057
R^2	0.096	0.132	0.323
Bank Fixed Effects	Y	Y	Y
Year Fixed Effects	Y	Y	Y

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Table 3: Bank Health and Net Borrowing

This table presents the results of the net borrowing regressions for the full panel. Net borrowing is defined either as current period outstanding debt minus the previous year divided by total assets or as the difference in *log* debt over the same period. All regressors, with the exception of Cash Flow, are lagged one year unless otherwise noted. Nonperforming is the ratio of nonperforming loans plus loans 90+ days past due divided by the total outstanding loans. Tangibility is the ratio of net PPE to assets. Cash Flow is defined as earnings before extraordinary items plus depreciation over the same period divided by total assets at the start of the period. All estimates are Huber-White corrected for heteroskedasticity and clustered at the firm-bank level.

	(1)	(2)	(3)	(4)
Nonperforming	-1.149***	-1.244***	-2.622***	-2.513***
	(0.330)	(0.324)	(0.937)	(0.911)
Tangibility		-0.303		-0.370
		(0.250)		(0.234)
Market-to-Book		0.0633***		0.194***
		(0.011)		(0.038)
Cash Flow		0.000141		-0.0284
		(0.018)		(0.035)
Log(Sales)				-0.151***
				(0.046)
N	3717	3635	3717	3626
R^2	0.0303	0.0733	0.0264	0.0809
Firm-Bank Fixed Effects	Y	Y	Y	Y
Year Efects	Y	Y	Y	Y

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Table 4: Bank Health and Investment

This table presents the results of the investment regressions for the full panel. The dependent variable is defined as capital expenditures divided by beginning of period PPE. All regressors, with the exception of Cash Flow, are lagged one year unless otherwise noted. All models include an unreported negative cash flow indicator. Nonperforming is the ratio of nonperforming loans plus loans 90+ days past due divided by the total outstanding loans. Tobins-Q is defined as the market value of assets divided by the book value of assets, where the market value of assets equals the book value of assets plus the market value of common equity less the sum of the book value of common equity and balance sheet deferred taxes. Cash Flow is defined as earnings before extraordinary items plus depreciation, divided by PPE. Rated indicates the presence of a debt rating, while Junk indicates the subset of those ratings which are BB+ or below. Z-Score is the sum of 3.3 times pre-tax income, sales, 1.4 times retained earnings, and 1.2 times net working capital all divided by total assets. All estimates are Huber-White corrected for heteroskedasticity and clustered at the firm-bank level.

	(1)	(2)	(3)	(4)	(5)
Nonperforming$_{t-1}$	-0.948***	-1.008***	-1.325***	-0.813***	-0.617**
	(0.281)	(0.298)	(0.426)	(0.287)	(0.300)
Tobins-Q	0.0558***	0.0561***	0.0530***	0.0393***	0.0392***
	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
CashFlow	0.108***	0.103***	0.101***	0.109***	0.110***
	(0.021)	(0.021)	(0.027)	(0.023)	(0.023)
CashFlow * I$_{CF<0}$	-0.166***	-0.169***		-0.163***	-0.164***
	(0.033)	(0.034)		(0.036)	(0.036)
CF * Nonperforming		0.439	1.944**		
		(0.466)	(0.961)		
Z-Score				0.0585***	0.0585***
				(0.008)	(0.008)
Log(Assets)				-0.0513***	-0.0507***
				(0.017)	(0.017)
Rated				0.0199	0.0190
				(0.017)	(0.017)
Junk				-0.0231*	-0.0229*
				(0.013)	(0.013)
Nonperforming$_t$					-0.511
					(0.378)
N	3535	3535	3152	3280	3280
R^2	0.163	0.164	0.168	0.219	0.220

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Table 5: Bank Health and Investment: Robustness Check

This table presents the results of the investment regressions with additional category-time effects as controls. The dependent variable is defined as capital expenditures divided by beginning of period PPE. Region-Year effects represent a separate dummy variable for every unique combination of the year and the U.S. Census Division in which the firm is headquartered from years 1987 to 2008. Firm headquarters locations are taken from Compustat. Industry-Year effects contain a separate dummy variable for every unique year-industry combination from years 1987 to 2008 and 30 Fama-French industry classifications. Each model also includes an unreported negative cash flow indicator. All estimates are Huber-White corrected for heteroskedasticity and clustered at the firm-bank level.

