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Tracing the Impact of Bank Liquidity Shocks: Evidence from an Emerging Market

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We examine the impact of liquidity shocks by exploiting cross-bank liquidity variation induced by unanticipated nuclear tests in Pakistan. We show that for the same *firm borrowing from two different banks, its loan from the bank experiencing a 1 percent larger decline in liquidity drops by an additional 0.6 percent. While banks pass their liquidity shocks on to firms, large firms—particularly those with strong business or political ties—completely compensate this loss by additional borrowing through the credit market. Small firms are unable to do so and face large drops in overall borrowing and increased financial distress.* (*JEL* E44, G21, G32, L25)

Banks around the world, particularly in emerging markets, often face large shocks to their supply of liquidity due to regime shifts, speculative bank runs, "hot money" flows, or exchange rate volatility.[1] Many argue that banks pass these fluctuations on to borrowing firms even when there is no change in the firms' overall credit worthiness. This can lead to large real effects if firms are unable to withstand liquidity shortages from their banks.

Thus, a complete understanding of how bank liquidity shocks affect the economy requires estimating two separate channels simultaneously: the *bank lending channel*, i.e., the inability of banks to cushion borrowing firms against bank-specific liquidity shocks; and the *firm borrowing channel,* i.e., the inability of firms to smooth out bank lending channel effects by borrowing from alternative sources of financing.

Existing work provides increasing evidence on the bank lending channel (Anil K. Kashyap, Jeremy Stein, and David Wilcox 1993; Joe Peek and Eric S. Rosengren, 1997; Kashyap and Stein 2000; Daniel Paravisini, forthcoming). While some argue that this also leads to economic recessions (Ben Bernanke 1983; Peek and Rosengren 2000), others find the economic impact to be insignificant (Adam Ashcraft 2006) or varying by firm type (Mark Gertler and Simon Gilchrist

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[1] The average standard deviation of the real cost of deposits is 1.6 percent in G7 countries but 12.9 percent in 25 major emerging markets, and the standard deviation of real demand deposit growth is 14 percent and 24 percent, respectively (IMF International Financial Statistics (IFS), 1980–2005).

1994; Kashyap, Owen Lamont, and Stein 1994). This suggests that the firm borrowing channel may be the critical factor in determining whether and how the bank lending channel gets transmitted to the economy. However, investigating this has proven difficult.

The difficulty in simultaneously estimating the bank lending and firm borrowing channels stems from a lack of data that links banks to individual firms over time, as well as identification concerns. For example, events that trigger changes in liquidity supply are often accompanied by changes in investment returns and therefore credit demand. In this paper, we propose a new empirical methodology for identifying the bank lending channel, and exploit a natural experiment using loan-level data to estimate the bank lending and firm borrowing channels simultaneously.

Our methodology for estimating the bank lending channel focuses on firms' borrowing from multiple banks, where the banks differ in their exposure to liquidity shocks. Using firm fixed effects (FEs), in first-differenced data, we compare how the *same* firm's loan growth from one bank changes *relative* to another more affected bank. To the extent this *within firm* comparison fully absorbs firm-specific changes in credit demand, the estimated difference in loan growth can be plausibly attributed to differences in bank liquidity shocks.

We implement our methodology using variation in bank liquidity shocks induced by the unexpected nuclear tests of Pakistan in 1998, and use quarterly loan-level panel data (1996–2000) representing the universe of corporate lending in Pakistan (18,000 firms). The nuclear tests immediately led the government (in anticipation of balance of payment problems) to restrict withdrawals of dollar-denominated deposit accounts to local currency only, and at an unfavorable exchange rate. This sudden collapse of the dollar deposit market disproportionately affected banks that relied more on dollar deposit as their source of liquidity.

Our within firm comparison reveals that a percentage point decline in bank liquidity supply leads to 0.6 percent reduction in the amount lent by the bank. There is also a large lending channel effect on the extensive margin: a 1 percent fall in bank liquidity reduces the probability of lending to new clients by 12 basis points and the probability of continuing lending to existing clients by 21 basis points. The bank lending channel works primarily through its impact on quantity, as we find no evidence of a change in loan price due to bank liquidity shocks.

While the firm FEs approach restricts analysis to firms with multiple banking relationships, comparing the fixed effect and OLS estimates shows that the latter is an underestimate, i.e., nuclear tests induced a *negative* correlation between the credit supply and demand shocks. This is plausible because, as we show later, banks that received larger shocks to their liquidity supply (those with more dollar deposits) were better banks lending to better firms. If these firms were more able to cope with the changing macro environment, then their credit demand shocks would be less adverse *relative* to firms at other (unaffected) banks. This allows us not only to show that the bank lending channel is present for all firms, but also to present OLS estimates for the firm borrowing channel, since they are likely to be conservative for the same reason.

Estimating the firm borrowing channel shows that while the bank lending channel was large for all firm types, large firms (top 30 percent by size), especially political firms and firms belonging to large conglomerates, undo the entire bank lending channel effect. They do so by borrowing more from existing and more liquid banking relationships, and by forming new banking relationships. Smaller unconnected firms, on the other hand, are unable to hedge the bank lending channel and consequently face large overall drops in borrowing.

The inability of small firms to undo the lending channel shocks affects their financial outcomes as well. A small firm that borrows from a bank with a 1 percent larger decline in liquidity is 2 percent more likely to enter into financial distress a year after the nuclear tests. On the other hand, there is no such effect for large firms, consistent with the finding that large firms hedge lending channel shocks.

While theoretical work, such as that by Bernanke and Alan S. Blinder (1988), Bernanke and Gertler (1989), Bengt Holmström and Jean Tirole (1997), and Stein (1998), emphasizes that the transmission of financial shocks to the economy requires credit market imperfections at both the bank and firm level, the empirical literature has mostly focused on the banking side. Our paper differs in that it simultaneously tests for market frictions at both the bank and firm level.

This empirical literature initially utilized time-series correlations between changes in liquidity and changes in loans (or output) to argue that liquidity shocks have real consequences (Bernanke and Blinder 1992; Bernanke 1983; Bernanke and Harold James 1991). Concerns that such correlations are confounded with economy-wide productivity shocks has led to work such as Gertler and Gilchrist (1994), Kashyap, Lamont, and Stein (1994), Kashyap and Stein (2000), and Ashcraft (2006) which uses *cross-sectional* variation in liquidity supply across banks or firms to purge economy-wide shocks. Others use instrumental variables (Paravisini, forthcoming) or look for natural experiments (Peek and Rosengren 1997, 2000; Ashcraft 2005) that generate exogenous (to demand) liquidity supply shocks. In contrast, our methodology need only assume that a firm's credit demand shock is the same across its lenders.

Our results highlight the importance of simultaneously estimating the bank lending and firm borrowing channels. While the bank lending channel is large and present for all firms, a subset is able to undo its financial impact. In contrast, the inability of small and unconnected firms to cancel out the bank lending channel shocks means that such shocks can have significant *distributional* consequences.

In what follows, Section I describes the data and the institutional background. Section II presents our empirical methodology. Sections III and IV provide results on the bank lending and firm borrowing channels, and their impact on firm financial distress. Section V concludes.

I. Institutional Background and Data

A. *The 1998 Liquidity Crunch*

Unanticipated nuclear tests by India on May 11, 1998, led to retaliatory nuclear tests by Pakistan on May 28. These events resulted in a large and sudden liquidity shock for banks in Pakistan. The extent of this shock varied across banks based on their exposure to dollar denominated deposit accounts. We outline the sequence of events that led to these changes.

Dollar Deposit Accounts.—By the early 1990s Pakistan had a relatively liberalized banking sector with significant private and foreign bank participation. Banking reforms during this period included the introduction of foreign currency (mostly dollar) deposit accounts. The scheme was aimed at stopping the flight of dollars overseas by allowing citizens to hold foreign currency within Pakistan.

An important feature of the dollar accounts was that local banks accepting dollar deposits could not retain dollars. Banks had to surrender dollars to the central bank, the State Bank of Pakistan (SBP), in return for rupees at the prevalent exchange rate. When a depositor demanded his dollars back (with interest), the bank obtained dollars from the central bank in exchange for rupees at the *initial* (time of deposit) exchange rate. Therefore, all exchange rate risk between the time of deposit and withdrawal was borne by the central bank (see SBP notification #54, June 7, 1992). The SBP charged banks a 3 percent annual fee for providing this insurance. Given currency devaluation trends, these dollar deposit accounts were widely popular and by May 1998, in a span of six years, dollar deposits had grown to 43.5 percent of total deposits.

However, the exposure to dollar deposits was not uniform across banks. As of December 1997, the percentage of a bank's deposits denominated in dollars varied from 0 percent to 98 percent, with a standard deviation of 27 percent. This cross-bank variation was clearly not exogenous and depended on a host of factors such as the customer base of a bank, its marketing strategy, and its perceived outlook. In particular, as we show in Section II, better and more profitable banks held a higher percentage of dollar deposits.

"Freeze" on Dollar Deposit Accounts.—When India and Pakistan tested nuclear devices in May 1998, the international community swiftly imposed sanctions on both countries. The sanctions were limited to military sales and financial assistance and did not involve any major trade sanctions. However, suspension of exchange rate liquidity support from the International Monetary Fund (IMF) led to balance of payment problems for Pakistan.

Anticipating these problems, the prime minister of Pakistan declared that all foreign currency accounts would be "partially frozen." This meant that dollar deposit holders could withdraw money in rupees only at the current and disadvantaged exchange rate. The freeze thus amounted to a partial default on dollar deposits by the government, with depositors losing 10 to 15 percent of their deposit value.

The loss of confidence as a result of this partial default turned out to have a serious impact on the banking sector. Dollar deposit holders withdrew their money from banks despite being able to do so only at disadvantaged exchange rates. Figure 1 traces the aggregate dollar deposits over time and shows the sudden and precipitous withdrawal from dollar accounts after the nuclear tests, with these deposits falling by a half within a year of the freeze.

Part of this liquidity exited the Pakistani banking system, as it was reconverted to dollars through the black market and invested abroad. Since the deposit run was experienced by banks with larger dollar deposit accounts, the liquidity shock varied substantially across banks, with several rupee deposit reliant banks continuing to enjoy pre–nuclear test deposit growth.

The nuclear tests thus led to sharp cross-sectional variation in deposit-led liquidity shocks experienced by banks. Figure 2 illustrates this variation for all 42 commercial banks that issued demandable deposits in local and foreign currency. It plots the average annual change in liquidity for these banks from December 1997 to December 1999 against their pre–nuclear test reliance on dollar deposits. Each observation is plotted proportional to bank size in December 1997. The graph shows a strong negative relationship between dollar deposit exposure and bank liquidity changes,[2] which we exploit in our estimation strategy in Section II.

While the nuclear induced liquidity shocks are somewhat unique in their origin and lack of anticipation, the magnitude of these shocks is fairly representative of liquidity shocks experienced by Pakistan and other emerging markets. While the 1998 events reduced deposit growth from 17 percent to 5 percent, the Pakistani economy experienced such low deposit growth on at least four separate occasions in the prior two decades. The high volatility of the banking sector in Pakistan is also common to other emerging markets. The standard deviation of *real* annual growth rates of demand deposits was 15.8 percent (1.65 times the mean growth rate) in Pakistan during the 1980–2005 period, compared to 24 percent (2.1 times the mean growth rate) for 26 major emerging markets.[3] Such variability is also not uncommon in developed economies, with G7 countries seeing a standard deviation of demand deposit growth of 13.7 percent during the same period.

The banking sector in Pakistan is also liberal and representative of emerging markets. Private, foreign, and government banks constitute roughly equal shares of domestic lending. Financial reforms in the early 1990s brought uniform prudential regulations in line with international

[2] Changes in deposits refer to book values and hence are not influenced by current price fluctuations.

