Shareholder Financial Difficulties and Firms' Risk-Shifting Behavior: Evidence from the 2003 Mutual Fund Scandal

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Abstract

We examine how shareholder financial difficulties affect firms' risk-shifting behavior. Using the 2003 mutual fund scandal as a financial shock to institutions' risk-shifting incentives, we find that lenders charge higher loan spreads after the scandal. The results are more evident when the scandal is more severe, when tainted institutions have poorer performance and stronger abilities to influence firms, and when firms have higher shareholder-debtholder conflict and greater information asymmetry. Lenders also impose more covenants after the scandal. We further find that bond and stock returns around scandal announcements are negatively correlated and that firms' post-scandal investments and riskiness increase significantly.

***Keywords**:* Risk shifting, Shareholder-debtholder conflict, 2003 mutual fund scandal, Cost of debt, Covenant, Tainted institution, Information asymmetry

***JEL Classification**:* G21, G23, G32, M41

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1. Introduction

Conflicts of interest between shareholders and debtholders have been one of the most important issues in finance (e.g., Jensen and Meckling, 1976; Harris and Raviv, 1991; Leland, 1994). With risky debt outstanding, shareholders, as residual claimholders, have strong incentives to transfer resources from debtholders to themselves by influencing management to overinvest in risky projects or pay out excessive dividends (referred to as "asset substitution" or "risk shifting"). Evidence on asset substitution, however, is mixed, largely due to the endogenous nature of leverage and risk-taking and the lack of data.[1] Moreover, there is limited evidence about how shareholder characteristics and financial conditions affect their incentives to influence management to engage in risk shifting because most prior studies focus mainly on firm characteristics, such as debt structure and managerial compensation, when examining firms' risk-shifting behavior.[2] This lack of evidence is surprising given that large shareholders are the main beneficiaries of asset substitution and thus should have a strong influence on managers' risk-shifting decisions.

In this paper, we extend prior studies on asset substitution by examining how an unexpected large deterioration in shareholder financial conditions aggravates lenders' concern about risk shifting and thus affects the ex ante loan contract terms of firms whose shares are held by shareholders suffering from such a deterioration. Specifically, using the 2003 mutual fund scandal that led to massive fund outflows and declines in fund values as an exogenous source of variation in tainted institutions' risk-shifting incentives, we investigate how lenders tighten the price and nonprice loan terms of firms held by these institutions to protect against anticipated shareholder misappropriation of assets. We also examine whether the

[1] For example, Leland (1998) and Parrino and Weisbach (1999) show that the costs associated with risk shifting are low, suggesting that risk shifting is not a significant factor that affects a firm's optimal capital structure. Gilje (2016), using a natural experiment with exogenous changes to leverage, also shows that firms in the oil and gas industry reduce risk when their leverage is high and when they approach distress. In contrast, Ericsson (2000) finds that the costs of risk shifting reduce a firm's optimal leverage by up to 20%. Eisdorfer (2008) further shows that the uncertainty of financially distressed firms is associated with an increase in investment and that this increase in investment reduces asset value.

[2] See Smith and Warner (1979), Green (1984), and Barnea et al. (1980) for the relation between a firm's debt structure and its risk-shifting behavior and Brander and Poitevin (1992), John and John (1993), and Subramanian (2003) for the effect of managerial compensation contracts on a firm's risk-shifting behavior.

characteristics of tainted shareholders and firms, such as shareholders' pre-scandal performance and abilities to influence their portfolio firms, and the levels of firms' shareholder-debtholder conflict and information asymmetry affect lenders' incentives to adjust anticipated costs arising from an increase in potential risk shifting. Finally, we investigate how firms' risk-taking behavior changes in the post-scandal period.

The 2003 mutual fund scandal was one of the largest scandals in the financial advisory industry over the past few decades. On September 3, 2003, New York Attorney General Eliot Spitzer accused several funds belonging to four mutual fund groups (i.e., Bank of America, Bank One, Janus, and Strong) of engaging in illegal trading activities, such as late trading and market timing. Thereafter, at least 26 large fund families were implicated in the scandal during the 2003-2004 period and charged for their illegal attempts to benefit favored clients at the expense of other clients.[3] As a result, tainted institutions suffered massive money outflows after the scandal. According to a news report in *Financial News* in November 2003, two months after news of the scandal broke, investors had pulled nearly $35 billion from the six largest mutual funds implicated in the scandal, mainly due to their concern that the funds' illegal trading activities would damage their investments.[4] Figure 1 plots the size-weighted mean cumulative monthly net flows (%) of implicated and non-implicated fund families from the scandal announcement month to the subsequent three years, where each fund family's total net assets is used as the weight. Implicated fund families lost 10.8% of their initial total net assets within one year after the scandal announcement, while non-implicated fund families accumulated 9.5% more capital during the same period. This significant negative fund flow for implicated fund families translated into considerable revenue loss, which continued

[3] Late trading is an illegal practice that allows some clients of funds to purchase fund shares at the closing price after the market closes. Due to the differences in trading time across countries, funds' clients can use their private information available in other countries' markets for late trading, which allows them to profit at the expense of funds' other long-term buy-and-hold clients. Market timing as an investment strategy is not illegal per se, but by allowing selected clients to trade more frequently than specified in fund documents and prospectus, it dilutes the value of fund shares and increases the costs of administering the fund borne by other clients, which can be considered fraudulent.
[4] See https://www.fnlondon.com/articles/investors-pull-from-tainted-funds-20031201.

to worsen in subsequent years.[5]

Institutional shareholders, especially mutual funds, hold a significant portion of equity in the U.S. stock market. Since mutual funds' revenue comes mainly from fees based on assets under management, they care about money inflows and outflows from their clients. Therefore, any negative shocks that result in massive outflows, such as the 2003 mutual fund scandal, can have a significant financial impact on these institutions and thus are expected to incentivize them to take actions that help mitigate the adverse effect of such shocks. For example, to prevent further outflows and attract new inflows, affected institutions may attempt to boost their short-term performance. They may also influence their portfolio firms to engage in aggressive risk-taking in order to increase their wealth at the expense of debtholders. Anticipating the increased risk-shifting behavior of firms owned by tainted institutions, ex ante, lenders that want to protect their claims on firm value should pass the costs associated with such behavior along to firms by charging higher loan spreads and imposing more stringent covenants (Jensen and Meckling, 1976). Thus, firms held by institutional block shareholders implicated in the scandal likely experience a larger increase in loan spreads and a stricter covenant requirement in the post-scandal period than other firms. Given that the 2003 mutual fund scandal was driven entirely by institutions' illegal behavior and not by their portfolio firms, it should represent a setting where firms face an exogenous shock to their tainted institutions' increased incentives for risk shifting.[6]

Risk-shifting theory would expect the effects of tainted institutions' financial difficulties on loan spreads to vary with the severity of these institutions' illegal activities during the 2003 scandal, their characteristics, and firm characteristics. First, risk-shifting theory predicts that the negative effect of shareholder scandals on loan spreads is more pronounced for firms held by tainted institutions that suffer more from their illegal activities. To the extent that the effect of institutions' illegal activities on their

[5] Houge and Wellman (2005) estimate that the fee income lost across all funds managed by implicated fund families was more than $844 million in the six months following the scandal announcement.

[6] Consistent with this view, Antón and Polk (2014) argue that the 2003 mutual fund scandal helps eliminate the concern about the endogeneity of fund managers' stock investment decisions, and Crane, Koch, and Michenaud (2019) use this scandal as a shock that provides plausibly exogenous variation in clique ownership.

financial health is greater when these activities are more severe, institutions with a higher severity of illegal activities are expected to have stronger incentives to transfer wealth from debtholders to themselves by influencing their portfolio firms to engage more aggressively in risk-shifting activities. Anticipating such an increase in risk-shifting activities, a lender should adjust the loan spreads of firms held by these institutions more adversely to alleviate its perceived risk of high asset substitution.

Second, risk-shifting theory predicts that the adverse effect of shareholder scandals on the cost of bank debt is more evident when tainted institutions perform poorly prior to the scandal and when they can exert a strong influence on their portfolio firms. Compared to well-performing tainted institutions, poorly performing tainted institutions are likely to be more severely affected by the scandal because large fund outflows and the corresponding revenue loss would significantly increase their financial distress risk. Thus, poorly performing tainted institutions should have stronger risk-shifting incentives to appropriate their portfolio firms' assets, which leads to an increase in loan spreads for their portfolio firms. Lenders' concern about risk shifting is also expected to be greater when tainted institutions have a strong influence on their portfolio firms. For example, tainted institutions with higher ownership in their portfolio firms before a scandal or those that hold shares of firms for a longer period are likely to more actively influence management (Shleifer and Vishny, 1986; Bushee, 2001; Chen, Harford, and Li, 2007). To the extent that institutions suffering from financial difficulties have strong incentives to implement policies that allow them to quickly recover, they may prefer short-term profits to long-term profits. Consequently, if the short-term benefits of firms' risk-shifting activities are larger than the long-term benefits of institutions' monitoring of firm management, tainted institutions that have stronger power over firms are likely to influence their portfolio firms to engage more aggressively in risk shifting, leading to higher borrowing costs to these firms.

Third, we predict that the adverse effect of shareholders' financial difficulties on loan spreads is more severe for firms with higher shareholder-debtholder conflict. Debtholders' concerns about potential wealth transfer from themselves to shareholders are likely to be greater if firms have maintained policies that favor shareholders, such as policies that lead to excessive dividend payout, overinvestment, and high risk. Since

these policies hurt debtholders by exacerbating asset substitution problems, lenders that want to internalize the costs of such problems will demand higher interest rates when their borrowers' shareholders suffer from large financial difficulties.

Finally, we expect the adverse effect of shareholder scandals on loan spreads to be more evident for firms with higher information asymmetry. To alleviate the risk of being expropriated due to increased risk shifting in firms held by tainted block institutions, lenders may have to incur larger costs to obtain relevant information on firms with higher information asymmetry and to monitor these firms effectively. Because the information asymmetry vis-à-vis borrowers makes it difficult for lenders to assess the extent of the change in borrowers' risk-taking behaviors and requires lenders to undertake more intense due diligence and monitoring (Sufi, 2007), lenders are expected to change loan prices more unfavorably when tainted institutions' portfolio firms have a less transparent information environment.[7]

To test these predictions, we use a propensity score matched sample of treatment firms whose shares are held by tainted block institutions in the year immediately before the scandal and their control firms whose shares are not held by tainted block institutions in the same year. To ensure that tainted institutions continuously influence treatment firms after the scandal, we require that the same tainted block institutions continuously hold equity ownership in treatment firms for three years after the scandal, either as blockholders or as top ten institutional shareholders with at least 1% ownership.[8] We also require that tainted institutions do not hold equity in control firms for three years after the scandal as blockholders to ensure that control firms' risk shifting has nothing to do with tainted institutions' financial problems arising from the scandal. These propensity score matching procedures and restrictions help alleviate the potential endogeneity problem that shareholders' risk-shifting incentives are affected by firm-specific characteristics.

[7] Consistent with this prediction, Cheng and Subramanyam (2008) and Mansi, Maxwell, and Miller (2011) find that analysts' information production reduces bond spreads and improves credit ratings.

[8] Given that many financial block institutions do not hold equity in the same firms longer than one year (e.g., Kang, Luo, and Na, 2018), requiring institutions to be blockholders of the same firms for three years leaves us few treatment firms. Thus, we include firms whose tainted institutions sell some of their block ownership and become top ten institutional shareholders with at least 1% ownership after the scandal in treatment firms. We use a three-year holding period to fully capture the potential effect of scandals on firms' risk-taking behavior.

We then perform difference-in-differences tests over the three years before and the three years after the scandal.

Consistent with our hypothesis, we find that treatment firms experience a significant increase of 32.63 basis points in loan spreads in the three years following the scandal, which translates into 17.94% of the sample mean.[9] We also find that the adverse effects of the scandal on loan spreads are more pronounced when the misconduct is more severe, as measured by tainted institutions' lower scandal abnormal announcement stock returns, when tainted institutions perform poorly before the scandal, and when tainted institutions own larger shares in their portfolio firms and hold shares for a longer period before the scandal. Thus, lenders are more concerned about the potential increase in risk-shifting activities when tainted institutions have stronger incentives and power to influence their portfolio firms.

We further find that the adverse effects of shareholder scandals on loan spreads are more pronounced for treatment firms that pay out excessively, treatment firms that overinvest, and treatment firms that have higher risk, as measured by cash flow volatility. Given that these firms have greater shareholder-debtholder conflict, the results suggest that firms' failure to adopt debtholder-friendly policies accentuates lenders' concern about potential asset substitution problems. The adverse effects are also particularly evident among informationally more opaque treatment firms, such as smaller firms, firms with lower analyst following, and firms with higher forecast dispersion. These results suggest that high information asymmetry vis-à-vis borrowers makes it difficult for banks to assess borrowers' opportunistic behaviors and incentives to engage in risk shifting due to a negative shareholder shock, resulting in higher loan spreads.

