What's Bank Reputation Worth? The Effect of Fraud on Financial Contracts and Investment

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June 2011

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1 Introduction

What drives banks' incentives to monitor is a long-standing question in research and policy on financial intermediation. Assessing this question empirically gains importance as financial innovations —i.e., securitization, loan sales, loan syndication, CDSs— reduce banks' financial stake in the projects they oversee, potentially diminishing contractual incentives to monitor (see Keys, Mukherjee, Seru and Vig (2008), and Sufi and Mian (2009) for recent evidence). This paper explores the role of bank reputation as a non-contractual incentive mechanism to monitor.

The theoretical role of reputation in ameliorating asymmetric information problems in contexts where contracts are incomplete has been highlighted as early as in Friedman (1962), Akerlof (1970), and Fama (1980). In the banking context, a financial institution's future cash flows from origination, information collection, and underwriting depend crucially on investors' belief that banks, both, possess an effective monitoring technology and are willing to use it on their behalf. If limits to contracting are severe, banks will have an incentive to invest to prevent events that can be associated with incompetence or misbehavior as a delegated monitor. To date, however, there is limited systematic evidence on the degree to which reputation capital can ameliorate contracting problems in finance.

This paper provides evidence that bank reputation can substitute for formal contractual incentives to monitor in the context of the syndicated credit market. Syndicated lending, in which two or more lenders provide funds to a firm under a common loan contract, provides a unique setting for analyzing reputation incentives to monitor. In a syndicated loan, one or more *lead* banks are responsible for monitoring the borrower, while the remaining *participant* banks provide part of the funding of the loan. Lead banks collect up-front fees for their monitoring services and, thus, the amount of funding provided by the lead bank, i.e., the monitoring bank's *skin in the game*, is the sole source of contractual incentives to monitor in a single shot game. These two features simplify greatly the analysis of incentives and contracting relative to contexts in which the financial claims of the monitor and the principal

differ greatly, or where contracts are complex (i.e., debt versus equity holders, securitization). In addition, from a theory standpoint, unobservable monitoring, limited contractual incentives, and the repeated nature of the syndication process, give scope for reputation as a commitment device. From an econometric standpoint, syndicated loans allow implementing within-firm estimators discussed below.

We exploit large corporate frauds of U.S. firms as a source of variation in lender monitoring reputation. The fundamental hypothesis of the empirical analysis is that the failure to detect, or unwillingness to report, fraudulent activity constitutes a breach in the bank's agreement to monitor on behalf of investors. We show in a simple moral hazard framework that a breach by a lender results in higher-powered contractual incentives in equilibrium when reputation and contractual incentives are substitutes.[1] In the syndicated debt market, this implies that a negative reputation shock results in an *increase* in the supply of credit by banks in a monitoring role.

Testing empirically the predicted effect of a reputation loss on loan funding supply poses two identification problems. The first one is common to all supply side studies: how to disentangle changes in the supply of credit from changes in borrower creditworthiness. For example, suppose that J. P. Morgan Chase, one of Enron's main lenders, funds a larger fraction of its other borrowers' loans after Enron's fraud is discovered. This increase in the share of funding may result from a concurrent increase in the perceived risk of J. P. Morgan Chase's borrowers. This increase may be either caused by the fraud discovery, as investors' update their beliefs about the bank's ability to distinguish good from bad borrowers, or may be the cause of fraud discovery, as fraud is more likely to be uncovered when overall borrower creditworthiness declines. Thus, a positive correlation between lead arranger shares and reputation events, such as fraud discoveries or large defaults, does not have a causal interpretation.

[1] In this framework, reputation is an informal enforcement mechanism where a breach in the agreement (fraud discovery) is punished by reversion to the one-shot Nash equilibrium. Similar predicitons are delivered in an adverse selection framework, in which reputation is a probability distribution over possible bank types.

To address this problem, we adapt the within-firm estimator developed in Khwaja and Mian (2008) to the syndicated loan market in the U.S.. To measure changes in the supply of credit by banks whose reputation was potentially tarnished by fraud, we compare how their funding of syndicated loans changes after fraud discovery relative to the funding provided by other banks to the *same firm*. The within-firm estimator is consistent in the presence of changes in the extensive lending margin (composition of borrowing firms), and accounts for all observable and unobservable changes in firm characteristics in the intensive lending margin.

The second identification problem arises from the effect that fraud discovery may have on credit supply through mechanisms other than reputation. For example, fraud discovery is typically followed by default, bankruptcy, and litigation, all of which may deteriorate the lender's balance sheet and result in a credit supply contraction.[2] We isolate the effect of reputation by taking the within-firm difference between the supply change of monitoring and non-monitoring banks. Intuitively, the change in credit supply by monitoring banks confounds the reputation effect and the balance sheet effect, while any change by non-monitoring banks can be solely through the balance sheet effect. Thus, the difference between the two obtains the reputation effect on credit supply of monitoring banks while accounting for all time-varying firm characteristics.

The bulk of the analysis focuses on the discovery of the corporate frauds by Enron and WorldCom in September 2001 and June 2002. Each of these frauds led, at their time, to the largest corporate bankruptcies in history. Two stylized facts provide suggestive evidence that banks monitoring the fraudulent firms suffered reputation losses. First, banks actively involved in loan syndication and security underwriting for Enron and WorldCom were the targets of lawsuits for alleged accounting irregularities, breaches in fiduciary duties, or negligence in monitoring, all of which culminated in out of court settlements.[3] Second, the same

[2] See a theoretical discussion of this lending channel in Bernanke and Blinder (1988), Holmstrom and Tirole (1997), and Stein (1998).

[3] Citigroup, for example, reported $1.66 billion in payments and $4.25 billion in forgone claims agreed upon to settle Enron-related fraud litigation in 2008. See Bloomberg.com article by Scinta, C.: "Citigroup

banks suffered market value losses of a magnitude that is difficult to reconcile with the direct costs due to direct loan exposure or litigation costs.[4]

We find that after the fraud discoveries, banks actively lending to Enron and WorldCom during the year prior to the discovery increase the supply of funding to syndicated loans in which they perform monitoring roles. Monitoring banks' increase in skin in the game is substantial: funding increases by 40% on average during the two years following the fraud discovery. This result extrapolates to other time periods when we extend the analysis to include all large U.S. corporate frauds between 1995 and 2004, identified in Dyck, Morse, and Zingales (2010). This increase in funding supply is distinctive of a reputation loss, since fraud is expected to have the opposite effect on the supply of credit through other mechanisms.

We show that the results do not extrapolate to firm bankruptcies that were not accompanied by fraud discovery.[5] This indicates that firm bankruptcies do not affect bank reputation, suggesting that there is no implicit agreement between banks an investors that default will not occur. This is consistent with the theoretical insight, summarized in MacLeod (2007), that "reputation should be based on whether or not a party has breached an agreement and not upon quality per se."

Banks actively lending to the fraudulent firms also engage in other screening and monitoring activities, such as security underwriting. Although we cannot pin down whether the source of the reputation loss is exclusively due to monitoring in the syndicated market, we find that the effect of fraud discoveries is twice as large on banks that had monitoring roles in syndicated loans before the discoveries relative to those that did not. This indicates that the reputation loss due to a breach in the agreement to monitor in the context of syndicated

Settles Enron Litigation for $1.66 Billion", March 26 2008.

[4]For example, J. P. Morgan Chase and Citigroup, lead arrangers of the syndicated loans to Enron during the year before its fraud discovery, experienced negative market adjusted returns of 4% during the 20 days surrounding the announcement of the Enron's bankruptcy. The implied drop in market capitalization more than doubles the face value of the debt and post bankruptcy litigation settlement payments. In contrast, the valuation of banks with no lending relationship to Enron was unaffected.

[5]We test using the firms in bankruptcy sample in Jorion and Zhang (2009) that do not appear in the fraud sample in Dyck, Morse, and Zingales (2010).

lending is of first order importance in explaining our findings.

Fraud discovery has a substantial effect on the unconditional supply of credit of banks linked to fraudulent firms. The within-firm point estimates indicate that the supply of funds to new syndicated loans drops by at least 25% during the two years following the fraud discoveries. We also find suggestive evidence that the credit supply decline has real effects. Firms borrowing from banks that suffered a reputation loss face a 28% increase in borrowing costs and reduce investment by 20% after the fraud discoveries relative to the sample means, when compared to firms in the same industry, location, and size quintile, that borrowed from unaffected banks. The sizable magnitude of these effects is unlikely to be explained by the direct exposure of banks' balance sheets to default, as their total claims rarely represent more than a few basis points of the bank's total assets. The findings are suggestive that contractual and informal incentives are imperfect substitutes in the context of loan syndication: relying on contractual incentives alone may result in lower equilibrium lending and investment.

Prior work on the role of bank reputation on financial contracts explores the correlation between proxies for bank reputation, such as bank market share, experience, or past defaults, and the characteristics of financial contracts.[6] We show that this approach obtains biased point estimates and standard errors that may lead to incorrect statistical inference about the effect of bank reputation on the supply of credit. Therefore, our findings highlight that accounting for changes in unobserved firm characteristics is fundamental for the empirical analysis of the supply of credit and its consequences. The use of within-firm estimators that account for such variation has been limited to studies in countries with Credit Registries, such as Argentina, Pakistan, Peru, and Spain, due to their stringent data requirements (see, for example, Paravisini (2008), Schnabl (2010), and Jimenez et al. (2011)). The main methodological contribution of this paper is to illustrate how this class of estimators can be

[6] See for example Fang (2005) and the references therein for studies on the correlation of bank market share with the price and quality of underwriting services. Similar references related to Venture Capital reputation can be found in Hsu (2004). In the context of syndicated lending, Sufi (2007) shows a (negative) correlation between bank market share and lead bank shares, and Gopalan et al. (2009) shows past corporate defaults are correlated with less syndication and a larger share of loans funded by lead arrangers of the defaulting firm.

implemented using much broadly available syndicated loan data.

Our findings highlight a novel mechanism through which the banking sector can transmit and amplify real shocks. Negative shocks can break down reputation as a source of non-contractual incentives and lead to a decline in the supply of capital and investment. This represents a substantial departure from existing empirical work on the lending channel, which focuses on whether shocks to the financial sector affect the supply of credit but typically has not been able to identify the mechanisms behind such transmission.[7]

The rest of the paper proceeds as follows. Section 2 describes the empirical setting and provides a simple theoretical framework for the analysis. Section 3 describes the two estimation methods employed in the empirical analysis. Section 4 describes the data sources and provides summary statistics. Sections 5 and 6 present the within-firm and firm level results, respectively. Section 7 concludes.

2 Empirical Setting and Framework

This section provides a brief description of the syndicated loan market characteristics, emphasizing those that make it an interesting laboratory to study the role of reputation in financial contracts. Then it provides an account of the corporate fraud events and their potential effect on bank reputation. Finally it provides a simple theoretical framework that delivers the implications of a lender reputation loss on the supply of funding by monitoring banks in the syndicated credit market.

2.1 Syndicated Lending

Syndicated credit is a common contractual arrangement in corporate capital sourcing, accounting for over 50% of the corporate finance in the U.S. during our sample period (Weidner (2000)). In a syndicated loan, two or more banks agree to jointly make a loan to

[7] For recent U.S. evidence, see for example, Ashcraft (2005), Leary (2009), Loutskina and Strahan (2009), and Chava and Purnanandam (2010).

a borrower under common terms and conditions. Members of the syndicate fall into one of two groups. The *lead arrangers* of the syndicate are responsible for assessing borrower creditworthiness prior to issuing the loan and monitoring the firm after the loan has been issued. The lead arrangers also negotiate the terms of the loan agreement, and administer the documentation, funding, and repayments. Lead arrangers collect an up front fee for these services. *Participant* banks in the syndicate provide funding with little or no direct contact with the borrower. After negotiating contract terms with the borrower, lead arrangers prepare documentation that contains information about the borrower's repayment prospects. Participants use this documentation to make a decision of whether to provide funding to the syndicate under the stipulated contract characteristics, and in which amount.[8]

There are three key characteristics of the syndicated loan market that make it an ideal laboratory for studying the role of reputation incentives in bank monitoring decisions. First, syndication reduces the lead bank's expected loss in case of loan default, in a similar fashion as securitization and loan sales do. As a result, syndication reduces the monitoring incentives relative to standard single lender credit, a setting already ailed by contracting limitations. Second, lead and participant banks have identical claims on firm cash flows except for one dimension: their relative amounts of funding they provide (the fee for monitoring services is collected up front). This simplifies greatly the analysis of incentives and contracting between the informed and uninformed lenders relative to a setting in which informed and uninformed lenders have claims with varying information sensitivity (i.e., debt versus equity holders). In the theory framework and the empirical analysis we will focus on this observable contract characteristic, the amount of funding by the lead bank for a given loan size, to pin down predictions that are unique to the reputation mechanism. Third, syndicate members interact repeatedly to write one time contracts. The lack of long term agreements among lead arrangers and participants suggests that state-contingent outcomes are too complex to allow

[8]See Dennis and Mullineaux (2000), François and Missonier-Piera (2007), and Sufi (2007) for a more detailed description of the syndicated credit market. For theory on syndication, see Wilson (1968), and Pichler and Wilhelm (2001).

contracting over all outcomes at a reasonable cost (Hart and Moore (1988)). Contracting limits and repeated interactions allow reputation to play a role in the syndicated credit market.

