How Important is the Informational Advantage of Relationship Lenders?

: The Impact of Regulation Fair Disclosure

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Abstract

This paper examines how the implementation of Regulation Fair Disclosure (Reg FD) affected credit markets. We argue that, although disclosing private information to lenders is exempt from Reg FD, this regulation imposed an additional disclosure risk on borrowers because this exemption was conditional upon creditors abiding by a confidentiality agreement. We hypothesize that after the introduction of Reg FD, borrowers reduced their information disclosures by relying increasingly on relationship lenders who had produced proprietary information about borrowers from prior interactions. Using a natural experiment relating to changes in borrower-lender relationships surrounding the introduction of Reg FD, we show that after Reg FD, switching to new (non-relationship) lenders was less likely and any switching was associated with higher costs of debt. Further, borrowers were more dependent on relationship banking with lead lenders retaining a larger proportion of the loans they syndicated. Moreover, in the secondary market for syndicated loans, bid-ask spreads significantly increased following the implementation of Reg FD. Finally, we find that there was a movement away from public debt issuance towards a greater reliance on bank debt in the form of syndicated loans in the post-Reg FD period. We interpret these results as evidence of an increased level of information asymmetry in credit markets after the introduction of Reg FD.

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1. Introduction

The Securities and Exchange Commission's (SEC) Regulation Fair Disclosure (Reg FD) took effect on October 23, 2000. Reg FD imposed new restrictions on selective disclosures of material non-public information to equity market professionals, such as institutional investors and financial analysts.[1] The intention of Reg FD was to level the playing field for investors, by requiring public disclosure of material private information to all market participants. However, both academics and practitioners have expressed concerns about a potential "chilling effect", on the amount of information actually disseminated to the market following the adoption of this regulation.[2]

Prior research investigating the impact of Reg FD on companies' disclosure practices and the "quantity and quality" of information has produced mixed results (Healy, 2007).[3] [4] One of the biggest challenges facing this strand of research lies in the measurement of the "quantity and quality" of information in the capital market.[5] For example, Sidhu, Smith, Whaley, and Willis

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[1] SEC's (2000) final ruling on Reg FD states that "four categories of persons to whom selective disclosure may not be made absent a specified exclusion. The first three are securities market professionals -- (1) broker-dealers and their associated persons, (2) investment advisers, certain institutional investment managers and their associated persons, and (3) investment companies, hedge funds, and affiliated persons. These categories will include sell-side analysts, many buy-side analysts, large institutional investment managers, and other market professionals who may be likely to trade on the basis of selectively disclosed information. The fourth category of person included in Rule 100(b) (1) is any holder of the issuer's securities, under circumstances in which it is reasonably foreseeable that such person would purchase or sell securities on the basis of the information".

[2] For example, the association for Investment Management and Research (now the CFA Institute) argues, "Corporations will almost certainly curtail the information flow to the market to avoid having to decide on the spot whether certain information will be deemed to be material after the fact by the SEC…" (AMIR, 2000).

[3] There is mixed evidence with regard to firms' disclosure behavior after Reg FD. For example, Heflin, Subramanyam, and Zhang (2003) show that management earnings forecasts increased for the first three quarters after the implementation of Reg FD. In the meantime, while Wang (2007) and Gome, Gorton, and Madureira (2007) show that some firms reduce their public disclosure following the adoption of Reg FD, Bushee, Matsumoto, and Miller (2004) suggest that firms did not reduce their disclosure through open conference calls.

[4] A strand of research studies the effect of Reg FD on analyst forecasts. For example, Heflin *et al*. (2003) report that there was no significant change in analyst forecast accuracy and dispersion after Reg FD. Agrawal, Chadha, and Chen (2006) document that analyst' forecasts become less accurate and more dispersed after Reg FD. In addition, Gintschel and Markov (2004) show that the information content of analyst reports decreased as stock price became less responsive to the release of earning forecasts (reduced by about 28%) in the post- Reg FD period.

[5] It is difficult to measure the quality of information disclosed by firms. For example, Thakor (2012) points out that information about firms' strategy is qualitative and subjective in nature, leading to divergent interpretations by

(2008) extract an adverse selection component from stock market bid-ask spreads to show that the level of information asymmetry increased after Reg FD, consistent with a decline in the quantity and quality of information being disclosed to the investment population.[6] In addition, Duarte, Han, Harford, and Yang (2008) document that the cost of equity for firms traded on NASDAQ increased in the post-Reg FD period. Both papers suggest that information quantity and quality in the equity market declined following the introduction of Reg FD.

While most of these studies focus on the effect of Reg FD on the equity market or its participants, little is known about how this regulation influenced loan or credit markets and their participants, such as bank lenders and borrowers.[7] This gap in the literature is unfortunate given the wide reliance of public firms on bank loans as part of their capital structure. Consequently, this paper examines how Reg FD affected the quantity and quality of information production among different loan market participants in the post- Reg FD period.

Although Reg FD provided an exemption regarding the communication of private information to creditors, this exemption was conditional upon creditors abiding by a confidentiality agreement (SEC, 2000).[8] As a result, companies bore the additional disclosure risk of making private material information publicly available if creditors directly, or indirectly, break the confidentiality agreement.[9]

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investors.

[6] Sidhu *et al.* (2008) argue that the adverse selection cost component of the bid-ask spread represents the risk premium market makers charge to cover potential losses from trading with informed individuals. Thus, this measure captures the level of information asymmetry between informed and uninformed investors in the market.

[7] Firms' disclosure of private information to credit rating agencies was exempt from Reg FD until the introduction of Dodd Frank Act in 2010. Jorion, Liu, and Shi (2005) examine stock price reactions to announcements of credit rating upgrades (downgrades) and found that stock price responded to such announcements more significantly in the post Reg FD period, implying that change of credit rating carried more information content after Reg FD.

[8] Motivated by this exemption, Petacchi (2012) examines whether the implementation of Reg FD caused a significant change in firms' capital structure. She finds a significantly increased average leverage ratio among public firms in the post-Reg FD period and attributes this finding to firms' intention to avoid public disclosures.

[9] The SEC, in its final ruling, puts forth that "if a reporting issuer releases material information non-publicly during an unregistered offering with no such understanding about confidentiality, we believe that disclosure under Regulation FD is appropriate…". SEC further states that "Public companies undertaking unregistered offerings will

Ivashina and Sun (2011) show that, unlike relationship lenders, institutional lenders, such as hedge funds and other institutional investors, are more likely to trade on private information obtained through participating in loan syndications.[10] As an example, in March 2006, a large movie rental chain (Movie Gallery) held a private conference call with about 200 lenders most of which are hedge fund lenders. The company announced its poor financial condition to these potential lenders before making it publicly available information. During the next two days, Movie Gallery's shares were heavily traded and its stock plummeted 25%, triggering an SEC investigation (see, Jenny Anderson, *As Lenders, Hedge Funds Draw Insider Scrutiny*, N.Y. TIMES, October 16, 2006). In addition, Ivashina, Massoud, Nadia, Saunders, and Stover (2009) find evidence consistent with lenders releasing private information to "strong" corporate customers to better enable them to takeover "weak" company borrowers so as to reduce the risk of loan defaults. These examples show that companies bear an additional public disclosure risk if agents breach their fiduciary duties while possessing material private information about the company. Agents that commonly possess such private information are banks and other lenders.

In examining whether information flows were reduced in credit markets after the introduction of Reg FD, we utilize stylized facts regarding asymmetric or imperfect information among borrowers and lenders. Specifically, information asymmetries exist between borrowers and lenders in general (see, e.g., Diamond, 1984, 1991; Boyd and Prescott, 1986; Ramakrishman and Thakor, 1984). Moreover, relationship lenders, having invested in collecting and generating

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need to consider the impact their selective disclosure could have on any exemption they use. Before an exempt offering begins, issuer's counsel should advise the client of the potential complications that selective disclosure of material non-public information could raise." (see, SEC Final Rule: Selective Disclosure and Insider Trading", 2000)

[10] Boot and Thakor (2000) define a "relationship loan" as a loan that permits banks to use their expertise to improve the borrower's project payoff. They also define a "transactional loan" as a "pure funding transaction" without lenders' sector-specific investments. Compared to bank lenders, non-bank institutional lenders are considered more transaction-oriented and, as a result, they are more likely to misuse the information they receive during the loan syndication process.

"reusable" proprietary information about borrowers, through multiple interactions in the past, face lower information asymmetries relative to non-relationship lenders (Boot, 2000; Bharath, Dahiya, Saunders, and Srinivasan, 2011). Consequently, facing higher disclose risk of public dissemination of private information, firms would either choose to disclose less private information to new (non-relationship) lenders or only disclose such information to relationship lenders. [11] As a result, non-relationship lenders will incur even higher information production costs (such as the increased collection of soft information) in assessing the creditworthiness of new borrowers. Thus, we hypothesize that borrowers' switching to new (non-relationship) lenders became more costly after Reg FD, since non-relationship lenders would charge higher fees and spreads to compensate for their increased information collection costs and risk.

Moreover, information asymmetries also exist between lead lenders (agents) and participating lenders in loan syndications (see, e.g., Dennis and Mulineaux, 2000; Sufi, 2007). If Reg FD imposed higher information disclosure risk and caused borrowers to convey less information to their lenders, we expect that both borrowers and syndicate participants would have been more inclined to join syndicates with a relationship lead lender. Therefore, we hypothesize that there would have been an increase in relationship banking intensity in the syndicated loan market after the introduction of Reg FD.

In addition, information asymmetries exist between secondary loan market buyers and sellers. Compared to syndicated loan sellers (most commonly, "so called" participant lenders), loan buyers (such as pension funds, foreign banks, and mutual funds) that comprise a significant proportion of the secondary market for syndicated loans, may be relatively less informed. Indeed,

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[11]A strand of research highlights how disclosure risk could impact firms' financing decisions. For example, Yosha (1995) predicts that good quality firms choose to borrow from a single lender because the proprietary information they disclose to lenders could be leaked to product market competitors through multilateral lending agreement. Asker and Ljunqvist (2010) document that, to avoid information leakage, industry competitors would not select the same equity/debt underwriters. These studies suggest that disclosure risk does affect firms' selection of lenders.

Wittenberg-Moerman (2009) identifies several factors that drive loan bid - ask spreads, indicating the level of information asymmetry between loan sellers and buyers. If Reg FD increased the information gap between loan buyers and sellers, we hypothesize that the bid-ask spread on loans traded in the secondary loan market would have increased after Reg FD.

To test the above hypotheses, we conduct a natural experiment by analyzing syndicated loans originated before and after Reg FD over one-year, three-year, and five-year windows centered on the Reg FD passage (event) year 2000. To avoid contamination from the event year 2000, we remove loans originated in 2000. For example, using the one-year window, we compare the characteristics of loans originated in 1999 to those originated in 2001. We focus on the results in the one-year window, where there is less concern about other contaminating events. We also investigate bid-ask spreads in the secondary loan market over one-month windows surrounding the event month October 2000.[12]

Overall, we find that, after the introduction of Reg FD, borrowers needed to pay 31 basis points more in all-in-spread drawn (AISD), representing $465,000 in additional annual interest payments on a typical /median sized loan in our sample, in order to borrow from new (non-relationship) lenders. Importantly, this result remains robust over the one-year, three-year, and five-year windows surrounding the passage of Reg FD, supporting the hypothesis that switching to new lenders became more costly for borrowers in the post- Reg FD period.

Based on our hypothesis of the change in relationship banking intensity associated with loans made to borrowers before and after the introduction of Reg FD, we find that borrowers became more dependent on relationship banking after the passage of Reg FD. This finding is consistent with the notion that, relationship lenders played a more important role in reducing

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[12] Our results on the average loan bid-ask spread are robust for 2-month and 3-month windows around the event month.

borrowers' concerns about private information disclosure risk following the introduction of Reg FD.

Next, focusing on the structure of loan syndications, we find that when forming loan syndicates, lead lenders retained a higher percentage of loans after Reg FD, consistent with an increased need for lead lenders' certification at time of loan origination for participating syndicate lenders.

The evidence from the secondary loan market is also consistent with our prediction that average loan bid-ask spreads would have significantly increased from the month before to the month after the implementation of Reg FD due to greater information asymmetries among loan sellers and buyers.

