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**NANYANG
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SINGAPORE

THREE ESSAYS ON CORPORATE FINANCE

SI FANGBO

NANYANG BUSINESS SCHOOL

2021

THREE ESSAYS ON CORPORATE FINANCE

SI FANGBO

NANYANG BUSINESS SCHOOL

A thesis submitted to the Nanyang Technological University in partial fulfilment of the
requirement for the degree of Doctor of Philosophy

2021

Statement of Originality

I hereby certify that the work embodied in this thesis is the result of original research, is free of plagiarised materials, and has not been submitted for a higher degree to any other University or Institution.

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This thesis **does not** contain any materials from papers published in peer-reviewed journals or from papers accepted at conferences in which I am listed as an author.

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Summary

My thesis covers three important aspects in corporate finance field, including risk-shifting problem, the impact of cyber risk on managerial incentives, and the organizational benefit of holding company structure.

In Chapter 1, we examine how shareholder financial difficulties affect firms' risk-shifting behaviour. Using the 2003 mutual fund scandal as a shock to institutions' risk-shifting incentives, we find that lenders charge higher loan spreads and impose more covenants after the scandal. The results are more evident when the scandal is severer, when tainted institutions have a longer holding horizon before the shock, and when firms have greater shareholder-debtholder conflicts, poorer governance, and higher information asymmetry. Moreover, bond returns around the scandal announcement date are negatively correlated with stock returns. We also find increases in firm leverage, investments, and payouts after the scandal for firms whose tainted institutions suffer more in the scandal.

In Chapter 2, we investigate whether cyberattacks affect managerial incentives and behaviours by studying the profitability of insider trading. We find that insiders who trade shares 90 days before the disclosure of cyberattacks earn significantly higher profits. The effect is more pronounced when firms have higher information asymmetry, poorer governance, and insufficient risk management systems, and when firms are headquartered in states with an effective data breach notification law. We also find that insiders at industry competitors of attacked firms earn higher profits than those at other firms prior to the disclosure of cyberattacks, particularly they possess information advantages through social connections with board members in attacked firms.

In Chapter 3, we examine the impact of a holding company structure on

announcement returns for mergers and acquisitions (M&As). We find that holding-company acquirers experience higher abnormal stock returns around the M&A announcement date and better post-merger long-term operating performance than non-holding-company acquirers. Further analyses show that the value-enhancing impact of the holding company structure is more pronounced when acquiring firms are better governed and have higher tax avoidance. The results support the view that the holding company structure enhances firm value by mitigating the manager-shareholder conflict and reducing tax costs.

Chapter 1

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

Abstract

We examine how shareholder financial difficulties affect firms' risk-shifting behavior. Using the 2003 mutual fund scandal as a shock to institutions' risk-shifting incentives, we find that lenders charge higher loan spreads and impose more covenants after the scandal. The results are more evident when the scandal is severer, when tainted institutions have a longer holding horizon before the shock, and when firms have greater shareholder-debtholder conflicts, poorer governance, and higher information asymmetry. Moreover, bond returns around the scandal announcement date are negatively correlated with stock returns. We also find increases in firm leverage, investments, and payouts after the scandal for firms whose tainted institutions suffer more in the scandal.

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

JEL Classification: G21, G23, G32, M41

I. 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, due largely to the endogenous nature of leverage and risk-taking and the lack of data.¹ We also have limited evidence about shareholder characteristics and the circumstances under which shareholders have strong incentives to influence the management to engage in risk-shifting because most of prior studies focus mainly on firm characteristics, such as debt structure and managerial compensation, in 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 strong influence on managers in risk-shifting decisions.

In this paper, we extend prior studies on asset substitution by examining how an unexpected and large change in shareholder financial conditions aggravates the lenders’ concern about risk-shifting and thus affects their ex ante loan contract terms. Specifically,

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 can reduce the firm’s optimal leverage by up to 20%. Eisdorfer (2008) further provides evidence on risk-shifting behaviors of financially distressed firms by showing the positive effect of uncertainty on the investment and the diminishing effect of investment on asset value along with increasing uncertainty in financially distressed firms.

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 the firm’s risk-shifting behavior.

using the 2003 mutual fund scandal that leads to substantial fund outflows and declines in fund value as an exogenous source of variation in tainted institutions' risk-shifting incentives, we investigate how lenders tighten price and non-price terms of loans for these institutions' portfolio firms to protect against anticipated shareholder misappropriation of assets. We also examine whether tainted shareholder and firm characteristics, such as shareholders' type and firms' governance and information environment, influence lenders' incentives to mitigate anticipated costs arising from an increase in potential risk-shifting and how firms' financial and investment policies change in the post-scandal period.

The 2003 mutual fund scandal is one of the largest scandals in the financial advisory industry over the past decades. Since 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) on September 3, 2003 for their illegal trading activities, such as late trading and market timing, at least 25 large fund families were implicated in the scandal in 2003 and 2004 and charged for their illegal attempt to benefit some favored clients at the expense of other clients.³ As a result, tainted institutions suffer massive money outflows after the scandal. Figure 1 compares the cumulative monthly net inflows between implicated fund families and non-implicated fund families. On average, implicated fund families lost about 14%, 20%, and 26% capitals in one year, two years, and three years after the scandal, respectively. In contrast, non-implicated fund families accumulate about

³ Late trading is an illegal practice that allows some clients of the funds to purchase their shares at the closing price after the market closing. Due to the differences in trading time across countries, these clients can use their private information available in other countries' markets for their late trading, which allows them to profit at the expense of the funds' other long-term buy-and-hold clients. Market timing is an investment strategy in which investors buy and sell fund shares frequently to take advantage of inefficiencies in mutual fund pricing. While market timing is not illegal per se, it can harm other investors in the same fund because it dilutes the value of fund shares and increases the costs of administering the fund that are borne by these investors. Funds are accused of illegal market timing in the 2003 scandal because they allow some clients to trade more frequently than specified in their fund documents and prospectus.

7%, 15%, and 28% more assets in one year, two years, and three years after the scandal, respectively. The massive outflows in implicated funds certainly translate to a considerable loss of revenue.⁴

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 particularly care about money inflows and outflows from their clients. Therefore, any negative shocks, such as the 2003 mutual fund scandal, that result in massive outflows can have a significant financial impact on these institutions and thus incentivize them to take actions that help mitigate the adverse effect of the shocks. For example, affected institutions may attempt to boost up their short-term performance to refrain from further outflows and attract new inflows. They may also influence their portfolio firms to engage in aggressive risk-taking in order to increase their wealth at the expense of debtholders.⁵ 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 would 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 is entirely driven by institutions' illegal behavior, not by their portfolio firms, it should represent a setting where firms face

⁴ Houge and Wellman (2005) estimate that these declines translate into \$844.7 million of combined revenue losses with an average expense ratio of 1.42%, over the 6-month, post-announcement period.

⁵ Intervention (or voice) from institutional investors is well investigated in the literature (e.g., Shleifer and Vishny, 1986; Bolton and von Thadden, 1998; McCahery et al., 2016). McCahery et al. (2016) survey institutional investors and find that 63% of respondents state that they have engaged in direct discussions with management in the past five years, and 45% state that they have had private discussions with a company's board outside of management's presence.

an exogenous shock to their tainted institutions' incentives for risk-shifting.⁶

The risk-taking theory predicts that the effects of tainted institutions' financial difficulties on loan contract terms vary depending on the severity of their illegal activities during the 2003 scandal,⁷ their investment horizon, and portfolio firms' existing shareholder-debtholder conflicts, governance quality, and information environment. First, it predicts the negative effect of shareholder scandals on loan contract terms to be more pronounced for firms owned by tainted institutions with higher severity of their illegal activities. To the extent that these institutions suffer more outflows from their illegal activities, they may have stronger incentives to transfer wealth from debtholders to themselves and thus influence their portfolio firms more aggressively to engage in risk-shifting activities. Thus, a lender, anticipating such an increase in risk-shifting activities, should adjust the loan contract terms of firms with such institutions more adversely to alleviate its perceived risk of high asset substitution.

Second, we expect that the adverse effect of shareholder scandals on loan contract terms varies across institutions' prior holding horizon. The traditional wisdom in the literature of institutional ownership argues that institutions with longer holding horizon are more likely to focus on the long-run value of firms. For example, Bushee (2001), which classifies institutional investors into three types, transient, quasi-indexer, and dedicated investors according to their investment horizon, turnover, and concentration, shows that dedicated institutions tend to hold shares of their portfolio firms for a long-term period.⁸

⁶ Consistent with this view, Antón and Polk (2014) and Crane, Koch, and Michenaud (2019) use the 2003 mutual fund scandal as an exogenous shock to institutional ownership.

⁷ For example, the Securities and Exchange Commission (SEC) imposes civil penalties to tainted institutions, ranging from 0 to \$142 million. The disgorgement ordered to be paid back from tainted institutions illegal gains ranges from 0 to \$250 million.

⁸ Bushee's classification on investor types change over time. In fact, most tainted institutions in our sample become quasi-indexers after the scandal. Thus, we do not examine the cross-sectional variations in the impact

Relatedly, Chen, Harford, and Li (2007) find that dedicated institutions with a long investment horizon are more likely to monitor management compared to other institutions (i.e., transient and quasi-indexer institutions). However, such incentives may change when these institutions suffer from financial difficulties and seek quick recovery. Since longer holding horizon also indicates that such institutions have built up strong connections with the management of portfolio firms, they can exploit such connections to influence portfolio firms to engage in risk-shifting activities. Thus, lenders are expected to be more concerned about the potential increase in risk-shifting activities for implicated institutions with longer holding horizon and adjust loan contract terms more aggressively.

Third, the adverse effect of shareholders' financial difficulties on loan contract terms is expected to be stronger for firms with greater shareholder-debtholder conflicts. Debtholders' concerns about potential wealth transfer from themselves to shareholders are likely to be greater if firms have maintained the policies that favor shareholders, such as policies that lead to excessive payouts, overinvestments, and high risk. Since these policies hurt debtholders by exacerbating asset substitution problems, lenders who want to internalize the costs of such problems would demand higher interest rates and require more stringent covenants when their borrowers' shareholders suffer from large financial difficulties.

Fourth, the negative effect of the 2003 mutual fund scandal on loan contract terms is likely to vary across firms with good and bad governance. Prior studies show that high quality of a firms' internal governance benefits debtholders by limiting opportunistic management behavior and improving financial reporting quality (e.g., Klein, 2002;

of different types of tainted institutions.

Cornett, Marcus, and Tehranian, 2008). However, good governance can also incentivize managers to take more risk-shifting activities (e.g., Ferreira and Laux, 2007; Phan and Hegde, 2013), which hurts debtholders. Thus, a priori, it is unclear what effect corporate governance has on loan contract terms.

Finally, we expect the adverse effect of shareholder scandals on loan contract terms to be more evident for firms with higher information asymmetry than for those with lower information asymmetry. To alleviate the risk of being expropriated due to increased risk-taking by 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 contract terms more unfavorably when tainted institutions' portfolio firms have less transparent information environment.⁹

To test these predictions, we use a propensity-score matched sample of treatment firms that are held by tainted block institutions in the year immediately before the scandal and their control firms that 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 in treatment firms for three years after the scandal, either as blockholders or non-blockholders.¹⁰ We also require that

⁹ 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.

¹⁰ Given that many financial block institutions do not hold equity in the same firms longer than one year (Kang, Luo, and Na, 2018), requiring them to be blockholders of the same firms for three years leaves us few treatment firms. Thus, we include tainted institutions that sell some of their block ownership in the firms and become non-blockholders after the scandal in the treatment sample. We use a three-year holding period to fully capture the potential effect of scandals on firms' risk-taking behavior.

tainted institutions do not hold equity in control firms for three years after the scandal as blockholders to make sure 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.¹¹ 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 20.78 basis points in loan spreads in the three years following the scandal, which translates into 13.09% of the sample mean. We also find that lenders impose more covenants for treatment firms after the scandal.¹² Thus, banks consider potential adverse impacts of shareholder financial difficulties on firms' risk-shifting when adjusting the price and non-price terms of loans.¹³

Turning to cross-sectional heterogeneity in the changes in loan contract terms, we find that the adverse effects of the scandal on loan contract terms are more pronounced for firms held by tainted institutions with higher severity of their illegal activities, such as tainted institutions that experience lower stock market reactions around the scandal announcement date, those that receive higher civil penalties by the SEC, and those that have to pay back

¹¹ Tainted institutions' risk-shifting incentives are likely to be different among firms with heterogeneous characteristics. By excluding firms that are newly purchased and those that are sold after the scandal and focusing on homogenous firms during three years after the scandal, we can eliminate the potential impact of the variation in firm characteristics on loan contract terms and their influence on institutions' risk-shifting incentives.

¹² We measure covenant strictness by taking the sum of five covenant indicators: the existence of more than two financial covenants, dividend restriction, asset sales sweep, equity issuance sweep, and debt issuance sweep.

¹³ 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 made by these bank institutions from the sample.

higher disgorgement as illegal gains.

We also find that lenders charge higher loan spreads and impose more covenants after the scandal only when tainted institutional shareholders have longer investment horizon prior to the shock. Thus, though institutions with longer investment horizon are more likely to focus on the long-run value of firms, lenders are more concerned about the possibility that such institutions may exploit their power in portfolio firms to engage in more risk-shifting activities to help themselves to recover from the scandal.

We further find that the adverse effects of shareholder scandals on loan contract terms are more pronounced for firms that pay out excessively, firms that overinvest, and firms that have higher idiosyncratic risk. Given that these firms have greater shareholder-debtholder conflicts, the results suggest that firms' failure to adopt debtholder-friendly policies accentuate lenders' concern about potential asset substitution problems.

For corporate governance quality, we find weak evidence that the adverse effects of the scandal on loan contract terms are more pronounced when treatment firms have a lower proportion of outside directors on the board, when their CEOs also serve as board chair, and when their CEOs have a lower proportion of equity-linked compensation. Thus, good governance alleviates debtholders' concern on risk-shifting. The adverse effects are also particularly evident among informationally more opaque firms, such as smaller firms, firms with lower analyst following, and firms with higher absolute discretionary accruals. These results suggest that high information asymmetry vis-à-vis borrowers makes it difficult for banks to assess an increase in borrowers' incentives to engage in risk-shifting due to a negative shareholder shock, which makes lenders charge higher loan spreads and impose more stringent covenants.

Next, to shed additional light on the evidence on 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 bond abnormal returns, non-negative stock abnormal returns, and the negative correlation between these two abnormal returns around the scandal announcement date. Consistent with these predictions, we find that the mean and median cumulative abnormal returns for bonds of affected firms with tainted institutions from the scandal announcement date to one day after the scandal announcement date (CAR (0, 1)) are -0.9% and -0.3%, respectively, both of which are significant at the 1% level. In contrast, the corresponding mean and median CARs (0, 1) for stocks are insignificant -0.1% and 0.0%, respectively. Moreover, the Pearson correlation between these bond and stock returns is significantly negative with the coefficient of -0.27. In untabulated tests, we also find that long-term bond and stock returns for treatment firms over 36 months after the scandal announcement month are negatively and significantly correlated with each other, while the corresponding long-term bond and stock returns for control firms are insignificantly correlated each other.

Lastly, we examine whether firms' risk-taking behaviors change in the post-scandal period. We find that treatment firms' leverage increases significantly after the scandal when their tainted institutions suffer more in the scandal. These treatment firms also invest more and increase their payouts to shareholders after the scandal. Thus, ex post, firms whose blockholders experience severer scandals engage in more risk-taking activities after the scandal, which suggests that having contracts that eliminate debtholder-shareholder conflicts, including asset substitution problems, is costly and incomplete (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 conflicts exacerbate 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 firms' risk-shifting due to a negative shock, charge significantly higher loan spreads of more than 13% of the sample average loan spread 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 use shareholder financial conditions and their type (i.e., dedicated versus non-dedicated institutions) as the factors that influence shareholder-debtholder conflicts and show that these shareholder characteristics significantly affect price and nonprice terms of the 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 an increase in loan spreads and the strictness in covenants, which suggests that lenders take the changes in shareholder's 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 these misconducts on the financial advisory industry and management firms (Qian, 2006; Dimmock, Gerken, and Graham, 2018; Egan, Matvos, and Seru, 2018), we study how misconducts by financial advisors affect their portfolio firms' risk-taking behaviors and debtholders' reassessment of loan contract terms, thus calling attention to the broader impact of misconducts 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 contract terms 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 contract terms. In Section 5, we discuss the comovements between stock and bond abnormal returns around the scandal announcement date and in the post-scandal period. Section 6 investigates how firms' risk-taking behaviors change after the scandal. Section 7 summarizes and concludes the paper.

II. Sample and Summary Statistics

2.1. Sample

To obtain our sample firms, we start with 25 mutual fund families implicated in the 2003 mutual fund scandal. We collect the list of these implicated fund families from various sources, including *Factiva* and administrative proceedings by the SEC. Table 1 provides the information on the name of implicated fund families, the initial public reporting date of the implication, assets under management (AUM) excluding money market funds, the civil penalty imposed by regulatory agencies for illegal trading, and the disgorgement ordered to be paid back from illegal gains. We obtain the information on AUM, civil penalties, and disgorgements from McCabe (2009). When the data is missing, we manually collect it from the administrative proceedings against implicated mutual funds. The total AUM of implicated mutual funds are over \$1 trillion, accounting for more than 20% of the mutual fund industry's AUM excluding money market funds (i.e., AUM in long-term mutual funds) (McCabe, 2009). Civil penalties (disgorgements of profits) show a large variation across implicated mutual funds, ranging from \$0 to \$142 million (\$250 million).

We then search the information on implicated mutual fund families' management companies and ultimate parent firms ("tainted institutions"), if any, from their websites and 10-K filings, and obtain the information on tainted institutions' block equity ownership in each portfolio firm from the Thomson Reuters Institutional (13F) database. We focus on tainted institutions, not on fund families, because their financial conditions are significantly deteriorated by their implicated subsidiaries' businesses in the mutual fund industry and the adverse spillover effect of implicated funds on their other non-implicated fund families.

Moreover, tainted institutions can exert an 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 CRSP, respectively, into affected and non-affected firms. Affected firms are firms 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 that are not held by tainted block institutions but 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 in the New York Stock Exchange (NYSE), Nasdaq, and 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) and utility (SIC codes 4900-4999) industries. These procedures result in 715 unique affected firms in 2003 and 2004 (756 firm-year observations) and 2,710 unique non-affected firms (4,227 firm-year observations) in the same years. The affected and non-affected firms have 378 and 1,858 loan facilities reported in the Loan Pricing Corporation's (LPC) DealScan database in the year immediately before the scandal year, respectively.¹⁴

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 regression of *Treatment* (an indicator that takes the value of one if a firm is held by tainted block institutions in the year immediately before the

¹⁴ We thank Michael Roberts for sharing the DealScan-Compustat link file on his website: <http://finance.wharton.upenn.edu/~mrrobert/>. We consider the link is valid if the matching score between company names in the two databases is over 80.

scandal year, and zero otherwise) on the natural logarithm of total assets, ROA, stock performance, book leverage, Tobin's q , tangibility, cash flow volatility, institutional ownership, and R&D expenses scaled by total assets. We follow Graham, Li, and Qiu (2008) and Lin et al. (2013) and choose the matching variables that can measure firms' risk, performance, growth opportunity, the availability of collateral, and corporate governance. We require the treatment and control firms to be in the same industry (the same two-digit SIC codes) and in the same fiscal year and 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 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 in treatment firms for three years after the scandal, either as blockholders or non-blockholders.¹⁵ We further require that no tainted institutions hold equity in control firms for three years after the scandal as blockholders to make sure 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 0.1) and one-to-one matching without replacement. Our final sample consists of 1,119 firm-loan observations (556 loans issued by 112 treatment firms and 563 loans issued by 112 control firms) from 2000 to 2007.¹⁶

¹⁵ In our propensity-score matched sample, the mean (median) values of ownership by tainted block institutions in treatment firms at the beginning of years $t+1$, $t+2$, and $t+3$ are 0.077 (0.065), 0.064 (0.052), and 0.055 (0.037), respectively. And among 120 tainted block institutions prior to the shock, 77, 62, and 45 institutions are still block institutions in years $t+1$, $t+2$, and $t+3$, respectively.

¹⁶ Not all tainted institutions of 25 implicated mutual funds are used in our propensity-score matched sample because some tainted institutions are acquired within three years after the scandal, and thus they stop filing Form 13F to the SEC about the information on their equity holdings (e.g., FleetBoston). Some 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 25 tainted institutions, 14 are used in our propensity-score matching analyses:

2.2. Summary statistics

Panel A of Table 2 presents summary statistics for a sample of 756 affected firm-year observations in the year immediately before the scandal year and 4,227 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 operating risk (i.e., lower cash flow 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, higher institutional ownership, and lower R&D intensity. Overall, these results suggest that affected and non-affected firms differ in several dimensions of firm characteristics.

Panel B of Table 2 compares the characteristics of loans issued by affected and non-affected firms in the year immediately before the scandal year. We find that loan maturity and the frequencies of collateral requirements (i.e., secured loans) are not significantly different between affected and non-affected firms. However, affected firms' loans are larger and have lower loan spreads and covenant strictness index scores than those of non-affected firms, possibly due to better operating performance and lower risk of affected firms shown in Panel A. We measure the loan spreads as all-in-spread-drawn, a rate that a firm pays in basis points over LIBOR or the LIBOR equivalent and the covenant strictness index as the sum of the following five covenant indicators: dividend restriction, the existence of more than two financial covenants, asset sales sweep, equity issuance sweep,

Allianz Dresdner, AXA Financial, Bank of America, Deutsche Bank, Franklin Resources, Gabelli, Heartland Advisor, J.W. Seligman & Co., Janus Capital Management, MFS Investment Management, Putnam Investment Management, United States Trust, Wachovia, and Waddell & Reed.

and debt issuance sweep.

Panel C of Table 2 presents the differences in descriptive statistics between 112 treatment firms and 112 control firms used in the propensity-score matched analysis. None of the firm characteristics show significant differences between them, except for the difference in the median value of tangibility, which is significant at the 10% level. Thus, our propensity-score matching approach identifies control firms that have very similar characteristics as treatment firms.

III. Effect of Shareholder Scandals on Loan Contract Terms: Difference-in-Differences Tests

In this section, to examine the impact of shareholder scandals on loan contract terms, we perform difference-in-differences tests using a propensity-score matched sample of 1,119 firm-loan observations. Specifically, we estimate ordinary least squares (OLS) regressions in which the dependent variables are the natural logarithm of a loan spread and the covenant strictness index, respectively.¹⁷ 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 is 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 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. We also control for

¹⁷ Loans with missing information on covenants used to compute the covenant strictness index are excluded from the analyses using the covenant strictness index as the dependent variable, reducing sample size from 1,119 to 253 observations.

loan-specific characteristics, including loan maturity and loan amount, and macroeconomic variables, including credit spread (the difference in yields between BAA and AAA corporate bonds) and term spread (the difference in yields between 10-year and 2-year treasury bonds obtained from Federal Reserve Board). All these control variables are measured at the beginning of the fiscal year except for credit spreads and term spreads that are measured at one month before the loan facility issuance date. We further include loan purpose, loan type, firm, and year fixed effects to control for the 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.¹⁸ In column (1) in which we use loan spreads as the dependent variable, we find that the coefficient on the interaction term between *Treatment* and *Post* is positive and significant at the 5% level. The coefficient estimate of 0.123 suggests that loan spreads of firms held by tainted block institutions increase by 13.09% ($e^{0.123} - 1$) after a scandal. Given that the average loan spread for the propensity-score matched sample is 158.78 basis points, this number indicates that firms held by tainted block institutions experience an increase in the average loan spread of 20.78 basis points. In column (2) in which we use the covenant strictness index as the dependent variable, we find that the coefficient on the interaction term between *Treatment* and *Post* is positive and significant at the 10% level. Thus, banks tend to impose more covenants on firms held by tainted institutions after the scandal.

