# Information Spillovers and Cross Monitoring between the Stock Market and Loan Market: Evidence from Reg SHO

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**Abstract**

We explore information spillovers and cross monitoring between the stock and loan markets. To break simultaneity between the stock and loan markets, we use a regulatory experiment, Regulation SHO, that relaxes short selling constraints for a randomly selected sample of Russell-3000 stocks, which directly affects information production and monitoring by short sellers in the stock market but is exogenous to the loan market. We find that while firms without bank monitors exhibit a significant decline in stock prices upon the announcement of SHO, firms with bank monitors do not react. Further evidence shows that firms affected by SHO enjoy a 21 basis point lower loan spread that increases to 36 basis points for bank-dependent firms. Regulation SHO, however, does not appear to affect non-price loan terms such as loan maturity, amount, collateral, and covenants. Overall, our evidence suggests bi-directional information spillovers and cross monitoring between the stock and loan markets. The effects on loan markets are consistent with a reduction in the information monopoly that banks possess over their borrowers.

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# 1 Introduction

The costs and benefits of short selling constraints are hotly debated in the regulatory community and in the academic literature. The debate centers on the benefits of increased price discovery and information production versus the potential costs of manipulation. Prior academic work documents numerous effects of short selling constraints that include stock liquidity improvements, enhanced price discovery, and more informative stock prices. While the effects for stockholders have been well established, less is known about how short selling constraints influence other firm stakeholders. For example, other external stakeholders, like debtholders, may benefit from enhanced stock liquidity and stock price informativeness, and they may also be affected by potential stock price manipulation.

We explore how short selling constraints on a firm's stock affects the loan contracts it receives from its banks. The cross-monitoring hypothesis suggests that firm stakeholders benefit from the monitoring and information collection conducted by other stakeholders. We examine the bi-directional implications of the cross-monitoring and information spillovers between the stock market and the syndicated loan market to see how price discovery in the equity market influences bank contracting terms and vice versa. We find that reductions in short-sale constraints in the stock market lead to reductions in the cost of bank loans. Exploring the relation in reverse, we find that price discovery in the stock market depends on whether a firm has a bank relationship, presumably because the bank provides information and/or certification benefits to the stock market. Taken together we document significant bi-directional cross-monitoring benefits that suggest that reducing frictions in one market has spillover benefits that affect claimants across markets.

To identify these effects our tests exploit an exogenous event, the adoption of Regulation SHO. Short selling has been largely constrained in the U.S. historically. The uptick rule that was established in 1935, for example, prohibits short sales when stock prices are declining, which imposes

a significant cost on short sellers. In July 2004, the Security and Exchange Commission (SEC) announced Regulation SHO that removed the uptick rule restriction for a randomly selected group of firms (pilot group) that represent one third of the Russell 3000 firms listed on NYSE, NASDAQ, and AMEX. The other two thirds of the Russell 3000 firms consist of non-pilot group for which the uptick rule remained in effect. This sudden regulatory change provides a quasi-laboratory setting that exogenously shifts a firm's short sale constraints, but does not directly affect the loan market. Because the uptick rule was removed, Regulation SHO effectively reduced the cost of short selling, which should entice informed traders and result in improved price transparency. In addition, lower short selling constraints may reduce managerial agency costs if short sellers can profit by uncovering managerial misdeeds.

Banks, as inside lenders, may also affect information production and managerial discipline. Banks screen and monitor borrowers, which entails ongoing information production and collection costs. If information produced in the stock market affects these costs, then we would expect the adoption of Regulation SHO to potentially influence bank loan pricing and terms in two ways. First, if banks have lower screening and monitoring costs due to information spillovers from short sellers, then the savings may be passed on to borrowers via lower loan spreads. Second, if reducing short selling constraints allows traders to better profit from negative private information, then firm managers may have a lower incentive to engage in self-interested policies. This reduced agency concern could benefit banks if such managerial actions are value destructive. The reverse is also true. If banks provide information and monitoring benefits that are valuable to shareholders, then relaxation of short-selling constraints will be less influential for firms with bank relationships. In other words, if the banks reduce the likelihood that managers engage in self-interested policies and provide certification of firm condition, then short-sellers will have a lower probability of uncovering

such misdeeds and will, ex-ante, engage in less information production by would-be short sellers.

We begin our tests of the above conjectures by exploring whether the stock market reaction to SHO depends on the firm's bank relationship. Grullon, Michenaud and Weston (2015) document significant declines in the stock prices of SHO-affected firms around the revelation of SHO participation. Part of this decline in prices is argued to be driven by anticipation that negative information about the firm will be revealed. Given that banks monitor firms, we expect such anticipation to depend on the existence of a bank monitor. We indeed find that this is the case. The average cumulative abnormal return (CAR) for a SHO participating firm with a bank relationship is 0.01% while that for participating firms lacking a bank relationship is -3.10%, and the two CARs are significantly different from each other at the 1% level. Given that smaller firms likely have a greater degree of asymmetric information, we stratify the sample based on firm size. We find that both small and large firms experience insignificant CARs when they have a bank relationship (and significant negative CARs otherwise). We find similar results if we stratify the sample based on the availability and level of credit ratings. These findings are consistent with the screening and monitoring services banks provide and is consistent with the literature that argues these services have spillover benefits to shareholders (James, 1987). While splitting by firm size and other firm characteristics alleviate selection concerns about the types of firms that borrow from banks, such concerns cannot be completely eliminated. So we next explore within the sample of bank borrowers to see whether the terms of the loans are affected by the change in the information environment induced by Regulation SHO.

We find evidence consistent with these information effects. Using a difference-in-difference (DiD) design, we find that borrowers affected by SHO experience a 12.7% reduction in their cost of loans (equivalent to 21 basis points). This difference increases to 21.6% (36 basis points) for bank-

dependent borrowers (i.e. borrowers that lack a bond credit rating). These findings are consistent with two plausible interpretations. First, SHO may increase short sellers 'incentive to uncover information which could reduce the monitoring and information production costs of lenders. A portion of the reduced costs could be passed on to borrowers in the form of lower loan spreads. Second, banks that have a pre-SHO relationship with the firm (i.e., the incumbent bank) may already incurred information and monitoring costs, limiting any direct spillover benefits from regulation SHO to the incumbent bank. Other potential lenders to the firm, however, may face lower information and monitoring costs post-SHO and competitive pressure could force incumbent banks to lower loan spreads. In other words, regulation SHO could reduce the information monopoly of incumbent banks (see, e.g., Rajan, 1992).

In an attempt to distinguish these two alternative interpretations, we explore the effect of Regulation SHO on non-price terms of loan deals. Prior studies show that non-price terms, such as maturity, covenants, and collateralization may alleviate agency costs. If Regulation SHO alleviates agency concerns, the first interpretation of our earlier finding, then non-price loan terms would likely adjust (particularly among incumbent banks). Specifically we would predict that Regulation SHO would lead to reductions in the number and tightness of covenants, to lengthening of maturity, and to the reduction in the use of collateral.

We find no effects of Regulation SHO on non-price loan terms. To the extent that these non-price loan terms are designed to mitigate agency and information concerns, and assuming bank monitoring effectively substitutes for any disciplinary effects from short selling, these no-results suggest that the influence of information and monitoring channel on managerial behavior appears negligible. Moreover, we find non-price terms for incumbent banks are unchanged around SHO as well. Taken together, the evidence suggests that our findings on the effect of Regulation SHO on

loan spreads is more likely due to the reduction in the information monopoly that banks possess over bank-dependent borrowers.

Our study makes a number of important contributions. First, we demonstrate that financial market frictions in equity markets significantly impact other stakeholders'contracting in other markets. Specifically, bank loan terms are significantly affected by the financial market frictions in the equity market. This observation suggests the benefits gained from reducing financial market frictions are much larger than simply the influence on shareholders. A number of papers (e.g., see Holmström and Tirole, 1993; Pagano and Röell, 1998)) argue the information production associated with being a publicly traded firm will behoove other stakeholders. For example, firms with public equity must adhere to disclosure requirements and may entice analyst coverage that results in information that may be useful to creditors. We show that the microstructure of equity trading also has a pronounced effect on loan terms.