In addition, we find that lenders impose stricter general covenants on treatment firms after the scandal but do not impose stricter financial covenants. Unlike financial covenants, which place limits on various accounting variables and ratios that must be maintained while debt is outstanding, general covenants usually specify early retirement of loans conditional on events, such as asset sales, equity issuance, debt issuance, excess cash flow, and insurance proceeds. General covenants also restrict the payment of excessive

[9] Some tainted bank institutions, such as Bank of America and Bank One, can serve as both shareholders and lenders. Our results are qualitatively the same if we exclude loans issued by these bank institutions from the sample.

dividends and mandate voting to approve changes to items such as term changes and collateral release. To the extent that risk-shifting activities generally involve misappropriation of assets and increases in payouts and firm risk, our results suggest that lenders prefer general covenants, which are more suitable to limit borrowers' risk-taking activities, to financial covenants to alleviate their concerns about a potential increase in firms' risk shifting resulting from tainted institutions' financial shock.

Next, to shed additional light on the evidence of risk shifting caused by the scandal, we examine the comovement between stock and bond abnormal returns around the scandal announcement date. Potential wealth transfers from debtholders to shareholders (i.e., risk shifting) suggest negative abnormal bond returns, non-negative abnormal stock returns, and a negative correlation between these two abnormal returns. Consistent with these predictions, we find that the mean and median cumulative abnormal returns (CARs) for bonds of affected firms with tainted institutions from the scandal announcement date to one day after the scandal announcement date, CARs (0, 1), are -0.6% and -0.2%, respectively, both of which are significant at the 5% level. In contrast, the corresponding mean and median CARs (0, 1) for stocks are insignificant 0.0% and 0.1%, respectively. Moreover, the Pearson correlations of bond CARs (0, 1) with stock CARs (-1, 1) and stock CARs (-1, 0) are significantly negative, with coefficients of -0.19 and -0.39, respectively. We also find that the Pearson correlation between bond CARs (-1, 1) and stock CARs (-1, 0) is a significant -0.24. Consistent with our prior findings that the adverse effects of the scandal on loan spreads vary with institutional characteristics, we find that bond market reactions to the scandal announcement are more negative when the misconduct is more severe and when tainted institutions perform poorly before the scandal.[10]

Finally, we examine whether firms' risk-taking behaviors change in the post-scandal period. We find that treatment firms' investment and riskiness increase significantly after the scandal. Thus, ex post, firms held by tainted institutions engage in more post-scandal risk-taking activities, which suggests that designing

[10] The magnitudes of the bond market reactions are also more negative for affected firms in which institutions own larger block ownership before the scandal and hold shares for a longer period, although the differences in bond CARs between the two groups are not significant.

loan contracts that can completely eliminate debtholder-shareholder conflict, including asset substitution problems, is costly (Jensen and Meckling, 1976; Grossman and Hart, 1986; Hart and Moore, 1990; Hart, 1995).

Our paper contributes to the literature in several important ways. First, it adds to the literature on risk shifting. Although a large number of theoretical studies (e.g., Jensen and Meckling, 1976; Harris and Raviv, 1991; Leland, 1994) show how shareholder-debtholder conflict exacerbates firms' risk-shifting behavior and affect firm policies, empirical evidence on the existence and magnitude of risk shifting is mixed due to the endogeneity of leverage and risk-taking (e.g., Leland, 1998; Parrino and Weisbach, 1999; Ericsson, 2000; Eisdorfer, 2008; Gilje, 2016). Using the 2003 mutual fund scandal as a quasi-shareholder shock to shareholders' increased incentives for risk shifting, we show that lenders, anticipating an increase in such risk shifting, charge significantly higher loan spreads to firms with tainted institutions and impose more stringent covenants.

Moreover, unlike prior studies that focus mainly on firm characteristics, we focus on shareholder characteristics when examining risk shifting. For example, Smith and Warner (1979) discuss how firms' secured debt changes their risk-shifting incentives, and Brander and Poitevin (1992), John and John (1993), and Subramanian (2003) show that firms' compensation structure affects risk shifting. Instead of focusing on these firm characteristics, we consider shareholders' financial conditions and their abilities to influence portfolio firms when assessing shareholder-debtholder conflict and show that these shareholder characteristics significantly affect the price terms of loans.

Second, our study adds to the strand of literature that examines the determinants of loan contract terms. Prior literature investigates how credit risk, information risk, and shareholder rights affect loan contract terms. For example, Graham, Li, and Qiu (2008) show that loans initiated after financial restatements, which increase firms' credit risk and information risk, have significantly higher spreads and more covenant restrictions. Chava, Livdan, and Purnanandam (2009) find that a firm's lower takeover defense significantly increases its cost of bank loans. We document that firms whose shareholders encounter financial difficulties suffer from increases in loan spreads and covenants, which suggests that lenders take the changes in

shareholders' financial conditions into account when assessing loan contract terms.

Finally, we contribute to the literature on misconduct by financial advisors. Unlike prior studies that examine the effect of this misconduct on the financial advisory industry and management firms (Qian, 2006; Dimmock, Gerken, and Graham, 2018; Egan, Matvos, and Seru, 2018), we study how misconduct by financial advisors affects portfolio firms' risk-taking behaviors and debtholders' reassessment of loan contract terms, thus calling attention to the broader impact of misconduct by financial advisors on the economy.

The rest of the paper is organized as follows. In Section 2, we discuss the data and sample characteristics. In Section 3, we investigate the effect of the scandal on loan spreads using difference-in-differences regressions with a propensity score matched sample. Section 4 discusses cross-sectional heterogeneity in the impact of the scandal on loan prices. In Section 5, we investigate the effect of the scandal on loan covenants, and in Section 6, we examine the comovements between stock and bond abnormal returns around the scandal announcement date. Section 7 investigates how firms' risk-taking behavior changes after the scandal. Section 8 summarizes and concludes the paper.

2. Sample and Summary Statistics

2.1. Sample

To construct our sample firms, we start with 26 mutual fund families implicated in the 2003 mutual fund scandal. We collect a list of these implicated fund families from various sources, including *Factiva* and administrative proceedings by the Securities and Exchange Commission (SEC). Information on implicated mutual fund families' investment advisors and ultimate parent firms (hereafter called the "tainted institutions") is obtained from their websites and 10-K filings. Table 1 provides the details about implicated fund families, including the initial public reporting date of the implication and assets under management (AUM) of implicated investment advisors. We obtain information on AUM from the Form ADV filed by implicated investment advisors immediately before the initial public scandal reporting date. The total AUM of implicated investment advisors is approximately $1.66 trillion, accounting for more than 20% of the

mutual fund industry's AUM (McCabe, 2009).

We obtain information on tainted institutions' block equity ownership in each portfolio firm from the Thomson Reuters Institutional (13F) Holdings database. We focus on tainted institutions, not on fund families, to fully consider the adverse spillover effect of implicated fund families on tainted institutions' other non-implicated fund families and the deterioration of tainted institutions' financial conditions caused by their implicated subsidiaries' businesses in the mutual fund industry. Moreover, tainted institutions can exert a strong influence on portfolio firms of their mutual fund families through their direct control of fund families.

Next, we classify firms that have financial and stock return data available in Compustat and the Center for Research in Security Prices (CRSP), respectively, into affected and non-affected firms. Affected firms are firms whose shares are held by tainted block institutions in the year immediately before the scandal year (i.e., 2003 or 2004, in which regulatory agencies charge mutual fund families for their illegal trading activities), and non-affected firms are firms whose shares are not held by tainted block institutions but are held by at least one non-tainted institutional blockholder in the same year. We exclude firms that are not held by any block institution and those that are not listed on the New York Stock Exchange (NYSE), Nasdaq, or the American Stock Exchange with common shares (i.e., CRSP share codes = 10 or 11). We also exclude firms that belong to financial (Standard Industry Classification (SIC) codes 6000-6999) or utility (SIC codes 4900-4999) industries. These procedures result in 729 unique affected firms in 2003 and 2004 (770 firm-year observations) and 2,707 unique non-affected firms (4,210 firm-year observations) in the same years. The affected and non-affected firms have 426 and 2,099 loan facilities, respectively, reported in the Loan Pricing Corporation's (LPC) DealScan database in the year immediately before the scandal year.[11]

Finally, to mitigate the endogeneity concern associated with observable omitted variable bias, we use a propensity score matching approach. Specifically, we obtain the propensity score using the logit

[11] We thank Michael Roberts for sharing the DealScan-Compustat link file on his website: http://finance.wharton.upenn.edu/~mrrobert/.

regression of *Treatment* (an indicator that takes the value of one if a firm's shares are held by tainted block institutions in the year immediately before the scandal year and zero otherwise) on the natural logarithm of total assets, book leverage, stock return volatility, ROA, excess stock return, Tobin's *q*, tangibility, and institutional block ownership. We choose these matching variables since the previous literature shows that firm risk, performance, growth opportunity, the availability of collateral, and corporate governance are important determinants of the price and nonprice terms of loan contacts (Graham, Li, and Qiu, 2008; Lin et al., 2013). We require the treatment and control firms to be in the same industry (the same Fama-French 48 industry classification code) and in the same fiscal year and to have the same credit rating classes (i.e., no rating, non-investment rating (below BBB-), and investment rating (above BBB-)). We also require that the treatment and control firms have at least one loan issued in the periods both three years before and three years after the scandal year. In addition, to ensure that tainted institutions continuously influence treatment firms after the scandal, we require that the same tainted block institutions continuously hold equity ownership in treatment firms for three years after the scandal either as blockholders or as top ten institutional shareholders with at least 1% ownership. We further require that no tainted institutions hold equity in control firms for three years after the scandal as blockholders to ensure that control firms' risk shifting has nothing to do with their institutional shareholders' scandal. Each treatment firm is matched to a control firm with the closest propensity score (a caliper of 0.05) and one-to-one matching without replacement. Our final sample consists of 707 firm-loan observations (326 loans issued by 65 treatment firms and 381 loans issued by 65 control firms) from 2000 to 2007.[12]

2.2. Summary statistics

[12] Not all tainted institutions of the 26 implicated mutual funds are used in our propensity scored matched sample because some tainted institutions are acquired within three years after the scandal, and they stop filing Form 13F to the SEC, which makes it difficult to obtain information on their equity holdings (e.g., FleetBoston). Other tainted institutions also do not continuously hold equity in the same firms for the entire three years after the scandal (e.g., Federated and Strong). Of the 26 tainted institutions, 12 are used in our propensity score matching analyses: Allianz Dresdner, AXA Financial, Bank of America, Deutsche Bank, Franklin Resources, Gabelli, Heartland Advisor, J.W. Seligman & Co., Janus Capital Management, Putnam Investment Management, United States Trust, and Wachovia.

Panel A of Table 2 presents summary statistics for the sample of 770 affected firm-year observations in the year immediately before the scandal year and 4,210 non-affected firm-year observations in the same year. We find that compared to non-affected firms, affected firms are larger, perform better (i.e., higher ROA), and have lower risk (i.e., lower stock return volatility) before the scandal, which suggests that tainted institutions hold equity in well-performing, low-risk firms. However, affected firms have worse stock performance and lower Tobin's *q*. We also find that affected firms have significantly higher leverage and larger institutional block ownership. Overall, these results suggest that affected and non-affected firms differ in several firm characteristics. Panel A also shows loan characteristics for the sample of 426 loans issued by affected firms and 2,099 loans issued by non-affected firms in the year immediately before the scandal year. Loan maturity and the frequency of imposing collateral requirements (i.e., secured loans) are not significantly different between loans issued by affected and non-affected firms. However, affected firms' loans are larger, have lower spreads, and carry a smaller number of total covenants than those of non-affected firms, possibly due to their better operating performance and lower risk. We measure the loan spread by all-in-spread-drawn, a rate that a firm pays in basis points over LIBOR or the LIBOR equivalent, and we measure the number of total covenants by the sum of the number of general covenants, the number of financial covenants, and the value of an indicator for whether the loan requires collateral.

Panel B of Table 2 presents the differences in descriptive statistics between the 65 treatment firms and 65 control firms used in the propensity score matched analysis. None of the firm characteristics differ significantly between them. Thus, our propensity score matching approach identifies control firms that have very similar characteristics as treatment firms.