¹⁸ Tainted institutions may also invest in debt securities and hence can be debtholders of their portfolio firms. Our results still hold if we exclude treatment firms with such tainted institutions.

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 lenders, anticipating firms' increased risk-shifting behaviors, charge higher loan spreads and impose more stringent covenants to them.

A key assumption of the difference-in-differences test is that treatment and control firms have parallel trends in outcome variables 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 column (1) of Table 3 with four indicators, $Before^{-1}$, $After^{+1}$, $After^{+2}$, and $After^{+3}$, where $Before^{-1}$ is an indicator that takes the value of one for treatment firms at year $t-1$ 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.¹⁹ Thus, the coefficient on $Before^{-1}$ ($After^{+n}$) captures how the loan spreads of treatment firms at year $t-1$ (year $t+n$) are different from those of treatment and control firms at year $t-2$ and year $t-3$. If the parallel trend assumption holds, we should observe an insignificant coefficient on $Before^{-1}$.

Figure 2 plots the point estimates and the 95% confidence intervals of the coefficients on $Before^{-1}$ and $After^{+n}$. Squares connected by blue line and spikes connected by red line represent the point estimates and the 95% confidence intervals of coefficients on $Before^{-1}$ ($After^{+n}$), respectively. We find that the point estimate of the coefficient on $Before^{-1}$ is close to zero (p -value = 0.56), suggesting that the loan spreads of treatment and control firms

¹⁹ We do not examine whether the parallel trend assumption holds for the test using the covenant strictness index as the independent variable because the sample size is reduced by almost 75% due to missing information on covenants.

move in parallel prior to the scandal year. However, there is a large increasing trend in the point estimates of the coefficients on $After^{+2}$ (p -value = 0.04) and $After^{+3}$ (p -value = 0.05), indicating that lenders charge higher loan spreads for treatment firms starting two years after the scandal.

Overall, 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 loan contract terms of treatment firms.

IV. Cross-Sectional Heterogeneity in Changes in Loan Contract Terms

In this section, we examine how the effects of shareholder scandals on loan contract terms differ across the severity of tainted institutions' illegal activities, institutions' holding horizon, and firm characteristics, such as shareholder-debtholder conflicts, managerial agency problems (i.e., corporate governance), and information environment.

4.1. The severity of tainted institutions' illegal activities

We first examine how the effect of shareholder scandals on loan contract terms varies depending on the severity of tainted institutions' illegal activities. Tainted institutions are likely to have stronger incentives to transfer wealth from debtholders to themselves if they suffer more outflows from their illegal activities or if they experience a more considerable decrease in share value associated with such activities. Thus, we expect the adverse effect on loan contract terms to be more pronounced for firms owned by tainted institutions with higher severity of their illegal activities. We focus on three measures of the severity of tainted institutions' illegal activities: three-day cumulative abnormal return around the

scandal announcement date (CAR (-1, 1)) for tainted block institutions, civil penalties charged to tainted block institutions, and disgorgements. The CAR (-1, 1) is estimated using the market model.²⁰ Civil penalties and disgorgements are obtained from SEC administrative proceedings. We decompose the interaction term between *Treatment* and *Post* into two terms: *High severity* $\times$ *Post* and *Low severity* $\times$ *Post*, where *High (Low) severity* is an indicator that takes the value of one if the value of severity, measured by the CAR (-1, 1), is below (above) its sample median, and zero otherwise.²¹ For civil penalty (disgorgement), *High (Low) severity* takes the value of one if its value is above (below) the sample median, and zero otherwise.

The results are reported in Table 4. In columns (1)-(3), we use loan spreads as the dependent variable, and in columns (4)-(6), we use the covenant strictness index as the dependent variable. We find that the coefficients on *High severity* $\times$ *Post* are positive and significant at least at the 5% level for all three measures of severity, while these on *Low severity* $\times$ *Post* are positive and insignificant. In particular, the coefficient on *High severity* $\times$ *Post* in column (1) is 0.227, indicating that banks charge higher loan spreads of 25.48% for firms whose tainted block institutions experience a larger drop in share value around the scandal reporting date than for control firms. The difference in coefficients on these two interaction terms is significant when the severity of scandals is measured by CAR (-1, 1).

Thus, lenders adjust loan contract terms of firms more adversely when firms' tainted institutions suffer more from financial difficulties due to higher severity of their illegal

²⁰ 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.

²¹ More than 90% of tainted institutions' CARs are negative around the scandal announcement date.

activities, supporting our hypothesis that lenders internalize anticipated costs of firms' aggressive risk-shifting more heavily when they perceive firms' shareholders to have more significant financial difficulties.

4.2. The holding horizon of tainted institutions

Next, we examine whether our results are different depending on institution's holding horizon by dividing tainted institutions into two subgroups based on their holding period prior to the scandal announcement date. We predict lenders to charge higher loan spreads and impose more stringent covenants to firms whose tainted institutions have longer holding period. Though institutions with longer investment horizon are typically considered to focus on the long-run value of firms rather than on myopic short-term profits, such incentives may change when they suffer from financial difficulties. Moreover, longer holding horizon provides these institutions sufficient power to influence the management of portfolio firms. Therefore, lenders would adjust loan contract terms more aggressively for such firms. To test this prediction, we decompose the interaction term between *Treatment* and *Post* into *Prior holding longer than 3 years* $\times$ *Post* and *Prior holding shorter than 3 years* $\times$ *Post*, where *Prior holding longer (shorter) than 3 years* is an indicator that takes the value of one if the tainted institution has been a shareholder or blockholder of the treatment firm for more (less) than 3 years prior to the scandal announcement date and zero otherwise.

The results are reported in Table 5. Consistent with our prediction, in all four columns, the coefficients on *Prior holding longer than 3 years* $\times$ *Post* are positive and significant at the 5% level, whereas those on *Prior holding shorter than 3 years* $\times$ *Post* are insignificant.

The difference in coefficients between these two interaction terms is significant for both loan spreads and covenant strictness index when the *Prior holding longer (shorter) than 3 years* indicator is measured using prior blockholding horizon.

4.3. Firms' shareholder-debtholder conflicts

As a further test of cross-sectional heterogeneity in changes in loan contract terms, in this subsection, we focus on firms' shareholder-debtholder conflicts. Firms' policies that exacerbate shareholder-debtholder conflicts, 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 increase firm risk, benefiting shareholders at the expense of debtholder. Thus, ex ante, lenders are expected to tighten the loan contract terms more unfavorably for firms that adopt debtholder-unfriendly policies. To test this prediction, we first measure *Payout* as the sum of dividends and share repurchases scaled by total assets, *Investment inefficiency* as the residual estimated from a regression of total investments on lagged sales growth for each industry-year based on the Fama and French 48-industry classification (Biddle, Hilary, and Verdi, 2009), and *IVOL* as the standard deviation of residuals estimated from the regression of daily excess returns on Fama-French three factors over the fiscal year (i.e., stock return idiosyncratic volatility). We then divide treatment firms into two subgroups according to the sample median of each of these three measures, firms with a

higher debtholder-shareholder conflict (*More conflict*) and those with a lower debtholder-shareholder conflict (*Less conflict*). *More (Less) conflict* is an indicator that takes the value of one for firms in which *Payout*, *Investment inefficiency*, or *IVOL* is above (below) its sample median, and zero otherwise.

The results are reported in Table 6. In columns (1)-(3), the dependent variable is the natural logarithm of loan spreads, and the measures of debtholder-shareholder conflicts are *Payout*, *Investment inefficiency*, and *IVOL*, respectively. We find positive and significant coefficients on *More conflict* $\times$ *Post* and insignificant coefficients on *Less conflict* $\times$ *Post* in all three regressions. The difference in coefficients between the two interaction terms is significant in columns (2) and (3). It is noteworthy that the magnitude of the average increase in loan spreads for firms with a greater shareholder-debtholder conflict is economically larger than that for firms with a lower shareholder-debtholder conflict. For example, while the average increase in loan spreads for firms that do not overinvest is only 8.22%, the corresponding number for firms that overinvest is almost five times larger at 38.12%.

In columns (4)-(6), we replace the natural logarithm of loan spreads with the covenant strictness index as the dependent variable and find that the results are similar to those in columns (1)-(3).²²

Overall, these results strongly support our prediction that lenders demand a higher return on loans and impose stricter covenants in loan contracts when affected firms adopt debtholder-unfriendly policies.

²² In untabulated test, we replace *Investment inefficiency* with an *Overinvestment* indicator, which takes the value of one if the value of *Investment inefficiency* is positive, and zero otherwise. We find that our results are qualitatively similar.

4.4. Firms' governance quality

We next turn our attention to how firms' governance quality affects lenders' response to shareholder scandals. Although good corporate governance may induce managers to undertake high-risk projects to benefit shareholders (Ferreira and Laux, 2007; Laeven and Levine, 2009; Phan and Hegde, 2013), it can also limit managers' opportunistic behaviors and reduce managerial shirking, thus benefiting both shareholders and debtholders (Klein, 2002; Cornett, Marcus, and Tehranian, 2008). Therefore, it is an empirical question whether which of these two competing effects dominates in our analyses. We use three measures of a firm's governance: the proportion of outside directors on the board (the ratio of the number of outside directors to the total number of directors on the board), CEO-Chair duality (indicator), and the product market competition (Herfindahl-Hirschman Index (*HHI*)). *HHI* is computed as the sum of the squared market share (measured as sales) of each firm in each two-digit-SIC industry and in each year. Using each of these measures of a firm's governance, we divide treatment firms into firms with poor governance (*Poor governance*) and those with good governance (*Good governance*). *Poor (Good) governance* takes the value of one if the CEO is (is not) the chair of the board or the proportion of outside directors on the board (*HHI*) is below (above) its sample median, and zero otherwise.

Table 7 reports the results. We find that the coefficients on *Poor governance* $\times$ *Post* are positive and significant in all six regressions, while those on *Good governance* $\times$ *Post* are positive and significant only in columns (1) and (6). The difference in coefficients between the two interaction terms is significant at the 5% level in column (4) in which we

use the covenant strictness index as the dependent variable and the proportion of outside directors as the measure of a firms' governance.

In sum, the results in this subsection provide some weak evidence that debtholders demand higher loan spreads and more stringent covenants for firms with poor governance.

4.5. Firms' information environment

Lastly, we examine how the effects of shareholder scandals on loan contract terms vary across firms' information environment. Lenders have to incur costs to obtain firm-specific information and monitor firms' management. Such costs tend to increase as information asymmetry vis-à-vis firms becomes greater as high information asymmetry limits lenders to obtain 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 and impose more stringent covenants when firms have higher information asymmetry. We use three measures as the proxies for firms' information asymmetry, firm size, analyst coverage (the number of unique analysts following the firm), and the absolute discretionary accrual (the absolute value of discretionary current accruals computed using the modified Jones (1991) model) and divide treatment firms into two subgroups according to the sample median of each of these three measures, *High information asymmetry* and *Low information asymmetry*. *High (Low) information asymmetry* takes the value of one for firms whose value of information asymmetry, measured by firm size, analyst coverage, or absolute discretionary accruals, is above (below) its sample median, and zero otherwise.

The results are presented in Table 8. Consistent with our prediction 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, we find that the coefficients on *High information asymmetry* $\times$ *Post* are positive and significant in all six regressions. In contrast, none of the coefficients on *Low information asymmetry* $\times$ *Post* are insignificant. However, the differences in coefficients between these interaction terms are insignificant.

V. 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 non-negative, while the market's ex-ante valuation of bond value should be negative. Risk-shifting also suggests that scandal announcement returns for bonds are negatively correlated to those for stocks due to wealth transfers from debtholders to shareholders.

To test these predictions, we obtain bond and stock price data from the Trade Reporting and Compliance Engine (TRACE) and the Center for Research in Security Prices (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 two months 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 4.

Panel A of Table 9 reports bond and stock CARs around the scandal announcement date. The sample consists of 81 affected firms with bond outstanding. We find that the mean and median bond CARs (0, 1) of affected firms are -0.8% and -0.3%, respectively, both of which are significant at the 1% 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 of Table 9, we present the Pearson correlation coefficients between bond and stock CARs. Consistent with our expectation, we find a negative and significant correlation between bond CARs (0, 1) and stock CARs (0, 1). The magnitude of the correlation coefficient is also large at -0.22. Bond CARs (0, 1) are also negatively and significantly correlated with stock CARs (-1, 1) and stock CARs (-1, 0).

Overall, these results suggest that scandals that impose significant financial difficulties to shareholders do indeed exacerbate conflicts of interest between shareholders and debtholders.²³

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

If deterioration of large shareholders' financial conditions due to their scandals 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 with greater protections against asset substitution, it may be difficult for lenders to precisely assess, ex-ante, 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 (i.e., shareholders' risk-shifting incentives). To address this issue, in this section, we examine how firms' risk-taking behaviors, such as debt financing, investment, and payout to shareholders, change after the scandal.

²³ As an additional test of the wealth transfer from bondholders to shareholders, in untabulated tests, we examine long-term bond and stock returns for 41 treatment firms and 29 control firms used in Table 3 that have bond price data available over 36 months after the scandal announcement month. Long-term raw bond (stock) returns are computed by cumulating monthly bond (stock) returns over 36 months after the scandal announcement year. Long-term abnormal bond (stock) returns are computed by subtracting the returns of Merrill Lynch bond indices with same Moody's rating as those of the bonds (CRSP value-weighted portfolios) over 36 months after the scandal announcement month from long-term raw bond (stock) returns over the same period. When a firm has multiple bonds outstanding, we use issue-size-weighted portfolio returns as long-term bond returns. Consistent with our prediction, we find that the correlation coefficient between long-term raw (abnormal) bond returns and long-term raw (abnormal) stock returns for treatment firms is -0.351 (-0.344), significant at the 5% level, while that between long-term raw (abnormal) bond returns and long-term raw (abnormal) stock returns for control firms is small and insignificant at -0.030 (-0.030).

We start with a sample of all treatment and control firms used in the previous analyses from 2000 to 2007. Our final sample consists of 643 firm-year observations of 112 treatment firms and 650 firm-year observations of 112 control firms. We use several measures of firms' risk-taking behavior, including the ratios of total debt, capital expenditure, R&D expense, acquisition expenditure, total investment (the sum of capital expenditure, R&D expense, and acquisition expenditure), and payout (the sum of dividends and share repurchases), measured at the end of the fiscal year, to lagged total assets.

The results using these measures of firms' risk-taking behavior as the dependent variables are reported in Table 10. We decompose the interaction term between *Treatment* and *Post* into *High severity* $\times$ *Post* and *Low severity* $\times$ *Post* because potential risk-shifting activities should vary across the magnitude of tainted institutions' incentives to engage in such activities. In column (1) in which the dependent variable is book leverage, we find that the coefficient on *High severity* $\times$ *Post* is a significant 0.035, suggesting that compared to control firms, treatment firms increases their leverage 3.5% more after the scandal when their tainted institutions suffer from server scandals. In columns (2)-(5) in which we use the changes in firms' investments in the post-scandal period as the dependent variables, we find that the coefficients on *High severity* $\times$ *Post* are all positive and significant except for column (3) in which we use R&D intensity as the measure of a firms' risk-taking behavior. In column (6), the dependent variable is operational risk measured by the volatility of EBITDA scaled by the average EBITDA in a sixteen-quarter window.²⁴ We find that the coefficient on *High severity* $\times$ *Post* is a significant 0.108 in column (6). However, none of the coefficients on *Low severity* $\times$ *Post* are significant in columns (1)-(6). Of six

²⁴ This measure is used in many prior studies, such as Minton and Schrand (1999), Harford (1999), DeFond and Hung (2003), and Danielova et al., (2013).

regressions, the differences in coefficients between *High severity* $\times$ *Post* and *Low severity* $\times$ *Post* are significant in three regressions (columns (1), (4), (5)).

Thus, tainted institutions that suffer more from the scandal have stronger incentives to influence firms to engage in risk-shifting after the scandal than those with less severe scandals. These results, together with those in Table 4 that lenders demand higher ex ante pricing terms and impose stricter ex ante non-pricing terms for firms held by tainted institutions that suffer more in the scandal, also suggest that contracting that completely eliminates shareholder-debtholder conflicts is difficult and incomplete.

VII. Robustness Tests

To check the robustness of our results, we conduct several additional tests. First, prior studies have shown that pricing and non-pricing terms of bank loans could be jointly determined (e.g., Graham et al., 2008; Costello et al., 2011). To address this issue, we follow the spirit of Campello and Gao (2017) to estimate a system of equations for loan spread and covenant strictness index using three-stage-least-squares (3SLS). The results are similar to those in Table 3.

Second, it is possible that downward price pressure in portfolio firms due to high redemptions in implicated institutions could also encourage lenders to charge a higher loan spread. Lowered share price can mechanically increase the debt-to-equity ratio, and thus creditors may simply charge a higher loan spread for a worse debt-to-equity ratio. To rule out this alternative explanation, I control for liquidity in difference-in-differences analyses since this explanation likely depends on the liquidity of the shares held by implicated institutions. The results still hold after controlling for Amihud's illiquidity measure which

is the daily ratio of absolute stock return to its dollar volume, averaged over the fiscal year.

VIII. Summary and Conclusion

In this paper, we investigate how shareholder financial difficulties affect firms' risk-shifting behavior. We argue that financial difficulties of tainted institutions incentivize them to influence their portfolio firms to engage in aggressive risk-shifting and lenders, anticipating such increased risk-shifting behaviors, charge higher loan spreads and impose more stringent covenants to these firms. Consistent with this argument, using the 2003 mutual fund scandal that leads to substantial fund outflows of tainted institutions and declines in their value as an exogenous shock to shareholders' financial conditions, we find that firms held by tainted block institutions experience a large increase in loan spreads and stricter covenant requirements in the post-scandal period, particularly when the scandal is severer. We further find that the adverse effects of shareholder scandals on loan contract terms are evident only when tainted institutions hold the stocks of affected firms more than 3 years before the scandal announcement date, suggesting that they can exploit their connections with portfolio firms built upon their longer investment horizon to engage in risk-shifting activities for the purpose of quick recovery. The results are also more pronounced when firms have greater shareholder-debtholder conflicts, poorer governance, and higher information asymmetry. Thus, firms' adoption of debtholder-friendly policies, high quality governance, and transparent information environment helps alleviate lenders' concerns about the adverse effect of shareholder financial difficulties on firms' risk-shifting behaviors.

In additional analyses, we find that bondholders (shareholders) experience negative and significant (insignificant) CARs (0, 1) around the scandal announcements date and that the bond CARs are negatively and significantly correlated with stock CARs. These results suggest that the scandal that imposes significant financial difficulties to shareholders indeed increase shareholders' risk-shifting incentives and thus exacerbate potential wealth transfers from debtholders to shareholders.

Turning to firms' post-scandal risk-taking behaviors, we find that firms held by tainted institutions suffering more from the scandal significantly increase their leverage and payouts to shareholders and invest more after the scandal. These results further support the view that the financial difficulties of large shareholders have a significant effect on firms' risk-shifting. The results also suggest that contracts that completely eliminate shareholder-debtholder conflicts are costly and incomplete.

Overall, our findings provide strong evidence on the importance of shareholder financial difficulties in firms' risk-shifting and add to the literature on asset substitution that is short of evidence on the role of shareholder characteristics in 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 misconducts by investment advisors have an impact on various aspects of the economy, not only on the financial advisory industry and fund management firms but also on risk-taking behaviors of investment advisors' portfolio firms and debtholders' loan contract designs.

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Figure 1
Cumulative Monthly Percentage Net Inflows of Implicated and Non-implicated fund families

This figure presents the mean of cumulative monthly percentage net inflows of implicated and non-implicated fund families from the scandal announcement month to the subsequent three years. Monthly percentage net inflow of fund i in month t is calculated as

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

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 . The mean of cumulative monthly percentage net inflows for implicated (non-implicated) fund families is weighted by each family's total net assets.

Figure 2
Pre-trend Analysis of Loan spreads around the Scandal

This figure assesses the validity of the difference-in-differences test by examining how the loan spreads of treatment firms at year $t-1$ (year $t+n$) are different from those of treatment and control firms at year $t-2$ and year $t-3$, where year t is the scandal year. The sample consists of 1,119 firm-loan observations (556 loans issued by 112 treatment firms and 563 loans issued by 112 control firms) from 2000 to 2007. Treatment (control) firms are firms held (not held) by tainted block institutions in the year immediately before the scandal year (i.e., the year 2003 or 2004 in which regulatory agencies charge mutual fund families for their illegal late trading and market timing practice). Each treatment firm is matched to a control firm using a propensity-score matching approach without replacement and a caliper of 0.1. 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, ROA, stock performance, book leverage, Tobin's q , tangibility, cash flow volatility, institutional ownership, and R&D expenses scaled by total assets. We require the treatment and control firms to be in the same industry (the same two-digit SIC codes) and in the same fiscal year and to have at least one loan issued both three years before and three years after the scandal announcement date. We estimate the following regression:

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

where $\text{Before}_{i,t}^{-1}$ is an indicator that takes the value of one for treatment firms at year $t-1$, and zero otherwise and $\text{After}_{i,t}^{+n}$ 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, loan types, and loan purposes, respectively. Standard errors are adjusted for heteroskedasticity and clustered at the firm level. Squares connected by blue line and spikes connected by red line represent the point estimates and the 95% confidence intervals of coefficients β_n , respectively.

Table 1
List of Mutual Fund Families That are Involved in the 2003 Mutual Fund Scandal

This table presents the list of 25 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 implicated from various sources, including *Factiva* and administrative proceedings by the Securities and Exchange Commission (SEC). We search the information on implicated mutual fund families' management companies and ultimate parent firms, if any, from their websites and 10-K filings. The information on asset under management (AUM), civil penalty, and disgorgement are obtained from McCabe (2009). When the information is missing, we manually collect it from the SEC administrative proceedings. AUM is measured on August 30, 2003 and excludes money market funds.

