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Essays on Corporate Debt Structure

Kyuyoung Oh
University of Kentucky, kyuohfinance@gmail.com
Author ORCID Identifier:
https://orcid.org/0009-0006-8254-0634
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Kyuyoung Oh, Student

Dr. Kristine W. Hankins, Major Professor

Dr. Russell Jame, Director of Graduate Studies

ESSAYS ON CORPORATE DEBT STRUCTURE

DISSERTATION

A dissertation submitted in partial fulfillment of the
requirements for the degree of Doctor of Philosophy
in the Gatton College of Business and Economics
at the University of Kentucky

By
Kyuyoung Oh
Lexington, Kentucky

Director: Dr. Kristine W. Hankins, Professor of Finance
Lexington, Kentucky
2024

https://orcid.org/0009-0006-8254-0634

ABSTRACT OF DISSERTATION

ESSAYS ON CORPORATE DEBT STRUCTURE

The dissertation consists of three chapters exploring three different facets of corporate debt structure choice. In the first chapter, “Risk Management and the Choice between Secured and Unsecured Debt: Evidence from Natural Experiment,” I study whether and how corporate hedging affects firms’ choice between secured and unsecured debt. Exploiting the introduction of steel futures as a natural experiment, I provide causal evidence that risk management enables firms to switch from secured to unsecured debt without sacrificing debt capacity. Cross-sectional evidence supports the interpretation that risk management drives the results. The effects are stronger for firms that are more likely to engage in financial hedging, that derive relatively less benefit or face higher costs from employing secured debt, and that are less financially healthy but with some collateral capacity for financial hedging. To the extent that secured debt financing is associated with a loss of operating flexibility or future financing slack for borrowing firms, my findings suggest a potential channel through which risk management increases firm value that could be masked when heterogeneous debt types are treated uniformly. In the second chapter, “Stock Price Informativeness and Debt Heterogeneity,” I ask whether and how stock price informativeness (SPI) affects corporate debt heterogeneity, defined as the degree of dispersion in debt choice. I document strong evidence that SPI is positively related to a more heterogeneous debt structure. A battery of tests, including a quasi-natural experiment and an IV-2SLS analysis, supports causal interpretation. Cross-sectional evidence reveals that the relation is stronger for firms with higher expected financial distress costs and a higher degree of information asymmetry, consistent with the notion that the reduction in distress costs and alleviation of information asymmetry are the main mechanisms. My findings add to the growing literature on the real effects of financial markets by showing that SPI significantly influences firms’ debt concentration structure adjustment through the channels distinct from the learning channel. In the third chapter, “Government Subsidies and the Choice between Bank and Public Debt,” I ask whether and how government subsidies to U.S. corporations – a growing area of policy interest – affect firms’ choice between bank and public debt. I find that subsidized firms shift their debt financing structure toward more public debt and away from bank debt. These key findings are robust to alternative regression model specification as well as an IV approach exploiting changes in powerful congressional committee chairmanships. Cross-sectional tests reveal that the documented effect of government subsidies on the shift toward public debt is amplified among firms with better governance, severe information asymmetry, and more positive political sentiment, consistent with the notion that enhanced external scrutiny and increased perceived credibility stemming from endorsement effects diminish the need for the traditional monitoring and informational roles of bank debt.

KEYWORDS: Debt Structure, Capital Structure, Agency Costs of Debt,
Risk Management, Stock Price Informativeness, Government Subsidies

Author's signature: Kyuyoung Oh

Date: April 17, 2024

ESSAYS ON CORPORATE DEBT STRUCTURE

By
Kyuyoung Oh

Kristine Hankins
Director of Dissertation

Russell Jame
Director of Graduate Studies

April 17, 2024
Date

To my wife, son, parents, parents-in-law, and extended family;
This dissertation is a tribute to your unconditional love, endless support, and faith in every step of my academic journey.

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Reflecting on this moment as I close the acknowledgments section, I am also reminded of my cherished memories with my grandfather, Yu Kwon Oh, and maternal grandmother, Won Jae Lee, who first affectionately dubbed me “Dr. Oh” when I was merely six. I believe that if they were here to see this dissertation, they would have been very proud. They should be very pleased to know that I have lived up to the affectionate nickname they gave me.

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CHAPTER 1. Risk Management and the Choice between Secured & Unsecured Debt: Evidence from Natural Experiment

1.1 Introduction

Does corporate risk management affect debt structure? A large body of theoretical and empirical literature has suggested potential links between hedging and capital structure, emphasizing the benefits of hedging in alleviating capital market frictions and thus allowing for more or cheaper debt financing (Smith and Stulz, 1985; Bartram et al., 2011; Chen and King, 2014). However, as emphasized by Rauh and Sufi (2010), a firm's debt often consists of different types of instruments with different contractual features such as security, seniority, or control provisions, all of which have implications on firm value beyond their relative impact on the interest rate on debt due to either direct or indirect restrictions they impose on borrowing firms. Therefore, ignoring such debt heterogeneity can mask additional benefits or costs that hedging confers or imposes on firms beyond debt capacity and the cost of debt, and direct empirical evidence on how risk management interacts with the choice between different debt types is scarce in the existing literature. In this paper, I try to fill this gap by investigating the relationship between corporate risk management and debt structure choice. Specifically, I examine whether and how corporate hedging affects the choice between secured and unsecured debt.

My focus on debt security choice among other facets of looking into a firm's debt structure is motivated by the potential link between the cost-benefit tradeoff of secured debt usage and corporate hedging suggested by the "lender protection hypothesis" of Smith and Warner (1979). According to Smith and Warner (1979), legal restrictions imposed on collateralized assets protect lenders from potential expropriative actions by borrowing firms, leading to a reduction in the cost of borrowing. At the same time, however, borrowers need to sacrifice some degree of their operational flexibility because they are restricted from selling or issuing additional claims against the pledged assets. In a competitive lending market, borrowing firms pledge collateral and issue secured debt only when the benefits of doing so outweigh the costs. Hence, to the extent that hedging reduces the volatility of cash flows and lowers the risk of financial distress, the costs of secured debt usage in the form of the loss of operating flexibility or financing slack may outweigh the benefits if lenders perceive hedging firms as having less threat of potential expropriative actions and accordingly do not offer substantially lower interest rates on secured loans relative to unsecured. Indeed, recent work by Benmelech et al. (2022) finds that lenders are not willing to provide lower credit spreads for secured debt relative to unsecured debt during normal economic conditions or when the borrowing firm is financially healthy. Therefore, I predict that corporate hedging should lead firms to rely less on secured debt and more on unsecured debt.

Merely adopting the standard OLS regression in assessing the effect of corporate hedging activities on firm secured debt usage is subject to typical endogeneity concerns. For example, unobservable firm-specific investment opportunities can drive both hedging and secured debt financing decisions (i.e., the omitted variable concern). In addition, a large amount of secured debt already sitting on the balance sheet may weaken the firm’s incentive hedge, which is a reverse causality concern.

To overcome endogeneity concerns, I exploit the introduction of steel futures as a natural experiment in a similar spirit to Almeida et al. (2017), and provide causal evidence on how corporate hedging affects firms’ choice between secured and unsecured debt. The introduction of exchange-traded futures contract represents an exogenous increase in the ability to hedge, or decrease in the cost of hedging, steel price risk, irrespective of individual firm's investment opportunities. This, in turn, disproportionately benefits firms with a more steel price-sensitive cost structure. The setting enables me to implement a difference-in-differences (DiD) design under the assumptions that the classification into the treatment and control groups based on the sensitivity to steel price risk can be accurately done and both groups are similar except for their ability to hedge steel price risk post-introduction.

The results from the DiD test are consistent with the prediction that risk management should lead firms to rely less on secured debt and more on unsecured debt. I find that steel exposed firms (i.e., treated firms) decrease their secured debt usage relative to less steel exposed firms (i.e., control firms) and substitute unsecured debt for secured debt after the introduction of steel futures. The magnitudes of the effect are both statistically and economically significant. For example, steel exposed firms decrease the use of secured debt relative to assets (total debt) by 1.6%p to 1.7%p (4.5%p) more than non-exposed firms after the introduction of steel futures. Given that the sample average of the secured debt to asset ratio and of the secured debt to total debt ratio are 8.8% and 35.8%, respectively, those coefficients represent about an 18.2% relative decrease in secured debt to asset ratio and about a 12.6% relative decrease in secured debt to total debt ratio for treated firms. Moreover, while unsecured debt to asset ratio increases by 1.0%p to 1.1%p for treated firms relative to control firms after the introduction of steel futures, I do not find a statistically meaningful differential response for total debt to asset ratio. These findings are consistent with steel exposed firms' switching from the secured to the unsecured form of debt without sacrificing their debt capacity.

I find no significant violations of the core assumptions of difference-in-differences design in my setting, and the results are similar after I conduct a matched DiD analysis using propensity score matching (PSM). Moreover, further corroborating my interpretation of the results that risk management enables firms to choose unsecured over secured debt and leads them to decrease the relative proportion of secured debt, an analysis looking at significant debt issuance events reveals that steel exposed firms are significantly more likely to issue unsecured debt in a large-scale (i.e., more than 1% of lagged book assets) relative to non-exposed firms after the introduction of steel futures.

As channel analysis, I first explore implications of hedging on several firm-level outcomes to shed light on the proposed chain of mechanisms whereby firms engage in risk management, which stabilizes cash flow, ultimately leading to a reduction in default risk and cost of financial distress. I find evidence consistent with steel exposed firms' disclosing using commodity derivatives in their 10-K filings more relative to control firms after the introduction of steel futures. Moreover, steel exposed firms experience a pronounced decrease in both cash flow volatility and default probability after the steel futures introduction, reinforcing the assertion that the main results of the paper indeed stem from steel exposed firms' exploiting steel futures instrument for hedging.

Next, I present cross-sectional evidence from several subsample analyses to see from which part of the sample the statistically significant effects revealed from the DiD test primarily come out. The results reveal that the effects are mainly driven by firms with a higher propensity or incentive to engage in financial hedging, that derive relatively less benefit from using secured debt or more fragile to a loss in operating flexibility imposed by the secured debt contracts, and that are financially less healthy but with some collateral capacity to be able to engage in financial hedging. Overall, the results of cross-sectional analysis lend support to the argument that the decrease in secured debt usage by treated firms indeed reflects the effect of risk management and ensuing alteration in the trade-off between the benefits and costs of secured debt usage as implied by the lender protection hypothesis.

This paper contributes to three strands of literature. First, it adds to the literature on the determinants of corporate debt structure choice, especially the choice of whether to issue debt secured or unsecured. Even though several theoretical studies have guided why firms would choose to issue secured debt (Myers, 1977; Smith and Warner, 1979; Stulz and Johnson, 1985), direct empirical evidence on the determinants of secured debt has been relatively scarce in the literature. Exploiting natural experiment, I provide causal evidence that the availability of hedging option results in less reliance on secured debt, suggesting firms' engagement in risk management practices as one of the determinants of the secured/unsecured mix of corporate debt.

Second, it contributes to the literature on risk management and value implications of corporate hedging activities. Several studies on corporate hedging have identified increased productivity (Cornaggia, 2013), higher debt capacity and investment (Perez-Gonzalez and Yun, 2013; Chen and King, 2014; Biguri et al., 2022), and reduction in the probability of financial distress and underinvestment risk (e.g., Gilje and Taillard, 2017) as channels through which hedging affects firm value. In addition to the list of intermediate channels introduced by the literature so far as above, I uncover a potential channel through which risk management increases firm value that could be masked when heterogeneous debt types are treated uniformly. That is, hedging enables firms to replace secured debt with unsecured without sacrificing debt capacity. Conceptually, then, hedging firms can enjoy higher flexibility in operation, conserve more financial slack for future financing, and face fewer restrictions for a given level of debt, which, all else equal, should factor into an increase in firm value. Recent findings by Benmelech et al. (2022) on firm risk- or macro condition-contingent pricing of secured debt, along with my natural experiment results, render support to the link between risk management and firm value through this potential channel of debt security structure choice.

Lastly, it sheds light on the nascent literature on the secular decline of secured debt usage by firms. Benmelech et al. (2024) document that U.S. firms' reliance on secured debt has declined steadily over time. They suggest developments in accounting, contract law, and bankruptcy law as potential explanations for the decline of secured debt. My findings provide suggestive evidence that financial innovation in the form of exchange-traded futures instruments that help firms hedge their risks may also be one of the contributors that can account for firms' relying less on secured debt over time. Moreover, the lender protection hypothesis, the main economic mechanism on which I build empirical predictions and which I ultimately identify, closely embeds the main aspects of the benefit-cost trade-off framework that Benmelech et al. (2024) adopt in analyzing potential factors that contribute to the decline of secured debt usage.

The rest of the chapter is organized as follows. Section 1.2 presents hypothesis development. Section 1.3 discusses empirical strategy and specification. Section 1.4 describes the data, sample, and variables used in the analysis. Section 1.5 presents the main empirical findings. Section 1.6 conducts channel analyses, testing implications of hedging on several firm-level outcomes and providing cross-sectional evidence from subsample analyses. Section 1.7 concludes.

1.2 Hypothesis Development

According to the lender protection hypothesis by Smith and Warner (1979), firms optimally choose whether to issue secured debt depending on the relative benefits and costs of doing so. On the one hand, firms need to sacrifice some degree of their operational flexibility because they are restricted from selling or issuing additional claims against the pledged assets without the approval of the lien-holder. At the same time, however, these legal restrictions imposed on the collateralized assets are what give rise to the benefit of secured debt, the reduced cost of borrowing; such legal restrictions effectively preclude potential lender expropriations by borrowing firms such as asset substitution or claim dilution. As the lenders are protected from expropriative actions, they feel more comfortable under a secured debt contract and agree to offer a lower interest rate.

One natural prediction from the lender protection hypothesis is that a firm is likely to find issuing secured debt more beneficial when the perceived threat of expropriative actions by the borrowing firm looms as more substantial in potential lenders' eyes (Bao and Kolasinski, 2016). Consistent with prediction, recent work by Benmelech et al. (2022) documents that firms issue secured debt on a contingent basis, as lenders are not willing to provide lower credit spreads for secured debt relative to unsecured debt during normal times. That is, when a firm is financially healthy or during good macroeconomic conditions, lenders expect that the borrower has less temptation for expropriative actions, so they do not offer substantially lower interest rates on secured loans relative to unsecured. Hence, the relative benefit of security varies with the state of the (borrowing) firm or the economy, and secured debt financing is more appealing for firms whose value of assets is more volatile, for firms with low credit quality, or during adverse macroeconomic conditions.

I formulate the main hypothesis based on the above. Hedging benefits firms by reducing the volatility of cash flows, conferring them a lower risk of financial distress and a lower chance of bankruptcy. In other words, risk management leads to a lower probability of the occurrence of a state of the world in which firms are more prone to engage in lender expropriation activities. As the threat of lender expropriation becomes lower, for firms that hedge, the benefit of issuing secured debt becomes less prominent, not fully compensating for the loss of operational flexibility. This leads me to predict that firms with an effectively working risk management scheme in place should find it more appealing to rely less on secured debt financing; they do not feel a need to sacrifice their operating flexibility by issuing secured debt when they can attain the desired level or cost of debt with either secured or unsecured form of debt financing. From this prediction follows the main hypothesis to be tested in this paper:

H0: Risk management does not affect firms' choice between secured and unsecured debt financing.

Ha: Risk management leads firms to rely less on secured debt and more on unsecured debt.