3. Effect of Shareholder Scandals on Loan Spreads

In this section, to examine the impact of shareholder scandals on loan spreads, we perform difference-in-differences tests using a propensity score matched sample of 707 firm-loan observations. Specifically, we estimate ordinary least squares (OLS) regressions in which the dependent variable is the natural logarithm of a loan spread. Our key independent variable of interest is an interaction term between

Treatment and *Post*. *Treatment* is an indicator that takes the value of one if a firm's shares are held by the tainted block institution in the year immediately before the scandal year and zero otherwise. *Post* is an indicator that takes the value of one for a firm-year in the post-scandal period (year *t+1*, year *t+2*, and year *t+3*) and zero for a firm-year in the pre-scandal period (year *t–1*, year *t–2*, year *t–3*), where year *t* is the scandal year. We control for the variables used in propensity score matching in the regressions.[13] We also control for loan-specific characteristics, including loan maturity, loan amount, and performance pricing (an indicator that takes the value of one if a loan contract has the performance pricing feature and zero otherwise), and macroeconomic variables, including credit spread (the difference in yields between BAA and AAA corporate bonds obtained from Federal Reserve Economic Data) and term spread (the difference in yields between 10-year and 2-year treasury bonds obtained from Federal Reserve Economic Data). All firm characteristics are measured as of the fiscal year-end immediately before the loan active date. Macroeconomic variables are measured as of the month immediately before the loan active date. We further include loan purpose, loan type, firm, and year (or industry-by-year) fixed effects to control for unobservable omitted loan and firm characteristics and time trends. We estimate *p*-values using robust standard errors that adjust for heteroskedasticity (White, 1980) and cluster standard errors at the firm level. We winsorize all continuous variables at the 1st and 99th percentiles to mitigate the impact of outliers on the results.

Table 3 reports the results. In column (1), we find that the coefficient on the interaction term between *Treatment* and *Post* is positive and significant at the 5% level.[14] The coefficient estimate of 0.165 suggests that loan spreads of firms held by tainted block institutions increase by 17.94% ($e^{0.165} - 1$) after a scandal. Given that the average loan spread for the propensity score matched sample is 181.87 basis points, this

[13] Tainted institutions may engage in large net selling in the post-scandal period due to an unusually high volume of redemption requests, which likely puts a downward pressure on the prices of stocks held by these tainted institutions. To control for the effect of this downward price pressure on loan spreads, in untabulated tests, we include the Amihud (2002) illiquidity measure (the average ratio of the daily absolute return to the daily dollar trading volume in a given year) as an additional control in the regression. The results remain qualitatively the same.

[14] *Treatment* and *Post* are included as separate variables in all regressions but absorbed by firm and year (industry-by-year) fixed effects, respectively.

number indicates that firms held by tainted block institutions experience an increase of 32.63 basis points in the average loan spread.

In column (2), we decompose *Treatment (indicator)* into two indicators: *Treatment with low (high) institution CAR*, which takes the value of one if the tainted institution's CAR (-1, 1) around the scandal announcement is below (above) its sample median CAR (-1, 1) and zero otherwise. We estimate the CAR using the market model of 220 trading days of returns beginning 280 days before and ending 61 days before the scandal announcement date. We use the CRSP value-weighted return as a proxy for the market return. Consistent with our prediction, we find that the coefficient on the interaction term between *Treatment with low institution CAR* and *Post* is positive and significant at the 1% level, while that between *Treatment with high institution CAR* and *Post* is insignificant: the coefficient on *Treatment with low institution CAR* × *Post* is a significant 0.303, indicating that banks charge higher loan spreads of 35.39% ($e^{0.303}$-1) for firms whose tainted block institutions experience a larger drop in share value around the scandal announcement date than for other firms. The difference in coefficients between *Treatment with low institution CAR* × *Post* and *Treatment with high institution CAR* × *Post* is significant at the 5% level.

In columns (3) and (4), to control for unobservable time-varying differences across industries, we repeat the analyses in columns (1) and (2) by replacing year fixed effects with industry-by-year fixed effects. Our results remain qualitatively the same.

Overall, these results are consistent with our hypothesis that shareholders facing financial difficulties are incentivized to influence their portfolio firms to engage in risk shifting and that lenders, anticipating firms' increased risk-shifting behavior, charge higher loan spreads to these firms.

A key assumption of the difference-in-differences test is that treatment and control firms have parallel trends in loan spreads before the scandal. To assess the validity of the parallel trend assumption behind our difference-in-differences test, we follow Bertrand and Mullainathan (2003) and Jiang, Levine, and Lin (2016) and replace the interaction term between *Treatment* and *Post* in columns (1) and (3) of Table 3 with five indicators, $Before^{-2}$, $Before^{-1}$, $After^{+1}$, $After^{+2}$, and $After^{+3}$, where $Before^{-n}$ is an indicator that takes the value of one for treatment firms at year *t–n* and zero otherwise, and $After^{+n}$ is an indicator that takes the

value of one for treatment firms at year $t+n$ and zero otherwise. Thus, the coefficient on $Before^{-n}$ ($After^{+n}$) captures how the loan spreads of treatment firms at year $t–n$ (year $t+n$) are different from those of treatment and control firms at year $t–3$. If the parallel trend assumption holds, we should observe an insignificant coefficient on $Before^{-2}$ *and* $Before^{-1}$.

Table 4 presents the results. We find that the coefficients on both $Before^{-2}$ *and* $Before^{-1}$ are insignificant, suggesting that the loan spreads of treatment and control firms move in parallel prior to the scandal year. However, the coefficients on $After^{+1}$, $After^{+2}$, and $After^{+3}$ are positive and significant. These results support the parallel trend assumption behind our difference-in-differences test, verifying the exogeneity of the scandal event and its causal effect on the loan prices of treatment firms.

4. Cross-Sectional Heterogeneity in Changes in Loan Spreads

In this section, we examine how the effects of shareholder scandals on loan spreads differ across tainted institution characteristics (i.e., past performance and abilities to influence portfolio firms) and firm characteristics (i.e., shareholder-debtholder conflict and information environment).

4.1. Tainted institution characteristics

We first examine cross-sectional heterogeneity in the effects of shareholder scandals on loan prices across firms with different tainted institution characteristics. Lenders' concern about borrowers' risk-shifting behaviors tends to be particularly severe when tainted institutions perform poorly before the scandal because poor performance increases tainted institutions' financial distress risk and thus further incentivizes them to influence their portfolio firms to engage in risk-shifting activities. Consequently, our results in Table 3 are expected to be more pronounced for firms with poorly performing tainted institutions than for those with well-performing tainted institutions. Lenders' concern about borrowers' risk-shifting behaviors is also likely to be stronger when tainted institutions' influence on their portfolio firms is greater, such as when they have larger ownership in portfolio firms and when they serve as long-term shareholders of portfolio firms. When these institutions suffer from financial shocks, their strong power over firms will

allow them to influence firms aggressively to pursue risk-shifting activities, which helps boost their short-term profits. Thus, lenders likely charge higher loan spreads to firms whose tainted institutions have greater power over firms.

To test these predictions, we divide treatment firms into two subgroups according to their tainted institutions' performance and power prior to the scandal. We measure tainted institutions' performance by Tobin's *q* (the ratio of the book value of total assets minus the book value of equity plus the market value of equity to the book value of total assets) and operating performance (the ratio of operating income before depreciation to total assets). We measure tainted institutions' power over firms by their blockholding period (the number of quarters in the pre-scandal period during which the tainted block institution holds block ownership in treatment firms) and their block ownership (the percentage of ownership held by the tainted block institution at the beginning of the scandal year). We then decompose *Treatment (indicator)* into two indicators: *Treatment with poor institution performance* and *Treatment with good institution performance*. Similarly, we decompose *Treatment (indicator)* into *Treatment with high institution influence* and *Treatment with low institution influence*. *Treatment with poor (good) institution performance* takes the value of one if the tainted institution's Tobin's *q* or operating performance before the scandal year is below (above) the sample median Tobin's *q* or operating performance and zero otherwise. *Treatment with high (low) institution influence* takes the value of one if the tainted institution's blockholding period or block ownership in treatment firms before the scandal year is above (below) the sample median blockholding period or block ownership and zero otherwise.

Panel A of Table 5 reports the results for difference-in-differences tests in which we use Tobin's *q* (columns (1) and (2)) and operating performance (columns (3) and (4)) of tainted institutions as measures of their performance. We find that the coefficient on *Treatment with poor institution performance* × *Post* is positive and significant in all four columns, while the coefficient on *Treatment with good institution performance* × *Post* is insignificant in these columns. The differences in coefficients between the two interaction terms are significant in all four columns. Thus, lenders adjust loan prices more aggressively for treatment firms whose tainted institutions perform poorly before the scandal.

Panel B of Table 5 presents the results for difference-in-differences tests in which we use tainted institutions' pre-scandal blockholding period (columns (1) and (2)) and block ownership in firms (columns (3) and (4)) as measures of their power over firms. We find positive and significant coefficients on *Treatment with high institution influence* × *Post* in all four columns. In contrast, the coefficient on *Treatment with low institution influence* × *Post* is insignificant in all columns except column (3). The differences in coefficients between the two interaction terms are significant in columns (1) and (2). Overall, these results suggest that tainted shareholders' strong power over firms exacerbates lenders' concern about risk shifting.

4.2. Firm characteristics

4.2.1 Shareholder-debtholder conflict

As a further test of cross-sectional heterogeneity in changes in loan spreads, we focus on firms' shareholder-debtholder conflict. Firms' policies that increase shareholder-debtholder conflict, such as excessive payout, overinvestment, and high-risk policies, can accentuate debtholders' concern about potential wealth transfers from themselves to shareholders. For example, excessive dividends reduce the assets available to debtholders, increasing debtholders' exposure to potential wealth transfers. Consistent with this view, Acharya, Le, and Shin (2013) argue that a firm's risk-shifting incentives motivate the payment of excessive dividends. Similarly, overinvestment in risky projects increases firm risk, benefiting shareholders at the expense of debtholders. Thus, ex ante, lenders are expected to adjust loan prices more unfavorably for firms that adopt debtholder-unfriendly policies.

To test this prediction, we perform difference-in-difference-in-differences (DDD) tests. We first measure dividend payout as the ratio of total dividends to the market value of equity, investment inefficiency as the residuals estimated from the regressions of total investments scaled by lagged total assets on lagged sales growth, leverage, cash to total assets, firm age, the natural logarithm of total assets, and stock return performance for each industry-year based on the Fama and French 48-industry classification (Richardson, 2006; Biddle, Hilary, and Verdi, 2009), and cash flow volatility as the standard deviation of

quarterly cash flows from operations over the three fiscal years prior to the loan initiation year scaled by total assets. In each year, we then define *High conflict (indicator)* separately for treatment and control firms; this indicator takes the value of one if the firm's dividend payout (cash flow volatility) is above the sample median dividend payout (cash flow volatility) and zero otherwise. *High conflict (indicator)* also takes the value of one if the level of the firm's investment inefficiency is above the 75th percentile in the sample (i.e., overinvestment) and zero otherwise.[15] We then interact *High conflict (indicator)* with *Treatment* and *Post*. The coefficient on this triple interaction term captures the difference between the differential lender response to treatment firms with higher shareholder-debtholder conflict relative to control firms with higher shareholder-debtholder conflict and the differential lender response to treatment firms with lower shareholder-debtholder conflict relative to lenders of control firms with lower shareholder-debtholder conflict.

The results are reported in Table 6. In columns (1) and (2), columns (3) and (4), and columns (5) and (6), the measures of shareholder-debtholder conflict are dividend payout, investment inefficiency, and cash flow volatility, respectively. We find positive and significant coefficients on the triple interaction term in all six regressions. It is noteworthy that the magnitude of the average increase in loan spreads after the scandal for treatment firms with greater shareholder-debtholder conflict is economically large and significant. For example, the coefficient of 0.545 for the triple interaction term in column (4), in which we use investment inefficiency as a measure of shareholder-debtholder conflict and control for firm and industry-by-year fixed effects, suggests that loan spreads for firms with higher shareholder-debtholder conflict increase by 72.46% ($e^{0.545}$-1) in the post-scandal period. Overall, these results strongly support our prediction that lenders demand a higher return on loans when affected firms adopt debtholder-unfriendly policies.

[15] The sample medians of investment inefficiency for treatment and control firms in each year are mostly negative in our sample. Thus, we follow Biddle et al. (2009) and classify firms that are in the top quartile of investment inefficiency as those that overinvest.