Fund family	Management Companies	Ultimate Parent firm	Initial public reporting date	AUM (\$ billions)	Civil penalty (\$ millions)	Disgorgement (\$ millions)
Alliance Bernstein	Alliance Capital	AXA Financial	2003-09-30	51.8	100.0	150.0
Columbia	Columbia Mgmt.	FleetBoston Financial	2004-01-15	49.1	70.0	70.0
Evergreen	Evergreen Inv. Mgmt.	Wachovia	2004-08-04	46.2	4.0	29.0
Excelsior	US Trust	Charles Schwab	2003-11-14		0.4	0.0
Federated	Federated Investors	Federated Investors	2003-10-22	47.8	45.0	27.0
Franklin Templeton	Franklin Advisers	Franklin Resources	2003-09-03	124.4	25.0	30.0
Fred Alger	Fred Alger Mgmt.	(Private)	2003-10-03	3.1	10.0	30.0
Fremont	Fremont Inv. Advisors	(Private)	2003-11-24	2.9	2.0	2.0
Gabelli	Gabelli Funds	Gabelli Asset Mgmt.	2003-09-03		5.0	11.0
Heartland	Heartland Advisors	(Private)	2003-12-11		3.8	0.0
ING	ING Investments	ING Groep NV	2004-03-11		1.5	1.4
Invesco	Invesco Funds Group	Amvescap PLC	2003-12-02	76.1	142.0	235.0
Janus	Janus Capital Mgmt.	Janus Capital Group	2003-09-03	98.3	51.0	82.0
Loomis Sayles	Loomis Sayles & Co.	CDC Asset Mgmt.	2003-11-13			
MFS	MFS Inv. Mgmt.	Sun Life Financial	2003-12-09	73.1	50.0	175.0
Nations	Banc of America Capital Mgmt.	Bank of America	2003-09-03	30.3	125.0	250.0
One Group	Banc One Inv. Advisors	Bank One	2003-09-03	40.6	40.0	10.0
PBHG Funds	Pilgrim Baxter & Associates	Old Mutual PLC	2003-11-13	6.4	50.0	40.0
PIMCO	PIMCO Advisors	Allianz Group	2004-02-13	132.3	58.0	10.0
Putnam	Putnam Inv. Mgmt.	Marsh & McLennan	2003-09-19	143.6	100.0	54.0
RS Investments	RS Inv. Mgmt.	(Private)	2004-03-03	4.1	14.0	12.0

Scudder	Deutsche Inv. Mgmt. Americas	Deutsche Bank AG	2004-01-23	61.0	26.0	103.0
Seligman	J&W Seligman & Co.	(Private)	2004-01-07	9.1	0.0	6.0
Strong	Strong Capital Mgmt.	(Private)	2003-09-03	24.8	40.0	40.0
Waddell & Reed	Waddell & Reed Inv. Mgmt.	Waddell & Reed Financial	2003-11-12	19.4	12.0	40.0

Table 2
Summary Statistics

Panel A of this table presents summary statistics for a sample of 756 firm-year observations (*affected firms*) that are 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) and 4,227 firm-year observations (*non-affected firms*) that are not held by tainted block institutions in the same year. Panel B shows the characteristics of 378 loans issued by *affected firms* and 1,858 loans issued by *non-affected firms* in the year immediately prior to the scandal year. Part C presents descriptive statistics of a propensity score matched sample of 112 treatment firms (firms that are held by tainted block institutions in the year immediately before the scandal) and 112 control firms (firms that are not held by tainted block institutions in the same year). Each treatment firm is matched to a control firm using a propensity score matching approach without replacement and a caliper of 0.1. 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, ROA, stock performance, book leverage, Tobin's *q*, tangibility, cash flow volatility, institutional ownership, and R&D expenses scaled by total assets. We require the treatment and control firms to be in the same industry (the same two-digit SIC codes) and in the same fiscal year. We also require that the treatment and control firms have at least one loan issued both three years before and three years after the scandal announcement date. All variables are measured at the beginning of the fiscal year and all continuous variables are winsorized at the 1st and 99th percentiles. Appendix provides a detailed description of the variables. ***, **, and * denote that *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 characteristics

Variable	Affected firms (N=756): A		Non-affected firms (N=4,227): B		Test of difference (A-B): <i>p</i> -value	
	Mean	Median	Mean	Median	<i>T</i> -test	Wilcoxon <i>z</i> -test
Log (total assets)	6.353	6.293	5.817	5.722	0.000***	0.000***
ROA	-0.022	0.026	-0.049	0.022	0.002***	0.014**
Stock performance	0.152	0.047	0.249	0.075	0.002***	0.119
Book leverage	0.232	0.207	0.196	0.153	0.000***	0.000***
Tobin's <i>q</i>	1.735	1.369	1.886	1.464	0.003***	0.002***
Tangibility	0.261	0.206	0.252	0.179	0.329	0.030**
Cash flow volatility	0.017	0.011	0.023	0.014	0.000***	0.000***
Institutional ownership	0.659	0.698	0.540	0.564	0.000***	0.000***
R&D expenses	0.041	0.000	0.061	0.007	0.000***	0.000***

Panel B: Loan characteristics

Variable	Loans issued by Affected firms (N=378): A		Loans issued by Non-affected firms (N=1,858): B		Test of difference (A-B): <i>p</i> -value	
	Mean	Median	Mean	Median	<i>T</i> -test	Wilcoxon <i>z</i> -test
Loan amount (\$ million)	244.185	147.500	221.666	100.000	0.240	0.000***
Loan maturity (month)	38.249	36.000	37.154	36.000	0.356	0.720
Loan spread (basis point)	193.490	175.000	219.037	207.500	0.001***	0.001***
Secured (indicator)	0.762	1.000	0.787	1.000	0.361	0.361
Covenant strictness index	4.398	5.000	4.513	5.000	0.223	0.055*

Panel C: Descriptive statistics of a propensity score matched sample

Variable	Treatment firms (N=112): A		Control firms (N=112): B		Test of difference (A-B): <i>p</i> -value	
	Mean	Median	Mean	Median	<i>T</i> -test	Wilcoxon <i>z</i> -test
Log (total assets)	7.004	6.983	7.036	7.023	0.859	0.838
ROA	0.017	0.040	0.029	0.039	0.408	0.801
Stock performance	0.099	0.085	0.212	0.122	0.121	0.941
Book leverage	0.284	0.261	0.291	0.270	0.781	0.707
Tobin's <i>q</i>	1.574	1.313	1.511	1.392	0.539	0.866
Tangibility	0.290	0.252	0.249	0.196	0.136	0.056*
Cash flow volatility	0.012	0.009	0.010	0.008	0.245	0.318
Institutional ownership	0.699	0.701	0.682	0.713	0.478	0.391
R&D expenses	0.014	0.000	0.016	0.000	0.622	0.796

Table 3
Effect of Shareholder Scandals on Loan Contract Terms

This table presents estimates from difference-in-differences regressions of loan contract terms in a propensity score matched sample. The sample consists of 1,119 firm-loan observations (556 loans issued by 112 treatment firms and 563 loans issued by 112 control firms) from 2000 to 2007. Treatment firms are firms 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 practice) and control firms are firms that are not held by tainted block institutions in the same year. Each treatment firm is matched to a control firm using a propensity score matching approach without replacement and a caliper of 0.1. 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, ROA, stock performance, book leverage, Tobin's *q*, tangibility, cash flow volatility, institutional ownership, and R&D expenses scaled by total assets. We require the treatment and control firms to be in the same industry (the same two-digit SIC codes) and in the same fiscal year. We also require that the treatment and control firms have at least one loan issued both three years before and three years after the scandal announcement date. The dependent variable in column (1) 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 dependent variable in column (2) is the covenant strictness index computed as the sum of the following five covenant indicators: dividend restriction, the existence of more than two financial covenants, asset sale sweep, equity issuance sweep, and debt issuance sweep. *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 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 control variables are measured at the beginning of the fiscal year and all continuous variables are winsorized at the 1st and 99th percentiles. 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.

Independent variable	Log (loan spread) (1)	Covenant strictness index (2)
Treatment (indicator) $\times$ Post (indicator)	0.123** (0.047)	0.351* (0.065)
Firm characteristics		
Log (total assets)	-0.079 (0.237)	-0.426** (0.031)
ROA	-1.224*** (0.000)	-1.702** (0.012)
Stock performance	-0.047* (0.055)	-0.293** (0.016)
Book leverage	0.398** (0.023)	0.646 (0.240)
Tobin's <i>q</i>	-0.063 (0.108)	0.404 (0.101)
Tangibility	-0.289 (0.491)	0.704 (0.615)
Cash flow volatility	-5.209** (0.032)	-12.992 (0.114)
Institutional ownership	0.065 (0.694)	-0.600 (0.361)
R&D expenses	3.667** (0.036)	-1.088 (0.642)
Credit rating (indicator)	0.116* (0.087)	-0.097 (0.575)
Non-investment grade rating (indicator)	0.336*** (0.002)	0.128 (0.600)
Loan characteristics		
Log (loan maturity)	0.033 (0.278)	0.071 (0.465)
Log (loan amount)	-0.005 (0.831)	-0.027 (0.354)
Macroeconomic variables		
Credit spread	-0.254	0.065

	(0.117)	(0.881)
Term spread	0.132***	0.114
	(0.001)	(0.505)
Loan purpose fixed effects	Yes	Yes
Loan type fixed effects	Yes	Yes
Firm fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Adjusted R^2	0.829	0.874
Number of observations	1,119	253

Table 4
Effect of Shareholder Scandals on Loan Contract Terms by the Severity of Scandals

This table presents difference-in-differences regressions of loan contract terms by the severity of shareholder scandals. The sample consists of 556 loans issued by 112 treatment firms and 563 loans issued by 112 control firms from 2000 to 2007. Treatment firms are firms 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 practice) and control firms are firms that are not held by tainted block institutions in the same year. Each treatment firm is matched to a control firm using a propensity score matching approach without replacement and a caliper of 0.1. 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, ROA, stock performance, book leverage, Tobin's *q*, tangibility, cash flow volatility, institutional ownership, and R&D expenses scaled by total assets. We require the treatment and control firms to be in the same industry (the same two-digit SIC codes) and in the same fiscal year. We also require that the treatment and control firms have at least one loan issued both three years before and three years after the scandal announcement date. The dependent variable in columns (1)-(3) 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 dependent variable in columns (4)-(6) is the covenant strictness index computed as the sum of the following five covenant indicators: dividend restriction, the existence of more than two financial covenants, asset sale sweep, equity issuance sweep, and debt issuance sweep. Institution CAR (-1, 1) is the cumulative abnormal return estimated using the market model 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. Civil penalty and disgorgement are obtained from SEC administrative proceedings. *High (Low) severity* is an indicator that takes the value of one if the value of severity, measured by the institution CAR (-1, 1), is below (above) its sample median, and zero otherwise. For civil penalty and disgorgement, *High (Low) severity* takes the value of one if their value is above (below) its sample median, 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 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. 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.

Independent variable	Log (loan spread)			Covenant strictness index		
	Institution CAR (-1, 1)	Civil penalty	Disgorge- ment	Institution CAR (-1, 1)	Civil penalty	Disgorge- ment
	(1)	(2)	(3)	(4)	(5)	(6)
High severity (indicator) $\times$ Post (indicator): a	0.227*** (0.008)	0.174** (0.024)	0.185** (0.022)	1.967*** (0.000)	0.617** (0.026)	0.582** (0.030)
Low severity (indicator) $\times$ Post (indicator): b	0.044 (0.565)	0.030 (0.698)	0.028 (0.712)	0.135 (0.551)	0.124 (0.606)	0.156 (0.500)
Control variables (same as in Table 3)	Yes	Yes	Yes	Yes	Yes	Yes
<i>P</i> -value for the test of $a = b$	0.047	0.132	0.114	0.000	0.180	0.220
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	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.831	0.830	0.830	0.897	0.877	0.876
Number of observations	1,052	1,119	1,119	232	253	253

Table 5
Effect of Shareholder Scandals on Loan Contract Terms by Shareholder Characteristics

This table presents difference-in-differences regressions of loan contract terms by shareholder characteristics. The sample consists of 556 loans issued by 112 treatment firms and 563 loans issued by 112 control firms from 2000 to 2007. Treatment firms are firms 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 practice) and control firms are firms that are not held by tainted block institutions in the same year. Each treatment firm is matched to a control firm using a propensity score matching approach without replacement and a caliper of 0.1. 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, ROA, stock performance, book leverage, Tobin's *q*, tangibility, cash flow volatility, institutional ownership, and R&D expenses scaled by total assets. We require the treatment and control firms to be in the same industry (the same two-digit SIC codes) and in the same fiscal year. We also require that the treatment and control firms have at least one loan issued both three years before and three years after the scandal announcement date. The dependent variable in column (1) 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 dependent variable in column (2) is the covenant strictness index computed as the sum of the following five covenant indicators: dividend restriction, the existence of more than two financial covenants, asset sale sweep, equity issuance sweep, and debt issuance sweep. *Prior holding longer (shorter) than 3 years* is an indicator that takes the value of one if the tainted institution has been a shareholder or blockholder of the treatment firm for more (less) than 3 years prior to the scandal announcement date 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 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. 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.

Independent variable	Log (loan spread)		Covenant strictness index	
	Holding	Blockholding	Holding	Blockholding
	(1)	(2)	(3)	(4)
Prior holding longer than 3 years * post (indicator): a	0.160** (0.029)	0.367*** (0.001)	1.170*** (0.000)	1.045*** (0.002)
Prior holding shorter than 3 years * post (indicator): b	0.080 (0.339)	0.064 (0.321)	0.119 (0.513)	0.176 (0.346)
Control variables	Yes	Yes	Yes	Yes
P-value for the test of a=b	0.393	0.006	0.002	0.014
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	Yes	Yes
Adjusted/Pseudo R-square	0.829	0.832	0.889	0.881
Number of observations	1,119	1,119	253	253

Table 6
Effect of Shareholder Scandals on Loan Contract Terms by Shareholder-Debtholder Conflicts

This table presents difference-in-differences regressions of loan contract terms by firms' shareholder-debtholder conflicts. The sample consists of 556 loans issued by 112 treatment firms and 563 loans issued by 112 control firms from 2000 to 2007. Treatment firms are firms 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 their illegal late trading and market timing practice) and control firms are firms that are not held by tainted block institutions in the same year. Each treatment firm is matched to a control firm using a propensity score matching approach without replacement and a caliper of 0.1. 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, ROA, stock performance, book leverage, Tobin's *q*, tangibility, cash flow volatility, institutional ownership, and R&D expenses scaled by total assets. We require the treatment and control firms to be in the same industry (the same two-digit SIC codes) and in the same fiscal year. We also require that the treatment and control firms have at least one loan issued both three years before and three years after the scandal announcement date. The dependent variable in columns (1)-(3) 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 dependent variable in columns (4)-(6) is the covenant strictness index computed as the sum of the following five covenant indicators: dividend restriction, the existence of more than two financial covenants, asset sale sweep, equity issuance sweep, and debt issuance sweep. *Payout* is the sum of dividends and share repurchases scaled by total assets. *Investment inefficiency* is the residual estimated from a regression of total investments on lagged sales growth for each industry-year based on the Fama and French 48-industry classification (Biddle, Hilary, and Verdi, 2009). *IVOL* (stock return idiosyncratic volatility) is the standard deviation of residuals estimated from the regression of daily excess returns on Fama-French three factors over the fiscal year. *More (Less) conflict* is an indicator that takes the value of one if the value of *Payout*, *Investment inefficiency*, or *IVOL* is above (below) its sample median, 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 the pre-scandal period (year $t-1$, year $t-2$, year $t-3$), where year t is the scandal year. *More (less) conflict* and all control variables are measured at the beginning of the fiscal year and all continuous variables are winsorized at the 1st and 99th percentiles. 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.

Independent variable	Log (loan spread)			Covenant strictness index		
	Payout	Investment inefficiency	IVOL	Payout	Investment inefficiency	IVOL
	(1)	(2)	(3)	(4)	(5)	(6)
More conflict (indicator) $\times$ Post (indicator): a	0.157**	0.258***	0.201**	0.709***	0.899**	0.485**
	(0.048)	(0.008)	(0.013)	(0.002)	(0.023)	(0.013)
Less conflict (indicator) $\times$ Post (indicator): b	0.125	0.045	0.058	0.124	0.518	-0.010
	(0.121)	(0.480)	(0.385)	(0.592)	(0.109)	(0.966)
Control variables (same as in Table 3)	Yes	Yes	Yes	Yes	Yes	Yes
<i>P</i> -value for the test of a = b	0.704	0.008	0.071	0.064	0.362	0.032
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	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.829	0.827	0.830	0.917	0.906	0.880
Number of observations	1,033	952	1,119	218	190	253

Table 7
Effect of Shareholder Scandals on Loan Contract Terms by Governance Quality

This table presents difference-in-differences regressions of loan contract terms by firms' governance quality. The sample consists of 556 loans issued by 112 treatment firms and 563 loans issued by 112 control firms from 2000 to 2007. Treatment firms are firms 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 practice) and control firms are firms that are not held by tainted block institutions in the same year. Each treatment firm is matched to a control firm using a propensity score matching approach without replacement and a caliper of 0.1. 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, ROA, stock performance, book leverage, Tobin's *q*, tangibility, cash flow volatility, institutional ownership, and R&D expenses scaled by total assets. We require the treatment and control firms to be in the same industry (the same two-digit SIC codes) and in the same fiscal year. We also require that the treatment and control firms have at least one loan issued both three years before and three years after the scandal announcement date. The dependent variable in columns (1)-(3) 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 dependent variable in columns (4)-(6) is the covenant strictness index computed as the sum of the following five covenant indicators: dividend restriction, the existence of more than two financial covenants, asset sale sweep, equity issuance sweep, and debt issuance sweep. Proportion of outside directors is the ratio of the number of outside directors to the total number of directors on the board. CEO-Chair duality is an indicator that takes the value of one if the CEO is also the chair of the board, and zero otherwise. HHI (Herfindahl-Hirschman Index) is the sum of the squared market share (measured as sales) of each firm in each two-digit-SIC industry and in each year. *Poor (Good) governance* is an indicator that takes the value of one if the CEO is (is not) the chair of the board or the value of the proportion of outside directors or HHI is below (above) its sample median, 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 the pre-scandal period (year $t-1$, year $t-2$, year $t-3$), where year t is the scandal year. *Poor (good) governance* and all control variables are measured at the beginning of the fiscal year and all continuous variables are winsorized at the 1st and 99th percentiles. 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.

Independent variable	Log (loan spread)			Covenant strictness index		
	Proportion of outside directors	CEO-Chair duality	HHI	Proportion of outside directors	CEO-Chair duality	HHI
	(1)	(2)	(3)	(4)	(5)	(6)
Poor governance (indicator) × Post (indicator): a	0.168*	0.147**	0.136*	0.752***	0.345*	0.530*
	(0.063)	(0.044)	(0.058)	(0.005)	(0.079)	(0.072)
Good governance (indicator) × Post (indicator): b	0.107*	0.108	0.110	0.128	0.405	0.336*
	(0.088)	(0.180)	(0.175)	(0.573)	(0.362)	(0.088)
Control variables (same as in Table 3)	Yes	Yes	Yes	Yes	Yes	Yes
<i>P</i> -value for the test of a = b	0.521	0.666	0.765	0.045	0.894	0.427
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	Yes	Yes	Yes	Yes	Yes
Adjusted <i>R</i> ²	0.833	0.831	0.829	0.881	0.873	0.874
Number of observations	1,108	1,111	1,119	253	253	253

Table 8
Effect of Shareholder Scandal on Loan Contract Terms by Information Asymmetry

This table presents difference-in-differences regressions of loan contract terms by firms' information asymmetry. The sample consists of 556 loans issued by 112 treatment firms and 563 loans issued by 112 control firms from 2000 to 2007. Treatment firms are firms 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 practice) and control firms are firms that are not held by tainted block institutions in the same year. Each treatment firm is matched to a control firm using a propensity score matching approach without replacement and a caliper of 0.1. 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, ROA, stock performance, book leverage, Tobin's *q*, tangibility, cash flow volatility, institutional ownership, and R&D expenses scaled by total assets. We require the treatment and control firms to be in the same industry (the same two-digit SIC codes) and in the same fiscal year. We also require that the treatment and control firms have at least one loan issued both three years before and three years after the scandal announcement date. The dependent variable in columns (1)-(3) 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 dependent variable in columns (4)-(6) is the covenant strictness index computed as the sum of the following five covenant indicators: dividend restriction, the existence of more than two financial covenants, asset sale sweep, equity issuance sweep, and debt issuance sweep. Firm size is the natural logarithm of total assets. Analyst coverage is the number of unique analysts following the firm. Absolute discretionary accrual is the absolute value of discretionary current accruals computed using the modified Jones (1991) model. *High (low) information asymmetry* is an indicator that takes the value of one if the value of information asymmetry, measured by firm size, analyst coverage, or absolute discretionary accruals, is above (below) its sample median, 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 the pre-scandal period (year $t-1$, year $t-2$, year $t-3$), where year t is the scandal year. *High (low) information asymmetry* and all control variables are measured at the beginning of the fiscal year and all continuous variables are winsorized at the 1st and 99th percentiles. 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.

Independent variable	Log (loan spread)			Covenant strictness index		
	Firm size	Analyst coverage	Absolute discretionary accrual	Firm size	Analyst coverage	Absolute discretionary accrual
	(1)	(2)	(3)	(4)	(5)	(6)
High information asymmetry (indicator) × Post (indicator): a	0.199**	0.188**	0.140*	0.477**	0.358*	0.394**
	(0.030)	(0.027)	(0.071)	(0.020)	(0.070)	(0.042)
Low information asymmetry (indicator) × Post (indicator): b	0.067	0.063	0.117	0.378	0.298	0.272
	(0.277)	(0.328)	(0.113)	(0.141)	(0.422)	(0.219)
Control variables (same as in Table 3)	Yes	Yes	Yes	Yes	Yes	Yes
<i>P</i> -value for the test of a = b	0.141	0.139	0.779	0.682	0.873	0.472
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	Yes	Yes	Yes	Yes	Yes
Adjusted <i>R</i> ²	0.830	0.830	0.829	0.866	0.873	0.872
Number of observations	1,119	1,119	1,093	253	253	248

Table 9
Bond and Stock Returns around the Scandal Announcements and Their Correlations

This table presents the mean and median cumulative abnormal returns (CARs) around the scandal announcement date for bonds and stocks of 81 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 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. In Panels A and B, 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 two months prior to the scandal announcement month to the total market value of all bonds outstanding for a firm at the same time. Stock abnormal returns are calculated using the Fama-French-Carhart four-factor model (Carhart, 1997) using 220 trading days of returns beginning 280 days before and ending 61 days before the scandal announcement date. 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: Univariate results of bond and stock CARs

	N	Bond CARs		Stock CARs	
		Mean	Median	Mean	Median
CAR (-1, 1)	81	-0.004	-0.000	0.000	-0.002
CAR (-1, 0)	67	0.001	-0.000	0.003	0.000
CAR (0, 1)	68	-0.008***	-0.003***	-0.001	0.000

Panel B: Pearson correlation coefficients between bond and stock CARs

Bond CARs	Stock CARs		
	CAR (-1, 1)	CAR (-1, 0)	CAR (0, 1)
CAR (-1, 1)	-0.132	-0.278***	-0.135
CAR (-1, 0)	0.104	0.091	0.085
CAR (0, 1)	-0.231**	-0.435***	-0.266**

Table 10
Effect of Shareholder Scandals on Firms' Post-Scandal Risk-Taking Behaviors

This table presents difference-in-differences regressions of firms' risk-taking behaviors in the post-scandal period. The sample consists of 643 firm-year observations of 112 treatment firms and 650 firm-year observations of 112 control firms from 2000 to 2007. Treatment firms are firms 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 practice) and control firms are firms that are not held by tainted block institutions in the same year. Each treatment firm is matched to a control firm using a propensity score matching approach without replacement and a caliper of 0.1. 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, ROA, stock performance, book leverage, Tobin's *q*, tangibility, cash flow volatility, institutional ownership, and R&D expenses scaled by total assets. We require the treatment and control firms to be in the same industry (the same two-digit SIC codes) and in the same fiscal year. The dependent variables in columns (1)-(6) are the ratios of total debt, capital expenditure, R&D expense, acquisition expenditure, total investment (the sum of capital expenditure, R&D expense, and acquisition expenditure), and operational risk (the volatility of EBITDA scaled by the average EBITDA in a sixteen-quarter window). *High (Low) severity* is an indicator that takes the value of one if the value of severity, measured by the institution CAR (-1, 1), is below (above) its sample median, and zero otherwise. Institution CAR (-1, 1) is the cumulative abnormal return estimated using the market model 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. Civil penalty and disgorgement are obtained from SEC administrative proceedings. *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 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. 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.

	Book leverage	Capital expenditure	R&D expense	Acquisition expenditure	Total investment	Operational risk
	(1)	(2)	(3)	(4)	(5)	(6)
High severity (indicator) $\times$ Post (indicator): a	0.035***	0.010*	0.001	0.041**	0.051**	0.108*
	(0.002)	(0.087)	(0.516)	(0.045)	(0.021)	(0.067)
Low severity (indicator) $\times$ Post (indicator): b	0.011	0.007	0.001	-0.003	0.006	0.029
	(0.361)	(0.296)	(0.418)	(0.899)	(0.772)	(0.697)
Control variables (same as in Table 3)	Yes	Yes	Yes	Yes	Yes	Yes
<i>P</i> -value for the test of a = b	0.054	0.751	0.818	0.036	0.057	0.226
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted <i>R</i> ²	0.869	0.710	0.936	0.187	0.292	0.408
Number of observations	1,209	1,203	1,209	1,147	1,144	1,198

Appendix

The Appendix provides detailed descriptions of all variables used in the tables.