We speak to the literature that debates the effect of short selling constraints on asset prices and the cost of capital (e.g., Miller, 1977; Chen, Hong, and Stein, 2002; Battalio and Schultz, 2006; Diether, Lee, and Warner, 2009; Boehmer, Jones, and Zhang, 2008, 2013). We show that while alleviating short sale constraints may increase the cost of equity, this will be partially offset by a decrease in the cost of loans. Our paper also contributes to the literature that explores the effect of short sellers on corporate investment and financing decisions (e.g., Gilchrist, Himmelberg, and Huberman, 2005; Grullon, Michenaud, and Weston, 2015; Massa, Zhang, and Zhang, 2015).

Finally, we add to the banking literature. Much work has been done on how banks, as information producers with inside access to the firm better screen and monitor lenders. These benefits behoove not only the bank, but other stakeholders as well. Our results show that bank relationships

provide a certification and monitoring benefit to shareholders. Most prior evidence on this involves the announcement of a bank loan and the elicited response in the equity markets (see James (1987), Billett, Flannery and Garfinkel (1995), and most recently Ross (2010)), which involves the endogenous decision to obtain and announce a loan. Our results show such benefits exist in a setting in which this particular form of endogeneity is absent. Prior work also demonstrates a dark side to banks. Rajan (1992) shows that when banks obtain private information the resulting information monopoly allows banks to "hold-up"the firm by charging higher loan spreads. Our results show that the information production in the equity market may help alleviate the banks'information monopoly.

# 2 Sample and Variable Construction

## 2.1 Sample construction

We start with the Russell 3000 index in June 2004 when constructing the sample. The SEC's first pilot order issued on July 28, 2004 (Securities Exchange Act Release No. 50104) describes in detail how the pilot and non-pilot stocks in the Regulation SHO program are chosen. Following that description, we exclude stocks that are not listed on the NYSE, AMEX, or NASDAQ NM, and we exclude stocks that go public or have spin-offs after April 30, 2004. Of the remaining 2,952 stocks, we identify 986 pilot stocks according to the published list of the SEC's pilot order. The remaining 1,966 stocks comprise the initial non-pilot sample. We further exclude financial, utilities, and non-U.S. firms from our sample. We then merge this sample with CRSP daily stock returns and exclude stocks with missing price information, price smaller than 5 dollars or greater than 1,000 dollars. This procedure results in 1,539 stocks for the stock announcement return tests, among which 527 are pilot stocks and 1,012 are non-pilot stocks.

We use Dealscan to construct our sample of bank loans for our DiD analysis. We require that the firm has loans originated within 2 years before the approval of Regulation SHO (June 23, 2004) and within 2 years after the implementation of Regulation SHO (May 5, 2005). We also require that firms have non-missing loan contract information in the Dealscan database and non-missing Compustat financial information in the same sample period. We then match loan contract information with the most recent fiscal quarter-end financial data before loan originations. We drop loans whose primary purpose is debtor-in-possession, share repurchase, or Leveraged Buyouts (LBO). We conduct our loan contract analysis at the loan facility level for loan spreads, collateral amount, and maturity. We conduct loan covenant analysis at the loan deal level. Our final sample consists of 1,621 loan facilities from 410 firms for the facility-level analysis.

### 2.2 Bank relationship variables

We construct several measures to capture firms' bank relationships. Our primary variable, BANKCUR, is an indicator that equals one if a firm has an outstanding U.S. commercial bank relationship and zero otherwise. This definition follows Sufi (2009)'s argument that domestic banks, as informed lenders, provide effective certification and monitoring services to borrowers. To measure relationship strength, we construct two variables: STRONGAMT, that measures dependence on the relationship bank, and LONGREL that measures length of the relationship Following Bharath, Dahiya, Saunders, and Srinivasan (2011), we first look into firms' borrowing history in the past 5 years prior to Regulation SHO. We define a firm to have a relationship bank if this bank has been a lead arranger or sole lender for two or more different deals. Then, for each firm-bank pair, we compute the percentage of amount borrowed from this particular bank to total amount the firm has borrowed in the past 5 years. STRONGAMT is an indicator variable that equals one if a firm has a relationship lender and it borrows more than two thirds of loans from that lender and zero

otherwise. The second variable, LONGREL, built on the bank relationship duration measure in Ongena and Smith (2001), is an indicator variable that equals one if the time gap between the first and last loans from the same lender is larger than the sample median and zero otherwise.

Previous studies show that lenders' reputation affects firm value through screening and monitoring channels (e.g. Billett, Flannery, and Garfinkel, 1995; Ross, 2010). We further construct on a lender's reputation following Ross (2010): the first measure, DOM, is an indicator variable that equals one if a firm has borrowed from one of dominant banks–Citi, JP Morgan Chase and Bank of America, and zero otherwise. Because most loans in Dealscan are syndicated loans, we use the lead arranger in these cases. Taking account of bank mergers and information aggregation within financial conglomerates, we aggregate financial institutions to their parent companies and assign acquired firms to their acquirers at the effective date of the merger. Acquiring financial firms inherit both previous lead arranger-participant relationships and previous borrowing firm relationships of the acquired firm.

### 2.3 Loan contract variables

We draw our loan contract variables mainly from Dealscan. We use the variable, All-in-Spread-Drawn, as a measure on loan spreads. We use facility amount scaled by borrowing firms'total assets to measure loan amount. Previous studies on debt contracts suggest that debt contract terms are often jointly determined, and non-price terms are also affected when firms'credit quality changes (e.g. Graham, Li, and Qiu, 2008). Therefore, besides loan price terms, we examine changes on non-price loan terms around Regulation SHO as well. We construct four non-price loan terms. The first variable, Maturity, is the number of months between the start date and the end date of a loan facility. The second variable, Collateral, is an indicator variable that equals one if a loan is secured and zero otherwise. To further examine whether lendersâĂŹ monitoring incentives on

borrowing firms change around Regulation SHO, we use the number of financial covenants and covenant cushions (Denis and Wang, 2014) that is defined as the percentage difference between the underlying covenant variable and its contractual limit. The former variable captures the degree of restrictions on a firm's management in a similar spirit to the G-index on shareholder governance (Bradley and Roberts, 2004), and the latter captures the tightness of loan covenants.

### 2.4 Summary statistics

Table 1 provides summary statistics of the variables. Panel A shows descriptive statistics of loan characteristics including both price terms (spreads) and non-price terms (maturity, loan amount, collateral, and covenants). We find that the distribution of loan characteristics are similar to previous studies on bank loan contracting (e.g. Bharath, Dahiya, Saunders, and Srinivasan, 2011). Note that our sample period, 2002-2007, is a period of credit expansion with institutional funds flowing into syndicated loans (e.g. Ivashina and Sun, 2011). As a result, loan spreads are declining for both pilot and non-pilot firms throughout our sample period. However, our DiD analysis effectively removes this general time trend in loan yields. Panel B reports the descriptive statistics of borrowing firm characteristics prior to a loan origination. An average firm has book value assets of 5.2 billion, book leverage of 30.5%, tangible asset ratio of 35.4%, ROA of 3.6%, is covered by 13 analysts, and has a market-to-book ratio of 1.3. 67% of our sample firms have credit ratings. Panel C reports the summary statistics of stock announcement returns around Regulation SHO. The average CAR around the Regulation SHO announcement date is -0.67% and the median CAR is -0.10%. In our sample, 52.2% of firms have an outstanding bank relationship prior to Regulation SHO announcement.