4.2.2 Information asymmetry

Next, we examine how the effects of shareholder scandals on loan spreads vary with firms' information environment. Lenders must incur costs to obtain firm-specific information and monitor firm management. Such costs tend to increase as information asymmetry vis-à-vis firms becomes greater because high information asymmetry limits lenders to obtaining soft information about firms and thus makes it difficult to assess the potential effects of shareholder financial difficulties on risk shifting. As such, we expect lenders to charge higher loan spreads when firms have poorer information environments. We use three variables as measures of firms' information asymmetry: analyst coverage (the number of unique analysts following the firm), forecast dispersion (the standard deviation of valid forecasts in the month of the fiscal-year-end, scaled by the absolute value of the mean forecasts), and firm size (the natural logarithm of total assets). In each year, we define *High information asymmetry (indicator)* separately for treatment and control firms; this indicator takes the value of one if the firm's analyst coverage (size) is below the sample median analyst coverage (size) and zero otherwise. *High information asymmetry (indicator)* also takes the value of one if analysts' forecast dispersion about the firm's earnings is above the sample median forecast dispersion and zero otherwise. As in Table 6, our key independent variable of interest is a triple interaction term among *High information asymmetry*, *Treatment*, and *Post*.

The results are presented in Table 7. In columns (1) and (2), in which we use analyst coverage as a measure of a firm's information opacity, we find that the coefficient on the triple interaction term is positive and significant. When we use forecast dispersion and firm size as measures of information opacity and control for year fixed effects in the regressions, the coefficient on the triple interaction term is positive but insignificant (columns (3) and (5)). However, it becomes positive and significant when we replace year fixed effects with industry-by-year fixed effects in columns (4) and (6). These results suggest that lenders' perception of the increase in firms' risk-shifting behavior arising from shareholder financial difficulties is more adverse for firms with higher information asymmetry.

5. Effects of Shareholder Scandals on Loan Covenants

Covenants are an important nonpricing term of loan contracts in the private debt market. Lenders tend to use covenants to restrict firms' risk-taking activities, and thus, they are likely to impose more stringent covenants on firms when they are concerned about an increase in potential risk shifting arising from the financial difficulties of firms' large shareholders. To test this prediction, we estimate difference-in-differences regressions in which the dependent variable is covenant intensity. We measure covenant intensity by the number of total covenants (the sum of the number of general covenants, the number of financial covenants, and the value of an indicator for whether the loan requires collateral). We also measure covenant intensity by the number of general covenants and the number of financial covenants.

Table 8 report the results. We find that the coefficients on *Treatment* × *Post* are positive and significant in columns (1) and (2), in which the dependent variables are the number of total covenants and the number of general covenants, respectively. However, the coefficient is insignificant in column (3), in which the dependent variable is the number of financial covenants. Given that covenants are used to limit borrowers' opportunistic behaviors by intervening in their financial and investment decisions (e.g., Graham, Li, and Qiu, 2008; Chava, Kumar, and Warga 2010), these results indicate that lenders view tainted institutions' financial shock due to the scandal as an increase in their portfolio firms' potential future risk-shifting activities, which incentivizes lenders to tighten general covenants.[16]

6. Correlations between Bond and Stock Returns around the Scandal Announcement Date

If the scandal indeed increases shareholders' risk-shifting incentives and thus amplifies firms' risk-taking activities in the post-scandal period, the market's ex ante valuation of equity value around the scandal announcement date should be positive or at least nonnegative, while the market's ex ante valuation of bond value should be negative. Risk shifting also suggests that scandal announcement returns for bonds are

[16] We require the covenants to be non-missing in the DealScan database when measuring covenant intensity. This requirement reduces the sample size for the regressions in which we use the number of total (general) covenants as the dependent variables to 128 (129). This small sample size prevents us from examining the cross-sectional heterogeneity in the effects of scandals on covenants across different characteristics of tainted institutions and firms and performing the analysis for the joint determination of loan prices and covenant intensity.

negatively correlated with those for stocks due to wealth transfer from debtholders to shareholders.

To test these predictions, we obtain bond and stock price data from the Trade Reporting and Compliance Engine (TRACE) and CRSP, respectively, and compute CARs around the scandal announcement date for bonds and stocks of affected firms held by tainted block institutions in the quarter immediately before the scandal announcement date. We follow Bessembinder et al. (2009) to clean bond trading data and compute bond CARs. Specifically, we delete trades under $100,000, canceled trades, corrected trades, and commission trades. Daily bond prices are estimated by the trade-size-weighted average of intraday individual transaction prices. Bond CARs are measured as the difference between affected firms' bond holding period returns from day t_1 before the scandal announcement date to day t_2 after the scandal announcement date and the value-weighted portfolio holding period returns of bonds during the corresponding holding period that have the same Moody's credit ratings as those of affected firms' bonds. When an affected firm has multiple bonds outstanding, we use the portfolio CAR as the bond CAR, which is computed as the value-weighted average of multiple bonds' CARs. We use as the weight the ratio of the market value of each bond in the month prior to the scandal announcement month to the total market value of all bonds outstanding for a firm at the same time. Stock CARs are calculated using the same approach as that used in Table 3.

Panel A of Table 9 reports bond and stock CARs around the scandal announcement date. The sample consists of 97 affected firms with bonds outstanding. We find that the mean and median bond CARs (0, 1) of affected firms are -0.6% and -0.2%, respectively, both of which are significant at the 5% level. The mean and median bond CARs (-1, 1) are also negative but insignificant. In contrast, the corresponding mean and median stock CARs are small and insignificant.

In Panel B, we present the Pearson correlation coefficients between bond and stock CARs. Consistent with our expectation, we find that bond CARs (0, 1) are negatively and significantly correlated with stock CARs (-1, 1) and stock CARs (-1, 0). The magnitudes of the correlation coefficients are also large at 0.185 and 0.389, respectively. We also find a negative and significant correlation between bond CARs (-1, 1) and

stock CARs (-1, 0) with a correlation coefficient of -0.241.[17]

Overall, these results suggest that scandals that impose significant financial difficulties on shareholders do indeed exacerbate conflicts of interest between shareholders and debtholders.

To further shed light on debtholders' concerns about borrowers' involvement in potential risk shifting induced by tainted institutions' financial difficulties, we investigate whether bond market reactions to scandal announcements vary across tainted institution characteristics. As in Tables 3 and 5, we divide affected firms whose shares are held by tainted block institutions prior to the scandal into two subgroups according to the severity of the scandal measured by tainted institutions' CARs (-1, 1), tainted institutions' past performance, and tainted institutions' abilities to influence portfolio firms.

The results are presented in Table 10. Consistent with our prediction, we find that bond CARs (0, 1) are negative and significant for affected firms with lower institution CAR, lower institution Tobin's *q*, and poorer institution operating performance, while they are positive and significant for affected firms with higher institution CAR, higher institution Tobin's *q*, and better institution operating performance. The differences in the mean and median bond CARs (0, 1) between the two subgroups are significant at the 5% level or better, suggesting that bondholders' concerns about risk shifting are evident only when the misconduct is more severe or when tainted institutions perform poorly before the scandal. We also find that the magnitudes of the mean and median bond CARs (0, 1) are more negative for firms in which tainted institutions hold block ownership for a longer period and hold larger block ownership before the scandal, although they are not significantly different from the magnitudes of the mean and median bond CARs (0, 1) for other firms.

Overall, these results provide strong evidence of wealth transfer from bondholders to shareholders when firms' large shareholders suffer from financial difficulties, which reflects an increase in potential shareholder risk-shifting incentives to overcome such difficulties.

[17] In untabulated tests, we find that the Pearson correlations of bond CARs (0, 1) and bond CARs (-1, 1) with stock CARs (0, 1) and stock CARs (-1, 1) for non-affected firms are all significantly positive, which further supports our argument that shareholder financial difficulties affect firms' risk-shifting behavior.

7. Firms' Risk-Taking Behaviors in the Post-Scandal Period

If the deterioration of large shareholders' financial conditions due to their involvement in the scandal incentivizes them to exploit debtholders by influencing firms to take greater risk, we expect firms with tainted block institutions to engage in more risk shifting after the scandal than those without such institutions. Although lenders, anticipating such an increase in risk shifting, adjust loan contract terms to ensure greater protection against asset substitution, it may be difficult for lenders, ex ante, to precisely assess the severity of asset substitution and to monitor shareholders' ex post actions. It is also possible that contracting is incomplete (Jensen and Meckling, 1976; Grossman and Hart, 1986; Hart and Moore, 1990; Hart, 1995), and thus, it cannot completely control agents' moral hazard problems. To address this issue, in this section, we examine how firms' risk-taking behaviors change after the scandal.

We start with a sample of all affected and non-affected firms from 2000 to 2007 and obtain a propensity score matched sample using the same approach as that in Table 2 except that we do not require issuance of loans by treatment and control firms in the periods three years before and three years after the scandal year. Our final sample consists of 947 firm-year observations of 160 treatment firms and 937 firm-year observations of 160 control firms. As shown in Panel A of Table 11, we find that none of the firm characteristics between treatment and control firms are significant, except that the difference in the median stock return volatility is significant at the 10% level.

We use several measures of firms' risk-taking activities, including dividend payout (the ratio of total dividends to the market value of equity), total investments (the sum of capital expenditures, R&D expenses, and acquisition expenditures minus the sales of PP&E, scaled by total assets), and stock return volatility (the standard deviation of daily stock returns in a given year). The results using these measures of firms' risk-taking behavior as the dependent variables are reported in Panel B of Table 11. In columns (1) and (2), in which the dependent variable is dividend payout, we find that the coefficient on *Treatment* × *Post* is positive and insignificant. In columns (3)-(4) and columns (5)-(6), in which the dependent variables are total investments and stock return volatility, respectively, we find positive and significant coefficients on

the interaction term between *Treatment* and *Post* in all four regressions, suggesting that the investment and riskiness of treatment firms increase significantly after the scandal. The magnitudes of these increases are also economically large: the ratio of total investments to total assets (stock return volatility) increases more than 2.5% (0.3%) in column (3) (column (5)), which accounts for 22.9% (9.7%) of the average ratio of total investments to total assets (stock return volatility) for the full sample.

These results suggest that tainted institutions, which suffer from financial difficulties due to the scandal, have strong incentives to influence firms to engage in risk shifting after the scandal. The results also suggest that having contracts that can completely eliminate shareholder-debtholder conflict is difficult and costly.

8. Summary and Conclusion

In this paper, we investigate how shareholder financial difficulties affect firms' risk-shifting behavior. We argue that the financial difficulties of tainted institutions incentivize them to influence their portfolio firms to engage in aggressive risk shifting and that lenders, anticipating this increase in risk-shifting behaviors, charge higher loan spreads to these firms. Consistent with this argument, using the 2003 mutual fund scandal that leads to massive fund outflows of tainted institutions and declines in fund value as an exogenous shock to shareholders' financial conditions, we find that firms whose shares are held by tainted block institutions experience a large increase in loan spreads in the post-scandal period, particularly when the misconduct is more severe. We further find that the adverse effects of shareholder scandals on loan prices are more evident when tainted institutions perform poorly and when tainted institutions have stronger abilities to influence portfolio firms. The results are also more pronounced when firms have greater shareholder-debtholder conflict and when firms have higher information asymmetry. Thus, firms' adoption of debtholder-friendly policies and transparent information environments helps alleviate lenders' concerns about the adverse effect of shareholder financial difficulties on firms' risk-shifting behaviors. Moreover, we find that lenders impose a larger number of general covenants after the scandal to restrain borrowers' risk-taking activities.

In additional analyses, we find that bondholders (shareholders) experience negative (positive) and significant (insignificant) CARs (0, 1) around the scandal announcement date and that bond CARs are negatively and significantly correlated with stock CARs. These results suggest that the scandal, which imposes significant financial difficulties on shareholders, indeed increases shareholders' risk-shifting incentives, resulting in wealth transfers from debtholders to shareholders. The bond market reactions to the scandal are particularly negative for affected firms with lower institution CAR, lower institution Tobin' *q*, and poorer institution operating performance.

Turning to firms' post-scandal risk-taking behaviors, we find that the investment and riskiness of treatment firms increase significantly after the scandal, further supporting the view that the financial difficulties of large shareholders have a significant effect on firms' risk shifting.

Overall, our findings provide strong evidence of the importance of shareholder financial difficulties for firms' risk shifting and add to the literature on asset substitution, which lacks evidence of the importance of shareholder characteristics for risk shifting. Given that institutional shareholders, particularly mutual funds as investment advisors, have significant ownership in many publicly listed firms in the U.S., our findings also suggest that misconduct by investment advisors has an impact on various aspects of the economy beyond the financial advisory industry and fund management firms; it also affects the risk-taking behaviors of investment advisors' portfolio firms and debtholders' loan contract designs.