1.3 Empirical Strategy and Specification

A straightforward way to estimate the effect of risk management on debt security choice would be to simply regress a variable for firm secured or unsecured debt usage on proxies for hedging. However, merely adopting the standard OLS regression in examining the effect of hedging on the choice between secured and unsecured debt by firms is subject to typical endogeneity concerns. For example, a financially constrained firm that faces high investment opportunity may choose to forego available hedging options and issue more secured debt for financing such opportunity (i.e., the omitted variable concern). Besides, there also exists a reverse causality concern in which a large amount of secured debt already sitting on the balance sheet may curb the firm's incentive to engage in risk management practices. These endogenous determinations of firm risk management decisions and debt structure choice make establishing causality under a simple OLS framework challenging.

To overcome endogeneity concerns, I exploit as a natural experiment the introduction of steel futures in a similar spirit to Almeida et al. (2017) to establish the causal effect of risk management on the choice between secured and unsecured debt. In 2008, the London Metals Exchange and the Chicago Mercantile Exchange introduced steel futures products in April and October, respectively. These introductions of exchange-traded futures contract represent an exogenous increase in the ability for or decrease in the cost of hedging steel price risk, which disproportionately benefits firms whose cost structure is especially sensitive to steel price movements. As Almeida et al. (2017) note, industry participants did not strongly call for the creation of steel futures, adding credence to the assertion that firms with different sensitivities to steel price experienced an exogenous variation in their ability to hedge for reasons unrelated to future investment opportunities or other (unobservable) firm characteristics. Therefore, the introduction of steel futures enables me to implement a difference-in-differences (DiD) design under the assumptions that the classification into the treatment and control groups based on the sensitivity to steel price risk can be accurately done and both groups are similar except for their ability to hedge steel price risk post-introduction. Perez-Gonzalez and Yun (2013), Cornaggia (2013), and Biguri et al. (2022) adopt similar approaches, but with different commodity types, in their studies on the causal effect of risk management on firm value, firm productivity, and stock return variance, respectively.

The DiD specification for the natural experiment takes the following form; $Debt\ Variable_{it} = \alpha + \beta_1 steel\_exposed_i * futures\_available_t + \gamma X_{it-1} + Firm\ FE + Year\ FE + \varepsilon_{it}$ *steel_exposed* equals one if steel takes up greater than or equal to 1% of a firm's inputs as provided by the BEA input-output tables and zero otherwise (Almeida et al., 2017). *futures_available* equals one from 2008 to 2011 and zero from 2005 to 2007 (in fiscal years); I look at three years before and four years after the introduction of steel futures. X_{it-1} is a vector of firm controls lagged by one year. Standard errors are clustered at the firm level.

Linking my main prediction to the natural experiment setting, I expect that steel exposed firms, which should be more willing to hedge steel price risk and benefit more from it relative to those less exposed, will find issuing secured debt less lucrative and issuing unsecured debt more appealing after the introduction of steel futures. Accordingly, I expect β_1, the main coefficient of interest, to be negative and statistically significant when the dependent variable is related to the degree of secured debt usage by firms. Conversely, I expect β_1 to be positive and statistically significant when the dependent variable proxies for firms' degree of unsecured debt usage.

1.4 Variable and Sample Description

1.4.1 Variables

Detailed definition and calculation of the variables are shown in Appendix A (Table A.1).

1.4.1.1 Debt Variables

Four debt-related variables are used as the dependent variable in regressions throughout the study; *total_debt, secdebt_asset, nonsecdebt_asset, and secdebt_debt*. Among these four variables, *secdebt_asset*, which I term 'secured debt to asset ratio,' and *secdebt_debt*, 'secured debt ratio,' are the dependent variables of interest that measure firms' secured debt usage. Following the standard practice in the literature on secured debt, I use the Compustat variable 'dm' as the year-end amount of secured debt of a firm (Bao and Kolasinski, 2016; Benmelech et al., 2024; Rampini and Viswanathan, 2022). By construction, *secdebt_asset* + *nonsecdebt_asset* = *total_debt*.

1.4.1.2 Firm-level Characteristics

I calculate six variables to control for firm characteristics. *size, Tobins_Q, tangibility,* and *profitability* control for the standard determinants of capital structure documented in the literature (Frank and Goyal, 2009). I additionally include in the regression model *cash* given its potential role as precautionary savings and *cf_vol* to control for its relation with firm operating or financial risk and hedging incentives. All these firm-level control variables are lagged by one year to alleviate potential endogeneity concerns. I also winsorize all control variables at the 1^{st} and 99^{th} percentile.

1.4.2 Sample Selection

I begin the sample selection process with the Compustat Annual Fundamentals database from 2000 to 2015. The time unit of the data panel is firm fiscal year (*fyear*). I drop utilities (SIC 4900-4949) and financial (SIC 6000-6999) firms and firms whose shares are not traded on NYSE, NASDAQ, and AMEX. I additionally drop firms with SIC code of 9995 (i.e., non-operating establishments). I also exclude firm-year observations with missing or non-positive total assets or book equities and with total assets less than $1 million. To ensure that my debt-related variables lie within a sensical range, I require that each firm-year observation has i) positive *total_debt* (i.e., I keep only levered firms in my sample), ii) book leverage and market leverage (defined as the total amount of debt scaled by the sum of total debt amount and market value of equity) less than or equal to 1, and iii) both *sectdebt_asset* and *nonsecdebt_asset* less than or equal to 1. Next, I keep observations from 2005 to 2011, three years before and four years after the introduction of steel futures, and then drop firms whose average annual steel exposure during the pre-shock period cannot be calculated. Lastly, I require that each firm has at least one observation for both pre- and post-futures introduction periods and has non-missing values for all debt-related and firm characteristics variables. This series of sample refinement steps yields a sample of 9,524 firm-year observations from 2005 to 2011. Table 1.1 presents the summary statistics of the selected sample.

1.5 Results: Natural Experiment

1.5.1 Main Results

Table 1.2 presents the natural experiment results. All regression specifications include firm and year fixed effects, and *steel_exposed* and *futures_available* are absorbed by firm and year fixed effects, respectively. The coefficient on the interaction term, *steel_exposed*futures_available,* is the main coefficient of interest, which captures the relative change in the dependent variable for steel exposed firms compared to non-exposed firms after the introduction of steel futures. For each of the four debt-related variables, I run two difference-in-differences regression models; one with *size* as the only control variable and the other that controls for the full list of firm characteristics variables. As will be shown below, the inclusion of the full list of controls does not quantitatively or qualitatively affect my results.

Consistent with the main prediction, the results show that treated firms decrease their secured debt usage relative to control firms after the introduction of steel futures. Specifically, Columns (1) and (2) show that steel exposed firms decrease their secured debt to asset ratio by 1.6%p to 1.7%p more than non-exposed firms, statistically significant at less than one percent level. Moreover, Columns (3) and (4) indicate that, relative to non-exposed firms, the secured debt ratio decreased by 4.5%p more for steel exposed firms, also statistically significant at less than one percent level. The sample average of the secured debt to asset ratio and of the secured debt ratio are 8.8% and 35.8% (Table 1.1), respectively. Thus, those coefficients represent about an 18.2% relative decrease in secured debt to asset ratio and about a 12.6% relative decrease in secured debt ratio for treated firms, which are economically large. Overall, the results from Column (1) to Column (4) provide causal evidence on the economically and statistically significant effect of risk management on firm secured debt usage, measured by either secured debt to asset ratio or secured debt ratio.

The results in Columns (5) to (8) further reveal that steel exposed firms, after the steel futures introduction, adjust their debt structure toward having more unsecured debt without sacrificing their debt capacity. While the results under Columns (7) and (8) indicate that there is no statistically different post-introduction response between the exposed and non-exposed firms when it comes to the amount of total debt scaled by asset, Columns (5) and (6) show that treated firms experience a relative increase in the unsecured debt to asset ratio of 1.0%p to 1.1%p, statistically significant at less than five percent level.

Taken altogether, the results presented in Table 1.2 are in line with the assertion that the exogenous hedging opportunity created by the introduction of steel futures, disproportionately benefiting steel-exposed firms' ability to hedge, causes those exposed firms to be able to switch from secured to unsecured debt without sacrificing the total amount of debt they can employ. Given that hedging decreases the volatility of cash flows (Bartram et al., 2011), this, in turn, is consistent with one of the main predictions from the lender protection hypothesis that secured debt is less beneficial when the borrowing firm is safer or less volatile and thus the probability of expropriative actions on loans is lower.

1.5.2 Significant New Debt Issue Events

To corroborate the previous results that risk management enables firms to choose unsecured over secured debt and leads them to decrease the relative proportion of secured debt, I examine debt issuance events. In doing so, I construct three dummy variables to proxy for significant debt issuance events of firms, following Bao and Kolasinski (2016);

$$NewNetDebt_{it} = 1 \; if \; \frac{dltis_{it} - dltr_{it}}{Total\ Assets_{it-1}} \geq 0.01$$

$$NewSecured_{it} = 1 \; if \; \frac{dm_{it} - dm_{it-1}}{Total\ Assets_{it-1}} \geq 0.01$$

$$NewUnsecured_{it} = 1 \; if \; \frac{(dltis_{it} - dltr_{it}) - (dm_{it} - dm_{it-1})}{Total\ Assets_{it-1}} \geq 0.01$$

Table 1.3 shows the results of the natural experiment with the above three dummies as dependent variables. Columns (1) and (2) show that, post-introduction, steel exposed firms have a significantly higher tendency relative to non-exposed firms to issue new debt that is equal to or more than 1% of lagged book assets. Columns (3) to (6) further indicate that the significantly higher incidence of large-scale new debt issuance by treated firms is mainly accounted for by large-scale unsecured debt issuance events, not secured debt issuance. Given that the mean *NewUnsecured* in the sample is 0.251 (Table 1.1), the results under Columns (5) or (6) represent an increase in the likelihood of significant new unsecured debt issuance by 19.1% for treated firms. Overall, the debt issuance analysis suggests that risk management leads firms to switch to unsecured debt financing from secured, augmenting the original natural experiment results that indicate both statistically and economically meaningful effects of risk management on the amount of secured debt but no effect on the total amount of debt employed.

1.5.3 Validity of Natural Experiment

I now check whether the core assumptions of difference-in-differences design are met to claim the validity of the causal interpretation of the results. A valid DiD estimation is based on the assumptions that i) both treatment and control firms exhibit parallel trends in outcome variables during the pre-shock period and ii) both groups are similar in characteristics, at least for observable dimensions.

First, I examine whether the parallel trends assumption holds in my setting. I primarily examine *secdebt_debt* and *nonsecdebt_asset* because they are the core measures of the firm secured and unsecured debt usage, respectively. Figure 1.1.a displays the dynamics of the coefficient estimates pertaining to the results in Columns (3) to (4) in Table 1.2. For this analysis, I omit the last fiscal year before the futures introduction (i.e., the fiscal year 2007) as the reference point. I find that secured debt ratio begins to show a clearly decreasing pattern after the futures introduction year, and the coefficients turn significantly negative at less than five percent level after one year from the shock. Moreover, I find no economically significant evidence of pre-trends. The timing and sign of the secured debt ratio response support the validity of the setting.

The patterns presented in Figure 1.1.b further support my interpretation of the results. The point estimates during the pre-introduction years hover close to zero, indicating no economically significant evidence of pre-trends. However, I can observe that unsecured debt to asset ratio begins to show a clearly increasing pattern after the futures introduction year, and the coefficients turn significantly positive after one year from the shock.

In sum, both Figure 1.1.a and Figure 1.1.b suggest that the violation of the parallel trends assumption does not seem to be much of a concern in my setting. In addition, the obvious decreasing (increasing) trends in *secdebt_debt* (*nonsecdebt_asset)* observed during the post-introduction period add confidence to the interpretation of the results that the coefficients of the interaction term capture the post-shock differential response between the two groups of firms.

Next, I check whether both groups of firms are similar in characteristics before the introduction of steel futures, at least for observable dimensions. Table 1.4 provides the t-tests of differences in firm characteristics by steel exposed and non-exposed firms, based on mean values during the pre-introduction period. Treated and control firms are statistically near similar except for profitability (statistically different at less than ten percent level) and tangibility (marginal statistical difference), so it is less likely that the differences in observable firm characteristics drive my natural experiment results. Regardless, I conduct a matched DiD analysis using propensity score matching (PSM) to check if there arises any material change in the natural experiment results after the difference in profitability and tangibility between the two groups is accounted for. I match firms on *size*, *tangibility*, *profitability*, and debt variables using propensity score matching method, matched with replacement within a caliper width of 0.01%. The reason for adding debt variables to the list of variables used in PSM is to address the potential concern that the significant pre-introduction level difference in those variables may drive the original natural experiment results.

Table 1.5 reveals that the matching procedure properly addresses the difference in *profitability* and *tangibility*, and the two groups of firms are now statistically similar across all six characteristics as well as across all four debt variables. The patterns presented in Figure 1.2.a to Figure 1.2.b are very similar to those presented in Figure 1.1.a to Figure 1.1.b. Moreover, Table 1.6 reveals that the matched DiD results are quantitatively and qualitatively similar to the original natural experiment results (Table 1.2). Overall, the t-test and matching results suggest that the differences in firm observables are less likely to drive the main results.

1.5.4 Robustness Tests

In this subsection, I implement a series of additional tests to check the robustness of my findings, including the use of an alternative definition of the treatment group and placebo tests.

1.5.4.1 Alternative Definition of Treatment Group

I test whether the original natural experiment results are robust to an alternative criterion for categorizing firms as steel exposed. Specifically, I define a firm as steel exposed if its average steel input exposure during the pre-introduction period falls within the top quintile and non-exposed for the remaining quintiles.

Table 1.7 presents the results. The patterns, signs, and magnitudes of the coefficients for the interaction term are strongly consistent with the original natural experiment results in Table 1.2, indicating that the differential effects of futures introduction on debt security structure of the two groups of firms are robust to the alternative definition of treatment status.

1.5.4.2 Placebo (Falsification) Tests

I implement two placebo (falsification) tests to evaluate whether the presented results in Table 1.2 are unique to the steel futures introduction event. In the first placebo test, I randomly assign treatment and control status to firms regardless of their pre-introduction steel exposure. For the second, I set the placebo event year as the fiscal year 2012 and use fiscal years 2009 to 2011 and 2012 to 2015 as pre- and post-event periods, keeping and using the same treatment and control firms used in the original natural experiment analysis.

The results are presented in Panel A and Panel B of Table 1.8, respectively. As shown from the results presented under each panel, none of the coefficients of main interest are statistically significant. The two falsification tests altogether reinforce the validity of the original findings by suggesting that the observed effects in the main DiD analysis are indeed specific to the steel exposed firms and steel futures introduction event and are not merely due to random chance or other unobserved events during the specified period.

1.6 Channel Analyses

1.6.1 Testing Implications of Hedging on Firm Outcomes

To further validate the assertion that the main results of the paper truly reflect steel exposed firms' exploiting steel futures instrument for hedging, I examine the proposed chain of mechanisms whereby firms engage in risk management, which stabilizes cash flow, ultimately leading to a reduction in default risk and cost of financial distress. To do so, I test implications of hedging on several firm-level outcomes, specifically focusing on risk management disclosure, cash flow stability, and default risk.

As shown in Table 1.9, I find evidence consistent with the proposed chain of mechanisms. The results in Columns (1) and (2) confirm that, after the introduction of steel futures, steel exposed firms are more likely to mention using commodity derivatives as a hedging tool in their 10-K filings, statistically significant at less than five percent level. Furthermore, Columns (3) and (4) show that steel exposed firms experience a reduction in their cash flow volatility relative to control firms after the steel futures introduction. Lastly, the results in Columns (5) and (6) provide causal evidence that hedging ultimately reduces firms' probability of default (measured based on the Merton model).