In order to address the concerns of contaminated events both at the market level (such as macro-economic conditions) and at the corporate level, we also use private firm borrowers (*i.e.*, borrowers that were not subject to SEC Reg FD) as a control group and conduct the same tests on their cost of loans, relationship banking intensity, lead lender share, as well as their secondary market loan bid-ask spreads over the same time horizons.[13] [14]Interestingly, we find no significant effects of Reg FD introduction on private firms in the credit markets. In addition, if we randomly switch the event year/month and conduct a simulated analysis centered on a number of "hypothetical" event years/months, we find that our results are only significant when we use the actual event year/month of Reg FD introduction. [15]

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[13] As will be discussed in more detail later, our use of the private borrower control group also provides a control for the NBER dated recessionary period that occurred March 2001-November 2001. Specifically, both public and private firms were impacted by the recession.

[14] See Steffen and Saunders (2012) for a discussion of private firm borrowers participating in the syndicated loan market.

[15] See Petacchi(2012) for a similar experimental design.

Finally, to further analyze the impact of Reg FD, we also investigate the change in loan announcement effects and firm-wide propensity towards issuing bank debt *vs.* public debt surrounding the introduction of Reg FD. We find that stock prices reacted more strongly to the public announcement of a new loan issue and there was a shift by firms from publicly issued debt to bank debt financing following the adoption of Reg FD.

Our paper provides new insights into the role of relationship banking. The existing theories predicting how relationship banking impacts borrowers have not reached a consensus on whether relationship banking would benefit or impose additional costs on borrowers (*e.g.* Sharpe, 1990; Greenbaum *et al.*,1989; Boot, 2000; Boot and Thakor,1994). We take the adoption of Reg FD as an exogenous information event and show that relationship banking becomes more important when firms face higher private information disclosure risks.

In addition to yielding further insights into the value of banking relationships for borrowers, our "natural experiment" regarding information disclosure costs and incentives, carries an important message for regulators and policy makers. Jorion, Liu and Shi (2005) show that Reg FD's introduction increased information content of credit rating revision. Petachhi (2012) finds that the implementation of Reg FD caused firms to switch from equity financing to debt financing. Our paper complements these findings and provides new evidence on how Reg FD affected credit markets. In particular, we find evidence consistent with the introduction of Reg FD in 2000, increasing information asymmetries among credit market participants, with important effect on loan pricing and firms' choice of debt financing sources.

The remainder of the paper is organized as follows. Section 2 develops our major hypotheses. Section 3 discusses our sample and methodology. Section 4 reports the empirical results while section 5 concludes.

2. Hypothesis development

The main objective of our natural experiment is two-fold. First, we examine how the introduction of Reg FD affected borrowers and lenders in general in credit markets. Second, taking the implementation of Reg FD as a significant exogenous information event, we provide new insights into the role of relationship banking in reducing borrower-lender information production costs. In particular, we seek to answer the question as to whether relationship banks share the benefit, or take an opportunity to exploit borrowers when the loan market is impacted by an exogenous shock regarding the cost of private information distribution for firms.

As discussed above, Reg FD prohibited selective information disclosures. In the private debt market, although communications of private material information to lenders were exempt from Reg FD, issuing firms still faced a greater disclosure risk if there was a breach of confidentiality by the lenders. Ivashina and Sun (2011) provide evidence that institutional lenders trade on private information they receive from participating in loan syndications. Furthermore, Ivashina *et al.* (2009) demonstrate that banks' private information regarding firm health may well be disclosed to bank clients for the purposes of merger and acquisition (M & A) transactions, consistent with the selective disclosure of confidential private information to outside agents. If firms choose to disclose less private information to avoid these types of disclosure risk, or if lenders rely on the selective disclosure of borrowers to obtain firm-specific private information, we expect that after the adoption of Reg FD information asymmetries increased among borrowers and lenders, among lead lenders and participating lenders, and also among loan buyers and sellers in the secondary market for syndicated loans.

To test these predictions, we first investigate the information asymmetry effects of Reg FD on borrowers and lenders by comparing the cost of loans when borrowing from relationship

lenders rather than from non-relationship lenders around Reg FD passage. We expect that relationship lenders were relatively less affected by any reduction in private information disclosures arising from the adoption of Reg FD than non-relationship lenders.

H1. Due to higher private information production costs regarding borrowers faced by non-relationship lenders, switching to new lenders became relatively more costly for borrowers after Reg FD.[16]

Similar to non-relationship lenders, participating syndicate lenders are likely to have been more reluctant to join a loan syndicate without a relationship lead lender after the introduction of Reg FD.

H2. The average relationship banking intensity of borrowers and the average lead lender share in the loan syndications increased after the introduction of Reg FD.

Finally, we examine whether there was an information asymmetry effect in the secondary loan market among loan buyers and sellers. If Reg FD caused a reduction in material private information disclosed to prospective investors (loan buyers), the so-called "chilling effect", we would expect to find loan bid-ask spreads widening after the introduction of Reg FD.[17]

H3: Bid-ask spreads increased in the secondary loan market after the introduction of Reg FD.

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[16] Competing theories have suggested that relationship lending can either increase or decrease the cost of borrowing (See, e.g., Boot and Thakor, 1994; Greenbaum, Kanatas, and Venezia, 1989; Sharpe, 1990). On the one hand, repeated lending can reduce the future information production costs arising from past efforts. When relationship lenders share this saving with borrowers, the cost of debt will be lower. On the other hand, if relationship lenders exploit their information advantage, they may "lock in" borrowers and charge information rent leading to a higher cost of borrowing. However, this information rent extraction effect is more likely to exist in firms with high level of information opacity such as small private firms with very limited access to funds beyond relationship lenders (Bharath *et al*., 2011). Since our sample only consists of public firms, we expect that the benefit of relationship banking dominates the information rent extraction effect.

[17] According to Sufi (2009), the secondary loan market only became active after the introduction of syndicated loan ratings by Moody's and Standard & Poor's in 1995.

We also conduct numerous robustness checks on the above hypotheses and examine how the public *vs.* bank debt structures of firms was affected by Reg FD.

3. Sample and methodology

3.1. Sample

Our initial data sample includes all US dollar *Dealscan* loans issued by U.S. firms between 1995 and 2005. To allow for a period of time during which borrowing firms react to the change in regulation, we omit all loans issued in the year 2000 in which Reg FD was formally discussed and implemented.[18] We choose our sample from 1995 to 2005 to allow a maximum five-year window before and after the event year. Our initial data consists of 60,529 U.S. loan facilities. After matching our sample with Compustat (through a link between *Dealscan* and Compustat provided by Michael Roberts), we are left with a sample of 24,880 observations.[19] Financial and accounting variables from Compustat were retrieved for the last fiscal year end prior to the year of loan deal origination. We further screened the data using the following criteria: (1) omit missing observations on the all-in-spread drawn (AISD); (2) omit missing observations on financial data items needed in the multivariate regression tests; (3) retain only loans with the base rate "LIBOR".[20]

After this screening, we construct three samples for one-year, three-year, and five-year periods surrounding the Reg FD event year 2000 (see Figure 1 for an illustration of the construction of the three samples). To be included in the one-year/three-year/five-year samples, we require that a firm must have issued loan(s) in sample year(s) both before and after the event

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[18] Reg FD took effect on Oct. 23, 2000. Before the formal implementation of Reg FD, the comment period started in December 1999. Accordingly, we assume that firms took some time to adjust their disclosure practices in 2000 and hence we remove loans issued in the year 2000 from the sample.

[19] More information about the link between *Dealscan* and Compustat can be found in Chava and Roberts (2008).

[20] Our results remain robust if we remove all loans issued by financial companies (with 4-digit SIC code 6000-6999) from our sample.

year. Our final one-year sample contains 1,610 loan facilities, while the three-year and five-year sample contain 8,448 and 14,131 loan facilities, respectively.[21][22]

3.2. Construction of lending relationship intensity measures

Following Bharath *et al*. (2011), we construct two borrower-lender relationship intensity variables for each loan based on the borrower's interaction with the syndicated loan lead lenders in a 5-year period prior to the date of loan origination. The first variable is *REL (Number)*$_{il}$ calculated for each loan l lent to firm i. Each syndicated loan has at least one lead bank or, in some cases, multiple lead banks. For each lead bank b lending loan l to firm i, we calculate the following to capture the bank's relationship strength:

$$REL(Number)_{bil} = \frac{Number\ of\ Loans\ by\ Bank\ b\ to\ Firm\ i\ in\ the\ Last\ 5\ Years}{Total\ Number\ of\ Loans\ Made\ to\ Firm\ i\ in\ the\ Last\ 5\ Years}$$

For each loan l, we calculate the above relationship strength for all the lead syndicated lenders involved and the highest value of *REL (Number)*$_{bil}$ across all lead banks is assigned to loan l.

The second variable is *REL (Amount)*$_{il}$ for loan l lent to firm i. Thus, for each bank b, we calculate the following:

$$REL(Amount)_{bil} = \frac{Amount\ of\ Loans\ by\ Bank\ b\ to\ Firm\ i\ in\ the\ Last\ 5\ Years}{Total\ Amount\ of\ Loans\ Made\ to\ Firm\ i\ in\ the\ Last\ 5\ Years}$$

Again, the highest value of *REL (Amount)*$_{bil}$ obtained across all the lead banks is assigned to loan l.

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[21] The number of observations varies in the reported tables depending on the specification of the regression equations.

[22] *Dealscan* does not differentiate between newly originated loans and restated/amended loans. According to Roberts and Sufi (2009), 90% of long-term loan contracts are renegotiated and amended before maturity. We search for SEC filings and identify all restated/amended loans based on the SEC filings for the one-year sample. In the robustness tests reported in Section 4.3.1, we remove all the identified restated/amended loans from the sample and re-do the baseline analysis.

As an example, Crown Pacific Partners borrowed from a loan syndicate on December 1st, 1999. There were four lead banks who led this loan syndication: Bank of America, Bank of Montreal, Union Bank of California, and Key Bank NA. In the past 5 years (from 1995-1999 prior to December 1st, 1999), the borrower borrowed from 15 loan facilities amounting in total to $1.64 billion. Out of these15 loans, Bank of America led 9 loan syndicates amounting to a total of $1.08 billion far more than any other lead banks. Therefore, the relationship intensity *Rel(Number)$_{bil}$* between Bank of America and the borrower equaled 0.6 (9/15=0.6), while the *Rel (Amount)$_{bil}$* equaled 0.655 (1.08/1.64=0.655). Taking the maximum of the relationship intensity measure among lead banks, we assign 0.6 to the relationship intensity measure *Rel(Number)$_{bil}$*, and 0.655 to Rel(Amount)$_{bil}$* for the loan facility to Crown Pacific Partners dated December 1, 1999.

In addition to the above two measures, another variable of interest, *Switch$_{il}$* is assigned to loan *l* made to firm *i* and takes the value of one if none of the lead lenders in a loan syndicate had lent to firm *i* in the last 5 years, and zero otherwise.[23] For the Crown Pacific Partners December 1999 loan, *Switch$_{il}$* equaled zero because two of the lead banks (Bank of America and Bank of Montreal) of the loan syndicate had lent to the borrower in the last 5 years. Finally, we also construct a *Number of Relationship Lenders$_{il}$* variable for each loan *l* made to firm *i* by counting the total number of relationship lead lenders involved in the loan *l* to this borrower. For the December 1999 Crown Pacific Partners loan, the *Number of Relationship Lenders* equaled 2.[24]

When constructing the relationship intensity measures, we take into account any potential impact of bank mergers on banking relationships and recognize the potential inheritance of

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[23] Our *Switch* dummy is a reversal of "relationship dummy" defined in Bharath *et al*. (2011).
[24] The two other lead lenders, bank of California and Key Bank had not been relationship lenders over the prior 5 years.

borrowing-lending relationship from the acquired bank to acquiring bank.[25] As argued in Bharath

et al. (2011) and Schenone (2010), the information that a bank has regarding a client is likely to

be transferred to the merged entity after a bank merge. Finally and importantly, in the robustness

section of the paper, we take into account any biases caused by some loans being restructured

rather than newly originated in our sample.