2.2 Corporate Frauds and Lender Involvement[9]

Two of the largest corporate frauds and subsequent bankruptcies in corporate history of the U.S. occurred during the three quarters between October 2001 and June 2002. The magnitude and scope of the scandals led to the demise of Arthur Andersen, one of the *Big Five* accounting firms, and the passage of the Sarbanes-Oxley Act of 2002, a comprehensive corporate governance legislation labeled as "the most far-reaching reforms of American business practices since the time of Franklin D. Roosevelt" by then U.S. President G. W. Bush.[10]

Enron's and WorldCom's frauds entailed repeated accounting manipulations to cover liabilities, hide expenses, and create the appearance of profits and growth. Enron, for example, made use of mark-to-market accounting to overstate the value of long term contracts, and the use of special purpose entities to off-load losses and liabilities from its balance sheet (Healy and Palepu (2003)). WorldCom delayed the reporting of expenses and operating costs in excess of $9 billion by classifying them as long-term investments. In both cases, sustained poor operating performance and falling stock prices led their CEOs to step down and facilitated the discovery of accounting irregularities. The SEC investigations into the frauds resulted in criminal and civil charges against top executives of both companies.

Several of the largest financial institutions in the U.S. by asset size had direct monitoring responsibilities of the two companies during the months that preceded the fraud discoveries. Enron and WorldCom received two syndicated facilities each between the fourth quarter of 2000 and the third quarter of 2001 (the four quarters preceding Enron's bankruptcy). J.P.

[9] The information in this section comes from Enron and WorldCom regulatory filings to the SEC, bankruptcy filing documents, Enron Creditors Recovery Corporation, and press reports from *LexisNexis*.

[10] See press article: Bumiller, E., "Corporate Conduct: Bush Signs Bill Aimed at Fraud In Corporations", *The New York Times*, July 31 2002.

Morgan Chase and Citigroup were the lead arrangers in Enron's $2.15 billion facilities. J.P. Morgan Chase and Bank of America Corporation were the lead arrangers in WorldCom's $4.25 billion facilities (Enron's and WorldCom's facilities had 47 and 26 participant banks respectively). Aside from their roles in the syndicated loans, lead and participant banks often acted as underwriters of Enron's and/or WorldCom's debt or equity public offerings.

These banks' involvement in the accounting irregularities was highlighted ex post by the litigation brought against them by Enron and WorldCom investors. Enron Creditors Recovery Corporation (ECRC) filed complaints against 11 major banks and financial institutions for the "alleged involvement of those banks in the fraud, breaches of fiduciary duties, and civil conspiracy that created losses in the tens of billions of dollars."[11] Also, Citigroup and J.P. Morgan Chase were named, among others, as defendants in lawsuits related to alleged accounting irregularities in the books and records of WorldCom and the underwriting of its debt securities. In the latter, defendants were accused that they "either knew or were reckless or negligent in not knowing that the securities were sold to plaintiffs on the basis of misrepresentations and omissions of material facts concerning the financial condition of WorldCom."[12]

2.3 Framework: Implications of a Lender Reputation Loss

This subsection presents a brief discussion of the implications of a bank reputation loss on the funding supply of syndicated loans. We guide our discussion using a stylized model chosen solely to capture how the reputation loss affects the conditional supply of credit of monitoring banks, keeping constant borrower quality and loan size. We present a version of the reputation model based solely on moral hazard, and discuss the implications of a model based on adverse selection and other extensions.

[11] See description of the Megaclaims litigation at:
http://www.enron.com/index.php?option=com_content&task=view&id=10&Itemid=19

[12] See J.P. Morgan Chase Co. annual report to the SEC (10K) for period ending 12/31/2002 (page 21).

2.3.1 Simple Syndication Technology

Consider a lender that has access to an investment opportunity (loan) where a \$1 investment produces an payoff of R with probability p. With probability $1-p$, fraud occurs and the payoff is zero. The lender has a proprietary monitoring technology that allows it to increase p (reduce the fraud probability) at some cost $f(p)$, with f increasing and convex. The magnitude of p can be interpreted in reduced form as the bank's effort in screening loan applicants ex ante, or monitoring borrowers ex post.

The lender also has access to a syndication technology, that allows it to fund only an amount s of the investment (syndicate participants invest $1-s$).[13] The lender gains an amount $g(s)$ from syndication, where $g(1) = 0$, $g'(s) < 0$, and $g''(s) < 0$. This function captures in reduced form the diversification, regulatory arbitrage, or other benefits from syndication. In exchange for syndication the lead arranger charges an up-front fee T. This two-part tariff structure resembles closely the actual pricing of syndicated loans, as described in the previous section. Thus, the per-period profit function of the lead arranger is $T_t + s_t(p_t R - 1) - f(p_t) + g(s_t)$. For simplicity we assume that the lead arranger is a monopolist and charges the up-front fee that makes the participants break even: $T_t = (1-s_t)(p_t R - 1)$.[14]

2.3.2 Moral Hazard in Monitoring

Monitoring by the lead arranger is unobservable by the participants. Only whether fraud occurs or not can be contracted on. In the one-period game with asymmetric information, the syndication contract characteristics T and s are chosen first, and then the lead bank makes a monitoring choice to maximize per-period profits. Thus, the lead arranger chooses

[13] Since we have normalized the loan amount to \$1, s can be interpreted as the lead arranger's supply of funds or its share of the loan.

[14] The monopoly power may result if the bank is privately informed about the creditwirthiness of the firm, as in Rajan (1992). This assumption can be relaxed. Competition can, in theory, increase incentives to build a reputation. See, for example, Hörner (2002).

a p^o such that:[15]

$$f'(p^o) = sR \tag{1}$$

The amount of funding s is chosen to maximize per period profits of the lead arranger subject to the break-even constraint of the participants and the incentive compatibility constraint (1). The optimal amount of funding by the lead arranger, s^o, is given implicitly by:

$$\frac{g'(s^o)}{1-s^o} = -\frac{R^2}{f''(p^o)} \tag{2}$$

2.3.3 Reputation

The role for reputation arises in the repeated infinite-horizon game. We assume that the lead arranger and the participants are restricted to writing a sequence of one period contracts. This assumption seems restrictive given the simplicity of the payoffs, but reflects accurately the contracting environment in syndicated lending.

The following state-contingent strategies by the lead and the participants are an equilibrium that represents a Pareto improvement over the static equilibrium. The participants accept to pay a fee higher than the one implied by the one-period game at time 0 and in every period thereafter, as long as no fraud occurred in each preceding period. If fraud occurs, i.e., if the reputation of the lead arranger is tarnished, fees and funding amounts revert to the one-period equilibrium. For simplicity we consider only the case where this reversion lasts forever (in general, a finite reversion period will be optimal). The optimization problem in the repeated game setting is given by:

$$V_t = \max_{s,T} T + s(pR-1) - f(p) + g(s) + p\beta V_{t+1} + (1-p)\beta V^o$$

where V^o is the discounted present value of one-period game profits received in perpetuity. The lead arranger's monitoring level satisfies:

[15] The superindex o stands for "one-period".

$$f'(p^*) = sR + \beta (V_{t+1} - V^o) \tag{3}$$

The incentive compatibility constraint in the infinite-horizon setting (3) implies that for any given lead bank funding amount, the lead arranger chooses a higher level of monitoring than in the one-period game. The reason is that the lead arranger takes into consideration the value of the future profits that can be derived from preventing fraud, i.e., maintaining its reputation as a good monitor. In short, reputation allows the lead arranger to commit to a higher monitoring for any given stake in the loan. If fraud occurs and the reputation of the lead arranger is damaged, the level of monitoring for any given contractual incentives s reverts to the lower level implied by (1).

2.3.4 Reputation Loss and Lead Bank Credit Supply

Our relationship of interest is the effect of a reputation loss on the supply of funding by the lead bank. This relationship depends the functional form of (2).[16] Under the standard assumption that the cost of inducing monitoring effort is convex in contractual incentives, i.e., if the cost in units of s of inducing the same 1% increase in monitoring p is higher at higher levels of p, then the non-contractual incentives to monitor through reputation and the contractual incentives through lead bank's supply of funding are substitutes.[17] In other words, the infinite-horizon equilibrium given by (2) and (3) implies that, as long as the lead bank's reputation is intact, the equilibrium monitoring is higher and the lead bank's credit supply is lower than in the one-shot game: $s^o > s^*$. The reason is that reputation incentives induce higher monitoring levels for free, which increases the marginal cost of inducing additional monitoring through contractual incentives.

A reputation loss in this model implies reversion to the one shot game, which results in an increased supply of credit by the lead bank, i.e., larger lead arranger loan shares. This is

[16] The optimal amount of funding by the lead bank in the repeated game setting is also given by (2).
[17] This occurs when $f''' > 0$ in this setting.

the key prediction that we take to the data to identify the reputation channel.

2.3.5 Discussion

A similar set of predictions can be delivered through a model where lead banks are heterogeneous in the productivity of their monitoring technologies, $f'(p)$.[18] In such specification, participants use observed fraud realizations to update beliefs about bank monitoring productivity and the amount of funding by the lead arranger is declining the lead arranger's monitoring reputation —banks can fund a smaller fraction of the loan when they are perceived to have a high monitoring ability. This implies that after a fraud is observed, participants' priors about the monitoring ability of the lead arranger are revised downwards and the amount of funding of the lead bank increases. The two models have identical predictions regarding the supply of credit by lead arrangers.[19] We attempt to distinguish them empirically through their distinct predictions of a reputation loss on the secondary market prices of syndicated loans.

The framework is limited in several respects. First, we have assumed that loans have a fixed size. The introduction of variable loan size complicates the analysis because it requires considering how loan size affects monitoring costs and the repayment probability. Under the standard assumption that higher leverage amplifies moral hazard problems between the bank and the firm, then loan size will be smaller after a negative reputation shock.[20] This implies that a reputation loss may have a direct effect on the supply of credit. We will attempt to disentangle this direct effect in the results section. Importantly, the predicted increase in the supply of credit of the lead arranger *conditional* on loan size is unaltered in this model.

A second restriction of the simple framework is that it delivers predictions for a given

[18] See Fudenberg and Maskin (1982) and MacLeod (2007) for a theoretical discussion and Banerjee and Duflo (2000) for an application to the Indian custom software industry.

[19] The predicted level of effort in the two models may be different, since banks that suffer a reputation loss may exert more moniroting effort to re-establish their reputation.

[20] That is, the cost of monitoring is a function of loan size L: $f(p, L)$, and $f_L > 0$. The effect of loan size on monitoring incentives depends on the magnitude of f_{pL}. Thus, allowing loan size to vary may weaken or amplify the effect of the reputation loss on the funding supply by the lead arranger relative to the fixed loan size model, but the prediction on the sign of the change remains unaltered.

project profitability R. In an extension with borrower heterogeneity, the lowest profitability borrowers will not be financed after the reputation shock. The change in the borrower quality pool after the reputation shock poses a problem for the empirical estimation in general, because borrower quality is unobservable. However, the within-firm estimation discussed below fully accounts for this selection issue.

Third, we have ignored the incentive problem and reputation concerns of the participant banks regarding their own suppliers of capital. Participant banks must choose an unobservable screening effort when deciding whether to provide funds in a syndicated loan. It is possible that a bad outcome also affects the reputation of participant banks and the contracts between them and their capital suppliers. We explore this indirectly by looking at the effect of the Enron/WorldCom events on the supply of credit of participant banks.

3 Estimation

The key challenge in estimating the effect of a reputation shock on the conditional supply of credit by banks in a monitoring role is that the fraction of the loan financed by the lead arranger will also vary in response to changes in firm creditworthiness. If a borrower's quality drops after the fraud events, banks' incentives to monitor/screen the firm change and so do the incentives provided to the lead arranger in equilibrium. This can be easily seen in the context of our framework in the previous section from equation 3: a decline in the expected return R reduces the bank's marginal return on monitoring effort p, and thus affects the equilibrium lead bank's credit supply. Thus, it is essential to account for time series variation in firm credit quality in order to identify empirically the reputation effect on contractual incentives.