# 3 Empirical Results

## 3.1 Bank monitors and CARs around SHO announcement

We first examine whether SHO participating firms' abnormal stock reactions to the announcement of Regulation SHO depends on the firms' existing bank monitors. Grullon, Michenaud, and Weston (2015) show significant declines in the stock prices of pilot firms around the revelation of SHO participation, using the same setting as ours. They argue that part of this decline in prices is driven by market anticipation that negative information about the firm will be revealed. Given that banks screen and monitor their borrowers, we expect that stock price declines caused by market anticipation will depend on the existence of a bank monitor. Specifically, our conjecture is that stock prices will drop less for firms with existing bank monitors. This is because banks should have already produced valuable information and effectively monitored their borrowers, which is observed by equity market participants and priced into their stocks.

We compare market-adjusted stock returns of pilot and non-pilot firms based on whether the firms have existing bank monitors. We begin by conducting univariate tests on market-adjusted returns for pilot firms with an existing bank relationship, pilot firms without an existing bank relationship, non-pilot firms with an existing bank relationship, and non-pilot firms without an existing bank relationship to examine which subgroup of firms experiences significant abnormal returns and compare average abnormal returns across subsamples. We report the results in Table 2 Panel A. Pilot firms without an existing bank relationship experience a significant -3.10% CAR around the Regulation SHO announcement. Pilot firms with an existing bank relationship, however, do not experience significant abnormal returns. This observation suggests that the existence of a bank monitor helps SHO-affected firms to offset the negative announcement effect of Regulation SHO. As a comparison, we do not observe significant CARs for non-pilot firms regardless of the existence

of bank relationships. Row 3 of Panel A reports the difference of CARs between pilot and non-pilot firms among subsamples. For firms with an existing bank relationship, stock returns of pilot firms do not show a significant difference from control firms. In contrast, pilot firms underperform non-pilot firms by -2.70% on average if they do not have existing bank relationships. This observation is consistent with Grullon, Michenaud, and Weston (2015). In Table 2 Panel B, We re-examine the tests in a multivariate setting using the following model:

$$\text{CAR}_i = \alpha + \beta_1 \text{Pilot}_i + \beta_2 \text{Pilot}_i * \text{BANKCUR}_i + \beta_3 \text{BANKCUR}_i + \text{Controls}_i + \epsilon_i \qquad (1)$$

where i indexes firm. CAR is the 11-day market-adjusted abnormal stock return. The key variable of interest is Pilot*BANKCUR, which is an indicator that equals one if the firm is in the pilot group and has an existing loan with a domestic commercial bank and zero otherwise. Control is a set of control variables that includes firm assets, market-to-book ratio, leverage, institutional ownership, and 1-digit SIC industry fixed effects.

In column (1), we present the results without control variables, which is the regression form of Table 2 Panel A. In column (2), we present the regression results with control variables. The coefficient estimates of $\beta_2$ is positive and its magnitude is comparable to that of $\beta_1$, which on average effectively eliminates pilot firms' underperformance relative to non-pilot firms if pilot firms have existing bank relationships. Overall, our results suggest that significant stock price declines upon the SHO announcement documented by Grullon, Michenaud, and Weston (2015) are mainly driven by firms without existing bank relationship. For firms with an outstanding bank relationship we find no negative stock price reaction.

Our findings in Table 2 are consistent with the literature that argues screening and monitoring by banks have spillover benefits to shareholders (e.g. James, 1987). One concern, however, is that bank borrowers are less transparent and more susceptible to short-sellers uncovering negative information. In other words, the different reactions to the SHO announcement documented above may be driven by differences in firm characteristics rather than bank monitors. To alleviate this concern, in Table 3, we further partition the sample by firm size, the availability and level of credit ratings, the strength of bank relationships, and the reputation of lenders. If our results are not driven by self-selection of firm borrowing, we should observe the differential reactions of firms with and without existing bank monitors persistent regardless of the way we partition our sample. To test our conjecture, we estimate equation (1) separately in subsamples partitioned by firm size, the availability and level of credit ratings, the strength of bank relationships, and the reputation of lenders.

In Panel A, we split the sample by firm size. We define a firm as a small firm if its total assets fall in the bottom tercile of Russell 3000 Index firms and a large firm if its total assets are in the top two tercile of Russell 3000 Index firms. Small firms are less transparent, while large firms typically are covered by a larger number of financial analysts, have greater institutional ownership, and are generally assumed to have less information asymmetry. We estimate equation (1) separately for large firms and small firms. The coefficient estimates of $\beta_1$ are negative and significant in both subsamples, consistent with our earlier findings that, compared to non-pilot firms, pilot firms without a bank monitor experience a significant drop in stock prices upon the SHO announcement. We further show that the coefficient estimates of $\beta_1$ are not significantly different across subsamples. The p-values of F-statistics that test the joint significance of $\beta_1 + \beta_2$ are quite large. Hence, we cannot reject the null hypothesis that pilot firms with bank monitors do not experience stock price

declines upon the SHO announcement. Regarding the coefficient estimate of the interaction terms, both are positive and one of them is statistically significant in the subsample of small firms. The difference in $\beta_2$ across the subsamples, however, is not statistically significant. Our findings suggest that the differential reactions of firms with and without existing bank monitors persistent in both large and small firms.

In Panels B and C, we split the sample by the availability and level of bond credit ratings. In Panels D and E, we split the sample based on the strength of a firm's existing bank relationships. In Panel F and G, we split the sample based on the reputation of a firm's lenders. In all panels, we find that the coefficient estimate of $\beta_2$ are positive in all subsamples, and the differences in $\beta_2$ across subsamples are not statistically significant. The evidence suggests that the differential reactions of firms with and without bank monitors remain the same regardless of the way we partition the sample. Hence, our results are unlikely to be driven by a firm's self-selection into firm borrowing but are likely to be driven by the screening and monitoring services provided by an informed lender.

### 3.2 SHO and loan spreads

The cross-monitoring hypothesis suggests that banks benefit from the monitoring and information collection conducted by other stakeholders, such as short sellers, and will reduce the spreads they charge on their borrowers. This argument could be true because of two reasons. First, if banks have lower screening and monitoring costs due to information spillovers from short sellers, then these may be passed on to borrowers via lower loan spreads. Second, if reducing short selling constraints allows traders to better profit from negative private information, then firm managers may have a lower incentive to engage in self-interested policies. This reduced agency concern could benefit banks if such managerial actions destroy firm value. In this subsection, we explore how Regulation SHO affects loan spreads in the DiD framework.

Before undertaking the DiD analysis, we first verify the premise that the assignment of pilot and non-pilot firms from the Russell 3000 index was random. We compare borrower and loan characteristics of pilot and non-pilot firms. Because our sample only includes firms that have loan originations both before and after Regulation SHO, many firms that do not have loans are excluded. We report the results in Table 4.

In the top panel, we compare borrower characteristics of the closest fiscal quarter end before the Regulation SHO Pilot Program approval date. Borrower characteristics include firm assets, leverage, Z-scores, tangibility, profitability, interest coverage, current ratio, and market-to-book ratio. In the bottom panel, we compare loan characteristics of the most recent loan deals that are initiated before the SHO approval date. Loan characteristics include loan spreads, maturity, amount, collateral, and the number and tightness of covenants. We do both a t-test for the differences in means and the Wilcoxon z-test for the differences in medians. Neither mean nor median of the differences is statistically significant, which suggests that both group of firms and their loans exhibit similar characteristics before Regulation SHO.

Next, we check the satisfaction of the parallel trend assumption, a key identifying assumption, of the DiD approach. The parallel trend assumption requires that, in the absence of Regulation SHO, the observed DiD estimator is zero. Specifically, the assumption requires similar pre-SHO trends in loan spreads for both pilot and non-pilot firms, but does not require the level of loan spreads to be identical before Regulation SHO. This is because the distinctions are differenced out in the estimation. We check the satisfaction of the parallel trend assumption. In Figure 1, we plot the dynamics of loan spreads of pilot and non-pilot firms two years before and after Regulation SHO. It shows that loan spreads are trending closely in parallel for the two groups in the two years leading up to the event.