1.6.2 Cross-Sectional Evidence

I conduct several subsample analyses to provide further evidence that the decrease (increase) in secured debt (unsecured debt) usage by treated firms indeed reflects the effect of risk management and ensuing alteration in the trade-off between the benefits and costs of secured debt usage as implied by the lender protection hypothesis. The literature on risk management and secured debt guides my predictions regarding by what specific types of firms in the sample the results presented in Table 1.2 are primarily driven and the choice on relevant proxies for splitting the sample. The main dependent variables of interest are *secdebt_asset*, *secdebt_debt*, and *nonsecdebt_asset* throughout the subsample analysis.

1.6.2.1 Propensity or Incentive to Hedge

First, I examine whether the results are mainly driven by firms that have higher incentive or propensity to hedge. Firms should be able to post collateral to engage in financial hedging, and such collateral requirements are often met by tangible assets. Therefore, more tangible firms should be in a better position to engage in financial hedging and have a higher propensity to do so when actual hedging opportunity in line with their needs exists (Rampini and Viswanathan, 2010; Almeida et al., 2017). Another relevant aspect of firms that is associated with their incentive to hedge is the volatility of input costs. Intuitively, firms that historically have experienced higher input volatility would be more incentivized to hedge if such an opportunity becomes available at a lower cost or in a more established manner. All these lines of argument lead me to predict that the results presented in Table 1.2 should be primarily driven by firms with more tangible assets or higher volatility of input costs, as they are more likely to actually exploit the hedging opportunity provided by the introduction of steel futures.

I split the sample on *tangibility* and *COGS_vol*, where *COGS_vol* is defined as the standard deviation of cost of goods sold calculated over five years scaled by sale. *high_tangibility* = 1 if a firm has top tercile pre-introduction average *tangibility* and *high_tangibility* = 0 if bottom tercile. *high_COGS_vol* = 1 or 0 is determined in an analogous manner, based on tercile cuts. For the analysis based on *high_tangibility*, I exclude *tangibility* from the regression model.

Table 1.10 presents the results. Consistent with my prediction, the negative (positive) and statistically significant coefficients for the interaction term when the dependent variable is related to firm secured debt (unsecured debt) usage are observed only for the subsample of firms with higher tangibility (Panel A) and with higher volatility of input costs (Panel B). These results corroborate the argument that risk management explains the decrease in secured debt usage by treated firms; among the treated firms, only those with high tangibility or high volatility of input costs are able or willing to actually hedge their steel price risk post-introduction, so they become less reliant on secured debt relative to their counterpart (i.e., non-exposed *high-tangibility* or *high-COGS_vol* firms). This pattern does not hold for the other part of the sample because *low-tangibility* or *low-COGS_vol* treated firms are less able or willing to exploit the hedging opportunity.

1.6.2.2 Benefits and Costs of Secured Debt Usage

Next, I investigate whether the results are mainly driven by firms that derive relatively less benefit from using secured debt or whose nature of business is relatively more fragile to the inconvenience of use restrictions on pledged assets or a loss in operating flexibility imposed by the secured debt contracts (i.e., firms that need to bear a higher cost of secured debt usage). I rely on two proxies guided by the literature; *Redeployability* (Kim and Kung, 2017) and *INFLEX* – a measure for operating inflexibility (Gu et al., 2018).

Benmelech and Bergman (2009) show that debt tranches secured by more redeployable collateral exhibit lower credit spreads. Put in another way, firms with less redeployable assets relatively benefit less from secured debt usage because the reduction in credit spreads per unit of assets pledged is lower for them. Moreover, low-redeployability firms are less flexible in adjusting their investment in the face of uncertainty (Kim and Kung, 2017), rendering asset encumberment imposed by secured debt more costly for them. Related to this point, the cost of having assets encumbered for using secured debt should be higher for firms with less operating flexibility in the first place. Therefore, I expect a more pronounced effect of risk management on secured debt financing from low-redeployable or high-*INFLEX* firms, as they are the ones that face relatively less benefit and higher cost from using secured debt.

INFLEX, the measure for the degree of operating inflexibility, is calculated using Compustat quarterly data as below;

$$INFLEX_{i,t} = \frac{max_{i,0,t}\left(\frac{OPC}{Sales}\right) - min_{i,0,t}\left(\frac{OPC}{Sales}\right)}{std_{i,0,t}\left(\Delta \log\left(\frac{Sales}{Assets}\right)\right)}\ , where\ OPC = COGSQ + XSGAQ$$

As *INFLEX* is a firm-level range measure, I calculate it over 2000 to 2007, requiring at least 8 quarterly observations for each firm. *low_redeploy* = 1 if a firm has bottom tercile pre-introduction average *redeployability* and *low_redeploy* = 0 if top tercile. *high_INFLEX* = 1 if a firm has top tercile pre-introduction *INFLEX* or 0 if bottom tercile.

Table 1.11 shows the results. Consistent with my prediction, the strongly negative (positive) and statistically significant coefficients for the interaction term when the dependent variable is related to firm secured debt (unsecured debt) usage are observed only for the subsample of firms with lower redeployability (Panel A) and with higher operating inflexibility (Panel B). These results strengthen my interpretation of the results in Table 1.2 that the negative effect of hedging on secured debt reflects firms' trade-off between the benefits and costs of secured debt financing as implied by the lender protection hypothesis.

1.6.2.3 Financial Health

Lastly, I consider the financial health of firms. I predict that the reduction in secured debt usage brought about by the enhanced risk management ability should be bigger for less financially healthy firms. This prediction directly comes from the main implication of the lender protection hypothesis that firm financial health should be negatively associated with secured debt usage. Indeed, Rampini and Viswanathan (2022) show a near-monotonic relationship between credit quality as proxied by credit ratings and secured debt usage, whereby low-rated firms use secured debt more. Therefore, given that the marginal benefit of hedging on financial health is higher for financially less healthy firms (Chen and King, 2014), financially weaker firms should experience a steeper improvement in financial conditions from hedging and thus have a higher incentive to exploit the opportunity for adjusting their debt security structure enabled by the hedging.

To proxy for firm financial health, I use Altman's Z score. This time, instead of splitting the sample solely based on the Z score, I implement a two-way independent sort of firms based on *tangibility* and Altman's Z score. I do so in order to address the potential monotonic relation between the financial health proxy and tangibility in the sample; if firms with lower Z score are also low-tangible firms, some of those financially weaker firms may be less able to hedge due to collateral constraints, adding noise to subsample estimations and confounding the interpretation that the effect of risk management is behind the results. The cut points for 2x2 independent sorts are pre-introduction Z score below or above 3 and median *tangibility*. To be consistent with my prediction, I should find more pronounced effects for firms with both lower Z score and higher tangibility (i.e., Z<3, high tangible firms).

Table 1.12 presents the results. Consistent with my prediction, the negative (positive) and statistically significant coefficients for the interaction term when the dependent variable is related to firm secured debt (unsecured debt) usage are only present for firms with Z score less than 3 and *tangibility* above the median. These results indicate that the results in Table 1.2 are mainly driven by financially weaker firms with less collateral constraints, which should be the main beneficiaries of the exogenous enhancement of risk management ability as jointly dictated by the lender protection hypothesis and the degree of the marginal benefit from hedging.

1.7 Conclusion

Motivated by the lender protection hypothesis suggesting a potential link between corporate hedging and firm secured debt usage, this paper provides causal evidence that risk management leads firms to rely less on secured debt and more on unsecured debt. Specifically, I examine the effect of an exogenous increase in firms' ability to hedge input price risk on firm's debt security structure choice, exploiting the introduction of steel futures as a natural experiment. I show that the enhanced hedging opportunity causes firms to be able to switch from secured to unsecured debt without sacrificing the total amount of debt they can employ. In addition, cross-sectional evidence reveals that the effects are concentrated in firms with a higher incentive to engage in financial hedging, that derive relatively less benefit or face higher (indirect) costs from using secured debt, and that are financially less healthy but with some collateral capacity for financial hedging, consistent with the predictions from the lender protection hypothesis.

If I posit that the reason treated firms decrease their secured debt ratio is indeed the less prominent post-hedging unsecured-secured spread offered by lenders, my results also provide suggestive evidence on the potential channel through which risk management increases firm value. That is, hedging firms can conserve similar debt capacity and cost of debt even when they rely more on unsecured debt, so there is less need for them to bear the costs of secured debt financing, such as reduced operating flexibility or less slack for future financing. Recent findings by Benmelech et al. (2022) on firm risk- or macro condition-contingent pricing of secured debt, along with my natural experiment results, render support to the link between risk management and firm value through this potential channel of debt security structure choice.

Table 1.1: Summary statistics

Table 1.1 reports summary statistics of the selected sample. The sample consists of 9,524 firm-year observations from 2005 to 2011 (fiscal years). Detailed definitions and calculations of the variables are shown in Table A.1 of Appendix A.

Variable	**N**	**mean**	**sd**	**min**	**p10**	**p50**	**p90**	**max**
total_debt	9524	0.232	0.180	0.000	0.013	0.207	0.490	0.908
secdebt_asset	9524	0.088	0.142	0.000	0.000	0.011	0.296	0.888
nonsecdebt_asset	9524	0.144	0.147	0.000	0.001	0.107	0.352	0.858
secdebt_debt	9524	0.358	0.388	0.000	0.000	0.167	0.979	1.000
NewNetDebt	9524	0.304	0.460	0.000	0.000	0.000	1.000	1.000
NewSecured	9524	0.202	0.401	0.000	0.000	0.000	1.000	1.000
NewUnsecured	9524	0.251	0.433	0.000	0.000	0.000	1.000	1.000
size	9524	6.289	1.926	2.125	3.656	6.333	8.778	11.136
Tobins_Q	9524	1.584	0.718	0.468	0.914	1.387	2.507	4.865
tangibility	9524	0.284	0.240	0.009	0.044	0.201	0.684	0.912
profitability	9524	-0.004	0.177	-1.028	-0.173	0.039	0.121	0.326
cash	9524	0.159	0.185	0.000	0.010	0.088	0.430	0.877
cf_vol	9524	0.058	0.059	0.005	0.015	0.040	0.114	0.461

Table 1.2: Natural experiment – Difference-in-differences tests

Table 1.2 presents the DiD test results on how the introduction of steel futures affects firms' debt security structure choice. I estimate the following regression model:

$$Debt\ Variable_{it} = \alpha + \beta_1 steel\_exposed_i * futures\_available_t + \gamma X_{it-1} + Firm\ FE + Year\ FE + \varepsilon_{it}$$

where $steel\_exposed_i$ equals one if steel takes up greater than or equal to 1% of a firm's inputs on average during the pre-introduction period and zero otherwise; $futures\_available_t$ equals one from 2008 to 2011 and zero from 2005 to 2007 (in fiscal years); and X_{it-1} is a vector of controls lagged by one year. t-statistics based on standard errors clustered at firm level are reported in parentheses below the coefficient estimates. *, **, *** represent statistical significance at the 10%, 5%, and 1% level, respectively.

VARIABLES	(1) secdebt _asset	(2) secdebt _asset	(3) secdebt _debt	(4) secdebt _debt	(5) sonsecdebt _asset	(6) nonsecdebt _asset	(7) total_debt	(8) total_debt
steel_exposed*	**-0.016*** **	**-0.017*** **	**-0.045*** **	**-0.045*** **	**0.011** **	**0.010** **	**-0.005**	**-0.007**
futures_available	**(-4.04)**	**(-4.30)**	**(-3.17)**	**(-3.19)**	**(2.25)**	**(2.09)**	**(-0.86)**	**(-1.26)**
size	0.009**	0.012**	0.008	-0.002	0.006	0.011**	0.016***	0.023***
	(2.23)	(2.57)	(0.65)	(-0.15)	(1.34)	(2.08)	(2.85)	(3.90)
Tobins_Q		-0.000		-0.003		-0.007*		-0.007*
		(-0.05)		(-0.29)		(-1.85)		(-1.69)
tangibility		0.052*		0.005		0.052*		0.104***
		(1.83)		(0.08)		(1.83)		(2.99)
profitability		-0.047***		-0.019		-0.070***		-0.118***
		(-5.53)		(-0.68)		(-5.42)		(-8.30)
cash		-0.020		-0.017		-0.011		-0.031
		(-1.45)		(-0.35)		(-0.66)		(-1.51)
cf_volatility		-0.035		-0.227**		0.007		-0.028
		(-1.00)		(-2.09)		(0.19)		(-0.59)
Constant	0.033	0.006	0.317***	0.399***	0.102***	0.070*	0.135***	0.076*
	(1.22)	(0.18)	(4.13)	(4.17)	(3.52)	(1.82)	(3.91)	(1.75)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	9,524	9,524	9,524	9,524	9,524	9,524	9,524	9,524
Adjusted R-squared	0.755	0.757	0.658	0.658	0.702	0.706	0.773	0.781

Table 1.3: Significant new issuance of debt

Table 1.3 presents the results of significant new debt issuance analysis. Following Bao and Kolasinski (2016), I construct three dummy variables for this analysis. *NewNetDebt* equals one if a firm issues new debt, regardless of secured or unsecured, that is equal to or more than 1% of lagged book assets for a given year and zero otherwise. *NewSecured* equals one if a firm issues new secured debt that is equal to or more than 1% of lagged book assets for a given year and zero otherwise. *NewUnsecured* equals one if a firm issues new unsecured debt that is equal to or more than 1% of lagged book assets for a given year and zero otherwise. I estimate the following regression model:

$$Debt\ Issuance\ Dummy_{it} = \alpha + \beta_1 steel\_exposed_i * futures\_available_t + \gamma X_{it-1} + Firm\ FE + Year\ FE + \varepsilon_{it}$$

where $steel\_exposed_i$ equals one if steel takes up greater than or equal to 1% of a firm's inputs on average during the pre-introduction period and zero otherwise; $futures\_available_t$ equals one from 2008 to 2011 and zero from 2005 to 2007 (in fiscal years); and X_{it-1} is a vector of controls lagged by one year. t-statistics based on standard errors clustered at firm level are reported in parentheses below the coefficient estimates. *, **, *** represent statistical significance at the 10%, 5%, and 1% level, respectively.