Variable	Definition	Source
Absolute discretionary accrual	Absolute value of discretionary current accrual computed using the modified Jones (1991) model	Compustat
Acquisition expenditure	Acquisition expenditures (<i>aqc</i>) / total assets (<i>at</i>)	Compustat
Analyst coverage	Number of unique analysts following a firm in a given year	I/B/E/S
Book leverage	Total debts (<i>dltt</i> + <i>dlc</i>) divided by total assets (<i>at</i>)	Compustat
Capital expenditure	Capital expenditures (<i>capex</i>) / total assets (<i>at</i>)	Compustat
Institution CAR (-1, 1)	Cumulative abnormal return estimated using the market model 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
Cash flow volatility	Standard deviation of the quarterly operating income before depreciation over the past three years (<i>oibdpq</i>) divided by total assets (<i>at</i>). We require at least four non-missing quarterly cash flows	Compustat
CEO-Chair duality (indicator)	One if the CEO is also the chair of the board, and zero otherwise	BoardEx
Civil penalty	Total dollar amount imposed by regulatory agencies against mutual fund management companies for their illegal trading activities discovered in 2003 or 2004	SEC Administrative Proceedings
Covenant strictness index	Sum of the following five covenant indicators: dividend restriction, the existence of more than two financial covenants, asset sales sweep, equity issuance sweep, and debt issuance sweep	DealScan
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 Board
Disgorgement	Total dollar amount that mutual fund management companies gain as a result of illegal trading activities discovered in 2003 or 2004, which is ordered by regulatory agencies to be paid back	SEC Administrative Proceedings
Firm size	Natural logarithm of total assets (<i>at</i>)	Compustat
HHI	Sum of the squared market share (measured as sales) of each firm in each two-digit-SIC industry and in each year	Compustat
High (Low) information asymmetry (indicator)	One if the value of information asymmetry, measured by firm size, analyst coverage, or absolute discretionary accrual, is above (below) its sample median, and zero otherwise	Compustat, I/B/E/S
High (Low) severity (indicator)	One if the value of severity, measured by the CAR (-1, 1), is below (above) its sample median, and zero otherwise. For civil penalty and disgorgement, <i>High (Low) severity</i> takes the value of one if their value is above (below) its sample median, and zero otherwise	CRSP, SEC Administrative Proceedings
Institutional ownership	Number of shares held by institutional investors divided by total shares outstanding	Thompson13F
Investment inefficiency	Residual estimated from a regression of total investments on lagged sales growth for each industry-year based on the Fama and French 48-industry classification (Biddle, Hilary, and Verdi, 2009)	Compustat
IVOL	Standard deviation of residuals estimated from regression of daily excess returns on Fama-French three factors in a given fiscal year	CRSP
Loan purpose (indicator)	Indicators for loan purpose (corporate purposes, working capital, debt repayment, acquisition, 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 of dollars	DealScan
Log (loan maturity)	Natural logarithm of loan maturity measured in months	DealScan
More (Less) conflict (indicator)	One if the value of <i>Payout</i> , <i>Investment inefficiency</i> , or <i>IVOL</i> is above (below) its sample median, and zero otherwise	Compustat
Non-investment grade rating (indicator)	One if a firm's S&P long-term credit rating is below BBB, and zero otherwise	Compustat
Operational risk	The volatility of EBITDA scaled by the average EBITDA is a sixteen-quarter window	Compustat
Payout	Sum of dividends (<i>dvc</i> + <i>dvp</i>) and share repurchases (<i>prstk</i>), scaled by total assets (<i>at</i>)	Compustat
Poor (Good) governance (indicator)	One if the CEO is (is not) the chair of the board or the value of the proportion of outside directors or HHI is below (above) its sample median, and zero otherwise	Compustat, BoardEx
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 the pre-scandal period (year $t-1$, year $t-2$, year $t-3$), where year t is the scandal year	
Prior holding longer (shorter) than 3 years (indicator)	One if the tainted institution has been a shareholder or blockholder of the treatment firm for more (less) than 3 years prior to the scandal announcement date and zero otherwise	
Proportion of outside directors	Ratio of the number of outside directors to the total number of directors on the board	BoardEx
ROA	Net income (<i>ni</i>) divided by total assets (<i>at</i>)	Compustat
R&D expense	R&D expenses (<i>xrd</i>) divided by total assets (<i>at</i>). This variable is set to zero if R&D expense is missing	Compustat
Secured (indicator)	One if the bank loan is secured by collateral, and zero otherwise	DealScan
Stock performance	Buy-and-hold return for the year net of the CRSP value-weighted index return	CRSP
Tangibility	Net property, plant, and equipment (<i>ppent</i>) divided by total assets (<i>at</i>)	Compustat
Term spread	Difference in yields on ten-year and one-year treasury bonds	Federal Reserve Board
Tobin's q	[Total assets (<i>at</i>) – common/ordinary equity (<i>ceq</i>) + market value of equity ($prcc_f \times csho$)] / total assets (<i>at</i>)	Compustat
Total investment	Sum of capital expenditures (<i>capx</i>), R&D expenses (<i>xrd</i>), and acquisition expenditures (<i>aqc</i>), scaled by total assets (<i>at</i>)	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	

Chapter 2

Cyber Risk and Insider Trading

Abstract

We investigate whether cyberattacks affect managerial incentives and behaviors by studying the profitability of insider trading. We find that insiders who trade shares 90 days before the disclosure of cyberattacks earn significantly higher profits. The effect is more pronounced when firms have higher information asymmetry, poorer governance, and insufficient risk management systems, and when firms are headquartered in states with an effective data breach notification law. We also find that insiders at industry competitors of attacked firms earn higher profits than those at other firms prior to the disclosure of cyberattacks, particularly they possess information advantages through social connections with board members in attacked firms.

Keywords: Cyber risk, Hacking, Insider trading, Competitor

JEL Classification: G14, G32, G38

I. Introduction

While recent studies have examined how cyber risk, relatively new and emerging operational risk, has affected a firm's disclosure policy (Amir, Levi, and Livne, 2018; Lending, Minnick, and Schorno, 2018), CSR investment (Akey, Lewellen, and Liskovich, 2018), risk management and compensation (Kamiya et al., 2020), little is known about whether and how cyber risk affects insiders' incentives and behaviors. In this paper, we investigate this issue by examining insider trading activities of firms experiencing cyberattacks.

It is ex-ante unclear whether cyberattacks prompt insider trading. On the one hand, we expect insiders have strong incentives to exploit their privileged information about the extent of potential damage to make abnormal returns before the news on cyberattacks is publicly disclosed. Although data breach disclosures are mandated, companies have discretion to disclose the details of events. The lack of details in disclosure policy provides insiders with greater incentives to sell their shares to avoid potential personal wealth losses. Supporting this argument, Amir, Levi, and Livne (2018) find that firms tend to underreport cyberattacks especially when the potential damage caused by the incident is greater. In addition, due to the unknown, emerging nature of cyber risk, it is costly for outsiders to assess the extent of a cyberattack and a firm's ability to remediate the risk. This information asymmetry would further increase insiders' incentives to exploit their information advantages.

On the other hand, cyberattacks may not prompt insider trading. Although insiders have better access to corporate information, sufficiently high litigation cost will deter insiders from selling shares before the revelation of the news on cyberattacks. Studies show

that firms that experience cyberattacks face increasing legal risk (Romanosky, Hoffman, and Acquisti, 2014) and stringent monitoring from various stakeholders such as auditors (Li, No, and Boritz, 2016) and regulators (Rosati, Gogolin, and Lynn, 2018). With the intensified monitoring scrutiny, insiders are more likely to be constrained to engage in insider trading exploiting their private information.

To examine insider trading activities upon cyberattacks, we use a sample of 440 cyberattacks on public corporations reported in the Privacy Rights Clearinghouse (PRC) database and the Audit Analytics (AA) database over the period of 2005 to 2017. We first investigate factors that may affect insiders' trading of shares before the public disclosure of cyberattacks. We find that insiders are more likely to trade on their private information of cyberattacks if attacked firms are larger and have higher growth opportunities (i.e., lower book-to-market ratio). We also find that the likelihood of insiders' selling stocks ahead of the public disclosure of a cyberattack is lower when attacked firms have higher proportion of outside directors and firms operate in states in which mandated data breach notification laws in place. The findings suggest that firm-level governance and regulatory enforcements play a role in restricting insider trading before public revelation of cyberattacks. Insiders also tend to trade shares more when the extent of damage from a cyberattack is greater (i.e., lower cyberattack announcement returns).

Next, we examine whether insiders make abnormal returns by trading shares of attacked firms before the public revelation of the event using a propensity-score-matched sample. Following the insider trading literature (e.g., Ravina and Sapienza, 2010; Skaife, Veenman, and Waggener, 2013), we focus on the returns that insiders make from trading the company stock to measure the amount of information about market consequences of

cyberattacks possessed by insiders. Our difference-in-differences analysis shows that insiders, who trade shares 90 days before the disclosure date of cyberattacks, earn significantly higher profits of 14% in terms of market-adjusted buy-and-hold abnormal return over 180 calendar days or 11.1 basis points in terms of Carhart-alpha over 180 calendar days. We also find that insiders' trading profitability is higher when they trade shares prior to the public disclosure of cyberattacks with greater severity, measured as the number of records breached. Higher trading profitability of insiders at attacked firms is also more pronounced among a subgroup of opportunistic traders.

Focusing on the nature of cyber risk that is distinct from other operational risk (e.g., burglaries), we next explore specific circumstances under which insiders' benefits of trading exceed the costs.²⁵ We expect insiders of attacked firms to sell shares only when their net private benefits from doing so are significant. To examine this issue, we test whether insiders' trading profitability varies depending on the magnitude of insiders' net benefits measured by various firm and insider characteristics, including information environment, governance quality, risk management system, insider attributes, and legal regulations.

First, we predict that the higher profits by insiders' trading ahead of the disclosure of cyberattacks are more pronounced for firms with higher information asymmetry than those with lower information asymmetry. Because information asymmetry deters outsiders from obtaining and processing firm-specific information, insiders in firms with higher

²⁵ For example, financial information stolen by hackers does not belong to the attacked firm, but it is entrusted with the firm. Therefore, cyberattacks can result in legal penalties (third-party), reputational costs arising from dealing with customers (related-party), and a decrease in a firm's self-respect and social standing (first-party). Supporting this view, Kamiya et al. (2019) show that cyberattacks result in indirect costs such as adverse effects on industry and geography peers.

information asymmetry can take advantage of their private information more easily. Consistent with this argument, Huddart and Ke (2007) find a significantly positive relationship between abnormal returns of insiders' trading and information asymmetry measures. Moreover, given the unique and complex nature of cyberattacks, opaque information environment would further amplify insiders' information advantage about the incidents and thus increase their net benefit from engaging in trading prior to the public disclosure of cyberattacks.

Second, we examine how insider trading upon cyberattacks varies across firms with good and bad governance. Prior studies show that firm-level corporate governance systems restrict insiders' ability to profit from exploiting private information (Dai et al., 2016; Jagolinzer et al., 2011), suggesting that better-governed firms are able to restrict insiders from exploiting private information before the public disclosure of the cyberattacks. Therefore, insiders of attacked firms with poor governance quality may bear less costs from engaging in insider trading, resulting in higher net benefits. Thus, we expect insiders who trade upon cyberattacks to earn higher profits in firms with poorer governance quality.

Third, we investigate whether the presence of risk management system can mitigate insiders' incentives to exploit their information advantage about cyberattack incidents. While insiders of attacked firms have better access to firms' ability to remediate the attack, insiders' such information advantage would be reduced when firms implement rigorous risk management policy. Regarding cyber risk, a firm may hire more outside board members with IT expertise, assign a Chief Information Officer, and deploy risk committees to supervise and control cyber risk. We expect that insiders earn higher profits from trading before cyberattacks are disclosed in firms without such risk management system compared

to those with such risk management system.

Fourth, we examine whether insiders' trading profits vary depending on their personal attributes, such as IT expertise, ownership, and positions as CEO or CFO. Insiders with IT expertise may possess more privileged information since related IT knowledge helps them obtain more detailed information on cyberattacks and their potential damage. To the extent that insiders with higher ownership may suffer more wealth loss for cyberattacks, they may have stronger incentives to engage in trading prior to the disclosure of cyberattacks. Insiders serving as CEO or CFO bear more scrutiny from regulators after SOX (Wang, Shin, and Francis, 2012), suggesting that their net benefits from engaging in insider trading are lower. Thus, insiders are expected to earn higher profits prior to the disclosure of cyberattacks if they have IT expertise, they own more in the firms, and they do not serve as CEO or CFO.

Lastly, we examine how data breach disclosure regulations, specifically data breach notification laws, affect insiders' ability to exploit private information ahead of the public disclosure of the incidents. A priori, the effect of such laws on insiders' trading profitability upon cyberattacks is unclear. Mandatory disclosures may reduce insider trading if such disclosures speed up the public revelation of incidents and restrict insiders' ability to exploit their privileged information. However, since mandatory disclosure of cyberattack incidents will reveal a negative event to the market which insiders are able to anticipate, insiders have strong incentives to plan their trading accordingly to prevent potential wealth loss.²⁶

Consistent with our predictions, we find that insiders' profits are more pronounced when attacked firms are younger, have more R&D expenditures, and have higher analyst

²⁶ Supporting this argument, Chen, Hilary, and Tian (2019) find that the annual trading profit is larger after the laws become effective using the staggered adoption of data breach notification laws at the state level.

forecast dispersion. Thus, more opaque information environment is in favor of insiders to exploit their information advantage about cyberattack incidents. We also find that insiders earn higher profits in firms with relatively poorer governance quality measured by lower proportion of outsider directors, higher proportion of busy directors, the presence of dual class, and the adoption of staggered boards or poison pills, consistent with the view that better governance restricts insider from profiting from their private information. Turning to risk management system, we find that insider trading profitability is reduced significantly when attacked firms have CIOs or risk committees or have higher proportion of outside directors with IT expertise. We also find that insiders with IT expertise or higher ownership earn higher profits prior to the disclosure of cyberattacks, while insiders who serving as CEO or CFO earn less profits. Lastly, we find some evidence that insider trading profits upon cyberattacks are higher if the attacked firms are headquartered at the states in which the data breach notification laws are effective, suggesting that such legal regulations may have unintended consequences.

In addition to examining trading profitability of insiders at attacked firms, we also investigate whether cyberattacks affect insiders at industry peers of attacked firms. If insiders of peer firms learn from a cyberattack that the occurrence of the attack is more related to the target-specific factors, we would not expect cyberattacks prompt insider trading in peer firms. However, cyberattacks may provide insiders of peer firms with incentives to sell shares of peer firms when they believe that the occurrence of cyberattacks conveys the industry-wide issue, not target-specific problems. Kamiya et al., (2020) find that cyberattacks adversely affect the stock price of firms in the target firm's industry, suggesting that attacks appear to reveal adverse information that affects all firms in the

industry. Consistent with this argument, we find that insiders of peer firms also earn higher profits from trading around 90 days before the public disclosure of cyberattacks, especially when cyberattacks result in a greater number of records breached.

We further examine possible channels through which peer firms' insiders obtain information of cyberattacks that occur at their industry rivals, which allow them to profit. We focus on social connection of insiders in peer firms with target firms. Prior studies show that social networks play an important role in insider trading behavior and performance (Cao et al., 2015; Goergen, Renneboog, and Zhao, 2019). For example, Cao et al. (2015) document that independent directors socially connected to their firms' senior executives earn significantly higher returns in stock sales transactions than independent directors with no such connections. Consistent with their findings, we find that insiders of peer firms who have social ties with board members in target firms earn higher profits than those who do not have social ties.

Our paper contributes to the literature in several important ways. First, it adds to the literature on cyber risk, an emerging operation risk. Although some recent studies show that cyber risk has real impact on corporate policies (e.g., Amir, Levi, and Livne, 2018; Lending, Minnick, and Schorno, 2018; Kamiya et al., 2019), little is known about the impact of cyberattack events on insiders' incentives and how insiders react on such events. We show that insiders indeed take their private information advantage about the cyberattack events and earn significantly positive profits from engaging in trading ahead of the public disclosure of cyberattacks.

Second, our study further extends the literature on insider trading and negative events with serious consequences. Early studies document significant insider trading activities

prior to the filing of bankruptcy petitions (Gosnell, Keown, and Pinkerton, 1992; Seyhun and Bradley, 1997) and in the presence of fraudulently misstated financial statements (Summers and Sweeney, 1998). Unlike these studies, we focus on cyberattacks, an increasingly important and emerging risk. Though investment in risk management could reduce the probability of being attacked, it can never be zero (Kamiya et al., 2020). Also due to the lack of details in disclosure policy, managers have discretion to disclose cyberattack events (e.g., timing, details on the extent of the damage), which incentivizes insiders from taking advantage of such privileged information.

Among studies investigating cyberattacks and insider trading, Amir, Levi, and Livne (2019) and Chen, Hilary, and Tian (2019) are closest to our study. The former focuses on the relationship between the likelihood and frequency of insider trading and the voluntary disclosure behavior (withholding or disclosing information), while the later shows that data breach notification laws unintendedly prompt opportunistic insider trading. Our study, however, focus on trading profitability of insiders as a proxy for the amount of information insiders possess for cyber risks. Moreover, our study also highlight that cyberattacks have broader impact on the industry by showing that insiders at industry peers of attacked firms also earn significant profits.

Third, we also contribute to the literature on information transmission. Prior studies document specific channels through which insiders obtain information to make abnormal returns within a firm, such as social networks based on employment, education, family, and geography proximity (Cao et al., 2015; Ahern, 2017). We show that social connections with their industry rivals are also important channels through which insiders obtain privileged information and make profits.

The rest of this paper is organized as follows. In Section 2, we describe our sample construction and present firm characteristics. Section 3 discusses various factors affecting insiders of attacked firms to engage in insider trading. In Section 4, we examine the impact of cyberattacks on insider trading profitability. In Section 5, we investigate cross-sectional variations of insiders' trading profits depending on their net benefits. Section 6 discusses the impact of cyberattacks on trading profitability of insiders at industry competitors. Section 7 summarizes and concludes the paper.

II. Data and Sample

We obtain data on cyberattacks²⁷ from the Privacy Rights Clearinghouse (PRC) database and the Audit Analytics (AA) database²⁸ over the period of 2005 to 2017. We combine the two sets of cyberattack incidents and clean duplicated incidents covered by both databases, which results in 587 unique cyberattacks. We further require attacked firms to be listed in New York Stock Exchange, the American Stock Exchange, or Nasdaq, to have financial and stock return data available in Compustat and CRSP, and to be traded as common shares (CRSP share code = 10 or 11). These procedures yield a final sample of 440 cyberattacks experienced by 277 unique firms.

We obtain insider trading information from Thomson Financial Insiders Data Feed (IDF), which covers all trade information on insider activity as reported on SEC form 3, 4,

²⁷ Breaches such as lost or stolen breaches, insider breaches, or misplacement breaches are not included in our analyses. These breaches are more likely to be caused by firms' weak internal governance, which may create endogeneity problem on investigating the effect of such breaches on insider trading profitability.

²⁸ We obtain 321 cyberattacks that occur in publicly traded firms from the PRC database and 466 cyberattacks from the AA database. For each incident, both databases provide identifiers of the attacked firm (e.g., company name, company CIK number, etc.), the date when the cyberattack incident is disclosed to the public (i.e., disclosure date), the date when the cyberattack incident occurs (i.e., occurrence date), the date when the firm discovers the cyberattack incident (i.e., discovery date), number of records breached in the incident, and other incident-related information.

5, and 144. We focus on valid open market purchase and sale transactions of common shares made by officers and directors.²⁹ Following prior literature, we exclude transactions where shares traded exceed trading volume for that day and purge all transactions whose prices fall outside the daily trading range reported on CRSP. These transactions are likely to be private transactions or misreports of the trading date.³⁰ We also require transactions to be more than 100 shares and with trading prices more than \$2 to analyze economically meaningful transactions.

Table 1 presents summary statistics for 277 firms experiencing cyberattacks over the period of 2005 to 2017 and 7,342 firms that have the same four-digit SIC code as the attacked firm as of the fiscal year immediate before the cyberattack disclosure date. For firms experiencing multiple cyberattacks in a given fiscal year, we keep the earliest incident in that year. This filtering reduces the sample size to 385 incidents from 440 incidents. We further divide each group of firms into two subgroups according to whether the firm has at least one transaction made by a director or an officer during the *intra period*, the period from 90 days before the cyberattack disclosure date to one day before the cyberattack disclosure date. The differences between firms experiencing cyberattacks and firms experiencing no cyberattacks are consistent with prior literature (e.g., Kamiya et al., 2020). Attacked firms are likely to be larger and older, have higher Tobin's q , higher ROA and lower R&D expenses, and are less financially constrained. A further check on firms experiencing cyberattacks shows that 75.32% of such firms have insiders engaging in trading in the *intra period* compared to 60.21% in the subgroup of firms experiencing no cyberattacks. Moreover, firms with insider trading when experiencing cyberattacks are

²⁹ A valid transaction is one without a cleanse code of "A" or "S" in Thomson Financial Insiders Data Feed.

³⁰ See Jeng, Metrick, and Zeckhauser (2003) for detailed discussions about insider trading data issues.

largest and have highest Tobin's q and ROA among four types of firms. Firms with insider trading also tend to have higher past stock performance which is consistent with Lakonishok and Lee (2001) that insiders are contrarian investors who sell stock with good past performance. Turning to the comparison of insider trading in attacked and unattacked firms, we find that insiders in attacked firms sell significantly more shares in more times than those in unattacked firms. Moreover, attacked firms have more insiders engaging in selling the company stocks than unattacked firms. These findings suggest that insiders in attacked firms do take their information advantage and engage in trading ahead of the public disclosure of cyberattacks.

III. Likelihood of Insiders Trading Prior to the Public Disclosure of Cyberattacks

To investigate the determinants of insiders engaging in insider trading upon cyberattacks, we estimate probit regressions in which the dependent variable is an indicator that takes the value of one if a firm has at least one insider transaction during the *intra period* in the sample of firms experiencing cyberattacks. We include year and industry fixed effects in probit regressions to control for unobservable characteristics. We use the sample of 308 events that have the cyberattack disclosure date available.

Table 2 reports the estimation results. Column (1) reports estimate of probit regressions on various firm characteristics. The results show that the likelihood of insiders' trading upon cyberattacks is positively related to firm size and negatively related to book-to-market ratio. Several corporate governance and risk management investment measures are added in column (2). Larger board size and higher proportion of outside directors lead to lower likelihood of insiders' trading, which is consistent with literature (e.g., Dai et al.,

2016). It implies that firms with good corporate governance are able to restrict insiders from exploiting private information about cyberattack incidents. We further add two indicator variables in column (3), the existence of general counsel and whether the firm's headquarter is in a state with the adoption of the data breach notification laws. Though lack of significance, both of these two variables have negative coefficients, indicating that the existence of general counsel and state law may restrict insider trading activities ahead of the disclosure of cyberattacks. In columns (4) and (5), we add two more variables as proxies for the severity of cyberattacks, cyberattack announcement abnormal return (CAR (-1, 1)) and the natural logarithm of number of records breached. We find some weak evidence that more severe cyberattacks are (i.e., lower CAR (-1, 1) or higher number of records breached), insiders are more likely to engage in insider trading upon cyberattacks. These findings suggest that as insiders' potential wealth loss increases along with the severity of cyberattacks, their benefits from engaging in insider trading (i.e., avoiding potential wealth loss) may exceed the costs, which prompts insiders trading.