	(1)	(2)
Nonperforming	-0.897***	-0.847***
	(0.276)	(0.324)
Tobins-Q	0.0576***	0.0475***
	(0.010)	(0.010)
CashFlow	0.108***	0.0913***
	(0.020)	(0.020)
CashFlow * $I_{CF<0}$	-0.165***	-0.152***
	(0.033)	(0.033)
N	3535	3535
R^2	0.238	0.338
FixedEffects	Firm-Bank	Firm-Bank
TimeEffects	Region-Year	Industry-Year

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Table 6: Bank Health and Investment: Errors-in-Variables Treatment

This table presents the results of a Two-Step GMM, errors-in-variables model using higher order moments. The estimation methodology follows Erickson and Whited (2000, 2002), with Tobin's-Q treated as a mis-measured regressor and all others treated as perfectly measured. The model is estimated as a fixed-effects model, in line with the earlier specifications, by first de-meaning all regressors. Models are identified by the highest order moment used in each specification. Each model also includes an unreported negative cash flow indicator and interaction term as well as year fixed effects.

	(1) GMM3	(2) GMM4	(3) GMM5
Nonperforming$_{t-1}$	-1.123***	-1.122***	-1.131***
	(0.294)	(0.272)	(0.276)
Error Adjusted q	0.215*	0.215***	0.226***
	(0.121)	(0.0275)	(0.0267)
CashFlow	0.0635*	0.0636***	0.0606***
	(0.0343)	(0.0163)	(0.0161)
N	3535	3535	3535

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Table 7: Investment Regressions Across Changes in Banking Relationship Status

This table presents the results of the fixed effects regression of bank health on firm investment, from 1985 to 2008, including periods extending prior to the initiation of the banking relationship and subsequent to the expiration of the relationship. Each firm-bank pair is extended to all time periods during which the firm and bank have recorded information in both data sets. Pre-relationship period represents all firm-bank-year observations prior to the first recorded loan between the firm and bank. Post-relationship period represents all firm-bank-year observations after the bank was supplanted as the firm's main lender. Current represents all observations within the main sample. All estimates are Huber-White corrected for heteroskedasticity and clustered at the firm level.

	(1)	(2)	(3)
Nonperforming	0.235	0.238	
	(0.185)	(0.188)	
Nonperforming * Current		-1.182***	-0.944***
		(0.335)	(0.280)
Nonperforming * Post			-0.0380
			(0.234)
Nonperforming * Pre			0.395
			(0.252)
N	6583	10118	10118

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Table 8: Nonperforming Loans by Type

This table presents the results of the full panel investment regressions. The dependent variable is defined as capital expenditures divided by beginning of period PPE. Nonperforming is the ratio of nonperforming loans plus loans 90+ days past due divided by the total outstanding loans. NPL (C&I) is calculated as the sum of commercial and industrial non-performing loans plus those 90+ days past due scaled by total outstanding loans. NPL (non-C&I) is calculated as the remaining non-performing loan amount scaled by total outstanding loans. All estimates are Huber-White corrected for heteroskedasticity and clustered at the firm level.