VARIABLES	(1) New NetDebt	(2) New NetDebt	(3) New Secured	(4) New Secured	(5) New Unsecured	(6) New Unsecured
steel_exposed*futures_available	**0.048****	**0.050****	**0.003**	**0.003**	**0.048*****	**0.047*****
	(2.49)	**(2.56)**	**(0.18)**	**(0.19)**	**(2.66)**	**(2.61)**
size	-0.144***	-0.157***	-0.094***	-0.096***	-0.081***	-0.085***
	(-9.17)	(-8.57)	(-6.61)	(-5.80)	(-5.17)	(-4.81)
Tobins_Q		0.066***		0.023*		0.039***
		(4.76)		(1.94)		(3.11)
tangibility		0.280***		-0.106		0.219**
		(2.64)		(-1.02)		(2.20)
profitability		0.257***		0.087**		0.020
		(5.84)		(2.21)		(0.50)
cash		-0.074		0.118**		-0.225***
		(-1.12)		(2.05)		(-3.69)
cf_volatility		-0.314**		-0.072		-0.281**
		(-2.35)		(-0.58)		(-2.17)
Constant	1.198***	1.126***	0.789***	0.788***	0.750***	0.706***
	(12.15)	(8.43)	(8.89)	(6.65)	(7.61)	(5.54)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	9,524	9,524	9,524	9,524	9,524	9,524
Adjusted R-squared	0.137	0.145	0.155	0.157	0.094	0.098

Table 1.4: Firm characteristics comparison by steel exposure (Pre-introduction period)

Table 1.4 reports the means and standard errors of firm characteristics variables before the introduction of steel futures, separately for steel-exposed and non-exposed firms. $steel\_exposed_i$ equals one if steel takes up greater than or equal to 1% of a firm's inputs on average during the pre-introduction period and zero otherwise. p-values of t-tests of differences in mean values of the two groups are presented in the last column.

	steel_exposed == 1		steel_exposed == 0			
	Mean	SE	Mean	SE	Diff	p-value
size	6.164	0.072	6.156	0.058	0.008	0.930
Tobins_Q	1.730	0.026	1.690	0.022	0.040	0.256
tangibility	0.282	0.009	0.262	0.007	**0.020**	**0.076**
profitability	0.011	0.006	-0.005	0.005	**0.015**	**0.051**
cash	0.159	0.007	0.169	0.006	-0.010	0.288
cf_volatility	0.054	0.002	0.057	0.002	-0.002	0.327

Table 1.5: Comparison by steel exposure – PSM matched sample

Table 1.5 reports the means and standard errors of debt and firm characteristics variables before the introduction of steel futures, separately for the matched treated (steel exposed) and control (non-exposed) firms. Treated and control firms are matched on *size, tangibility, profitability,* and debt variables using propensity score matching method, matched with replacement within a caliper width of 0.01%. $steel\_exposed_i$ equals one if steel takes up greater than or equal to 1% of a firm's inputs on average during the pre-introduction period and zero otherwise. p-values of t-tests of differences in mean values of the two groups are presented in the last column.

	steel_exposed == 1		steel_exposed == 0			
	Mean	SE	Mean	SE	Diff	p-value
total_debt	0.185	0.007	0.195	0.009	-0.010	0.385
secdebt_asset	0.046	0.004	0.046	0.004	-0.001	0.866
nonsecdebt_asset	0.139	0.007	0.148	0.008	-0.009	0.411
secdebt_debt	0.292	0.017	0.293	0.019	0.000	0.994
size	6.137	0.094	6.250	0.109	-0.114	0.428
Tobins_Q	1.763	0.035	1.733	0.040	0.029	0.582
tangibility	0.233	0.010	0.228	0.011	0.005	0.755
profitability	0.008	0.007	0.012	0.008	-0.004	0.710
cash	0.179	0.009	0.165	0.011	0.014	0.303
cf_volatility	0.054	0.002	0.056	0.003	-0.002	0.565

Table 1.6: Natural experiment – DiD tests with PSM matched sample

Table 1.6 presents the natural experiment results with the matched sample. Treated and control firms are matched on *size, tangibility, profitability,* and debt variables using propensity score matching method, matched with replacement within a caliper width of 0.01%. I estimate the following regression model:

$$Debt\ Variable_{it} = \alpha + \beta_1 steel\_exposed_i * futures\_available_t + \gamma X_{it-1} + Firm\ FE + Year\ FE + \varepsilon_{it}$$

where $steel\_exposed_i$ equals one if steel takes up greater than or equal to 1% of a firm's inputs on average during the pre-introduction period and zero otherwise; $futures\_available_t$ equals one from 2008 to 2011 and zero from 2005 to 2007 (in fiscal years); and X_{it-1} is a vector of controls lagged by one year. t-statistics based on standard errors clustered at firm level are reported in parentheses below the coefficient estimates. *, **, *** represent statistical significance at the 10%, 5%, and 1% level, respectively.

VARIABLES	(1) secdebt _asset	(2) secdebt _asset	(3) secdebt _debt	(4) secdebt _debt	(5) nonsecdebt _asset	(6) nonsecdebt _asset	(7) total_debt	(8) total_debt
steel_exposed* futures_available	**-0.018*****	**-0.018*****	**-0.060*****	**-0.060*****	**0.019****	**0.017****	**0.001**	**-0.001**
	(-3.35)	**(-3.42)**	**(-2.93)**	**(-2.91)**	**(2.43)**	**(2.30)**	**(0.10)**	**(-0.12)**
size	0.006	0.006	0.001	-0.008	0.006	0.011	0.013	0.017*
	(1.12)	(0.93)	(0.08)	(-0.39)	(0.87)	(1.25)	(1.62)	(1.92)
Tobins_Q		-0.002		-0.006		-0.011**		-0.013**
		(-0.69)		(-0.47)		(-2.02)		(-2.27)
tangibility		0.092**		0.096		-0.014		0.078
		(2.15)		(0.79)		(-0.26)		(1.18)
profitability		-0.013		0.004		-0.068***		-0.081***
		(-1.25)		(0.08)		(-3.73)		(-4.23)
cash		-0.009		0.087		-0.028		-0.037
		(-0.56)		(1.17)		(-0.98)		(-1.13)
cf_volatility		-0.037		-0.233		0.038		0.001
		(-0.83)		(-1.30)		(0.68)		(0.01)
Constant	0.021	0.007	0.301**	0.347**	0.100**	0.097	0.120**	0.104
	(0.60)	(0.16)	(2.49)	(2.39)	(2.10)	(1.47)	(2.45)	(1.53)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,044	4,044	4,044	4,044	4,044	4,044	4,044	4,044
Adjusted R-squared	0.642	0.645	0.620	0.620	0.708	0.712	0.742	0.748

Table 1.7: Robustness tests – Alternative definition of treatment group

Table 1.7 reports the natural experiment results using an alternative definition of the treatment group. I estimate the following regression model:

$$Debt\ Variable_{it} = \alpha + \beta_1 steel\_exposed_i * futures\_available_t + \gamma X_{it-1} + Firm\ FE + Year\ FE + \varepsilon_{it}$$

where $steel\_exposed_i$ equals one if a firm's average steel input exposure during the pre-introduction period belongs to the top quintile and zero otherwise. This robustness test aims to verify if the main findings from the original analysis hold when using a different criterion for categorizing firms as steel exposed. All other features of the analysis, other than the definition of steel exposed firms, remain the same as the original analysis: $futures\_available_t$ equals one from 2008 to 2011 and zero from 2005 to 2007 (in fiscal years), and X_{it-1} is a vector of controls lagged by one year. t-statistics based on standard errors clustered at firm level are reported in parentheses below the coefficient estimates. *, **, *** represent statistical significance at the 10%, 5%, and 1% level, respectively.

VARIABLES	(1) secdebt_asset	(2) secdebt_debt	(3) nonsecdebt_asset	(4) total_debt
steel_exposed*futures_available	**-0.014*****	**-0.039*****	**0.012****	**-0.002**
	(-3.42)	**(-2.80)**	**(2.41)**	**(-0.40)**
size	0.012**	-0.002	0.011**	0.023***
	(2.55)	(-0.16)	(2.05)	(3.86)
Tobins_Q	-0.000	-0.003	-0.007*	-0.007*
	(-0.06)	(-0.29)	(-1.87)	(-1.72)
tangibility	0.054*	0.010	0.050*	0.104***
	(1.89)	(0.14)	(1.78)	(2.98)
profitability	-0.047***	-0.019	-0.070***	-0.117***
	(-5.51)	(-0.68)	(-5.40)	(-8.28)
cash	-0.019	-0.013	-0.013	-0.032
	(-1.37)	(-0.27)	(-0.74)	(-1.52)
cf_volatility	-0.034	-0.226**	0.007	-0.027
	(-0.99)	(-2.08)	(0.19)	(-0.59)
Constant	0.006	0.398***	0.071*	0.077*
	(0.17)	(4.15)	(1.84)	(1.76)
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	9,524	9,524	9,524	9,524
Adjusted R-squared	0.757	0.658	0.706	0.781

Table 1.8: Robustness tests – Placebo (falsification) tests

Table 1.8 shows the results from two placebo tests. Panel A presents the results from tests in which I randomly assign treatment and control firms, regardless of their pre-introduction steel exposure. All other features of the analysis, other than the random assignment of the treatment status (indicated by $pseudo\_exposed_i = 1$), remain the same as in the original analysis: $futures\_available_t$ equals one from 2008 to 2011 and zero from 2005 to 2007 (in fiscal years), and X_{it-1} is a vector of controls lagged by one year. Panel B reports the results from the analysis assuming a placebo event year, where $pseudo\_available_t = 1$ for observations in the post-pseudo event period (2012-2015) and zero for the pre-pseudo period (2009-2011). All other features of the analysis stay the same as in the original analysis: the same set of treatment and control firms are used, and X_{it-1} is a vector of controls lagged by one year. t-statistics based on standard errors clustered at firm level are reported in parentheses below the coefficient estimates. *, **, *** represent statistical significance at the 10%, 5%, and 1% level, respectively.

Panel A: Random treatment and control firms (i.e., pseudo treat)

VARIABLES	(1) secdebt_asset	(2) secdebt_debt	(3) nonsecdebt_asset	(4) total_debt
pseudo_exposed*futures_available	**0.001**	**0.009**	**0.003**	**0.003**
	(0.15)	**(0.68)**	**(0.54)**	**(0.60)**
size	0.011**	-0.004	0.011**	0.023***
	(2.37)	(-0.32)	(2.16)	(3.85)
Tobins_Q	-0.001	-0.004	-0.007*	-0.007*
	(-0.19)	(-0.43)	(-1.81)	(-1.76)
tangibility	0.051*	0.002	0.053*	0.104***
	(1.78)	(0.03)	(1.85)	(2.97)
profitability	-0.046***	-0.014	-0.071***	-0.117***
	(-5.32)	(-0.49)	(-5.46)	(-8.24)
cash	-0.021	-0.020	-0.011	-0.032
	(-1.53)	(-0.42)	(-0.64)	(-1.54)
cf_volatility	-0.033	-0.222**	0.007	-0.026
	(-0.96)	(-2.04)	(0.19)	(-0.57)
Constant	0.009	0.405***	0.068*	0.077*
	(0.25)	(4.18)	(1.76)	(1.76)
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	9,524	9,524	9,524	9,524
Adjusted R-squared	0.756	0.657	0.706	0.781

Table 1.8 (continued)

Panel B: Placebo event year (i.e., pseudo post)

VARIABLES	(1) secdebt_asset	(2) secdebt_debt	(3) nonsecdebt_asset	(4) total_debt
steel_exposed*pseudo_available	**-0.006**	**-0.013**	**0.001**	**-0.005**
	(-1.17)	**(-0.87)**	**(0.23)**	**(-0.83)**
size	0.012	-0.031*	0.035***	0.048***
	(1.58)	(-1.83)	(5.70)	(5.26)
Tobins_Q	-0.002	-0.003	-0.006*	-0.008*
	(-0.43)	(-0.24)	(-1.70)	(-1.81)
tangibility	0.081**	0.048	0.035	0.116***
	(2.20)	(0.56)	(0.99)	(3.04)
profitability	-0.032***	0.018	-0.040***	-0.071***
	(-2.68)	(0.54)	(-3.29)	(-4.85)
cash	-0.037*	-0.039	-0.021	-0.058**
	(-1.65)	(-0.68)	(-1.14)	(-2.25)
cf_volatility	-0.091*	-0.407***	0.058	-0.034
	(-1.81)	(-2.93)	(1.32)	(-0.55)
Constant	0.007	0.611***	-0.091*	-0.083
	(0.13)	(4.92)	(-1.95)	(-1.29)
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	8,817	8,817	8,817	8,817
Adjusted R-squared	0.738	0.674	0.730	0.805

Table 1.9: Testing implications of hedging

Table 1.9 presents the results of analyses testing the implications of hedging on various firm-level outcomes, including risk management disclosure, cash flow stability, and default risk. I estimate the following regression model:

$$DepVar_{it} = \alpha + \beta_1 steel\_exposed_i * futures\_available_t + \gamma X_{it-1} + Firm\ FE + Year\ FE + \varepsilon_{it}$$

where $steel\_exposed_i$ equals one if steel takes up greater than or equal to 1% of a firm's inputs on average during the pre-introduction period and zero otherwise; $futures\_available_t$ equals one from 2008 to 2011 and zero from 2005 to 2007 (in fiscal years); and X_{it-1} is a vector of controls lagged by one year. In Columns (1) and (2), *commodityhedge* is a dummy variable that equals one if a firm reports using commodity derivatives in its 10-K filings and zero otherwise. In Columns (3) and (4), the dependent variable is cash flow volatility. As it is used as the dependent variable, I do not include (lagged) *cf_volatility* as a control variable in regressions. In Columns (5) and (6), *prob_default* is the default probability based on the Merton Distance-to-Default (DD) model as delineated in Bharath and Shumway (2008). t-statistics based on standard errors clustered at firm level are reported in parentheses below the coefficient estimates. *, **, *** represent statistical significance at the 10%, 5%, and 1% level, respectively.

VARIABLES	(1) commodity hedge	(2) commodity hedge	(3) cf _volatility	(4) cf _volatility	(5) prob _default	(6) prob _default
steel_exposed*	**0.026****	**0.027****	**-0.006****	**-0.006****	**-0.014*****	**-0.015*****
futures_available	**(2.05)**	**(2.09)**	**(-2.06)**	**(-2.40)**	**(-3.41)**	**(-3.72)**
size	-0.000	-0.000	-0.035***	-0.032***	-0.002	0.005
	(-0.05)	(-0.02)	(-5.66)	(-5.31)	(-0.45)	(1.07)
Tobins_Q		0.002		-0.008***		-0.013***
		(0.28)		(-2.58)		(-4.88)
tangibility		0.032		-0.024		0.098***
		(0.48)		(-1.20)		(3.86)
profitability		0.048**		-0.060***		-0.089***
		(2.22)		(-4.74)		(-7.26)
cash		0.016		0.017		-0.003
		(0.53)		(1.13)		(-0.21)
cf_volatility		0.062				0.020
		(0.90)				(0.62)
Constant	0.193***	0.173**	0.281***	0.281***	0.040*	-0.008
	(3.34)	(2.15)	(7.21)	(6.40)	(1.69)	(-0.24)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,600	7,600	9,524	9,524	9,320	9,320
Adjusted R-squared	0.753	0.754	0.605	0.613	0.314	0.336

Table 1.10: Cross-sectional evidence – Propensity or incentive to hedge

Table 1.10 presents the results of subsample analysis split on variables related to firms' propensity or incentive to hedge. In Panel A, firms are split on *tangibility*, so I do not include *tangibility* as a control variable in regressions. *high_tangibility* equals one if a firm has top tercile pre-introduction average *tangibility* and zero if bottom tercile. In Panel B, firms are split on the volatility of the cost of goods sold (*COGS_vol*). *COGS_vol* is defined as the standard deviation of cost of goods sold calculated over five years scaled by sale. *high_COGS_vol* = 1 or 0 is determined in an analogous manner to *high_tangibility*, based on tercile cuts. t-statistics based on standard errors clustered at firm level are reported in parentheses below the coefficient estimates. *, **, *** represent statistical significance at the 10%, 5%, and 1% level, respectively.