3.3. Methodology

We first conduct a univariate analysis on the one-year loan origination sample

surrounding the passage of Reg FD (year 2000), *i.e.*, 1999 *vs.* 2001 to show the unconditional

results on how the costs of debt, relationship banking intensity, and the other loan/firm

characteristics evolved over the pre- to post- Reg FD period.

We then employ panel regressions with firm and year fixed effects to specifically test our

three hypotheses.

To test Hypothesis H1, we utilize the following specification in our loan spread

regressions controlling for firm and loan characteristics as well as firm and year fixed effects [26](a

full description of the variables used in these tests can be found in the Table 1):

$$
\begin{aligned}
Spread_{i,l,t} = {} & \beta_0 + \beta_{after}\, after_{i,l,t} + \beta_{switch}\, Switch_{i,l,t} + \beta_{switch\_after}\, Switch_{i,l,t} * after_{i,l,t} \\
& + \beta_{firm\_characteristics}\ Firm\ Characteristics_{i,l,t} + \beta_{loan\_characteristics}\ Loan\ Characteristics_{i,l,t} \qquad (1)\\
& + \beta_{year\_effects}\ Year\ Fixed\ Effects_{i,l,t} + \beta_{firm\_effects}\ Firm\ Fixed\ Effects_{i,l,t} + \varepsilon_{i,t} + \mu_{i,l,t}
\end{aligned}
$$

The dependent variable is the initial loan spread, measured by AISD in basis points. The

main variable of interest is the interaction term *Switch*after,* where *after* equals one if the loan

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[25] Our bank merge data are collected from Thomson-Reuters SDC database. We consider 254 largest bank mergers from 1987 to 2009 with the transaction value greater than $100 million and acquired share greater than 51%. For example, JP Morgan & Co (*Dealscan* lenderid 6531) merged with Chase Manhattan Corp. (*Dealscan* lenderid 6530) in 2000 (effective on December 31, 2000) and formed JP Morgan Chase (*Dealscan* lenderid 38939). In this case, the prior banking relationships developed by JP Morgan & Co and Chase Manhattan Corp. are assumed to be transferred to the new entity JP Morgan Chase after the M & A effective date.

[26] We only control for the firm fixed effects in the 1-year samples.

was originated after Reg FD or zero if before. The coefficient estimate on this interaction term captures the average increase/decrease in loan spread from the pre- to the post- Reg FD period for borrowers switching to new (non-relationship) lenders. We expect that the estimated coefficient on this interaction term to be significant and positive if our hypothesis H1 holds, *i.e.,* switching to new non-relationship lenders became more costly after Reg FD.

To test the first part of our Hypothesis H2, we run banking relationship intensity regressions in two steps. In the first step, we remove the time trend in relationship banking intensity by regressing our relationship banking intensity measure ((*Rel(Number)* or *Rel(Amount)*)) on time *t*. We define *t* equal to 1 in year 1995, and 2, 3, 4..., 11 in years 1996, 1997,…, and 2005, respectively. Then we compute the residuals from these regressions. In the second step, we utilize the following specification regressing the residual of *Rel (Number)* or *Rel(Amount)* on the explanatory variables shown below:

$$
\begin{aligned}
\textit{Residual } (\mathrm{Re}l(\textit{Number})_{i,l,t} \textit{ or } \mathrm{Re}l(\textit{Amount})_{i,l,t}) &= \beta_0 + \beta_{after} \textit{after}_{i,l,t} \\
&+ \beta_{firm\_characteristics} \textit{ Firm Characteristics}_{i,l,t} + \beta_{loan\_characteristics} \textit{ Loan Characteristics}_{i,l,t} \qquad (2) \\
&+ \beta_{year\_effects} \textit{ Year Fixed Effects}_{i,l,t} + \beta_{firm\_effects} \textit{ Firm Fixed Effects}_{i,l,t} + \varepsilon_{i,t} + \mu_{i,l,t}
\end{aligned}
$$

The main variable of interest is the dummy variable *after.* The coefficient estimate on "*after*" captures the average increase/decrease in relationship banking intensity when borrowers issued loans after Reg FD. We expect that the estimated coefficient on "*after*" to be significant and positive if our hypothesis H2 holds, indicating that the reliance of borrowers on relationship lenders became stronger after the introduction of Reg FD.

To test the second part of our Hypothesis H2, we use lead lender share of a syndicated loan retained as the dependent variable in the following specification:

$$
\begin{aligned}
\textit{Lead Lenders Share}_{i,l,t} &= \beta_0 + \beta_{after} \textit{after}_{i,l,t} \\
&+ \beta_{firm\_characteristics} \textit{ Firm Characteristics}_{i,l,t} + \beta_{loan\_characteristics} \textit{ Loan Characteristics}_{i,l,t} \qquad (3) \\
&+ \beta_{year\_effects} \textit{ Year Fixed Effects}_{i,l,t} + \beta_{firm\_effects} \textit{ Firm Fixed Effects}_{i,l,t} + \varepsilon_{i,t} + \mu_{i,l,t}
\end{aligned}
$$

The dependent variable is the loan share (in percentage) held by lead lenders in a loan syndicate. The main variable of interest is the dummy variable "*after*". The coefficient estimate on "*after*" captures the average increase/decrease in loan shares held by lead lenders in loan syndications when borrowers issued loans after Reg FD. We expect t the estimated coefficient on "*after*" to be significant and positive if our hypothesis H2 holds, indicating that lead lenders retained a greater portion of loans after Reg FD's introduction.

Finally, to test hypothesis H3, we conduct a univariate analysis to examine how the average bid-ask spread on the same loans traded on the secondary loan market changed from one month before the event month (October of 2000) to one month after the event month.[27] We expect the average bid-ask spread to have increased after Reg FD, consistent with an increase in the degree of information asymmetry among secondary loan market buyers and sellers. In addition, as private firms are not regulated by the SEC and were not affected by Reg FD, we control for private firms to make sure any increase in the secondary market loan bid-ask spreads was not a general market trend that applied to both groups of firms. We also verify our univariate analysis results using a multivariate regression specification:

$$Bid - Ask \ \ Spread \ = \alpha + \beta \cdot after + \gamma \cdot public + \varphi \cdot after \cdot public + \varepsilon \quad (4)$$

Where "*public*" is an indicator variable taking the value of one if a firm is a public firm, or zero otherwise. We expect the coefficient estimate on the interaction term "*after*public*" to be positive and statistically significant.

4. Empirical analysis

4.1. Univariate results

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[27] To avoid the problem of large sample bias in the mean difference analysis, we first take the average of bid-ask spread for each loan facility included in our sample in pre- and post Reg FD period respectively before conducting the mean difference analysis. To be included in the sample, we require that a loan has to be continuously traded in both the pre- and post- Reg FD periods.

Table 2 presents a comparison of our main variables in the pre- and post- Reg FD years (*i.e.,* 1999 *vs.* 2001) for the 1-year sample. First, the average all-in-spread drawn (AISD) for firms that issued loans both in 1999 and 2001 significantly increased from 146.89 basis points in the pre- Reg FD period (1999) to 163.46 basis points in the post- Reg FD period (2001), which is statistically significant at the 1% level. The variable "*Switch*" indicates whether the borrower received loans from a new lender. Table 2 shows that the percentage of loans that involved new lenders decreased from 39% in the pre- Reg FD period to 24% in the post- Reg FD period and the decrease was statistically significant at the 1% level.

As discussed in section 3.2, we construct measures to reflect the intensity of relationship banking. The first relationship lending measure, *Rel (Number)*, increased in the post- Reg FD period from 0.42 to 0.54 (*significant at the 1% level*), suggesting that firms borrowing in the post Reg FD period involved greater relationship intensity post-Reg FD. Similarly, the second measure *Rel(amount)* increased from 0.45 to 0.57(*significant at the 1% level*). Moreover, the presence of relationship lenders (*Number of Relationship Lenders*) in a syndicate also increased from an average of 1.79 to an average of 2.34 (*significant at the 1% level*). The significant increase in the relationship intensity and the number of relationship lenders in syndicated loans in the period immediately following the adoption of Reg FD suggests that borrowers were more likely to seek lenders with whom they had a long-term relationship. This is because relationship lenders were less likely to divulge new private information to external clients for concern about risking the breaking of their lending relationship and/or because relationship lenders were more able to "reuse" their existing private information about the borrower generated from prior interactions.

Table 2 also shows that the percentage of syndicate loan share held by lead lenders (*Lead Lender Share*) increased from 59.94% (1999) to 64.20% (2001), significant at the 5% level. The increase in *Lead Lender Share* after Reg FD is consistent with the information asymmetry among lead syndicate lenders and participating syndicate lenders increasing after the adoption of the Reg FD.

4.2. Multivariate results

4.2.1. Test of H1: Cost of loans

In H1, we predict that due to the incentives of borrowers to reduce the quantity and quality of information disclosure in the post- Reg FD period, a new (non-relationship lender) faced higher information generating costs than a relationship lender. Consequently, to compensate a new lender for this greater information cost, firms incurred higher costs of borrowing if they switched to a new non-relationship lender after Reg FD.

To test this hypothesis, we regress the AISD on "*after*", "*switch*" and their interaction term "*switch*after*", where "*after*" indicates whether the year of loan origination was in the post-Reg FD period; "*switch*" indicates whether the borrower switched to a new lender; and the variable of interest, "*switch*after*", indicates whether a borrowing firm switched lenders in the post Reg-FD period. We predict a positive and significant coefficient on "*switch*after*". We present results in Table 3. Columns (1), (2), and (3) report the results using the 1-year, 3-year, and 5-year samples, centered on the year of the passage of Reg FD in 2000, as the pre- and post-Reg FD years respectively. The coefficients on the interaction term *switch*after* are positive and significant for the 1 year, 3 year, and 5 year samples. For example, in the 1-year sample, if a borrower switched to a new lender after Reg FD, its cost of borrowing increased 31 basis points (*significant at the 10% level*), representing a $465,000 additional annual interest payment for borrowing a median sized loan in our sample. We also find that switching firms had significantly

higher costs of borrowing in the 3-year and 5-year samples, although the increase was smaller in magnitude in these years than that for the 1-year sample, consistent with the asymmetric information effect weakening over time.

The signs on the other variables are consistent with those documented in the prior literature (*e.g*., Dennis, Nandy and Sharpe, 2000; Bharath *et al*., 2011). For example, in the 1-year sample, we find that borrowers that were required to post collateral (*secured*) paid higher spreads (*coefficient=54.46, significant at the 1% level*). Credit revolvers (*revolver*) were associated with lower interest rates *(coefficient=-33.08, significant at the 1% level*) compared with term loans. In terms of firm characteristics, we find that large firms (*lsale*) were granted loans with a lower AISD, although the coefficient on *lsale* is not significant in the 1-year sample. The leverage ratio is associated with a higher AISD (*coefficient=24.97*, *significant at the 10% level* in the 3-year sample; *coefficient=31.36*, *significant at the 1% level* in the 5-year sample). High market to book and profitability ratios tend to reduce the AISD. Moreover, the greater number of analysts covering a borrowing firm tends to be associated with a lower cost of bank debt. Finally, more lead lenders results in a lower AISD, consistent with a greater diffusion of private information among syndicate lenders.

4.2.2. Test of H2: Relationship banking intensity and lead lender share

In H2, we posit that the Reg FD would have induced a borrower to rely more heavily on relationship lenders who have generated superior and reusable private information in the past about a borrower by bonding through an "implicit" contract formed with a borrower as a result of its interactions through past lending activities. We test this hypothesis by examining whether there was an increase in relationship borrowing in the years after Reg FD. Similar to our analysis in testing H1, we construct three samples with 1-year (column 1 of Table 4), 3-year (column 2), and 5-year (column 3) windows before and after 2000 (the year of Reg FD took effect) as the

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pre- Reg FD and post- Reg FD years, respectively. In Table 4, we report the results using two alternative relationship measures, detrended *Rel(number)* and *Rel(amount)*. As controls, in our multivariate regression model, we include natural logarithm of loan amount (*lloansize*), leverage (*levg*), a non-investment grade indicator variable (*noninvestmentgrade*), a non-rated indicator variable (*notrated*), firm size (*lsale*), market to book ratio (*mkbk*), and profitability (*prof*). Firms that borrow many times have a higher tendency to establish a relationship with a lender and therefore we control for this determinant of relationship intensity by including the number of times a borrower had raised loans (*noofborrow*) in the past. We also control for loan purpose fixed effects, firm fixed effects, and year fixed effects (except in the 1-year sample). We find that relationship intensity increased significantly in the year after 2000 for the 1-year and the 5-year samples (but not the 3-year sample). For example, the increase in the intensity is about 12% for the 1-year sample, using both measures, *significant at the 5% level*. In the 5-year sample, the increase was approximately 7%-9% with both relationship intensity measures significant at the 5% level.