	(1)	(2)	(3)
$\text{Nonperforming}_{t-1}$		-1.188**	
		(0.493)	
NPL (non-C&I Loans)	-1.333***		-1.188**
	(0.445)		(0.493)
NPL (C&I Loans)		0.660	-0.528
		(0.877)	(0.535)
Tobins-Q	0.0544***	0.0546***	0.0546***
	(0.011)	(0.011)	(0.011)
CashFlow	0.108***	0.108***	0.108***
	(0.021)	(0.021)	(0.021)
N	3527	3527	3527

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Table 9: Bank Effects and Geographic Distance

This table presents the investment regressions split across three distance measures. For each firm-bank combination, I estimate whether the matched bank and firm headquarters are located in the same state or region or are located in differing states or regions. The distance is calculated as number of miles between the zip code of the firm, as recorded by Compustat, and the zip code of the bank as recorded by Call Report item rssd9220. The p(difference) statistic reports the Chi-Sqared significance level of the difference between the Nonperforming coefficients in each model under the Wald test. All estimates are Huber-White corrected for heteroskedasticity and clustered at the firm-bank level.

	State		Region	
	Same	Different	Same	Different
Nonperforming	-0.650	-1.337***	-0.674	-1.576***
	(0.558)	(0.412)	(0.452)	(0.501)
Tobins-Q	0.0465**	0.0629***	0.0502***	0.0655***
	(0.020)	(0.014)	(0.016)	(0.016)
CashFlow	0.167***	0.0980***	0.106***	0.105***
	(0.051)	(0.022)	(0.035)	(0.026)
p(difference)		0.323		0.178
N	798	2587	1226	2125

	Distance	
	Less than 500 Miles	More than 500 Miles
Nonperforming	-0.656*	-1.975***
	(0.371)	(0.574)
Tobins-Q	0.0528***	0.0623***
	(0.016)	(0.017)
CashFlow	0.114***	0.102***
	(0.032)	(0.027)
p(difference)		0.0517
N	1577	1694

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Table 10: Determinants of Investment Sensitivity

This table presents the investment regressions split across ex-ante proxies for bank dependence. Each proxy is calculated at the initiation of the lending relationship, such that all yearly observations in a given firm-bank pair are assigned to only one model. Firms which had a bond rating of BBB- or greater are classified as Investment grade firms. Firms with a bond rating of BB+ or less are classified as Junk grade. Firms which had no bond rating as of the initiation of the relationship are classified as Unrated. Firm age, Asset Tangibility, and Bank Debt/PPE are divided into the top and bottom third of each category. Firm age is the number of years that the firm has appeared in Compustat. Asset Tangibility is the ratio of net property, plant, and equipment to total assets. Bank Debt/ PPE is the ratio of estimated outstanding bank debt divided by net PPE. The p(difference) statistic reports the Chi-Sqared significance level of the difference between the Nonperforming coefficients in each model under the Wald test. For the Debt Rating groups, p(difference) tests the difference of the coefficient in each model from the coefficient in the Investment model. All estimates are Huber-White corrected for heteroskedasticity and clustered at the firm-bank level.

	Debt Rating			Firm Age	
	Investment	Junk	Unrated	Young	Old
Nonperforming	-0.321	-2.695***	-0.714**	-1.137**	-0.870**
	(0.491)	(0.910)	(0.355)	(0.514)	(0.380)
Tobins-Q	0.0292***	0.0644**	0.0645***	0.0817***	0.0357***
	(0.010)	(0.025)	(0.018)	(0.018)	(0.011)
CashFlow	0.101***	0.124*	0.108***	0.138***	0.0732***
	(0.030)	(0.067)	(0.030)	(0.043)	(0.012)
p(difference)		0.0188	0.510		0.673
N	1403	537	1595	1084	1532

	Asset Tangibility		Bank Debt/PPE	
	Low	High	Low	High
Nonperforming	-1.491**	-0.471	-0.205	-1.521**
	(0.576)	(0.451)	(0.416)	(0.622)
Tobins-Q	0.0621***	0.0442***	0.0392**	0.0694***
	(0.019)	(0.015)	(0.016)	(0.022)
CashFlow	0.0921***	0.268***	0.272***	0.0826***
	(0.019)	(0.057)	(0.050)	(0.018)
p(difference)		0.0747		0.159
N	1110	1163	1160	1123