[3] The numbers are based on IFS data and 26 major emerging markets included in the Morgan Stanley global equity index (MSCI).

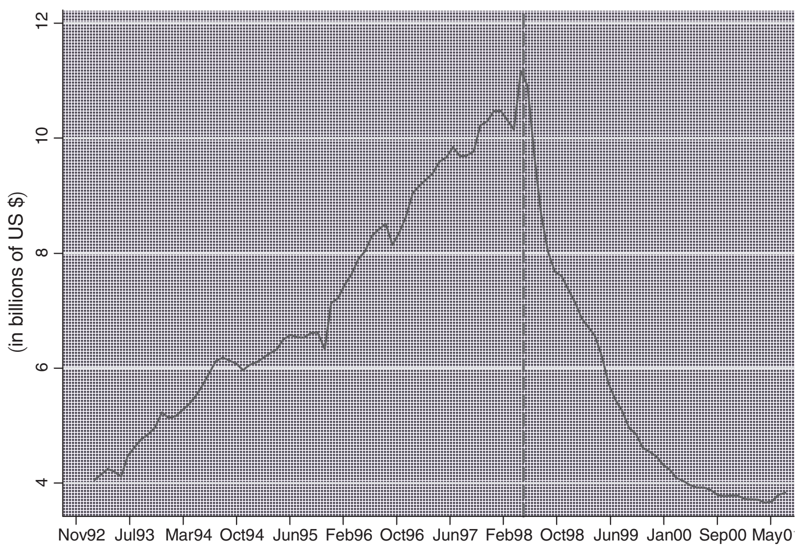

Figure 1. Total Dollar Deposits

Notes: Figure 1 examines the prevalence of foreign currency deposit accounts in Pakistan. These accounts (introduced in the early 1990s) grew steadily until March 1998, the date of the nuclear shock (indicated by the dashed line), and then fell rapidly after that.

banking practices (Basel Accord) and autonomy was granted to the SBP for regulation. While political efforts have been made in the past to bring banking in accordance with Islamic *shariah* laws, it has not had any significant functional impact on banking. For all practical purposes, banking follows global norms with deposit and lending rates determined by the market. As our results will show, however, interbank markets may not be as efficient and are typically used for short-term liquidity management rather than dealing with larger and more persistent liquidity shocks, as those induced by the nuclear tests.

Finally, we should note that while the tests were not accompanied by significant trade sanctions, it is likely that the economy faced credit demand shocks, with the large currency depreciation hurting importers and domestic producers. Therefore, it will be important that we remain cognizant of, and purge, such demand effects when identifying the impact of the liquidity supply shock to banks.

B. *Data*

Our primary data come from the credit information bureau (CIB) of the SBP. The central bank maintains these data to monitor and regulate the lending activities of banks. It has quarterly loan-level information on the universe of corporate loans outstanding in Pakistan between July 1996 and March 2000. The data include the history of each loan with information on the amount and type of loan outstanding, default amounts, and loan type. It also has information on the name, location, and board of directors of the borrowing firm and its bank. We combine these data with annual balance sheet information on banks.

In terms of data quality, our personal examination of the collection and compilation procedures, as well as consistency checks on the data, suggest that it is of high quality. CIB was part of a large effort by the central bank to set up a reliable information sharing resource that all banks

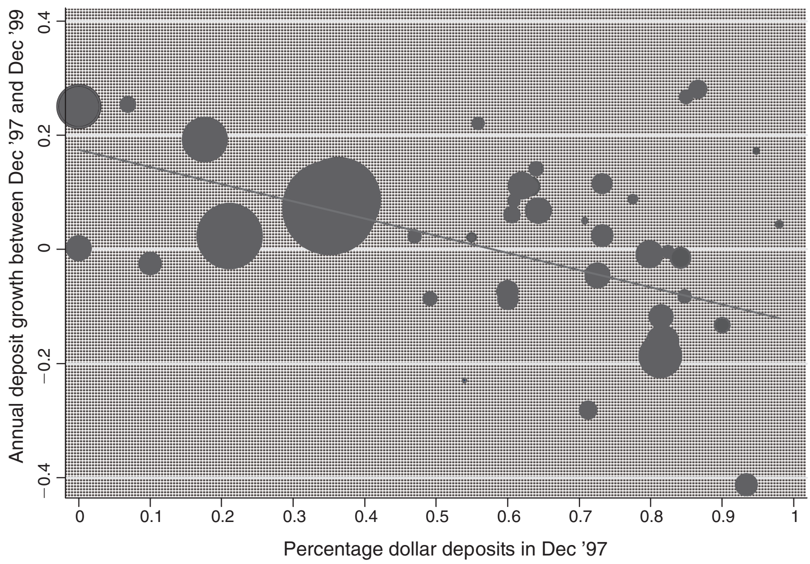

Figure 2. Annual Deposit Growth in Deposits against Initial Dollar Deposit Exposure (weighted)

Notes: Figure 2 illustrates the relationship between the change in liquidity/deposit base after the nuclear shock and the percentage of a bank's deposits held in foreign currency accounts. Each observation is one of the 42 commercial banks in Pakistan that issued demandable deposits in both local and foreign currency. The *y*-axis is the annual change in liquidity for these banks from December 1997 to December 1999 and the *x*-axis is their pre–nuclear test reliance on dollar deposits. Each observation is plotted proportional to its bank size in December 1997. The graph shows a strong negative relationship between dollar deposit exposure and changes in bank liquidity.

could access. Perhaps the most credible signal of data quality is that all local and foreign banks refer to information in CIB on a daily basis to verify the credit history of prospective borrowers. We checked with one of the private banks in Pakistan and found that they use CIB information about prospective borrowers explicitly in their internal credit scoring models. We also ran several internal consistency tests on the data, such as aggregation checks, and found the data to be of excellent quality. As a random check, we also confirmed the authenticity of the data from a bank branch by comparing it to the portfolio of that branch's loan officer.

Although the original data include 145 financial intermediaries, for most of our analysis we restrict our sample to the 42 commercial banks that were allowed to open demandable deposits (including dollar deposits). The remaining financial intermediaries had private or institutional sources of funding and are excluded because we do not have information on their changes in liquidity. The sample restriction should not be a big concern, however, for two reasons. First, the excluded financial intermediaries make up only 22 percent of overall lending at the time of the nuclear tests. Second, since the excluded institutions were not taking dollar or rupee deposits, they were unlikely to have been significantly affected by the nuclear tests, and therefore including them in our sample makes no qualitative difference to the results of this paper. We do, however, include lending by all these financial intermediaries when we examine aggregate firm outcomes such as overall firm borrowing and default rates, since these intermediaries could play an important role in hedging firms against the bank lending channel shocks.

We use the data above to analyze the impact of the liquidity crunch resulting from the nuclear tests of May 1998. Our starting point is the set of all performing private business loans given out by the 42 commercial banks at the time of nuclear tests. This gives us a sample of 22,176 loans to 18,647 firms. A "loan" in our paper is defined as a bank-firm pair. There are more loans than firms, since a single firm may borrow from multiple banks. Although we have quarterly data on the 22,176 loans from July 1996 to March 2000, for most of the analysis we collapse our quarterly time dimension into equal-duration, single "pre" and "post" nuclear test periods by taking time-series averages of loans.[4] This time collapsing of data has the advantage that our standard errors are robust to concerns of auto-correlation (see Marianne Bertrand, Esther Duflo, and Sendhil Mullainathan 2004).

Table 1 presents summary statistics for the loan-, firm-, and bank-level variables in our primary dataset. Since our data cover the universe of all business loans, there is large variation in loan sizes. For example, the average loan size is about 16 million rupees, median is 2.5 million rupees, and the ninety-ninth percentile loan is 230 million rupees. Given the large size variation, we checked both size-weighted and unweighted results to ensure that our conclusions are not entirely driven by the large number of very small loans, or a small number of very large loans. The table also presents loan distribution across firms by different firm attributes, such as size, political connections, membership in business conglomerates, and others. A firm is considered politically connected if one of its directors is a politician. It is considered to be a "conglomerate" firm if it is a member of a large network of firms that are linked through common directors, i.e., interlocked boards.

In some of the empirical specifications run, we expand the sample in Table 1 to include new firms financed by commercial banks after nuclear tests (18,299 loans), as well as loans given out by the 103 noncommercial banks.

II. Empirical Methodology

This section outlines a simple econometric model that highlights the traditional identification problem in the lending channel literature, and how our firm fixed effects approach addresses it. We then describe how we use our approach to go beyond the bank lending channel and estimate the extent to which firms are able to compensate their lending channel shocks.

A. *Estimating the Lending Channel: The Traditional Identification Problem*

Consider a two-period model with bank i providing financing to firm j each period. For simplicity, assume that a bank can lend to only one firm, while firms can borrow from multiple banks.[5] In the first period t, a bank and firm negotiate a loan of size L_{ij}^t. The bank finances this loan by issuing demandable deposits D_i^t, and seeking alternative financing B_i^t (such as equity, bonds, etc.). Since L_{ij}^t is the only bank asset, the following accounting identity must hold:

$$D_i^t + B_i^t \equiv L_{ij}^t. \tag{1}$$

[4] The time-series averages are taken after converting all values to real 1995 rupees. Moreover, we exclude the quarter of the nuclear tests from these calculations. The pre-shock period covers July 1996 through March 1998, while the post-shock period covers July 1998 through March 2000.

[5] We should note that our purpose is not to build a fully specified model of bank intermediation. We shall deliberately focus only on those features that highlight the fundamental econometric issues.

TABLE 1—SUMMARY STATISTICS

Panel A: Loan-level variables (22,176 loans)				
Variable	Mean	S.D.		
Pre–nuclear test total lending (000)	16,479	60,768		
Change in log lending	−0.003	1.23		
Post–nuclear test default rate	6.8%	26.1%		
Pre–nuclear test interest rate (%)	15.9%	2.7%		
Loan type	*Fixed*	*Working capital*	*Letter of credit*	*Other*
Percent of total lending	32.5%	56.1%	4.2%	7.2%
Panel B: Borrower/firm attributes (18,647 firms)				
Politically connected	*No*	*Yes*		
Percent of total lending (of total firms)	54% (76%)	46% (24%)		
Size	*Small*	*Large*		
Percent of total lending (of total firms)	6.4% (70%)	94% (30%)		
Location (city size)	*Small*	*Medium*	*Large*	*Unclassified*
Percent of total lending (of total firms)	6% (14%)	13% (18%)	80% (62%)	6% (2%)
Multiple relationship	*Yes*	*No*		
Percent of total lending (of total firms)	66% (10%)	34% (90%)		
Business network size	*Non-conglomerate*	*Conglomerate*		
Percent of total lending (of total firms)	36% (85%)	64% (15%)		
Panel C: Bank-level variables (42 banks)				
Variable	*Mean*	*S.D.*		
Bank assets Dec ’97	33886.3	63884.7		
Average ROA (’96 & ’97)	0.013	0.027		
Capitalization rate (’96 & ’97)	0.082	0.054		
Percentage of dollar deposits (Dec ’97)	0.60	0.27		
Average default rate (’96 & ’97)	0.086	0.13		
Growth in deposits (Dec ’97 to Dec ’99)	0.046	0.30		
Bank type	*Private*	*Foreign*	*Government*	
Percent of total lending	33.8%	36.8%	29.4%	

Notes: A “loan” is defined as a bank-firm pair, i.e., multiple loans of a firm from the same bank are aggregated up. The loan-level data comprise all performing loans given out by the 42 commercial banks at the time of nuclear testing that continued to be serviced. The pre and post data are averaged over June 1996 to March 1998, and June 1998 to March 2000, respectively. Note that since we include only performing pre-nuclear loans, the default rate just prior to nuclear tests is zero by construction. Loan interest rate in panel A is available for 39 banks only.