The coefficients on other control variables in the regressions are also consistent with the findings in Gopalan, Udell, and Yerramilli (2011) who examine how relationship lending evolves over a firm's life cycle. For example, both their study and our study find that when firms have higher market-to-book ratios and borrow a larger amount of loans, they rely less on relationship lenders.[28]

In the second part of H2, we hypothesize that a reduction in the incentives to disclose private information will increase information asymmetries among syndicate participants, forcing lead lenders to hold a greater percentage of a syndicated loan amount in the post- Reg FD years

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[28] Gopalan *et al*. (2011) use a dummy variable "*new_relationship*" as the dependent variable, which is the opposite of the relationship intensity we use in this paper. In other words, the firms with lower relationship intensity are firms that are forming new relationships.

than in the pre- Reg FD years. To test this, we analyze whether lead lenders held a significantly greater percentage of a syndicated loan at the time of origination in the year after Reg FD compared to the year before Reg FD, controlling for other lead lender share determinants. We report the results in Table 5. Overall, the results are consistent with hypothesis H2. Specifically, the lead lender share increased 13.34 percentage points in 2001 compared to that in 1999 (*significant at the 5% level*). When the sample period is extended to 3 or 5 years for the pre- and post- periods, the results are weakened. For example, in the 3-year sample, the coefficient on *after* is 5.80 (*significant at the 10% level*) and in the 5-year sample, coefficient on *after* is 5.78 (*significant at the 5% level*).

4.2.3. Test of H3: Loan bid-ask spreads

To examine the impact of Reg FD on the loan trading and prices in the secondary loan market, we calculate the average bid-ask spread for each loan in a 1-month window surrounding Reg FD's introduction, and analyze the change in the loan's bid-ask spread from one month before to one month after the event month (October 2000).[29] We omit the event month to allow secondary market prices a period of time to react to the introduction of Reg FD. To ensure that results are not driven by changes in the composition of loans traded, we construct a sample that requires the same loan facility to be traded in both the pre- and post- event one month windows. As the first test, we report the difference in the average of loan bid-ask spreads between September 2000 and November 2000. As robustness tests, we randomly take other months as hypothetical event months (from October 1999 to October 2001 except for the true event month of October 2000) and apply the same rule to sample selection, *i.e.*, we compare a loan's average

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[29] The results surrounding the "true" event month also hold for in a 2-month and a 3-month windows. That is, when we compare the average bid-ask spread of the traded loans in August and September 2000 and that in November and December 2000 (*i.e.*, the 2-month window), or compare July-September 2000 with November 2000-January 2001 (*i.e.*, the 3-month window), the mean difference test results are all statistically significant.

bid-ask spread for the month before with that for the month after a "hypothetical" event month. In addition, we also use loans to private firms as a control group. Because private firms were not subject to Reg FD, we do not expect to observe any change in the average bid-ask spreads for traded private firm loan facilities surrounding the implementation of Reg FD.

We report the results in Table 6. Panel A presents the univariate analysis of the change in the average loan bid-ask spread in a 1-month window around the true/hypothetical event months. For the loans issued by public firms, the change in the average loan bid-ask spread is significant only in the months immediately surrounding October 2000 (the issuance month of Reg FD). Specifically, from the month (September 2000) before Reg FD was implemented to the month (November 2000) after Reg FD was implemented, the average loan bid-ask spread increased 27.6 basis points and this increase was significant at the 1% level. This change in bid-ask spread is consistent with Reg FD significantly increasing information asymmetry concerns among loan buyers and loan sellers immediately after the implementation of Reg FD in October 2000.

We next look at the change in the bid-ask spread for traded loans issued by private firms over the same period. We find that there was no significant change in these firms' loan bid-ask spreads between September 2000 and November 2000. In Table 6 Panel B, we include both private and public firms in the same multivariate regression and test whether the change in the bid-ask spread was more significant for public firms between the pre- and the post- Reg FD period. The coefficient estimate on the interaction term "*after*public*" is positive and significant at the 5% level (*coefficient=0.099*).

4.3. Robustness tests

4.3.1. Amended/Restated loans

Roberts and Sufi (2009) document that many loans documented in the *Dealscan* database are actually renegotiations of prior agreements (*i.e.*, restated/amended credit agreements) rather

than newly originated loans. Renegotiated /restructured loans may lead to significant changes in loan amounts, maturity, syndicate lenders, and/or loan pricing unrelated to Reg FD's informational effects. To better disentangle the impact of the borrower-lender renegotiations on the loan contract terms from the impact of Reg FD, we collect additional information from the SEC filings (10-Ks, 10-Qs, and 8-Ks) for the one-year sample surrounding Reg FD's introduction. This allows us to better distinguish restated/ amended loans from newly-issued loans.

Specifically, for each loan deal, we employ a text search algorithm and search for the loan borrower's SEC filings by their CIK number in the deal active year using the following keywords: "amendment", "amend", "restatement", "restate", "extend", "extension", "credit", "agreement", "loan" and "facility". To identify a corresponding SEC filing, we first locate the deal active year and the deal active month in the filing, and then search for the above listed keywords one by one around the deal active year and month. Then, we extract the text surrounding the deal active year and month from the filing if one of the keywords appears in the text. By doing so, we are able to identify the related SEC filings for 1,277 loan facilities. Through manually checking the extracted text, we identify 252 restated/amended loans from these 1,277 loan facilities. Removing the 252 restated/amended loans from the sample of loans, we obtain a sample of newly issued loans and redo the tests of H1 to H3 using this more refined sample.

Table 7 shows that the results of the effect of Reg FD on the cost of switching lenders, relationship banking intensity, and the percentage of a loan held by lead bank(s), presented in Tables 3, 4, and 5, remain robust when examining only newly originated loans surrounding the introduction of Reg FD. In particular, Column 1 of Table 7 shows that the estimated coefficient

on the interaction term *switch*after* is statistically significant at the 10% level (*coefficient=38.53 basis points*). Column 2 shows that the relationship banking intensity increased by 14% (significant at the 5% level) post-Reg FD, while Column 3 shows that the percentage of a loan held by lead bank(s) increased on average by 14.97% (significant at the 10% level) post-Reg FD.

4.3.2. "Hypothetical" event year tests and effect of Reg FD on private firms

Because the year of implementation of Reg FD may have been accompanied by other significant economic and regulatory events, we conduct two sets of robustness tests to rule out possible alternative explanations for our findings regarding Hypotheses H1 to H3. In the first set of robustness tests, we follow Petacchi (2012) and use several "hypothetical" event years to test whether a similar change in relationship intensity, lead lender share, and switching costs occurred.[30] In the second set of robustness tests, we use private firms that are not subject to Reg FD as a control group, but would have been subject to similar macro conditions as public firms. The premise of using private firms as a control group is that while economic conditions are likely to affect both private and public firms, Reg FD is expected to have little or no impact on private firms' information disclosure incentives compared to those of public firms.

We report the robustness test results for H1 in Table 8. Panel A of Table 8 compares the result surrounding the true event year (2000) and the results surrounding the "hypothetical" event years. In particular, we use 1996, 1997, 1998, 2002, 2003, and 2004, as the "hypothetical" event years, respectively. For example, when we use 1998 as the "hypothetical" event year, we compare the relevant characteristics of loans issued in 1997 with those issued in 1999.[31] We exclude 1999 and 2001 as "hypothetical" event years because using either one of these years would involve loans issued in 2000 as part of the actual 1-year pre- and post-event samples. We

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[30] The hypothetical event time for the bid-ask spread change is already included in the main analysis.
[31] We only report the switching event year robustness tests results based on the 1-year sample, although using the 3-year sample produces the similar results.

find that the interaction term "*switch*after*" is only positive and statistically significant in column (4), where the change in the switching cost is examined around the true event year (*i.e.* year 2000). When we randomly use other years as "hypothetical" event years, the cost of switching did not increase or decrease for the borrower.

Panel B of Table 8 compares the change in the cost of switching to new lenders between the pre- and post-Reg FD periods for public firms compared to private firms. The estimated coefficient on the interaction term "*switch*after*" is only significant for the sample of public firms.

We report the robustness test results for H2 in Table 9 and Table 10. We find a pattern consistent with the pattern found in the robustness tests of H1. That is, the coefficient on "*after*" is only positive and significant when the event year is the true event year (*i.e.*, 2000). The comparison with private firms also reveals that the lead lender share or relationship banking intensity only increased for loans issued by public firms post-2000.[32]

4.3.3. NBER dated recession period (March 2001-Novermber 2001)

March to November 2001 is identified as an economic recession period by the National Bureau of Economic Research (NBER) Business Cycle Dating Committee. This recession period overlaps with the post-Reg FD period. Therefore, our results might be driven, in part, by a potential confounding effect, *i.e.*, the 2001 economic downturn. We find that, the conditional AISD (after controlling for firm and loan risk characteristics) increased by 24 basis points from

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[32] To address the concern that 9/11 disaster may have a confounding effect on the cost of borrowing, relationship intensity, and lead lender share, we exclude the loans issued after 9/11 for each test. The untabulated results show that the main results hold robust. In addition, instead of focusing on switch dummy, we also investigate how relationship banking intensity (measured by *Rel(Num)* and *Rel (Amount))* affect the cost of borrowing in the pre-Reg FD and post- Reg FD periods. The untabulated results show relationship banking intensity helps reduce the cost of debt more significantly in the post-Reg FD period. Lastly, our main results remain robust after we remove all the financial firms (with SIC code ranging from 6000 to 6999) from our sample. All these additional robustness results are available upon request.

1999 to 2001 (*statistically significant at the 1% level*) for public company borrowers and the cost to private borrower increased by 28 basis points (significant at the 10% level). However, the difference in the increase in cost for private firms relative to public firms was not statistically significant.[33] Therefore, we use private firms as a control group and analyze the change in the cost of switching lenders from the pre- to post-Reg FD period. By doing so, we are able to disentangle the effect of Reg FD from that of the 2001 economic downturn. As described above, Table 8 Panel B shows that the estimated coefficient on the interaction term "*switch*after*" is only significant for the sample of public firms but not for the sample of private firms. As an additional test, we pool the samples of public and private firms together and conduct a three dimension difference in differences analysis. In Table 11, we show that, after control for "*public*", "*switch*", and "*after*" dummies, and the three double interaction terms "*private*after*", "*public*switch*", and "switch* after", the estimated coefficient on the triple interaction term "*public*switch*after*" is positive and statistically significant (*46.8 basis points, significant at the 5% level*), suggesting that the results we obtain on the cost of switching lenders is robust when we use private firms as a control group.

4.3.4. Loan announcement effects

In Table 12, we examine whether reduced private information flows by borrowers to lenders after the passage of Reg FD caused greater new "loan announcement effects" on borrower stock returns in the post- Reg FD period. We use November 1, 1999 to September 1, 2000 as the pre- Reg FD period and January 1, 2000 to August 31, 2001 as the post- Reg FD period. Consistent with a greater loan certification effect by lead lenders, we find that in the pre- Reg FD period, market-adjusted announcement returns were not significant for event windows (-3, 3), (-1, 1), and (0, 1) days, centered on the deal active date. However, in the post- RegFD

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[33] These results are available upon request.

period, the market adjusted announcement returns were respectively 0.99%, 0.67%, and 0.55%

for the (-3, 3), (-1, 1), and (0, 1) day windows, which are all significant at the 5% level or better.