We adapt the within-firm estimator in Khwaja and Mian (2008) to account for this and other potential confounding effects related to time-varying firm shocks. Our estimator exploits the fact that syndicated loans have multiple lead arrangers as well as multiple

participants. Intuitively, we compare —for the same firm— the change in the relative amount of funding provided by lead and participating banks affected by the frauds to the same change in relative funding by banks unaffected by them. Our counterfactual, the change in the relative funding amounts by lead and participating banks unaffected by the reputation shock to the same firm, accounts for all time-varying firm characteristics.

3.1 Credit Supply by Monitoring Banks

We estimate the following within-firm difference-in-differences specification:

$$\bar{y}_{ijl}^{post} - \bar{y}_{ijl}^{pre} = \alpha_i + \beta_0 . ExposedBank_j + \beta_1 . LeadDebt_l + \beta_2 . ExposedBank_j . LeadDebt_l + \varepsilon_{ijl} \quad (4)$$

The variable y_{ijl} is (log) flow of new syndicated debt by bank type j ($j = 1$ for banks affected by the reputation shock, zero otherwise) to firm i. The subindex l captures the fact that banks can supply debt as a lead arranger or a participant in a syndicated loan ($l = 1$ if lead, zero otherwise).[21] The specification is defined in (log) loan levels in order to remain consistent with the *skin in the game* interpretation. As described in the theoretical framework, a bank's contractual monitoring incentives are related to the amount of exposure, which is given by the share of the loan times the loan size (and not the share of the loan only). In line with Khwaja and Mian (2008), we collapse the pre-fraud and post-fraud periods into one observation to reduce the bias introduced by serial correlation. Thus, the dependent variable is the change in the average flow of syndicated credit from bank j, with syndicate role l, to firm i, before and after the beginning of the fraud scandal wave in the third quarter of 2000. The pre-fraud period includes the eight calendar quarters before the Enron bankruptcy announcement. We use a 1-year and a 2-year post-fraud periods in the estimations to explore the dynamics of the shock on credit outcomes.

The first right-hand side variable is a firm fixed effect (FE). Including the FE in the first

[21] Notice that as a result of the disaggregation of loa flows by bank exposure and role in the syndicate there are four observations per firm in each time period.

differenced equation is equivalent to introducing firm-quarter dummies in a panel estimation. In other words, the firm FE accounts for all time varying determinants of debt flow levels. The first right-hand side variable of interest, $ExposedBank$, is a dummy equal to one for exposed banks (when $j = 1$). The coefficient on this variable represents the proportional change in debt flows by banks exposed to the fraud events relative to not-exposed banks, to the same firm. The coefficient on this variable, β_0, is the within-firm estimator of the effect of the shock on the supply of credit from Khwaja and Mian (2008).

Note that first-differencing also accounts for unobserved time-invariant heterogeneity across exposed and not-exposed banks, between lead and participant banks, and their interaction. Since there are two periods, pre and post fraud discovery wave, first-differencing is numerically equivalent to estimating the panel specification with bank type fixed effects, where the four bank types are defined by the interaction between the $ExposedBank_j$ and the $LeadDebt_l$ dummies.

The second right-hand side variable, $LeadDebt$, is a dummy equal to one for debt supplied by banks in a lead role in the syndicate. Its coefficient, β_1, represents the average change in lead debt flow relative to participant debt flow to the same firm before and after the fraud events. Our main variable of interest is the interaction between the exposed bank dummy and the lead debt dummy. The coefficient on this variable, β_3, reflects how the proportion of lead to participant debt flows changes differentially for banks affected by the corporate frauds relative to banks whose reputations were not exposed.

The coefficient β_3 in specification (4) is estimated only from variation in the intensive margin of lending. In other words, it is estimated only from firms that obtain credit before and after the fraud discovery from banks that were and were not exposed to the fraud. This implies that, although the estimators are consistently estimated, they do not necessarily extrapolate to the extensive lending margin, i.e., whether a bank continues to lend or not after the reputation shock. This external validity issue is a common limitation of within firm estimators (see discussion in Khwaja and Mian (2008), Paravisini (2008), and Schnabl

(2010)).

All the standard errors are estimated allowing for clustering at the 2-digit SIC industry level. Although this approach leads to very conservative standard errors, it is necessary since fraud events tend to be clustered in time and correlated with economic downturns of the sector the firm belongs to.

3.2 Interpretation

The coefficient β_2 in specification (4) is given by the difference between two separate within-firm estimations of the change in the supply of credit, one for lead arrangers and one for participants. Consider the following within-firm estimating equations for the supply of credit of lead and participant banks:

$$\bar{y}^{post}_{ij(l=1)} - \bar{y}^{pre}_{ij(l=1)} = \alpha'_i + \beta_0^{Lead}.ExposedBank_j + \varepsilon'_{ij} \tag{5}$$

$$\bar{y}^{post}_{ij(l=0)} - \bar{y}^{pre}_{ij(l=0)} = \alpha''_i + \beta_0^{Participant}.ExposedBank_j + \varepsilon''_{ij}. \tag{6}$$

The change in the supply of credit of lead arrangers, β_0^{Lead}, will confound two distinct effects: 1) the overall decline in the supply credit, and 2) the increase in the supply of credit as a lead arranger due to the reputation loss. On the other hand, the change in the supply of credit as a participant bank, $\beta_0^{Participant}$, is unaffected by the reputation loss and thus can be interpreted as the pure effect of the overall decline in the supply of credit. The coefficient β_2 in specification (4) is given by $\beta_0^{Lead} - \beta_0^{Participant}$ and, thus, can be interpreted as the change in the supply of credit of lead arrangers due to the reputation shock, after conditioning on the overall decline in the supply of credit. In the Appendix we discuss the results of estimating the parameters of specifications (5) and (6) separately.

3.3 Identifying Assumptions and Bias

Within-firm estimators are robust to firm selection issues because they are obtained from variation in the intensive lending margin. The estimate of the effect on the supply of credit, β_0, is obtained only from the subset of firms that receive syndicated credit from banks affected and unaffected by the reputation shocks before and after the scandals. The estimate for the effect on the conditional supply of credit by lead banks, β_2, is obtained from firms that also receive lead and participant debt from both types of banks before and after the shock. Thus, within-firm estimates are consistent even if a reputation loss induces banks to lend to different types of firms.

To understand the identification assumptions behind the within-firm estimators it is useful to spell out under which circumstances they are violated. It is easier to begin with β_0, the estimate for the effect on the supply of credit. This coefficient is negative if the reputation shock induces banks to reduce their credit supply. The magnitude of this coefficient represents the proportional credit supply reduction, as long as banks whose reputation remains intact do not increase their credit supply in response. If non-affected banks supply more funding to the syndicate in response, β_0 will be biased upwards. This can occur in practice because the negative credit supply shock by affected banks induces the firm to demand more credit from other banks. This upward bias is, however, bounded. In the extreme case where non-affected banks fully substitute the lending reduction by affected banks, the estimated parameter β_0 will be twice the true effect of the reputation shock on the supply of credit. We take this effect into consideration when interpreting the results and explore directly whether substitution occurs using an alternate identification strategy, discussed in the next subsection.

The estimate of the reputation shock on the conditional supply of credit of lead banks, β_2, will be biased if the change in funding by lead banks affected by the reputation shock affects, in turn, the funding amount by lead banks with untarnished reputations. This can occur if there are strategic complementarities in the monitoring activities of affected and

unaffected lead arrangers. This bias is, however, of second order relative to the direct effect of the reputation shock. The reason is that the decline in the affected banks' monitoring level, which causes the change in unaffected bank credit supply, is attenuated by the increase in contractual incentives.[22]

4 Data and Variable Definitions

4.1 DealScan

The DealScan database is collected by Reuters/Loan Pricing Corporation from SEC and Federal Reserve filings, and directly from private debt markets. The initial sample contains information on 69,055 loan facilities (78% syndicated) issued by 5,868 different lenders to U.S. firms from 1990 to 2005. In theory, it is straightforward to obtain the loan amounts outstanding using information on the facility initiation date, the amount of each facility, the shares of each lender in the facility. In practice, however, the data on the lender shares is incomplete or missing in 69% of the facilities. We exclude from the analysis sample all facilities that are missing all information on lender shares (44%). Because the within-firm estimates compare syndicated loans to the same firm before and after the fraud events, they are internally consistent in the presence of potential selection issues that may arise due to missing data. However, syndicates with complete share information are on average larger, both in loan amount and number of participants, than those with missing information.[23] Thus, our analysis sample is not representative of the universe of syndicated loans.

We impute lender shares for the facilities where shares are incomplete to increase the

[22] Existing research suggests that lead bank activities are strategic complements, since lead banks have specialized roles within the syndicate (François and Missonier-Piera (2007)). This implies that the within-firm estimate of β_2 will be biased toward zero. Intuitively, the bank that suffers the reputation shock monitors less in equilibrium, which reduces the incentives of the unaffected bank to monitor, which in turn leads to larger funding amounts by unaffected lead banks.

[23] The median syndicated facility with complete (incomplete) share information has 7 (3) banks and an amount of $150 million ($70 million).

sample size, although our results are robust to this imputation.[24] After the imputation 56% of facilities have complete lender share information. Using the sample with complete information we construct a database including lender shares, DealScan lender ID, DealScan borrower firm ID, and other facility information.

4.2 Definition of Bank Exposure and Summary Statistics

Table I shows the list of the top 40 syndicated lenders by number of facilities during the four quarters before the Enron Bankruptcy. The table shows the fraction of syndicated lending to Enron, WorldCom, and other firms involved in corporate fraud scandals. The table shows a substantial overlap in the set of banks that had a lending relationship with Enron, WorldCom and the other fraudulent firms. For this reason we cannot exploit the variation induced by each fraud independently. We define a lender as exposed to a fraud event if it participated in loan facilities to Enron (49 banks) or WorldCom (28 banks) during the four quarters prior to Enron's bankruptcy. The magnitude and statistical significance of the results is robust to an exposure definition that also includes lending relationships to Adelphia Communications, Global Crossing, KMART, and Qwest Communications International, whose frauds were uncovered during the same period.

We define bank exposure using lending relationships established before the beginning of the corporate fraud wave because the likelihood of lending relationships will be endogenously affected by fraud discovery. In some specifications, we define a lender as exposed if it is the *lead* arranger to Enron or WorldCom during the same period. Note from Table I that exposed

[24] The imputation proceeded in the following five steps. 1) Facilities with incomplete lead bank shares (0.08%): we assign the median value of available lead bank shares to each lead bank without a share in the same facility. At the end of this step, the information of the shares of lead banks is either all complete or all missing. 2) Facilities with all missing lead bank shares, but with some participant bank shares (0.26%): we first assign the median value of available participant bank shares to each participant bank without a share. The unassigned share we distribute evenly across the lead banks. 3) Facilities with complete lead bank shares but all or some missing participant bank shares (1.47%): assign the remaining share equally amongst all participant banks without a share. 4) Facilities without lead banks and the lender shares are all or partially missing (23.31%): assign the remaining share equally among all banks without a share. 5) Facilities with all lead bank shares missing but all participant bank shares complete (0.28%): assign the remaining share equally among all lead banks.

banks are the largest players in the syndicated market both by number of facilities and by volume of lending. The fraction of the syndicated lending flow allocated to the fraudulent firms, 0.0092, is small on average. Among the exposed banks, this fraction is relatively smaller for the largest banks in the sample and the lead banks.

We hand-match the lender names in DealScan with the lender names in National Information Center, a repository of financial data and institution characteristics collected by the Federal Reserve System. We obtain the RSSN ID from this site which is then used to match DealScan with Call Report data to obtain lender financial statements. Among the 5,868 lenders in the full DealScan sample, 193 banks are identified to have unique RSSN ID and appear in the Call Report in the third quarter of 2001. These 193 banks are then collapsed at the parent bank level to have 100 unique RSSN ID for their parent banks. We finally hand-match these 100 parent banks to CRSP, to obtain a subsample of sixty-seven public banks.[25]

Figure 1 plots the cumulative value-weighted returns of the portfolios of banks classified as exposed and not-exposed to Enron and WorldCom during the 20 trading days surrounding each company's bankruptcy announcement. Panel A shows that banks classified as exposed to Enron experience on average a 2% decline in returns relative to the not-exposed banks around Enron's bankruptcy. The decline in returns is around 5% for banks exposed as lead arrangers to Enron. Similar patterns are shown in Panel B around the WorldCom bankruptcy, although the valuation effects are twice as large. These findings are consistent with those in Jorion and Zhang (2009), who show that bankruptcy announcements cause negative abnormal equity returns and increases in CDS spreads for creditors. These plots also indicate that our exposure classification provides a meaningful proxy for the vulnerability of bank returns to shocks to Enron and WorldCom. The magnitude of the estimated effect is difficult to reconcile with the size of the direct exposure of these banks to Enron and WorldCom through syndicated lending shown in Table I.

[25]Banks that merged during our sample period are considered as the same bank before the merger.