Panel A: Tangibility

VARIABLES	secdebt_asset high_ tangibility=1	 high_ tangibility=0	secdebt_debt high_ tangibility=1	 high_ tangibility=0	nonsecdebt_asset high_ tangibility=1	 high_ tangibility=0
steel_exposed* futures_available	**-0.029*** (-3.81)**	**-0.009 (-1.30)**	**-0.088*** (-3.91)**	**-0.035 (-1.16)**	**0.031*** (3.65)**	**0.000 (0.04)**
size	0.011	0.009	-0.012	0.003	0.018*	0.001
	(1.07)	(1.32)	(-0.55)	(0.11)	(1.85)	(0.14)
Tobins_Q	-0.011*	0.002	-0.018	-0.006	-0.003	-0.007
	(-1.74)	(0.47)	(-1.09)	(-0.38)	(-0.39)	(-1.11)
profitability	-0.038*	-0.041***	0.032	-0.036	-0.099***	-0.066***
	(-1.83)	(-3.93)	(0.56)	(-0.91)	(-3.96)	(-4.04)
cash	-0.020	-0.032*	-0.010	-0.047	-0.013	-0.009
	(-0.64)	(-1.67)	(-0.11)	(-0.66)	(-0.37)	(-0.38)
cf_volatility	-0.072	-0.022	-0.033	-0.148	0.002	-0.039
	(-0.89)	(-0.44)	(-0.16)	(-0.86)	(0.03)	(-0.66)
Constant	0.085	0.016	0.535***	0.354**	0.043	0.132**
	(1.21)	(0.38)	(3.43)	(2.39)	(0.64)	(2.52)
Diff(coef)	-0.020***		-0.052***		0.030**	
p-value	0.000		0.002		0.000	
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,373	2,965	3,373	2,965	3,373	2,965
Adjusted R-squared	0.792	0.656	0.727	0.553	0.711	0.689

Panel B: COGS Volatility

VARIABLES	secdebt_asset high_ COGS_vol=1	 high_ COGS_vol=0	secdebt_debt high_ COGS_vol=1	 high_ COGS_vol=0	nonsecdebt_asset high_ COGS_vol=1	 high _COGS_vol=0
steel_exposed* futures_available	**-0.019** (-2.22)**	**-0.010 (-1.47)**	**-0.089*** (-2.88)**	**0.001 (0.04)**	**0.027*** (2.95)**	**0.011 (1.04)**
size	0.023***	-0.018	0.006	-0.054*	0.011	0.025*
	(2.91)	(-1.59)	(0.24)	(-1.66)	(1.34)	(1.70)
Tobins_Q	0.002	-0.004	0.002	-0.013	-0.016**	-0.008
	(0.31)	(-0.64)	(0.08)	(-0.65)	(-2.06)	(-0.72)
tangibility	0.096*	0.014	0.072	0.070	0.133**	-0.000
	(1.87)	(0.29)	(0.59)	(0.41)	(2.45)	(-0.00)
profitability	-0.066***	-0.043	-0.113**	0.030	-0.082***	-0.083***
	(-4.08)	(-1.61)	(-1.99)	(0.44)	(-3.13)	(-2.62)
cash	0.021	0.003	-0.011	-0.028	0.010	-0.014
	(1.02)	(0.10)	(-0.14)	(-0.22)	(0.31)	(-0.37)
cf_volatility	0.024	-0.157*	-0.235	-0.716***	-0.021	-0.003
	(0.37)	(-1.90)	(-1.11)	(-2.62)	(-0.30)	(-0.02)
Constant	-0.083	0.209***	0.336**	0.715***	0.053	0.006
	(-1.57)	(2.61)	(1.99)	(2.94)	(0.89)	(0.05)
Diff(coef)	-0.008**		-0.090***		-0.016***	
p-value	0.050		0.000		0.005	
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,133	2,166	2,133	2,166	2,133	2,166
Adjusted R-squared	0.720	0.790	0.603	0.712	0.692	0.709

Table 1.11: Cross-sectional evidence – Benefits and costs of secured debt usage

Table 1.11 presents the results of subsample analysis split on variables related to relative benefits and costs of secured debt usage. In Panel A, firms are split on *redeployability*. *low_redeploy* equals one if a firm has bottom tercile pre-introduction average *redeployability* and zero if top tercile. In Panel B, firms are split on the degree of operating inflexibility (*INFLEX*). *high_INFLEX* equals one if a firm has top tercile pre-introduction average *INFLEX* and zero if bottom tercile. t-statistics based on standard errors clustered at firm level are reported in parentheses below the coefficient estimates. *, **, *** represent statistical significance at the 10%, 5%, and 1% level, respectively.

Panel A: Redeployability

	secdebt_asset		secdebt_debt		nonsecdebt_asset	
VARIABLES	low_ redeploy=1	low_ redeploy=0	low_ redeploy=1	low_ redeploy=0	low_ redeploy=1	low_ redeploy=0
steel_exposed* futures_available	**-0.022*** (-2.60)**	**-0.008 (-0.71)**	**-0.085*** (-3.46)**	**-0.027 (-0.62)**	**0.030*** (3.37)**	**0.001 (0.06)**
size	0.006	0.023**	-0.019	0.015	0.024**	0.007
	(0.86)	(2.17)	(-0.82)	(0.56)	(2.56)	(0.81)
Tobins_Q	-0.000	0.002	-0.007	-0.009	-0.013**	-0.002
	(-0.07)	(0.28)	(-0.49)	(-0.40)	(-2.40)	(-0.32)
tangibility	0.075*	0.105*	0.105	0.063	0.004	0.037
	(1.81)	(1.72)	(0.99)	(0.51)	(0.08)	(0.87)
profitability	-0.026*	-0.058***	0.063	-0.037	-0.103***	-0.043
	(-1.79)	(-3.31)	(1.45)	(-0.68)	(-4.76)	(-1.47)
cash	-0.018	-0.007	0.045	-0.042	-0.010	-0.011
	(-0.81)	(-0.23)	(0.50)	(-0.47)	(-0.32)	(-0.43)
cf_volatility	-0.069	0.102	-0.117	0.062	0.067	-0.012
	(-1.05)	(1.47)	(-0.56)	(0.30)	(0.80)	(-0.17)
Constant	0.046	-0.095	0.505***	0.252	0.003	0.094
	(0.82)	(-1.21)	(3.01)	(1.33)	(0.04)	(1.49)
Diff(coef)	-0.013***		-0.059***		0.029***	
p-value	0.002		0.000		0.000	
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,126	3,014	3,126	3,014	3,126	3,014
Adjusted R-squared	0.776	0.743	0.672	0.645	0.688	0.732

Panel B: Inflexibility (INFLEX)

	secdebt_asset		secdebt_debt		nonsecdebt_asset	
VARIABLES	high_ INFLEX=1	high_ INFLEX=0	high_ INFLEX=1	high_ INFLEX=0	high_ INFLEX=1	high_ INFLEX=0
steel_exposed* futures_available	**-0.016** (-2.19)**	**-0.011 (-1.60)**	**-0.081*** (-3.16)**	**-0.002 (-0.08)**	**0.027*** (2.89)**	**-0.007 (-0.85)**
size	0.006	0.011	0.002	0.003	0.021**	0.012
	(0.74)	(1.36)	(0.09)	(0.11)	(2.51)	(1.42)
Tobins_Q	0.005	-0.001	0.004	0.002	-0.006	-0.007
	(0.99)	(-0.17)	(0.25)	(0.08)	(-1.14)	(-1.03)
tangibility	0.063	-0.011	-0.005	-0.125	0.061	0.056
	(1.32)	(-0.18)	(-0.05)	(-0.92)	(1.38)	(1.17)
profitability	-0.030**	-0.089***	0.005	-0.082	-0.085***	-0.054**
	(-2.37)	(-3.54)	(0.10)	(-1.30)	(-4.58)	(-2.14)
cash	-0.030	-0.060*	-0.114	-0.058	0.012	0.009
	(-1.35)	(-1.80)	(-1.40)	(-0.63)	(0.48)	(0.25)
cf_volatility	-0.061	-0.033	-0.206	-0.306	0.072	0.020
	(-1.23)	(-0.35)	(-1.20)	(-1.06)	(1.31)	(0.20)
Constant	0.037	0.028	0.405***	0.354*	-0.012	0.071
	(0.70)	(0.46)	(2.85)	(1.87)	(-0.21)	(1.01)
Diff(coef)	-0.005*		-0.079***		-0.034***	
p-value	0.080		0.000		0.000	
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,931	3,063	2,931	3,063	2,931	3,063
Adjusted R-squared	0.699	0.768	0.591	0.722	0.652	0.764

Table 1.12: Cross-sectional evidence – Financial health

Table 1.12 presents the results of subsample analysis split on firm financial health as measured by Altman's Z score. Instead of splitting the sample solely based on the Z score, I implement a two-way independent sort of firms based on *tangibility* and Altman's Z score to address the potential monotonic relation between the financial health proxy and tangibility. The cut points for 2x2 independent sorts are pre-introduction Z score below or above 3 and median *tangibility.* Because *tangibility* is one of the sorting variables, I do not include *tangibility* as a control variable in regressions. In Panel A, the dependent variable is *secdebt_asset.* In Panel B, the dependent variable is *secdebt_debt.* In Panel C, the dependent variable is *nonsecdebt_asset. t*-statistics based on standard errors clustered at firm level are reported in parentheses below the coefficient estimates. *, **, *** represent statistical significance at the 10%, 5%, and 1% level, respectively.

Panel A: secdebt_asset by independent sorts on Altman's Z score and tangibility

	secdebt_asset			
VARIABLES	Z<3 | high tangible	Z<3 | low tangible	Z>=3 | high tangible	Z>=3 | low tangible
steel_exposed*futures_available	**-0.043*** **	**-0.015**	**-0.011**	**-0.007**
	(-3.52)	**(-1.04)**	**(-1.04)**	**(-0.92)**
size	0.015	0.011	0.014	-0.001
	(0.98)	(0.92)	(0.92)	(-0.07)
Tobins_Q	-0.004	0.007	-0.006	0.004
	(-0.34)	(0.71)	(-1.33)	(0.98)
profitability	-0.028	-0.047**	-0.039	-0.031**
	(-0.95)	(-2.51)	(-1.46)	(-2.20)
cash	0.019	0.018	-0.037	-0.041*
	(0.38)	(0.60)	(-0.92)	(-1.78)
cf_volatility	-0.079	-0.007	0.023	-0.162**
	(-0.64)	(-0.09)	(0.32)	(-2.42)
Constant	0.094	0.010	-0.019	0.057
	(0.83)	(0.13)	(-0.18)	(1.17)
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	1,671	1,171	1,305	1,483
Adjusted R-squared	0.780	0.644	0.723	0.612

Panel B: secdebt_debt by independent sorts on Altman's Z score and tangibility

	secdebt_debt			
VARIABLES	Z<3 | high tangible	Z<3 | low tangible	Z>=3 | high tangible	Z>=3 | low tangible
steel_exposed*futures_available	**-0.119*** **	**-0.032**	**-0.007**	**-0.021**
	(-4.12)	**(-0.69)**	**(-0.19)**	**(-0.53)**
size	-0.009	0.039	-0.006	-0.006
	(-0.32)	(1.24)	(-0.12)	(-0.19)
Tobins_Q	-0.004	-0.006	-0.007	0.015
	(-0.13)	(-0.23)	(-0.32)	(0.77)
profitability	0.056	-0.005	0.035	-0.035
	(0.76)	(-0.09)	(0.31)	(-0.58)
cash	-0.049	0.123	0.086	-0.148
	(-0.47)	(1.24)	(0.54)	(-1.30)
cf_volatility	-0.028	0.109	-0.011	-0.459*
	(-0.09)	(0.41)	(-0.04)	(-1.72)
Constant	0.558***	0.101	0.368	0.407*
	(2.60)	(0.48)	(1.10)	(1.90)
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	1,671	1,171	1,305	1,483
Adjusted R-squared	0.737	0.528	0.716	0.604

Table 1.12 (continued)

Panel C: nonsecdebt_asset by independent sorts on Altman's Z score and tangibility

	nonsecdebt_asset			
VARIABLES	Z<3 | high tangible	Z<3 | low tangible	Z>=3 | high tangible	Z>=3 | low tangible
steel_exposed*futures_available	**0.053*** **	**0.015**	**-0.009**	**0.011**
	(4.21)	**(0.82)**	**(-0.87)**	**(1.10)**
size	0.018	-0.001	0.003	0.007
	(1.39)	(-0.08)	(0.21)	(0.59)
Tobins_Q	-0.009	-0.005	-0.005	-0.013
	(-0.64)	(-0.53)	(-0.79)	(-1.61)
profitability	-0.095**	-0.091***	-0.056	-0.045*
	(-2.56)	(-3.44)	(-1.55)	(-1.78)
cash	-0.029	-0.053	-0.053	0.056
	(-0.50)	(-1.44)	(-1.35)	(1.24)
cf_volatility	0.025	-0.098	-0.034	-0.046
	(0.17)	(-1.05)	(-0.41)	(-0.50)
Constant	0.063	0.174**	0.117	0.067
	(0.64)	(2.08)	(1.26)	(0.87)
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	1,671	1,171	1,305	1,483
Adjusted R-squared	0.659	0.628	0.740	0.734

Figure 1.1: Difference-in-differences – Dynamics of debt variables responses

Figure 1.1 plots coefficient estimates from the following dynamic difference-in-differences regression model:

$$Debt\ Variable_{it} = \alpha + \sum_{\tau=-3,\tau\neq-1}^{\tau=3} \beta_\tau\, steel\_exposed_i * \mathbb{I}\{t=\tau\} + \gamma X_{it-1} + Firm\ FE + Year\ FE + \varepsilon_{it}$$

where $steel\_exposed_i$ equals one if steel takes up greater than or equal to 1% of a firm's inputs on average during the pre-introduction period and zero otherwise; and X_{it-1} is a vector of controls lagged by one year. As this analysis is based on fiscal year-ends, the pre-introduction period is from 2005 ($\tau = -3$) to 2007 ($\tau = -1$), and the post-introduction period is from 2008 ($\tau = 0$) to 2011 ($\tau = 3$). The fiscal year 2007 ($\tau = -1$) is excluded as the reference level. The red dots correspond to estimates of the β_τ coefficients. The vertical bars correspond to 95% confidence intervals. Standard errors are clustered at the firm level. For Figure 1.1.a, the debt variable is *secdebt_debt*. For Figure 1.1.b, the debt variable is *nonsecdebt_asset*.

Figure 1.1.a: Secured Debt to Total Debt Ratio (*secdebt_debt*)

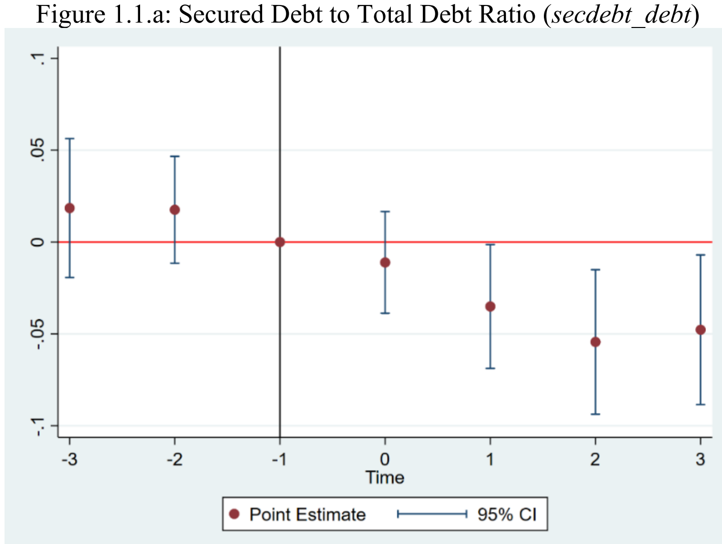


Figure 1.1.b: Unsecured Debt to Asset Ratio (*nonsecdebt_asset*)

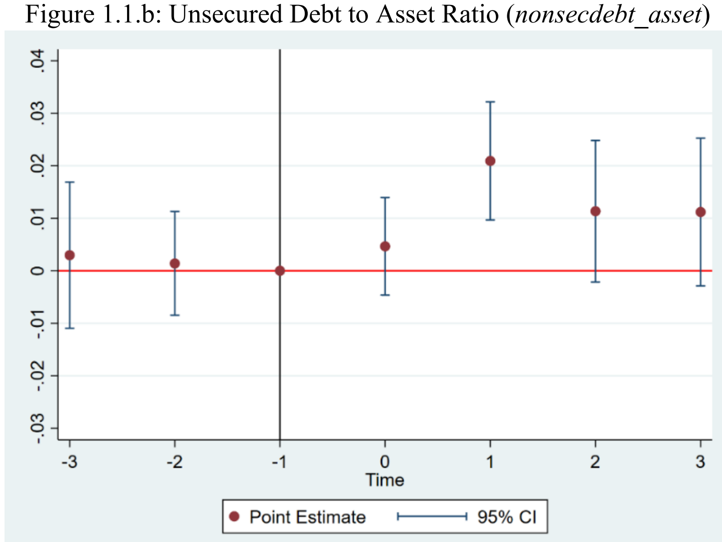


Figure 1.2: Dynamics of debt variables responses (PSM matched sample)

Figure 1.2 plots coefficient estimates from the following dynamic difference-in-differences regression model using the PSM matched sample:

$$Debt\ Variable_{it} = \alpha + \sum_{\tau=-3,\tau\neq-1}^{\tau=3} \beta_\tau\, steel\_exposed_i * \mathbb{I}\{t=\tau\} + \gamma X_{it-1} + Firm\ FE + Year\ FE + \varepsilon_{it}$$

where $steel\_exposed_i$ equals one if steel takes up greater than or equal to 1% of a firm's inputs on average during the pre-introduction period and zero otherwise; and X_{it-1} is a vector of controls lagged by one year. As this analysis is based on fiscal year-ends, the pre-introduction period is from 2005 ($\tau = -3$) to 2007 ($\tau = -1$), and the post-introduction period is from 2008 ($\tau = 0$) to 2011 ($\tau = 3$). The fiscal year 2007 ($\tau = -1$) is excluded as the reference level. The red dots correspond to estimates of the β_τ coefficients. The vertical bars correspond to 95% confidence intervals. Standard errors are clustered at the firm level. For Figure 1.2.a, the debt variable is *secdebt_debt*. For Figure 1.2.b, the debt variable is *nonsecdebt_asset*.