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Figure 1
Cumulative Monthly Net Flows (%) of Implicated and Non-implicated Fund Families

This figure presents the size-weighted mean cumulative monthly net flow (%) of implicated fund families that are charged by regulatory agencies in 2003 or 2004 for their illegal late trading and market timing practices and non-implicated fund families from the scandal announcement month to the subsequent three years, where each fund family's total net assets is used as the weight. The monthly percentage net flow of fund *i* in month *t* is calculated as

$$Net\ flow_{it} = \frac{TNA_{it} - (1 + R_{it})TNA_{i,t-1} - MGN_{it}}{TNA_{i,t-1}}$$

where TNA_{it} is total net assets for fund *i* in month *t*, R_{it} is the return for fund *i* in month *t*, and MGN_{it} is the increase in total net assets due to fund mergers in month *t*.

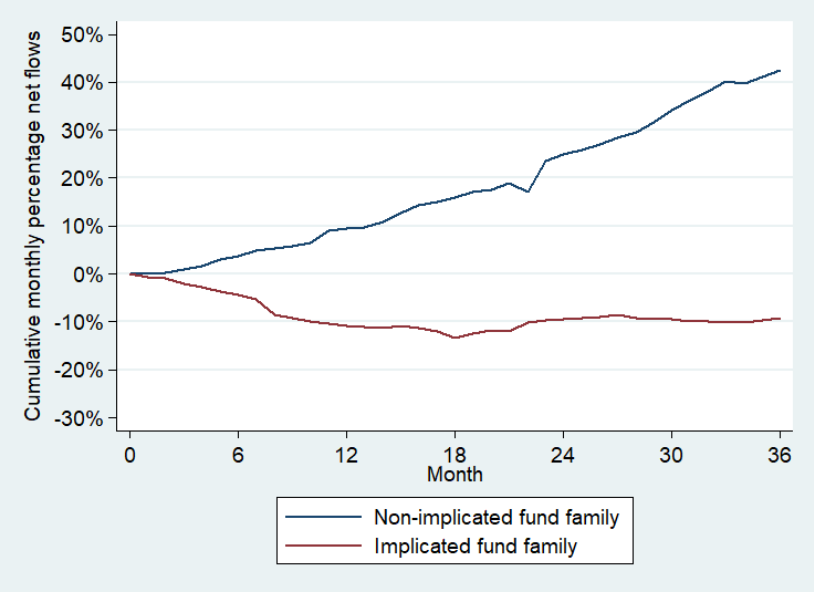

Table 1
List of Implicated Mutual Fund Families during 2003 and 2004 Mutual Fund Scandals

This table presents a list of 26 mutual fund families that are charged by regulatory agencies in 2003 or 2004 for their illegal late trading and market timing practices. We collect the list of these fund families from various sources, including *Factiva* and administrative proceedings by the Securities and Exchange Commission (SEC). We search for information on implicated mutual fund families' investment advisors and ultimate parent firms, if any, from their websites and 10-K filings. Information on assets under management (AUM) in the last column is obtained from the Form ADV filed by implicated investment advisors immediately before the initial public scandal reporting date.

Fund family	Investment advisor	Ultimate parent firm	Initial public reporting date	AUM ($ billions)
Alliance Bernstein	Alliance Capital Mgmt.	AXA Financial	2003-09-30	386.58
Columbia	Columbia Mgmt.	FleetBoston Financial	2004-01-15	126.20
Evergreen	Evergreen Inv. Mgmt.	Wachovia	2004-08-04	169.76
Excelsior	US Trust	Charles Schwab	2003-11-14	12.40
Federated	Federated Investors	Federated Investors	2003-10-22	150.02
Franklin Templeton	Franklin Advisers	Franklin Resources	2003-09-03	105.90
Fred Alger	Fred Alger Mgmt.	(Private)	2003-10-03	8.43
Fremont	Fremont Inv. Advisors	(Private)	2003-11-24	5.63
Gabelli	Gabelli Funds	Gabelli Asset Mgmt.	2003-09-03	8.68
Heartland	Heartland Advisors	(Private)	2003-12-11	1.38
ING	ING Investments	ING Groep NV	2004-03-11	31.99
Invesco/AIM	Invesco Funds Group	Amvescap PLC	2003-12-02	0.85
Janus	Janus Capital Mgmt.	Janus Capital Group	2003-09-03	128.80
Loomis Sayles	Loomis Sayles & Co.	CDC Asset Mgmt.	2003-11-13	
MFS	MFS Inv. Mgmt.	Sun Life Financial	2003-12-09	101.72
Nations	Banc of America Capital Mgmt.	Bank of America	2003-09-03	192.91
One Group	Banc One Inv. Advisors	Bank One	2003-09-03	161.95
PBHG	Pilgrim Baxter & Associates	Old Mutual PLC	2003-11-13	9.28
PIMCO	PIMCO Advisors	Allianz Group	2004-02-13	16.64
Prudential	Prudential Securities	Prudential Financial	2003-11-04	1.63
Putnam	Putnam Inv. Mgmt.	Marsh & McLennan	2003-09-19	169.59
RS Investments	RS Inv. Mgmt.	(Private)	2004-03-03	0.08
Scudder	Deutsche Inv. Mgmt. Americas	Deutsche Bank AG	2004-01-23	189.04
Seligman	J&W Seligman & Co.	(Private)	2004-01-07	18.85
Strong	Strong Capital Mgmt.	(Private)	2003-09-03	8.47
Waddell & Reed	Waddell & Reed Inv. Mgmt.	Waddell & Reed Financial	2003-11-12	38.52

Table 2
Summary Statistics

Panel A of this table presents summary statistics for a sample of 770 firm-year observations of *affected firms* whose shares are held by tainted block institutions in the year immediately before the scandal year (i.e., 2003 or 2004, in which regulatory agencies charge mutual fund families for their illegal late trading and market timing practices) and 4,210 firm-year observations of *non-affected firms* whose shares are not held by tainted block institutions in the same year. For loan characteristics, the sample consists of 426 loans issued by *affected firms* and 2,099 loans issued by *non-affected firms* in the year immediately prior to the scandal year. Panel B presents descriptive statistics for a propensity score matched sample of 65 treatment *affected firms* and 65 control *non-affected firms*. Each treatment firm is matched to a control firm using a propensity score matching approach without replacement and a caliper of 0.05. The propensity score is calculated using the logit regression of *Treatment* (an indicator that takes the value of one if a firm is an *affected firm* and zero otherwise) on the natural logarithm of total assets, Tobin's *q*, leverage, ROA, tangibility, stock return volatility, excess stock return, and institutional block ownership. We require the treatment and control firms to be in the same industry (the same Fama-French 48 industry classification code) and in the same fiscal year, to have the same credit rating classes (i.e., no rating, non-investment rating (below BBB-), and investment rating (above BBB-)), and to have at least one loan issued both three years before and three years after the scandal announcement date. We also require that tainted block institutions continuously hold equity ownership in treatment firms for three years after the scandal year either as blockholders or as top ten institutional shareholders with at least 1% ownership. Firm characteristics are measured at the beginning of the fiscal year, and all continuous variables are winsorized at the 1st and 99th percentiles. The appendix provides a detailed description of the variables. ***, **, and * denote that the *p*-values of *t*-tests (Wilcoxon *z*-tests) for mean (median) differences in firm and loan characteristics between *affected* and *non-affected firms* are significant at the 1%, 5%, and 10% levels, respectively.

Panel A: Firm and loan characteristics: Full sample

	Affected firms (N=770): A		Non-affected firms (N=4,210): B		Test of difference (A-B): *p*-value	
Variable	Mean	Median	Mean	Median	*t*-test	Wilcoxon *z*-test
Firm characteristics:						
Log (total assets)	6.348	6.298	5.816	5.719	0.000***	0.000***
Tobin's *q*	1.748	1.383	1.884	1.458	0.007***	0.007***
Book leverage	0.229	0.202	0.196	0.154	0.000***	0.000***
ROA	-0.021	0.027	-0.049	0.022	0.001***	0.006***
Tangibility	0.260	0.205	0.252	0.180	0.370	0.053*
Stock return volatility	0.035	0.032	0.038	0.033	0.003***	0.056*
Excess stock return	0.152	0.047	0.250	0.077	0.002***	0.111
Institutional block ownership	0.255	0.249	0.193	0.165	0.000***	0.000***
Loan characteristics:						
Loan amount ($ millions)	229.873	130.000	214.559	100.000	0.389	0.000***
Loan maturity (months)	39.610	36.000	38.469	36.000	0.317	0.722
Loan spread (bp)	200.563	175.000	226.186	225.000	0.001***	0.000***
Number of total covenants	11.962	12.000	12.322	12.500	0.156	0.094*
Number of general covenants	7.250	8.000	7.278	8.000	0.859	0.734
Number of financial covenants	3.097	3.000	3.050	3.000	0.557	0.581
Secured (indicator)	0.773	1.000	0.807	1.000	0.190	0.190

Panel B: Firm characteristics: Propensity score matched sample

	Treatment firms (N=65): A		Control firms (N=65): B		Test of difference (A-B): *p*-value	
Variable	Mean	Median	Mean	Median	*t*-test	Wilcoxon *z*-test
Log (total assets)	6.639	6.701	6.839	6.677	0.402	0.516
Tobin's *q*	1.410	1.242	1.486	1.202	0.602	0.694
Book leverage	0.286	0.262	0.313	0.246	0.460	0.789
ROA	-0.009	0.036	0.012	0.032	0.390	0.994
Tangibility	0.265	0.235	0.273	0.233	0.810	0.818
Stock return volatility	0.033	0.030	0.035	0.029	0.710	0.905
Excess stock return	0.097	0.067	0.122	0.124	0.770	0.725
Institutional block ownership	0.222	0.208	0.213	0.219	0.608	0.594

Table 3
Effects of Shareholder Scandals on Loan Spreads

This table presents estimates of difference-in-differences regressions in which the dependent variable is the natural logarithm of all-in-spread-drawn, a rate that a firm pays in basis points over LIBOR or the LIBOR equivalent. The sample consists of 707 firm-loan observations (326 and 381 loans issued by treatment and control firms, respectively) from 2000 to 2007. Treatment (control) firms are firms whose shares are held (not held) by tainted block institutions in the year immediately before the scandal year (i.e., 2003 or 2004, in which regulatory agencies charge mutual fund families for their illegal late trading and market timing practices). Each treatment firm is matched to a control firm using a propensity score matching approach without replacement and a caliper of 0.05. The propensity score is calculated using the logit regression of *Treatment* (an indicator that takes the value of one if a firm's shares are held by the tainted block institution in the year immediately before the scandal year and zero otherwise) on the natural logarithm of total assets, Tobin's *q*, book leverage, ROA, tangibility, stock return volatility, excess stock return, and institutional block ownership. We require the treatment and control firms to be in the same industry (the same Fama-French 48 industry classification code) and in the same fiscal year, to have the same credit rating classes (i.e., no rating, non-investment rating (below BBB-), and investment rating (above BBB-)), and to have at least one loan issued both three years before and three years after the scandal announcement date. We also require that tainted block institutions continuously hold equity ownership in treatment firms for three years after the scandal year either as blockholders or as top ten institutional shareholders with at least 1% ownership. *Post* is an indicator that takes the value of one for a firm-year in the post-scandal period (year *t+1*, year *t+2*, and year *t+3*) and zero for a firm-year in the pre-scandal period (year *t–1*, year *t–2*, year *t–3*), where year *t* is the scandal year. *Treatment with low* (*high*) *institution CAR* is an indicator that takes the value of one if the tainted institution's cumulative abnormal return from one day before to one day after the scandal announcement date is below (above) the sample median CAR and zero otherwise. We estimate the CAR using the market model and use the CRSP value-weighted return as a proxy for the market return. All firm characteristics are measured as of the fiscal year-end immediately before the loan active date. Macroeconomic variables are measured as of the month immediately before the loan active date. All continuous variables are winsorized at the 1st and 99th percentiles. The appendix provides a detailed description of the variables. *P*-values reported in parentheses are based on standard errors adjusted for heteroskedasticity and clustered at the firm level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	Dependent variable = Log (loan spread)			
Independent variable	(1)	(2)	(3)	(4)
Treatment (indicator) × Post (indicator)	0.165**		0.173**	
	(0.025)		(0.035)	
Treatment with low institution CAR (indicator) × Post (indicator): a		0.303***		0.261**
		(0.000)		(0.017)
Treatment with high institution CAR (indicator) × Post (indicator): b		0.068		0.095
		(0.462)		(0.353)
Firm characteristics				
Log (total assets)	-0.073	-0.092	-0.016	-0.039
	(0.369)	(0.244)	(0.848)	(0.657)
Tobin's *q*	-0.105*	-0.088*	-0.117**	-0.136**
	(0.050)	(0.094)	(0.039)	(0.033)
Book leverage	0.247	0.236	0.438*	0.422*
	(0.159)	(0.166)	(0.076)	(0.097)
ROA	-0.608*	-0.558*	-0.506	-0.375
	(0.063)	(0.097)	(0.336)	(0.487)
Tangibility	0.072	-0.224	0.240	0.144
	(0.889)	(0.664)	(0.696)	(0.811)
Stock return volatility	-1.047	-0.393	-0.745	1.575
	(0.710)	(0.893)	(0.875)	(0.747)
Excess stock return	-0.049	-0.050	-0.074	-0.081*
	(0.148)	(0.147)	(0.129)	(0.100)
Institutional block ownership	0.135	0.141	0.277	0.273
	(0.625)	(0.597)	(0.309)	(0.340)
Non-investment grade rating (indicator)	0.129	0.146	0.126	0.127
	(0.211)	(0.145)	(0.284)	(0.287)
Investment grade rating (indicator)	-0.235*	-0.206	-0.148	-0.097
	(0.097)	(0.152)	(0.443)	(0.608)
Loan characteristics				
Log (loan maturity)	0.067	0.061	0.051	0.051
	(0.215)	(0.286)	(0.436)	(0.470)
Log (loan amount)	-0.086***	-0.088***	-0.088***	-0.092***
	(0.000)	(0.000)	(0.000)	(0.000)