This is consistent with the market response to new loan announcements being stronger and more

significant in the post-Reg FD period than in the pre- Reg FD period, with a mean difference of

0.8%, 0.52%, and 0.49% for returns over the (-3,3), (-1,1), and (0,1) day windows. That is, a

bank's new loan announcement carried a stronger information effect in the post Reg-FD period

than in the pre-Reg FD period. This finding is also consistent with Jorion *et al.*'s (2005) finding

that credit rating revisions triggered stronger stock price reaction in the post-Reg FD period.

4.3.5. Propensity to issue public debt vs. bank debt

It has been assumed that banks are better able to address the asymmetric information

problem because they are more efficient and effective monitors than the other lenders (Leland

and Pyle, 1977; Diamond, 1984; and Boyd and Prescott, 1986). Thus, firms with higher levels of

information asymmetry are more likely to borrow from banks. Denis and Mihov (2003) show

evidence that firms' level of information asymmetry will impact their choice of debt financing.

In particular, they document that large firms with a higher proportion of fixed assets to total

assets (*i.e.*, greater tangibility) tend to borrower from the public debt market. If Reg FD

increased information asymmetries among lenders and borrowers, we would expect to see a shift

from public debt to bank debt following the adoption of Reg FD.[34]

To test this prediction, we collect all non-convertible new public bond issues made by

non-financial firms from Thomson Reuters SDC database and their syndicated loans over

the1995-2005 period. We construct 1-year and 2-year samples surrounding the Reg FD's

introduction year (year 2000). To be included in the samples, we require a firm to issue at least

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[34] While Petacchi (2012) analyzes the shift from equity to debt in a firm's capital structure following the adoption of
Reg FD, we expect a shift by firms from issuing public debt to bank debt.

one type of debt in both pre- and post-Reg FD periods. As such, we find that there were 242

public bond issues and 386 bank loan issues to 244 firms in 1999 (*i.e.* pre-Reg FD period in the

1-year sample), compared to 196 public bond issues and 506 bank loan issues to these firms in

2001 (*i.e.* post-Reg FD period).

Next, we conduct a *Probit* regression analysis. [35] The dependent variable is an indicator

variable that takes the value of one if a firm received bank debt or zero if it received public debt.

The results are reported in Table 13. In columns (1) and (2), we show that borrower propensity

for raising bank debt significantly increased in the post-Reg FD periods (*coefficient on*

"after"=0.17, significant at the 10% level in the 1-year sample; *coefficient on "after"=0.28,*

significant at the 5% level in the 2-year sample). Interestingly, in columns (3)-(5), we interact

borrower's size, borrower's z dummy (an indicator variable taking the value of one if a

borrower's Altman's Z score is less than 1.81, or zero otherwise), and borrower's credit rating

(non-investment grade indicator) with the "*after*" dummy. We find that the coefficient on

"*lsale*after*" is negative (-0.26) and significant at the 1% level suggesting that larger firms were

less likely to switch from public debt to bank debt after Reg FD's introduction than smaller

firms. In addition, the coefficients on "*z dummy* after*" and "*non-investmentgrade * after*" are

negative and significant at the 10% level suggesting that the propensity of raising bank debt

increased to a greater extent for firms with lower credit worthiness (i.e., lower Altman's Z score

or lower credit rating).

In an unreported table, we conduct the same analysis on 1-year samples surrounding

'hypothetical' event years. We find that an increase in the propensity of receiving bank debt was

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[35] The results remain robust if we include private placements and debt issues under the rule 144A and treat these two types of issues as well as private loan issues as private debt. We find that there was a shift by firms from issuing public debt to private debt following the adoption of Reg FD.

only present when we compare the immediate pre- and post-Reg FD period. For other "hypothetical" event years, we either find an insignificant coefficient or a significantly negative coefficient on "*after*" (*i.e.* decreased propensity of issuing bank debt).

5. Conclusion

In this paper, we investigate whether the implementation of Reg FD had implications for credit markets and in particular on borrower-lender participants in the loan market. This topic has been overlooked in the literature mainly because Reg FD grants an exemption for selective disclosures of private information to creditors. However, we argue that Reg FD imposed an additional disclosure risk on potential borrowers because they needed to make any selectively-disclosed private information public if such information transferred to lenders was then directly or indirectly disclosed to selected investors or clients (see, e.g., Ivashina *et al*., 2009; Ivashina and Sun, 2011; Massoud *et al*., 2011). Under such circumstances, we hypothesize that borrowers would seek to rely more on existing relationship lenders because borrowers have formed an implicit contract or bonding with them over time. Moreover, because of the reusability of the information relationship lenders have gathered through past lending activities, relationship lenders are less reliant on the provision of new private information by a borrowing firm.

Our empirical results show that, after Reg FD, it was more costly in terms of AIDS for borrowers to switch to non-relationship lenders. We take this as evidence that non-relationship lenders had less access to new private information and therefore needed to incur higher costs to produce new information which was reflected in borrower loan rates. In addition, we find that, when syndicating a loan, lead lenders tended to keep a higher share of the loan to address increased asymmetric information concerns among syndicate members, in particular, between lead and participant lenders. We also show that, the average loan bid-ask spread (a direct

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measure of the degree of information asymmetry among loan buyers and sellers) increased from

the month before to the month after the implementation of Reg FD. Finally, we show that Reg

FD had a direct effect on firms' choice of debt financing sources, *i.e.*, firms became more reliant

on bank debt financing than public debt financing. Overall, our empirical evidence suggests that

the adoption of Reg FD, as an information disclosure event, had a material effect on credit

markets, banks and borrowers in addition to equity markets, and that relationship banking was

significantly benefitted.

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Figure 1. Construction of Our Samples

This figure illustrates how we construct the 1-year, 3-year, and 5-year samples. We exclude loans issued in the year 2000 from each sample.
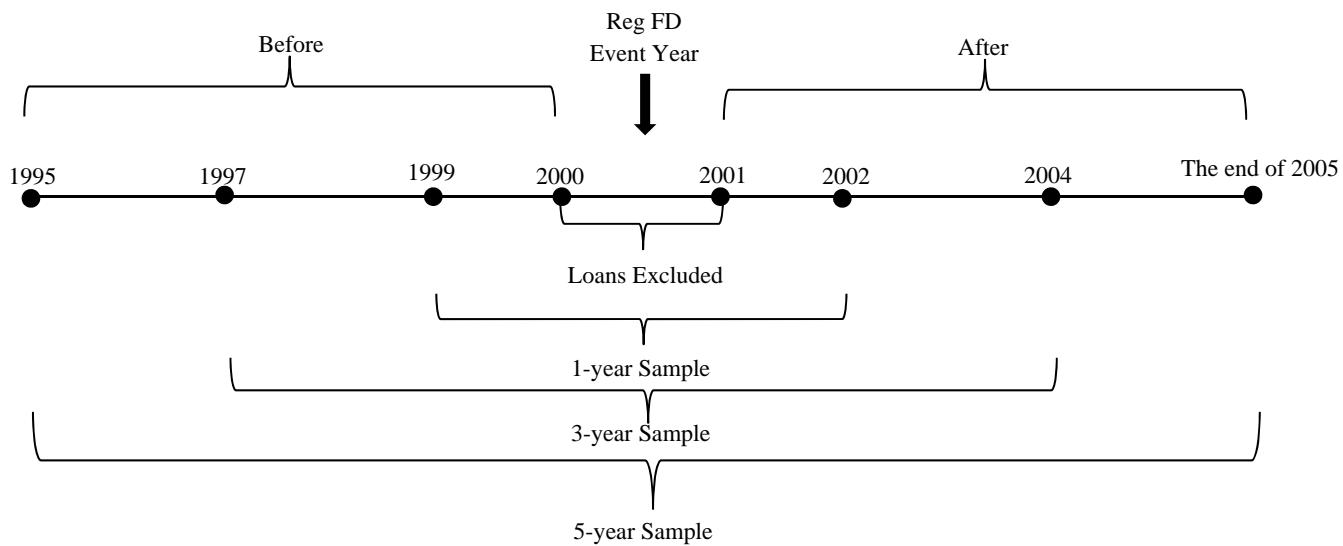
Table 1: Definition of Variables

This table provides detailed definitions for all the variables used in the paper.

Variables	Definition
Variables of Interest	
All-in-Spread Drawn	Initial all-in-spread drawn is defined as the basis point coupon spread over LIBOR plus the annual fee and the upfront fee spread, if there is any.
Switch	An indicator variable takes a value of one if none of the lenders in a loan syndicate is relationship lender, or zero otherwise.
Relationship Intensity (Number)	For each lead lender *b* involved in a loan facility issued by firm i, we calculate the following: $$REL(Number)_b = \frac{Number\ of\ Loans\ by\ Bank\ b\ to\ Firm\ i\ in\ the\ Last\ 5\ Years}{Total\ Number\ of\ Loans\ Made\ to\ Firm\ i\ in\ the\ Last\ 5\ Years}$$ For each loan facility, we assigned the highest value obtained across all the lead lenders to the variable Relationship Intensity (Number).
Relationship Intensity (Amount)	For each lead lender b involved in a loan facility issued by firm i, we calculate the following: $$REL(Amount)_b = \frac{Amount\ of\ Loans\ by\ Bank\ b\ to\ Firm\ i\ in\ the\ Last\ 5\ Years}{Total\ Amount\ of\ Loans\ Made\ to\ Firm\ i\ in\ the\ Last\ 5\ Years}$$ For each loan facility, we assigned the highest value obtained across all the lead lenders to the variable Relationship Intensity (Number).
No. of Relationship Lenders	The total number of relationship lenders involved in a loan facility.
Lead lenders Share	Percentage of a loan held by lead lender(s) in a loan syndicate.
After	An indicator variable takes a value of one if a loan facility is issued after Reg FD, or zero otherwise.
Loan Characteristics	
lloansize	Natural logarithm of the loan facility amount in 2006 dollars.
Lmat	Natural logarithm of loan maturity in months.
secured	An indicator variable takes a value of one if a loan is secured, and zero otherwise.
missingsecured	An indicator variable takes a value of one if the secured status of a loan is missing, and zero otherwise.
revolver	An indicator variable takes a value of one for revolving credit, and zero for term loans.
nooflead	Natural logarithm of one plus the total number of lead lenders involved in a loan facility.
noofborrows	The number of times a borrower has made loan deals in the past.
Borrower Characteristics	
Lsale	Natural logarithm of the borrower's total sales.
Levg	The borrower's book value of total debt over book value of total assets.
Mkbk	The borrower's market-to-book ratio, calculated as (TA+MKVALF-CEQ)/TA, where TA is the book value of total assets, MKVALF is the market value of the firm at the fiscal year end, and CEQ is the book value of total common equity.
Prof	The borrower's profitability ratio calculated as EBITDA/SALES, where EBITDA is earnings before interest, tax, and depreciation & amortization.
investment grade	An indicator variable takes the value of one if a loan borrower has an S & P long term senior debt rating of "BBB" and above, or zero otherwise.
not rated	An indicator variable takes the value of one if a loan borrower does not have an S & P long term senior debt rating, or zero otherwise.
analysts	The total number of financial analyst forecasts.
z dummy	Altman bankruptcy Z-score is calculated Z = 1.2 X1 + 1.4 X2 + 3.3 X3 + 0.6 X4 + 1.0 X5 where X1 is working capital/total assets, X2 is retained earnings/total assets, X3 is earnings before interest and taxes/total assets, X4 is market value equity/book value of total liabilities, and X5 is sales/total assets (Altman, 1968). z dummy is an indicator variable that takes the value of 1 if z<1.81, or 0 if z>=1.81.
Tangibility	Firm's Property, Plant, and Equipment (PPE) over total assets.

Table 2. Summary Statistics before and after Regulation FD

This table provides summary statistics of main variables in our multivariate analysis based on the 1-year sample from 1999 to 2001. Loans issued in the year 2000 are excluded from the sample. The 1-year sample is constructed by selecting firms that issued at least one or more loans in both the pre- and post-Reg FD period. We report the comparison of loan/firm characteristics in the pre- and post- Reg FD periods. While the column "*Before*" provides the summary statistics for the pre-Reg FD period (1999), the column "*After*" provides the summary statistics for the post-Reg FD period (2001). Definitions of all the variables are reported in Table 1. The column "*Tests*" reports the mean difference *t-test* statistics and the *ranksum Z* statistics. *, **, and *** indicates significant at 10%, 5% and 1% level respectively.