Politically connected = dummy for whether one of the firm directors ran in a national or provincial election in the 1993 or 1997 elections; Size = the total borrowing by a firm from all the banks; Small = bottom 70 percent; Location = type of city/town borrower belongs to: Big (> 2 million), Medium (0.5–2 million), and Small (< 0.5 million). In regressions, however, each city/town is included as a separate dummy variable. Multiple relationship = indicates whether firm borrows from multiple banks at time of shock; Business network size classifies firms into networks based on interlocked board membership (see Khwaja and Mian 2005); Congolomerate firms are those that belong to a large network (more than 100 firms).

Models of the lending channel such as Stein (1998) are based on costly external financing. We incorporate this feature by assuming that banks can raise deposits costlessly, but only up to $\overline{D}_i^t$. Beyond this limit, it is costly to raise additional financing (B_i^t) with the marginal cost given by $(\alpha_B B_i^t)$ where $\alpha_B > 0$. The overall bank credit supply function $(D_i^t + B_i^t)$ is thus linear in the cost of funds.

On the credit demand side, we assume that the marginal return on a loan of amount L_{ij}^t is decreasing in size and given by $(\overline{r}_j - \alpha_L L_{ij}^t)$. The equilibrium amounts of B_i^t and L_{ij}^t are thus determined by the intersection of linear supply and demand curves in each period.

At the end of first period t, the economy (i.e., banks and firms) receives two types of shocks. The first, a “credit supply” shock, determines the level of deposits available to each bank in period $t + 1$. In particular, the supply of deposits for bank i in $t + 1$ is given by $\overline{D}_i^{t+1} = \overline{D}_i^t + \overline{\delta} + \delta_i$, where $\overline{\delta}$ and δ_i are economy-wide and bank-specific shocks, respectively. The second shock is a "credit demand" shock that firm j experiences in the form of a shock to its productivity. In particular, the marginal return on its loan L_{ij}^{t+1} next period is now given by: $\overline{r}_j - \alpha_L L_{ij}^{t+1} + \overline{\eta} + \eta_j$. The productivity shock $(\overline{\eta} + \eta_j)$ reflects an economy-wide and a firm-specific component, respectively.

Given the linear setup of our model, equilibrium is determined by jointly solving the first order conditions (FOCs)[6] and accounting identity (1) for L_{ij} and B_i. Solutions for the two periods (ignoring corner solutions) can be combined into a single first-differenced equation:

$$\Delta L_{ij} = \frac{\alpha_B}{(\alpha_L + \alpha_B)}(\overline{\delta} + \delta_i) + \frac{1}{(\alpha_L + \alpha_B)}(\overline{\eta} + \eta_j). \tag{2}$$

Equation (2), although derived from an admittedly simple model, highlights some important issues. First, it shows the importance of costly external financing. Without this assumption (i.e., with $\alpha_B = 0$), banks would be in a Modigliani-Miller (MM) world and shocks to deposits or "liquidity shock" (δ) would have no impact on equilibrium loan amounts. Second, and more important, equation (2) highlights the identification problem in estimating the causal impact of a liquidity shock on loans. This can be seen more easily by rewriting (2) as

$$\Delta L_{ij} = \frac{1}{(\alpha_L + \alpha_B)}(\alpha_B \overline{\delta} + \overline{\eta}) + \frac{\alpha_B}{(\alpha_L + \alpha_B)}\delta_i + \frac{1}{(\alpha_L + \alpha_B)}\eta_j. \tag{3}$$

The first term on the right-hand side (RHS) of (3) is a constant reflecting economy-wide shocks. Thus first-differencing takes out *all* secular time trends in the economy through this constant denoted by $\beta_0\,(= 1/(\alpha_L + \alpha_B)[\alpha_B \overline{\delta} + \overline{\eta}])$. The second term on the RHS contains the main coefficient of interest. Let $\beta_1 = \alpha_B/(\alpha_L+\alpha_B)$. Then β_1 captures the "lending channel" for each incremental unit of deposits lost. The OLS regression typically run to estimate (3) is

$$\Delta L_{ij} = \beta_0 + \beta_1\,\Delta D_i + \eta_j + \varepsilon_{ij}, \tag{4}$$

where $\Delta D_i = \delta_i$ represents the bank-specific change in deposits. However, the estimate $\hat{\beta}_1^{OLS}$ in (4) will be biased if $corr(\Delta D_i, \eta_j) \neq 0$. This isolates the fundamental problem: in general, ΔD_i and η_j are likely to be *positively* correlated. For example, liquidity shocks (ΔD_i) such as bank runs are more likely to occur in banks that receive some bad news (η_j) about the quality or productivity of the firms they lend to.

B. *An Unbiased Estimate of the Lending Channel: Firm Fixed Effects*

A positive correlation between ΔD_i and η_j leads to an overestimate of β_1 if (4) is estimated using OLS, because $\hat{\beta}_1^{OLS} = \beta_1 + [cov(\Delta D_i, \eta_j)/var(\delta_i)]$. We adopt a new method for identifying the lending channel β_1 by introducing firm fixed effects β_j in (4):

$$\Delta L_{ij} = \beta_j + \beta_1\,\Delta D_i + \varepsilon_{ij}. \tag{5}$$

[6] The FOC is $\alpha_B B_i^t = \overline{r} - \alpha_L L_{ij}^t$ in period t, and $\alpha_B B_i^{t+1} = \overline{r} + \overline{\eta} + \eta_j - \alpha_L L_{ij}^{t+1}$ in period $t + 1$.

Since the fixed effects β_j are introduced after first-differencing the data, they absorb all firm-specific credit demand shocks η_j. The FE approach thus tests whether the *same* firm borrowing from two different banks experiences a larger decline in lending from the bank facing a relatively greater fall in its liquidity supply. Since the comparison is across banks for the *same* firm, firm-specific demand shocks are absorbed by the firm fixed effect.[7] However, we can estimate the FE coefficient $\hat{\beta}_1^{FE}$ only in the sample of firms with multiple-banking relationships.

While the fixed effects strategy does not require that the liquidity supply and demand shocks be uncorrelated (since the latter are absorbed by the firm fixed effect), biases could arise if the liquidity supply shock were anticipated. The concern is that if such shocks are anticipated, banks may adjust their lending or firms adjust their borrowing *prior* to the shock. This would lead to either an under- or overestimate of the bank lending channel, depending on the direction of the preshock loan adjustments. Unanticipated shocks, as is the case in the natural experiment examined in this paper, remove such concerns.

Although firm fixed effects address the main identification concerns expressed in the literature, there may be some additional questions. For example, perhaps a firm's loan demand is bank-specific and is correlated with shocks to the bank's liquidity. This can happen if, (a) nuclear shocks disproportionately affect export/import demand, (b) firms get "export/import related" loans from banks that specialize in the tradeable sector, or (c) these export/import intensive banks had more dollar deposits and thus suffered a larger liquidity crunch as well. We shall address this and other related concerns in detail in Section IVD.

C. *Estimating the Impact on Firm-Level Outcomes*

We also utilize the firm fixed effects estimates of the bank lending channel to argue that we can provide conservative estimates of the impact of the liquidity shock on firm-level outcomes such as a firm's total borrowing—the *firm borrowing channel*—and firm financial distress as measured by its default rate on external borrowing. The former examines whether firms can negate the effects of adverse lending channel shocks from existing banks by borrowing from more liquid banks. The latter examines whether firms, if unable to borrow more, can draw on internal/informal resources or instead enter financial distress.

Let Y_j^t be a firm-level attribute of interest in period t (such as a firm's total borrowing from all banks or its average default rate on its loans). The reduced form firm borrowing channel can be determined by estimating the following first-differenced equation:

$$\Delta Y_j = \beta_0^F + \beta_1^F \Delta \bar{D}_j + \eta_j, \tag{6}$$

where $\Delta \bar{D}_j$ is the average liquidity shock faced by firm j's preshock banks. If the firm borrowing channel completely insulates a firm from the bank lending channels, then the liquidity shocks should have no net impact on the firm's aggregate outcomes, i.e., β_1^F should be zero.

Equation (6) has the same identification concerns as equation (4), namely that $\Delta \bar{D}_j$ might be positively correlated with η_j. Unlike before, however, we can no longer put in firm fixed effects since (6) is aggregated to the firm level. We therefore adopt a different strategy, based on the nature of nuclear test–induced liquidity shocks to estimate β_1^F. Suppose we could prove that

[7] This argument is slightly more subtle. Once we recognize a bank lends to multiple firms, equation (3) has to be modified to include idiosyncratic demand shocks experienced by these other "$-j$" firms (i.e., firms borrowing from the bank other than firm j). The firm fixed effect will absorb only firm j's demand shock and the other "$-j$" firms' demand shocks that co-move with j's demand shock. However, since these remaining components are, by construction, orthogonal to j's demand shock, β_1 is identified. Put another way, all one requires for identification is that firm j's bank experiences a *net* (of other firms' demands) liquidity supply shock that is orthogonal to firm j's credit demand.

the circumstances generating the liquidity shocks ($\Delta \bar{D}_j$) actually led to a *negative* correlation between $\Delta \bar{D}_j$ and unobserved demand shocks (η_j). Then, even an OLS estimate of β_1^F in equation (6) is useful, as it gives us an underestimate of the true effect.

How Are Bank Liquidity and Loan Demand Shocks Correlated in the Cross Section?— Liquidity supply and demand shocks are likely to be positively correlated in the time series in general, for reasons mentioned earlier. However, it is less clear whether these shocks are positively correlated *cross-sectionally*, i.e., across lenders at a given point in time. In fact, in our case we demonstrate that the nuclear test–induced liquidity demand and supply shocks are negatively correlated in the cross section. We first show evidence in favor of this claim and then provide an empirical test to check whether the correlation is indeed negative in the data.

Figure 2 shows that banks with a greater proportion of dollar deposits experienced larger declines in liquidity. Columns 1 and 2 of Table 1 confirm the statistical significance of this relationship, in terms of both t-stats and R^2. Column 2, which weighs each observation by bank size and is thus economically more meaningful, shows that a 1 percent increase in the percentage of dollar deposits held by a bank prior to nuclear tests leads to a 0.30 percent decline in bank liquidity. The R^2 is also high at 40 percent. Columns 3 through 6 show that although the dollar-reliant banks suffered larger liquidity declines, they were initially lending to better-quality firms. This is reflected by the fact that more dollar reliant banks had significantly lower default rates, and significantly higher profitability. Similar results are obtained if we replace percentage dollar deposits with actual deposit change on the RHS, i.e., banks that experienced larger declines in deposits were initially more profitable and had lower defaults.

If more profitable firms are better able to adapt to adverse macro shocks induced by the nuclear tests, then our assertion that $\Delta \bar{D}_j$ and η_j are negatively correlated is valid.[8] While the evidence in Table 2 is suggestive, we can also offer a more direct test for the negative correlation by using the FE estimate from equation (5). Since $\hat{\beta}_1^{FE}$ provides an unbiased estimate of β_1, we can write $\hat{\beta}_1^{OLS} = \hat{\beta}_1^{FE} + [cov(\Delta D_i, \eta_j)/var(\delta_i)]$. Thus the difference between the OLS estimate $\hat{\beta}_1^{OLS}$ and the FE estimate $\hat{\beta}_1^{FE}$ provides a direct test of how ΔD_i is correlated with η_j. In the results section we will show that the OLS estimate is smaller than the FE estimate in the *same sample* of multiple-bank firms for which we run the FE estimate. Thus $corr(\Delta \bar{D}_j, \eta_j) < 0$ and the OLS estimates of firm-level outcomes will be underestimates.[9]

We should note that the assumption we are implicitly making here is that the same selection that applies to multiple-bank firms (for which we can estimate the bank lending channel by using firm fixed effects) also holds for single-bank firms. In other words, banks with better multiple-relationship firms also have better single-relationship firms. This is not only plausible, but examining the equivalent of columns 3–6 in Table 2 restricting loans only to single-relationship firms shows the same pattern: banks with greater liquidity shocks do have better single-relationship firms.