Performance pricing (indicator)	-0.026	-0.036	-0.059	-0.046
	(0.592)	(0.453)	(0.274)	(0.404)
Macroeconomic factors				
Credit spread	-0.409**	-0.351*	-0.513**	-0.390
	(0.026)	(0.078)	(0.029)	(0.124)
Term spread	0.118**	0.095**	0.104*	0.062
	(0.012)	(0.033)	(0.090)	(0.326)
P-value for the test of a = b		0.030**		0.208
Loan purpose fixed effects	Yes	Yes	Yes	Yes
Loan type fixed effects	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	No	No
Industry-by-year fixed effects	No	No	Yes	Yes
Adjusted R^2	0.824	0.826	0.841	0.839
Number of observations	707	674	666	639

Table 4
Pre-trend Analysis of Loan Spreads around the Mutual Fund Scandal

This table presents estimates of regressions that examine the pre-trend effects of the mutual fund scandal on loan spreads. The sample consists of 707 firm-loan observations (326 and 381 loans issued by treatment and control firms, respectively) from 2000 to 2007. Treatment (control) firms are firms whose shares are held (not held) by tainted block institutions in the year immediately before the scandal year (i.e., year 2003 or 2004, in which regulatory agencies charge mutual fund families for their illegal late trading and market timing practices). Each treatment firm is matched to a control firm using a propensity-score matching approach without replacement and a caliper of 0.05. The propensity score is calculated using the logit regression of *Treatment* (an indicator that takes the value of one if a firm's shares are held by the tainted block institution in the year immediately before the scandal year and zero otherwise) on the natural logarithm of total assets, Tobin's *q*, book leverage, ROA, tangibility, stock return volatility, excess stock return, and institutional block ownership. We require the treatment and control firms to be in the same industry (the same Fama-French 48 industry classification code) and in the same fiscal year, to have the same credit rating classes (i.e., no rating, non-investment rating (below BBB-), and investment rating (above BBB-)), and to have at least one loan issued both three years before and three years after the scandal announcement date. We also require that tainted block institutions continuously hold equity ownership in treatment firms for three years after the scandal year either as blockholders or as top ten institutional shareholders with at least 1% ownership. We estimate the following regression:

$$Log\ (loan\ spread)_{i,t}=\alpha_i+\alpha_t+\alpha_j+\alpha_k+\beta_{-2}Before^{-2}_{i,t}+\beta_{-1}Before^{-1}_{i,t}+\beta_{+1}After^{+1}_{i,t}+\beta_{+2}After^{+2}_{i,t}+\beta_{+3}After^{+3}_{i,t}+\gamma X_{i,t}+\varepsilon_{i,t},$$

where $Before^{-n}$ is an indicator that takes the value of one for treatment firms at year $t–n$ and zero otherwise, where year t is the scandal year; $After^{+n}$ is an indicator that takes the value of one for treatment firms at year $t+n$ and zero otherwise; $X_{i,t}$ are the control variables used in Table 3; and α_i, α_t, α_j, and α_k are fixed effects for firms, years (industries and years), loan types, and loan purposes, respectively. *P*-values reported in parentheses are based on standard errors adjusted for heteroskedasticity and clustered at the firm level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	Dependent variable = Log (loan spread)	
Independent variable	(1)	(2)
Before^{-2} (indicator)	0.169	0.153
	(0.124)	(0.221)
Before^{-1} (indicator)	0.162	0.007
	(0.188)	(0.965)
After^{+1} (indicator)	0.263**	0.214*
	(0.025)	(0.093)
After^{+2} (indicator)	0.267**	0.197*
	(0.027)	(0.091)
After^{+3} (indicator)	0.334**	0.347**
	(0.038)	(0.017)
Control variables (same as in Table 3)	Yes	Yes
Loan purpose fixed effects	Yes	Yes
Loan type fixed effects	Yes	Yes
Firm fixed effects	Yes	Yes
Year fixed effects	Yes	No
Industry-by-year fixed effects	No	Yes
Adjusted R^2	0.825	0.841
Number of observations	707	666

Table 5
Effects of Shareholder Scandals on Loan Spreads by Tainted Institutions' Characteristics

This table presents estimates of difference-in-differences regressions by tainted institutions' characteristics in which the dependent variable is the natural logarithm of all-in-spread-drawn, a rate that a firm pays in basis points over LIBOR or the LIBOR equivalent. The sample consists of 707 firm-loan observations (326 and 381 loans issued by treatment and control firms, respectively) from 2000 to 2007. Treatment (control) firms are firms whose shares are held (not held) by tainted block institutions in the year immediately before the scandal year (i.e., 2003 or 2004, in which regulatory agencies charge mutual fund families for their illegal late trading and market timing practices). Each treatment firm is matched to a control firm using a propensity score matching approach without replacement and a caliper of 0.05. The propensity score is calculated using the logit regression of *Treatment* (an indicator that takes the value of one if a firm's shares are held by the tainted block institution in the year immediately before the scandal year and zero otherwise) on the natural logarithm of total assets, Tobin's *q*, book leverage, ROA, tangibility, stock return volatility, excess stock return, and institutional block ownership. We require the treatment and control firms to be in the same industry (the same Fama-French 48 industry classification code) and in the same fiscal year, to have the same credit rating classes (i.e., no rating, non-investment rating (below BBB-), and investment rating (above BBB-)), and to have at least one loan issued both three years before and three years after the scandal announcement date. We also require that tainted block institutions continuously hold equity ownership in treatment firms for three years after the scandal year either as blockholders or as top ten institutional shareholders with at least 1% ownership. *Treatment with poor (good) institution performance* is an indicator that takes the value of one if the tainted institution's Tobin's *q* or operating performance before the scandal year is below (above) the sample median Tobin's *q* or operating performance and zero otherwise. The tainted institution's Tobin's *q* is measured by the ratio of the book value of total assets minus the book value of equity plus the market value of equity to the book value of total assets. The tainted institution's operating performance is measured by the ratio of operating income before depreciation to total assets. *Treatment with high (low) institution influence* is an indicator that takes the value of one if the tainted institution's blockholding period or block ownership in treatment firms before the scandal year is above (below) the sample median blockholding period or block ownership and zero otherwise. The blockholding period is measured as the number of quarters in the pre-scandal period during which the tainted block institution holds block ownership in treatment firms. Block ownership is measured as the percentage of ownership held by the tainted block institution at the beginning of the scandal year. *Post* is an indicator that takes the value of one for a firm-year in the post-scandal period (year *t+1*, year *t+2*, and year *t+3*) and zero for a firm-year in the pre-scandal period (year *t–1*, year *t–2*, year *t–3*), where year *t* is the scandal year. All firm characteristics are measured as of the fiscal year-end immediately before the loan active date. Macroeconomic variables are measured as of the month immediately before the loan active date. All continuous variables are winsorized at the 1st and 99th percentiles. The appendix provides a detailed description of the variables. *P*-values reported in parentheses are based on standard errors adjusted for heteroskedasticity and clustered at the firm level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Difference-in-differences regressions by tainted block institutions' prior performance

	Dependent variable = Log (loan spread)			
	Tobin's *q*		Operating performance	
Independent variable	(1)	(2)	(3)	(4)
Treatment with poor institution performance (indicator) × Post (indicator): a	0.296***	0.312***	0.311***	0.331***
	(0.001)	(0.001)	(0.001)	(0.001)
Treatment with good institution performance (indicator) × Post (indicator): b	0.007	-0.051	0.004	-0.050
	(0.944)	(0.626)	(0.967)	(0.639)
P-value for the test of a = b	0.010**	0.003***	0.008***	0.003***
Control variables (same as in Table 3)	Yes	Yes	Yes	Yes
Loan purpose fixed effects	Yes	Yes	Yes	Yes
Loan type fixed effects	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	No	Yes	No
Industry-by-year fixed effects	No	Yes	No	Yes
Adjusted R^2	0.828	0.842	0.831	0.839
Number of observations	674	639	637	602

Panel B: Difference-in-differences regressions by tainted block institutions' influence on firms

	Dependent variable = Log (loan spread)			
	Blockholding period		Block ownership	
	(1)	(2)	(3)	(4)
Treatment with high institution influence (indicator) × Post (indicator): a	0.297***	0.349***	0.178*	0.202*
	(0.000)	(0.000)	(0.060)	(0.073)
Treatment with low institution influence (indicator) × Post (indicator): b	0.053	0.012	0.154*	0.148

	(0.562)	(0.907)	(0.088)	(0.126)
P-value for the test of a = b	0.013**	0.009***	0.824	0.678
Control variables (same as in Table 3)	Yes	Yes	Yes	Yes
Loan purpose fixed effects	Yes	Yes	Yes	Yes
Loan type fixed effects	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	No	Yes	No
Industry-by-year fixed effects	No	Yes	No	Yes
Adjusted R^2	0.827	0.844	0.824	0.841
Number of observations	707	666	707	666

Table 6
Effects of Shareholder Scandals on Loan Spreads by Firms' Shareholder-Debtholder Conflict

This table presents estimates of difference-in-differences regressions by firms' shareholder-debtholder conflict in which the dependent variable is the natural logarithm of all-in-spread-drawn, a rate that a firm pays in basis points over LIBOR or the LIBOR equivalent. The sample consists of 707 firm-loan observations (326 and 381 loans issued by treatment and control firms, respectively) from 2000 to 2007. Treatment (control) firms are firms whose shares are held (not held) by tainted block institutions in the year immediately before the scandal year (i.e., 2003 or 2004, in which regulatory agencies charge mutual fund families their illegal late trading and market timing practices). Each treatment firm is matched to a control firm using a propensity score matching approach without replacement and a caliper of 0.05. The propensity score is calculated using the logit regression of *Treatment* (an indicator that takes the value of one if a firm's shares are held by the tainted block institution in the year immediately before the scandal year and zero otherwise) on the natural logarithm of total assets, Tobin's *q*, book leverage, ROA, tangibility, stock return volatility, excess stock return, and institutional block ownership. We require the treatment and control firms to be in the same industry (the same Fama-French 48 industry classification code) and in the same fiscal year, to have the same credit rating classes (i.e., no rating, non-investment rating (below BBB-), and investment rating (above BBB-)), and to have at least one loan issued both three years before and three years after the scandal announcement date. We also require that tainted block institutions continuously hold equity ownership in treatment firms for three years after the scandal year either as blockholders or as top ten institutional shareholders with at least 1% ownership. In each year, we define *High conflict (indicator)* separately for treatment and control firms; this indicator takes the value of one if the firm's dividend payout (cash flow volatility) is above the sample median dividend payout (cash flow volatility) and zero otherwise in columns (1) and (2) (columns (5) and (6)). *High conflict (indicator)* takes the value of one if the level of the firm's investment inefficiency is above the 75th percentile in the sample (i.e., overinvestment) and zero otherwise in columns (3) and (4). Dividend payout is measured as the ratio of total dividends to the market value of equity. Cash flow volatility is measured as the standard deviation of quarterly cash flows from operations over the three fiscal years prior to the loan initiation year scaled by total assets. Investment inefficiency is measured as the residual estimated from regressing total investments scaled by lagged total assets on lagged sales growth, leverage, cash to total assets, firm age, the natural logarithm of total assets, and excess stock return for each industry-year based on the Fama-French 48 industry classification. *Post* is an indicator that takes the value of one for a firm-year in the post-scandal period (year *t+1*, year *t+2*, and year *t+3*) and zero for a firm-year in the pre-scandal period (year *t–1*, year *t–2*, year *t–3*), where year *t* is the scandal year. All firm characteristics are measured as of the fiscal year-end immediately before the loan active date. Macroeconomic variables are measured as of the month immediately before the loan active date. All continuous variables are winsorized at the 1st and 99th percentiles. The appendix provides a detailed description of the variables. *P*-values reported in parentheses are based on standard errors adjusted for heteroskedasticity and clustered at the firm level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	Dependent variable = Log (loan spread)					
	Dividend payout		Investment inefficiency (overinvestment)		Cash flow volatility	
Independent variable	(1)	(2)	(3)	(4)	(5)	(6)
Treatment (indicator): a	Absorbed by firm fixed effects					
Post (indicator): b	Absorbed by year (industry-by-year) fixed effects					
High conflict (indicator): c	0.250**	0.078	0.113	0.075	0.090	0.073
	(0.011)	(0.543)	(0.210)	(0.486)	(0.273)	(0.513)
a × b	-0.022	-0.205	0.070	0.169*	-0.030	0.045
	(0.837)	(0.271)	(0.417)	(0.061)	(0.781)	(0.740)
a × c	-0.291**	-0.208	-0.105	0.010	-0.275**	-0.116
	(0.030)	(0.149)	(0.413)	(0.957)	(0.049)	(0.504)
b × c	-0.254**	-0.385**	-0.252**	-0.302*	-0.077	-0.120
	(0.033)	(0.047)	(0.033)	(0.061)	(0.435)	(0.343)
a × b × c	0.254*	0.462**	0.517***	0.545**	0.359**	0.297*
	(0.067)	(0.043)	(0.005)	(0.038)	(0.033)	(0.094)
Control variables (same as in Table 3)	Yes	Yes	Yes	Yes	Yes	Yes
Loan purpose fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Loan type fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	No	Yes	No	Yes	No
Industry-by-year fixed effects	No	Yes	No	Yes	No	Yes
Adjusted R^2	0.827	0.843	0.831	0.850	0.828	0.843
Number of observations	707	666	656	620	661	618