After ensuring that we have a balanced sample of pilot and non-pilot firms and the parallel trend assumption is not violated, we examine whether pilot firms' loan spreads change after the reduction in short-selling costs due to Regulation SHO. Following earlier studies, we estimate the following model:

$$\text{Ln(Spread)}_{l,i,t} = \alpha + \beta_1 \text{Pilot}_i * \text{During}_t + \beta_2 \text{Control}_{i,t} + \text{Firm}_i + \text{Year}_t + \epsilon_{l,i,t} \quad (2)$$

where i indexes firm, t indexes year, and l indexes loan. $Ln(Spreads)$ is the natural logarithm of loan spreads. *During* is an indicator variable that equals one if the loans are originated after the effective date of Regulation SHO (May 05, 2005) and zero otherwise. *Control* is a set of control variables that includes firm characteristics (firm size, market-to-book, book leverage, tangibility, profitability, and z-score) and loan characteristic (loan amount, collateral, maturity, performance pricing, loan purpose, and loan types). We control for firm and year fixed effects in the regressions. We cluster standard errors at the firm level to address possible correlations among residuals within firms.

One potential concern of the above specification is that, although Regulation SHO represents an exogenous shock to short selling cost, a reverse causality may still arise if firms with different cost of private loans are associated with certain characteristics that determine their inclusion of the pilot group. To address this concern, we follow Bertrand and Mullainathan (2003) to examine the dynamics of loan spreads surrounding Regulation SHO. If reverse causality is present, we should observe changes in spreads of loans originated before Regulation SHO. Specifically, we estimate the following model:

$$\begin{aligned}\text{Ln(Spread)}_{l,i,t} =& \alpha + \beta_1 \text{Pilot}_i * \text{Year1}_t + \beta_2 \text{Pilot}_i * \text{Year2}_t + \beta_3 \text{Pilot}_i * \text{Year}(-1)_t \\ &+ \beta_4 \text{Control}_{i,t} + \text{Firm}_i + \text{Year}_t + \epsilon_{l,i,t} \end{aligned} \tag{3}$$

where i indexes firm, t indexes year, and l indexes loan. *Year 2* is an indicator variable that equals one if the loan is originated in two years after the effective date of Regulation SHO and zero otherwise. *Year 1* is an indicator variable that equals one if the loan is originated in one year after the effective date of Regulation SHO and zero otherwise. $Year(-1)$ is an indicator variable that equals one if the loan is originated in one year before the announcement of Regulation SHO and zero otherwise. If there does not exist reverse causality, we should observe significant coefficient estimates of $\beta_1$ and $\beta_2$ but insignificant coefficient estimate of $\beta_3$. Similar to equation (2), we control for firm and year fixed effects and cluster standard errors at the firm level. We report the results in Table 5.

Column (1) reports the results estimating equations (2). The coefficient estimate of $\beta_1$ is negative and significant at the 1% level. The magnitude of $\beta_1$ suggests that pilot firms experience a 12.7% reduction in loan spreads (equivalent to 21 basis points) surrounding Regulation SHO compared to non-pilot firms. In column (2), we present the results estimating equation (3). We find negative and significant coefficient estimate of $\beta_1$ and $\beta_2$, but insignificant coefficient estimate of $\beta_3$. This non-result suggests that loan spreads do not appear to reverse cause Regulation SHO.

Next, we examine how this result varies with a firm's information environment and bank dependence. We use the availability of public bond credit ratings to capture a firm's information environment. Faulkender and Petersen (2006) show that having credit ratings is a proxy for a firm's

access to public bond markets. Hence, firms that lack access to public bonds are more likely to be bank-dependent and should be affected more by Regulation SHO. In columns (3) and (5), we estimate equation (2) separately for firms with and without credit ratings. The coefficient estimates of $\beta_1$ are negative and significant at the 5% level in both regressions. However, the magnitude of $\beta_1$ in column (5) where bank-dependent borrowers are examined is larger. The difference in loan spreads increases to 21.6% (equivalent to 36 basis points) for bank-dependent borrowers. In columns (4) and (6), we estimate equation (3) separately for firms with and without credit ratings. We find a similar result: the reduction in bank loan spreads is more pronounced for bank-dependent borrowers. At the bottom of the table, we report the statistics that test the equality in the key variable coefficient estimates. It shows that the differences in the effect of SHO on bank loans are significant between borrowers with and without credit ratings. This cross-sectional test further strengthens our argument that improvements in information collection and monitoring by short sellers from the stock markets benefit more for more opaque and hence bank-dependent borrowers.

A unique feature of the SHO experiment is that its repeal in 2007 allows us to check the internal validity of the experiment. In August, 2007, the uptick test restriction for all stocks were removed. Hence, non-pilot firms become treatment firms because they experienced a reduction in short selling constraints while pilot firms become controls firms because their short selling constraints remained the same. Hence, we carry out a DiD test for the "reversal" of the Regulation SHO experiment using the same set of pilot and non-pilot firms. We focus on their loans originated within 2 years around August, 2007. Specifically, we estimate the following model:

$$\text{Ln(Spread)}_{l,i,t} = \alpha + \beta_1 \text{Pilot}_i * \text{Post}_t + \beta_2 \text{Control}_{i,t} + \text{Firm}_i + \text{Year}_t + \epsilon_{l,i,t} \tag{4}$$

where i indexes firm, t indexes year, and l indexes loan. Post is an indicator variable that equals

one if the loan is originated in two years after the repeal of Regulation SHO Pilot program and zero otherwise. If the reduction in loan spreads is caused by the relaxation of short sale constraints, we expect the non-pilot firms experience reductions in loan spreads and hence observe a positive estimate of the DiD estimate $\beta_1$. We report the results in Panel B of Table 5. The coefficient estimates of $\beta_1$ are positive but statistically insignificant for the full sample and also subsamples both with and without credit ratings. These findings could be driven by anticipation of the repeal of the Regulation SHO Pilot program. Boehmer, Jones, and Zhang (2016) find that anticipation prior to the formal repeal of Regulation SHO caused spillover effects from the treatment group (non-pilot firms) to the control group (pilot firms), which makes the repeal less informative than the adoption of Regulation SHO.

### 3.3 Robustness checks on loan spreads

In this subsection, we undertake three robustness checks to ensure that the effect of Regulation SHO on loan spreads are likely causal.

First, a common criticism of studies that use Regulation SHO is that it represents one regulatory change that took place in 2004. Hence, unobservable shocks occurred prior to 2004 or coincide with SHO could have driven both the inclusion in the pilot program and reductions in loan spreads, which undermines the causal inference we draw from the experiment. Note that, although this argument is unlikely because SEC picked pilot stocks based on the ranking of Russell 3000 stocks'trading volume on an exogenously given date, which is highly likely to be random, we still perform a test to address this concern. Specifically, we do a placebo test by artificially picking a "pseudo-event "year, 2001 (three years before the actual Regulation SHO year), when we assume a regulatory shock reduced short selling constraints. We, however, keep the true set of pilot and non-pilot firms identified by Regulation SHO. We estimate equations (2) and (3) with *During, Year 2, Year 1, and Year(-1)*

defined based on the pseudo-event year, and report the results in Panel A of Table 6.

None of the coefficient estimates of key variables of interest is statistically significant in the main regressions in columns (1) and (2). In columns (3) to (6), in which we partition the sample based on firms'availability of credit ratings, we continue to observe no change in loan spreads around this artificially chosen event year.