[8] One could, alternatively, argue that although banks with more dollar deposits were of better quality, they may systematically lend to those firms whose liquidity demand co-moves with their liquidity supply (see Kashyap, Raghuram Rajan, and Stein (2002) for the full theoretical argument). If this were true, then more dollar reliant banks would also experience larger liquidity demand shocks. While the argument is valid in general, it is unlikely to apply in our context because of the exchange rate insurance provided by the central bank. The insurance implied that banks did not have an incentive to hedge exchange rate fluctuation when making lending decisions. A related concern arises if better firms experienced large demand shocks (for example, if the demand shock was worse for the trade sector). As dicussed later, however, this does not seem to be the case either.

[9] While our argument is in terms of the bank-specific liquidity shock, δ_i, and a firm's demand shock, η_j, equation (6) aggregates the bank-specific liquidity shocks across all of firm j's banks. However, it is easy to show that $corr(\delta_i, \eta_j) < 0 \vee i \Rightarrow corr(\bar{\Delta} D_i, \eta_j) < 0$ since $\bar{\Delta} D_i$ is just a weighted average of the δ_i's.

Table 2—Bank-Level Correlations with Pretest Dollar Deposit Exposure

Dependent variable	Average annual growth in bank deposits (Dec ’99–Dec ’97) (1)	(2)	Average pre–nuclear test default rate (3)	(4)	Average pre–nuclear test bank ROA (5)	(6)
Percentage of deposits in dollars in Dec ’97	−0.17	−0.30	−0.27	−0.31	0.044	0.061
	(0.08)	(0.06)	(0.06)	(0.06)	(0.014)	(0.016)
Constant	0.12	0.17	0.25	0.28	−0.013	−0.022
	(0.05)	(0.03)	(0.04)	(0.04)	(0.009)	(0.009)
Bank size weighted	No	Yes	No	Yes	No	Yes
Observations	42	42	42	42	42	42
R-squared	0.09	0.4	0.33	0.38	0.2	0.26

Notes: The regressions examine how dollar deposit reliant banks were affected by the liquidity shock—columns 1 and 2—and how they differed before—columns 3–6. The sample is the 42 commercial banks that were allowed to open dollar deposits and hence were directly affected by the “dollar freeze” as a result of the nuclear tests in May 1998. Average pre–nuclear test default rate is the loan-size weighted default rate of loans from a given bank from July 1996 to March 1998. The bank-level default rate is defined here as a fraction between 0 and 1. Average pre–nuclear test ROA is the average ROA of a bank over fiscal years 1996 and 1997 (years end in December). Robust standard errors in parentheses.

These results, therefore, provide direct evidence for our claim that due to the somewhat unique nature of the liquidity shock induced by the nuclear tests, both the OLS estimates of the firm borrowing channel and of the liquidity shock’s impact on a firm’s financial distress are likely to be underestimates of the true effect.

III. Results: The Bank Lending Channel

Figure 3 examines the bank lending channel nonparametrically by separating loans into those from “positive” and “negative” liquidity banks. Positive liquidity banks refer to banks that had above median growth in deposits after the (nuclear) shock, while negative liquidity banks refer to those with below median deposit growth. We aggregate loans within each bank category by quarter, and plot the logarithm of aggregate lending over time. Doing so puts greater weight on larger loans and ensures our results are economically meaningful. Log aggregate lending is normalized to zero in the quarter of nuclear tests (1998:II). The y-axis values can thus be interpreted as growth rates in lending relative to the nuclear shock quarter.

The aggregate trends in Figure 3 illustrate the bank lending channel and provide support for our identification strategy. First, the trend in lending *before* the shock is similar between positive and negative liquidity shock banks. Consequently any divergence in trend after the shock cannot be attributed to preexisting differential trends. Second, there is a sharp divergence in trends right after the nuclear tests. This divergence in lending due to a bank’s liquidity shock is the “bank lending channel” and can be estimated as a double-difference, i.e., the difference in lending between positively and negatively affected banks *after* the shock *less* the difference between the two *before* the shock.

We next take our empirical methodology to data using regression analysis. We start with the time-collapsed loan-level data described in Section I, with one pre– and one post–nuclear test observation for each loan. Alternatively, we could have estimated equation (5) in the time series data by including firm-quarter fixed effects. Doing so provides similar results but, as mentioned earlier, we prefer to collapse the time dimension to obtain more conservative standard errors. For expositional convenience, we divide our analysis of the bank lending channel into two parts, an “intensive margin” referring to a reduction in the amount of lending to firms borrowing at the

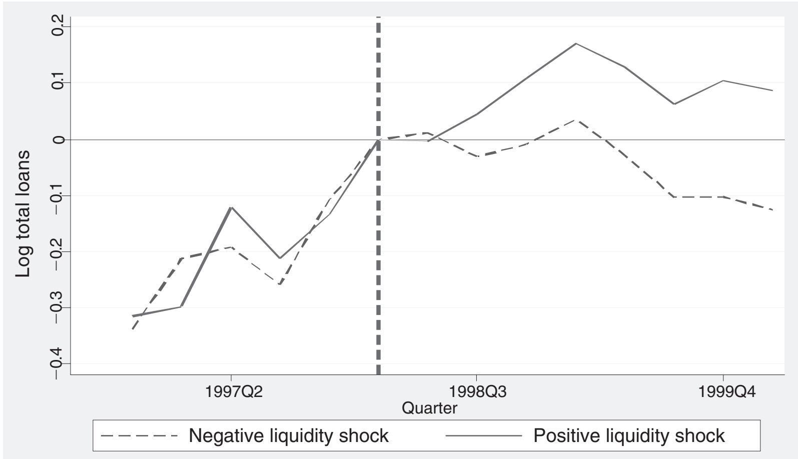

FIGURE 3. BANK LENDING CHANNEL

Notes: Figure 3 illustrates the bank lending channel by comparing lending to firms borrowing from two types of banks: negative and positive liquidity (shock) banks, with the former defined as banks whose deposit growth was below the median deposit growth in the economy, and the latter defined as banks whose deposit growth was above the median. The figure includes only firms that were borrowing and not in default at the time of the nuclear shock. For each quarter we aggregate all the loans to these firms for the positive and negative liquidity banks and plot the time series for this aggregate lending. To ease comparability we normalize the *y*-axis so that the logarithm of lending for both positive and negative liquidity banks is forced to be zero at the time of the shock, i.e., the time series illustrates the log-ratio of total loans in a given quarter relative to the quarter of the liquidity shock. The *y*-axis values can then be readily interpreted as growth rates in lending relative to the nuclear shock quarter.

time of the liquidity shock, and an "extensive margin" referring to the denial of credit to existing borrowers and to new borrowers.

A. *The Intensive Margin*

There were 22,176 performing loans to 18,647 firms at the time of the nuclear tests that continued borrowing some amount after the tests as well.[10] Table 3 estimates the first-differenced specification (4). We regress the *change* in log loan amount as a result of the nuclear tests on the *change* in log bank liquidity. Since the liquidity shock occurs at the bank level, changes in loans from the same bank may be correlated. Therefore, all our loan-level regressions cluster errors at the bank level. Since there are only 42 banks in our main sample, standard errors are likely to be conservative.

Column 1 in Table 3 presents the preferred FE estimation strategy in equation (5) that provides an unbiased estimate of the bank lending channel coefficient. The FE sample is restricted to the 1,864 multibank firms with a total of 5,382 loans. The results indicate a large bank lending channel: a 1 percent decline in bank liquidity leads to a 0.6 percent decline in the bank's loan

[10] For the intensive margin sample we exclude firms that immediately and entirely stop borrowing from their bank(s) after the shock, i.e., firms that don't borrow anything *in every post-shock period*. Such firms show up as large outliers in our first-difference log-specification and would therefore unduly influence our estimates. Including these firms only increases the magnitude of our estimates.

Table 3—The Bank Lending Channel—Intensive Margin

	Δ Log loan size						
	FE	FE	FE	OLS	OLS	OLS	OLS
Dependent variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Δ Log bank liquidity	0.60	0.63	0.64	0.46	0.64	0.30	0.33
	(0.09)	(0.10)	(0.11)	(0.14)	(0.17)	(0.12)	(0.15)
Δ Log bank liquidity × small firms						0.57	0.40
						(0.26)	(0.21)
Small firms						0.18	0.24
						(0.06)	(0.03)
Lag Δ log bank liquidity		0.15					−0.13
		(0.10)					(0.14)
Preshock average bank ROA		0.99					−0.27
		(1.73)					(1.66)
Log bank size		0.02					−0.02
		(0.03)					(0.03)
Preshock bank capitalization		−1.16					0.09
		(0.97)					(1.13)
Preshock bank default rate		−0.869					−0.518
		(0.36)					(0.32)
Government bank dummy		0.13					−0.01
		(0.06)					(0.08)
Foreign bank dummy		0.01					−0.12
		(0.06)					(0.08)
Fixed effects	Firm	Firm	Firm × loan-type				Firm Controls
Constant	—	—	—	−0.06	−0.04	−0.14	—
				(0.04)	(0.04)	(0.03)	
Number of observations	5,382	5,382	5,382	5,382	22,176	22,176	22,176
R-squared	0.44	0.44	0.6	0.01	0.02	0.03	0.05

Notes: These regressions examine the bank lending channel for the set of firms borrowing at the time of the shock (the intensive margin) in more detail. All quarterly data for a given loan are collapsed to a single pre– and post–nuclear test period. The nuclear test occurred in the second quarter of 1998, so all observations from 1996:III to 1998:I for a given loan are time-averaged into one. Similarly, all observations from 1998:III to 2000:I are time-averaged into one. Data are restricted to: (a) banks that take retail (commercial) deposits (78 percent of all formal financing), and (b) loans that were not in default in the first quarter of 1998 (i.e., just before the nuclear tests). Columns 1–4 are run on the sample of firms that borrow from multiple banks (preshock) and include firm fixed effects (firm interacted with loan type for column 4). Columns 5–7 also include firms borrowing from single banks and run an OLS specification. Firm controls in column 7 include dummies for each of the 134 cities/towns the firm is located in, 21 industry dummies, whether the firm is politically connected, its membership in a business conglomerate, and whether it borrows from multiple banks. Standard errors in parentheses are clustered at the bank level (42 banks in total).

to a firm. Since the firm fixed effects in column 1 are added after first differencing the data, they absorb all time-varying firm-specific factors, including firm-specific credit demand shocks. Columns 2 and 3 show that this result is robust to adding bank- and loan-level controls, including loan type interacted with firm fixed effects. We will return to these results in more detail when we discuss robustness issues at the end of the section. We should note that we can also isolate deposit shock variation to a bank arising from foreign currency accounts (FCA) exposure, by instrumenting the deposit shock by a bank's preshock FCA. Doing so (regressions not shown) provides similar results.