Table 7
Effects of Shareholder Scandals on Loan Spreads by Firms' Information Opacity

This table presents estimates of difference-in-differences regressions by firms' information asymmetry in which the dependent variable is the natural logarithm of all-in-spread-drawn, a rate that a firm pays in basis points over LIBOR or the LIBOR equivalent. The sample consists of 707 firm-loan observations (326 and 381 loans issued by treatment and control firms, respectively) from 2000 to 2007. Treatment (control) firms are firms whose shares are held (not held) by tainted block institutions in the year immediately before the scandal year (i.e., 2003 or 2004, in which regulatory agencies charge mutual fund families for their illegal late trading and market timing practices). Each treatment firm is matched to a control firm using a propensity score matching approach without replacement and a caliper of 0.05. The propensity score is calculated using the logit regression of *Treatment* (an indicator that takes the value of one if a firm's shares are held by the tainted block institution in the year immediately before the scandal year and zero otherwise) on the natural logarithm of total assets, Tobin's *q*, book leverage, ROA, tangibility, stock return volatility, excess stock return, and institutional block ownership. We require the treatment and control firms to be in the same industry (the same Fama-French 48 industry classification code) and in the same fiscal year, to have the same credit rating classes (i.e., no rating, non-investment rating (below BBB-), and investment rating (above BBB-)), and to have at least one loan issued both three years before and three years after the scandal announcement date. We also require that tainted block institutions continuously hold equity ownership in treatment firms for three years after the scandal year either as blockholders or as top ten institutional shareholders with at least 1% ownership. In each year, we define *High information asymmetry (indicator)* separately for treatment and control firms; this indicator takes the value of one if the firm's analyst coverage (size) is below the sample median analyst coverage (size) and zero otherwise in columns (1) and (2) (columns (5) and (6)). *High information asymmetry (indicator)* takes the value of one if the analysts' forecast dispersion about the firm's earnings is above the sample median forecast dispersion and zero otherwise in columns (3) and (4). Analyst coverage is measured as the number of unique analysts following the firm. Forecast dispersion is measured as the standard deviation of valid forecasts in the month of the fiscal-year-end, scaled by the absolute value of the mean forecasts. Firm size is measured as the natural logarithm of total assets. *Post* is an indicator that takes the value of one for a firm-year in the post-scandal period (year *t+1*, year *t+2*, and year *t+3*) and zero for a firm-year in the pre-scandal period (year *t–1*, year *t–2*, year *t–3*), where year *t* is the scandal year. All firm characteristics are measured as of the fiscal year-end immediately before the loan active date. Macroeconomic variables are measured as of the month immediately before the loan active date. All continuous variables are winsorized at the 1st and 99th percentiles. The appendix provides a detailed description of the variables. *P*-values reported in parentheses are based on standard errors adjusted for heteroskedasticity and clustered at the firm level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	Dependent variable = Log (loan spread)					
	Analyst coverage		Forecast dispersion		Firm size	
Independent variable	(1)	(2)	(3)	(4)	(5)	(6)
Treatment (indicator): a	Absorbed by firm fixed effects					
Post (indicator): b	Absorbed by year (industry-by-year) fixed effects					
High information asymmetry (indicator): c	0.038	0.087	-0.031	-0.103	-0.019	0.240
	(0.669)	(0.382)	(0.810)	(0.521)	(0.890)	(0.214)
a × b	0.055	-0.028	0.013	-0.091	0.085	0.001
	(0.540)	(0.800)	(0.899)	(0.530)	(0.401)	(0.993)
a × c	-0.056	-0.217	0.053	-0.128	-0.021	-0.297
	(0.702)	(0.267)	(0.746)	(0.609)	(0.894)	(0.184)
b × c	-0.024	-0.221	-0.146	-0.222	0.091	-0.068
	(0.841)	(0.135)	(0.251)	(0.169)	(0.414)	(0.639)
a × b × c	0.248*	0.509***	0.217	0.612**	0.093	0.340*
	(0.094)	(0.003)	(0.274)	(0.033)	(0.523)	(0.067)
Control variables (same as in Table 3)	Yes	Yes	Yes	Yes	Yes	Yes
Loan purpose fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Loan type fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	No	Yes	No	Yes	No
Industry-by-year fixed effects	No	Yes	No	Yes	No	Yes
Adjusted R^2	0.826	0.844	0.870	0.859	0.822	0.843
Number of observations	707	666	411	384	707	666

Table 8
Effects of Shareholder Scandals on Loan Covenants

This table presents estimates of difference-in-differences regressions in which the dependent variable is the number of total (general, financial) covenants. The sample consists of 504 firm-loan observations (226 and 278 loans issued by treatment and control firms, respectively) from 2000 to 2007. Treatment (control) firms are firms whose shares are held (not held) by tainted block institutions in the year immediately before the scandal year (i.e., 2003 or 2004, in which regulatory agencies charge mutual fund families for their illegal late trading and market timing practices). Each treatment firm is matched to a control firm using a propensity score matching approach without replacement and a caliper of 0.05. The propensity score is calculated using the logit regression of *Treatment* (an indicator that takes the value of one if a firm is held by the tainted block institution in the year immediately before the scandal year and zero otherwise) on the natural logarithm of total assets, Tobin's *q*, book leverage, ROA, tangibility, stock return volatility, excess stock return, and institutional block ownership. We require the treatment and control firms to be in the same industry (the same Fama-French 48 industry classification code) and in the same fiscal year, to have the same credit rating classes (i.e., no rating, non-investment rating (below BBB-), and investment rating (above BBB-)), and to have at least one loan issued both three years before and three years after the scandal announcement date. We also require that tainted block institutions continuously hold equity ownership in treatment firms for three years after the scandal year either as blockholders or as top ten institutional shareholders with at least 1% ownership. *Post* is an indicator that takes the value of one for a firm-year in the post-scandal period (year *t+1*, year *t+2*, and year *t+3*) and zero for a firm-year in the pre-scandal period (year *t–1*, year *t–2*, year *t–3*), where year *t* is the scandal year. All firm characteristics are measured as of the fiscal year-end immediately before the loan active date. Macroeconomic variables are measured as of the month immediately before the loan active date. All continuous variables are winsorized at the 1st and 99th percentiles. The appendix provides a detailed description of the variables. *P*-values reported in parentheses are based on standard errors adjusted for heteroskedasticity and clustered at the firm level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	Dependent variable =		
	Number of total covenants	Number of general covenants	Number of financial covenants
Independent variable	(1)	(2)	(3)
Treatment (indictor) × Post (indicator)	1.721*	1.903***	-0.392
	(0.073)	(0.001)	(0.181)
Control variables (same as in Table 3)	Yes	Yes	Yes
Loan purpose fixed effects	Yes	Yes	Yes
Loan type fixed effects	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Adjusted R^2	0.918	0.940	0.715
Number of observations	128	129	504

Table 9
Cumulative Abnormal Bond and Stock Returns around the Scandal Announcement Date and Their Correlations

This table presents the mean and median cumulative abnormal bond and stock returns (CARs) around the scandal announcement date for a sample of 97 firms whose block institutions are charged by regulatory agencies in 2003 or 2004 for their illegal late trading and market timing practices (*affected firms*) (Panel A) and the Pearson correlation coefficients between bond and stock CARs (Panel B). We require firms to have non-missing bond CAR (-1, 1). We obtain bond trading data from the Trade Reporting and Compliance Engine (TRACE) database and delete trades under $100,000, canceled trades, corrected trades, and commission trades. Daily bond prices are estimated by the trade-size weighted average of intraday individual transaction prices. Bond CARs are computed as the difference between *affected firms*' bond holding period returns from day t_1 before the scandal announcement date to day t_2 after the scandal announcement date and the value-weighted portfolio holding period returns of bonds during the same period that have the same Moody's credit ratings as those of *affected firms*' bonds. When an *affected firm* has multiple bonds outstanding, we use the portfolio CAR as the bond CAR, which is computed as the value-weighted average of multiple bonds' CARs. We use as the weight the ratio of the market value of each bond in the month prior to the scandal announcement month to the total market value of all bonds outstanding for a firm at the same time. Stock CARs are estimated using the market model from one day before to one day after the scandal announcement date. The market model parameters are estimated using 220 trading days of returns beginning 280 days before and ending 61 days before the scandal announcement date, where the CRSP value-weighted return is used as a proxy for the market return. The daily abnormal returns are cumulated to obtain the stock CAR from day t_1 before the scandal announcement date to day t_2 after the scandal announcement date. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Mean and median bond and stock CARs

		Bond CARs		Stock CARs	
Event window	N	Mean	Median	Mean	Median
CAR (-1, 1)	97	-0.003	-0.000	0.001	-0.002
CAR (-1, 0)	82	0.025	-0.001	0.004	0.002
CAR (0, 1)	83	-0.006**	-0.002**	0.000	0.001

Panel B: Pearson correlation coefficients between bond and stock CARs

	Stock CARs		
Bond CARs	CAR (-1, 1)	CAR (-1, 0)	CAR (0, 1)
CAR (-1, 1)	-0.077	-0.241**	-0.094
CAR (-1, 0)	-0.158	-0.121	-0.068
CAR (0, 1)	-0.185*	-0.389***	-0.172

Table 10
Cumulative Abnormal Bond Returns around the Scandal Announcement Date by Tainted Institutions' Characteristics

This table compares the cumulative abnormal bond returns (CARs) around the scandal announcement date for subsamples of firms with different characteristics of tainted block institutions that are charged by regulatory agencies in 2003 or 2004 for their illegal late trading and market timing practices (*affected firms*). We obtain bond trading data from the Trade Reporting and Compliance Engine (TRACE) database and delete trades under $100,000, canceled trades, corrected trades, and commission trades. Daily bond prices are estimated by the trade-size weighted average of intraday individual transaction prices. Bond CARs (0, 1) are computed as the difference between *affected firms*' bond holding period returns from the scandal announcement date to one day after the scandal announcement date and the value-weighted portfolio holding period returns of bonds during the same period that have the same Moody's credit ratings as those of *affected firms*' bonds. When an *affected firm* has multiple bonds outstanding, we use the portfolio CAR as the bond CAR, which is computed as the value-weighted average of multiple bonds' CARs. We use as the weight the ratio of the market value of each bond in the month prior to the scandal announcement month to the total market value of all bonds outstanding for a firm at the same time. The tainted institution's stock CAR (-1, 1) is the CAR for tainted block institutions from one day before to one day after the scandal announcement date. The market model parameters are estimated using 220 trading days of stock returns beginning 280 days before and ending 61 days before the scandal announcement date, where the CRSP value-weighted return is used as a proxy for the market return. The tainted institution's Tobin's *q* is measured by the ratio of the book value of total assets minus the book value of equity plus the market value of equity to the book value of total assets. The tainted institution's operating performance is measured by the ratio of operating income before depreciation to total assets. The blockholding period is measured by the number of quarters in the pre-scandal period during which the tainted block institution holds block ownership in treatment firms. Block ownership is measured by the percentage of ownership held by the tainted block institution at the beginning of the scandal year. In Panel A, the sample is divided into two subgroups according to the sample median institution stock CAR (-1, 1) around the scandal announcement date, and in Panels B, C, D, and E, the sample is divided into two subgroups according to the sample median institution Tobin's *q*, operating performance, blockholding period, and block ownership, respectively, all of which are measured prior to the scandal announcement. ***, **, and * in the first four columns denote significance at the 1%, 5%, and 10% levels, respectively, and those in the last two columns denote that *p*-values of *t*-tests (Wilcoxon *z*-tests) for mean (median) differences in bond CARs (0, 1) between the two subsamples of firms are significant at the 1%, 5%, and 10% levels, respectively.