Figure 1.2.a: Secured Debt to Total Debt Ratio (*secdebt_debt*)

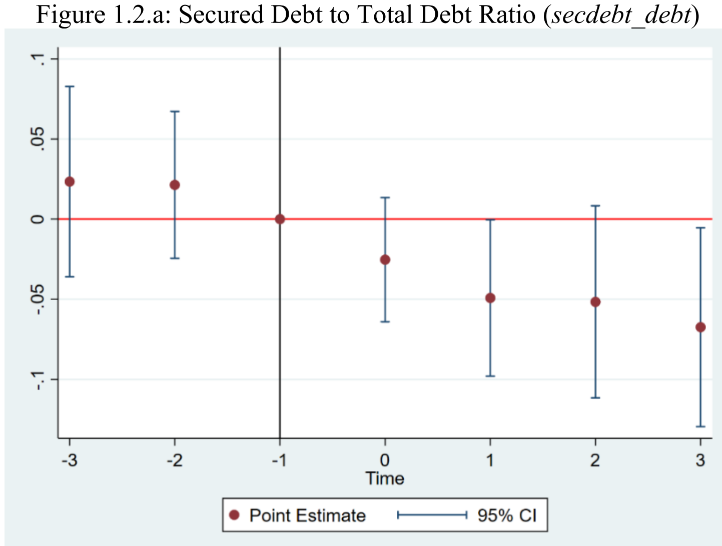
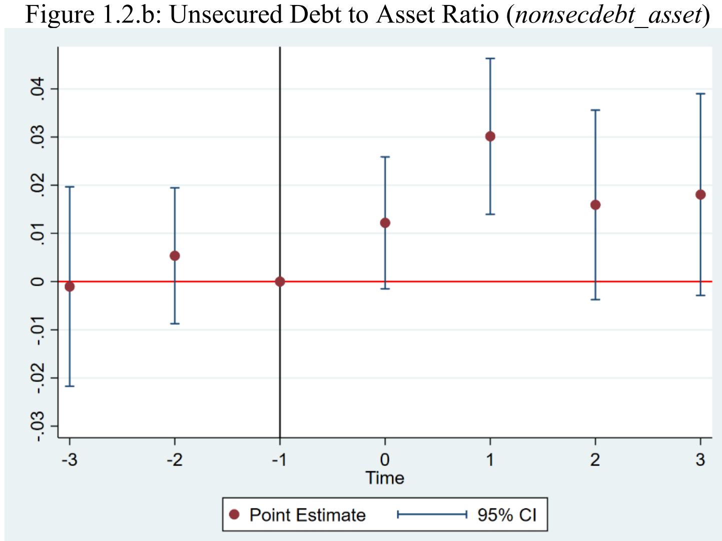


Figure 1.2.b: Unsecured Debt to Asset Ratio (*nonsecdebt_asset*)



CHAPTER 2. Stock Price Informativeness and Debt Heterogeneity

2.1 Introduction

Information asymmetry is central to capital structure decisions (Flannery, 1986; Bharath et al., 2009) – affecting both monitoring and expected distress costs. Yet little is known about how it affects debt heterogeneity. As debt is the primary source of financing for a majority of firms in the U.S. (e.g., Graham et al., 2015), the extent to which firms diversify or concentrate debt structure has the potential to alter corporate outcomes through its implications on bankruptcy resolution, financial and operational flexibility imposed by debt contracts, and overall debt repayment incentives (Ivashina et al., 2016; Lou and Otto, 2020; Zhong, 2021). Moreover, the rise of passive investing has reduced the information content of equity prices (e.g., Israeli et al., 2017; Sammon, 2023; Morck and Yavuz, 2024), making it imperative to understand how changes in the institutional investing landscape affect capital structure decisions. Despite such timeliness and relevance, no studies have explored the direct link between stock price informativeness (SPI) and debt concentration. This paper fills this gap by providing causal evidence that SPI significantly affects the degree of debt heterogeneity.

Bolton and Scharfstein (1996) provide a theoretical framework to motivate the link between stock price informativeness and debt concentration. Noting that a debt structure with more dispersed ownership by multiple creditor types can exacerbate the risk of coordination failure and conflicts of interest among the creditors, their theory highlights the optimal level of debt heterogeneity balances the benefit of deterring strategic default against the higher distress costs associated with the higher vulnerability to liquidity defaults. To the extent that changes in information asymmetry affect either expected distress costs or the effectiveness of disciplinary governance mechanisms, this should induce firms to rebalance their debt composition. That said, there are competing predictions on how the information content of prices should affect the heterogeneity of debt.

On the one hand, stock price informativeness may increase debt heterogeneity. First, high SPI of the prospective borrowers can be desirable to creditors because it helps improve the accuracy of equity market-based default prediction models, which leads to lower credit prices and costs of financial distress to the borrowing firms (Maffett and Owens, 2018). Moreover, price discovery by informed traders can alleviate information collection and monitoring costs borne by creditors through the information spillover and cross-monitoring channels, ultimately alleviating the general information asymmetry problems between the firm and creditors (Billett et al., 2020; Ho et al., 2022). Relatedly, Brogaard et al. (2017) provide evidence that enhanced stock liquidity decreases firm default risk mainly through the improved stock price informativeness channel. Collectively, these suggest that enhanced price informativeness should lead to lower financial distress costs for borrowing firms and reduce the cost of debt heterogeneity in Bolton and Scharfstein (1996)'s framework. Accordingly, one may expect a positive relation between SPI and debt heterogeneity, assuming that the strategic default deterrence feature of debt heterogeneity is recognized and utilized by firms. I term this prediction the "reduced expected financial distress costs" (or "distress costs") hypothesis.

On the other hand, a competing prediction can be made if the information content of equity prices buttresses corporate governance. For example, Holmstrom and Tirole (1993) note that more informative prices enable firms to write more efficient managerial contracts. Thus, if firms actively substitute external and internal governance mechanisms depending on their relative efficacy, changes in governance or monitoring costs stemming from stock price informativeness may lessen the firm's reliance on debt heterogeneity to deter managers from pursuing strategic defaults or excessively risky managerial behavior (Lin, 2022). Therefore, the "substitution of governance mechanisms" (or "governance substitution") hypothesis dictates that stock price informativeness should be negatively related to debt heterogeneity. In this regard, the direction of the effect of stock price informativeness on the degree of debt heterogeneity is ex-ante unclear and purely an empirical question.

To test the predictions outlined above, I formally define debt heterogeneity as the degree of dispersion in the allocation of total debt into different types or sources throughout the study and measure it as one minus a normalized HHI across the seven different debt types used by firms using Capital IQ debt structure data, following Colla et al. (2013). I augment this continuous measure of debt heterogeneity with a count measure, the number of different debt types that the firms use. To measure stock price informativeness, I adopt probability of informed trading (PIN) and price nonsynchronicity, which are widely adopted SPI measures in the literature (e.g., Chen et al., 2007; Ferreira et al., 2011; Bennett, Stulz, and Wang, 2020). I also construct stock price fragility developed by Greenwood and Thesmar (2011) and use it as a negative proxy for SPI in the tests designed to address endogeneity concerns. Importantly, all three SPI measures are calculated based on the average of quarterly SPI measures over the previous four quarters (i.e., from q-4 to q-1) to alleviate potential reverse causality concern.

As preliminary evidence, I first show results from a univariate analysis in which I sort firms into deciles according to their stock price informativeness after controlling for size. As shown in Figure 2.1, I find that the degree of debt heterogeneity monotonically increases with SPI in a steep fashion, measured with either PIN or price nonsynchronicity. To investigate the relation between SPI and debt concentration structure in a more formal fashion, I estimate panel OLS regressions of debt heterogeneity on SPI proxies and a host of firm-level control variables in addition to firm and industry-by-year fixed effects. I find strong evidence that debt heterogeneity is positively associated with SPI, corroborating the patterns revealed from the univariate analysis. Specifically, a one standard deviation increase in SPI measure is associated with a 4.8% to 5.2% increase in debt heterogeneity relative to the sample mean, all statistically significant at less than 1% level. The baseline result is robust to the alternative measurement of debt heterogeneity with the count measure. As all baseline specifications directly control for book leverage ratio, it is less likely that the results are driven by the mechanical positive correlation between the amount of debt employed by a firm and the degree of heterogeneity in the existing debt structure. Overall, both univariate and baseline panel OLS results strongly support the reduced expected distress costs hypothesis over the governance substitution hypothesis by revealing a robust positive relationship between SPI and a more heterogeneous debt structure.

Although the baseline results show strong evidence of the positive impact of stock price informativeness on debt heterogeneity, potential endogeneity concerns make it difficult to establish causality from SPI to debt concentration structure. One prominent example of such concern could be that a time-varying shift in firm-specific external information environment that is unobservable to an econometrician can influence both investors' incentives to collect private information for their trading and firm debt capacity.

I take three approaches to address endogeneity issues. First, I provide panel evidence relating debt heterogeneity to stock price fragility (Greenwood and Thesmar, 2011). Stock price fragility quantifies the exposure to expected non-fundamental price movements as a function of institutional investor ownership concentration and correlations of owners' expected liquidity-driven trades. As the main source of stock price disruption arises from institutional owners' correlated liquidity trading needs, it is arguably a cleaner (negative) proxy for SPI that is less correlated with firm fundamentals (Friberg, Goldstein, and Hankins, 2024). Consistent with the baseline results, I find that stock price fragility is negatively associated with debt heterogeneity. Specifically, a one standard deviation increase in stock price fragility is associated with a 3.6% decrease in debt heterogeneity relative to the sample mean, statistically significant at less than 1% level. The results are robust to the alternative measurement of debt heterogeneity with the count measure. Moreover, I find zero book leverage response by firms to changes in stock price fragility, lending credence to the argument that I am genuinely capturing firms' debt "structure" adjustments in response to the changes in SPI, not merely capturing the mechanical correlation between firm debt usage and debt heterogeneity.

Second, I implement difference-in-differences (DiD) analysis in a quasi-natural experiment setting that exploits the BlackRock-Barclays Global Investors (BGI) merger in 2009 as an exogenous shock to stock price fragility. The idea behind this quasi-natural experiment setting is that a merger between two large financial institutions that both run a family of numerous funds can significantly change the stock price fragility of affected firms by increasing the degree of ownership concentration of portfolio companies and the expected correlation of investor flows between the individual funds under the merged entity. Following Friberg, Goldstein, and Hankins (2024), I define treated firms as those jointly held by BGI and BlackRock before the merger announcement, while control firms are those held by only one of BGI and BlackRock during the same period. Again, consistent with the baseline and fragility panel evidence, I find that treated firms, which indeed experience a substantial increase in stock price fragility, adjust toward a more concentrated, or less heterogeneous, debt structure relative to control firms after the merger event. Dynamics of the coefficient estimates reveal no significant evidence of pre-trends, and similar results hold in the tests using propensity score-matched sample. Overall, the results from the quasi-natural experiment exploiting the BlackRock-BGI merger help support the causal interpretation of the results in this study, corroborating credence to the distress costs hypothesis.

Third, I use an instrumental variable (IV) approach to provide further evidence that the positive effect of stock price informativeness on debt heterogeneity is likely causal. I instrument SPI measures with the natural log of the fiscal year-beginning nominal stock price level. Chan et al. (2017) find that high stock price levels impede informed trading on stocks and thus reduce the informativeness of stock prices. After all, some degree of uninformed trading is needed to facilitate informed trading, and high nominal stock prices can create barriers for uninformed investors to enter the market by imposing budget constraints. The IV analysis shows that the positive impact of SPI on debt heterogeneity remains both statistically and economically significant, regardless of different SPI measures. The magnitude, statistical significance, and F-statistics of the first-stage results alleviate the weak instrument concern. Moreover, I am not particularly aware of any theories or empirical evidence relating the nominal stock price level to corporate debt concentration structure through alternative channels as convincing as the SPI channel. In sum, the IV-2SLS analysis provides support for the causal effects consistent with the distress costs hypothesis by alleviating concerns of potential endogeneity issues.

Lastly, I conduct subsample analyses exploiting the cross-sectional nature of the sample to provide evidence on the channels through which SPI affects corporate debt concentration structure. I explore two specific channels that can bolster support for the validity of the proposition that the results in this study are indeed in line with the predictions from the distress costs hypothesis: the expected distress cost channel and the information asymmetry channel.

When it comes to the expected distress cost channel, I expect more significant effects of SPI for firms with higher expected distress costs. Intuitively, if high SPI lowers financial distress costs which in turn leads to a more heterogeneous debt structure based on the trade-off framework suggested by Bolton and Scharfstein (1996), the marginal decline in such costs in response to the improved SPI should be higher for firms facing higher costs of financial distress. Consistent with this prediction, I find that the relation between SPI and debt heterogeneity is only significant for firms with higher default probability, higher cash flow volatility, and lower asset redeployability. These results buttress the interpretation that SPI induces firms to choose a more heterogenous debt structure by indeed reducing distress costs, one of the main mechanisms suggested by the distress costs hypothesis.

The main implication of the information asymmetry channel is that the effect of SPI on debt heterogeneity should be more pronounced for firms with a higher degree of information asymmetry, as the marginal reduction in both the information risk and the general level of information asymmetry in response to the improved SPI should be greater for those types of firms. Consistent with this prediction, I find that the relation between SPI and debt heterogeneity is only significant for small firms, firms with higher exposure to uncertainty, and, among the sample of unrated firms, firms with higher reliance on bank debt. These findings strongly support the notion that mitigating the information risk and alleviating overall information asymmetry indeed underlies the positive relationship between SPI and debt heterogeneity.

This study extends the literature that explores the determinants of firms' debt concentration structure. Colla et al. (2013) examine debt heterogeneity within a firm's debt structure and find that there exists a significant cross-sectional heterogeneity in the degree of debt concentration by firms, calling for future research on potential explanations behind such cross-sectional heterogeneity in debt concentration structure. While existing studies have suggested accounting quality (Li et al., 2021), CEO risk-taking incentives (Castro et al., 2020), and country-level creditor protection (John et al., 2021) as meaningful determinants of the degree of debt heterogeneity chosen by firms, no studies have systematically examined the potential link between stock price informativeness and debt heterogeneity. My work fills this gap by providing robust causal evidence that SPI positively impacts the degree of debt heterogeneity, suggesting SPI as another important determinant of debt concentration structure distinctive from the existing list of determinants revealed by the previous studies.