Figure 4 graphically illustrates how the firm FE approach addresses the concern of supply-demand correlation by holding fixed the identity of a firm across positive and negative liquidity banks. This figure is the graphical counterpart to the regression in column 1 and the firm fixed effects counterpart to Figure 3. For each firm, we classify its bank as a "positive liquidity shock

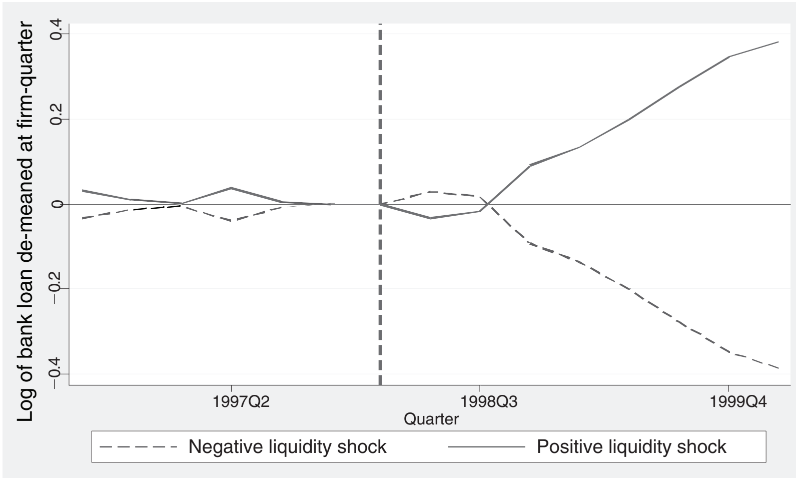

FIGURE 4. BANK LENDING CHANNEL WITH FIRM FE

Notes: Figure 4 illustrates the bank lending channel by comparing lending *within* the same firms that borrow from two types of banks: (relative to the firm's mean bank) negative and positive liquidity (shock) banks. This figure is the counterpart of the fixed effects regression in column 1 of Table 3. Specifically, we restrict to firms that were borrowing (and not in default) from at least two banks before the shock. For each firm we classify its loans into those from banks that had a change in liquidity greater (positive) or less (negative) than this firm's average bank. We then de-mean each of the firm's loans (by subtracting the firm's average loan in each quarter). The figure then aggregates all the de-meaned negative bank and positive bank loans and plots this logarithm on the *y*-axis. Given our classification process, we are guaranteed that the same firm shows up in both the plotted lines, and that one line is the negative mirror image of the other. Given this de-meaning, if the bank lending channels were correctly identified, we would expect to find little/no lending difference between the two series before the shock, but a divergence afterward. The figure shows that this is indeed the case.

bank" if its liquidity shock is higher than the median liquidity shock for all banks lending to that firm. The remaining banks lending to that firm are classified as "negative liquidity shock banks." The figure then plots the firm's de-meaned log-loan amounts (subtract the firm's log average loan amount over its history) for its positive and negative liquidity banks. Figure 4 shows there is little significant difference between loans taken by the same firm from positive and negative liquidity banks *before* the nuclear tests. However, soon after the liquidity shock hits, there is a sudden and sharp divergence in loans given out by the two sets of banks to the *same* firm. Since firm-level changes in loan demand are taken out by construction, Figure 4 provides a cleaner (than Figure 3) identification of the bank lending channel effect.

Column 4 estimates the OLS bank lending channel coefficient using the same multibank sample of column 1. The OLS coefficient drops to 0.46, compared to 0.60 for FE. As Section II highlighted, the drop in the OLS coefficient implies that a bank's liquidity supply and its client firms' loan demand shocks are cross-sectionally negatively correlated. Consequently, OLS provides an underestimate of the true effect.

Column 5 repeats the OLS specification of column 4 on the full sample of firms. The bank lending channel coefficient is larger in the full sample, suggesting that the lending channel effect is larger for single relationship firms. Since our previous figures suggested differences across firms based on their size, columns 6 and 7 explore this further. We divide firms into small and large firms—where small refers to firms in the bottom 70 percent of the size distribution and large refers to the top 30 percent (see Figure 7 for a justification for this cutoff). Doing so shows that the lending channel is indeed stronger for smaller firms: column 6 in Table 3 shows that for a 1 percent drop in a bank's liquidity, its lending to a small firm drops 0.87 percent for small firms versus 0.3 percent for large firms. Column 7 shows that this result is robust to adding firm and bank controls—the estimates are now 0.73 percent for small firms versus 0.33 percent for large firms.[11] Figures 1A and 1B in the online Appendix (available at http://www.aeaweb.org/articles.php?doi=10.1257/aer.98.4.1413) illustrate this by presenting Figure 3 separately for large and small firms. The differential treatment by firm size is particularly stark since one would have thought that smaller firms would value (and pay for) greater insurance from their lenders against liquidity shocks. This greater shock transmission to smaller firms will be a recurrent theme in our results and will be discussed in more detail below.

B. *The Extensive Margin*

Do bank liquidity shocks also affect the extensive margin of banks by forcing them either to stop lending to firms altogether or reduce the intake of new firms? We begin by testing if the "exit rate" of firms is higher for banks harder hit by the liquidity crunch. For each loan, we create a variable, *EXIT,* which is 1 if the loan is not renewed at some point during the first post–nuclear test year. As before, we use the firm FE approach to control for changes in loan demand at the firm level, and test whether the *same* firm borrowing from different banks is more likely to exit a negative liquidity shock bank. This translates into estimating the following FE specification on multibank firms:

$$EXIT_{ij} = \beta_j + \beta_1 \, \Delta D_i + \varepsilon_{ij} . \tag{7}$$

β_1 is the coefficient of interest.

Column 1 in Table 4 runs the FE specification and shows that a 1 percent reduction in bank liquidity leads to a 21-basis-point increase in the probability of exit for a loan (that is, about a 1 percent increase in probability since the mean exit rate for loans was 20.7 percent during this period). Column 2 shows that the result remains robust to adding preshock bank-level controls such as the bank's return on assets, size, capitalization ratio, portfolio quality, and ownership type.

Column 3 then examines whether smaller borrowers experience the same (or larger) impact. Since small borrowers typically borrow from a single bank only (and would therefore be absorbed by the firm fixed effect), we prefer to run an OLS specification on the full sample of firms. The results in column 3 show there is no significant difference in exit rates in response to the liquidity shock between large and small borrowers.[12]

We next test if liquidity shocks also affect banks' ability to make new loans. We start with all loans given out in the post–nuclear test year (35,921 loans) and create a variable *ENTRY,* which is one if the loan was first made in the post–nuclear test period. Using *ENTRY* as the left-hand-side (LHS) variable, we repeat the analysis presented in columns 1 through 3.

Column 4 shows that liquidity supply has a significant impact on a bank's ability to issue new loans. A 1 percent reduction in bank liquidity reduces its probability of making a loan to a new client by 12 basis points (the mean entry rate in the data was 38.5 percent). The firm fixed

[11] While these regressions could in principle include firm fixed effects, since few of the small firms borrow from multiple banks, doing so would leave little small-firm variation to explore.

[12] Using a nonlinear probit model gives the same results as our linear specification. We prefer to use the linear model since the results are then comparable with the firm FEs specification where we cannot use a probit model.

TABLE 4—THE BANK LENDING CHANNEL—EXTENSIVE MARGIN

Dependent variable	Exit?			Entry?		
	FE (1)	FE (2)	OLS (3)	FE (4)	FE (5)	OLS (6)
Δ Log bank liquidity	−0.21	−0.19	−0.16	0.12	0.15	0.087
	(0.05)	(0.05)	(0.059)	(0.05)	(0.04)	(0.049)
Small			0.084			0.2
			(0.019)			(0.015)
Small × Δ log bank liquidity			0.077			0.11
			(0.084)			(0.067)
Constant	—	—	—	—	—	—
Firm fixed effects	Yes	Yes		Yes	Yes	
Bank controls		Yes	Yes		Yes	Yes
Firm controls			Yes			Yes
Number of observations	6,517	6,517	26,730	8,516	8,516	35,921
R-squared	0.48	0.49	0.09	0.54	0.55	0.21

Notes: These regressions examine how the bank lending channel affected exit and entry of firms (from borrowing). Data are restricted to: (a) banks that take retail (commercial) deposits (78 percent of all formal financing), and (b) loans that were not in default in the first quarter of 1998 (i.e., just before the nuclear tests). Columns 1–3 look at exit by including all loans that were outstanding at the time of the nuclear tests. For a given loan, "exit" is classified as one if the loan is not renewed and the firm exits its banking relationship by the first postshock year. Columns 1–2 further limit the sample to only firms that were borrowing from multiple banks before the shock and include firm fixed effects. Columns 4–6 look at entry and include all loans given out after the nuclear tests quarter. For a given loan, "entry" is classified as one if the loan was made for the first time in the postshock year. Columns 4–5 further limit the sample to only firms that were borrowing from multiple banks after the shock and include firm fixed effects. All regressions include bank level controls: lagged change in bank liquidity, preshock bank ROA, log bank size, bank capitalization, fraction of portfolio in default, and dummies for foreign and government banks. The OLS regressions also include an extensive set of firm-level controls that include dummies for each of the 134 cities/towns the firm is located in, 21 industry dummies, whether the firm is politically connected, its membership in a business conglomerate, and whether it borrows from multiple banks. Standard errors in parentheses are clustered at the bank level (42 banks in total).

effects once again ensure that the entry effect is not driven by unobserved firm-level time-varying factors (such as shocks to credit demand). Column 5 shows that the effect remains robust to bank-level controls. Column 6 runs OLS in the full sample of firms, and shows that while large borrowers are more likely to start new relationships with positive liquidity banks, this effect is twice as large for small borrowers, i.e., not only are small borrowers more likely to enter, but they do so more (less) for banks with greater (lower) liquidity.

Tables 3 and 4 show that bank liquidity shocks have large lending channel effects both on the intensive and extensive margins. The results suggest that the MM theorem breaks down at the bank level, and shocks to the banking sector are transmitted to firms through changes in the banks' lending patterns. The magnitude of these effects is large. A one-standard-deviation shock to bank liquidity (30 percent—Table 1) leads to an 18 percent decline in lending, a 6.3 percentage point increase in the likelihood of exit, and a 3.6 percentage point decrease in the likelihood of new loan origination.

In addition, the results show that the bank lending channel is larger for small firms. To the extent that smaller firms are less able to hedge against such shocks, this is a striking result and raises concerns (explored below) that small firms may fare even worse in terms of overall financial outcomes. We should also emphasize, however, that the bank lending channel is large even for the relatively protected large firms. Rerunning the preferred firm-fixed effect specification in column 1 of Table 3 for large firms gives a point estimate of only 0.6; that is, a 1 percent drop in a bank's liquidity reduces its lending to a large firm by 0.6 percent. Thus, for large firms, while their initial lender does protect them from its liquidity shock relatively more than a smaller client, this protection is limited and by no means sufficient. This leaves open the question, examined later, of whether large firms can fully hedge the bank lending channel shock by borrowing more from other banks.

C. *Do Liquidity Shocks Affect the Price of Loans?*

Thus far, we have focused on the amount of loans given out by a bank. We now test whether, in addition to this quantity effect, bank liquidity shocks also affect the price of loans, i.e., the interest rate charged. While the loan-level CIB dataset does not record interest rates charged, another data source from the central bank records the average interest rate for loans of different sizes charged by a given bank branch at a point in time. This data can be used to proxy for the interest rate charged on a given loan using loan size and location information.

Using these data, we compute the change in interest rates from December 1997 to December 1999 for each loan and regress this variable on change in log of bank liquidity, as before.[13] Column 1 in Table 5 presents our preferred firm fixed effects specification and shows no statistically significant effect; i.e., a firm does not experience interest rate increases from its bank that experienced a greater liquidity fall relative to one that did not. Comparing within the same firm assures us that the interest rate comparison is not affected by any firm-level differences in credit demand, etc.