We limit the firm population to firms that had at least one loan facility reported in DealScan between the fourth quarter of 2000 and the first quarter of 2002 and that are not in financial or utility industries. In some specifications we will also exclude firms in the telecommunications, energy, electrical equipment, and software industries, all potentially directly related to Enron, WorldCom, and the other fraudulent firms through commercial links. Table II presents the descriptive statistics of the structure of the syndicated loans to our final sample of firms during the four quarters before the Enron bankruptcy. The fraction of total syndicated credit financed by banks in the lead role is 29.7% (panel 1). In contrast, the unweighted average fraction of lead arranger participation over the full firm sample is 70.6% (panel 2). This is in line with existing evidence on the syndicated loan market that documents a larger lead bank share in loans to smaller firms. Also, the lead bank share in the subsample of syndicated debt by exposed banks is 37.7%, reflecting the fact that exposed banks in our sample lend to larger firms on average.

Some specifications rely on classifying firms according to their exposure through their main lenders. We define a firm to be exposed to the corporate frauds if it had at least one loan facility between the fourth quarter of 2000 and the first quarter of 2002 in which at least one participant in the syndicate is exposed to Enron or WorldCom, and zero otherwise.[26]

To obtain firm financial statement data we hand-match the firm names in DealScan with firm names in COMPUSTAT - North America. Among firms that had at least one loan facility between the fourth quarter of 2000 and the first quarter of 2002, 1,368 are private firms and 1,358 are publicly-traded firms with unique GVKEY in COMPUSTAT. After excluding private firms and firms in the mentioned industries, our final firm sample includes 1,193 public firms. Table III presents the firm descriptive statistics during the four quarters before the Enron bankruptcy. Firms classified as exposed (587) are larger on average than non-exposed firms, as measured by market capitalization. This is expected since exposed banks are larger and tend to lend to larger firms. The amount of syndicated credit scaled

[26] In unreported robustness checks we use an alternate definition, in which a firm is classified as exposed if at least one lead arranger of the facility was exposed to Enron (WorldCom). All the results are unchanged.

by firm assets, however, is the same on average among exposed and not-exposed firms, and so is leverage. We show evidence below that our specifications account for observed and unobserved differences across exposed and not-exposed firms that are related to syndicated lending and other outcomes.

5 Empirical Results

5.1 Unconditional Evidence

Figure 2 plots the proportion of the loan amount funded by lead arrangers, separately for exposed and not-exposed lead banks (pre-Enron means removed to facilitate comparison). The two vertical lines mark the beginning and end of the corporate fraud wave. Before the frauds, the fraction of loan funding provided by exposed and not-exposed monitoring banks evolves in parallel. After the fraud wave, exposed banks fund a larger fraction of loans relative to not-exposed banks. The increase is substantial: the fraction of the loans funded by exposed lead arrangers is 10 percentage points higher a year after the fraud wave.

Figure 3 plots the time series of the unconditional flow of new syndicated credit by exposed and not-exposed banks during the same period. Panel A plots total funding, and panels B and C plot separately funding as lead and as a participant in a syndicate. The figures show that the syndicated debt flow of exposed and not-exposed banks move together until the third quarter of 2001, when the debt of exposed banks declines relative to not-exposed banks. The lending decline occurs regardless of the role of the bank in the syndicate and also appears to be substantial: exposed bank syndicated debt grows on average at a 40% to 50% slower rate than not-exposed bank debt during the two years following the Enron scandal.

Figures 2 and 3 suggest that exposed banks supply less syndicated credit overall, and in particular supply less credit as lead arrangers after the fraud events. But conditioning on loan size, lead arrangers fund a larger fraction of loans. These stylized facts are consistent with

the predicted effect of a lender reputation loss when reputation and contractual incentives are substitutes. As previously discussed, these patterns may result from unobservable changes in firm creditworthiness. We now confirm that these patterns hold after fully accounting for unobservable changes in borrower characteristics.

5.2 Reputation and Contractual Incentives

Table IV presents the estimated coefficients of specification (4), which measures the effect of the Enron/WorldCom events on the conditional and unconditional supplies of credit by banks in a monitoring role. The first coefficient, on the *ExposedBank* dummy is negative and significant across all specifications. The magnitude, -0.35, implies that the proportion of exposed bank debt to not-exposed bank debt in new syndicated lending decreased by 35% during the year after the Enron fraud (Table IV, column 1). The magnitude of the decline is 48% when the post period is expanded to two years after the Enron fraud (Table IV, column 3). These estimates imply that the supply of credit by banks lending to Enron and WorldCom declined after the fraud discoveries. The magnitude of the estimates is consistent with the observed patterns in Figure 3, and is robust to excluding from the sample firms in the energy, electrical equipment, and software industries, which may have commercial ties to Enron and WorldCom (Table IV, panel 2). Although debt substitution biases upwards the within-firm estimates, in the most conservative scenario of full substitution our estimates imply that the flow of new syndicated credit declines by 24%.[27]

The estimated coefficient on the interaction term, $ExposedBank \times LeadDebt$, is positive and of the same magnitude in all specifications and subsamples. The estimate is significant in all subsamples when estimated using the two-year post-Enron estimation period. The sign of the estimate implies that, after conditioning on the overall change in credit supply, banks with a lending relationship with Enron/WorldCom increased the supply of credit when acting as lead arrangers. In other words, banks with a monitoring role must fund a larger

[27] In the worst case scenario where the estimates are two times the real decline in lending. This effect is statistically significant even without adjusting the standard errors to the smaller coefficient size.

amount of any given syndicated loan after their reputation is tarnished by fraud discoveries.

The estimated magnitude of the coefficient indicates that banks with monitoring roles increase their supply of funding by 33%. This implies an increase in the fraction of the loan funded by lead arrangers of 10 percentage points relative to the pre-fraud mean of 30.1%. This represents a substantial increase in the syndicate composition and the contractual incentives to monitor. The sign and magnitude of the coefficient are consistent with the hypothesis that non-contractual incentives provided through reputation and contractual incentives provided through larger stakes in the loan are substitutes in the context of syndicated lending.

Section 3 highlighted that specification (4) fully controls for time invariant characteristics of the four types of lenders in our estimation (given by the combinations of $ExposedBank$ and $LeadDebt$). It is possible, however, that lending by different types of banks evolved differently during our analysis period. This is plausible since banks exposed to the fraudulent firms are the largest in the financial system, and these banks are potentially more exposed to systematic shocks. The key for our interpretation of the results is that shocks that occur concurrently with the fraud discoveries that can explain the unconditional decline in the supply of credit, are difficult to reconcile with the increase in the conditional supply of credit by lead arrangers.

For example, consider the possibility that events that occurred concurrently with the corporate fraud wave (i.e., 2001 recession, general decline in the telecommunications industry, attack on the World Trade Center, Argentine default) affected disproportionately the cost of capital of large banks in the U.S., leading to a decline in their supply of credit. This can partially explain the negative coefficient obtained on the $ExposedBank$ variable, but cannot explain why the same banks' supply of credit increased conditioning on their monitoring role in a syndicate.[28]

[28] In general, a credit supply reduction that is due to an increase in the banks' cost of capital is not expected to have a differential effect on the credit supplies of lead arrangers and participants, holding firm characterstics constant. In our simple model of Subection 2.3, a change in the cost of financing affects only the decision of whether to finance the project or not, and thus funding by lead banks for a given project

To corroborate this, we augment specification (4) with a control variable for the size of the lenders of each firm i with exposure status j and role l in the syndicate. We use the (log) total amount of loans in the pre period as a proxy for size. The coefficient on this variable accounts for differential changes in lending by each type of bank depending on their initial size. In the example above, we expect the coefficient on this variable to be negative if larger banks were disproportionately affected by a recession in 2001.

Columns 2 and 4 of Table IV shows the results of the augmented specification. The coefficient on the bank size variable is negative, indicating that lending by large banks dropped more than for small banks during our sample period. All the point estimates of the coefficient on the interaction term, $ExposedBank \times LeadDebt$, are positive, significant, and of the same magnitude after including the lender size control. This corroborates that the increase in the conditional supply of credit of banks in lead arranger roles is unrelated to size. This compositional effect on the supply of credit is consistent with the reputation account and difficult to reconcile with other potential sources of variation in the supply of credit.

The magnitude of the parameter on $ExposedBank$ is smaller in the specification augmented with bank size, suggesting that indeed the estimated decline in the supply of credit is partially explained by the fact that lending declined more for large than for small banks during the analysis period. However, the coefficient is still negative and significant, suggesting that exposure to the fraudulent firms explains the decline in the supply of credit above and beyond lender size.

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are unaffected. In the extension with heterogeneous projects, an increase in the cost of financing implies that the lowest profitability projects are not funded, but does not change the lead arranger's funding for a given project profitability. In the extension with project scale, a financing cost increase will lead to a smaller loans for any given firm, which potentially lowers moral hazard and monitoring costs and leads to an proportionally larger decline in funding by lead arrangers.

5.3 Type of Exposure

We investigate the heterogeneity of the effect of the reputation shock across banks that had a direct monitoring role as lead arrangers to Enron and WorldCom and those with participant roles in the syndicated loans before the fraud discoveries. To do so we repeat the estimation of specification (4) introducing a separate indicator for banks that were exposed as lead arrangers and as participants in syndicated loans before Enron events. Note that bank type j had two potential values per firm i (exposed and not-exposed) in specification (4), while it has three potential values (exposed as lead, exposed as participant, and not-exposed) in the new specification. The point estimates are presented in Table V.

The point estimates indicate that the negative reputation shock had a larger negative effect on the supply of credit of banks with a lead role in syndicates prior to the frauds. However, the difference is not statistically significant at the standard levels. The estimates of the interaction coefficients indicate that only banks exposed as lead arrangers experience a statistically significant increase in contractual incentives through larger lead shares after the fraud events. The point estimates suggest that the increase in contractual incentives is roughly twice that experienced by banks exposed as participants. Although this relative magnitude of the effect is robust across all specifications, the difference is again not statistically significant. Thus, the cross sectional result are at most suggestive that the reputation shock had a larger effect on the credit supply and lead shares of banks with a substantial monitoring role in syndicated lending before the fraud events.

The fraud discoveries may have affected the reputation of banks with non-monitoring roles in loan syndicates through two different mechanisms. The first one, discussed in the theoretical framework, is that participating banks in a syndicate implicitly agree to monitor lead arrangers. Failing to detect that lead arrangers are not performing due diligence constitutes a breach vis-a-vis their own investors and leads to a reputation loss. Alternatively, participant banks may have suffered a reputation loss due to monitoring activities unrelated to loan syndication. As discussed in the previous section, banks lending to Enron and World-

Com with non-monitoring roles in syndicates also engaged in underwriting and investment banking activities with these firms, and were targets of lawsuits for breaches in fiduciary duties and improper monitoring. Although we cannot empirically distinguish between the two mechanisms, either interpretation suggest that implicit non-contractual incentives play a role that is broader than the one narrowly defined by the monitoring relationship between lead arrangers and borrowers in syndicated lending.

5.4 Bias Assessment

Prior research has documented that not accounting for unobserved changes in firm creditworthiness biases the estimation of changes in the supply of credit towards zero in Emerging Market contexts (Khwaja and Mian (2008), Paravisini (2008), Schnabl (2010)). To investigate the direction and magnitude of the estimation bias in our context, we estimate the panel version of specification (4), including firm and time dummies without first differencing. The right hand side variables in this panel specification include the same variables as specification (4) and their interaction with an indicator variable for the post-fraud discovery period, $Post_t$.

The coefficients on these interactions represent the measured change in lending by exposed, lead banks, and exposed lead banks without controlling for time-varying unobserved firm heterogeneity, and are reported in Table VI. Both the estimated effect on the overall supply of credit and on the conditional supply of credit by lead banks are biased towards zero, consistent with prior research.

In addition, the standard error estimates are consistently biased downwards. The biased standard errors can be between 20% and 40% lower than the unbiased estimates. This downward bias will lead to accept too often the alternative hypothesis that the coefficient is different from zero. The combination of the two biases implies that it is difficult to predict ex ante whether unaccounted for time-varying borrower heterogeneity will lead to commit more Type I or Type II inference errors.

5.5 External Validity: Other Frauds

The increase in the supply of credit by monitoring banks uniquely identifies the reputation channel. However, we cannot interpret the generalized decline in the supply of credit documented above to be the result of the fraud discoveries, because there are other macroeconomic factors that occur concurrently with the 2001 fraud wave that may explain it. In this subsection we verify how the results extrapolate to other fraud discoveries and other time periods by extending the analysis to all corporate frauds identified in Dyck, Morse, and Zingales (2010).