The second robustness test we do is to keep the true SHO event year but randomly assign firms into pilot and non-pilot groups by simulation. Specifically, in each simulation, we draw a random sample of 135 "pilot"firms from the pool of our sample firms in the event year (2004), and then treat the rest of the pool (the remaining 275 firms) as "non-pilot "firms. We do the DiD test by estimating equations (2) and (3) on this simulated sample and repeat this procedure 5,000 times. We then summarize the regression results from this bootstrapped sample, and report the distribution (i.e., mean, standard deviation, 25th percentile, median, and 75th percentile) of the DiD estimates, namely, the coefficient estimates on *Pilot*During, Pilot*Year 2, and Pilot*Year 1*, as well as their corresponding t-statistics in Table 6 Panel B.

As one can observe, the mean DiD estimates based on this simulated sample are all close to zero. In addition, the distribution of the t-statistics suggests that none of these DiD estimators are statistically significant. Hence, we cannot reject the null hypothesis that the DiD estimators obtained from this randomization test are zero.

### 3.4 SHO and non-price loan terms

We have so far shown that borrowers affected by SHO experience a significant reduction in the cost of loans and this effect is stronger for bank-dependent borrowers. This finding is consistent with two plausible interpretations. First, if short sellers' incentive to uncover information reduces lenders'

monitoring and information production costs, then banks may pass on part of the cost savings to borrowers via lower loan spreads. Second, the incumbent bank may not benefit from regulation SHO given the bank already possesses inside information; however, other potential lending banks learn more about the firm from short sellers. In this case the information and monitoring benefits produced by short sellers after SHO reduces the information monopoly that incumbent banks possess over bank dependent borrowers (see, Rajan, 1992). This reduced information monopoly power will in turn force incumbent banks to lower loan spreads offered to their borrowers (or lose them to competitor banks).

To help distinguish these two alternative interpretations, we explore the effect of Regulation SHO on non-price terms of loan deals. Previous literature shows that the interest rate (loan spread) charged may not be sufficient to solve agency concerns and can lead to credit rationing (see Stiglitz and Weiss (1981) and Williamson (1986)). Later studies show that non-price terms, such as maturity, covenants, and collateralization can mitigate agency and information problems between the borrower and lender. Rajan and Winton (1995), Gorton and Kahn (2000), Gârleanu and Zwiebel (2009), and Elkamhi et al. (2015) illustrate how covenants can reduce agency costs by screening borrowers as well as by incentivizing borrowers'behavior. Diamond (1991b) and Sharpe (1991) show that reducing the maturity of debt can alleviate agency problems. Last, Stulz and Johnson (1985) and Rajan and Winton (1995) show that posting collateral can reduce agency problems between borrowers and lenders. If the regulation SHO reduces lenders perceived agency concerns, then we would expect to observe non-price loan terms to adjust (particularly among incumbent banks, which we explore below). Specifically we would predict that regulation SHO would lead to reductions in the number and tightness of covenants, to lengthening of maturity, and to the reduction in the use of collateral.

Table 7 reports the results on non-price loan terms: loan maturity, collateral, amount scaled by assets, and the number and tightness of covenants. Specifically, we estimate equations (2) and (3) with the dependent variable replaced with Ln(Maturity), Collateral, Ln(Facility/Assets), Number of covenants, and Debt/EBITDA covenant cushion. None of the coefficient estimates of key variables of interest is statistically significant, suggesting that reductions in short selling costs due to Regulation SHO do not lead to looser non-price loan terms. The non-result indicates that the influence of information collection and monitoring activities by short sellers on managerial behavior is perceived to be negligible, which suggests that short sellers in the equity markets are likely to affect the bank loan market through the reduction in information monopoly that banks possess over their borrowers.

To provide further evidence for the information monopoly argument, we conduct DiD tests on loan spreads separately for firms that received loans from their incumbent banks and firms that borrowed from new banks after Regulation SHO. The results are shown in Table 8. Both groups of firms experience significant reduction in their loan spreads. The magnitude of loan spread reductions are similar and not statistically different from each other. Unreported results show that there are no significant changes in their non-price loan terms. These results suggest that, both incumbent banks and outside banks are affected by short-sellers' information production and this information spillover is reflected in loan spreads.

# 4 Conclusion

Information spillovers between financial markets has been widely explored in the literature. Certain information produced in one market not only facilitate price discovery for other type of securities, but also provide valuable information to investors in other markets. In this paper, we

focus on information spillovers and cross monitoring between the stock and loan market. We use a regulatory experiment, Regulation SHO Pilot Program, that relaxes short selling constraints on a randomly selected sample of Russell-3000 stocks, which directly affects information production and monitoring by short sellers in the stock market but is exogenous to the loan market. In this way we break simultaneity between the stock and loan markets.

We find that while firms without bank monitors exhibit a significant decline in stock prices upon the announcement of SHO, firms with bank monitors do not react. Further analysis shows that this result is unlikely driven by firms self-selecting into borrowing. We also find evidence that firms affected by SHO enjoy a 21 basis point lower loan spread and the reduction in loan spreads increases to 36 basis points for bank-dependent firms. Regulation SHO, however, does not appear to affect non-price loan terms such as loan maturity, amount, collateral, and covenants. Overall, our evidence suggests that there are bi-directional information spillovers and cross monitoring between the stock and loan markets, and they affect the loan markets mainly through the reduction in information monopoly that banks possess over their borrowers.

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## Figure 1

This figure shows dynamics of loan spreads of Pilot and control firms around Regulation SHO Pilot program. Year -1,-2 stands for 1 year and 2 years before Pilot Program Approval (June 2004) and Year +1, +2 stands for 1 year, 2 years after Pilot program implementation (May 2005). Year 0 is the gap year between Pilot program approval and implementation. The approval date of Pilot program (June 23, 2004) is one month ahead of public announcement (July 28, 2004).