IV. Impact on Insiders' Trading Profitability

In this section, we examine insider trading profitability prior to the disclosure of cyberattacks. Following prior literature, we define the profitability of insider trading as the unrealized capital gains made from purchases and the losses avoided by selling the company stock. More specifically, we use two approaches to measure insider trading profitability. We use the six-month market-adjusted abnormal return defined as the difference between a firm's buy-and-hold return over the 180 calendar days following the transaction date and the corresponding buy-and-hold return for the market (Profit_{BHAR}),

where we use the CRSP value-weighted index as a proxy for the market portfolio.³¹ As an alternative measure of insider trading profitability, we use the intercept from the Fama-French-Carhart four-factor model (Carhart, 1997) estimated over the 180 calendar days subsequent to the transaction date (Profit_{ALPHA}). For sale transactions, the cumulative abnormal returns are multiplied by -1.

To alleviate the potential endogeneity concern associated with omitted variable biases in examining insiders' trading profitability upon cyberattacks, we perform difference-in-differences tests using insider transactions during the period from 90 days before the cyberattack disclosure date to one day before the disclosure date (*Intra period*), and the period from one day after the cyberattack disclosure date to the subsequent 365 days (*Post period*) for a propensity-score matched sample.³² Specifically, for each treatment firm that experiencing a cyberattack, we identify a control firm that does not experience cyberattacks using propensity-score matching approach. The propensity score is calculated using the logit regression of *Cyberattack* (an indicator that takes the value of one if a firm experiences a cyberattack in a given year, and zero otherwise) on firm size, stock performance, stock return volatility, leverage, institutional blockholder, book-to-market ratio, analyst coverage, and firm age. We require both treated and matched firms to be in the same industry (measured by two-digit SIC codes) as prior studies show that cyberattacks tend to be clustered at certain industries (e.g., Kamiya et al., 2020). We also require treated and matched firms to be in the same fiscal year, so the control firm has an

³¹ Short-swing rule in Section 16(b) of the Securities Exchange Act of 1934 requires company insiders to return any profits made from the purchase and sale of company stock if both transactions occur within a six-month period.

³² The results using 30 days as the intra period are very similar to those using 90 days. The usage of 365 days in post period is a conservative choice. A longer post period could reduce the significance of our results.

“artificial” cyberattack year even if it does not experience a cyberattack (Chan, Chen, and Chen, 2013). This approach allows us to perform difference-in-differences tests for the changes in performance, financial health, and corporate policies surrounding the cyberattack. We then match, without replacement, an attacked firm with a non-attacked control firm that has the closest propensity score. The matching procedure results in 144 attacked firms and 144 matching firms. We then identify transactions by all directors and officers for these firms from 90 days before to 365 days after the disclosure of cyberattacks, resulting in 14,518 transactions.³³

We estimate ordinary least squares (OLS) regressions in which the dependent variables are insider trading profitability measures using a transaction-level sample. Our key independent variable of interest is an interaction term between *Intra period* and *Cyberattack*. *Intra period* is an indicator that takes the value of one for transactions that occur in the period from 90 days before the cyberattack disclosure date to one day before the disclosure date, and zero for transactions that occur in the period from one day after the disclosure date to the subsequent 365 days. *Cyberattack* is an indicator that takes the value of one if a firm experiences a cyberattack, and zero if a firm is a non-target control firm. We control for various factors affecting insider trading profitability identified by the prior literature such as firm size (Seyhun, 1986; Lakonishok and Lee, 2001), book-to-market ratio (Rozeff and Zaman, 1998; Piotroski and Roulstone, 2005), the past 360-day buy-and-hold returns (Piotroski and Roulstone, 2005), and analyst following (Frankel and Li, 2004). We further include stock volatility, firm age, institutional ownership, and analyst coverage

³³ To exclude any contaminated effects from other corporate events, we exclude insider transactions that are within 40 days prior to the dividend declarations, earning announcements, restatement announcements, and merger and acquisition announcements.

to control for variation in information asymmetry.³⁴ We also include leverage to control for firm default risk. All the control variables are measured as of the fiscal quarter immediately before the trading date. We include year-quarter and firm fixed effects to control for unobservable omitted time trends and firm characteristics.

Panel A of Table 3 presents descriptive statistics for the propensity-score matched sample consisting of 144 firms experiencing a cyberattack and their 144 matching firms. We find no significant difference between attacked firms and their matching firms, suggesting our matching approach identifies matching firms that are very close to treatment firms.

Panel B of Table 3 reports the results of the difference-in-differences analyses. In columns (1) and (3), we find that the coefficient on *Intra period (indicator) × Cyberattack (indicator)* is positive and significant at the 1% level. The coefficients of 0.140 and 0.111 indicate that insiders in attacked firms earn significantly higher profits by 14% in terms of market-adjusted abnormal buy-and-hold return (Profit_{BHAR}) or 11.1 basis points in terms of Carhart-alpha (Profit_{ALPHA}) over 180 calendar days before the disclosure of the cyberattack announcement, respectively.³⁵ In columns (2) and (4), we divide cyberattacks into two types by the severity of the incidents. We expect that the market reacts more negatively to cyberattacks that result in the loss of personal information records such as social security numbers, bank account, and credit card information compared to those without such loss. The severity of cyberattacks should significantly affect insiders' incentives to sell shares to avoid the potential loss. Consistent with our prediction, we find that the magnitude of

³⁴ Most of variables such as firm size, Tobin's q , and institutional ownership are also related to the determinants of cyberattacks identified by Kamiya et al. (2020).

³⁵ These results still hold if we exclude purchase transactions from the sample.

the coefficient on *Intra period (indicator) × Cyberattacks with records breaches (indicator)* is larger than the coefficient on *Intra period (indicator) × Cyberattacks with no records breaches (indicator)* and the difference in two coefficients is statistically different for both profitability measures.

Panel C of Table 3 reports the estimation results using a subsample of opportunistic trades. Cohen, Malloy, and Pomorski (2012) decode insider trading into two types, routine and opportunistic, and find that trades by routine insiders who trade shares in a consistent pattern are not informative about firms' futures. We follow their classification to define a routine trader as an insider who placed a trade in the same calendar month for at least three years in the past and define opportunistic traders as everyone else. Our results of higher trading profitability of insiders at attacked firms still hold in the subsample of opportunistic traders.

V. Cross-Sectional Heterogeneity in Insiders' Trading Profitability

In this section, we examine how insider trading profitability varies across various firm and insider characteristics including information environment, governance quality, risk management system, insider attributes, and legal regulations.

5.1. Insiders' trading profitability and firms' information environment

First, we examine whether insiders' trading profitability is different depending on firm's information environment. Intuitively, information environment is crucial for insider trading given that insiders trade based on their privileged information. Opaque information environment restricts outsiders' ability to obtain firm-specific information and thus allow

insiders to exploit their information advantage at the expense of outsiders. Prior studies have addressed the relationship between information environment and insider trading (e.g., Huddart and Ke, 2007). Moreover, information environment may matter much in the setting of cyber risk, an emerging and complex operational risk. Outsiders who may not have related expertise on cyber risk are confined more severely to acquire information related to cyberattack incidents, which intensifies insiders' information advantage and increases their net benefits from engaging in trading upon cyberattacks. Thus, we expect trading profitability before the disclosure of cyberattacks to be higher for insiders at firms with higher information asymmetry.

We use three measures as the proxies for firms' information asymmetry, firm age (the maximum of years listed in Compustat and years listed in CRSP), R&D intensity (the ratio of R&D expenditures to total assets), and analyst forecast dispersion (the standard deviation of analyst forecasts scaled by the absolute value of the average forecasts). We then define *High information asymmetry* is an indicator that takes the value of one if the value of firm age is below its sample median or the value of R&D intensity or forecast dispersion is above its sample median, and zero otherwise.

The results are presented in Table 4. Consistent with our prediction that information asymmetry increases insiders' trading profitability upon cyberattacks, we find that the coefficients on the interaction terms between *Intra period (indicator)* $\times$ *Cyberattack (indicator)* and *High information asymmetry* are positive and significant in all regressions.

5.2. *Insiders' trading profitability and firms' governance quality*

We next examine whether corporate governance affects insiders' ability to profit from

their information advantage before the disclosure of cyberattacks. Prior studies show that governance quality significantly affects insiders' ability to acquire information and their trading performance (Ravina and Sapienza, 2010; Dai et al., 2016). Ravina and Sapienza (2010) find that both executives and independent directors earn higher abnormal returns in poorly governed firms and that executives outperform independent directors in such firms, suggesting that the gap between insider information and the market information is larger in firms with weak governance. Dai et al., (2016) also show that corporate governance system significantly reduce the profitability of insider trading, indicating that insiders are more constrained to exploit private information at better-governed firms. Their findings that the more restrictive effect of corporate governance on insider sales suggest that good corporate governance could deter insiders from exploiting negative private information prior to the disclosure of cyberattacks. Thus, we expect that insiders should earn higher profitability in firms with poor governance quality.

We use four measures of a firm's governance quality: the proportion of outside directors on the board (the ratio of the number of outside directors to the total number of directors on the board), the proportion of busy directors on the board (the ratio of the number of outside directors who hold at least three directorships to the total number of directors), the existence of dual class, and the adoption of staggered boards or poison pills. We define *Poor governance quality* as an indicator that takes the value of one if the proportion of outside directors is below its sample median, the proportion of busy directors is above its sample median, the firm has dual class, or the firm has adopted staggered boards or poison pills, and zero otherwise.

Table 5 report the results. We find that the coefficients on the interaction between *Poor*

governance quality and *Intra period (indicator) × Cyberattack (indicator)* is positive and significant in all eight regressions. For example, when poor governance quality is measured by lower proportion of outside directors on the board, the coefficient on the interaction term is 0.218 for buy-and-holder abnormal returns and 0.137 for Carhart-alpha and both are significant at the 5% level. Consistent with our predictions, these findings suggest that better governance can restrict insiders from profiting from their information advantage about cyberattacks.

5.3. *Insiders' trading profitability and firms' risk management system*

As a further test of cross-sectional heterogeneity in insiders' trading profitability, we next examine whether insiders' trading profitability is affected by the extent to which attacked firms invest in their risk management. If attacked firms do not invest sufficiently in their risk management, this would send a strong signal to outside investors that the firm is not capable of remediating the incident and preventing the recurrence of the attack. Moreover, the absence of risk management systems could fail to supervise insiders from exploiting their information advantage over outsider investors. Thus, insiders are expected to earn higher profits in firms with insufficient risk management system.

We focus on three aspects of a firm's investment in risk management: the proportion of outside directors with information technology (IT) expertise, the assignment of Chief Information Officer, and the deployment of risk committee. We define *Absence of risk management system* as an indicator that takes the value of one if a firm has no risk management mechanism of these three aspects, and zero otherwise.

Table 6 reports the results. The key variable of interest is the interaction term involving

risk management variables. We find that the coefficient on the interaction term involving the proportion of outside directors with IT expertise is positive and significant at the 1% level in columns (1) and (4), suggesting that insiders' trading gains are higher when a firm has less outside talents who provide IT expertise. We also find that the coefficients on the interaction terms involving CIO and risk committee are positive although the significance level varies depending on trading profitability measures. These findings are in line with the view that the absence of risk management system intensifies insiders' information advantage particularly on cyberattack incidents.

5.4. Insiders' trading profitability and insiders' attributes

We further examine how insiders' attributes affect their ability to exploit information advantages. We expect that insiders who have relevant background such as IT expertise would be better able to access the extent of the damage caused by cyberattacks and the potential costs of the incident. We identify whether each insider have relevant IT expertise by examining their past and current employment history and educational background. In Table 7, we find that the coefficient on the interaction term involving IT expertise is positive but only significant when Carhart-alpha is used as the measure of trading profitability. The findings provide some weak evidence that IT expertise helps insiders to analyze attacked firms' ability to remediate cyberattacks and exploit their information advantages.

We also interact *Intra period (indicator)* $\times$ *Cyberattack (indicator)* with insiders' ownership and their position at attacked firms to examine how their trading profitability changes. Insiders with higher ownership may have stronger incentive to engage in insider

trading ahead of the public disclosure of cyberattacks since they may suffer from greater wealth loss than those with lower ownership. However, insiders who serve as CEO or CFO may not have strong incentive to exploit their information advantage given more scrutiny from regulators (Wang, Shin, and Francis, 2012). Consistent with our predictions, we find a positive coefficient on the interaction term involving insiders' ownership and a negative coefficient on the interaction term involving insiders' CEO/CFO positions for Carhart-alpha.

5.5. Insiders' trading profitability and data breach notification law

Lastly, we investigate the impact of the data breach notification laws on insiders' trading profitability upon cyberattacks. From 2003 to 2018, all states have implemented state-level disclosure regulations for breaches. These laws require firms to notify consumers or affected parties of the breach incident after it is discovered. It is ex-ante unclear how these disclosure requirements to affect insiders' trading incentives. On the one hand, the mandated data breach disclosure may reduce insiders' trading profitability by accelerating public revelations of cyberattacks and thus limiting insiders' ability to trade on private information. On the other hand, it is possible that the mandated data breach disclosure prompts opportunistic insider trading as managers have latitude in how and exactly when to disclose due to the lack of details in disclosure regulations.³⁶ We examine this issue by using the information of the location of the attacked firm's headquarter.

³⁶ For example, the Securities and Exchange Commission's (SEC) 2011 Guidance require publicly traded firms to disclose material cyberattacks in the SEC's Form 8-K filing. However, critics question the effectiveness of the guidance arguing that the interpretation of the definition of materiality is subjective and the disclosure requirements are too general and do not provide specific guideline on how much information should be disclosed.

The results of the analysis are reported in Table 8. We interact *Intra period (indicator)* $\times$ *Cyberattack (indicator)* with *State law*, where *State law* is an indicator that takes the value of one if a firm is headquartered in a state in which data breach notification law is effective in a given year, and zero otherwise. We change firm fixed effects in columns (1) and (3) to state fixed effects in columns (2) and (4) to control for unobservable state characteristics. We find the coefficient on the interaction term is positive and significant at the 5% level in columns (1) and (2) in which market-adjusted buy-and-hold abnormal return (Profit_{BHAR}) is used as the dependent variable. We also find positive coefficients on the interaction term when the dependent variable is Carhart-alpha (Profit_{ALPHA}), though lack of significance. These results are consistent with the view that the mandated data breach disclosure requirements induce insiders to trade opportunistically to protect their personal wealth.

VI. Impact on Insider Trading of Industry Competitors

6.1. Trading profitability of insiders of peer firms

In this section, we investigate whether cyberattacks also affect trading incentives of insiders at peer firms in the same industries as attacked firms. If cyberattacks only reveal the failure of risk management in attacked firms, we would not expect any impact of cyberattacks on peer firms. However, insiders at peer firms may also have strong incentives to engage in trading to prevent their wealth loss when they believe that such attacks convey industry-wide issue and peer firms also suffer negative market reactions upon the disclosure of incidents. For instance, an attack may signal that the cyber risk is common to firms in the industry, or an attack may increase the awareness of the risk for firms

throughout the industry. Consistent with this argument, Kamiya et al. (2020) show that disclosure of an attack adversely affects firms operating in the same industry as the target. Thus, insiders at peer firms are expected to exploit such information advantage and earn positive profits prior to the disclosure of cyberattacks.

To test our hypotheses, we start with 144 events from our propensity-score-matched sample in Section 4 and identify all peer firms with the same four-digit SIC code as attacked firms. We only include industries (four-digit SIC code) in which only one cyberattack occurs in a given year to avoid the overlapping of intra and post periods. We follow the same filtering criteria in Section 3 to clean insider transactions. These procedures result in 46,920 transactions by directors or officers of 964 industry peer firms during the (pseudo) intra and post periods, where intra period is defined as the period from 90 days before the cyberattack disclosure date to one day before the cyberattack disclosure date and post period is defined as the period from one day after the cyberattack disclosure date to the subsequent 365 days. Using this sample, we again estimate ordinary least squares (OLS) regressions in which the dependent variables are insider trading profitability measures.³⁷ We include all control variables and fixed effects that are used in Table 3. We further include event fixed effects to control for unobservable event-specific characteristics.

Panel A of Table 9 reports the results. We find that the coefficient on *Intra period (indicator)* is positive and significant in column (1) in which the dependent variable is market-adjusted buy-and-hold abnormal return (Profit_{BHAR}), while the coefficient is

³⁷ In untabulated tests, we find that the percentage of sales transactions by insiders of peer firms in 30 days before the disclosure of cyberattacks is 68.7% and significantly higher than the percentage of sales transactions in 30 days after the disclosure of cyberattacks (62.6%). The results using 60 days are very similar to those using 30 days. These results indicate that insiders in these peer firms sell more in the intra period compared to the post period.

positive but insignificant in column (3) when Carhart-alpha (Profit_{ALPHA}) is used as the dependent variable. When we further divide cyberattacks into two types by the severity of the incidents, we find that the magnitude of the coefficient on *Intra period (indicator) × Cyberattacks with records breaches (indicator)* is significantly larger than the coefficient on *Intra period (indicator) × Cyberattacks with no records breaches (indicator)* in column (2). These findings provide some weak evidence that insiders of peer firms also earn positive profits when they trade prior to the public disclosure of cyberattacks.

6.2. Insiders' social ties with the attacked firm's board members

We next consider the channel through which insiders at peer firms possess information advantage from attacked firms. We focus on insiders' social ties with the attacked firm's board members as a source of insiders' information advantage. Studies show that social connections are a specific channel through which insiders obtain and trade on firm-specific information (Cao et al., 2015; Goergen, Renneboog, and Zhao, 2019). Cao et al. (2015) find that independent directors with social ties to senior executives make higher abnormal returns than in stock sales but not in stock purchases. They argue that the effect of social connections concentrated on trading profits in stock sales is largely due to insiders' propensity to withhold bad news, which leads to higher level of information asymmetry between insiders and outsiders. We expect insiders who are socially connected with the attacked firm's board members to possess information advantages as they are better informed ahead of the market in critical industry-wide situations.

Following Fracassi and Tate (2012), we measure insiders' social ties with the attacked firms' board members in connections of four types: current employment, prior

employment, education, and other activities. The results are presented in Panel B of Table 9. We find that the coefficient on interaction term between *Intra period* and *Insider with social ties* is positive and significant for both market-adjusted buy-and-hold abnormal return (Profit_{BHAR}) and Carhart-alpha (Profit_{ALPHA}). The findings suggest that social connections help insiders at peer firms gain access to private information that contains industry-wide negative news from attacked firms' board of directors.

VII. Summary and Conclusion

In this paper, we investigate how cyberattacks affect insiders' incentives to engage in insider trading. Insiders face the trade-off between costs from legal risk and economic benefits from engaging insider trading. Managers have discretions to disclose the cyberattack incidents and they have information advantage over outsiders to make abnormal returns prior to the disclosure of cyberattacks. Meanwhile, they may be refrained from engaging in insider trading if there are sufficient high litigation cost. Using 440 cyberattacks reported on public firms, we find that insiders who trade shares 90 days before the public disclosure of cyberattacks earn significantly higher profits. We further find that insiders' trading profits upon cyberattacks are more pronounced in firms with higher information asymmetry, poorer governance quality and insufficient risk management system. We also find that insiders who have IT expertise and higher ownership and do not serve as CEO or CFO earn higher profits ahead of the disclosure of cyberattacks. The adoption of data breach notification laws also has positive impact on insiders' trading profitability upon cyberattacks, implying that such laws may have unintended consequences. These findings collectively suggest that cyberattacks prompt insider trading.

In additional analyses, we find that insiders of peer firms in the same industry as attacked firms also earn significantly positive profits during the period of 90 days before to one day before the disclosure of cyberattacks. Consistent with Kamiya et al. (2020), the results suggest that cyberattacks signal the industry-wide issue. In further analyses, we show that social connection of peer firms' insiders with target firms contributes to their trading profits, through mainly information transmission from target firms.

Overall, our findings provide strong evidence on insider trading activities prior to the disclosure of cyberattacks. Given the emerging and complex nature of cyber risk, our paper also calls attention to inadequate regulations on insiders' response on cyber risk, not only for targeted firms, but also for their competing firms.

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Table 1
Summary Statistics

This table presents summary statistics for 277 firms (385 events) experiencing cyberattacks over the period of 2005 to 2017 and 7,342 firms that have the same four-digit SIC code as the attacked firm as of the fiscal year immediate before the cyberattack disclosure date. We further divide each group of firms into two subgroups according to whether the firm has at least one transaction made by a director or an officer during the *intra period* (i.e., the period from 90 days before the cyberattack disclosure date to one day before the cyberattack disclosure date). The appendix provides detailed descriptions of the construction of the variables. ***, **, and * denote that *t*-tests for mean differences are significant at the 1%, 5%, and 10% levels, respectively.

	Firms experiencing cyberattacks (N=385)		Firms experiencing no cyberattacks (N=7,342)		Test of difference		
	Insider trading exists (N=290): a	No insider trading exists (N=95): b	Insider trading exists (N=4,421): c	No insider trading exits (N=2,921): d	a-b	c-d	a-c
Firm characteristics							
Total assets (\$ billions)	35.886	19.446	6.800	3.515	16.440***	3.285***	29.086**
Book-to-market ratio	0.386	0.581	0.611	0.746	-0.195***	-	-0.225***
Stock performance	0.059	-0.068	0.037	-0.076	0.127***	0.113***	0.021
Stock volatility	0.019	0.025	0.026	0.034	-0.006***	-	-0.007***
Firm age	29.724	29.168	19.097	17.908	0.556	1.189***	10.627**
Institutional block ownership	0.189	0.216	0.179	0.153	-0.027	0.026***	0.010
Leverage	0.248	0.272	0.147	0.163	-0.024	-	0.101***
Analyst coverage	18.944	12.787	6.762	4.088	6.157***	2.674***	12.183**
Tobin's <i>q</i>	2.234	1.817	1.909	1.688	0.417**	0.221***	0.325***
ROA	0.055	0.028	0.004	-0.063	0.027***	0.067***	0.051***
R&D/Assets	0.026	0.018	0.036	0.049	0.008	-	-0.010***
CAPX/Assets	0.037	0.037	0.024	0.026	-0.000	-0.002*	0.013***
Asset intangibility	0.817	0.779	0.894	0.879	0.038	0.015***	-0.077***
Sales growth	1.085	1.047	1.131	1.089	0.038*	0.042***	-0.046***
Financial constraint (indicator)	0.037	0.114	0.188	0.366	-0.077**	-	-0.152***
Board size	10.571	10.557	9.850	9.390	0.013	0.178***	0.720***
Proportion of outside directors	0.801	0.792	0.782	0.782	0.009	0.000	0.020**
CEO-chair duality (indicator)	0.553	0.508	0.532	0.496	0.044	0.036	0.020
CEO characteristics							
CEO ownership	0.017	0.016	0.020	0.023	0.001	-0.003	-0.003
CEO tenure	8.408	7.284	9.932	9.577	1.124	0.355	-1.524***
CEO age	55.853	55.573	55.972	55.868	0.280	0.104	-0.119
Insider trading characteristics							
Number of net sale transactions	23.634	0.000	8.249	0.000	23.634***	8.249***	15.385**
Number of net sale shares (millions)	0.628	0.000	0.072	0.000	0.628***	0.072***	0.556***
Dollar value of net sale shares (\$ millions)	38.320	0.000	2.803	0.000	38.320***	2.803***	35.517**
Number of insider sellers	3.528	0.000	2.079	0.000	3.528***	2.079***	1.448***
Number of net sale transactions per insider	5.097	0.000	2.173	0.000	5.097***	2.173***	2.923**

Table 2
Likelihood of Insiders' Trading: Using a Sample of Firms Experiencing Cyberattacks

This table presents estimates of probit regressions in which the dependent variable is an indicator that takes the value equal to one if a firm has at least one insider transaction during the *intra period*, and zero otherwise. The sample consists of 308 events that have the cyberattack disclosure date available. *Intra period* is defined as the period from 90 days before the cyberattack disclosure date to one day before the cyberattack disclosure date. All firm characteristics are measured at the end of the fiscal year immediately before the cyberattack disclosure date and all continuous variables are winsorized at the 1st and 99th percentiles. Appendix provides detailed descriptions of the construction 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.