The original sample in Dyck, Morse, and Zingales (2010) includes the 216 corporate frauds that took place in U.S. companies with more than $750 million in assets between 1996 and 2004. The frauds generate potential variation in bank reputation that is scattered over time and heterogeneous across banks. Appendix Table A2 shows the distribution of frauds by year. Almost 60% of the reported frauds occur before 2000, and there are very few frauds after 2002. We match 158 of these firms to 267 banks that participated in syndicated loans during the 4 quarters prior to the borrowers' alleged frauds. Among these banks, 55 banks acted as lead arrangers to the fraudulent firms. Appendix Table A3 reports a summary of banks' loan syndication activities and exposure to fraudulent firms, defined as before, by the top 40 banks ranked by number of syndicates during the 1996 to 2004 period. The top 40 banks are exposed on average to 11 frauds during the sample period, and the median bank of the entire DealScan population is exposed to at least one fraud.

We include in our analysis sample of 6,888 firms those that received at least: 1) one syndicated loan during the year prior to a fraud discovery, and 2) one syndicated loan during the two years following a fraud discovery. We then construct a database suitable for estimating the following specification (analog to (4)):

$$y_{ijlt} = \alpha_{it} + \beta_0.ExposedBank_{jt} + \beta_1.LeadDebt_{lt} + \beta_2.ExposedBank_{jt}.LeadDebt_{lt} + \varepsilon_{ijlt} \quad (7)$$

The left hand-side variable is the (log) amount of the syndicated loan, received by firm i during quarter t, that is funded by bank j ($j = 1$ for banks affected by the reputation shock) with role in the syndicate l ($l = 1$ if lead arranger). We cannot collapse the specification into pre and post periods as in specification (4) because now bank exposure status is changing over time. Thus, to account for all firm level variation in creditworthiness, the specification includes a dummy for each firm-quarter pair. The explanatory variables of interest are a dummy that turns to one if the funding at time t is provided by bank during the two years after it becoming exposed to a fraud event ($ExposedBank_{jt}$), and its interaction with a dummy equal to one if the funding is provided by the lead bank ($ExposedBank_{jt}.LeadDebt_{lt}$). The interpretation of the coefficients is analogous to those in the previous subsection. Estimating the specification without collapsing comes at the cost of potentially downward biased standard errors, which leads to wrong inferences on the statistical significance of the coefficients (Bertrand, Duflo, and Mullainathan (2004)). For this reason, we use this specification solely to corroborate whether the sign and magnitude of the estimates is in line with those obtained in Table IV.

The estimation results over the sample period between 1995 and 2004 are presented in Table VII, both with and without introducing a set of lender size controls, one for each quarter (columns 1 and 2). All the point estimates corroborate our previous findings: fraud discovery causes a decline in the overall supply of syndicated credit, but induces banks in a monitoring role to increase their supply of funding of syndicated loans. These results are unique to the reputation channel. To confirm that the estimates are not dependent on the sample period, we repeat the estimation interacting the right hand side variables with a dummy that turns to one before 2000 (Table VII, columns 3 and 4). The point estimates on this interaction indicate that the effect of fraud discovery on the overall supply of credit is larger during the earlier sample period. The reputation effect on the funding supply by monitoring banks is statistically indistinguishable between the two sample periods.

The findings confirm that the observed decline in the overall supply of credit documented

around the 2001 fraud wave is not due to the confounding effect of macroeconomic shocks affecting bank balance sheets. It is thus likely that the reputation mechanism amplified the effect of fraud discoveries on the supply of credit. Still, fraud discovery can also break down reciprocal agreements between lead arrangers and participants (Cai (2009)), or allow banks to learn about weaknesses in their monitoring processes (Murfin (2010)), leading to lower overall lending. Thus, we cannot further distinguish through which specific channel the fraud discoveries affect credit supply in our empirical context.

5.6 Fraud or Bankruptcy?

Since fraud discovery is typically followed by default and bankruptcy, the results so far do not allow to ascertain whether it is fraud discovery or default and subsequent bankruptcy that cause a reputation loss. We explore this issue by exploring how bank reputation is affected by firm bankruptcies that did not occur concurrently with fraud discoveries.

We use information from the website www.bankruptcydata.com to replicate the sample 721 of Chapter 11 bankruptcies that occurred between January 1999 and December 2005 in Jorion and Zhang (2009). To identify bankruptcies that did not occur concurrently with a fraud discovery, we exclude from this sample the firms that also appear in the fraud sample in Dyck, Morse, and Zingales (2010). We match bankruptcies to lenders using the same procedure above: a bank is considered exposed to a bankruptcy if it participated in syndicated loans to the bankrupt firm during the four quarters prior to the bankruptcy. Then we estimate specification (7) where the explanatory variable of interest, $ExposedBank_{jt}$, is a dummy that turns to one if the funding at time t is provided by bank during the two years after it becoming exposed to a bankruptcy event.

The estimated parameters on $ExposedBank_{jt}$ and $ExposedBank_{jt}.LeadDebt_{lt}$, shown in Table VIII, are both negative although not always significant at the standard levels. These estimates imply that bankruptcies that are not accompanied by default reduce credit supply, and that the reduction in credit supply is larger for banks in monitoring roles, respectively.

The coefficient on the interaction term has the opposite sign to that obtained in the fraud specifications above. This indicates that bankruptcies do not affect bank reputation unless they are accompanied by fraud. This conclusion is consistent with the notion that banks cannot implicitly commit to zero defaults and starkly contrasts the one reached when unobserved time series variation in firm creditworthiness is not accounted for (see, for example, Gopalan et al. (2009)). This contrast with prior work implies that defaults occur at times when the perceived borrower riskiness increases, and that a positive correlation between lead arranger shares and defaults cannot be interpreted causally.

6 Firm Level Results

In this section we explore the consequences of the reputation shock for cost of debt, secondary loan prices, and firm leverage and investment. Since within-firm estimates cannot be obtained for firm level outcomes, we account for variation of investment opportunities by comparing firms in the same industry, location, and size quintile, but that differ in the identity of their main lender. The identification strategy relies on the assumption that firm specific changes in investment opportunities are, on average, unrelated to the identity of their main lender after controlling for industry, location, and size specific shocks. We validate this assumption below by showing that outcomes of the exposed and not-exposed firms evolve in parallel prior to the fraud events.

We estimate the following firm-level first differenced specification:

$$\bar{y}_i^{post} - \bar{y}_i^{pre} = \gamma.ExposedFirm_i + \delta_i^{SIC2} + \delta_i^{state} + \delta_i^{sizeQ} + \omega_{it} \tag{8}$$

The dependent variable is the change in an outcome for firm i, before and after the quarter of Enron's fraud discovery (third quarter of 2000). The right-hand side variable of interest is a dummy equal to one if the firm is classified as exposed to the shock, and is coefficient represents the change in outcomes of the exposed firms relative to the not-exposed firms,

our difference-in-differences estimate of the effect of the shock. Since the equation is first differenced, it accounts for all unobservable firm heterogeneity (time invariant). To account for credit demand shocks we include a full set of industry, state of incorporation, and size quartile dummies. These account for average outcome changes that are common across all firms in the same industry, location, and size, that occur before and after the Enron events.

6.1 Validation of Identification Assumptions

Table III showed that exposed firms are larger and pay lower syndicated loan spreads than not-exposed firms. This implies that the two groups of firms are likely to differ along other unobservable dimensions. If these unobservable firm characteristics vary over time and are related to the demand for credit and investment, the identification assumptions of this difference-in-differences estimation are violated. However, if these unobserved characteristics are time-invariant or are balanced across the two groups, specification (8) provides an unbiased estimate of the effect on firm outcomes.

To verify whether the identification assumptions are likely to hold unconditionally, we plot in Figure 4, panels A, the time series of the average spread (relative to LIBOR) of new syndicated loans to exposed and not-exposed firms. The pre-Enron means have been removed to ease the comparison. The spreads paid by exposed and not-exposed firms evolve in parallel before the Enron events, which provides validation to the difference-in-differences identification assumptions.

6.2 Loan Prices

We test the effect of the fraud discoveries on loan prices by estimating the parameters of specification (8) using loan spreads as the dependent variable (Table IX, columns 1 and 2). The estimated effect over the two-year post Enron period indicate that loan spreads increase by 45 basis points on average after the Enron/WorldCom events, or 28.1% of the pre-Enron sample average. The overall results indicate that firms whose main lenders were exposed to

the fraud events experience an increase in their external financing costs.

Since some syndicated loans are traded in the secondary market, we attempt to estimate the effect of the reputation shock on the market prices of the loans after they are issued. Secondary market prices allow us, in principle, to distinguish the economic mechanism behind the reputation shock. On the one hand, in the pure moral hazard model of Section 2.3, fraud discovery triggers a reversion to the one shot game contract, but does not affect the assessed quality of the loans issued prior to the fraud discovery. On the other hand, if fraud discovery reflects poorly on the monitoring ability of exposed banks, it will lower the perceived quality of loans issued by exposed banks prior to the fraud discovery, which should be reflected as a decline in secondary market prices.

We obtain secondary market loan price quotes from the Loan Syndications and Trading Association (LSTA) Mark-to-Market Pricing service. The unit of observation in the LSTA database is a pair between a loan facility and a quotation date. For each observation, it provides information on quote date, a loan identification number that uniquely identifies a loan facility and the borrower, number of quotes, average of the bid (ask) quotes, and the average of average bid (ask) quotes. A noteworthy caveat is that the LSTA database provides loan quotations rather than actual transaction prices. Thus, the estimated effects must be interpreted as changes in the willingness to pay for the listed loans. Also, the database reveals the facility ID and/or LIN rather than the identity of the loan sellers and buyers. This implies that we cannot distinguish which part of the syndicated loan the quote applies to, and we must perform the secondary market price analysis at the firm level.[29]

Panel B of Figure 4 plots the time series of the median bid price for all of the firm loans, measured as percentage points of par, averaged across firms exposed and not-exposed to the fraud events through their lenders (pre-Enron means removed). The plot suggests that the median quotes for exposed firms declined by 2 to 3 percentage points after the Enron

[29] Out of 30,738 unique facilities in DealScan that were originated during 1999-2004 to U.S. firms, 3,033 facilities are traded during 1999-2004 and matched to LSTA database using one of the two common fields: facility ID and/or LIN. We are able to match 416 out of our firm population data to the LSTA data. There are 4,529 facility-quarter pairs with at least one quote for these 416 unique borrowers.

events. However, the difference-in-differences estimate for the effect of the frauds on the secondary market bids, shown in columns 3 and 4 of Table IX, is -0.81 percentage points but not statistically significant. Loan sale transactions are relatively sparse during our sample period, which leads to a small sample size and low precision estimates. Overall, the market price evidence is suggestive that the fraud events reflected poorly on the quality of borrowers associated with the affected banks, but our estimates are too imprecise to reach definitive conclusions.

6.3 Financial Policy and Investment

Table IX shows the estimated effect of the Enron/WorldCom events on firm debt and investment obtained over the subsample of publicly traded firms.[30] We find no statistically significant effect of the reputation shock on firm debt scaled by assets (Table IX, columns 5 and 6). This suggests that firms are able to substitute the decline in syndicated debt with other debt financing. Investment flow in fixed assets of exposed firms experiences a statistically significant decline after the Enron/WorldCom events. The decline is also economically significant: 20% of the sample mean during the year immediately following the Enron events. The measured decline in 14.5% over a two year period after the shock, indicating the investment begins to recover after a year. The overall findings suggest that the loss of reputation of a primary lender have a significant immediate effect on firms' external cost of financing and investment. They also suggest that the cost of substituting financing sources is substantial.

7 Conclusion

This paper provides evidence that bank monitoring reputation is a key determinant of the supply of credit, the characteristics of financial contracts, and investment. Our main

[30] In unreported estimations we verify that the results discussed so far hold on this subsample of firms.

results exploit the Enron and WorldCom corporate frauds in 2001 and 2002 as a source of variation in their lenders' monitoring reputation. Using a within-firm estimator, we find that the fraud discoveries cause a substantial decline in the supply of credit by banks with a prior lending relationship to these firms. Consistent with the reputation channel, we find that banks supply a larger amount of funding in loan syndicates when they have monitoring responsibilities as lead arrangers after the fraud discoveries. The results hold when we expand the analysis sample to include all corporate frauds in U.S. companies with more than $750 million in assets between 1996 and 2004.

Existing evidence of the empirical relevance of firm reputation on contracts is focused on environments with severe contracting limitations.[31] Greif (1989, 1991), for example, argues that a reputation based mechanism mitigated agency problems among the eleventh-century Maghribi traders in the absence of law-based institutions. Banerjee and Duflo (p. 989: 2000) analyze the customized software industry in India, where "the legal infrastructure is widely seen as quite primitive, limiting the scope for contracts." Our results emphasize that reputation can play a significant role in shaping the economic behavior of agents in developed legal environments.