Variable	Before				After				Tests	
	mean	Median	s.d.	obs.	mean	median	s.d.	obs.	Mean Difference	Rank Sum
Main Variables of Interest										
All-in-spread drawn	146.89	115.00	113.61	919	163.46	125.00	131.09	882	[2.87]***	[2.36]**
Switch	0.39	0.00	0.4886	919	0.24	0.00	0.4275	882	[-7.04]***	[-6.94]***
Rel(Num)	0.42	0.33	0.40	919	0.54	0.57	0.38	882	[6.17]***	[5.94]***
Rel(Amount)	0.45	0.45	0.42	919	0.57	0.66	0.39	882	[6.15]***	[5.8]***
Number of Relationship Lenders	1.79	1.00	2.75	919	2.34	1.00	3.07	882	[4.04]***	[6.16]***
Lead Lenders Share	59.94	60.00	26.22	284	64.20	64.86	22.27	255	[2.02]**	[1.87]*
Loan Characteristics										
lloansize	19.046	19.114	1.3727	919	19.206	19.186	1.4061	882	[2.45]**	[2.54]**
lmat	3.3385	3.5835	0.8382	775	3.1503	3.1781	0.8117	845	[-4.59]***	[-4.58]***
secured	0.3493	0	0.477	919	0.3333	0	0.4717	882	[-0.71]	[-0.71]
missingsecured	0.4266	0	0.4948	919	0.4592	0	0.4986	882	[1.39]	[1.39]
revolver	0.6594	1	0.4742	919	0.7404	1	0.4387	882	[3.76]***	[3.74]***
nooflead	1.877	1.9459	1.0719	919	1.9374	2.0794	0.9535	882	[1.26]	[1.03]
noofborrows	3.9924	3	3.2127	919	5.7483	5	3.9003	882	[10.45]***	[12.03]***
Firm Characteristics										
lsale	7.1103	7.2219	1.8001	909	7.627	7.7025	1.6546	882	[6.32]***	[5.87]***
levg	0.2982	0.2816	0.2233	908	0.2989	0.2712	0.2052	882	[0.06]	[0.53]
mkbk	1.7661	1.4507	1.0691	919	1.6508	1.2606	1.1684	882	[-2.19]**	[-5.89]***
prof	0.1954	0.1588	0.1771	919	0.1998	0.1527	0.1702	882	[0.54]	[-0.51]
investment grade	0.3493	0	0.477	919	0.4546	0	0.4982	882	[4.58]***	[4.56]***
notrated	0.4298	0	0.4953	919	0.2562	0	0.4368	882	[-7.88]***	[-7.75]***
analysts	2.6627	0	6.6293	919	4.6281	0	7.3982	882	[5.94]***	[7.54]***

Table 3. The Costs of Switching Lenders before and after Regulation FD

This table reports the test results of H1 predicting the changes in the costs of switching lenders in the pre- and post- Reg FD periods. We control for both year fixed effects and firm fixed effects in these regressions. The dependent variable is all-in-spread drawn (*AISD*) in basis points. We report the results based on the 1-year, 3-year, and 5-year samples in columns (1), (2), and (3), respectively. Sample periods are indicated in the first row. Loans issued in the year 2000 are excluded from all the samples. To be included in the sample, we require a firm to issue loans in both the pre- and post-Reg FD periods. Definitions of all the variables are reported in Table 1. The main variable of interest is the interaction term "*switch*after*". Superscripts ***, **, * correspond to statistical significance at the 1%, 5%, and 10% levels with standard error clustered at firm level, respectively.

Regression Variables	(1) 1999-2001		(2) 1997-2003		(3) 1995-2005	
after	24.48***	[6.814]	25.66***	[4.756]	49.17***	[7.104]
switch	-7.65	[8.172]	-5.08	[3.898]	-2.77	[2.823]
*switch*after*	*31.14**	*[16.379]*	*10.70**	*[5.895]*	*11.45***	*[4.554]*
lloansize	-4.82	[3.358]	-9.71***	[1.856]	-10.85***	[1.556]
lmat	9.42**	[4.086]	5.36**	[2.245]	3.99**	[1.686]
secured	54.46***	[12.918]	43.42***	[5.078]	47.71***	[3.620]
missingsecured	5.70	[6.616]	-0.10	[3.683]	4.08*	[2.352]
revolver	-33.08***	[7.004]	-39.00***	[2.891]	-42.02***	[2.301]
llender	-0.96	[6.540]	-5.90***	[2.134]	-6.72***	[1.550]
lsale	-4.07	[14.460]	-5.19	[4.709]	-7.92**	[3.272]
levg	-5.41	[34.419]	24.87*	[13.254]	31.36***	[9.842]
mkbk	-19.13***	[5.307]	-8.17***	[2.736]	-7.27***	[2.110]
prof	-56.16	[67.707]	-79.93***	[26.989]	-114.40***	[22.554]
investmentgrade	-22.64	[18.665]	-48.62***	[9.193]	-33.65***	[5.840]
notrated	4.33	[16.031]	-7.81	[6.493]	-2.79	[4.939]
analysts	-0.65	[0.531]	-0.78**	[0.319]	-0.93***	[0.253]
Loan Purposes	Yes		Yes		Yes	
Firm Fixed Effects	Yes		Yes		Yes	
Year Fixed Effects	No		Yes		Yes	
Observations	1,609		8,405		14,071	
R-squared	0.86		0.75		0.70	

Table 4. Change in Relationship Banking Intensity before and after Reg FD

This table reports the test results of H2 predicting the effect of Reg FD on the relationship banking intensity. We control for both year fixed effects and firm fixed effects in these regressions. The dependent variable for the regressions is the residual relationship intensity computed by regressing relationship banking intensity measured by *Rel(Num)* or *Rel(Amount)* on a time trend t (t=1...11 for year 1995 to 2005). We report the regression results based on the 1-year, 3-year, and 5-year samples in columns (1), (2), and (3), respectively. Sample periods are indicated in the first row. Loans issued in the year 2000 are excluded from all the samples. To be included in the sample, we require a firm to issue loans in both the pre- and post-Reg FD periods. Definitions of all the variables are reported in Table 1. Superscripts ***, **, * correspond to statistical significance at the 1%, 5%, and 10% levels with standard error clustered at firm level, respectively.

Variables	(1) 1999-2001 *Rel(Num)*		*Rel(Amount)*		(2) 1997-2003 *Rel(Num)*		*Rel(Amount)*		(3) 1995-2005 *Rel(Num)*		*Rel(Amount)*	
after	*0.12***	*[0.056]*	*0.12***	*[0.058]*	*0.04*	*[0.035]*	*0.02*	*[0.036]*	*0.09****	*[0.032]*	*0.07***	*[0.033]*
lloansize	0.02	[0.016]	0.03*	[0.018]	0.03***	[0.007]	0.05***	[0.007]	0.03***	[0.005]	0.04***	[0.005]
levg	0.19	[0.203]	0.22	[0.216]	0.18***	[0.067]	0.19***	[0.071]	0.03	[0.052]	0.04	[0.054]
noninvestmentgrade	-0.13	[0.102]	-0.18*	[0.102]	-0.10***	[0.034]	-0.12***	[0.037]	-0.09***	[0.027]	-0.10***	[0.028]
notrated	-0.25***	[0.092]	-0.27***	[0.096]	-0.10***	[0.039]	-0.12***	[0.040]	-0.10***	[0.029]	-0.10***	[0.030]
lsale	0.02	[0.051]	0.02	[0.054]	-0.02	[0.019]	-0.01	[0.021]	0.00	[0.013]	0.01	[0.014]
mkbk	-0.03	[0.027]	-0.04	[0.028]	0.03***	[0.008]	0.03***	[0.008]	0.01	[0.007]	0.01*	[0.008]
prof	0.15	[0.269]	0.28	[0.287]	-0.03	[0.111]	0.02	[0.114]	0.00	[0.079]	0.01	[0.084]
noofborrow	-0.02	[0.019]	-0.01	[0.020]	0.01	[0.007]	0.01	[0.008]	0.00	[0.005]	0.01	[0.005]
Loan Purposes	Yes		Yes		Yes		Yes		Yes		Yes	
Firm Fixed Effects	Yes		Yes		Yes		Yes		Yes		Yes	
Year Fixed Effects	No		No		Yes		Yes		Yes		Yes	
Observations	1,449		1,449		7,370		7,370		12,359		12,359	
R Squared	0.69		0.69		0.54		0.54		0.45		0.46	

Table 5. Change in Lead Lenders Share before and after Reg FD

This table reports the test results for the second part of H2 predicting the effect of Reg FD on loan share held by lead banks in loan syndicates. We control for both year fixed effects and firm fixed effects in these regressions. The dependent variable for the regressions is the loan share held by lead banks in a loan syndicate. We report the results based on the 1-year, 3-year, and 5-year samples in columns (1), (2), and (3), respectively. Sample periods are indicated in the first row. Loans issued in the year 2000 are excluded from all the samples. To be included in the sample, we require a firm to issue loans in both the pre- and post-Reg FD periods. Definitions of all the variables are reported in Table 1. Superscripts ***, **, * correspond to statistical significance at the 1%, 5%, and 10% levels with standard error clustered at firm level, respectively.

Variables	(1) 1999-2001		(2) 1997-2003		(3) 1995-2005	
after	*13.34***	*[6.098]*	*5.80**	*[3.134]*	*5.78***	*[2.464]*
lloansize	-1.69	[2.875]	2.20**	[0.887]	2.18***	[0.720]
lmat	3.08	[2.033]	0.59	[1.052]	-0.40	[0.756]
revolver	-1.91	[4.221]	-0.58	[1.483]	-0.82	[1.066]
levg	31.97	[42.708]	0.26	[9.052]	4.09	[5.644]
lsale	-8.92	[10.451]	0.08	[2.392]	-0.99	[1.437]
mkbk	0.38	[4.700]	-1.38	[1.142]	-0.74	[0.817]
Credit Ratings	Yes		Yes		Yes	
Loan Purposes	Yes		Yes		Yes	
Year Fixed Effects	No		Yes		Yes	
Firm Fixed Effects	Yes		Yes		Yes	
Observations	527		2,571		4,334	
R-squared	0.80		0.65		0.58	

Table 6. Change in Bid-Ask Spread of the Loans Traded on the Secondary Loan Market

Panel A of this table reports the univariate test result for H3 predicting the change in average loan bid-ask spread for the pre-Reg FD month to the post-Reg FD month. We also compare the result surrounding the true event month with those surrounding the "hypothetical" event months. Sample period for this table is from September 1999 to November 2001. This analysis is conducted for public firm loans and private firm loans, respectively. Using the daily quotation of the bid-ask spread, we first calculate the monthly average bid-ask spread for each loan facility traded on the secondary loan market. Change in bid-ask spread refers to the mean difference in the average loan bid-ask spread form the pre-event month to the post-event month (true or "hypothetical" event months). To ensure the facilities are comparable before and after the event month, we require facilities to be traded in both the pre- and post-event months. For example, when October 1999 is considered as an event month, the changes in bid-ask spread is calculated by comparing the average bid-ask spreads of the facilities traded in November 1999 and those of the same facilities traded in September 1999. Mean difference *t-stat* is reported in the column next to "Chg. Bid-Ask Spread" column. Superscripts ***, **, * correspond to statistical significance at the 1%, 5%, and 10% levels with standard error clustered at firm level, respectively. In Panel B, we report the difference in differences regression analysis results for public firm loans and private firm loans surrounding the true event month, October 2000. The difference in differences regression equation is as follows:

$$Bid - Ask \ Spread = \alpha + \beta \cdot after + \gamma \cdot public + \varphi \cdot after \cdot public + \varepsilon$$