Column 2 pushes this further by introducing the interaction of loan-type fixed effects with firm fixed effects. Doing so forces the comparison across the same firm borrowing the *same type* of loan across two different banks. Column 3 presents the OLS (and potentially biased) results in the full sample of firms. While the coefficient magnitude rises, so do standard errors. Column 4 examines whether the effect varies by firm size and finds no significant results. So while the bank lending channel affects the quantity of loan supply, it does not affect the average price charged by banks.

Note that the magnitude of the coefficient on change in liquidity is weakly positive, suggesting that even if there is a price effect, it is the opposite of what one would expect: instead of negatively shocked banks raising their interest rates, they are more likely to drop them. This is not surprising if the liquidity drop causes banks to cut lending to marginally riskier borrowers that are typically charged higher interest rates. More generally, the small magnitude could also be because interest rates are constrained due to interbank loan competition, or because of a fear of increasing moral hazard concerns. We should caution that it could also simply be that our average interest rate information is not disaggregated enough to capture differences in interest rates at the loan level across firms. This is unlikely, though, since a general examination of lending suggests there is not much variation in interest rates even at the loan level.

Our interest rate results are similar in spirit to Mitchell Peterson and Rajan (1994), who find that closer ties between the firm and its creditor increases the availability of credit but does not lower the price of credit. This suggests that quantity rather than price is the more relevant margin in bank-firm relationships.

D. *Robustness to Alternative Explanations*

Although the firm FE approach used to identify the bank lending channel resolves a number of empirical concerns by accounting for firm-specific time-varying variables such as changes in a firm's credit demand and other firm-level attributes, we address some remaining concerns.

[13] Out of the 42 commercial banks used in our analysis, interest rate information is available for 39.

Table 5—Liquidity Impact on Interest Rates

Dependent variable	Δ Interest rate			
	(1) FE	(2) FE	(3) OLS	(4) OLS
Δ Log bank liquidity	0.28	0.33	1.53	−0.43
	(0.16)	(0.21)	(1.02)	(0.67)
Small firms				0.20
				(0.21)
Δ Log bank liquidity × small firms				0.64
				(0.78)
Fixed effects	Firm	Firm × loan type		
Bank controls				Yes
Firm controls				Yes
Constant	—	—	−1.59	
			(0.34)	
Number of observations	5,161	5,161	21,769	21,769
R-squared	0.43	0.57	0.02	0.13

Notes: These regressions examine the impact of the liquidity shock on interest rates. The interest rate data are not available for each loan, but rather at the bank branch level for different loan size classifications. Using a borrower's bank branch and loan size information, we can then create a "proxy" loan-level interest rate. All quarterly data for a given loan are then collapsed to a single pre– and post–nuclear test period. The nuclear test occurred in the second quarter of 1998, so all observations from 1996:III to 1998:I for a given loan are time-averaged into one. Similarly, all observations from 1998:III to 2000:I are time-averaged into one. Data are restricted to: (a) banks that take retail (commercial) deposits (78 percent of all formal financing), and (b) loans that were not in default in the first quarter of 1998 (i.e., just before the nuclear tests). Columns 1–2 further restrict the data to firms that were borrowing from multiple banks before the shock (in order to include firm fixed effects). Column 4 includes additional bank- and firm-level controls. The bank controls are the lagged change in bank liquidity, preshock bank ROA, log bank size, bank capitalization, fraction of portfolio in default, and dummies for foreign and government banks. Additional firm-level controls are dummies for each of the 134 cities/towns the firm is located in, and 21 industry dummies. Standard errors in parentheses are clustered at the bank level (42 banks in total).

Loan-Specific Credit Demand.—Firm fixed effects take out spurious credit demand concerns by absorbing changes in credit demand at the firm level. This may not be sufficient, however, if a firm's credit demand is loan specific *and* shocks to loan demand are correlated with the bank liquidity shocks. For example, suppose that dollar reliant banks specialize in making longer-term loans, and nuclear tests led to a disproportionate reduction in the demand for long-term loans. Then, even in the absence of any bank lending channel, firms borrowing from dollar reliant banks will contract their relative borrowing from these banks.

We can test for such concerns using information on the type of loan taken by a firm. A loan in our dataset can be classified as: (a) short term (under 6 months) working capital loans, (b) longer-term fixed loans, or (c) nonfunded loans such as guarantees and letters of credit. We then control for loan types nonparametrically by interacting firm fixed effects with loan type to ensure comparison of the *same* loan type and for the *same* firm across banks (this gives us 2,731 fixed effects for the 1,864 firms in the sample of firms with multiple preshock banks). The result in column 3 of Table 3 shows no significant change in the lending channel coefficient. It is also worth noting here that since most of the firms belong to traditional sectors, such as textile and consumer goods, it is unlikely that firms take "specialized" loans in the first place.

A similar concern may be related to firms taking on export/import related loans from dollar reliant banks. If the export/import part of a firm's business suffered disproportionately more, then once again we will have a spurious within-firm correlation between credit demand and bank liquidity shocks. We test for this concern (regression not shown) by limiting our fixed effect analysis to the 1,588 firms that do not export at all, and find that the bank lending coefficient is unchanged (0.55 versus 0.60 in column 1 of Table 3).

Heterogeneity in Bank Response to Macro Shocks.—Could the lending channel coefficient be driven by inherent differences in how banks respond to the shocks induced by the nuclear tests? This is possible if there is such response heterogeneity *and* it is systematically correlated with a bank's liquidity shock. For example, perhaps the lending channel estimate is picking up differences in how foreign and local banks react to nuclear tests, since we know that foreign banks are more likely to deal in dollar deposits.

Suppose more dollar reliant banks (which we know are better banks ex ante) are generally more "cautious" in making loans or respond more to "hard information," and therefore react more than other banks to a given firm productivity shock. Then, dollar reliant banks, which also experience larger declines in deposits, might cut back lending to the same firm more, not because of a lack of liquidity but because they want to reduce their loan portfolio risk more than less affected banks.

We test for such concerns by including various bank characteristics that proxy for such differential lending sensitivity as controls. Column 2 in Table 3 includes several pre–nuclear test bank-level controls such as the bank's return on assets, lagged deposit growth, bank size, capitalization ratio, fraction of portfolio in default, and dummies for foreign and government banks.[14] Bank ROA, portfolio in default, and capitalization ratios in particular are likely to capture a bank's sensitivity to client quality, and bank-type dummies capture organizational differences. Moreover, we can also include bank-type dummies interacted with firm fixed effects (regressions not shown). The results indicate that the lending channel coefficient remains robust to all these bank-level controls.

While unlikely, one may still worry that these preshock measures are not appropriate because they are not good proxies for the bank's quality sensitivity; even if they are initially, they are not after the shock. We can address this remaining concern more directly by examining changes in the quality of the bank's portfolio after the shock. If the differential quality sensitivity criticism is valid, then the banks facing liquidity shocks should be relatively *more* unwilling to lend to firms experiencing negative productivity shocks, and therefore their portfolio quality should (relatively) increase after the shock. In Table 8 we will directly examine changes in firm default rates and, in fact, show that the opposite is true—banks with greater liquidity shocks see their client firms' default rates *increase*. Thus, it cannot be that these banks are becoming more quality conscious than other banks.

Other Robustness and Interpretational Concerns.—A potential concern may be that the bank lending channel captures an initial differential cost of capital difference, i.e., if banks had lower costs of capital on account of holding foreign currency accounts, then one may worry that their lending drop after the liquidity shock is not a result of the supply shock, but, rather, a price correction (such as the removal of the cheaper source of capital). If this were the case, then one would expect to see such banks *raise* the interest rates on their loans as a result of this price correction. However, the results in Table 5 show the opposite. Such negatively affected banks actually drop their interest rates, although this result is not economically or statistically significant.

One should also point out an alternate "deposits as collateral"–based explanation. To the extent that a firm's cash balances are informative about its cash flows and offer liquid collateral, banks may reduce lending to a firm simply because the firm draws down its cash reserves held as bank deposits. However, such level effects are taken into account by the firm fixed effects in our primary first-differenced specification. The firm-fixed-effects strategy is called to question only if a bank gives greater weight to a firm's cash reserves that are held at the bank itself, as compared to cash reserves held at other banks, and this goes against institutional evidence on lending practices.

[14] Pre-1998 ROA, bank size, and capitalization rate are averages over fiscal years 1996 and 1997 (fiscal years end in December). Pre-1998 deposit growth is calculated as growth in deposits from December 1996 to December 1997.

Moreover, this is particularly implausible for large firms since banks have access to audited information regarding such firm's cash reserves and flows. Since the bank lending channel holds for such large firms as well, it seems unlikely that it is driven by such collateral-based explanations.

Finally, a concern might be that our FE results represent a "strategic" withdrawal by firms from hard-hit banks. For example, a firm may choose to cut back borrowing from a bank facing liquidity problems and switch to more liquid banks for fear that the liquidity constrained bank might become insolvent in the future. This is unlikely, however, since banks hit by the liquidity crunch were historically more profitable (as borne out in Table 2) and, while they did see profitability decline post-shock, they still remained as profitable as the banks that did not lose liquidity. In fact, no bank declared insolvency after the nuclear tests.

It is worth noting that the last concern is more of an interpretational issue. Provided the loan decline is induced on account of the initial supply-side shock, i.e., firms borrow less from banks that were hit by a larger liquidity shock (that is exogenous to the firms' demand), one could interpret it as a bank lending channel.

IV. Results: Firm-Level Impact

A. *Can Firms Hedge Bank-Specific Liquidity Shocks?*

We have seen that negative shocks to a bank's liquidity supply translate into a drop in its client firms' loans for both large and small firms. However, such bank lending channels may not have any aggregate effect if firms can compensate for bank-specific loan losses by borrowing more from banks with greater liquidity. Unlike earlier empirical studies, we can test for the extent of such substitution, since our data match firms to all 145 bank and nonbank financial intermediaries in the economy.

So far, we restricted our attention to the 42 commercial banks that used demandable deposits as their source of liquidity. Since a firm might use any commercial or noncommercial bank to compensate for the lending channel, we now include all the 145 financial intermediaries in our analysis and construct the aggregate loan amount borrowed by each firm from all of these intermediaries before and after the nuclear tests.

We then compute the average liquidity shock faced by each firm by constructing a loan-size weighted average of the change in deposits for the banks that the firm borrowed from before the nuclear tests. In constructing this aggregate liquidity shock at the firm level, we assume that noncommercial banks experience the economy-wide change in liquidity. Since the noncommercial banks comprise only 22 percent of the market share, this assumption is not crucial for our results. For example, assuming instead that noncommercial banks experience no change in liquidity does not change our results.

As discussed in Section II, equation (6) provides a test for the extent of substitution. If there is no substitution, then β_1^F in (6) should be the same as that in (4), i.e., the same as the bank lending channel effect. At the other extreme, if there is full substitution, all firms will have equal access to lenders regardless of whom they borrowed from initially. In such a scenario, a given bank's liquidity crunch will have no impact on its firm's aggregate borrowing, and β_1^F will be zero since all firms will respond only to the aggregate liquidity shock (captured by β_0^F). More generally, the greater the substitution, the closer β_1^F will be to zero.