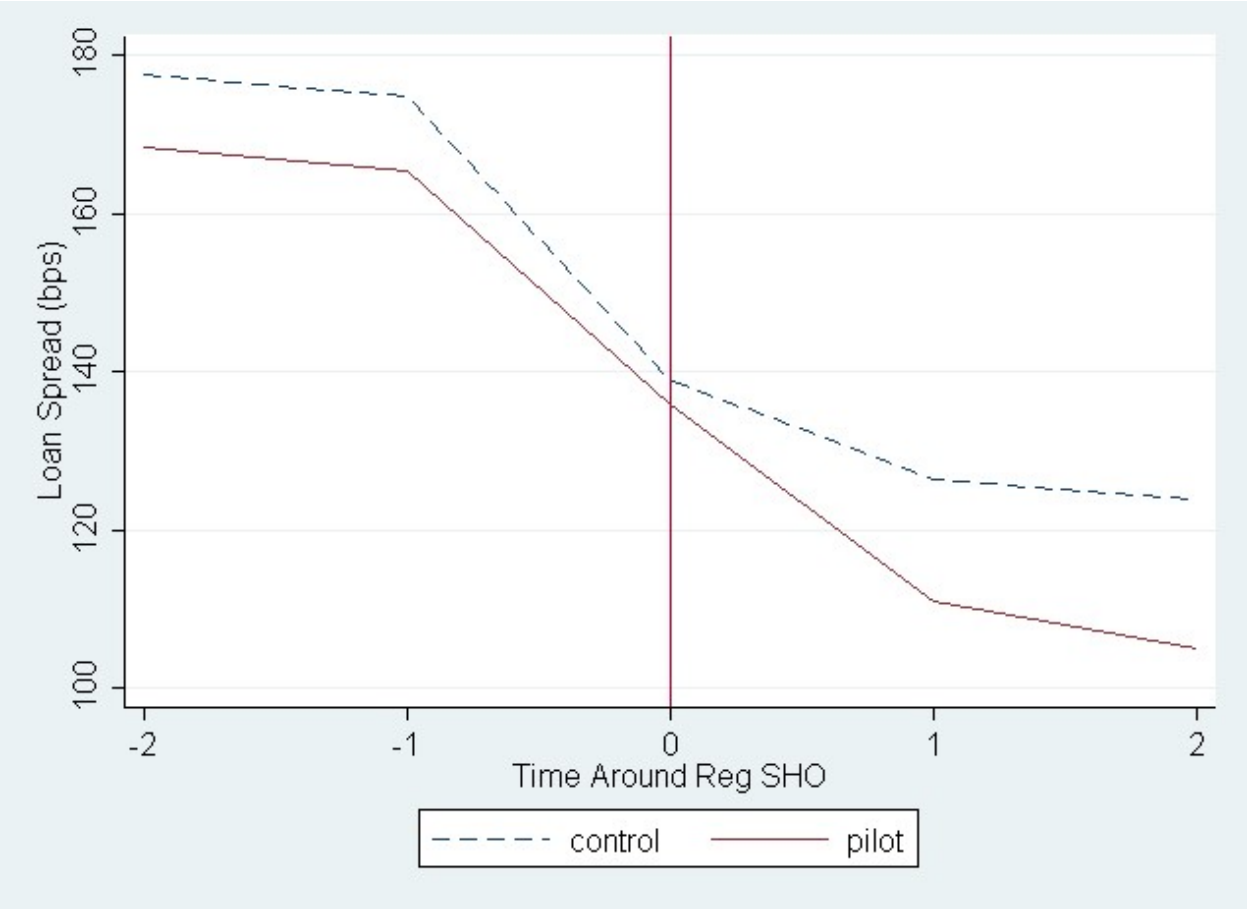


Figure 1: Dynamics of Loan Spreads around Regulation SHO

Table 1: Summary Statistics

Panel A shows summary statistics of loan characteristics for 1,621 loan facilities originated in the sample period of DiD test. Debt/EBITDA covenant cushion measure is defined by percentage difference between firms' actual Debt/EBITDA ratio and its contractual limit (Denis and Wang, 2014). The sample is drawn from non-financial, non-utility firms from June 2004 Russell 3000 Index and requires a firm to have loans origination before the approval of SHO Pilot program (June 2004) and after the implementation (May 2005). Firms are also required to be listed on NYSE, AMEX or NASDAQ through 2002 to 2007, have non-missing loan contract information in Dealscan and non-missing financial information in Compustat. Loans with primary purpose of Debtor-in-possession, share repurchase and LBO are excluded. All continuous variables are winsorized at the 5th and 95th percentiles. Panel B shows borrower characteristics from the most recent fiscal quarter end prior to loan origination. Panel C shows market-adjusted stock returns around the Regulation SHO announcement day. The event window is (-10, 1) (from July 14 to July 29, 2004). Stocks with missing price information during the event window and stocks with price smaller than 5 dollars or greater than 1000 dollars are dropped from the sample.

Panel A. Loan Characteristics

| Variable Names | N | Mean | SD | Min | P25 | Median | P75 | Max |
|---|---|---|---|---|---|---|---|---|
| Spread(bps) | 1,621 | 164.80 | 103.73 | 25 | 75 | 150 | 225 | 400 |
| Ln(Spread) | 1,621 | 4.87 | 0.74 | 3.22 | 4.32 | 5.01 | 5.42 | 5.99 |
| Maturity in Months | 1,621 | 49.79 | 22.12 | 2 | 36 | 60 | 60 | 240 |
| Ln(Facility Amount) | 1,621 | 19.29 | 1.11 | 16.81 | 18.52 | 19.34 | 20.03 | 21.28 |
| Collateral | 1,621 | 0.62 | 0.48 | 0 | 0 | 1 | 1 | 1 |
| Performance Pricing | 1,621 | 0.67 | 0.47 | 0 | 0 | 1 | 1 | 1 |
| Number of Financial Covenants | 1,182 | 2.02 | 1.36 | 0 | 1 | 2 | 3 | 6 |
| Debt/EBITDA Covenant Cushion | 357 | 0.51 | 0.28 | 0.00 | 0.30 | 0.48 | 0.74 | 1.00 |

Panel B. Borrower Characteristics Prior to Loan Origination

| Variable Names | N | Mean | SD | Min | P25 | Median | P75 | Max |
|---|---|---|---|---|---|---|---|---|
| Ln(Assets) | 1,157 | 7.68 | 1.29 | 5.49 | 6.64 | 7.63 | 8.65 | 10.04 |
| Book Leverage | 1,157 | 0.31 | 0.16 | 0.02 | 0.18 | 0.30 | 0.41 | 0.62 |
| ZSCORE | 1,130 | 4.40 | 5.62 | 0.16 | 1.26 | 2.38 | 4.73 | 23.78 |
| Tangibility | 1,157 | 0.35 | 0.24 | 0.04 | 0.15 | 0.30 | 0.54 | 0.84 |
| Profitability | 1,157 | 0.04 | 0.02 | 0.01 | 0.02 | 0.03 | 0.05 | 0.08 |
| Coverage | 1,086 | 13.24 | 18.11 | 1.22 | 3.39 | 6.27 | 13.88 | 76.60 |
| Current Ratio | 1,111 | 1.63 | 0.75 | 0.60 | 1.06 | 1.47 | 2.03 | 3.44 |
| Market to Book | 1,157 | 1.34 | 0.55 | 0.65 | 0.92 | 1.21 | 1.61 | 2.72 |

Panel C. Regulation SHO Stock Announcement Returns

| Variable Names | N | Mean | SD | Min | P25 | Median | P75 | Max |
|---|---|---|---|---|---|---|---|---|
| Daily Abnormal Return, in % | 18,468 | -0.04 | 2.89 | -50.83 | -1.17 | -0.06 | 1.04 | 73.11 |
| CAR(-10,1), in % | 1,539 | -0.67 | 9.39 | -56.93 | -4.69 | -0.10 | 3.51 | 172.78 |

Table 2: Regulation SHO Announcement Returns

This table presents the effect of an outstanding bank lending relationship on pilot firms' abnormal stock reaction to Regulation SHO announcement documented in Grullon, Michenaud, and Weston (2015). The dependent variable is 11-day market-adjusted return and event window is (-10, 1) around the Regulation SHO announcement day (from July 14 to July 29, 2004). The sample is drawn from non-financial, non-utility U.S. firms from the June 2004 Russell 3000 Index and firms are required to be listed on NYSE, AMEX or NASDAQ through 2002 to 2007 and have non-missing financial information in Compustat. Stocks with missing price information during the event window and stocks with price smaller than 5 dollars or greater than 1,000 dollars are dropped from the sample. BANKCUR is an indicator on whether a firm has an outstanding U.S. commercial bank relationship. All borrowing relationships are referred to sole lenders or lead arrangers in syndicated loans. All financial institutions are aggregated to its parent companies. Acquired firms are aggregated to their acquirers at the effective date of the merger. For Panel B, robust standard errors are shown in parentheses. ***, **, * indicate significance at the 1%, 5% and 10% levels using two-tailed tests.