Panel A: Univariate Analysis

Event Month	Public Firms			Private Firms		
	Chg. Bid-Ask Spread	t-Stat	Obs.	Chg. Bid-Ask Spread	t-Stat	Obs.
Oct-99	0.091	0.940	499	0.178	1.395	269
Nov-99	0.078	0.783	509	0.136	1.057	280
Dec-99	-0.025	-0.267	552	0.010	0.080	288
Jan-00	0.038	0.400	563	-0.040	-0.320	306
Feb-00	0.102	1.081	571	0.088	0.707	316
Mar-00	0.139	1.412	589	0.163	1.267	330
Apr-00	0.128	1.280	613	0.097	0.749	341
May-00	0.066	0.649	622	-0.005	-0.038	350
Jun-00	0.041	0.379	628	-0.030	-0.228	358
Jul-00	-0.032	-0.320	657	0.115	0.880	374
Aug-00	-0.053	-0.553	663	0.284**	1.968	366
Sep-00	0.141	1.462	685	0.302*	1.956	363
OCT-00 (Reg FD)	**0.276***	2.801	690	0.201	1.224	370
Nov-00	0.175*	1.700	689	0.148	0.882	360
Dec-00	0.106	1.019	691	0.119	0.700	362
Jan-01	0.079	0.737	697	0.188	1.085	364
Feb-01	0.048	0.454	712	0.227	1.357	371
Mar-01	0.040	0.392	720	0.320*	1.796	373
Apr-01	-0.010	-0.105	755	0.213	1.241	382
May-01	-0.071	-0.730	753	0.043	0.235	387
Jun-01	0.034	0.362	772	0.127	0.697	392
Jul-01	-0.010	-0.107	790	0.031	0.170	397
Aug-01	0.042	0.447	804	0.010	0.055	397
Sep-01 (911)	**0.380***	4.069	819	**0.370***	**2.036**	397
Oct-01	0.191***	2.063	815	0.269	1.558	402

Panel B: Comparing Public with Private Firms

Event Month	Dependent Variable: Bid-Ask Spread		
	Variables	Coef.	t-stat
Oct-00	after	0.15***	4.15
	public	-0.327***	-11.38
	after*public	0.099**	2.37
	constant	1.674***	67.44
obs.		43,923	
R-squared		0.01	

Table 7. Robustness Test: Remove Amended/Restated Loans

This table reports the robustness test results for the effect of Reg FD on the cost of switching lender, relationship banking intensity, and percentage of loan share held by lead bank(s). Sample period is from 1999 to 2001(i.e. the 1-year sample). Loans issued in the year 2000 are excluded from all the samples. We remove all restated/amended loans from the 1-year sample and redo the tests reported in Column (1) of Tables 2, 3, and 4. We control for firm fixed effects and loan purpose fixed effects in all the regressions. The dependent variable in Column (1) is All-in-spread Drawn (AISD). The dependent variable in Column (2) is the residual computed by regressing relationship banking intensity measured by *Rel(Num)* or *Rel(Amount)* on a time trend t (t=1...11 for year 1995 to 2005). The dependent variable in Column (3) is the loan share held by lead banks in a loan syndicate (in percentage). Definitions of all the variables are reported in Table 1. Superscripts ***, **, * correspond to statistical significance at the 1%, 5%, and 10% levels with standard error clustered at firm level, respectively.

Regression	(1)		(2)				(3)	
							Lead Lender Share	
Variables	AISD		Rel(Num)		Rel(Amount)		(%)	
after	*19.93***	*[8.753]*	*0.14***	*[0.067]*	*0.14***	*[0.070]*	*14.97**	*[7.786]*
Switch	-10.57	[12.054]						
*switch*after*	*38.52**	*[23.028]*						
Lloansize	-8.58	[5.381]	0.005	[0.027]	0.02	[0.032]	-2.3	[3.046]
Lmat	16.38**	[7.503]					3.82	[2.346]
Secured	46.70**	[19.367]						
Missingsecured	-1.65	[9.002]						
Revolver	-36.09**	[14.749]					-1.82	[3.144]
Llender	-1.93	[8.766]						
Lsale	4.35	[19.831]	0.002	[0.074]	0.001	[0.079]	-12.99	[12.803]
Levg	-27.72	[69.613]	0.36	[0.315]	0.401	[0.323]	44.73	[57.334]
Mkbk	-12.16	[8.629]	-0.07**	[0.035]	-0.07**	[0.037]	-0.34	[6.136]
Prof	-116.99	[78.379]	0.31	[0.401]	0.37	[0.410]		
Investmentgrade	5.05	[25.191]	-0.04	[0.094]	-0.07	[0.099]		
Notrated	8.74	[19.944]	-0.25**	[0.115]	-0.26**	[0.118]		
Analysts	-0.57	[0.668]						
Noofborrow			-0.02	[0.021]	-0.01	[0.023]		
Credit Ratings	No		No		No		Yes	
Loan Purposes	Yes		Yes		Yes		Yes	
Firm Fixed Effects	Yes		Yes		Yes		Yes	
Observations	906		843		843		329	
R-squared	0.88		0.76	0.76	0.76		0.86	

Table 8. Robustness Test: The Costs of Switching Lenders-Switching Event Year & Comparing Public Firms with Private Firms

Panel A examines the changes in the costs of switching lenders between the pre- to the post-event periods. We compare the results surrounding the true event year (Column (4)) and those surrounding the "hypothetical" event years. The event years and sample periods are indicated in the first row. Loans issued in the event year (either true or "hypothetical") are excluded from each sample. We control for firm fixed effects and loan purpose fix effects in these regressions. The dependent variable is all-in-spread drawn (AISD) in basis points. The regression results reported are based on the 1-year sample. To be included in the sample, we require a firm to issue loans in both the pre- and post-event periods. Panel B compares the changes in the costs of switching lenders of public firms with that of private firms surrounding the true event year 2000. Definitions of all the variables are reported in Table 1. The main variable of interest is the interaction term "*switch*after*". Superscripts ***, **, * correspond to statistical significance at the 1%, 5%, and 10% levels with standard error clustered at firm level, respectively.

Panel A: Switching Event Year

	(1)		(2)		(3)		(4)		(5)		(6)		(7)	
Event Year	1996		1997		1998		**2000**		2002		2003		2004	
Period	1995-1997		1996-1998		1997-1999		1999-2001		2001-2003		2002-2004		2003-2005	
after	-3.35	[5.312]	2.21	[7.095]	40.26***	[6.365]	24.48***	[6.814]	21.74***	[6.388]	22.91***	[6.977]	-52.57***	[7.093]
Switch	-2.65	[8.645]	-2.92	[8.738]	-12.51	[7.777]	-7.65	[8.172]	-2.36	[10.569]	-8.25	[8.980]	18.59*	[10.047]
switch*after	*-14.68*	*[11.549]*	*5.83*	*[11.999]*	*10.33*	*[10.632]*	*31.14**	*[16.379]*	*20.32*	*[14.179]*	*20.24*	*[18.125]*	*1.71*	*[16.271]*
Lloansize	-3.97	[3.393]	-12.34***	[3.814]	-3.89	[2.761]	-4.82	[3.358]	-8.83**	[3.911]	-7.27	[4.676]	-11.96***	[3.553]
Lmat	0.21	[4.385]	-1.33	[4.150]	5.41	[3.750]	9.42**	[4.086]	13.21***	[3.830]	9.88**	[4.134]	10.56**	[4.625]
Secured	23.80**	[11.653]	26.34***	[9.787]	23.59***	[8.555]	54.46***	[12.918]	34.53**	[14.240]	20.64*	[11.144]	37.31***	[10.375]
missingsecured	-8.02	[8.032]	13.14*	[7.918]	-1.03	[6.498]	5.70	[6.616]	-2.04	[7.430]	-8.47	[6.789]	14.33**	[6.838]
Revolver	-25.83***	[5.152]	-26.20***	[4.277]	-24.36***	[4.041]	-33.08***	[7.004]	-43.15***	[7.226]	-37.95***	[6.951]	-48.65***	[6.304]
Llender	-6.36**	[3.184]	-5.23	[3.630]	2.30	[3.162]	-0.96	[6.540]	-5.61	[6.014]	-9.22	[6.256]	-12.76***	[4.888]
Lsale	-10.21	[7.310]	-6.57	[5.514]	-3.06	[6.587]	-4.07	[14.460]	-30.87*	[16.997]	-25.67*	[14.506]	25.55	[16.240]
Levg	17.54	[28.181]	33.92	[30.972]	28.04	[27.869]	-5.41	[34.419]	43.46	[56.423]	27.36	[32.458]	-57.39	[49.919]
Mkbk	-1.91	[6.827]	-8.10**	[3.605]	-12.92**	[5.967]	-19.13***	[5.307]	-3.87	[3.091]	-11.97	[7.964]	-17.30*	[10.360]
Prof	-21.58	[36.379]	-11.52	[40.038]	-48.10	[55.274]	-56.16	[67.707]	-69.77	[59.428]	-67.70	[77.734]	-214.41***	[68.495]
investmentgrade	-25.31*	[13.853]	6.62	[15.713]	-29.46	[18.274]	-22.64	[18.665]	-69.07***	[21.442]	-20.66	[21.427]	3.01	[23.315]
Notrated	-24.69**	[12.288]	17.21	[16.151]	-11.35	[11.575]	4.33	[16.031]	-27.95	[26.384]	-4.59	[17.826]	26.65	[21.172]
Analysts	-0.31	[0.491]	0.04	[0.546]	-0.43	[0.798]	-0.65	[0.531]	-0.19	[0.806]	0.03	[0.810]	-0.41	[1.283]
Loan Purposes	Yes		Yes		Yes		Yes		Yes		Yes		Yes	
Firm Fixed Effects	Yes		Yes		Yes		Yes		Yes		Yes		Yes	
Observations	1,583		1,655		1,806		1,609		2,100		2,447		2,402	
R-squared	0.84		0.82		0.86		0.86		0.82		0.80		0.78	

**Panel B: Comparing Public Firms with Private Firms**

	(1) Public Firms		(2) Private Firms	
Event Year	2000		2000	
Period	1999-2001		1999-2001	
after	18.40***	[6.133]	31.88	[19.877]
switch	-4.65	[8.063]	2.14	[30.298]
switch*after	_31.71*_	_[16.475]_	_-29.61_	_[36.583]_
lloansize	-4.00	[3.007]	-0.11	[6.740]
levloan	87.75***	[14.739]	107.28***	[37.007]
lmat	8.57**	[3.877]	4.61	[7.009]
secured	37.53***	[12.227]	42.42	[26.056]
missingsecured	-0.34	[6.257]	12.92	[17.939]
revolver	-33.67***	[6.747]	-21.79*	[11.938]
llender	-5.18	[6.491]	-7.37	[12.002]
lsale	-5.73	[10.968]	-1.62	[22.110]
Loan Purposes	Yes		Yes	
Firm Fixed Effects	Yes		Yes	
Observations	1,610		270	
R-squared	0.87		0.91	

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Table 9. Robustness Test: Change in Relationship Banking Intensity-Switching Event Year & Comparing Public Firms with Private Firms

Panel A examines the change in the relationship banking from the pre- to the post-event periods. We compare the results surrounding the true event year (Column (4)) and those surrounding the "hypothetical" event years. The event years and sample periods are indicated in the first row. Loans issued in the event year (either true or "hypothetical") are excluded from each sample. We control for firm fixed effects and loan purpose fix effects in these regressions. The dependent variable for the regressions is the relationship intensity residual computed by regressing relationship banking intensity measured by Rel(Num) on a time trend variable t (t =1...11 for year 1995 to 2005). The regression results reported are based on the 1-year sample. To be included in the sample, we require a firm to issue loans in both the pre- and post-event periods. Panel B compares the changes in relationship banking intensity of public firms with that of private firms surrounding the true event year 2000. Definitions of all the variables are reported in Table 1. Superscripts ***, **, * correspond to statistical significance at the 1%, 5%, and 10% levels with standard error clustered at firm level, respectively.