Column 1 in Table 6 shows that, on average, firms are unable to compensate for the bank lending channel by increasing borrowing from more liquid banks.[15] Column 2 separates this effect

[15] We still cluster observations at the bank level in Table 6. However, since observations across banks are aggregated at the firm level, for multiple-relationship firms we use the largest lender of a firm as the unit of clustering. Similarly,

Table 6—The Firm Borrowing Channel

Dependent variable	Δ Log aggregate loan size			
	OLS (1)	OLS (2)	OLS (3)	OLS (4)
Δ Log bank liquidity	0.65 (0.04)	0.04 (0.09)	0.00 (0.09)	0.29 (0.11)
Small firms		0.18 (0.02)	0.19 (0.02)	0.28 (0.03)
Δ Log bank liquidity × small firms		0.80 (0.10)	0.64 (0.10)	0.48 (0.12)
Conglomerate firm?				0.09 (0.03)
Δ Log bank liquidity × conglomerate firm				−0.28 (0.14)
Political firm?				0.13 (0.02)
Δ Log bank liquidity × political firm				−0.29 (0.12)
Multiple relationship firms				0.18 (0.03)
Δ Log bank liquidity × multiple relationship firms				−0.05 (0.15)
Bank controls			Yes	Yes
Firm controls			Yes	Yes
Constant	0.04 (0.01)	−0.08 (0.02)	—	
Number of observations	18,647	18,647	18,647	18,647
R-squared	0.02	0.03	0.05	0.06

Notes: These regressions examine the impact of the liquidity shock on the total borrowing (across all lending institutions) of firms. All bank loans at a point in time (from any of the 145 lending institutions) for a given firm are summed to compute the aggregate firm-level loan size. The liquidity shock experienced by a firm is the (loan-size) weighted liquidity shock experienced by the banks it was borrowing from prior to the shock (lending institutions that do not hold deposits are assigned a liquidity shock of zero). All quarterly data for a given firm are then collapsed to a single pre– and post–nuclear test period. The nuclear test occurred in the second quarter of 1998, so all observations from 1996:III to 1998:I for a given loan are time-averaged into one. Similarly, all observations from 1998:III to 2000:I are time-averaged into one. Data are restricted to loans that were not in default in the first quarter of 1998 (i.e., just before the nuclear tests). Bank-level controls include lagged change in bank liquidity, pre-shock bank ROA, log bank size, bank capitalization, fraction of portfolio in default, and dummies for foreign and government banks. Firm-level controls include dummies for each of the 134 cities/towns the firm is located in, and 21 industry dummies. Standard errors in parentheses are clustered at the bank level, i.e., the largest lender for a firm.

for large and small borrowers and shows that while large borrowers fully offset their bank's liquidity shock, in stark contrast, the significant interaction term shows that small borrowers are unable to hedge the initial liquidity shock faced by their banks. For a 1 percent decline in their initial lender's liquidity, total borrowing for these firms drops by 0.87 percent (column 6, Table 3), essentially the same result—0.84 percent (column 2, Table 6)—as the bank lending channel for smaller firms. Thus, smaller borrowers are entirely unable to avoid the adverse liquidity shock by going to other lenders in the market. However, the average large firm completely hedges its bank lending channel effect: recall that the (OLS) estimates of the bank lending channel for large firms was 0.30 (column 6, Table 3). The analogous estimate for the firm borrowing channel for large firms is statistically lower and not significantly different from zero, at 0.04 (column 2, Table 6). Figures 2A and 2B in the online Appendix illustrate the same effect nonparametrically (analogous to Figure 3) in the time-series data.

for multiple relationship firms, "bank controls" are constructed by value-weighing bank data for each of the banks a firm borrows from.

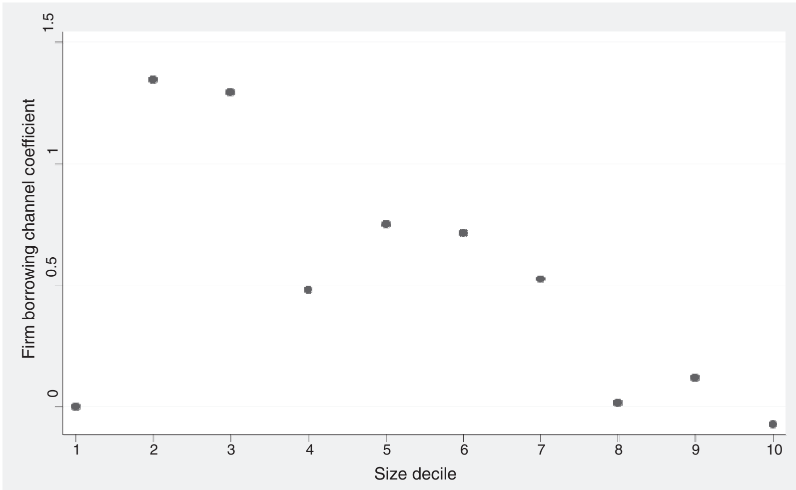

FIGURE 5. FIRM BORROWING CHANNEL COEFFICIENT BY SIZE DECILE

Notes: Figure 5 illustrates heterogeneity in the impact of bank liquidity shocks on overall firm borrowing for each borrower size decile. It does so by estimating the coefficient from an OLS specification similar to column 3 of Table 6, but where we separately estimate the impact of the liquidity shock on all ten borrower deciles (by preshock borrowing size). Apart from the lowest borrower decile (where we have little precision), we see that the impact on overall borrowing for the firm falls for larger borrowers. In fact, it is almost nonexistent for the largest three borrower deciles (our "large" borrower classification).

Figure 5 presents a nonparametric picture of this size heterogeneity. We compute the same effect separately for each firm borrowing size decile—Figure 5 plots the coefficient estimate for each size decile and shows how it progressively declines (except for the smallest decile, which probably has a lot of noise). In particular, the top three deciles show almost no effect and justify why we include them in our "large" borrower category.

Column 3 shows that this result remains robust to the inclusion of an extensive set of firm- and bank-level controls. Firm-level controls include dummies for each of the 134 cities/towns the firm is located in, 21 industry dummies, whether the firm is politically connected, its membership in a business conglomerate, and whether it borrows from multiple banks . Bank-level controls include lagged change in bank liquidity, pre-shock bank ROA, log bank size, bank capitalization, fraction of the bank's portfolio in default, and dummies for foreign and government banks.

Thus, while both large and small firms face a bank lending channel shock, small firms face the full brunt of the shock to their initial lender's liquidity and are entirely unable to hedge, while the average large firm is completely able to hedge. Given the preferential treatment of banks toward large firms we previously saw in the bank lending channel, this additional preferential treatment by other (unaffected) banks does not come as a surprise: small firms are hurt not only because their initial lender passes on a larger share of its liquidity shock to these firms, but also because these firms are less able to compensate this loss by going to other banks. One may speculate that the same underlying mechanism that causes the bank lending channel effect to be smaller for large firms also enables them to substitute out of the credit supply shock that they experience. Conversely, the fact that small firms face a larger bank lending channel may make it even harder for them to approach other lenders in order to hedge this shock.

Column 4 examines the size result further by effectively running a "horse race" between firm size and other firm attributes, such as whether the firm is a member of a large conglomerate, is politically connected, and had multiple lenders at the time of nuclear tests. We do so by including the interactions of these other firm attributes with its initial lenders' liquidity shock. While the coefficient on the omitted category (large "unconnected" firms, i.e., that are not members of a conglomerate, politically connected, and did not have multiple lenders) increases, the important point is that the "size" dummy has the largest impact in reducing the magnitude of bank liquidity shocks on firm aggregate borrowing: the marginal effect of "large" is –0.48, while the marginal effect of "political" and "conglomerate" is roughly half of that (–0.29 and –0.28, respectively).

Column 4 also raises the question of how large firms are able to hedge, especially if one interprets the increase in the coefficient on the large but "unconnected" firms as evidence that the hedging by large firms was on account of their conglomerate and political connections. However, we believe that such interpretations are at best tentative. A concern is that, not surprisingly, size is strongly positively correlated with the other firm attributes.[16] Not only does this decrease the ability to discriminate between their differential effects, but the "unconnected" large firms are also no longer as large: the average size of the large "unconnected" firms is around one-third of that for the "connected" large firms. Therefore, given the size heterogeneity results seen in Figure 5, one would naturally expect less hedging for the (smaller of the) large firms once we add interactions with other firm attributes. Despite this, we find that even the unconnected large firms partly hedge: the firm borrowing estimate, at 0.29 (Table 6, column 4), is smaller than the bank lending one (0.38) in the analogous bank lending channel regression (not given).

While our data really do not allow us to test whether the ability of large firms to hedge is due to some inherent productivity advantage or whether these firms exercise "power" to elicit favors from banks, regardless of the reason, firm size is the most important dimension compared to the other firm attributes examined. For example, it is not the case that large firms are offered protection primarily on account of their political connections. While not implausible, given our previous work on political rent provision by government banks in Pakistan (Khwaja and Mian 2005), in this case our evidence suggests that it is size that matters. Not only is size the most important hedging margin (Table 6, column 4) and large nonpolitical firms can hedge, but political connections by themselves are not sufficient: while the average large firm is completely hedged (Table 6, column 3), estimating an analogous specification (regression not shown) for political connections shows that the average political firm faces a 0.32 percent drop in its overall borrowing (the firm borrowing channel) for a 1 percent deposit shock to its initial lender.

While we remain agnostic about *why* large firms are better able to hedge, one can ask more mechanical yet revealing questions of *how* this hedging is obtained: are large firms hedged simply because they have a more diversified set of initial lenders or are they (also) better at accessing compensatory loans from existing and new banks?

The results in column 4 already suggest that the ability to hedge is not ex ante, i.e., it is not the case that large firms were borrowing from one bank that received a negative shock and another that received a positive shock: the interaction term on multiple banking relationships has no independent power in explaining hedging. Another way to analyze this is to compare the standard deviation of bank liquidity shocks with the standard deviation of these shocks within and between firms. Table 1 shows that the standard deviation of bank liquidity shocks is 30 percent. The *between* firm variation in bank liquidity shocks, i.e., standard deviation of the loan-weighted liquidity shock ($\Delta \bar{D}_j$) at the firm-level, is 22.9 percent, and the standard deviation of liquidity shocks *within* firms is 19.3 percent. The distribution of liquidity shocks is thus very similar between banks, between firms, and within firms. Hedging is therefore an active ex post phenomenon.

[16] For example, only 11.8 percent of lending within large firms goes to firms that are neither politically affiliated nor connected through a business conglomerate.

Table 7—Decomposing the Firm Borrowing Channel

Dependent variable	Δ Log aggregate loan size aggregating loans post test using only:		
	Existing banks OLS (1)	New banks OLS (2)	Existing and new OLS (3)
Δ Log bank liquidity	0.15	−0.40	0.04
	(0.09)	(0.08)	(0.09)
Small firms	0.24	−0.23	0.18
	(0.02)	(0.02)	(0.02)
Δ Log bank liquidity × small firms	0.68	0.53	0.80
	(0.10)	(0.08)	(0.10)
Constant	−0.19	−2.55	−0.08
	(0.02)	(0.02)	(0.02)
Number of observations	18,647	18,647	18,647
R-squared	0.03	0.01	0.03

Notes: These regressions explore how firms compensate for their banks' liquidity shock. We split a firm's total borrowing postshock between banks it was borrowing from before the shock (column 1) and banks it started borrowing from after the shock (column 2). The liquidity shock experienced by a firm is the (loan-size) weighted liquidity shock experienced by the banks it was borrowing from prior to the shock. (Lending institutions that do not hold deposits are assigned a liquidity shock of zero.) All quarterly data for a given firm are collapsed to a single pre– and post–nuclear test period. The nuclear test occurred in the second quarter of 1998, so all observations from 1996:III to 1998:I for a given loan are time-averaged into one. Similarly, all observations from 1998:III to 2000:I are time-averaged into one. Data are restricted to loans that were not in default in the first quarter of 1998 (i.e., just before the nuclear tests). Standard errors in parantheses are clustered at the bank level, i.e., the largest lender for a firm. The bank controls are lagged change in bank liquidity, preshock bank ROA, log bank size, bank capitalization, fraction of portfolio in default, and dummies for foreign and government banks. Firm-level controls include dummies for each of the 134 cities/towns the firm is located in, 21 industry dummies, whether the firm is politically connected, its membership in a business conglomerate, and whether it borrows from multiple banks. Standard errors in parentheses are clustered at the bank level, i.e., the largest lender for a firm.