The results presented in this paper are related to the academic and policy debate on the determinants and regulation of bank risk taking behavior. Regulation typically caps leverage, i.e., increase banks' skin in the game, to reduce excessive risk taking and instability of the financial sector. The call for regulation is based on the premise that depositors, insulated from risk through deposit insurance, are banks' marginal suppliers of capital.[32] However, marginal funding is provided by sophisticated –and uninsured— investors in the syndicated debt market. Our results show that in such an environment, market forces alone induce banks to increase their skin in the game when risk taking incentives increase.

Our paper also highlights the potential consequences of fraud on investment. Fraud involves expropriation of investors. Thus, an increase in its expected incidence can have

[31] See MacLeod (2007) for a recent survey.

[32] See references and discussion in Boyd and De Nicolo (2005).

large consequences on the supply of capital. The results in this paper show that this effect is particularly strong through the banking sector.

A APPENDIX

We corroborate a direct implication of the reputation shock on the supply of credit by lead arranger and by participants in new syndicated loans. Since a reputation loss implies in equilibrium that lead arrangers supply a larger amount of finding for any given loan, the resulting substitution from participant to lead funding of loans will have an amplifying (attenuating) effect on the decline in the unconditional flows of participant (lead) debt.

To verify this we estimate independently the effect of the fraud discoveries on the supply of credit by lead and participant banks using specifications (5) and (6). Note that these specifications only have variation at the bank-firm pair level. We verify that the results in the paper are robust to aggregating the data at this level in columns 1 and 2 of Table A-I, which report the estimates of the collapsed specification using all new syndicated credit as the left-hand side variable. Columns 3 through 6 of Table A-I report the estimated coefficients using new lead debt and new participant debt. The point estimates of the effect on the supply of lead credit are negative but extremely noisy and not statistically distinguishable from zero in all specifications. In contrast, the estimated effect on the supply of participant debt is negative and significant in all specifications.

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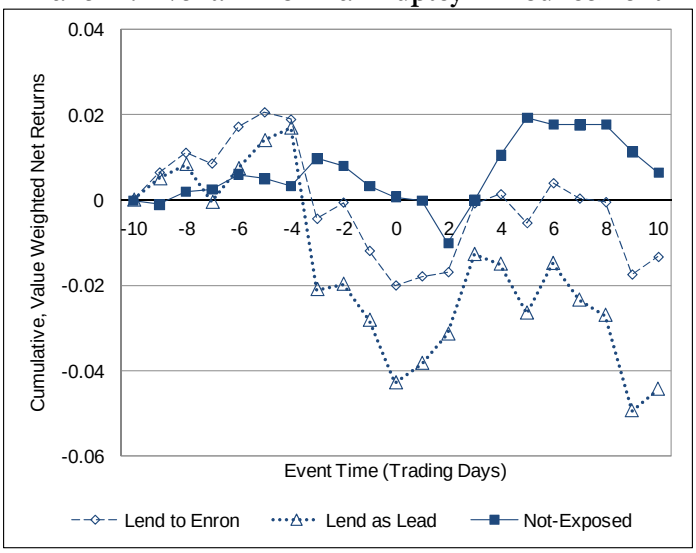
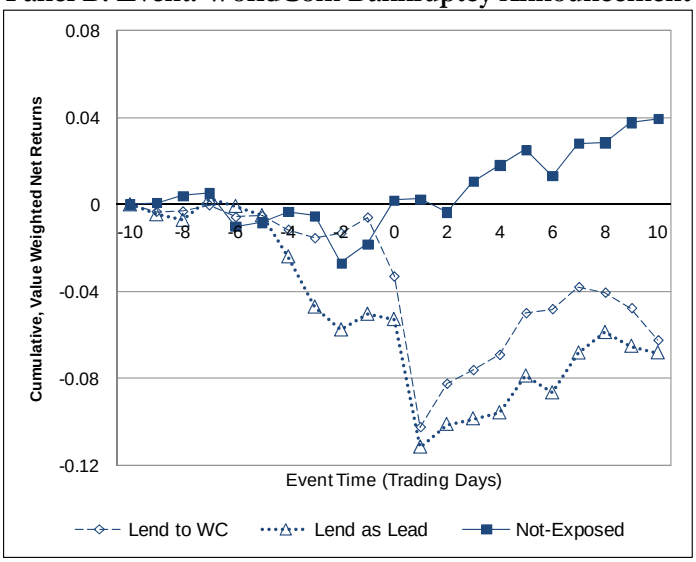
Figure 1
Lender Cumulative Net Return during 20 Trading Days around Enron/WorldCom Bankruptcies, by Lender Role in Enron/WorldCom Syndicated Lending

Panel A. Event: Enron Bankruptcy Announcement

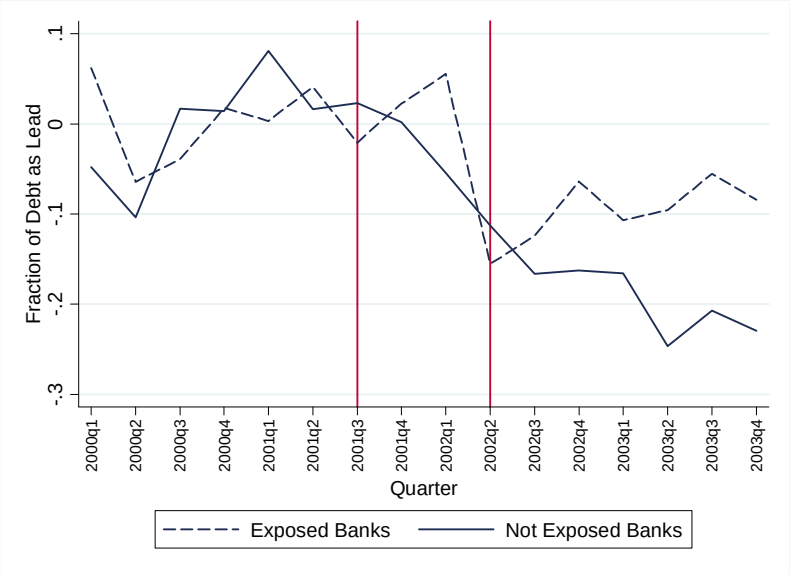


Panel B. Event: WorldCom Bankruptcy Announcement



Cumulative value-weighted net returns of the portfolios of public banks classified as exposed and not-exposed during the 20 days of trading around the announcement of Enron's and WorldCom's bankruptcies (December 3rd 2001 and July 22nd 2002, respectively). A bank is classified as Exposed (Exposed as a Lead) if it participated in loan facilities (as a lead arranger) to WorldCom or Enron during the four quarters prior to Enron's bankruptcy.

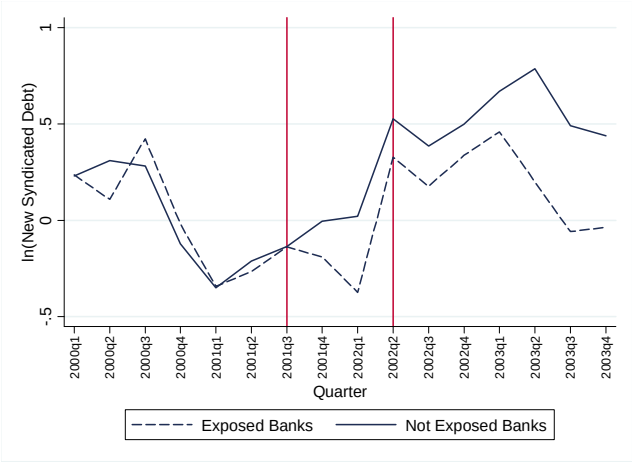
Figure 2
Conditional on Participation as a Lead Arranger, Fraction of Lead Debt to Total Facility, by Bank Role in the Enron/WorldCom Lending*



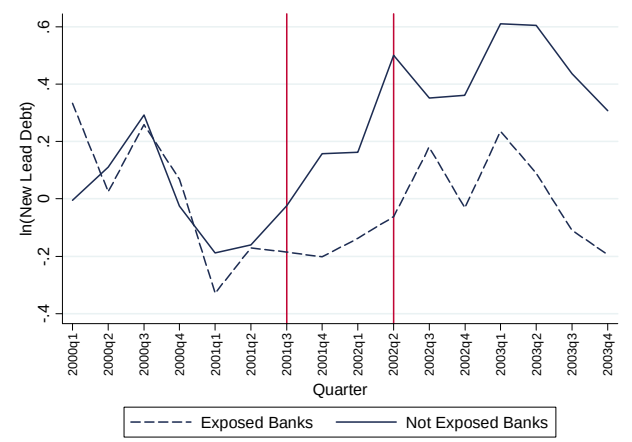
* Normalized to zero mean in the pre-Enron period. The vertical lines mark the beginning of the Enron (2001q3) and WorldCom (2002q2) events.

Figure 3
New Syndicated Debt (logs), by Bank Role in Enron/WorldCom Lending*

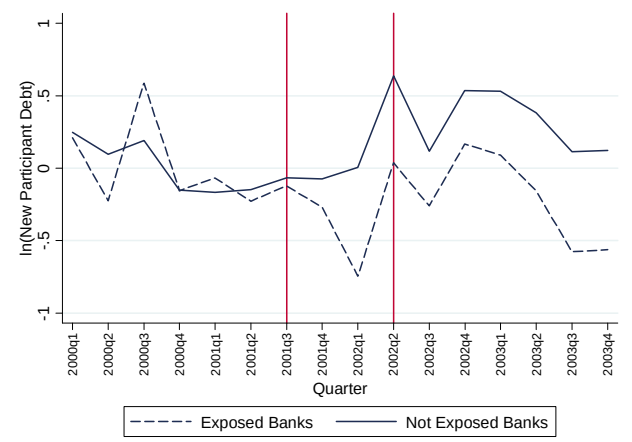
Panel A. Total Funding



Panel B: Funding Provided as Lead



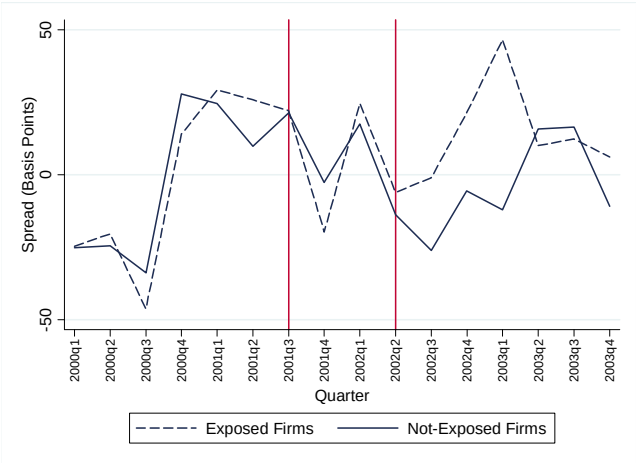
Panel C: Funding Provided as Participant



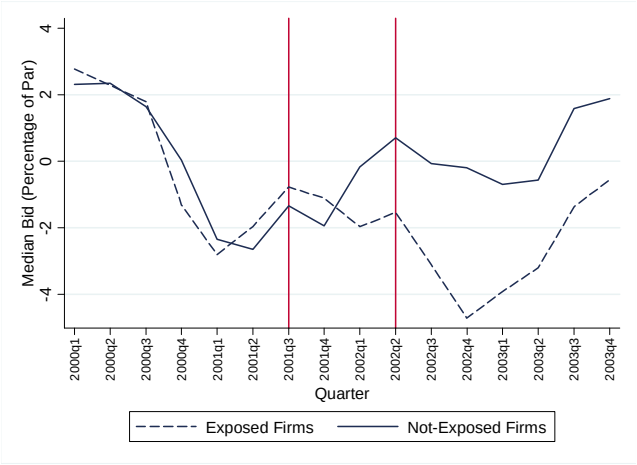
* Normalized to zero mean in the pre-Enron period. The vertical lines mark the beginning of the Enron (2001q3) and WorldCom (2002q2) events.

Figure 4
New Syndicated Loan Spreads and Secondary market Bids, By Firm Exposure

Panel A: Spread*



Panel B: Secondary Market Price (Median Bid)*



* Normalized to zero mean in the pre-period. The vertical lines mark the beginning of the Enron (2001q3) and WorldCom (2002q2) events.

Table I
Top 40 Lenders' Syndicated Lending between Q4-2000 and Q3-2001, Fraction to Enron and WorldCom, and Exposure to Post-Bankruptcy Litigation

Sources: *Dealscan*, *LexisNexis*, *Enron Creditors Recovery Corp.* Enron and WorldCom received two syndicated facilities each between Q3 of 2000 and Q3 of 2001. Enron's facilities were a $1.75 billion 364-day loan and a $400 million standby letter of credit (identical 49 lenders and 2 lead arrangers each). WorldCom's facilities were a $2.65 billion 364-day loan and a $1.6 billion 5-year line of credit (identical 28 lenders and 2 lead arrangers each). % lending by lead arrangers to Enron and WorldCom highlighted with bold typeface. *% to Other* is calculated based on $6.09 billion in four new facilities to other firms also involved in fraud scandals between Q3 of 2001 and Q2 of 2002 (Adelphia Communications, Arthur Andersen, KMART, and Qwest Communications International).