Independent variable	A firm has at least one insider trading during the <i>intra period</i> (indicator)				
	(1)	(2)	(3)	(4)	(5)
Firm size	0.305*** (0.001)	0.322*** (0.002)	0.335*** (0.001)	0.381*** (0.001)	0.308** (0.013)
Book-to-market ratio	-1.198*** (0.000)	-1.164*** (0.001)	-1.203*** (0.001)	-1.268*** (0.001)	-0.986*** (0.009)
Stock performance	0.206 (0.442)	0.319 (0.232)	0.336 (0.213)	0.277 (0.301)	0.505 (0.109)
Stock volatility	-18.646 (0.157)	-22.761* (0.100)	-21.944 (0.119)	-20.435 (0.150)	-21.914 (0.181)
Log (firm age)	-0.159 (0.335)	-0.111 (0.514)	-0.084 (0.617)	-0.125 (0.481)	-0.054 (0.800)
Institutional block ownership	0.845 (0.240)	0.862 (0.255)	0.925 (0.232)	0.890 (0.253)	1.066 (0.272)
Leverage	-0.437 (0.431)	-0.229 (0.688)	-0.287 (0.615)	-0.396 (0.508)	-0.231 (0.710)
Log (1+analyst coverage)	0.089 (0.629)	0.158 (0.427)	0.218 (0.278)	0.113 (0.587)	0.010 (0.966)
ROA	-0.779 (0.643)	-1.281 (0.459)	-2.001 (0.241)	-2.378 (0.176)	-1.371 (0.530)
Sales growth	-0.342 (0.481)	-0.397 (0.435)	-0.420 (0.422)	-0.461 (0.387)	-0.233 (0.680)
Financial constraint (indicator)	0.537 (0.294)	0.487 (0.347)	0.293 (0.574)	0.143 (0.784)	0.712 (0.291)
Log (board size)		-0.293 (0.581)	-0.312 (0.561)	-0.359 (0.510)	-0.194 (0.751)
Proportion of outside directors		-3.039** (0.025)	-3.544** (0.013)	-3.523** (0.015)	-3.130** (0.042)
CEO-Chair duality (indicator)		0.129 (0.480)	0.135 (0.458)	0.130 (0.472)	-0.094 (0.665)
CIO (indicator)		0.026 (0.894)	0.040 (0.836)	-0.007 (0.973)	0.026 (0.917)
Risk committee (indicator)		-0.361 (0.406)	-0.376 (0.401)	-0.362 (0.382)	-0.100 (0.868)
General counsel (indicator)			-0.174 (0.379)	-0.198 (0.321)	-0.176 (0.419)
State law (indicator)			-0.436 (0.114)	-0.436 (0.122)	-0.706* (0.050)
CAR (-1, 1)				-7.458* (0.058)	
Log (1 + number of records breached)					0.007 (0.680)
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Industry fixed effects (two-digit SIC)	Yes	Yes	Yes	Yes	Yes
Pseudo R^2	0.259	0.269	0.278	0.290	0.236
Observations	308	306	303	303	210

Table 3
Effects of Cyberattacks on Insiders' Trading Profitability

This table presents descriptive statistics for 144 treated firms that experience a cyberattack over the period of 2005 to 2017 and 144 matched control firms that do not experience a cyberattack over the same period (Panel A) and estimates of OLS regressions in which the dependent variables are insider trading profitability measures (Panel B and C). The sample consists of 14,518 transactions by directors or officers of 144 treatment firms during the intra and post periods and by those of 144 control firms over the same period. The propensity score is calculated using the logit regression of *Cyberattack* (an indicator that takes the value equal to one if a firm experiences a cyberattack, and zero otherwise) on firm size, stock performance, stock return volatility, leverage, institutional blockholder (indicator), book-to-market ratio, analyst coverage, and firm age. We require both treated and matching firms to be in the same industry (the same two-digit SIC codes) and in the same fiscal year. *Intra period* is an indicator that takes the value of one for transactions that occur over the period from 90 days before the cyberattack disclosure date to one day before the disclosure date, and zero for transactions that occur over the period from one day after the disclosure date to the subsequent 365 days. $Profit_{BHA}$ is the market-adjusted buy-and-hold abnormal return over 180 calendar days subsequent to the insider trading date. $Profit_{ALPHA}$ is the average daily abnormal return in percentage estimated using the Fama-French-Carhart four-factor model (Carhart, 1997) over 180 calendar days subsequent to the insider trading date. Panel C includes opportunistic transactions, defined as transactions made by insiders who do not trade shares in the same calendar month in the past three years (Cohen, Malloy, and Pomorski, 2012). Control variables are measured as of the fiscal quarter immediately before the trading date and all continuous variables are winsorized at the 1st and 99th percentiles. Appendix provides detailed descriptions of the construction of the variables. *P*-values reported in parentheses are based on standard errors clustered at the firm level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Panel A. Descriptive statistics for propensity-score matched sample firms

Variable	Treatment firms with a cyberattack (N=144): a		Control firms without a cyberattack (N=144): b		Test of difference: a-b	
	Mean	Median	Mean	Median	<i>t</i> -test	Wilcoxon <i>z</i> -test
Firm size	9.077	9.010	8.950	9.041	0.531	0.667
Book-to-market ratio	0.389	0.339	0.399	0.345	0.815	0.678
Stock performance	0.084	0.047	0.089	0.047	0.892	0.682
Stock volatility	0.020	0.018	0.019	0.017	0.735	0.856
Log (firm age)	3.068	3.045	2.998	2.944	0.407	0.430
Institutional blockholder (indicator)	0.889	1.000	0.882	1.000	0.854	0.853
Leverage	0.251	0.189	0.228	0.163	0.370	0.505
Log (1 + analyst coverage)	2.710	2.913	2.675	2.801	0.661	0.362

Panel B. Effects of cyberattacks on insider trading profitability: analyses using a full sample

Independent variable	Profit _{BHA}		Profit _{ALPHA} (%)	
	(1)	(2)	(3)	(4)
Intra period (indicator) × Cyberattack (indicator)	0.140*** (0.008)		0.111*** (0.001)	
Intra period (indicator) × Cyberattack with records breached (indicator): a		0.196*** (0.008)		0.166*** (0.000)
Intra period (indicator) × Cyberattack with no records breached (indicator): b		0.053 (0.175)		0.026 (0.365)
Firm size	0.125*** (0.010)	0.123** (0.014)	-0.025 (0.514)	-0.027 (0.495)
Book-to-market ratio	-0.150 (0.151)	-0.136 (0.186)	-0.035 (0.564)	-0.022 (0.736)
Stock performance	0.216* (0.085)	0.200* (0.088)	0.140 (0.169)	0.124 (0.189)
Stock volatility	0.969 (0.627)	1.707 (0.446)	0.759 (0.647)	1.479 (0.393)
Log (firm age)	-0.667** (0.028)	-0.635** (0.044)	-0.214 (0.312)	-0.183 (0.404)
Institutional blockholder (indicator)	-0.006 (0.848)	-0.012 (0.692)	-0.031 (0.330)	-0.037 (0.245)
Leverage	-0.298 (0.373)	-0.257 (0.438)	-0.163 (0.280)	-0.123 (0.402)
Log (1 + analyst coverage)	0.093 (0.411)	0.093 (0.400)	0.091 (0.291)	0.091 (0.273)

<i>P</i> -value for the test of $a=b$		0.076		0.009
Year-quarter fixed effects	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes
Adjusted R^2	0.663	0.669	0.608	0.619
Observations	14,518	14,518	14,518	14,518

Panel C. Effects of cyberattacks on insider trading profitability: analyses using a subsample of opportunistic trades

	Profit _{BHAR}		Profit _{ALPHA} (%)	
	(1)	(2)	(3)	(4)
Intra period (indicator) $\times$ Cyberattack (indicator)	0.057*		0.089***	
	(0.078)		(0.000)	
Intra period (indicator) $\times$ Cyberattacked firm with records breached (indicator): a		0.037		0.105***
		(0.271)		(0.000)
Intra period (indicator) $\times$ Cyberattack with no records breached (indicator): b		0.084*		0.068*
		(0.052)		(0.067)
<i>P</i> -value for the test of $a=b$		0.314		0.415
Control variables (those used in Panel B)	Yes	Yes	Yes	Yes
Year-quarter fixed effects	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes
Adjusted R^2	0.788	0.788	0.738	0.739
Observations	8,432	8,432	8,432	8,432

Table 4
Effects of Cyberattacks on Insider Trading Profitability by
Insiders' Net Benefits Measured as Information Asymmetry

This table presents estimates of OLS regressions in which the dependent variables are the market-adjusted buy-and-hold abnormal return over 180 calendar days subsequent to the insider trading date (Profit_{BHAR}) in columns (1)-(3) and the average daily abnormal return in percentage estimated using the Fama-French-Carhart four-factor model (Carhart, 1997) over 180 calendar days subsequent to the insider trading date (Profit_{ALPHA}) in column (4)-(6). The sample consists of 14,518 transactions by directors or officers of 144 treated firms experiencing a cyberattack during the intra and post periods and by those of 144 control firms over the same periods. The propensity score is calculated using the logit regression of *Cyberattack* (an indicator that takes the value equal to one if a firm experiences a cyberattack, and zero otherwise) on firm size, stock performance, stock return volatility, leverage, institutional blockholder (indicator), book-to-market ratio, analyst coverage, and firm age. We require both treated and matching firms to be in the same industry (the same two-digit SIC codes) and in the same fiscal year. *Intra period* is an indicator that takes the value of one for transactions that occur over the period from 90 days before the cyberattack disclosure date to one day before the disclosure date, and zero for transactions that occur over the period from one day after the disclosure date to the subsequent 365 days. Firm age is the maximum number of years listed in CRSP and years listed in Compustat. R&D intensity is the ratio of Research & Development expenditures to total assets. Forecast dispersion is the standard deviation of analyst forecasts scaled by the absolute value of the mean analysts' forecasts in the month immediately before the insider trading date. *High information asymmetry* is an indicator that takes the value of one if the value of firm age is below its sample median, or the value of R&D intensity or analyst forecast dispersion is above its sample median, and zero otherwise. Control variables are measured as of the fiscal quarter immediately before the trading date and all continuous variables are winsorized at the 1st and 99th percentiles. Appendix provides detailed descriptions of the construction 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.

Independent variable	Profit _{BHAR}			Profit _{ALPHA} (%)		
	Firm age	R&D intensity	Forecast dispersion	Firm age	R&D intensity	Forecast dispersion
	(1)	(2)	(3)	(5)	(6)	(7)
Intra period (indicator) × Cyberattack (indicator): a	0.060** (0.037)	0.035 (0.267)	0.034 (0.238)	0.065** (0.014)	0.041 (0.160)	0.050** (0.037)
High information asymmetry (indicator): b	0.169*** (0.001)	-0.010 (0.879)	-0.047* (0.090)	0.047 (0.323)	-0.006 (0.902)	-0.020 (0.312)
a × b	0.213*** (0.006)	0.165*** (0.010)	0.205*** (0.003)	0.115** (0.043)	0.110** (0.025)	0.116** (0.031)
Control variables (those in Panel C of Table 3)	Yes, except, log (firm age)	Yes	Yes	Yes, except log (firm age)	Yes	Yes
Year-quarter fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	14,518	14,518	14,384	14,518	14,518	14,384
Adjusted R ²	0.677	0.670	0.680	0.616	0.615	0.621

Table 5
Effects of Cyberattacks on Insider Trading Profitability by
Insiders' Net Benefits Measured as Governance Quality

This table presents estimates of OLS regressions in which the dependent variables are the market-adjusted buy-and-hold abnormal return over 180 calendar days subsequent to the insider trading date ($Profit_{BHA}$) in columns (1)-(3) and the average daily abnormal return in percentage estimated using the Fama-French-Carhart four-factor model (Carhart, 1997) over 180 calendar days subsequent to the insider trading date ($Profit_{ALPHA}$) in column (4)-(6). The sample consists of 14,360 transactions by directors or officers of 144 treated firms experiencing a cyberattack during the intra and post periods and by those of 144 control firms over the same periods. The propensity score is calculated using the logit regression of *Cyberattack* (an indicator that takes the value equal to one if a firm experiences a cyberattack, and zero otherwise) on firm size, stock performance, stock return volatility, leverage, institutional blockholder (indicator), book-to-market ratio, analyst coverage, and firm age. We require both treated and matching firms to be in the same industry (the same two-digit SIC codes) and in the same fiscal year. *Intra period* is an indicator that takes the value of one for transactions that occur over the period from 90 days before the cyberattack disclosure date to one day before the disclosure date, and zero for transactions that occur over the period from one day after the disclosure date to the subsequent 365 days. *Poor governance quality* is an indicator that takes the value of one if the proportion of outside directors is below its sample median, the proportion of busy directors is above its sample median, the firm has dual class, or the firm has adopted staggered boards or poison pills, and zero otherwise. Control variables are measured as of the fiscal quarter immediately before the trading date and all continuous variables are winsorized at the 1st and 99th percentiles. Appendix provides detailed descriptions of the construction 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.

	Profit _{BHA}				Profit _{ALPHA} (%)			
	Proportion of outside directors	Proportion of busy directors	Dual class	Staggered boards or poison pills	Proportion of outside directors	Proportion of busy directors	Dual class	Staggered boards or poison pills
Independent variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Intra period (indicator) × Cyberattack (indicator): a	0.034 (0.176)	0.095* (0.052)	0.029 (0.191)	0.064* (0.055)	0.044** (0.042)	0.075** (0.027)	0.044* (0.065)	0.052** (0.030)
Poor governance quality (indicator): b	0.056 (0.291)	-0.007 (0.894)	0.120 (0.256)	0.041* (0.053)	-0.039 (0.365)	-0.042 (0.190)	0.078 (0.202)	0.017 (0.403)
a × b	0.218** (0.011)	0.101** (0.018)	0.285*** (0.000)	0.091* (0.091)	0.137** (0.012)	0.082*** (0.008)	0.145*** (0.004)	0.071* (0.091)
Control variables (those in Panel C of Table 3)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-quarter fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	14,360	14,360	13,781	13,777	14,360	14,360	13,781	13,777
Adjusted R ²	0.676	0.663	0.711	0.699	0.621	0.615	0.647	0.641

Table 6
Effects of Cyberattacks on Insider Trading Profitability by
Insiders' Net Benefits Measured as Risk Management System

This table presents estimates of OLS regressions in which the dependent variables are the market-adjusted buy-and-hold abnormal return over 180 calendar days subsequent to the insider trading date ($Profit_{BHAR}$) in column (1) and the average daily abnormal return in percentage estimated using the Fama-French-Carhart four-factor model (Carhart, 1997) over 180 calendar days subsequent to the insider trading date ($Profit_{ALPHA}$) in column (2). The sample consists of 14,483 transactions by directors or officers of 144 treated firms experiencing a cyberattack during the intra and post periods and by those of 144 control firms over the same periods. The propensity score is calculated using the logit regression of *Cyberattack* (an indicator that takes the value equal to one if a firm experiences a cyberattack, and zero otherwise) on firm size, stock performance, stock return volatility, leverage, institutional blockholder (indicator), book-to-market ratio, analyst coverage, and firm age. We require both treated and matching firms to be in the same industry (the same two-digit SIC codes) and in the same fiscal year. *Intra period* is an indicator that takes the value of one for transactions that occur over the period from 90 days before the cyberattack disclosure date to one day before the disclosure date, and zero for transactions that occur over the period from one day after the disclosure date to the subsequent 365 days. *Absence of risk management system* is an indicator that takes the value of one if a firm has no risk management mechanism indicated in each column, and zero otherwise. Control variables are measured as of the fiscal quarter immediately before the trading date and all continuous variables are winsorized at the 1st and 99th percentiles. Appendix provides detailed descriptions of the construction 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.

Independent variable	$Profit_{BHAR}$			$Profit_{ALPHA}$ (%)		
	Proportion of outside directors with IT expertise	CIO	Risk committee	Proportion of outside directors with IT expertise	CIO	Risk committee
	(1)	(2)	(3)	(4)	(5)	(6)
Intra period (indicator) $\times$ Cyberattack (indicator): a	-0.041 (0.150)	0.065 (0.146)	0.018 (0.647)	-0.001 (0.973)	0.011 (0.702)	0.041 (0.164)
Absence of risk management system (indicator): b	-0.119 (0.118)	-0.029 (0.611)	-0.001 (0.992)	-0.055 (0.158)	0.112* (0.070)	-0.008 (0.885)
a $\times$ b	0.213*** (0.003)	0.068 (0.183)	0.121** (0.019)	0.130*** (0.003)	0.094*** (0.008)	0.067 (0.128)
Control variables (those in Panel C of Table 3)	Yes	Yes	Yes	Yes	Yes	Yes
Year-quarter fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	14,360	14,360	14,360	14,360	14,360	14,360
Adjusted R^2	0.670	0.661	0.662	0.617	0.613	0.611

Table 7
Effects of Cyberattacks on Insider Trading Profitability by
Insiders' Net Benefits Measured as Individual characteristics

This table presents estimates of OLS regressions in which the dependent variables are the market-adjusted buy-and-hold abnormal return over 180 calendar days subsequent to the insider trading date ($Profit_{BHAR}$) in column (1) and the average daily abnormal return in percentage estimated using the Fama-French-Carhart four-factor model (Carhart, 1997) over 180 calendar days subsequent to the insider trading date ($Profit_{ALPHA}$) in column (2). The sample consists of 14,483 transactions by directors or officers of 144 treated firms experiencing a cyberattack during the intra and post periods and by those of 144 control firms over the same periods. The propensity score is calculated using the logit regression of *Cyberattack* (an indicator that takes the value equal to one if a firm experiences a cyberattack, and zero otherwise) on firm size, stock performance, stock return volatility, leverage, institutional blockholder (indicator), book-to-market ratio, analyst coverage, and firm age. We require both treated and matching firms to be in the same industry (the same two-digit SIC codes) and in the same fiscal year. *Intra period* is an indicator that takes the value of one for transactions that occur over the period from 90 days before the cyberattack disclosure date to one day before the disclosure date, and zero for transactions that occur over the period from one day after the disclosure date to the subsequent 365 days. *Insider's personal attribute* is an indicator variable that takes the value of one if an insider has IT expertise, an insider's ownership is below its sample median, or an insider is serving as CEO or CFO, and zero otherwise. Control variables are measured as of the fiscal quarter immediately before the trading date and all continuous variables are winsorized at the 1st and 99th percentiles. Appendix provides detailed descriptions of the construction 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.

Independent variable	$Profit_{BHAR}$			$Profit_{ALPHA}$ (%)		
	IT expertise	Ownership	CEO/CFO	IT expertise	Ownership	CEO/CFO
	(1)	(2)	(3)	(4)	(5)	(6)
Intra period (indicator) $\times$ Cyberattack	0.137** (0.011)	0.016 (0.522)	0.147*** (0.005)	0.104*** (0.004)	0.037 (0.109)	0.119*** (0.000)
Insider's personal attribute (indicator): a	0.000 (0.978)	0.015 (0.355)	0.001 (0.868)	0.011 (0.389)	0.033 (0.111)	0.010 (0.209)
a $\times$ b	0.041 (0.301)	0.053 (0.351)	-0.027 (0.263)	0.072* (0.069)	0.124*** (0.001)	-0.038* (0.061)
Control variables (those in Panel C of Table 3)	Yes	Yes	Yes	Yes	Yes	Yes
Year-quarter fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	14,489	8,450	0.671	14,489	8,450	0.620
Adjusted R^2	0.663	0.791	14,276	0.612	0.761	14,276

Table 8
Effects of Cyberattacks on Insider Trading Profitability by
Insiders' Net Benefits Measured as Legal Regulations

This table presents estimates of OLS regressions in which the dependent variables are the market-adjusted buy-and-hold abnormal return over 180 calendar days subsequent to the insider trading date ($Profit_{BHAR}$) in columns (1) and (2) and the average daily abnormal return in percentage estimated using the Fama-French-Carhart four-factor model (Carhart, 1997) over 180 calendar days subsequent to the insider trading date ($Profit_{ALPHA}$) in columns (3) and (4). The sample consists of 14,334 transactions made by directors or officers of 144 treated firms during the intra and post periods and by those of 144 control firms over the same period. The propensity score is calculated using the logit regression of *Cyberattack* (an indicator that takes the value equal to one if a firm experiences a cyberattack over the period 2005 to 2017, and zero otherwise) on firm size, stock performance, stock return volatility, leverage, institutional blockholder (indicator), book-to-market ratio, analyst coverage, and firm age. We require both treated and matching firms to be in the same industry (the same two-digit SIC codes) and in the same fiscal year. *Intra period* is an indicator that takes the value of one for transactions that occur over the period from 90 days before the cyberattack disclosure date to one day before the disclosure date, and zero for transactions that occur over the period from one day after the disclosure date to the subsequent 365 days. *State law* is an indicator that takes the value of one if a firm is headquartered in a state in which a data breach notification law is effective in a given year, and zero otherwise. Control variables are measured as of the fiscal quarter immediately before the trading date and all continuous variables are winsorized at the 1st and 99th percentiles. Appendix provides detailed descriptions of the construction 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.

Independent variable	Profit _{BHAR}		Profit _{ALPHA} (%)	
	(1)	(2)	(3)	(4)
Intra period (indicator) × Cyberattack (indicator): a	-0.033 (0.626)	-0.134* (0.065)	0.040 (0.468)	-0.009 (0.845)
State law (indicator): b	-0.137 (0.108)	0.053 (0.431)	-0.098 (0.255)	0.007 (0.873)
a × b	0.181** (0.021)	0.231** (0.012)	0.074 (0.250)	0.055 (0.269)
Control variables (those in Panel C of Table 3)	Yes	Yes	Yes	Yes
Year-quarter fixed effects	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	No	Yes	No
State fixed effects	No	Yes	No	Yes
Observations	14,412	14,412	14,412	14,412
Adjusted R ²	0.667	0.457	0.610	0.315

Table 9
Insider Trading Profitability of Industry Peer Firms

This table presents estimates of OLS regressions in which the dependent variables are the market-adjusted buy-and-hold abnormal return over 180 calendar days subsequent to the insider trading date ($Profit_{BHAR}$) and the average daily abnormal return in percentage estimated using the Fama-French-Carhart four-factor model (Carhart, 1997) over 180 calendar days subsequent to the insider trading date ($Profit_{ALPHA}$) (Panel A). The sample consists of 46,920 transactions by directors or officers of 964 industry peer firms during the (pseudo) intra and post periods. We include firms operating industries (four-digit SIC code) in which only one cyberattack occurs in a given year. *Intra period* is an indicator that takes the value of one for transactions that occur over the period from 90 days before the cyberattack disclosure date to one day before the disclosure date, and zero for transactions that occur over the period from one day after the disclosure date to the subsequent 365 days. *Insider with social ties* is an indicator that takes the value of one for transaction made by peer firms' insiders who are socially connected to a board member of the attacked firms, and zero otherwise. Control variables are measured as of the fiscal quarter immediately before the trading date and all continuous variables are winsorized at the 1st and 99th percentiles. Appendix provides detailed descriptions of the construction 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. Peer firms' insider trading profitability

	$Profit_{BHAR}$		$Profit_{ALPHA}$ (%)	
	(1)	(2)	(3)	(4)
Intra period (indicator)	0.040** (0.028)		0.011 (0.428)	
Intra period (indicator) × Cyberattack with records breached (indicator): a		0.069** (0.011)		0.023 (0.304)
Intra period (indicator) × Cyberattack without records breached (indicator): b		0.013 (0.502)		0.001 (0.963)
Control variables (those in Panel C of Table 3)	Yes	Yes	Yes	Yes
<i>P</i> -value for the test of a=b		0.056		0.335
Event fixed effects	Yes	Yes	Yes	Yes
Year-quarter fixed effects	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes
Adjusted R^2	0.432	0.433	0.444	0.444
Observations	46,920	46,920	46,920	46,920

Panel B: Insiders' social connections with the attacked firm and their trading profitability

	$Profit_{BHAR}$	$Profit_{ALPHA}$ (%)
	(1)	(2)
Intra period (indicator): a	0.033* (0.082)	0.007 (0.632)
Insider with social ties (indicator): b	-0.010 (0.411)	-0.011 (0.252)
a × b	0.051** (0.043)	0.035* (0.060)
Control variables (those in Panel C of Table 3)	Yes	Yes
Event fixed effects	Yes	Yes
Year-quarter fixed effects	Yes	Yes
Firm fixed effects	Yes	Yes
Adjusted R^2	0.432	0.446
Observations	46,172	46,172

Appendix

The Appendix provides detailed descriptions of all variables used in the tables.