Panel A. Univariate Tests

| | (1) BANKCUR=1 | (2) BANKCUR=0 | (3) Diff. (1)-(2) |
|---|---|---|---|
| Pilot | 0.01 | -3.10*** | 3.11*** |
| T-stat | (0.03) | (-5.21) | (4.17) |
| Obs | 279 | 248 | 527 |
| Control | -0.00 | -0.40 | 0.40 |
| T-stat | (-0.01) | (-0.73) | (0.63) |
| Obs | 525 | 487 | 1,012 |
| Diff (Pilot-Control) | 0.01 | -2.70*** | 2.71*** |
| T-stat | (0.03) | (-3.35) | (2.79) |
| Obs | 804 | 735 | 1,539 |

Panel B. Multivariate Tests

| | (1) | (2) |
|---|---|---|
| Pilot | -2.702*** | -2.475*** |
| | (0.807) | (0.820) |
| Pilot*BANKCUR | 2.717*** | 2.619*** |
| | (0.974) | (0.996) |
| BANKCUR | 0.395 | -0.745 |
| | (0.626) | (0.618) |
| Firm Controls | No | Yes |
| Industry Fixed Effects | No | Yes |
| Observations | 1,539 | 1,504 |
| Adjusted R-squared | 0.012 | 0.036 |

Table 3: Regulation SHO Announcement Returns by Firm and Bank Relationship Characteristics
This table presents the effect of an outstanding domestic bank relationship on the difference of Regulation SHO cumulative announcement returns between pilot and control firms documented in Grullon, Michenaud, and Weston (2015), controlling for a specific firm or bank relationship characteristic. We divide the firms in Table 2 into two subsamples based on the characteristic under examination and run equation(1) for each subsample. Control variables include size, market to book ratio, book leverage, institutional ownership and 1-digit SIC industry fixed effects. The event window is (-10,1) around the Regulation SHO announcement day (from July 14 to July 29, 2004). The sample is drawn from non-financial, non-utility U.S. firms from the June 2004 Russell 3000 Index and firms are required to be listed on NYSE, AMEX or NASDAQ through 2002 to 2007 and have non-missing financial information in Compustat. Stocks with missing price information during the event window and stocks with price smaller than 5 dollars are dropped from the sample. BANKCUR is an indicator on whether a firm has an outstanding U.S. commercial bank relationship. STRONGAMT is an indicator on whether a firm have a relationship lender and it borrows more than 2/3 loans from that lender, in terms of loan amount; LONGREL is an indicator on whether the duration of borrowing relationship (the time gap between the first and last loans from the same lender) is larger than the sample median. DOM is an indicator on whether a firm has borrowed from one of the dominant banks–Citi, JP Morgan Chase and Bank of America. All financial institutions are aggregated to its parent companies. Acquired firms are aggregated to their acquirers at the effective date of the merger. Acquiring financial firms inherit both previous lead arranger-participant relationships and previous borrowing firm relationships of the acquired firm. P-values of regression coefficients (Pilot and Pilot*BANKCUR), F-test of join significance (Pilot+Pilot*BANKCUR) and Wald test of coefficient differences are shown in parentheses. ***, **, * indicate significance at the 1%, 5% and 10% levels.

| Panel A. By Size | | | | |
|---|---|---|---|---|
| | Obs | Pilot | Pilot*BANKCUR | Pilot+Pilot*BANKCUR |
| Small | 548 | -2.89*** | 4.21* | 0.62 |
| | | (0.03) | (0.06) | (0.43) |
| Big | 956 | -1.71* | 1.65 | 0.01 |
| | | (0.07) | (0.13) | (0.92) |
| Diff (Small-Big) | | -1.18 | 2.56 | |
| | | (0.47) | (0.30) | |

| Panel B. By Credit Rating Availability | | | | |
|---|---|---|---|---|
| | Obs | Pilot | Pilot*BANKCUR | Pilot+Pilot*BANKCUR |
| With Credit Rating | 650 | -3.32*** | 3.22** | -0.10 |
| | | (0.00) | (0.01) | (0.88) |
| No Credit Rating | 854 | -2.17** | 2.72 | 0.55 |
| | | (0.04) | (0.08) | (0.59) |
| Diff (With-Without) | | -1.15 | 0.50 | |
| | | (0.45) | (0.80) | |

*(Table 3 continued)*

| Panel C. By Credit Ratings | | | | |
|---|---|---|---|---|
| | Obs | Pilot | Pilot*BANKCUR | Pilot+Pilot*BANKCUR |
| Investment Grade | 361 | -2.88** | 3.10** | 0.22 |
| | | (0.02) | (0.03) | (0.75) |
| Speculative Grade | 289 | -3.22* | 2.45 | -0.77 |
| | | (0.07) | (0.26) | (0.54) |
| Diff (Investment-Speculative) | | 0.34 | 0.65 | |
| | | (0.87) | (0.79) | |

| Panel D. By Bank Relationship Strength | | | | |
|---|---|---|---|---|
| | Obs | Pilot | Pilot*BANKCUR | Pilot+Pilot*BANKCUR |
| STRONGAMT=1 | 753 | -2.13** | 2.42 | 0.29 |
| | | (0.05) | (0.10) | (0.77) |
| STRONGAMT=0 | 751 | -3.47*** | 3.55** | 0.08 |
| | | (0.00) | (0.01) | (0.90) |
| Diff (STRONGAMT=1-STRONGAMT=0) | | 1.34 | -1.13 | |
| | | (0.41) | (0.58) | |

| Panel E. By Bank Relationship Length | | | | |
|---|---|---|---|---|
| | Obs | Pilot | Pilot*BANKCUR | Pilot+Pilot*BANKCUR |
| LONGREL=1 | 492 | -2.88* | 2.64 | -0.24 |
| | | (0.06) | (0.13) | (0.73) |
| LONGREL=0 | 1,012 | -2.45** | 2.98** | 0.53 |
| | | (0.11) | (0.02) | (0.54) |
| Diff (LONGREL=1-LONGREL=0) | | -0.43 | -0.34 | |
| | | (0.81) | (0.87) | |

| Panel F. By Lending Bank Reputation | | | | |
|---|---|---|---|---|
| | Obs | Pilot | Pilot*BANKCUR | Pilot+Pilot*BANKCUR |
| DOM=1 | 624 | -3.08** | 2.88** | -0.20 |
| | | (0.01) | (0.04) | (0.75) |
| DOM=0 | 880 | -2.29** | 2.64 | 0.35 |
| | | (0.03) | (0.08) | (0.73) |
| Diff (DOM=1-DOM=0) | | 0.79 | 0.24 | |
| | | (0.62) | (0.91) | |

P-values of regression coefficients (Pilot and Pilot*BANKCUR), F-test of join significance (Pilot+Pilot*BANKCUR) and Wald test of coefficient differences are shown in parentheses.

Table 4: DiD Diagnostics

This table compares firm and loan characteristics of pilot and controls firms immediately before Regulation SHO Pilot program. Definitions of variables can be found in Table 1 Panel A. Borrower characteristics are from the closest fiscal quarter end before the Regulation SHO Pilot Program Approval Date (June 23, 2004). Loan Characteristics are from the most recent loans that initiated before Regulation SHO Approval Date.

| Variable | Pilot | Control | Difference | T-stat | Wilcoxon z-stat |
|---|---|---|---|---|---|
| Borrower Characteristics | | | | | |
| Ln(Assets) | 7.396 | 7.547 | -0.151 | -1.045 | -0.979 |
| Book Leverage | 0.294 | 0.299 | -0.005 | -0.285 | 0.005 |
| ZSCORE | 4.541 | 4.497 | 0.044 | 0.082 | 0.184 |
| Profitability | 0.037 | 0.035 | 0.002 | 1.196 | 0.945 |
| Tangibility | 0.341 | 0.341 | 0.000 | -0.017 | 0.065 |
| Coverage | 14.832 | 17.242 | -2.410 | -0.922 | 0.039 |
| Current Ratio | 1.834 | 1.733 | 0.101 | 1.067 | 1.554 |
| Market to Book | 1.376 | 1.433 | -0.057 | -0.918 | -1.140 |
| | | | | | |
| Loan Characteristics | | | | | |
| Ln(Spread) | 4.943 | 4.946 | -0.003 | -0.043 | 0.414 |
| Ln(Maturity in Months) | 3.603 | 5.587 | -1.984 | 0.260 | 0.306 |
| Ln(Facility Amount in Dollars) | 18.998 | 19.129 | -0.131 | -1.185 | -1.090 |
| Facility Amount/Total Assets | 0.193 | 0.202 | -0.009 | -0.429 | 0.983 |
| Collateral | 0.570 | 0.553 | 0.017 | 0.337 | 0.338 |
| Number of Financial Covenants | 1.659 | 1.171 | 0.488 | -0.417 | -0.632 |
| Debt/EBITDA Covenant Cushion | 0.486 | 0.473 | 0.013 | 0.252 | 0.413 |

Table 5: DiD Test on Loan Spreads

This table shows OLS regressions on Regulation SHO Pilot program's effect on loan spreads. The dependent variable is Ln(Facility Spread). The sample is drawn from non-financial, non-utility firms from June 2004 Russell 3000 Index and requires a firm to have loans origination before the approval of SHO Pilot program (June 2004) and after the implementation (May 2005). Loans with primary purpose of debtor-in-possession, share repurchase and LBO are excluded.Loan control variables include loan facility amount, collateral, maturity, performance pricing, loan purpose, and loan types. Firm controls include firm size, market-to-book, book leverage, tangibility, profitability, and modified z-score. Panel B took the original sample firms and shows the effect when the Pilot program expired and all the firms were exempted from the uptick price test. The Post period is defined from August 2007 to August 2009.Standard errors clustered by firm are in parentheses. ***, **, * indicate significance at the 1%, 5% and 10% levels using two-tailed tests.