I also contribute to the voluminous literature on the real effects of financial markets (Bond et al., 2012). Numerous studies have shown that the informativeness of secondary market prices has implications for corporate decisions or outcomes, mainly through the learning from prices channel (e.g., Chen et al., 2007; Ben-Nasr and Alshwer, 2016; Brogaard et al., 2017). I extend this line of research by providing evidence that SPI significantly influences firms' debt concentration structure adjustment through the channels that are distinctive from the learning channel. Moreover, given that debt heterogeneity has implications on bankruptcy resolution, financial and operational flexibility, and debt repayment incentives, my findings provide suggestive evidence of a potential mechanism, that is, a debt structure channel, through which the information content of equity market prices ultimately alters various firm-level outcomes.

The rest of the chapter is organized as follows. Section 2.2 describes the data, sample, and variables used in the analysis. Section 2.3 presents the main empirical findings. Section 2.4 provides cross-sectional evidence from subsample analyses. Section 2.5 concludes.

2.2 Variables and Sample Description

Detailed definition and calculation of the variables are shown in Appendix B (Table B.1).

2.2.1 Debt Concentration Structure Measures

I obtain firm-level debt structure data from Capital IQ to construct measures of debt concentration structure. Capital IQ breaks down each firm's total debt into seven different debt types that are mutually exclusive: commercial paper (CP), drawn credit lines (DC), term loans (TL), senior bonds and notes (SBN), subordinated bonds and notes (SUB), capital leases (CL), and other debt (Other). Following Colla et al. (2013) and Lou and Otto (2020), I adopt *Debt_Heterogeneity*, a measure based on the normalized Herfindahl-Hirschman Index (HHI) of the different debt types used by a firm, as the main measure of debt concentration structure throughout the study. Specifically, *Debt_Heterogeneity* is calculated as below;

$$Debt\_Heterogeneity_{it} = 1 - \left(\frac{SS_{it} - \frac{1}{7}}{1 - \frac{1}{7}} \right)$$

$$SS_{it} = \left(\frac{CP_{it}}{TD_{it}}\right)^2 + \left(\frac{DC_{it}}{TD_{it}}\right)^2 + \left(\frac{TL_{it}}{TD_{it}}\right)^2 + \left(\frac{SBN_{it}}{TD_{it}}\right)^2 + \left(\frac{SUB_{it}}{TD_{it}}\right)^2 + \left(\frac{CL_{it}}{TD_{it}}\right)^2 + \left(\frac{Other_{it}}{TD_{it}}\right)^2$$

where *TD* denotes the total amount of debt. By construction, *Debt_Heterogeneity* ranges between 0 and 1. Firms with zero *Debt_Heterogeneity* are those that rely only on a single debt type. On the other extreme of the spectrum, firms with *Debt_Heterogeneity* equals one are those that employ all seven types of debt in equal proportion. In this regard, *Debt_Heterogeneity* can be viewed as a continuous measure of debt concentration structure quantifying the degree of dispersion in the allocation of total debt into different types or sources, with higher *Debt_Heterogeneity* representing a more heterogenous (or less concentrated) structure in firms' existing debt.

In addition, I also calculate *Num_Debt_Types*, which counts the number of debt types found in a firm's existing debt structure. When counting debt types, I follow Li et al. (2021) and consider only debt types that account for at least five percent of a firm's total debt to prevent debt types making up only a negligible proportion (e.g., less than one percent) in a firm's debt structure from being treated equally as the debt type(s) with much higher economic substance. By construction, *Num_Debt_Types* range between 1 to 7, and, similar to *Debt_Heterogeneity*, higher *Num_Debt_Types* represents a more heterogenous (or less concentrated) debt structure.

2.2.2 Stock Price Informativeness (SPI) Measures

Throughout the study, I introduce and employ three measures of stock price informativeness, each constructed or obtained from different data sources. The three measures are calculated based on the probability of informed trading, price nonsynchronicity, and stock price fragility, respectively, which I describe one by one in order.

2.2.2.1 Probability of Informed Trading (PIN)

The first SPI measure used in this paper is based on the probability of informed trading (PIN), which is derived from structural market microstructure model by Easley et al. (1996). The calculation of the PIN of a stock is based on several parameters that can be estimated using intra-day transaction data such as TAQ data;

$$PIN = \frac{\alpha * \mu}{\alpha * \mu + (\varepsilon_b + \varepsilon_s)}$$

where α is the probability of new (private) information emerging, μ is the daily arrival rate of orders by informed traders, and ε_b and ε_s represent the daily arrival rate of uninformed buy and sell orders, respectively. In other words, PIN can be conceptualized as the ratio of the arrival rate of informed orders to the arrival rate of all orders, both uninformed and informed. Naturally, one would expect a higher PIN should be associated with higher informativeness of stock prices, as new and value-relevant information is more likely to be incorporated when there is more informed trading in a stock. In line with this reasoning, several existing studies in corporate finance have used PIN as a valid measure of stock price informativeness (e.g., Chen et al., 2007; Ferreira and Laux, 2007; Ferreira et al., 2011; Ben-Nasr and Alshwer, 2016; Bennett, Stulz, and Wang, 2020).

I obtain the probability of informed trading measure estimated at the quarterly frequency ("quarterly PINs") from Stephen Brown's website[1]. In a similar spirit to Bennett, Stulz, and Wang (2020), I define *PIN* in this study as the average of quarterly PINs over the previous four quarters (i.e., q-4 to q-1). Calculated *PIN*s are matched to other firm-level variables at fiscal year t with corresponding calendar quarter q. Calculating an SPI measure in this manner helps alleviate reverse causality or simultaneity concerns in regression frameworks. As the website provides the quarterly PIN measure from 1993 to 2010, analyses using *PIN* as the main SPI measure spans from fiscal years 2003 to 2010 (valid Capital IQ sample starts from the fiscal year 2003, as explained later).

2.2.2.2 Price Nonsynchronicity (NONSYNC)

Another main SPI measure is based on price nonsynchronicity, also commonly referred to as $1-R^2$ or firm-specific return variation. Using stock return data in CRSP daily file, I obtain firm-level price nonsynchronicity by first estimating the following regression model;

$$R_{it} = \alpha_i + \beta_{1i} R_{m,t} + \beta_{2i} R_{ind,t} + u_{it}$$

where R_{it} is stock *i*'s return on day t, $R_{m,t}$ is CRSP value-weighted market return on day t, and $R_{ind,t}$ is the return on the Fama and French 48-industry portfolio to which firm i belongs on day t. I estimate the above regression for a quarter interval on a rolling basis,

[1] https://terpconnect.umd.edu/~stephenb/EKOpins.html

requiring a firm-quarter to have a minimum of 30 trading days (given about 63 trading days in a quarter). I then keep the estimated R^2 from the regressions and take a logistic transformation of it to address the boundedness and skewness of R^2 as below;

$$Price\ Nonsynchronicity = \ln\left(\frac{1 - R^2}{R^2}\right)$$

The intuition behind price nonsynchronicity as a measure of SPI is that a firm's stock prices comove less with the market and the industry (which is translated into a lower R^2 or higher firm-specific residual return variation) when there is relatively more firm-specific information impounded into stock prices. In other words, the stock price should be more informative when a stock features less synchronicity with respect to the market and the industry price movements, as it may reflect a higher level of firm-specific information being incorporated into stock prices. In line with this reasoning, price nonsynchronicity is also widely adopted in the literature in corporate finance that studies various implications of stock price informativeness (e.g., Chen et al., 2007; Ferreira et al., 2011; Bennett, Stulz, and Wang, 2020).

I define *NONSYNC* as the average of quarterly price nonsynchronicity over the previous four quarters (i.e., q-4 to q-1). Calculated *NONSYNC*s are matched to other firm-level variables at fiscal year t with corresponding calendar quarter q. Analyses using *NONSYNC* as the main SPI measure span from fiscal years 2003 to 2018.

2.2.2.3 Stock Price Fragility (StkFrag)

I also adopt stock price fragility by Greenwood and Thesmar (2011) as one of the main SPI variables. Stock price fragility captures a firm's exposure to non-fundamental price shocks driven by institutional owners' liquidity-driven trading activities. Specifically, stock price fragility of stock i at time t is defined as below;

$$fragility_{it} = \left(\frac{1}{\theta_{it}}\right)^2 W_{it}' \Omega_t W_{it}$$

where θ_{it} is the market capitalization of the firm's stock, W_{it} is a vector of each mutual fund's portfolio allocation weight to stock i, and Ω_t is the variance-covariance matrix dollar fund flows. More intuitively, stock price fragility quantifies the exposure to expected non-fundamental price movements as a function of institutional investor ownership concentration and correlations of owners' expected liquidity-driven trades. Diversified ownership can minimize the price impact from idiosyncratic fund flow shocks experienced by an individual mutual fund since such shocks are more likely to be absorbed by other entities through the canceling trades. However, diversified ownership cannot fully mitigate the price impact from flow-driven shocks if the owners' liquidity shocks and trading needs are highly correlated. Therefore, the higher the ownership concentration and the higher the expected correlation of the liquidity trading needs to be faced by owners, the higher stock price fragility the portfolio firms face. The solid theoretical framework behind the measure and sophisticated estimation procedure that closely mimics the original model's intuition make stock price fragility a desirable candidate proxy for stock price informativeness. Importantly, as the main source of stock price disruption stems from individual institutional owner's funding dynamics and a stock's ownership structure determined prior to flow shock realizations, I argue that it is a cleaner proxy for SPI in the sense that it should be less correlated with firm fundamentals. This feature of fragility helps to alleviate endogeneity concerns in regression analysis.

I calculate quarterly stock price fragility closely following the steps and assumptions outlined in Greenwood and Thesmar (2011) and Friberg, Goldstein, and Hankins (2024). I use Thomson Reuters S12 database of 13F filings for individual mutual fund holdings, CRSP mutual fund file for calculating fund flows and obtaining fund characteristics, and MFLINKS prepared by WRDS for joining the holdings data with the CRSP mutual fund file. For this study, I define *StkFrag* as the average of the square root of quarterly stock price fragility over the previous four quarters (i.e., q-4 to q-1). Calculated *StkFrag*s are matched to other firm-level variables at fiscal year t with corresponding calendar quarter q. Analyses using *StkFrag* as the main SPI measure span from fiscal years 2003 to 2018.

2.2.3 Firm-level Characteristics

I construct firm-level characteristics variables to be used as control variables in regressions using financial and accounting data from Compustat. I construct eight firm-level control variables suggested by the literature as capital structure and debt concentration structure determinants. *size, Tobins_Q, tangibility,* and *profitability* control for the standard determinants of capital structure documented in the literature (e.g., Frank and Goyal, 2009). I additionally include in regression models *cf_vol, R&D,* and *dividend_payer* to further control for potential effects of firm riskiness, information opacity, and financial constraints on debt concentration structure (e.g., Colla et al., 2013; Li et al., 2021; Castro et al., 2020; Lou and Otto, 2020; John et al., 2021). Lastly, I directly control for firm debt level by including *leverage* in regressions.

2.2.4 Sample Selection

I begin the sample selection process with the Compustat Annual Fundamentals database matched with CRSP via linktable from 2001 to 2019. The time unit of data panel is firm fiscal year (*fyear*). First, financial firms (SIC codes from 6000 through 6999), regulated utilities (SIC codes from 4900 through 4949), non-operating establishments (SIC codes from 9000 through 9999), and firms whose shares are not ordinary common shares (i.e., firm-year observations with $shrcd \neq 10 \ or \ 11$) are excluded from the sample. Then, following Colla et al. (2013), I further remove (1) firms whose shares are not traded on NYSE, NASDAQ, and AMEX; (2) firm-year observations with missing or non-positive total book assets; (3) firm-years with missing or non-positive total debt (i.e., I keep only levered firms in the sample); and (4) firm-years with book or market leverage outside the unit interval. After applying the previous series of sample screening, I additionally exclude firm-years with missing book equities and total book assets less than $10 million. Then, the resulting sample of leveraged Compustat firms is merged with Capital IQ, requiring for each firm-year that the absolute difference between Compustat total debt and the aggregated debt reported in Capital IQ does not exceed five percent of the former. I refer to the resulting sample as the *base sample*, and merge this *base sample* with one of the three SPI measures when necessary. Thus, merging the *base sample* with each SPI measure yields three different final samples, which I refer to as *PIN sample, NONSYNC sample, and Fragility sample*, respectively. For each final sample, I require that each firm has non-missing values for all debt structure variables, SPI measure, and firm-level characteristics controls.

As the coverage of Capital IQ debt structure data became comprehensive after 2002, I begin my sample period from the fiscal year 2003. For the *NONSYNC sample* and *Fragility sample*, the sample period is from fiscal years 2003 to 2018; for the *PIN sample*, the sample period is from fiscal years 2003 to 2010. Table 2.1 presents the summary statistics for the selected sample; the presented summary statistics for debt structure and firm-level control variables are based on the *NONSYNC sample* as it is the most comprehensive among the three. All stock price informativeness and continuous firm-level control variables except *size* and *leverage* are winsorized at the 1st and 99th percentile.

2.3 Empirical Results

2.3.1 Univariate Analysis

I first provide univariate evidence on the relation between stock price informativeness and debt heterogeneity. For each year, I first sort firms into deciles by size to account for its impact on both SPI and leverage ratio. Then, within each size decile for each year, I sort firms into deciles according to the informativeness of their stock prices. Finally, the time-series mean of *Debt_Heterogeneity* for each stock price informativeness decile is calculated.

Figure 2.1 provides a graphical illustration of the univariate analysis results. The figure reveals that *Debt_Heterogeneity* increases in a monotonic fashion with SPI, measured with either *PIN* (Figure 2.1.a) or *NONSYNC* (Figure 2.1.b). Specifically, from firms that belong to the lowest *PIN* (*NONSYNC*) decile to those that belong to the highest *PIN* (*NONSYNC*) decile, *Debt_Heterogeneity* increases from 0.193 (0.225) to 0.338

(0.307). These jumps in *Debt_Heterogeneity* represent about 54% (31%) relative to the sample means, which are economically meaningful.

Overall, the pattern that emerges from the univariate analysis is in support of the reduced expected distress costs hypothesis that predicts a positive relationship between SPI and a more heterogeneous debt structure. In the following subsection, I investigate the relation between SPI and debt concentration structure more formally under the regression framework that allows me to control for more firm-level fundamental differences other than firm size.

2.3.2 Baseline Panel OLS Results

To examine the relation between stock price informativeness and the degree of debt heterogeneity, I run panel OLS regressions using the following specification:

$$y_{i,t} = \alpha + \beta \times SPI_{i,t} + \gamma X_{i,t} + Firm\ FE + Ind \times Yr\ FE + \varepsilon_{i,t}$$

where $y_{i,t}$ is debt concentration structure variable (i.e., *Debt_Heterogeneity* or *Num_Debt_Types*), $SPI_{i,t}$ is the measure of stock price informativeness (i.e., *PIN* or *NONSYNC*), and $X_{i,t}$ is a vector of firm-level controls. All regression specifications include firm and industry-by-year fixed effects, where the industry is defined under Fama and French 48-industry classification. Standard errors are clustered at the firm level. Note that, as explained in the previous section, both *PIN* and *NONSYNC* are calculated based on the average of quarterly SPI measures over the previous four quarters, which is then matched to other firm-level variables at fiscal year t with corresponding calendar quarter q; this is to alleviate potential reverse causality concern, in a similar spirit to Bennett, Stulz, and Wang (2020). Also, I include *leverage* as one of the firm-level controls for all specifications to directly address the concern that the results may be driven by the mechanical positive correlation between the amount of debt employed by a firm and the degree of heterogeneity in the existing debt structure.