Variable	Definition	Source
Absence of risk management system (indicator)	One if the proportion of directors with IT expertise is below the sample median, a firm has no Chief Information Officer, or a firm has no risk committee, and zero otherwise	BoardEx, GMI Ratings
Adoption of staggered boards or poison pills (indicator)	One if a firm adopts either staggered boards or poison pills, and zero otherwise	
Analyst coverage	The natural logarithm of one plus the average of the 12 monthly numbers of earnings forecasts for a given fiscal year	I/B/E/S
Analyst forecast dispersion	The standard deviation of all outstanding earnings-per-share forecasts for the current fiscal year scaled by the absolute value of the mean analysts forecast	I/B/E/S
Asset intangibility	The ratio of total assets minus total property, plant and equipment to total assets	Compustat
Book-to-market ratio	The ratio of the book value of equity to the market value of equity	Compustat
CAPX/assets	The ratio of capital expenditures to total assets	Compustat
CAR (-1, 1)	The cumulative abnormal return from one day before to one day after the disclosure date. The abnormal stock returns are calculated using the market model. The market model parameters are estimated using 220 trading days of return data beginning 280 days before and ending 61 days before the breach announcements, using the CRSP value-weighted return as a proxy for the market return. The daily abnormal stock returns are cumulated to obtain the CAR from one day before the attack announcement date to one day after the attack announcement date	CRSP
CEO-chair duality (indicator)	One if the CEO serves as the chair of the board, and zero otherwise	ExecuComp
CIO (indicator)	One if the firm has Chief Information Officer, and zero otherwise	BoardEx
Cyberattack (indicator)	One if a firm experiences hacking or malware-electronic entry by an outside party, malware, and spyware, and zero otherwise	PRC, Audit Analytics
Dual class (indicator)	One if the firm has dual class, and zero otherwise	GMI Ratings
Financial constraint (indicator)	One if a firm's WW index (Whited and Wu (2006)) is in the top tercile of the sample in a given year, and zero otherwise. $WW \text{ index} = -0.091 \times [(\text{income before extraordinary items} + \text{depreciation and amortization} / \text{total assets}) - 0.062 \times [\text{indicator that takes value equal to one if dividend for common shares} + \text{dividend for preferred shares is positive, and zero otherwise}] + 0.021 \times [\text{long-term debt} / \text{total assets}] - 0.044 \times [\log(\text{total assets})] + 0.102 \times [\text{average industry sales growth for each two-digit SIC industry and each year}] - 0.035 \times \text{sales growth}$	Compustat
Firm size	The natural logarithm of total assets	Compustat
General counsel (indicator)	One if a firm has a general counsel in a given year, and zero otherwise	GMI Ratings
High information asymmetry (indicator)	One if the value of firm age is below its sample median, or the value of R&D intensity or analyst forecast dispersion is above its sample median, and zero otherwise	Compustat, CRSP, I/B/E/S
Insider's personal attribute (indicator)	One if an insider has IT expertise, an insider's ownership is below its sample median, or an insider is serving as CEO or CFO, and zero otherwise	ExecuComp, GMI Ratings
Insider with social ties (indicator)	One if a director or an officer of the peer firm shares at least one type of social connections with the board members of the attacked firm, and zero otherwise. We follow Fracassi and Tate (2012) to construct four types of connections: current employment, past employment, education and other activities.	BoardEx
Institutional block ownership	The number of shares held by institutional shareholders that own more than 5% of a firm's equity scaled by the total number of shares outstanding	Thomson Reuters 13F
Intra period (indicator)	One for transactions that occur over the period from 90 days before the cyberattack disclosure date to one day before the disclosure date, and zero	Thomson Financial

	for transactions that occur over the period from one day after the disclosure date to the subsequent 365 days	Insiders Data Feed
Leverage	The ratio of long-term debt plus short-term debt to total assets	Compustat
Log (board size)	The natural log of the number of directors on the board	BoardEx
Log (CEO tenure)	The natural logarithm of the number of years since the year when a CEO has served in the firm	ExecuComp
Log (CEO age)	The natural logarithm of the CEO age	ExecuComp
Log (firm age)	The natural logarithm of max (years in CRSP, years in Compustat)	Compustat, CRSP
Poor governance quality (indicator)	One if the proportion of outside directors is below its sample median, the proportion of busy directors is above its sample median, the firm has dual class, or the firm has adopted staggered boards or poison pills, and zero otherwise	BoardEx, GMI Ratings
Profit _{ALPHA}	Average daily abnormal return in percentage estimated using Carhart's (1997) four-factor model over 180 calendar days subsequent to the insider trading date, multiplied by -1 for insider sales transactions	CRSP
Profit _{BHAR}	Market-adjusted buy-and-hold abnormal return over 180 calendar days subsequent to the insider trading date, multiplied by -1 for insider sales transactions	CRSP
Proportion of busy directors	Ratio of the number of outside directors who hold at least three directorships to the total number of directors	BoardEx
Proportion of directors with IT expertise	Ratio of the number of outside directors with IT expertise to the total number of directors	BoardEx
Proportion of outsider directors	Ratio of the number of outsider directors on the board to the total number of directors	BoardEx
R&D / Assets	The ratio of R&D expenditures divided by total assets	Compustat
Risk committee (indicator)	One if a firm has a risk committee, and zero otherwise	GMI Ratings
ROA	Net income divided by total assets	Compustat
Sales growth	Sales _{<i>t</i>} divided by Sales _{<i>t-1</i>}	Compustat
State law (indicator)	One if a firm is headquartered in a state in which a data breach notification law is effective in a given year, and zero otherwise	Chen, Hilary, and Tian (2019)
Stock performance	Buy-and-hold return for the year net of the CRSP value-weighted index return	Compustat, CRSP
Stock volatility	Standard deviation of a firm's daily stock returns during a fiscal year	Compustat, CRSP
Tobin's <i>q</i>	Book value of total assets minus book value of equity plus market value of equity divided by book value of total assets	Compustat

Chapter 3

Holding Company Structure and Mergers and Acquisitions

Abstract

We examine the impact of a holding company structure on announcement returns for mergers and acquisitions (M&As). We find that holding company acquirers experience higher abnormal stock returns around the M&A announcement date and better post-merger long-term operating performance than non-holding company acquirers. Further analyses show that the value-enhancing impact of the holding company structure is more pronounced when acquiring firms are better governed and have higher tax avoidance. The results support the view that the holding company structure enhances firm value by mitigating the manager-shareholder conflict and reducing tax costs.

Keywords: Holding company, Merger, Agency problem, Tax avoidance, Announcement return

JEL Classification: G32, G34, G38, H26

I. Introduction

A holding company structure is a common organizational form in public firms.³⁸ Prior studies have mainly focused on bank holding companies and public utilities holding companies given several waves of regulations and deregulations of such holding companies in the past half century (e.g., Aharony and Swary, 1981; Saunders, Strock, and Travlos, 1990; Ellul and Yerramilli, 2013). However, little is known about how the holding company structure affects firm value in non-bank and non-utilities industries. In this paper, we investigate this issue by exploiting mergers and acquisitions made by such holding companies.

Investigating holding companies in non-bank and non-utilities industries have several advantages in facilitating our understanding of the holding company structure compared to investigating them in bank and utilities industries. Firstly, since bank and utilities holding companies are heavily regulated, firms' incentives related to corporate policies in bank and utilities holding companies may be distorted. Thus, it is easier to draw clear inference on the impact of the holding company structure in the sample of non-bank and non-utilities industries. Secondly, exploiting holding companies in various industries provides considerable cross-sectional variations in the impact of the holding company structure. As firms in different industries have different capital structure, ownership structure, and corporate governance, it would be easier to reveal potential channels in which the holding company structure affects firm value.

We expect that a holding company structure can enhance the valuation of firms in mergers. First, the key feature of a holding company structure is that the top parent firm

³⁸ Among non-financial and non-utility public firms listed in the three major exchanges (i.e., NYSE, AMEX, Nasdaq) from 1996 to 2016, we find that, on average, more than 20% of them are holding companies.

does not engage in any day-to-day operations and all operations are conducted by subsidiaries. Top management teams are responsible for overseeing the firm and making strategic plans in operating, financing, and investment, while management teams in subsidiaries can focus on the improvement of their operating performance without being diverted by concerns on other business. Such clear division of working duties can help reduce the conflict of interests between managers and shareholders (i.e., agency problem). On the one hand, the holding company structure provides better accountability on managers in both parent and subsidiary firms, which induces managers to exert more efforts and restricts them from shirking. On the other hand, such a structure reduces the potential distraction of attentions for managers which has been discussed widely in the literature to be detrimental to firm value (e.g., Fich and Shivdasani, 2006; Falato, Kadyrzhanova, and Lel, 2014; Kempf, Manconi, and Spalt, 2017). Moreover, a holding company structure can attenuate the negative consequences of internal capital market. Though traditional views of internal capital market argue that headquarters can use their informational advantage and control rights to better allocate resources within the firms as compared to external capital market (e.g., Stein, 1997), Seru (2014) shows that information asymmetry between headquarters and divisions may actually hurt the value of the firms. The feature of better accountability and less distraction in the holding company structure can help to mitigate such impact by incentivizing managers in parent firms to collect related information for their strategic planning and closely monitor the operations of subsidiaries. Thus, by mitigating the agency problem involving managers, the holding company structure can enhance firm value.

Second, the holding company structure can also increase firm value through tax

savings. A large amount of literature has examined how tax motivations play important roles in determining the deployment of a holding company structure (e.g., Desai, Foley, and Hines, 2006; Dharmapala, Foley, and Forbes, 2011; Dyreng, Lindsey, and Thornock, 2013). A holding company can aggregate its cash flows from various subsidiaries with positive or negative incomes to decrease total tax liability. More importantly, given the variations in tax rates regulated in different jurisdictions, a major corporation might structure itself as a holding company with one subsidiary to own its brand name and trademarks, another to own its real estate, and others to operate each franchise, and strategically place certain parts of business in jurisdictions with lower tax rates. Holding companies incorporated in Delaware are typical examples of corporations for tax motivations. The Delaware state income tax law allows out-of-state companies to exempt some of their income from taxations if the firm incorporated in Delaware is a holding company that simply maintains and manages intangible assets. Therefore, a holding company arrangement can provide a firm much flexibility to limit its tax liability and thus enhance firm value.

To test our predictions, we use a large sample of US mergers and acquisitions during the period of 1997 to 2016. We identify whether an acquiring firm is a holding company through its business descriptions disclosed in annual reports (i.e., 10-K filings). We find that acquiring firms with holding company structures experience significantly higher abnormal returns around the merger announcement dates, compared to acquiring firms without holding company structures. Specifically, the average cumulative abnormal returns from one day before to one day after the merger announcements is 1.457% if the acquiring firms are holding companies, while the number is 1.004% if the acquiring firms are not

holding companies. We further find that cumulative abnormal returns of the value-weighted portfolios of the acquirer and the target are also significantly higher if the acquiring firms are holding companies, while the difference in target firms' cumulative abnormal returns between holding company acquirers and non-holding company acquirers are insignificant.

We also test cumulative abnormal returns in multivariate regression analyses controlling for various acquirer- and deal-specific characteristics. We again find significantly positive relation between acquirer's holding company status and cumulative abnormal returns of the acquirer and the value-weighted portfolio of the acquirer and the target. In addition, we estimate the post-merger changes in long-term operating performance by separately regressing the difference in post-merger operating performance between the combined firm (the acquirer and the target) and the control firm on the difference in pre-merger operating performance between the combined firm and the control firm in a subsample of holding company acquirers and a subsample of non-holding company acquirers. We find that though non-holding company acquirers also experience abnormal increases in operating performance after the merger, the increase is larger when the acquiring firms are holding companies. These results collectively suggest that the holding company structure enhances the valuation of firms in mergers.

We next explore whether the value-enhancing impact of the holding company structure varies depending on acquiring firm's governance quality and tax avoidance. First, Prior studies show that better corporate governance generally restrict managers from empire-buildings and thus increases the valuation of mergers and acquisitions (e.g., Masulis, Wang, and Xie, 2007; Wang and Xie, 2009). To the extent that a holding company

structure can mitigate the agency problem involving managers, better governance can further amplify such impact and increase firm value. Thus, we expect the value-enhancing impact of a holding company structure to be more pronounced in firms with better governance quality. Consistent with our arguments, we find that the positive relation between acquirers' abnormal returns and holding company status is stronger in firms with better governance quality.

Second, holding companies have more flexibility on managing tax expenses through the allocation of parent and subsidiaries in jurisdictions with better tax treatment. When target firms are acquired, holding company acquirers that already have good tax management can incorporate the new business under their tax management scheme and reduce the potential tax costs for the new business. Thus, we expect that the synergy of incorporating target firms into a holding company is higher in firms with higher tax avoidance before the merger. We use effective tax rate as a measure for tax avoidance to test this prediction. Consistently, we find that the higher abnormal returns of acquirers are more evident in holding company acquirers with higher tax avoidance.

Our paper contributes to the literature in at least two ways. First, we add to the literature on holding company. Prior studies mainly focus on bank holding companies and public utility holding companies in the context of regulations and deregulations of such holding companies (e.g., Aharony and Swary, 1981; Saunders, Strock, and Travlos, 1990; Ellul and Yerramilli, 2013). Our paper, however, show that the holding company structure is also important in non-financial and non-utilities industries. Particularly, we find that a holding company structure enhances firm value through reducing agency problem involving managers and increasing tax savings. These findings help to explain the rationale

for the existence of many holding companies in these industries.

Our paper also contributes to the literature on mergers and acquisitions. A large number of studies have examined many factors that affect the choice and the valuation of mergers and acquisitions, such as corporate governance (e.g., Masulis, Wang, and Xie, 2007), geographic proximity (e.g., Kang and Kim, 2008), corporate social responsibility (e.g., Deng, Kang, and Low, 2013), culture difference (e.g., Ahern, Daminelli, and Fracassi, 2015). In addition to these factors, our study show that the organizational structure also plays an important role in mergers and acquisitions.

The rest of this paper is organized as follows. In section 2, we describe our sample construction. Section 3 examines the impact of the holding company structure on abnormal returns around the merger announcements and post-merger changes in long-term operating performance. In section 4, we investigate cross-sectional variations of the value-enhancing impact of the holding company structure. Section 5 summarizes and concludes the paper.

II. Data and Sample

Our sample consists of US mergers and acquisitions between 1997 to 2016. The initial sample of mergers and acquisitions comes from Thomson Financial's Securities Data Company (SDC) Platinum database. We include all completed and domestic mergers and acquisitions that meet the following four selection criteria: (1) the deal value disclosed in SDC is greater than \$1 million, (2) the acquirer holds less than 50% of the target's shares before the announcement and owns 100% after the transaction, (3) the acquirer is publicly traded with stock return and financial data available from the Center for Research in Security Prices (CRSP) and Compustat, respectively, (4) the acquirer is not in the financial

or utilities industries (i.e., firms with primary Standard Industrial Classification (SIC) codes between 6000 and 6999 or between 4900 and 4999). These restrictions result in a sample of 15,965 successful mergers and acquisitions made by 4,762 firms.

We then identify the holding company status of these acquiring firms through manually reading their business description sections of annual reports (i.e., 10-K filings). According to Item 101 of Regulation S-K by the Securities and Exchange Commission (SEC), a firm must describe the year in which the registrant was organized and its form of organization and any material changes in the mode of conducting the business. Following this rule, a firm typically states its holding company status by claiming that "*we are a holding company and thus conduct all operations through subsidiaries*". Due to the variations in expressions, we manually go through the business description section for each firm's annual reports. Among 4,762 acquiring firms, we are able to identify the holding company status of 4,435 firms among which 3,543 firms are non-holding companies, 843 firms are holding companies, and 49 firms have reorganizations between holding company structure and non-holding company structure. Finally, we exclude deals with missing important characteristics of acquiring firm such as total assets, leverage, free cash flow, resulting in a final sample of 12,649 deals made by 3,877 firms.

Panel A of Table 1 presents the sample distribution of 12,649 mergers and acquisitions by year and acquiring industry (two-digit SIC code). The majority of mergers and acquisitions occur in the manufacturing industry and the service industry, accounting for more than 75% deals. The number of mergers in 1990s are larger compared to other years in every industry, consistent with the wave of mergers due to technological innovations.

Panel B of Table 1 presents summary statistics for our sample of 12,649 mergers and

acquisitions. We further divide our sample into two subsamples based on the acquirer's holding company status. Holding company acquirers have significantly higher leverage, free cash flow, and previous market-adjusted returns, while they also have significantly lower Tobin's q . Holding company acquirers also engage in larger mergers and acquisitions and use less stock as payments.

III. Empirical Results

In this section, we provide results for merger announcement returns and long-term operating performance.

3.1. Announcement effects

To test whether a holding company structure enhances firm value, we examine announcement returns for acquiring firms and value-weighted portfolios of the acquirer and the target. We measure abnormal returns by estimating the market model using two hundred trading days of return data ending 11 days before the merger announcement. The CRSP value-weighted return is used as a proxy for the market return. Daily abnormal stock returns are then cumulated to obtain the cumulative abnormal return (CAR) from day t_1 before the merger announcement date to day t_2 after the merger announcement date.

Table 2 reports the CARs for the full sample as well as the subsamples of holding company acquirers and non-holding company acquirers. Panel A presents the CARs for the acquiring firms. The mean and median CAR (-1, 1), CAR (-2, 2), and CAR (-5, 5) are all positive and significant. The subsample analyses show that both holding company acquirers and non-holding acquirers experience significantly positive CARs around the

merger announcement dates but the magnitude of CARs are significantly larger when the acquiring firms are holding companies. Panel B reports the CARs for the target firms. The sample size reduces from 12,649 to 1,680 due to the data availability of target firms. We find that target firms, on average, have even larger announcement returns than acquiring firms. However, unlike the pattern in Panel A for acquiring firms, the difference in target firms' announcement returns between holding company acquirers and non-holding company acquirers are negative and insignificant for CAR (-1, 1) and CAR (-2, 2) in mean tests. Panel C reports the CARs for value-weighted portfolios of the acquirer and the target using market capitalization ten days prior to the announcement date as the weight (Bradley, Desai, and Kim, 1988). We find similar results with CARs of acquiring firms reported in panel A.

Overall, the results in Table 2 show that compared to non-holding company acquirers, holding company acquirers lead to higher announcement returns for acquirers and value-weighted portfolios of the acquirer and the target, suggesting that a holding company structure tend to enhance the valuation of acquiring firms after the merger.

We next present estimates from multivariate ordinary least squares (OLS) regressions using the CAR (-1, 1) as the dependent variable. Our key independent variable of interest is *Holding company*, an indicator that takes the value of one if the acquiring firm is a holding company, and zero otherwise. Following Masulis, Wang, and Xie (2007), we control for various characteristics of acquiring firms such as firm size, leverage, free cash flow, Tobin's q , and previous market-adjusted return. We also control for various deal-specific characteristics such as relative deal size, industry mergers and acquisitions, hostile dummy, high tech dummy, diversifying merger dummy, public target dummy, private

target dummy, all-cash deal dummy, and stock deal dummy. We further include year and industry (two-digit SIC code) fixed effects to control for unobservable time trends and industry-specific characteristics. We winsorize all continuous variables at the 1st and 99th percentiles to alleviate the impact of extreme values.

Table 3 report the estimation results. The dependent variables are CAR (-1, 1) for acquiring firms in columns (1) and (2) and CAR (-1, 1) for value-weighted portfolios of the acquirer and the target in columns (3) and (4). We include acquiring firm characteristics only as control variables in columns (1) and (3) and further include deal-specific characteristics in columns (2) and (4). We find that the coefficient on *Holding company acquirer (indicator)* is positive and significant in all four regressions. For instance, controlling for acquiring firm characteristics and deal-specific characteristics, holding company acquirers experience 0.323% higher abnormal returns than non-holding company acquirers. This result further supports the view that a holding company structure enhances the valuation of firms in mergers.

3.2. Long-term operating performance

In this subsection, we provide further evidence that a holding company structure increases firm value by examining its impact on the post-merger changes in long-term operating performance of the merged firm. We follow the approach of Healy, Palepu, and Ruback (1992) and regress the difference in post-merger operating cash flow between the merged firm and the control firm on the difference in pre-merger operating cash flow between the merged firm and the control firm separately in two subsamples of acquiring firms with and without holding company structures. The constant term of the regression measures the abnormal change in operating cash flow between the pre-merger and post-

merger periods. The combined firm's pre-merger operating performance is weighted operating cash flow over the assets of the acquirer and the target prior to the merger using the acquirer's and the target's relative total assets as the weights. The pre- and post-merger are the two years prior to and after the event year, respectively.

We match each acquirer in our sample with a non-acquirer that is in Compustat using a propensity score matching approach. The variables used in the matching are firm size, leverage, and Tobin's q. We require matching firms to be in the same industry (two-digit SIC code) and in the same year and have same holding company status as the acquirer firms. These procedures result in 106 deals by holding company acquirers and 569 deals by non-holding company acquirers.

The results are presented in Table 4. We find that there are significant increases in post-merger periods for both subsamples, but the constant term for holding company acquirers (0.033) is higher than that for non-holding company acquirers (0.018). This result suggests that a holding company structure leads to more increase in post-merger operating performance, further supporting our expectation that a holding company structure enhances firm value.

IV. Cross-Sectional Heterogeneity of Announcement Returns

In this section, we investigate how merger announcement returns varies across acquiring firms' governance quality and tax avoidance.

4.1. Corporate governance quality

We first examine whether the impact of the holding company structure on the

valuation of mergers is different in firms with good governance and poor governance. Masulis, Wang, and Xie (2007) show that acquirers with more antitakeover provisions experience significantly lower announcement abnormal stock returns while acquirers operating in more competitive industries or separating the positions of CEO and chairman of the board experience higher abnormal announcement returns, suggesting that better governance has positive impact on the value-enhancing mergers by mitigating the manager-shareholder conflict of interests. If a holding company structure indeed reduces the agency problem involving managers, such impact should be stronger in firms with better governance. Thus, we expect that abnormal announcement returns of the acquirers are higher in holding company acquirers with better governance.