Panel A: Switching Event Year

	(1)		(2)		(3)		(4)		(5)		(6)		(7)	
Event Year	1996		1997		1998		**2000**		2002		2003		2004	
Period	1995-1997		1996-1998		1997-1999		1999-2001		2001-2003		2002-2004		2003-2005	
After	*-0.06*	*[0.045]*	*-0.01*	*[0.040]*	*0.00*	*[0.035]*	*0.12***	*[0.056]*	*-0.02*	*[0.031]*	*0.03*	*[0.025]*	*0.00*	*[0.027]*
Lloansize	0.06***	[0.014]	0.01	[0.017]	0.03**	[0.014]	0.02	[0.016]	0.05***	[0.013]	0.01	[0.014]	0.03***	[0.011]
Levg	0.16	[0.160]	-0.04	[0.155]	0.04	[0.114]	0.19	[0.203]	0.12	[0.198]	-0.06	[0.175]	0.01	[0.172]
noninvestmentgrade	-0.11	[0.121]	0.13	[0.091]	-0.06	[0.108]	-0.13	[0.102]	-0.14*	[0.075]	0.08	[0.079]	0.04	[0.088]
Notrated	-0.13	[0.106]	0.08	[0.098]	0.02	[0.112]	-0.25***	[0.092]	-0.18**	[0.088]	-0.01	[0.089]	0.01	[0.094]
Lsale	0.01	[0.060]	0.04	[0.030]	0.01	[0.040]	0.02	[0.051]	0.08	[0.056]	0.02	[0.063]	0.08	[0.051]
Mkbk	-0.02	[0.046]	0.03	[0.040]	0.04	[0.032]	-0.03	[0.027]	0.02	[0.016]	-0.02	[0.038]	0.04	[0.039]
Prof	0.41	[0.307]	0.41**	[0.171]	0.07	[0.233]	0.15	[0.269]	-0.05	[0.307]	0.40	[0.247]	-0.39	[0.291]
Noofborrow	0.01	[0.016]	0.03***	[0.010]	0.01	[0.012]	-0.02	[0.019]	0.03***	[0.009]	0.03***	[0.008]	0.02***	[0.006]
Loan Purposes	Yes		Yes		Yes		Yes		Yes		Yes		Yes	
Firm Fixed Effects	Yes		Yes		Yes		Yes		Yes		Yes		Yes	
Observations	1,687		1,799		2,033		1,449		2,149		2,502		2,434	
R Squared	0.60		0.60		0.62		0.69		0.61		0.63		0.64	

Panel B: Comparing Public Firms with Private Firms

	(1) Public Firms		(2) Private Firms	
Event Year	2000		2000	
Period	1999-2001		1999-2001	
After	*0.14***	*[0.058]*	*0.16*	*[0.130]*
Lloansize	0.02	[0.016]	-0.01	[0.033]
Levloan	0.02	[0.076]	0.07	[0.149]
Lsale	0.05	[0.040]	-0.02	[0.072]
Noofborrow	-0.02	[0.020]	0.02	[0.049]
Loan Purposes	Yes		Yes	
Firm Fixed Effects	Yes		Yes	
Observations	1,449		316	
R Squared	0.68		0.74	

Table 10. Robustness Test: Change in Lead Lenders Share-Switching Event Year & Comparing Public Firms with Private Firms

Panel A examines the change in the loan share held by lead banks in loan syndicates from the pre- to the post-event periods. We compare the results surrounding the true event year (Column (4)) with those surrounding the "hypothetical" event years. The event years and sample periods are indicated in the first row. Loans issued in the event year (either true or "hypothetical") are excluded from each sample. The dependent variable is loan share (in percentage) held by lead banks in a loan syndicate. The regression results reported are based on the 1-year sample. To be included in the sample, we require a firm to issue loans in both the pre- and post-event periods. Panel B compares the change in lead lender share of public firms with that of private firms surrounding the true event year 2000. Definitions of all the variables are reported in Table 1. Superscripts ***, **, * correspond to statistical significance at the 1%, 5%, and 10% levels with standard error clustered at firm level, respectively.

Panel A: Switching Event Year

	(1)		(2)		(3)		(4)		(5)		(6)		(7)	
Event Year	1996		1997		1998		**2000**		2002		2003		2004	
Period	1995-1997		1996-1998		1997-1999		1999-2001		2001-2003		2002-2004		2003-2005	
After	*10.73**	*[5.581]*	*4.24*	*[4.869]*	*3.36*	*[5.459]*	*13.34***	*[6.098]*	*0.64*	*[3.893]*	*-1.12*	*[3.352]*	*-2.91*	*[4.774]*
Lloansize	-6.39**	[2.741]	-3.86*	[2.246]	-2.02	[1.899]	-1.69	[2.875]	3.49	[2.310]	-0.01	[2.376]	-1.91	[2.308]
Lmat	-2.15	[2.911]	-3.27	[2.126]	-5.70**	[2.318]	3.08	[2.033]	-0.31	[1.704]	0.69	[2.144]	-1.58	[2.245]
Revolver	-3.49	[4.045]	-3.54	[2.873]	-3.38	[3.496]	-1.91	[4.221]	-10.28***	[3.523]	-0.65	[3.320]	-5.15*	[2.914]
Levg	-3.52	[30.044]	-24.95	[20.322]	23.48	[17.526]	31.97	[42.708]	-5.55	[26.583]	42.76	[27.678]	-22.68	[28.393]
Lsale	-1.91	[8.141]	0.19	[4.218]	12.58	[12.172]	-8.92	[10.451]	-8.92	[8.763]	-2.25	[6.389]	3.91	[10.975]
Mkbk	-7.54	[6.657]	-0.32	[2.129]	5.55	[7.175]	0.38	[4.700]	-2.07	[1.616]	4.10**	[1.872]	-0.28	[6.808]
Credit Ratings	Yes		Yes		Yes		Yes		Yes		Yes		Yes	
Loan Purposes	Yes		Yes		Yes		Yes		Yes		Yes		Yes	
Firm Fixed Effectes	Yes		Yes		Yes		Yes		Yes		Yes		Yes	
Observations	699		717		664		527		749		798		841	
R-squared	0.78		0.81		0.81		0.80		0.78		0.79		0.76	

Panel B: Comparing Public Firms with Private Firms

	(1) Public Firms		(2) Private Firms	
Event Year	2000		2000	
Period	1999-2001		1999-2001	
After	*11.15**	*[6.109]*	*16.29*	*[13.636]*
Lloansize	-1.57	[2.836]	0.13	[4.304]
Levloan	6.79	[12.001]	-5.42	[15.602]
Lmat	3.14	[2.220]	3.31	[3.717]
Revolver	-1.10	[4.397]	-10.21	[15.526]
Lsale	-5.99	[10.659]	-29.29*	[15.803]
Loan Purposes	Yes		Yes	
Firm Fixed Effectes	Yes		Yes	
Observations	527		49	
R-squared	0.79		0.92	

Table 11. Robustness Test: Control for the NBER Recession Dates

This table uses private firms as a control group to disentangle the effect of Reg FD from the effect of the 2001 economic downturn. We control for industry fixed effects and loan purpose fixed effects in the regressions. The dependent variable is all-in-spread drawn (*AISD*) in basis points. The regression results reported are based on the 1-year sample from 1999 to 2001. Loans issued in the year 2000 are excluded from the sample. To be included in the sample, we require a firm to issue loans in both the pre- and post-Reg FD periods. The sample includes both public and private firms. The main variable of interest is the triple interaction term "*public*switch*after*". Definitions of all the variables are reported in Table 1. Superscripts ***, **, * correspond to statistical significance at the 1%, 5%, and 10% levels with standard error clustered at firm level, respectively.

	Public and Private Firms	
Event Year	2000	
Period	1999-2001	
after	13.68***	[5.094]
Switch	-0.47	[17.700]
Public	-28.31**	[13.531]
switch*after	-20.83	[18.182]
public*switch	0.3	[18.545]
private*after	-5.72	[9.028]
*public*switch*after*	*46.76** *	*[20.144]*
Lloansize	-11.02***	[2.906]
Levloan	138.86***	[8.640]
Lmat	7.79**	[3.701]
Secured	44.25***	[8.043]
missingsecured	5.23	[4.570]
Revolver	-54.73***	[5.668]
Llender	2.32	[3.345]
Lsale	-4.39*	[2.302]
Loan Purposes	Yes	
Industry Fixed Effects	Yes	
Observations	1,880	
R-Squared	0.69	

Table 12. Additional Test: Average Cumulative Abnormal Returns to Loan Issue Announcement Before and After Reg FD

This table presents the average cumulative abnormal return (ACAR) to firms that made a loan issue announcement before and after the introduction of Reg FD. We use deal active dates as a proxy for the loan announcement dates. The sample period is from Nov 1, 1999 to August 31, 2001. We exclude all loans issued during the month of October, November, and December of 2000. Results for pre- and post-Reg FD period are reported in columns (1) and (2), respectively. In column (3), the difference in the mean ACAR between the pre-and post-Reg FD period and the student *t* statistics are reported. The superscripts for t statistics ***, **, and * stand for significance at the 1%, 5%, and 10% levels using a two-tailed test.

| | (1) | | | (2) | | | (3) | |
| | Before Reg FD (A) | | | After Reg FD (B) | | | Mean Difference (B-A) | |
Event Window	ACAR (%)	t-stat	obs.	ACAR (%)	t-stat	obs.	Difference in Mean	*t-Stat*
[-3,+3]	0.19	[0.66]	1503	0.99	[2.54]**	858	0.80	[1.66]*
[-1,1]	0.15	[0.87]	1503	0.67	[2.75]***	858	0.52	[1.73]*
[0,+1]	0.06	[0.38]	1503	0.55	[2.62]***	858	0.49	[1.88]*

Table 13. Additional Test: Propensity of Public Debt vs. Bank Debt

This table examines the changes in the propensity to issue public debt vs. bank debt from the pre- to the post- Reg FD period. We control for both year fixed effects and industry (1-digit SIC code) fixed effects in these Probit regressions. The dependent variable is an indicator variable that takes the value of one if a firm chose to issue a syndicated loan, or zero if the firm chose to issue public bonds. We report the regression results based on the 1-year and the 2-year sample in columns (1) and (2), respectively. Sample periods are indicated in the first row. Loans issued in the year 2000 are excluded from each sample. To be included in the sample, we require a firm to issue at least one type of debt in both the pre- and post-Reg FD periods. Definitions of all the variables are reported in Table 1. The main variable of interest is the "after" dummy. Superscripts ***, **, * correspond to statistical significance at the 1%, 5%, and 10% levels with standard error clustered at firm level, respectively.

Regression	(1)		(2)		(3)		(4)		(5)	
Variables	1999-2001		1998-2002		1998-2002		1998-2002		1998-2002	
after	0.17*	[0.100]	0.28**	[0.120]	2.52***	[0.499]	0.21*	[0.121]	0.23*	[0.125]
lsale*after					-0.26***	[0.056]				
z dummy*after							0.33*	[0.196]		
non-investmentgrade*after									0.38*	[0.205]
lloansize	0.42***	[0.061]	0.49***	[0.044]	0.50***	[0.046]	0.49***	[0.044]	0.49***	[0.044]
lsale	-0.47***	[0.070]	-0.44***	[0.052]	-0.35***	[0.055]	-0.44***	[0.052]	-0.44***	[0.053]
levg	-0.07	[0.400]	-0.49	[0.326]	-0.45	[0.328]	-0.49	[0.325]	-0.50	[0.326]
mkbk	-0.10*	[0.050]	-0.04	[0.043]	-0.03	[0.043]	-0.04	[0.043]	-0.04	[0.043]
prof	0.08	[0.801]	-0.11	[0.648]	-0.24	[0.641]	-0.12	[0.640]	-0.10	[0.656]
z dummy	-0.13	[0.177]	-0.04	[0.122]	-0.07	[0.125]	-0.21	[0.157]	-0.05	[0.121]
tangibility	-1.19***	[0.360]	-0.80***	[0.265]	-0.74***	[0.266]	-0.78***	[0.266]	-0.79***	[0.265]
non-investmentgrade	0.70***	[0.210]	1.04***	[0.157]	1.04***	[0.158]	1.03***	[0.158]	0.86***	[0.199]
notrated	2.47***	[0.369]	2.73***	[0.233]	2.79***	[0.226]	2.75***	[0.227]	2.73***	[0.236]
Industry Fixed Effects	Yes		Yes		Yes		Yes		Yes	
Year Fixed Effexts	No		Yes		Yes		Yes		Yes	
Observations	1330		3233		3233		3233		3233	
Pseudo R-squared	0.431		0.523		0.527		0.523		0.523	