#	Top 40 Syndicated Lenders (2000Q4 to 2001Q3)	Total # of Facilities		Total Facilities (million $)		% to Enron	% to WorldCom	% to Other	Post-Bankruptcy Litigations	
		Any	As Lead	Any Role	As Lead				Enron	WorldCom
1	Bank of America Corporation	1,700	1009	99,400	73,800	0.03%	**0.59%**	0.98%		Yes
2	J.P. Morgan Chase & Co.	1,223	740	124,200	109,000	**0.13%**	**0.47%**	0.31%	Yes	Yes
3	FleetBoston Financial Corp.	1,115	225	33,200	10,800	0.09%	0.36%	0.66%		
4	Wachovia Corporation	1,110	201	33,540	7,540	0.18%		0.90%		
5	Bank One Corporation	993	296	44,500	19,900	0.07%	0.27%	0.41%	Yes	
6	Citigroup Inc.	859	397	71,500	48,600	**0.22%**	0.17%	1.38%	Yes	Yes
7	Bank of New York Company	727	83	24,150	5,550	0.13%		0.75%	Yes	
8	U.S. Bancorp	663	97	9,970	1,040			1.33%		
9	Bank of Nova Scotia	591	47	20,160	3,660	0.15%	0.59%			
10	Suntrust Banks, Inc.	577	94	15,150	3,750	0.20%				
11	Wells Fargo & Co.	554	126	14,070	4,220		0.84%	1.29%		
12	BNP Paribas	503	47	19,470	1,670	0.16%	0.61%			Yes
13	Credit Suisse First Boston	488	121	30,000	15,100	0.10%			Yes	Yes
14	ABN AMRO Bank NV	475	54	20,440	3,340	0.15%	0.58%			Yes
15	Credit Lyonnais	443	36	13,410	1,310	0.23%	0.88%			
16	National City Corporation	412	89	5,630	1,970					
17	Mellon Financial Corporation	401	17	13,853	353		0.85%	1.03%		
18	Comerica Inc.	379	45	6,604	474			0.33%		
19	Bank of Tokyo-Mitsubishi Ltd	371	4	15,840	1,040	0.19%	0.74%			Yes
20	PNC Financial Services Group	370	101	7,610	1,150					
21	Keycorp	334	66	5,710	1,180			1.69%		
22	Barclays Bank Plc	291	15	14,920	1,220	0.20%			Yes	
23	Northern Trust Corporation	283	0	8,180	0	0.37%				
24	Union Bank of Canada	269	38	5,064	774					
25	Commerzbank AG	263	17	14,160	1,160					
26	Deutsche Bank Alex Brown	252	71	14,830	5,680	0.21%			Yes	Yes
27	Industrial Bank of Japan Ltd	251	1	8,100	30		1.46%			
28	Societe Generale	247	25	8,692	902	0.35%				
29	General Electric Capital Corp	244	77	4,020	2,180					
30	Fuji Bank Ltd	239	1	6,040	0		1.95%			
31	LaSalle Bank NA	210	17	2,004	254					
32	Heller Financial Inc	207	51	2,029	639					
33	Royal Bank of Canada	197	6	8,922	542	0.34%			Yes	
34	Toronto Dominion Bank	193	24	8,670	1,850				Yes	
35	Bank of Montreal	175	26	5,628	808	0.54%				
36	Royal Bank of Scotland Plc	174	5	6,752	72	0.45%	1.75%		Yes	
37	Dresdner Bank AG	172	6	6,448	868	0.47%				
38	Westdeutsche Landesbank GZ	172	1	7,440	0	0.41%	1.59%			
39	KBC Group	153	5	4,376	246	0.70%				
40	Harris Bankcorp, Inc	152	13	1,661	251					
	All others	10,625	1,108	297,313	62,429	0.25%	0.48%	0.01%	9 others	8 others

Table II
Descriptive Statistics, Syndicated Loans

Subsample: new syndicated loans issued between the fourth quarter of 2000 and the fourth quarter of 2001. A bank is classified as Exposed if it participated in loan facilities to WorldCom or Enron during the four quarters prior to Enron's bankruptcy. Excludes loans to firms in the financial, utility, telecommunications, and energy sectors.

	mean	sd	5th %-ile	median	95th %-ile	N
Panel 1. Aggregate Statistics, by quarter						
New Facilities (million $)	50,800	5,410	45,500	50,800	56,300	4
Fraction by Lead Banks	0.297	0.056	0.253	0.282	0.370	4
Fraction by Exposed Banks	0.640	0.022	0.610	0.643	0.663	4
Fraction Lead among Exposed	0.301	0.073	0.227	0.294	0.391	4
Panel 2. Firm-Quarter Statistics						
New Facilities (million $)	199	533	2	40	942	1,020
Fraction by Lead Banks	0.706	0.367	0.113	1.000	1.000	1,020
Fraction by Exposed Banks	0.377	0.411	0.000	0.157	1.000	1,020
Fraction Lead among Exposed	0.552	0.401	0.000	0.459	1.000	530

Table III
Sample Descriptive Statistics of Public Firms

Sample: results from hand-matching the firm names in DealScan with the firm names in COMPUSTAT - North America. Restricted to firms that had at least one loan facility between the fourth quarter of 2000 and the fourth quarter of 2001. Excludes firms in the financial, utility, telecommunications, energy, electrical equipment, or software industries.

	All Firms (n= 1,193)			Exposed Firms (n= 587)			Not-Exposed Firms (n= 606)		
	mean	median	sd	mean	median	sd	mean	median	sd
Market Capitalization ($ millions)	4,206	474	17,124	6,493	977	22,141	1,429	211	6,333
Spread on Syndicated Debt	160.8	150.0	114.3	144.7	125.0	112.9	206.8	210.0	105.6
Syndicated Origination/Assets	0.025	0.000	0.083	0.027	0.000	0.086	0.021	0.000	0.079
Syndicated Origination (Conditional)/Assets*	0.20	0.16	0.14	0.20	0.17	0.14	0.21	0.16	0.15
Total Debt/Assets	0.29	0.28	0.20	0.31	0.30	0.20	0.27	0.25	0.21
Cash/Assets	0.081	0.028	0.138	0.062	0.027	0.099	0.103	0.031	0.170
Investment in Fixed Assets/Assets	0.018	0.010	0.034	0.019	0.011	0.039	0.017	0.010	0.026

*Conditional on receiving a syndicated loan

Table IV
Effect of Fraud on Unconditional Credit Supply, and the Conditional Credit Supply of Banks with Monitoring Roles: Enron and WorldCom

Estimation results of within-firm specification (2):

$$\bar{y}_{ijl}^{post} - \bar{y}_{ijl}^{pre} = \alpha_i + \beta_0 \cdot ExposedBank_j + \beta_1 \cdot LeadDebt_l + \beta_2 \cdot ExposedBank_j \cdot LeadDebt_l + \varepsilon_{ijl}$$

The dependent variable represents the change in average debt of type *l* (lead or participant) of firm *i* with bank type *j* (exposed or not-exposed), before and after Enron's fraud discovery (after the third quarter of 2000). Specifications 2 and 4 include as a control variable the (log) total amount of loans issued during the pre period by lenders with exposure status *j* and role *l* in the syndicate. Standard errors (in parenthesis) are heteroskedasticity-robust and clustered at the firm industry level. *, ** and *** statistical significance at the 10, 5, and 1 percent levels respectively.

	$\ln(\text{New Debt}_{ijl})_{post} - \ln(\text{New Debt}_{ijl})_{pre}$			
Post-Enron Estimation Period:	1 Year	1 Year	2 Years	2 Years
	(1)	(2)	(3)	(4)
Panel 1. Full Sample				
ExposedBank	-0.3497**	-0.2988*	-0.4824***	-0.4131***
	(0.1494)	(0.1653)	(0.1012)	(0.1223)
LeadDebt	-0.0741	-0.0738	-0.0492	-0.0455
	(0.1538)	(0.1577)	(0.1348)	(0.1614)
ExposedBank x LeadDebt	0.311	0.3352	0.3168**	0.3387**
	(0.2254)	(0.2288)	(0.1514)	(0.1636)
$\ln(\text{Average Bank Total Loans})_{pre}$		-0.0319		-0.0399*
		(0.0342)		(0.0205)
Observations	436	436	673	673
R-squared	0.734	0.735	0.726	0.731
Panel 2. Excluding Energy, Electrical Equipment, and Software				
ExposedBank	-0.3345**	-0.2775	-0.4621***	-0.3805***
	(0.1570)	(0.1739)	(0.1024)	(0.1268)
LeadDebt	-0.1349	-0.1372	-0.0817	-0.0993
	(0.1384)	(0.1435)	(0.1371)	(0.1631)
ExposedBank x LeadDebt	0.3933*	0.4235*	0.3325**	0.3868**
	(0.2263)	(0.2289)	(0.1645)	(0.1778)
$\ln(\text{Average Bank Total Loans})_{pre}$		-0.0356		-0.0470*
		(0.0351)		(0.0249)
Observations	394	394	607	607
R-squared	0.703	0.705	0.713	0.719

Table V
Effect of Enron/WorldCom Frauds on the Composition of Syndicates, by Role of the Exposed Lender in the Syndicate

Estimation results of within-firm specification (2) augmented to include a separate indicator for banks that were exposed as lead arrangers and as participants in syndicated loans before Enron events:

$$\bar{y}_{ijl}^{\,post} - \bar{y}_{ijl}^{\,pre} = \alpha_i + \beta_0 \cdot ExposedBank_j + \beta_1 \cdot LeadDebt_l + \beta_2 \cdot ExposedBank_j \cdot LeadDebt_l + \varepsilon_{ijl}$$

The dependent variable represents the change in average debt of type *l* (lead or participant) of firm *i* with with bank type *j* (exposed or not-exposed), before and after Enron's fraud discovery (after the third quarter of 2000). Standard errors (in parenthesis) are heteroskedasticity-robust and clustered at the firm industry level. *, ** and *** statistical significance at the 10, 5, and 1 percent levels respectively.

	$\ln(\text{New Debt}_{ijl})_{post} - \ln(\text{New Debt}_{ijl})_{pre}$	
Post-Enron Estimation Period:	1 Year	2 Years
	(1)	(2)
Panel 1. Full Sample		
ExposedBank_asLead	-0.2522*	-0.4102***
	(0.1322)	(0.0954)
ExposedBank_asPart	-0.2769*	-0.3663**
	(0.1435)	(0.1389)
LeadDebt	-0.0793	-0.063
	(0.1459)	(0.1336)
ExposedBank_asLead x LeadDebt	0.3627	0.3551*
	(0.3453)	(0.2099)
ExposedBank_asPart x LeadDebt	0.1522	0.165
	(0.2604)	(0.2079)
Observations	463	707
R-squared	0.735	0.715
Panel 2. Excluding Energy, Electrical Equipment, and Software		
ExposedBank_asLead	-0.2295	-0.3843***
	(0.1380)	(0.0941)
ExposedBank_asPart	-0.2785*	-0.3899**
	(0.1511)	(0.1466)
LeadDebt	-0.1384	-0.0977
	(0.1351)	(0.1358)
ExposedBank_asLead x LeadDebt	0.4726	0.4137*
	(0.4097)	(0.2435)
ExposedBank_asPart x LeadDebt	0.252	0.2063
	(0.2420)	(0.2145)
Observations	419	637
R-squared	0.705	0.701

Table VI
Bias Due to Unaccounted Firm Heterogeneity: Fixed Effects Estimation

Estimation results of firm fixed effects specification (2):

$$\bar{y}_{ijlt} = \alpha_i + \gamma_0 \cdot ExposedBank_j + \gamma_1 \cdot LeadDebt_l + \gamma_2 \cdot ExposedBank_j \cdot LeadDebt_l + \gamma_4 Post_t + \left[\gamma'_0 \cdot ExposedBank_j + \gamma'_1 \cdot LeadDebt_l + \gamma'_2 \cdot ExposedBank_j \cdot LeadDebt_l\right] \times Post_t + \varepsilon_{ijl}$$

The dependent variable is the average debt of type *l* (lead or participant) of firm *i* with bank type *j* (exposed or not-exposed) and time *t* (pre or post Enron's bankruptcy). The terms without interactions are not included in the table for succinctness. Standard errors (in parenthesis) are heteroskedasticity-robust and clustered at the firm industry level. *, ** and *** statistical significance at the 10, 5, and 1 percent levels respectively.