Panel A. Loan Spreads Around Regulation SHO

| | Full Sample | | With Credit Ratings | | Without Credit Ratings | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Pilot*During | -0.127*** | | -0.106** | | -0.216*** | |
| | (0.043) | | (0.048) | | (0.087) | |
| Pilot*Year 2 | | -0.159** | | -0.074 | | -0.451** |
| | | (0.080) | | (0.079) | | (0.177) |
| Pilot*Year 1 | | -0.115* | | -0.130* | | -0.154 |
| | | (0.069) | | (0.073) | | (0.147) |
| Pilot*Year (-1) | | -0.006 | | -0.007 | | -0.101 |
| | | (0.065) | | (0.066) | | (0.151) |
| Loan Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Firm Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Firm FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 1,621 | 1,621 | 1,177 | 1,177 | 444 | 444 |
| Adjusted R-squared | 0.532 | 0.532 | 0.548 | 0.548 | 0.520 | 0.520 |

Wald test of equality on Pilot*Year 2 between subsamples yields a p-value of 0.05.

Panel B. Loan Spreads Post Regulation SHO

| | Full Sample | With Credit Rating | Without Credit Rating |
|---|---|---|---|
| | (1) | (2) | (3) |
| Pilot*Post | 0.074 | 0.151 | 0.049 |
| | (0.112) | (0.148) | (0.146) |
| Loan Controls | Yes | Yes | Yes |
| Firm Controls | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes |
| Firm FE | Yes | Yes | Yes |
| Observations | Yes | Yes | Yes |
| Adjusted R-squared | Yes | Yes | Yes |

Table 6: Robustness Tests on Loan Spreads

This table presents three robustness checks on the DiD tests in Table 5, Panel A. Panel A is the placebo test result on loan spread when we took the original sample firms and use June 2001 as a pseudo-event announcement date and May 2002 as pseudo implementation year. Panel B reports results for randomization tests based on 5000 simulated samples. For each simulation, we draw a random sample of 135 "pilot"firms from our original sample of 410 firms, and then treat the rest of the pool (275 of them) as "non-pilot"firms. We then perform the DiD tests as in Table 5 on this simulated sample. We repeat the simulation process 5000 times and summarize the distributions of the coefficients and t-stats for the main variables of interest.

Panel A. Using 2001 as Pseudo Event Year

| | Full Sample | | With Credit Ratings | | Without Credit Ratings | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Pilot*During | -0.045 | | -0.069 | | 0.104 | |
| | (0.056) | | (0.064) | | (0.103) | |
| Pilot*Year 2 | | -0.037 | | -0.067 | | 0.102 |
| | | (0.095) | | (0.108) | | (0.139) |
| Pilot*Year 1 | | 0.065 | | 0.042 | | 0.159 |
| | | (0.097) | | (0.109) | | (0.162) |
| Pilot*Year (-1) | | 0.102 | | 0.099 | | 0.033 |
| | | (0.098) | | (0.112) | | (0.174) |
| Loan Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Firm Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Firm FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 818 | 818 | 685 | 685 | 133 | 133 |
| Adjusted R-squared | 0.374 | 0.377 | 0.395 | 0.398 | 0.475 | 0.466 |

Panel B. Randomization Tests Based on 5000 Simulated Samples

| | Mean | SD | P25 | Median | P75 |
|---|---|---|---|---|---|
| Pilot*During | 0.0005 | 0.0453 | -0.0306 | 0.0001 | 0.0307 |
| T-stat | 0.0003 | 1.0394 | -0.6982 | 0.0032 | 0.6916 |
| | | | | | |
| Pilot*Year 2 | 0.0008 | 0.0742 | -0.0490 | 0.0012 | 0.0513 |
| T-stat | 0.0280 | 1.0410 | -0.6675 | 0.0163 | 0.7237 |
| Pilot*Year 1 | 0.0010 | 0.0729 | -0.0479 | -0.0001 | 0.0491 |
| T-stat | 0.0004 | 1.0460 | -0.6918 | -0.0020 | 0.6936 |

Table 7: DiD Tests on Non-Price Terms

This table shows Regulation SHO Pilot program's effect on non-price facility level loan terms (maturity, use of collateral and amount) and deal level terms (number of covenants and covenant cushion). Model (4) is ordered probit regression and the rest columns are OLS regressions. Debt/EBITDA Covenant Cushion is percentage difference between firmsâĂŹ actual Debt/EBITDA ratio and its contractual limit (Denis and Wang, 2014). Facility -level controls (maturity, collateral, performance pricing and amount) are included in Column (1)-(3) and deal level controls (whether the deal has a term loan, number of tranches, whether the deal is secured and deal amount) are included in Column (4) and (5). The sample is drawn from non-financial, non-utility firms from June 2004 Russell 3000 Index and requires a firm to have loans origination before the approval of SHO Pilot program (June 2004) and after the implementation (May 2005). Firms are also required to be listed on NYSE, AMEX or NASDAQ through 2002 to 2007, have non-missing loan contract information in Dealscan and non-missing financial information in Compustat. Loans with primary purpose of Debtor-in-possession, share repurchase and LBO are excluded. Continuous dependent variables are winsorized at 5th percentile. Standard errors clustered by firm are in parentheses. ***, **, * indicate significance at the 1%, 5% and 10% levels using two-tailed tests.

| | Ln(Maturity) | Collateral | Ln(Facility/Assets) | Number of Financial Covenants | Debt/EBITDA Covenant Cushion |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Pilot*During | 0.028 | 0.012 | 0.038 | 0.024 | -0.007 |
| | (0.039) | (0.037) | (0.071) | (0.134) | (0.027) |
| Pilot | | | | -0.006 | |
| | | | | (0.115) | |
| Loan Controls | Yes | Yes | Yes | Yes | Yes |
| Firm Controls | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes |
| Firm FE | Yes | Yes | Yes | Yes | Yes |
| Industry FE | No | No | No | Yes | No |
| Observations | 1,621 | 1,621 | 1,621 | 1,182 | 357 |
| Adjusted/Pseudo R-squared | 0.645 | 0.113 | 0.200 | 0.074 | 0.623 |

Table 8: Loan Spreads by Whether the Firm Stayed with Incumbent Bank after Regulation SHO
This table shows changes in loan spreads around Regulation SHO for firms staying with the incumbent bank after Regulation SHO and firms switching to new banks separately. Standard errors clustered by firm are reported in parentheses. Standard errors clustered by firm are in parentheses. ***, **, * indicate significance at the 1%, 5% and 10% levels using two-tailed tests.

| | Firms Staying with Incumbent Banks (1) | Firms Switching to New Banks (2)) |
|---|---|---|
| Pilot*During | -0.096* | -0.160** |
| | (0.054) | (0.069) |
| Loan Controls | Yes | Yes |
| Firm Controls | Yes | Yes |
| Year FE | Yes | Yes |
| Firm FE | Yes | Yes |
| Observations | 740 | 867 |
| Adjusted R-squared | 0.583 | 0.513 |