We use four measures of a firm's governance quality: the proportion of outside directors on the board (the ratio of the number of outside directors to the total number of directors on the board), the proportion of old directors on the board (the ratio of the number of directors who are over 72 years old to the total number of directors), the existence of CEO-Chair duality, and CEO's ownership. We divide the *Holding company acquirer (indicator)* into two indicators based on acquiring firm's governance quality measures. Specifically, we define *Holding company acquirer with better (poorer) governance* as an indicator that takes the value of one if the proportion of outside directors on the board of a holding company acquirer is above (below) the sample median, the proportion of old directors of a holding company acquirer is below (above) the sample median, a holding company acquirer's CEO does not (do) serve as Chairperson, or the CEO ownership of a holding company acquirer is above (below) the sample median, and zero otherwise.

Table 5 report the results. We find that the coefficients on *Holding company acquirer*

with better governance are positive and significant in all four regressions, while the coefficients on *Holding company acquirer with poorer governance* are insignificant, though the difference between these two coefficients are not significant. These results provide weak evidence that better-governed acquirers with holding company structures creates more value-enhancing in the mergers.

4.2. Tax avoidance

We next examine how the value-enhancing impact changes along with firms' tax management. The advantage of flexible tax planning grants holding companies much benefit on tax savings. For example, Dyreng, Lindsey, and Thornock (2013) estimate that a Delaware-based state tax avoidance, typically a formation of Delaware holding company, lowers state effective tax rates by between 0.7 and 1.1 percent, on average. When target firms are acquired by such holding companies, the new business is integrated into the tax management and may also enjoy better tax treatment afterwards. As a result, the synergy of mergers should be higher for acquiring firms with better tax management due to less potential tax costs in future operations. We expect that abnormal returns to be higher for holding company acquirers with higher tax avoidance. We use effective tax rate of acquiring firms before the merger as a proxy for firms' tax avoidance. Effective tax rate is calculated as total tax expenses divided by pre-tax income adjusted for special items. We then divide holding company acquirers into two groups based on their effective tax rates. Specifically, we replace *Holding company acquirer (indicator)* with two indicators: *Holding company acquirer with higher tax avoidance* and *Holding company acquirer with lower tax avoidance*, where *Holding company acquirer with higher (lower) tax avoidance*

is an indicator that takes the value of one if the effective tax rate of a holding company acquirer is below (above) the sample median, and zero otherwise.

Table 6 reports the results. We find the coefficient on *Holding company acquirer with higher tax avoidance* is 0.497 and significant at the 5% level, while the coefficient on *Holding company acquirer with lower tax avoidance* is insignificant. Though the difference between these two coefficients are not significant, this result provides some weak evidence that the synergy is higher in firms with better tax management.

V. Summary and Conclusion

In this paper, we investigate how a holding company structure affects the valuation of firms in non-financial and non-utilities industries by exploiting mergers and acquisitions made by holding companies. In a holding company, the parent firm does not conduct day-to-day operations but oversee the operating of subsidiaries and make strategic plans about operating, financing, and investment. Such clear division of working duties can hold better accountability for management teams in both parent and subsidiaries, and cause less distraction for them, which help to mitigate the manager-shareholder conflict of interests. A holding company structure also enjoy the benefits of tax savings through strategically allocating different business in suitable jurisdictions with better tax treatment. Consistent with our argument, we find that acquiring firms with holding company structures experience significantly higher abnormal stock returns than acquiring firms without holding company structures. The results are robust after controlling for various firm- and deal-specific characteristics. We also find that post-merger changes in long-term operating performance are also higher in holding company acquirers. Turning to the source of the

value creation, we find that such positive relation between announcement abnormal returns and acquirers' holding company status are more pronounced in firms with better governance and higher tax avoidance.

Overall, our findings provide some evidence on the benefit of a holding company structure in non-financial and non-utilities industries and thus provide the rationale for the existence of many holding companies in these industries.

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Table 1
Sample distribution and summary statistics

This table presents sample distribution and summary statistics for 12,649 U.S. mergers and acquisitions during 1997 to 2016. Our sample includes all completed mergers and acquisitions that meet the following selection criteria: (1) the deal value disclosed in SDC is greater than \$1 million, (2) the acquirer holds less than 50% of the target's shares prior to the announcement and owns 100% after the transaction, (3) the acquirer is publicly traded in NYSE, Nasdaq, and Amex exchanges with CRSP share code 10 or 11 (i.e. common shares) and has stock return and financial data available from CRSP and Compustat, respectively, (4) the acquirer is not in the financial or utilities industries. The appendix provides detailed descriptions of the construction of the variables. ***, **, and * denote that *t*-tests for mean differences and Wilcoxon z-test for median differences are significant at the 1%, 5%, and 10% levels, respectively.

Panel A: Sample distribution by year and acquirer industry (first two digits of the SIC code)

Year	Agriculture, forestry, and fisheries (01-09)	Mineral industries and construction (10-17)	Manufacturing (20-39)	Transportation and communications (40-48)	Wholesale trade and retail trade (50-59)	Service industries (70-89)	Total
1997	0	115	494	139	155	405	1,308
1998	0	108	497	114	152	397	1,268
1999	0	50	455	134	92	323	1,054
2000	0	49	424	87	63	287	910
2001	0	46	297	64	36	188	631
2002	0	39	279	42	39	183	582
2003	0	39	289	39	43	187	597
2004	0	47	334	41	41	213	676
2005	0	43	302	48	59	213	665
2006	0	57	320	56	58	162	653
2007	0	56	297	52	54	198	657
2008	0	44	216	29	33	149	471
2009	0	23	197	19	13	95	347
2010	0	39	208	20	21	112	400
2011	0	31	198	36	29	117	411
2012	0	36	206	40	34	123	439
2013	0	25	182	43	30	105	385
2014	0	31	225	40	26	130	452
2015	0	27	201	34	39	115	416
2016	0	36	169	21	24	77	327
Total	0	941	5,790	1,098	1,041	3,779	12,649

Panel B: Summary statistics

Variable	Full sample (N=12,649)		Subsample of holding company acquirers (N=2,879): A		Subsample of non-holding company acquirers (N=9,770): B		Test of difference (A-B)	
	Mean	Median	Mean	Median	Mean	Median	Mean	Median
Total asset (\$ millions)	5.457	0.585	5.424	0.825	5.466	0.520	-0.042	0.305***
Tobin's <i>q</i>	2.444	1.773	2.010	1.609	2.572	1.838	-0.563***	-0.229***
Leverage	0.142	0.104	0.201	0.176	0.125	0.085	0.076***	0.091***
Free cash flow	0.005	0.042	0.016	0.039	0.001	0.044	0.015***	-0.005*
Previous market-adjusted return	0.024	0.020	0.050	0.050	0.016	0.012	0.034***	0.038***
Relative deal size	0.207	0.067	0.248	0.076	0.196	0.065	0.052***	0.011***
Industry M&A	0.045	0.030	0.045	0.029	0.045	0.030	0.000	-0.001***
Hostile (indicator)	0.001	0.000	0.001	0.000	0.001	0.000	0.000	0.000
High tech (indicator)	0.280	0.000	0.161	0.000	0.315	0.000	-0.153***	0.000***
Diversifying merger (indicator)	0.395	0.000	0.381	0.000	0.399	0.000	-0.018*	0.000*
Public target (indicator)	0.153	0.000	0.151	0.000	0.153	0.000	-0.002	0.000
Private target (indicator)	0.532	1.000	0.494	0.000	0.544	1.000	-0.050***	-1.000***
Stock deal (indicator)	0.300	0.000	0.251	0.000	0.315	0.000	-0.064***	0.000***
All-cash deal (indicator)	0.764	1.000	0.759	1.000	0.765	1.000	-0.007	0.000

Table 2
Cumulative abnormal returns (CARs) around announcement dates

This table presents univariate tests of cumulative abnormal returns of acquirers, targets, and value-weighted portfolios of the acquirer and the target around announcement dates in a sample of 12,649 U.S. mergers and acquisitions during 1997 to 2016. Our sample includes all completed mergers and acquisitions that meet the following selection criteria: (1) the deal value disclosed in SDC is greater than \$1 million, (2) the acquirer holds less than 50% of the target's shares prior to the announcement and owns 100% after the transaction, (3) the acquirer is publicly traded in NYSE, Nasdaq, and Amex exchanges with CRSP share code 10 or 11 (i.e. common shares) and has stock return and financial data available from CRSP and Compustat, respectively, (4) the acquirer is not in the financial or utilities industries. The abnormal return is calculated using the market model. The market model parameters are estimated using two hundred trading days of return data ending 11 days before the announcement date. The CRSP value-weighted return is used as a proxy for the market return. The daily abnormal returns are cumulated to obtain the cumulative abnormal return (CAR) from day t_1 before the announcement date to day t_2 after the announcement date. The value-weighted portfolio CARs are the market-capitalization-weighted CARs of the acquirer and the target, where the market capitalization is measured at the 11th trading day prior to the announcement date. ***, **, and * denote that t -tests for mean differences and Wilcoxon z-test for median differences are significant at the 1%, 5%, and 10% levels, respectively.

Panel A: Acquirer CARs (in percent)								
	Full sample (N=12,649)		Subsample of holding company acquirers (N=2,879): A		Subsample of non- holding company acquirers (N=9,770): B		Test of difference (A-B)	
	Mean	Median	Mean	Median	Mean	Median	Mean	Median
CAR (-1, 1)	1.107***	0.463***	1.457***	0.635***	1.004***	0.407***	0.453***	0.228***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.005)	(0.002)
CAR (-2, 2)	1.172***	0.476***	1.580***	0.738***	1.052***	0.410***	0.528***	0.328***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.004)	(0.001)
CAR (-5, 5)	0.997***	0.504***	1.528***	0.861***	0.841***	0.404***	0.687***	0.457**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.004)	(0.012)
Panel B: Target CARs (in percent)								
	Full sample (N=1,680)		Subsample of holding company acquirers (N=379): A		Subsample of non- holding company acquirers (N=1,301): B		Test of difference (A-B)	
	Mean	Median	Mean	Median	Mean	Median	Mean	Median
CAR (-1, 1)	25.473***	20.633***	23.862***	18.411***	25.943***	21.257***	-2.081	-2.846*
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.178)	(0.062)
CAR (-2, 2)	26.371***	21.567***	24.415***	19.653***	26.941***	22.372***	-2.526	-2.719**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.107)	(0.034)
CAR (-5, 5)	27.839***	23.104***	25.386***	20.294***	28.554***	24.439***	-3.168*	-4.145**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.053)	(0.012)
Panel C: Value-weighted portfolio CARs (in percent) of the acquirer and the target								
	Full sample (N=1,680)		Subsample of holding company acquirers (N=379): A		Subsample of non- holding company acquirers (N=1,301): B		Test of difference (A-B)	
	Mean	Median	Mean	Median	Mean	Median	Mean	Median
CAR (-1, 1)	1.674***	1.043***	2.811***	1.707***	1.343***	0.827***	1.468***	0.880***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)
CAR (-2, 2)	1.627***	1.068***	2.653***	1.889***	1.329***	0.826***	1.324***	1.063***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.006)	(0.006)
CAR (-5, 5)	1.799***	1.597***	2.748***	2.085***	1.522***	1.430***	1.227**	0.654*

5)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.037)	(0.051)
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Table 3
Regressions of cumulative abnormal return (-1, 1) on acquirers' holding company status

This table presents regressions of cumulative abnormal returns (-1, 1) around announcement dates on acquirers' holding company status and other control variables in a sample of 12,649 U.S. mergers and acquisitions during 1997 to 2016. Our sample includes all completed mergers and acquisitions that meet the following selection criteria: (1) the deal value disclosed in SDC is greater than \$1 million, (2) the acquirer holds less than 50% of the target's shares prior to the announcement and owns 100% after the transaction, (3) the acquirer is publicly traded in NYSE, Nasdaq, and Amex exchanges with CRSP share code 10 or 11 (i.e. common shares) and has stock return and financial data available from CRSP and Compustat, respectively, (4) the acquirer is not in the financial or utilities industries. The dependent variable is cumulative abnormal return of the acquirer (in percent) from one day before to one day after the merger announcement date in columns (1) and (2), and cumulative abnormal return of the market-capitalization-weighted portfolio of the acquirer and the target from one day before to one day after the merger announcement in columns (3) and (4). The abnormal return is calculated using the market model. The market model parameters are estimated using two hundred trading days of return data ending 11 days before the announcement date. The CRSP value-weighted return is used as a proxy for the market return. Two-digit standard industrial classification (SIC) code dummies and calendar year dummies are included to control industry and year fixed effects, respectively. The appendix provides detailed descriptions of the construction of the variables. All continuous variables are winsorized at the 1st and 99th percentiles. *P*-values reported in parentheses are based on standard errors adjusted for heteroskedasticity and clustered at the acquirer level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	Dependent variable = CAR (-1, 1)			
	Acquirers		Value-weighted portfolios of the acquirer and the target	
	(1)	(2)	(3)	(4)
Holding company acquirer (indicator)	0.373** (0.035)	0.323* (0.065)	1.003** (0.024)	0.694* (0.099)
Firm size	-0.513*** (0.000)	-0.273*** (0.000)	-0.530*** (0.000)	-0.438*** (0.000)
Leverage	2.976*** (0.000)	1.064 (0.113)	4.824** (0.015)	3.310* (0.076)
Free cash flow	-0.036 (0.960)	-0.237 (0.743)	4.359** (0.017)	2.207 (0.222)
Tobin's <i>q</i>	-0.048 (0.387)	-0.008 (0.884)	-0.429*** (0.002)	-0.263* (0.069)
Previous market-adjusted return	-1.283*** (0.000)	-0.987*** (0.000)	-1.995*** (0.002)	-1.401** (0.027)
Relative deal size		2.814*** (0.000)		2.553*** (0.000)
Industry M&A		-1.874 (0.238)		-3.718 (0.394)
Hostile (indicator)		-0.762 (0.654)		1.725 (0.403)
High tech (indicator)		-0.116 (0.598)		-0.715 (0.207)
Diversifying merger (indicator)		-0.002 (0.988)		-0.034 (0.929)
Public target (indicator)		-2.675*** (0.000)		-0.164 (0.921)
Private target (indicator)		-0.308** (0.048)		-2.013 (0.583)
All-cash deal (indicator)		-0.026 (0.891)		-1.380*** (0.003)
Stock deal (indicator)		-0.450** (0.034)		-3.331*** (0.000)
Year fixed effects	Yes	Yes	Yes	Yes
Industry fixed effects (two-digit SIC)	Yes	Yes	Yes	Yes
Adjusted <i>R</i> ²	0.026	0.048	0.096	0.143
Observations	12,649	12,649	1,680	1,680

Table 4
Post-merger changes in long-term operating performance of the merged firms

This table presents regressions of post-merger changes in merged firms' operating performance in a sample of 675 U.S. mergers and acquisitions during 1997 to 2016. Our sample includes all completed mergers and acquisitions that meet the following selection criteria: (1) the deal value disclosed in SDC is greater than \$1 million, (2) the acquirer holds less than 50% of the target's shares prior to the announcement and owns 100% after the transaction, (3) the acquirer is publicly traded in NYSE, Nasdaq, and Amex exchanges with CRSP share code 10 or 11 (i.e. common shares) and has stock return and financial data available from CRSP and Compustat, respectively, (4) the acquirer is not in the financial or utilities industries. The dependent variable is the difference in the change in post-merger operating performance between the combined (acquirer and target) firm and the control firm. The combined firm's pre-merger operating performance is weighted operating cash flow over the assets of the acquirer and target prior to the merger using the acquirer and target's relative total assets as the weights. The pre- and post-merger are the two years prior to and after the event year, respectively. We match each acquirer in our sample with a non-acquirer that is in Compustat using a propensity score matching approach. The variables used in the matching are firm size, leverage, and Tobin's q . We require matching non-acquirer firms to be in the same industry (two-digit SIC code) and in the same year and have same holding company status as the acquirer firms. The constant of the regression measures the abnormal change in operating performance between the pre-merger and post-merger periods. P -values reported in parentheses are based on standard errors clustered at the acquirer level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	Dependent variable = Difference in post-merger operating performance between the combined firm and the control firm	
	Holding company acquirers	Non-holding company acquirers
	(1)	(2)
Constant	0.033** (0.023)	0.018** (0.017)
Difference in pre-merger operating performance between the combined firm and the control firm	0.715*** (0.000)	0.603*** (0.000)
Adjusted R^2	0.281	0.186
Observations	106	569

Table 5
Regressions of cumulative abnormal return (-1, 1) of acquiring firms by governance quality

This table presents regressions of cumulative abnormal return (-1, 1) of acquirer firms in a sample of 10,209 U.S. mergers and acquisitions during 1997 to 2016. Our sample includes all completed mergers and acquisitions that meet the following selection criteria: (1) the deal value disclosed in SDC is greater than \$1 million, (2) the acquirer holds less than 50% of the target's shares prior to the announcement and owns 100% after the transaction, (3) the acquirer is publicly traded in NYSE, Nasdaq, and Amex exchanges with CRSP share code 10 or 11 (i.e. common shares) and has stock return and financial data available from CRSP and Compustat, respectively, (4) the acquirer is not in the financial or utilities industries. The dependent variable is the cumulative abnormal return of the acquirer from one day before to one day after the merger announcement date. The abnormal return is calculated using the market model. The market model parameters are estimated using two hundred trading days of return data ending 11 days before the merger announcement. The CRSP value-weighted return is used as a proxy for the market return. *Holding company acquirer with better (poorer) governance* is an indicator that takes the value of one if the proportion of outside directors on the board of a holding company acquirer is above (below) the sample median, the proportion of old directors of a holding company acquirer is below (above) the sample median, a holding company acquirer's CEO does not (do) serve as Chairperson, or the CEO ownership of a holding company acquirer is above (below) the sample median, and zero otherwise. The appendix provides detailed descriptions of the construction of the variables. All continuous variables are winsorized at the 1st and 99th percentiles. *P*-values reported in parentheses are based on standard errors clustered at the acquirer level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	Proportion of outside directors	Proportion of old directors	CEO- Chair duality	CEO ownership
	(1)	(2)	(3)	
Holding company acquirer with better governance: a	0.587**	0.494**	0.428*	0.483*
	(0.016)	(0.021)	(0.068)	(0.091)
Holding company acquirer with poorer governance: b	0.207	0.185	0.374	0.021
	(0.369)	(0.492)	(0.102)	(0.936)
Control variables	Yes	Yes	Yes	Yes
<i>P</i> -value for the test of a = b	0.212	0.322	0.854	0.198
Year fixed effects	Yes	Yes	Yes	Yes
Industry fixed effects (two-digit SIC)	Yes	Yes	Yes	Yes
Adjusted <i>R</i> ²	0.047	0.047	0.045	0.048
Observations	10,209	10,209	10,290	6,630

Table 6
Regressions of cumulative abnormal return (-1, 1) of acquiring firms by tax avoidance

This table presents regressions of cumulative abnormal return (-1, 1) of acquirer firms in a sample of 10,209 U.S. mergers and acquisitions during 1997 to 2016. Our sample includes all completed mergers and acquisitions that meet the following selection criteria: (1) the deal value disclosed in SDC is greater than \$1 million, (2) the acquirer holds less than 50% of the target's shares prior to the announcement and owns 100% after the transaction, (3) the acquirer is publicly traded in NYSE, Nasdaq, and Amex exchanges with CRSP share code 10 or 11 (i.e. common shares) and has stock return and financial data available from CRSP and Compustat, respectively, (4) the acquirer is not in the financial or utilities industries. The dependent variable is the cumulative abnormal return of the acquirer from one day before to one day after the merger announcement date. The abnormal return is calculated using the market model. The market model parameters are estimated using two hundred trading days of return data ending 11 days before the merger announcement. The CRSP value-weighted return is used as a proxy for the market return. Effective tax rate is calculated as total tax expenses divided by pre-tax income adjusted for special items. *Holding company acquirer with higher (lower) tax avoidance* is an indicator that takes the value of one if the effective tax rate of a holding company acquirer is below (above) the sample median, and zero otherwise. The appendix provides detailed descriptions of the construction of the variables. All continuous variables are winsorized at the 1st and 99th percentiles. *P*-values reported in parentheses are based on standard errors clustered at the acquirer level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	Effective tax rate
	(1)
Holding company acquirer with higher tax avoidance: a	0.497** (0.040)
Holding company acquirer with lower tax avoidance: b	0.097 0.656
Control variables	Yes
P-value for the test of a = b	0.177
Year fixed effects	Yes
Industry fixed effects (two-digit SIC)	Yes
Adjusted R^2	0.048
Observations	12,461

Appendix

The Appendix provides detailed descriptions of all variables used in the tables.

Variable	Definition	Source
All-cash deal (indicator)	One if the deal is purely cash-financed and zero otherwise.	SDC
CAR (t_1 , t_2)	Cumulative abnormal return from day t_1 before the announcement date to day t_2 after the announcement date. Abnormal returns are measured by estimating the market model using 200 trading days of return data ending 11 days before the announcement date. The CRSP value-weighted return is used as a proxy for the market return	CRSP, SDC
CEO-Chair duality (indicator)	One if the CEO also serves as the chairperson of the board, and zero otherwise	BoardEx
CEO ownership	Ratio of shares held by CEO to the total shares outstanding	ExecuComp
Diversifying merger (indicator)	One if the acquirer and the target have different first two-digit standard industrial classification codes and zero otherwise	SDC
Firm size	The natural logarithm of total assets	Compustat
Free cash flow	Operating income before depreciation – interest expense – income taxes – capital expenditures, scaled by book value of total assets	Compustat
High tech (indicator)	One if the acquirer and the target operate in both high-tech industries defined by Loughran and Ritter (2004) and zero otherwise	SDC
Holding company acquirer (indicator)	One if an acquirer is a holding company before the announcement date of a M&A transaction. A holding company is defined as a firm which has no operation by itself but conducts all operations through its subsidiaries	10-K
Holding company acquirer with better (poorer) governance (indicator)	One if the proportion of outside directors on the board of a holding company acquirer is above (below) the sample median, the proportion of old directors of a holding company acquirer is below (above) the sample median, a holding company acquirer's CEO does not (do) serve as Chairperson, or the CEO ownership of a holding company acquirer is above (below) the sample median, and zero otherwise	BoardEx, ExecuComp
Holding company acquirer with higher (lower) tax avoidance (indicator)	One if the effective tax rate of a holding company acquirer is below (above) the sample median, and zero otherwise	Compustat
Hostile (indicator)	One if the deal is reported as hostile in SDC and zero otherwise	SDC
Industry M&A	The value of all corporate control transactions with disclosed value more than \$1 million in SDC for each prior year and two-digit SIC industry divided by the total book value of assets of all Compustat firms in the same two-digit SIC industry and year	SDC
Leverage	The ratio of book value of debts (the sum of long-term debt and short-term debt) to market value of total assets (book value of total assets minus book value of equity plus market value of equity)	Compustat
Effective tax rate	Total tax expenses divided by pre-tax income adjusted for special items	
Previous market-adjusted return	Acquirer's buy-and-hold abnormal stock return using 200 trading days of return data ending 11 days before the announcement date. The CRSP value-weighted return is used as a proxy for the market return	CRSP
Private target (indicator)	One if the target is a private firm and zero otherwise	SDC
Proportion of old directors	Ratio of the number of directors who are over 72 years old to the total number of directors on the board	BoardEx
Proportion of outside directors	Ratio of the number of outside directors to the total number of directors on the board	BoardEx
Public target (indicator)	One if the target is a public firm and zero otherwise	SDC
Relative deal size	Deal value reported in SDC over market value of acquirer equity. Market value of equity is calculated as number of shares outstanding multiplied by the stock price at the 11 th trading day prior to the announcement date	CRSP, SDC

Variable	Definition	Source
Stock deal (indicator)	One if the deal is at least partially financed by stocks and zero otherwise	SDC
Tobin's q	The ratio of market value of total assets (book value of total assets minus book value of equity plus market value of equity) to book value of total assets	Compustat