	$\ln(\text{New Debt}_{ijl})_{post} - \ln(\text{New Debt}_{ijl})_{pre}$			
Post-Enron Estimation Period:	1 Year	1 Year	2 Years	2 Years
	(1)	(2)	(3)	(4)
Panel 1. Full Sample				
ExposedBank x Post	-0.2454**	-0.1963	-0.3312***	-0.2735***
	(0.1200)	(0.1288)	(0.0886)	(0.0932)
LeadDebt x Post	-0.0421	0.0117	-0.1112	-0.0838
	(0.0866)	(0.0878)	(0.0838)	(0.0858)
ExposedBank x LeadDebt x Post	0.1926	0.1634	0.2980**	0.2930**
	(0.1409)	(0.1330)	(0.1211)	(0.1159)
$\ln(\text{Bank Total Loans})_{pre}$ x Post		-0.0190		-0.0256***
		(0.0116)		(0.0086)
Observations	3,641	3641	4,160	4160
R-squared	0.855	0.857	0.840	0.842
Panel 2. Excluding Energy, Electrical Equipment, and Software				
ExposedBank x Post	-0.2234*	-0.1738	-0.3224***	-0.2650**
	(0.1324)	(0.1421)	(0.0985)	(0.1035)
LeadDebt x Post	-0.0272	0.0169	-0.1093	-0.0926
	(0.0963)	(0.0981)	(0.0913)	(0.0961)
ExposedBank x LeadDebt x Post	0.1918	0.1688	0.3095**	0.3133**
	(0.1581)	(0.1485)	(0.1319)	(0.1269)
$\ln(\text{Bank Total Loans})_{pre}$ x Post		-0.0196		-0.0260***
		(0.0125)		(0.0096)
Observations	3,243	3243	3,714	3714
R-squared	0.852	0.854	0.836	0.838

Table VII
Effect of Fraud on Credit Supply: Extended Fraud Sample

Estimation results of within-firm specification:

$$y_{ijlt} = \alpha_{it} + \beta_0 \cdot ExposedBank_{jt} + \beta_1 \cdot LeadDebt_{lt} + \beta_2 \cdot ExposedBank_{jt} \cdot LeadDebt_{lt} + \varepsilon_{ijlt}$$

The left hand-side variable is the (log) amount of the syndicated loan, received by firm *i* during quarter *t*, that is funded by bank *j* (*j*=1 for banks affected by the reputation shock) with role in the syndicate *l* (*l*=1 if lead arranger). Specifications 2 and 4 include as controls a set of interactions between quarter dummies and the (log) total amount of loans issued during quarter *t* by lenders with exposure status *j* and role *l* in the syndicate. Columns 3 and 4 include interactions of all right-had side variables with a dummy equal to one for every quarter before 2000. Standard errors (in parenthesis) are heteroskedasticity-robust and clustered at the firm industry level. ** and *** statistical significance at the 5 and 1 percent levels respectively.

	ln(New Syndicated Debt$_{ijlt}$)			
	(1)	(2)	(3)	(4)
ExposedBank	-0.479***	-0.500***	-0.383***	-0.391***
	(0.053)	(0.052)	(0.071)	(0.067)
LeadDebt	-0.771***	-0.703***	-0.655***	-0.613***
	(0.065)	(0.061)	(0.085)	(0.082)
ExposedBank x LeadDebt	0.361***	0.321***	0.251**	0.229**
	(0.101)	(0.095)	(0.111)	(0.104)
ExposedBank x Pre2000			-0.172***	-0.195***
			(0.054)	(0.053)
LeadDebt x Pre2000			-0.210***	-0.164**
			(0.066)	(0.066)
ExposedBank x LeadDebt x Pre2000			0.133	0.084
			(0.121)	(0.120)
ln(Bank Total Loans) x Quarter Dummies		Yes		Yes
Observations	20,466	20,466	20,466	20,466
R-squared	0.824	0.828	0.825	0.828

Table VIII
Fraud or Bankruptcy? Effect of Bankruptcies not Accompanied by Fraud

Estimation results of within-firm specification:

$$y_{ijlt} = \alpha_{it} + \beta_0 \cdot ExposedBank_{jt} + \beta_1 \cdot LeadDebt_{lt} + \beta_2 \cdot ExposedBank_{jt} \cdot LeadDebt_{lt} + \varepsilon_{ijlt}$$

The left hand-side variable is the (log) amount of the syndicated loan, received by firm *i* during quarter *t*, that is funded by bank *j* (*j*=1 for banks that actively lent to a firm that eventually filed for Chapter 11 bankruptcy) with role in the syndicate *l* (*l*=1 if lead arranger). Specification 2 includes as controls a set of interactions between quarter dummies and the (log) total amount of loans issued during quarter *t* by lenders with exposure status *j* and role *l* in the syndicate. Standard errors (in parenthesis) are heteroskedasticity-robust and clustered at the firm industry level. ** and *** statistical significance at the 5 and 1 percent levels respectively.

	ln(New Syndicated Debt$_{ijlt}$)	
	(1)	(2)
ExposedBank (Bankruptcy)	-0.176***	-0.057
	(0.046)	(0.054)
LeadDebt	-0.532***	-0.559***
	(0.065)	(0.064)
ExposedBank (Bankruptcy) x LeadDebt	-0.125*	-0.115
	(0.070)	(0.070)
ln(Bank Total Loans) x Quarter Dummies		Yes
Observations	11,594	11,594
R-squared	0.827	0.836

Table IX
Effect of Enron/WorldCom Frauds on Loan Prices, Leverage, and Investment, Firm Level Estimation

Estimation results of firm fixed-effects panel specification (3):

$$\bar{y}_i^{post} - \bar{y}_i^{pre} = \gamma \cdot ExposedFirm_i + \delta^{SIC2}{}_i + \delta^{State}{}_i + \delta^{SizeQuartile}{}_i + \varepsilon_i$$

The dependent variable is the change in outcome *y* before and after Enron's fraud discovery (after the third quarter of 2000). The dependent variables are the interest rate spread relative to LIBOR in basis points (columns 1 and 2), the median secondary market bid in percentage points of par (columns 3 and 4), total debt scaled by assets (columns 5 and 6), and capital expenditures scaled by assets (columns 7 and 8), for firm *i*. The right-hand side variable of interest is a dummy equal to one if firm *i* had at least one loan facility between the fourth quarter of 2000 and the first quarter of 2002 in which at least one participant in the syndicate is exposed to Enron or WorldCom.

	Interest Spread		Median Secondary Market Bid		Debt/Assets		Capex/Assets	
Post_Enron Period:	1Year	2 Years	1Year	2 Years	1Year	2 Years	1Year	2 Years
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ExposedFirm	8.588	45.47**	-0.817	-0.809	0.0132	0.0093	-0.0036**	-0.0026**
	(29.550)	(19.630)	(0.694)	(0.822)	(0.0085)	(0.0093)	(0.0015)	(0.0013)
Industry Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Location Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Size Quintile Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	357	479	54	56	1,087	1,087	1,079	1,079
R-squared	0.575	0.442	0.998	0.999	0.292	0.263	0.271	0.294

Table A1
Effect of Fraud on Credit Supply by Lead Arrangers and Participants

Estimation results of within-firm specification (1):

$$\bar{y}_{ij}^{\,post} - \bar{y}_{ij}^{\,pre} = \alpha_i + \beta \cdot ExposedBank_j + \varepsilon_{ij}$$

The dependent variable is the average flow of new syndicated credit to firm *i* from bank type *j* (exposed, not-exposed) after the Enron bankruptcy (4th quarter of 2000 and afterwards), minus the average flow of new syndicated credit to firm *i* from bank type *j* before the Enron bankruptcy. Columns 3 and 4 (5 and 6) use new syndicated debt by lead arrangers (participants) of the syndicate. Standard errors (in parenthesis) are heteroskedasticity-robust and clustered at the firm industry level. *, ** and *** statistical significance at the 10, 5, and 1 percent levels respectively.

Dependent Variable	$\ln(\text{New Debt}_{ij})_{post} - \ln(\text{New Debt}_{ij})_{pre}$		$\ln(\text{New Lead Debt}_{ij})_{post} - \ln(\text{New Lead Debt}_{ij})_{pre}$		$\ln(\text{New Participant Debt}_{ij})_{post} - \ln(\text{New Participant Debt}_{ij})_{pre}$	
Post Period	1 Year	2 Years	1 Year	2 Years	1 Year	2 Years
	(1)	(2)	(3)	(4)	(5)	(6)
Panel 1. Full Sample						
ExposedBank	-0.4239**	-0.5352***	-0.5958	-0.305	-0.3503**	-0.4846***
	(0.2072)	(0.1672)	(3.0194)	(1.2303)	(0.1508)	(0.1151)
Observations	377	560	196	311	240	362
R-squared	0.767	0.779	0.998	0.98	0.76	0.77
Panel 2. Excluding Energy, Electrical Equipment, and Software						
ExposedBank	-0.3700*	-0.4887***		-0.3316		-0.4669***
	(0.2155)	(0.1677)		(1.6734)		-0.1207
Observations	337	499		275		332
R-squared	0.737	0.765		0.977		0.773

Table A2
Number of Frauds by Year

Sample includes the 216 corporate frauds that took place in U.S. companies with more than $750 million in assets between 1996 and 2004 indentified in Dyck, Morse, and Zingales (2010).

Year	Number	Percent
1994	4	1.85
1995	11	5.09
1996	14	6.48
1997	26	12.04
1998	34	15.74
1999	36	16.67
2000	43	19.91
2001	31	14.35
2002	13	6.02
2003	3	1.39
2004	1	0.46
Total	216	100

Table A3
Number of Frauds by Year

Summary banks' loan syndication activities and exposure to fraudulent firms, by the top 40 banks ranked by number of syndicates during the 1996 to 2004 period.

#	Top 40 Syndicated Lenders (1996Q1 to 2004Q4)	Total # of Facilities (Loans)	Total Funding to Syndicates (million $)	Number of Fraud Firm Exposures	% of Total Funding to Fraud Firms
1	Bank of America	14,932	775,862	52	2.57%
2	Bank of New York	8,036	232,268	35	2.68%
3	Chase Manhattan Bank	7,853	567,051	40	5.59%
4	Bank of Nova Scotia	6,662	177,623	32	2.99%
5	Citibank	6,637	532,323	35	4.06%
6	Credit Lyonnais	5,358	132,563	25	1.78%
7	ABN AMRO Bank NV	5,346	184,073	34	2.36%
8	BANK ONE Corp	5,066	194,571	16	1.82%
9	PNC Bank	4,680	93,769	23	3.29%
10	Mellon Bank	4,415	119,609	20	2.34%
11	NationsBank	4,270	187,825	20	2.46%
12	Wachovia Bank	4,213	133,782	15	2.25%
13	Societe Generale	4,076	113,595	16	1.16%
14	SunTrust Bank	3,690	89,049	12	2.11%
15	Comerica Bank	3,687	54,945	11	3.56%
16	National City Bank	3,585	44,517	5	3.47%
17	Credit Suisse First Boston	3,503	184,602	17	4.16%
18	US Bank NA	3,480	59,465	5	0.53%
19	General Electric Capital Corp	3,429	68,400	3	0.83%
20	Bank of Montreal	3,415	87,523	17	3.21%
21	Wells Fargo Bank	3,232	75,025	10	2.31%
22	First Chicago	3,162	99,883	11	1.74%
23	Fuji Bank Ltd	3,085	66,599	22	3.84%
24	Fleet National Bank	3,026	70,875	7	0.93%
25	Barclays Bank Plc	3,026	122,511	13	1.00%
26	Royal Bank of Canada	2,963	103,063	20	3.47%
27	Morgan Guaranty Trust	2,937	210,459	25	4.24%
28	Chemical Bank	2,793	254,609	3	1.59%
29	Toronto Dominion Bank	2,688	87,217	18	4.78%
30	CIBC	2,685	68,744	6	1.99%
31	Bankers Trust Co	2,656	95,782	6	1.10%
32	Fleet Bank	2,604	56,378	12	5.37%
33	Industrial Bank of Japan Ltd	2,585	76,160	21	4.28%
34	Sumitomo Bank	2,564	61,994	22	5.12%
35	JP Morgan	2,561	236,002	4	1.67%
36	Bank of Boston	2,515	40,967	6	0.75%
37	BNP Paribas	2,515	89,502	3	0.20%
38	Deutsche Bank AG	2,410	111,768	13	2.72%
39	Commerzbank AG	2,404	94,896	10	1.64%
40	Union Bank of California	2,386	39,868	8	0.96%
Statistics of all other lenders (n = 1,913)					
	Mean	94	2,238	2	5.50%
	Std. Dev.	253	7,197	4	9.29%
	Median	8	115	1	2.66%
	Max	2,350	88,294	52	89.34%
	Min	1	0	0	0.00%