Table 7 further explores how large firms hedge by separately examining a firm's overall borrowing after the shock from its existing banks and new banks. Column 1 first looks at overall borrowing post-shock from the banks a firm was *already* borrowing from prior to the shock. For every percent decrease in their banks' liquidity, large borrowers face a 0.15 percent drop in their aggregate borrowing.

Column 2 then looks at aggregate borrowing changes from banks a firm was *not borrowing* from before the shock. These changes are measured relative to the firm's total borrowing before the shock. The result shows that large borrowers also hedge by being able to borrow more from banks they were not borrowing from before, i.e., the negative coefficient implies that if a large firm's existing banks suffered an average negative liquidity shock, the firm is able to borrow more from new banks. The coefficient in column 2 is not directly comparable to that in column 1 since it measures the sensitivity of borrowing from new banks to the *old* banks' average liquidity shock. Results from Table 4 (columns 4 and 5) already show that these new banks are more likely to be ones with a positive liquidity shock. Column 3 repeats the same regression as in column 3 of Table 6 to provide an idea of how much of the hedging large borrowers obtain is from existing banks.[17]

[17] Note that the column 1 and 2 coefficients can't simply be averaged to obtain the column 3 coefficient. This is both because the weights are not equal—they depend on the average share of the firm's borrowing from old and new banks (it is smaller for new), and because the log-log specification implies the coefficient is only an approximate weighted average (holds for small liquidity shocks).

In terms of the relative magnitude of hedging from existing and new banks, the OLS estimate for the bank lending channel for large borrowers is 0.30 percent (column 6 of Table 3). This suggests that large borrowers compensate half of their loan loss by going to (relatively more) liquid preexisting banks and the remaining half by borrowing from (more liquid) banks with whom they did not have a prior relationship.[18]

B. *Firm Financial Outcomes*

If the lending channel shocks affect the aggregate borrowing of small but not large firms, one might expect only the former to experience any real impact of bank liquidity shocks. However, even the smaller borrowers may not be adversely affected if they can compensate for the lower aggregate external borrowing by tapping into internal cash reserves or other forms of informal financing, such as trade credit and family loans. If these internal and informal means of financing are sufficient, then a reduction in external financing might have little or no impact on a firm's real outcomes.

While we do not have firm-level output data, we do observe whether a firm goes into financial distress (default). We can thus test whether bank liquidity shocks also translate into a real impact at the firm level. We run regression specification (6) with a firm's default rate as the LHS variable. Since cross-default clauses make it unlikely a firm can default on one bank but not the other, and the data show that this is indeed the case, default rate is aggregated at the firm level.

Column 1 in Table 8 shows that firms that, on average, experience a reduction in their banks' liquidity experience higher default rates. In particular, a 1 percent reduction in liquidity of a bank increases the probability of default of its firm by about 13.7 basis points. Given a mean post–nuclear test default rate of 6.9 percentage points, this is a 2 percent increase in probability. Recall that our identification strategy suggests that the increase in default rate of firms more exposed to a liquidity crunch cannot be attributed to unobserved negative productivity shocks experienced by such firms, and that, if anything, this bias leads to an underestimate.

As an additional check on identification, we can run a "falsification exercise." This exercise runs an analogous regression to those in Table 8 but changes the dependent variable to the default rate change only over the period *prior* to the nuclear test. If our results are spuriously generated by preexisting (time-invariant) bank attributes, we would see the same relationship even if we use pre–nuclear test changes in firm default rate. To make this test identical to that in Table 8, we condition on firms that were not in default at the end of 1996 and then see whether they enter into default by the end of 1997 (before the nuclear shock). The results (regressions not shown) bear out the falsification test. Not only do we not get the same result as in Table 8—that small firms borrowing from the affected banks face greater default rates—but the result shows the opposite: both small and large firms that were borrowing from banks that were *going* to experience a negative deposit shock in the future had a *lower* default rate growth. This is consistent with our previous claim that banks whose deposits were negatively affected by the nuclear tests had in

[18] We had argued above that firm size is the most important of firm attributes for hedging. An alternate check that political connections do not drive the hedging results is to confirm that hedgers don't obtain their compensatory loans primarily through government banks. This is because, as we show in earlier work (Khwaja and Mian 2005), political firms receive preferential treatment exclusively from government but not private banks. Running the specification in column 2 of Table 7, but excluding government banks, shows that the coefficient of interest (on change in log bank liquidity) hardly changes at all (it is –0.39), while it is an insignificant –0.02 when only considering new government banks. Thus, the increased loans obtained by the large firms facing bank lending shocks are provided through new relationships with private domestic and foreign banks, not via new relationships with government banks.

TABLE 8—FIRM BORROWING CHANNEL IMPACT ON FIRM FINANCIAL DISTRESS

Dependent variable	Δ Firm default rate				
	OLS (1)	IV (2)	OLS (3)	OLS (4)	OLS (5)
Δ Log bank liquidity	−13.71 (7.44)		2.01 (3.46)	−2.36 (3.07)	−4.84 (3.80)
Δ Log firm loan		−45.45 (12.45)			
Small firms			3.61 (1.18)	1.09 (0.90)	0.91 (0.91)
Δ Log bank liquidity × small firms			−18.50 (4.57)	−13.62 (3.99)	−11.50 (3.83)
Conglomerate firm?					−3.41 (0.56)
Δ Log bank liquidity × conglomerate firm					10.15 (2.21)
Political firm?					−1.16 (0.58)
Δ Log bank liquidity × political firm					−2.27 (1.44)
Multiple relationship firms					−1.42 (0.85)
Δ Log bank liquidity × multiple relationship firms					−0.11 (2.77)
Bank controls				Yes	Yes
Firm controls				Yes	Yes
Constant	8.30 (1.35)	5.14 (0.75)	5.41 (0.77)	—	—
Number of observations	18,647	18,647	18,647	18,647	18,647
R-squared	0.01		0.02	0.05	0.05

Notes: These regressions examine the impact of the liquidity shock on the firm's average default rate. All bank loans at a point in time (from any of the 145 lending institutions) for a given firm are aggregated at the firm level to compute firm default rate, loan size, etc. The liquidity shock experienced by a firm is the (loan-size) weighted liquidity shock experienced by the banks it was borrowing from prior to the shock (lending institutions that do not hold deposits are assigned a liquidity shock of zero). All quarterly data for a given firm are then collapsed to a single pre– and post–nuclear test period. The nuclear test occurred in the second quarter of 1998, so all observations from 1996:III to 1998:I for a given loan are time-averaged into one. Similarly, all observations from 1998:III to 2000:I are time-averaged into one. Data are restricted to loans that were not in default in the first quarter of 1998 (i.e., just before the nuclear tests). Bank-level controls include lagged change in bank liquidity, preshock bank ROA, log bank size, bank capitalization, fraction of portfolio in default, and dummies for foreign and government banks. Firm-level controls include dummies for each of the 134 cities/towns the firm is located in, 21 industry dummies, whether the firm is politically connected, its membership in a business conglomerate, and whether it borrows from multiple banks. Standard errors in parentheses are clustered at the bank level, i.e., the largest lender for a firm.

fact better quality clients/portfolios to begin with, and therefore our estimates are likely to be an underestimate of the true effect.

Thus, not only does a liquidity crunch reduce overall lending to firms, but it also makes it more likely for the affected firms to enter financial distress. This is particularly important since it suggests that firms cannot compensate their loss of formal credit through informal channels such as drawing on internal capital or borrowing from sister/family firms.

If higher default rates for firms borrowing from more credit-crunched banks are due to reduced loans to the firms, we should see the same relationship between change in default rates and change in a firm's loans. In general, change in loan supply is endogenous to changes in a firm's demand conditions. A potential instrument for change in a firm's loan supply is the liquidity of the firm's bank.[19] Column 2 instruments the change in a firm's loans by the change in its bank's liquidity and shows an even larger effect on a firm's default rate of a reduction in its loan supply.

Recall that column 2 in Table 6 showed that larger borrowers experienced little or no reduction in aggregate borrowing due to their ability to hedge the bank-specific shocks, while smaller borrowers were unable to do so. If default rates increase due to the credit constraints faced by firms, one would expect the impact on default rates to be higher for smaller borrowers. This is confirmed in column 3. The results show that large borrowers experience no significant increase in their default rates when borrowing from liquidity constrained banks. In sharp contrast, however, smaller borrowers are affected adversely by the shock and are significantly more likely to go into financial distress. A 1 percent decrease in their banks' liquidity leads to about a 16-basis-point increase in their probability of default. Column 4 includes the full set of bank- and firm-level controls, and results remain qualitatively unchanged. A nonparametric graphical examination in the time series (Figures 3A and 3B in the online Appendix) shows, furthermore, that the effect on smaller firms shows up only a few quarters after the shock. This suggests smaller firms are able to use internal/informal sources of credit to survive in the short run but cannot keep this up for long.

Column 5 shows that this result is not affected if we allow for heterogeneity across other firms' attributes. However, having connections to a large conglomerate has independent advantages of its own in terms of not suffering the negative consequences of bank liquidity shocks. Note that we cannot run IV in columns 3 and 4 because, as Table 6 showed, the first stage does not hold for large borrowers. These results suggest that the liquidity shock has real financial consequences that vary starkly across large and smaller borrowers—the former remain protected from the shock, while the latter face its full brunt.

V. Concluding Remarks

The aim of this study is to trace how supply side bank liquidity shocks get transmitted to the rest of the economy. Our firm fixed effects approach provides a new way of isolating the credit supply channel by absorbing any firm-specific credit demand shocks. Since the data used in this paper are potentially available in many other countries as well, our methodology can be applied more widely to understand the lending channel transmission mechanism.

While the bank lending channel identified in this paper is large regardless of the type of borrowing firm, large firms do obtain greater protection both in terms of how much of a shock is passed onto them by their initial lender, and how much they are able to hedge against this shock by borrowing from other (unaffected) banks. Large firms are therefore doubly protected, and it is the combined effect of the differential bank lending and differential firm borrowing effects that makes them immune to bank liquidity shocks. Although the precise factors that determine why large firms are able to hedge are harder to identify, one conjectures that the same underlying mechanisms that cause the bank lending channel effect to be smaller for large firms also enable them to borrow more from other banks. While these factors are likely to include a firm's business, banking, and political affiliations, no one such factor is sufficient by itself.

Since about 90 percent of lending goes to such large firms that can hedge completely, one could argue that bank liquidity shocks have no significant aggregate effects. However, given that almost 70 percent of firms (by number) cannot hedge the negative lending channel shocks and consequently get exposed to increased likelihood of financial distress, bank liquidity shocks

[19] The first stage for this instrument is given in Table 4. To the extent that bank's experiencing greater liquidity shocks affect their client firm's financial condition only through the amount lent, the exclusion restriction for the instrument would be satisfied.

have serious long-term distributional implications. These distributional changes are likely to last not only due to the persistence of the initial effect, but also as they get reinforced through the series of liquidity shocks that hit economies. Macro shocks may therefore contribute to the "missing middle" in firm size distribution one often documents in emerging markets.

The inability of small firms to hedge suggests that the fixed costs of forming new banking relationships might be an important constraint in financial markets. Certain firms might be able to use their size or corporate and political affiliations to "buy" their way into preferential banking relationships. While this insurance based on size and linkages may be specific to our environment, it is likely that some form of firm heterogeneity exists in other, particularly emerging, economy environments as well. Therefore, how the corporate sector responds and evolves in response to large financial shocks, and how these corporate structures in turn influence financial market reforms, offer fruitful areas of future research.

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