Panel A: By tainted institutions' stock CARs (-1, 1) around the scandal announcement

	Affected firms with lower institution CAR (N=45): A		Affected firms with higher institution CAR (N=37): B		Test of difference (A-B): *p*-value	
	Mean	Median	Mean	Median	*t*-test	Wilcoxon *z*-test
CAR (0, 1)	-0.007***	-0.006***	0.000***	0.001	0.026**	0.019**

Panel B: By tainted institutions' Tobin's *q* prior to the scandal announcement

	Affected firms with lower institution Tobin's *q* (N=67): A		Affected firms with higher institution Tobin's *q* (N=13): B		Test of difference (A-B): *p*-value	
	Mean	Median	Mean	Median	*t*-test	Wilcoxon *z*-test
CAR (0, 1)	-0.005***	-0.004***	0.005***	0.006**	0.003***	0.003***

Panel C: By tainted institutions' operating performance prior to the scandal announcement

	Affected firms with poorer institution operating performance (N=67): A		Affected firms with better institution operating performance (N=13): B		Test of difference (A-B): *p*-value	
	Mean	Median	Mean	Median	*t*-test	Wilcoxon *z*-test
CAR (0, 1)	-0.005***	-0.004***	0.005***	0.006**	0.003***	0.003***

Panel D: By tainted institutions' blockholding period prior to the scandal announcement

	Affected firms with longer institution blockholding period (N=39): A		Affected firms with shorter institution blockholding period (N=44): B		Test of difference (A-B): *p*-value	
	Mean	Median	Mean	Median	*t*-test	Wilcoxon *z*-test
CAR (0, 1)	-0.009*	-0.002**	-0.003*	-0.000	0.247	0.422

Panel E: By tainted institutions' block ownership prior to the scandal announcement

	Affected firms with higher institution block ownership (N=42): A		Affected firms with lower institution block ownership (N=41): B		Test of difference (A-B): *p*-value	
	Mean	Median	Mean	Median	*t*-test	Wilcoxon *z*-test
CAR (0, 1)	-0.008*	-0.008**	-0.003*	0.000	0.298	0.247

Table 11
Effects of Shareholder Scandals on Firms' Post-scandal Risk-taking Behavior

Panel A of this table presents descriptive statistics of a propensity score matched sample of 160 treatment firms whose shares are held by tainted block institutions in the year immediately before the scandal year (i.e., 2003 or 2004, in which regulatory agencies charge mutual fund families for their illegal late trading and market timing practices) and 160 control firms whose shares are not held by tainted block institutions in the same year. Panel B presents estimates of difference-in-differences regressions in which the dependent variables are the ratio of total dividends to the market value of equity (columns (1) and (2)), the ratio of total investments to total assets (columns (3) and (4)), and stock return volatility (columns (5) and (6)). The sample consists of 1,884 firm-year observations (947 treatment firm-year observations and 937 control firm-year observations) from 2000 to 2007. Each treatment firm is matched to a control firm using a propensity score matching approach without replacement and a caliper of 0.05. The propensity score is calculated using the logit regression of *Treatment* (an indicator that takes the value of one if a firm's shares are held by the tainted block institution in the year immediately before the scandal year and zero otherwise) on the natural logarithm of total assets, Tobin's *q*, book leverage, ROA, tangibility, stock return volatility, excess stock return, and institutional block ownership. We require the treatment and control firms to be in the same industry (the same Fama-French 48 industry classification code) and in the same fiscal year and to have the same credit rating classes (i.e., no rating, non-investment rating (below BBB-), and investment rating (above BBB-)). We also require that tainted block institutions continuously hold equity ownership in treatment firms for three years after the scandal year either as blockholder or as top 10 institutional shareholders with at least 1% ownership. *Post* is an indicator that takes the value of one for a firm-year in the post-scandal period (year *t+1*, year *t+2*, and year *t+3*) and zero for a firm-year in the pre-scandal period (year *t–1*, year *t–2*, year *t–3*), where year *t* is the scandal year. All control variables are measured at the beginning of the fiscal year and all continuous variables are winsorized at the 1st and 99th percentiles. The appendix provides a detailed description of the variables. *P*-values reported in parentheses are based on standard errors adjusted for heteroskedasticity and clustered at the firm level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Descriptive statistics for a propensity score matched sample

	Treatment firms (N=160): A		Control firms (N=160): B		Test of difference (A-B): *p*-value	
Variable	Mean	Median	Mean	Median	*t*-test	Wilcoxon *z*-test
Log (total assets)	5.882	5.903	5.947	5.834	0.684	0.933
Tobin's *q*	1.478	1.193	1.608	1.259	0.194	0.230
Book leverage	0.196	0.167	0.204	0.160	0.732	0.896
ROA	-0.014	0.035	-0.008	0.033	0.730	0.850
Tangibility	0.246	0.188	0.228	0.168	0.397	0.223
Stock return volatility	0.034	0.030	0.037	0.033	0.103	0.090*
Excess stock return	0.097	0.064	0.096	0.086	0.982	0.597
Institutional block ownership	0.218	0.205	0.215	0.206	0.820	0.695

Panel B: Effects of shareholder scandals on firms' post-scandal risk-taking behavior

	Dividend payout		Total investments		Stock return volatility	
Independent variable	(1)	(2)	(3)	(4)	(5)	(6)
Treatment (indicator) × post (indicator)	0.003	0.002	0.025***	0.022**	0.003**	0.002**
	(0.502)	(0.529)	(0.005)	(0.020)	(0.011)	(0.021)
Firm characteristic control variables (same as in Table 3)	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	No	Yes	No	Yes	No
Industry-by-year fixed effects	No	Yes	No	Yes	No	Yes
Adjusted R^2	0.447	0.441	0.503	0.513	0.705	0.711
Number of observations	1,880	1,875	1,465	1,445	1,884	1,880

Appendix

This appendix provides detailed descriptions of all variables used in the tables.

Variable	Definition	Source
After^{+n}	One for treatment firms at year $t+n$ and zero otherwise, where year t is the scandal year	
Analyst coverage	Number of unique analysts following the firm	I/B/E/S
Before^{-n}	One for treatment firms at year $t–n$ and zero otherwise, where year t is the scandal year	
Blockholding period	Number of quarters in the pre-scandal period during which the tainted block institution holds block ownership in treatment firms	Thomson 13F
Block ownership	Percentage of ownership held by the tainted block institution at the beginning of the scandal year	Thomson 13F
Cash flow volatility	Standard deviation of quarterly cash flows from operations (*oancfy)* over the three fiscal years prior to the loan initiation year scaled by total assets (*at*)	Compustat
Cash to total assets	Cash (*che*) divided by total assets (*at*)	Compustat
Credit rating (indicator)	One if a firm has an S&P long-term credit rating and zero otherwise	Compustat
Credit spread	Difference in yields between BAA- and AAA-rated corporate bonds	Federal Reserve Economic Data
Dividend payout	Ratio of total dividends (*dvc* + *dvp*) to the market value of equity (*prcc_f* × *csho*)	Compustat
Excess stock return	Buy-and-hold return for the year net of the CRSP value-weighted index return	CRSP
Firm age	Natural logarithm of the number of years the firm has been listed on CRSP as of the start of the year	CRSP
Forecast dispersion	Standard deviation of valid forecasts in the month of the fiscal-year-end, scaled by the absolute value of the mean forecasts	I/B/E/S
High conflict (indicator)	One if the firm's dividend payout (cash flow volatility) is above the sample median dividend payout (cash flow volatility) or the level of the firm's investment inefficiency measured each year is above the 75th percentile of the sample each year (i.e., overinvestment) and zero otherwise, defined separately for treatment and control firms in each year	Compustat CRSP
High information asymmetry (indicator)	One if the firm's analyst coverage (size) is below the sample median analyst coverage (size) or the analyst's forecast dispersion about the firm's earnings is above the sample median forecast dispersion and zero otherwise, defined separately for treatment and control firms in each year	Compustat, I/B/E/S
Institutional block ownership	Number of shares held by institutional investors that own more than 5 percent of a firm's equity divided by the total number of shares outstanding	Thomson 13F
Institution CAR	Cumulative abnormal return for tainted block institutions from one day before to one day after the scandal announcement date. The market model parameters are estimated using 220 trading days of returns beginning 280 days before and ending 61 days before the scandal announcement date, where the CRSP value-weighted return is used as a proxy for the market return	CRSP
Investment inefficiency	Residual estimated from regressing total investments scaled by lagged total assets on lagged sales growth, leverage, cash to total assets, firm age, firm size, and stock return for each industry-year based on the Fama-French 48 industry classification (Richardson, 2006; Biddle, Hilary, and Verdi, 2009)	Compustat CRSP
Leverage	Ratio of total debts (*dltt* + *dlc*) to total assets (*at*)	Compustat
Loan purpose (indicator)	Indicators for loan purpose (corporate purposes, working capital, debt repayment, acquisition line, backup line for commercial paper, and others)	DealScan
Loan spread	All-in-spread-drawn, a rate that a firm pays in basis points over LIBOR or the LIBOR equivalent	DealScan
Loan type (indicator)	Indicators for loan type (term loan, revolver line of credit, 364-day facility, and others)	DealScan
Log (loan amount)	Natural logarithm of loan deal (facility) amount measured in $ millions	DealScan
Log (loan maturity)	Natural logarithm of loan maturity measured in months	DealScan
Log (total assets)	Natural logarithm of total assets (*at*)	Compustat

Non-investment grade rating (indicator)	One if a firm's S&P long-term credit rating is below BBB and zero otherwise	Compustat
Number of financial covenants	Number of covenants that place limits on accounting variables and ratios, which must be maintained while the debt is outstanding	Deal DealScan Scan
Number of general covenants	Number of covenants that restrict prepayment (including asset sales sweep, debt issuance sweep, equity issuance sweep, excess cash flow sweep, and insurance proceed sweep), dividends, and voting rights (including term change and collateral release)	DealScan
Number of total covenants	Sum of the number of general covenants, the number of financial covenants, and the value of an indicator for whether the loan requires collateral	DealScan
Operating performance	Ratio of operating income before depreciation (*oibdp*) to total assets (*at*)	Compustat
Performance pricing	One if a loan contract has the performance pricing feature and zero otherwise	
Post (indicator)	One for a firm-year in the post-scandal period (year $t+1$, year t+2, and year t+3) and zero for a firm-year in the pre-scandal period (year t–1, year t–2, year t–3), where year t is the scandal year	
ROA	Ratio of net income (*ni*) to total assets (*at*)	Compustat
Sales growth	Ratio of sales (*sale*) to lagged sales	Compustat
Stock return volatility	Standard deviation of daily stock returns in a given year	CRSP
Tangibility	Ratio of net property, plant, and equipment (*ppent*) to total assets (*at*)	Compustat
Term spread	Difference in yields on ten-year and one-year treasury bonds	Federal Reserve Economic Data
Tobin's q	Ratio of the book value of total assets (*at*) minus the book value of equity (*ceq*) plus the market value of equity (*prcc_f* × *csho*) to the book value of total assets (*at*)	Compustat
Total investments	Sum of capital expenditures (*capx*), R&D expenses (*xrd*), and acquisition expenditures (*aqc*) minus the sales of PP&E (*sppe*), scaled by total assets (*at*)	Compustat
Treatment (indicator)	One if a firm is held by the tainted block institution in the year immediately before the scandal year and zero otherwise	
Treatment with high (low) institution CAR	One if the tainted institution's cumulative abnormal return from one day before to one day after the scandal announcement date is below (above) the sample median CAR and zero otherwise	CRSP
Treatment with high (low) institution influence	One if the tainted institution's blockholding period or block ownership in treatment firms before the scandal year is above (below) the sample median and zero otherwise	Thomson 13F
Treatment with poor (good) institution performance	One if the tainted institution's Tobin's q or operating performance before the scandal year is below (above) the sample median Tobin's q or operating performance and zero otherwise	Compustat