Table 11: Elasticity of Investment to Credit

This table presents the first and second stage results from a 2SLS-IV regression. For each subsample, the first stage estimates the net borrowing of each firm in each period with $Nonperforming$ as the excluded instrument. Net borrowing is defined as the change in long and short-term debt scaled by beginning of period PPE. The second stage estimates firm investment as a function of the predicted value of net borrowing. The estimated elasticity of investment to $\Delta Debt$ is calculated for each subsample and reported below the coefficient estimates. The point elasticities are first calculated at the mean of each independent variable within each subsample. The point elasticities are also re-estimated at the mean of the independent variables from the entire sample and reported below the first estimates. All models are estimated using year and firm-bank fixed effects and the standard errors are Huber-White corrected for heteroskedasticity and clustered at the firm-bank level.

First Stage: Dependent Variable = $\Delta Debt/PPE$

		Distance		Debt Rating		
	Main	<500m	>500m	Investment	Junk	Unrated
Nonperforming	-5.938***	-5.166***	-6.978**	-0.598	-15.98**	-4.720**
	(1.593)	(1.951)	(3.133)	(1.364)	(7.214)	(1.870)
Tobins-Q	0.141***	0.0837**	0.218***	0.144***	0.305	0.119**
	(0.036)	(0.037)	(0.066)	(0.049)	(0.197)	(0.048)
CashFlow	0.380**	0.648***	0.262	0.193	0.272	0.416**
	(0.151)	(0.214)	(0.185)	(0.276)	(0.334)	(0.207)
Constant	-0.127	-0.200	-0.196	0.0262	-0.168	-0.490
	(0.192)	(0.182)	(0.279)	(0.163)	(0.575)	(0.397)
N	3535	1577	1694	1403	537	1595

Second Stage: Dependent Variable = $Investment/PPE$

	All	<500m	>500m	Investment	Junk	Unrated
ΔDebt/PPE	0.159***	0.120	0.260**	0.317	0.168**	0.163*
	(0.058)	(0.084)	(0.123)	(1.286)	(0.077)	(0.093)
Tobins-Q	0.0241*	0.0338*	-0.00645	-0.00881	0.000945	0.0326
	(0.013)	(0.018)	(0.034)	(0.148)	(0.050)	(0.020)
CashFlow	0.0687*	0.0701	0.0399	0.0112	0.0998	0.0608
	(0.037)	(0.070)	(0.056)	(0.441)	(0.088)	(0.053)
Elasticity						
at $\bar{X}_{Subsample}$	0.131	0.109	0.201	0.167	0.149	0.160
at $\bar{X}_{All}$		0.101	0.207	0.265	0.138	0.126

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Table 12: Bank Effects Over Time

This table presents the investment regressions, fitted with a time trend. The time trend t, takes a value of 1 in 1987, the first year of the sample, up to 22 in 2008, the last year of the sample. Model (1) presents the full sample, while Model (2) includes only firms for which the absolute largest lender could be successfully matched. Model (3) estimates a separate effect for each of 7 subsample periods. Coefficients on Tobin's-Q and Cash Flow are estimated but not unreported. All estimates are Huber-White corrected for heteroskedasticity and clustered at the firm-bank level.

	(1)	(2)	(3)
Nonperforming	-1.673***	-2.307***	
	(0.526)	(0.746)	
t (t=1, 1987)	-0.00952***	-0.00953**	
	(0.004)	(0.004)	
t * Nonperforming	0.102*	0.203**	
	(0.054)	(0.090)	
$\text{Nonperforming}_{1988-1990}$			-0.812
			(0.563)
$\text{Nonperforming}_{1991-1993}$			-1.194***
			(0.331)
$\text{Nonperforming}_{1994-1996}$			-1.362*
			(0.754)
$\text{Nonperforming}_{1997-1999}$			-1.136
			(1.433)
$\text{Nonperforming}_{2000-2002}$			0.812
			(0.977)
$\text{Nonperforming}_{2003-2005}$			0.313
			(0.550)
$\text{Nonperforming}_{2006-2008}$			-2.448
			(2.357)
N	3535	2728	3535

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$