Table 2.2 presents the baseline results. Columns (1) to (4) report the results from the *PIN sample*, and columns (5) to (8) show the results from the *NONSYNC sample*. Throughout all specifications, the results coherently indicate that the effect of stock price informativeness on the degree of debt heterogeneity is positive and both statistically (at less than 1% level) and economically significant. Specifically, taking the results in column (2), a one standard deviation increase in *PIN* is associated with an increase of 4.8% in *Debt_Heterogeneity* relative to the sample mean. Based on the results in column (6), a one standard deviation increase in *NONSYNC* is associated with an increase of 5.2% in *Debt_Heterogeneity* relative to the sample mean. The results in columns (4) and (8) show that the positive association between SPI measures and a more heterogenous debt structure is robust to the choice of how the degree of debt heterogeneity is measured (i.e., with *Num_Debt_Types*, a count measure of debt heterogeneity). Also, the regression coefficients for β are stable regardless of whether the minimal or the full list of control variables set is included. Overall, the baseline results provide evidence consistent with the prediction from the distress costs hypothesis, indicating the positive impact of SPI on debt heterogeneity that is both statistically and economically significant.

2.3.3 Addressing Endogeneity

Although the baseline results show strong evidence of the positive impact of stock price informativeness on debt heterogeneity, potential endogeneity concerns make it difficult to establish causality from SPI to debt concentration structure. While the way

PIN and *NONSYNC* are calculated alleviates reverse causality or simultaneity concerns, there still exist potential concerns regarding omitted variable bias. For example, a time-varying shift in firm-specific external information environment that is unobservable to an econometrician can influence both investors' incentives to collect and exploit private information for their trading and firm debt capacity, justifying the observed positive relationship between SPI measures and debt heterogeneity. Moreover, there also exists a concern that time-varying unobservable firm fundamentals not adequately captured by the selected control variables affect both SPI and debt structure. Such endogenous determination of stock price informativeness and debt structure choice make claiming causal effects under a simple OLS framework challenging.

I take three approaches to address endogeneity issues. First, I provide panel evidence relating debt heterogeneity to stock price fragility (Greenwood and Thesmar, 2011). Then, I use a quasi-natural experiment exploiting the BlackRock-BGI merger event. Additionally, I use an instrumental variable (IV) approach in which SPI measures are instrumented with year-beginning nominal stock price level.

2.3.3.1 Panel Evidence with Stock Price Fragility

As previously introduced and explained, stock price fragility (*StkFrag*) of a firm captures the exposure to expected non-fundamental price movements as a function of institutional investor ownership concentration and correlations of owners' expected liquidity-driven trades. As the measure captures stock price disruption arising from institutional owners' funding dynamics and a stock's predetermined ownership structure before flow shocks, it has the virtue of being a less-confounded, cleaner proxy for SPI that is unrelated to firm fundamentals. This feature of stock price fragility motivates panel regression analysis relating *StkFrag* to debt concentration structure, of which results should be less confounded by potential omitted variable bias concerns.

I first validate stock price fragility as a negative proxy for SPI. To do so, I regress an SPI measure on *StkFrag*, a set of suggested firm-level control variables, and firm fixed effects and year fixed effects. Firm-level controls include *size, Tobins_Q, tangibility, leverage,* and *R&D,* following Bennett, Stulz, and Wang (2020). Table 2.3 presents the results. The results indicate that *StkFrag* is significantly (at less than 1% level) negatively related to both measures of SPI previously employed. Regression coefficients imply that a one standard deviation increase in *StkFrag* is associated with a decrease of 1.8% in *PIN* and 3.8% in *NONSYNC*, respectively, relative to the sample mean. Both the significance and the economic magnitude of the results justify the use of *StkFrag* as a valid negative proxy for SPI that has the virtue of being less correlated with firm fundamentals.

Table 2.4 presents the results from panel OLS regressions relating debt concentration structure to stock price fragility. Except that *StkFrag* enters as the main measure of SPI, other features of the regression specification stay the same as the baseline panel OLS specification. As *StkFrag* is a negative proxy for SPI, it should be negatively associated with the measures of debt heterogeneity to be consistent with the baseline results using *PIN* and *NONSYNC.* I find it is exactly the case. Columns (1) and (2) show that *StkFrag* is negatively associated with *Debt_Heterogeneity,* statistically significant at less than 1% level, regardless of the set of control variables included. Based on the results in column (2), a one standard deviation increase in *StkFrag* is associated with a 3.6% decrease in *Debt_Heterogeneity* relative to the sample mean. Columns (3) and (4) indicate that the negative relation between stock price fragility and the degree of debt heterogeneity is robust to the alternative measure, *Num_Debt_Types*, statistically significant at less than 5% level. In columns (5) and (6), I replace debt heterogeneity measures with *leverage* to estimate the impact of stock price fragility on the amount of debt itself, aside from the debt concentration structure. Interestingly, the coefficients for *StkFrag* are not statistically significant at any conventional level, indicating no meaningful association between stock price fragility and firm leverage ratio. These results mirror the findings by Friberg, Goldstein, and Hankins (2024), who also report zero book leverage response by firms to changes in stock price fragility. The absence of *leverage* response to fragility that I find lends credence to the argument that I am capturing firms' debt "structure" adjustments in response to the changes in SPI; that is, capturing the effects other than the mechanical correlation between firm debt usage and debt heterogeneity.

In sum, the results from the analyses using stock price fragility as a less-confounded measure of SPI with respect to firm fundamentals corroborate the baseline findings of the positive relation between stock price informativeness and debt heterogeneity. This, in turn, provides further support to the distress costs hypothesis.

2.3.3.2 Quasi-natural Experiment: BlackRock-BGI Merger

To further address endogeneity concerns and buttress the causal interpretation of findings, I implement difference-in-differences (DiD) analysis in a quasi-natural experiment setting that exploits the BlackRock-Barclays Global Investors (BGI) merger in 2009 as an exogenous shock to stock price fragility. A merger between two large financial institutions that both run a family of numerous funds can significantly change stock price fragility of affected firms. First, it increases the degree of ownership concentration of portfolio companies due to the consolidation of some individual funds that had been run by separate fund families during the pre-merger period. In addition, now that individual mutual funds are managed and marketed by one fund investment company as a result of the merger, the expected correlation of investor flows between the individual funds increases, ultimately affecting portfolio companies' exposure to correlated liquidity-driven trading by owners. Therefore, given that stock price fragility is a positive function of the degree of ownership concentration and exposure to correlated liquidity-driven trading, I predict that a merger event will increase the stock price fragility of the firms heavily affected by the merger.

The BlackRock-BGI merger has several desirable features that make it a more preferred setting for identifying an exogenous variation in stock price fragility. First, as emphasized by Massa et al. (2021), the merger is of unprecedented scale, combining the two prominent asset management giants and ultimately affecting a large number of stocks in varying degrees. Second, the main drivers behind the merger were strategic motives of expanding into ETF segments by BlackRock, unrelated to the fundamentals of any of the specific firms held by the two entities pre-merger. Last but not least, the merger affects only the ownership concentration of the affected firms, leaving the level of institutional ownership itself unchanged (Friberg, Goldstein, and Hankins, 2024). This feature is particularly advantageous in the context of my study because it enables me to get around concerns regarding potential confounding effects from the changes in monitoring and governance following a change in institutional ownership level.

The difference-in-differences specification for the quasi-natural experiment exploiting the BlackRock-BGI merger event takes the following form:

$$y_{i,t} = \alpha + \beta \times merger\_treat_{i,t} + \gamma X_{i,t} + Firm\ FE + Ind \times Yr\ FE + \varepsilon_{i,t}$$

where $X_{i,t}$ is a vector of firm-level controls including the same set of variables as in the baseline specification. As the BlackRock-BGI merger was announced in June 2009 (i.e., 2009Q2), treatment status is assigned based on the fund family holdings information as of 2009Q1, a quarter before the merger announcement date. In a similar spirit to Friberg, Goldstein, and Hankins (2024), treated firms are those jointly held by BGI and BlackRock in 2009Q1, while control firms are those held by only one of BGI and BlackRock in 2009Q1. I define the post-merger period as the dates after September 2009, when the merger cleared the antitrust approval by the European Commission. Hence, $merger\_treat_{i,t}$ equals one for treated firms for fiscal years that end after 2009Q3 (i.e., September 2009, inclusive) and zero otherwise. The sample for this analysis spans from fiscal years 2006 to 2011. Standard errors are clustered at the firm level.

Table 2.5 reports the difference-in-differences estimation results. First, I confirm whether the BlackRock-BGI merger significantly increases the stock price fragility of treated firms relative to control firms. To do so, following Friberg, Goldstein, and Hankins (2024), I calculate stock price fragility at the level of family of funds, take the square root of it, and adopt it as the dependent variable in regressions. Because the merger happens first at the fund family level, I expect that immediate response from portfolio firms can be better captured with stock price fragility calculated at the level of family of funds compared to fragility calculated at the individual fund level (i.e., the original *StkFrag*). After all, fragility calculated at the individual fund level is expected to shift more gradually over time post-merger, as consolidation between and operational adjustments on individual funds made by the newly merged entity are likely to take time until fully achieved.

The results in column (1) confirm that, as expected, treated firms experience an increase in *Family_StkFrag* relative to control firms post-merger, statistically significant at less than 1% level. The magnitude of the main coefficient of interest implies that, post-merger, treated firms experience a significant increase in stock price fragility relative to control firms which corresponds to 10.3% of the pre-merger sample mean. Figure 2.2.a displays the dynamics of the coefficient estimates, with the fiscal year 2008 (i.e., $t = -1$) as the reference year. While I find no evidence of pre-trends in both statistical and economic terms, *Family_StkFrag* exhibits a clearly increasing pattern right after the merger event year. The timing and sign of the family level stock price fragility response support the validity of the setting.

Now, I turn to differential responses in debt concentration structure. Given that treated firms experience a substantial increase in stock price fragility, they should adjust toward a more concentrated, or less heterogeneous, debt structure to be consistent with the prior results that point to the positive impact of SPI on debt heterogeneity in support of the distress costs hypothesis. Indeed, I find it exactly the case. Column (2) shows the results when debt concentration structure is measured with *Debt_Heterogeneity*. The coefficient on *merger_treat* is negative and statistically significant at less than 5%, indicating that treated firms decrease the degree of debt heterogeneity relative to control firms after experiencing deterioration in stock price informativeness precipitated by the increased stock price fragility. Given that the pre-merger sample mean of *Debt_ Heterogeneity* is 0.30, a decline of 0.034 represents about an 11.3% relative decrease for treated firms, which is economically large. Figure 2.2.b displays the dynamics of the coefficient estimates, with the fiscal year 2008 (i.e., $t = -1$) as the reference year. I observe that the coefficients turn significantly negative in the year of the shock and remain persistently so over the three years of the post-merger period, all statistically significant at less than 5% level. Again, I find no significant evidence of pre-trends.

As shown in column (3), qualitatively similar results hold with *Num_Debt_Types,* indicating robustness to alternative ways to define debt concentration structure. Moreover, consistent with the panel evidence, I find no evidence of differential book leverage response between treated and control firms, as reported in column (4). The absence of book leverage response again strengthens the argument that I am capturing firms' debt concentration structure adjustment distinguished from debt capacity or usage in response to the exogenous changes in stock price informativeness.

Summing up, the results presented in Table 2.5 and Figure 2.2 provide convincing evidence that the exogenous deterioration in stock price informativeness leads firms to adopt a substantially more concentrated debt structure even when the amount of debt employed does not meaningfully change. In this regard, the results from the quasi-natural experiment exploiting the BlackRock-BGI merger help support the causal interpretation of the results in this study, corroborating credence to the distress costs hypothesis.

2.3.3.3 BlackRock-BGI Merger: Robustness

To further assess the validity of the results from the BlackRock-BGI merger analysis, I implement two sets of robustness tests: matched DiD tests and a placebo (falsification) test.

2.3.3.3.1 Propensity Score Matching (PSM) Analysis

To alleviate the concern that treated and control firms are significantly different from each other and such differences in firm characteristics drive the overall quasi-natural experiment results, I conduct a matched difference-in-differences analysis using propensity score matching (PSM). Specifically, I match treated firms in the original BlackRock-BGI merger analysis sample with control firms from the same sample based on pre-merger *size, Tobins_Q, leverage, and StkFrag*. I emphasize that the choice of the set of firm characteristics on which the two groups are matched is not arbitrary. *Size* and *Tobins_Q* should be significantly related to individual mutual fund's or fund company's holding decisions, as they often market investment products with specific styles that are defined along the dimensions of firm size (e.g., small-, mid-, and large-cap funds) or valuation multiples (e.g., value versus growth). *leverage* accounts for both firm debt usage and riskiness of the firm. Lastly, I include *StkFrag* as one of the matching variables to further alleviate concerns regarding pre-trends. After calculating propensity scores using these four variables, I implement 1-to-N matching with replacement based on the propensity scores.

Table B.2 shows post-matching differences in firm characteristics. Overall, the matching procedure properly addresses the differences in observable firm characteristics between the two groups of firms across most dimensions except for two characteristics, *dividend_payer* (statistically significant at less than 10% level) and *size* (statistically significant at less than 1% level). One might raise concerns regarding the statistical difference in terms of *size*. However, a detailed look at summary statistics for the matched sample reveals that the significant difference observed for *size* seems largely due to the innate high positive skewness of the variable that is not completely mitigated with the log transformation. In dollar terms, the mean difference in *size* corresponds to less than a half standard deviation (i.e., 0.49 of a standard deviation). Therefore, I argue that it does not pose a significant threat to the validity of the matching results.

Table 2.6 reports the matched difference-in-differences results. Except that only the matched treated and control firms are included in the sample, all the other specification choices remain the same as the original DiD regression specification. Columns (1) to (4) reveal that the matched DiD results are quantitatively and qualitatively similar to the original quasi-natural experiment results. Specifically, treated firms experience a statistically significant (at less than 5% level) increase in *Family_StkFrag* relative to control firms, corresponding to 9.3% of the pre-merger sample mean. In response, treated firms decrease *Debt_Heterogeneity* more than control firms by 16.6% of the pre-merger sample mean. Qualitatively similar results hold when *Num_Debt_Types* is the dependent variable, and the statistical significance and magnitude of the results are larger than those reported from the original analysis. As shown in column (4), I do not find any significant book leverage response, again in line with the panel and original DiD evidence.

Overall, the t-test and matching results suggest that the differences in firm observables are less likely to drive the quasi-natural experiment results.

2.3.3.3.2 Placebo Test

I also conduct a placebo test to further fortify credence to the findings from the quasi-natural experiment. Specifically, I create a pseudo-sample in which I assume that BGI merges with Bank of America (BoA) instead of BlackRock, and then define treated firms as those jointly held by BGI and BoA in 2009Q1 and control firms as those held by only one of BGI and BoA in 2009Q1. The rationale behind this falsification test is that there should be no meaningful fragility response by treated firms defined under the hypothetical merger event that did not actually exist, and with the absence of a meaningful fragility response, one should not observe a meaningful differential response in debt heterogeneity between the two groups of firms. All the other features of the analysis, other than assuming the BGI-BoA merger instead of the BGI-BlackRock merger, stay the same as the original analysis.

Table 2.7 reports the results of the placebo test. I find that the estimated treatment effect is not statistically significant in any of the four regressions; the results in columns (1) to (4) show that there are no differences in the changes in family-level fragility or debt heterogeneity between treated and control firms defined under the pseudo-merger event around 2009Q3. The absence of meaningful evidence from the pseudo-merger event highlights the uniqueness of the results found in the BlackRock-BGI merger event that actually happened.

2.3.3.4 IV Approach: Nominal Stock Price Level

Lastly, I use an instrumental variable (IV) approach to provide further evidence that the positive effect of stock price informativeness on debt heterogeneity is likely causal. Specifically, I instrument SPI measures (*PIN* and *NONSYNC*) with the natural log of the fiscal year-beginning nominal stock price level (*ln_price*). The choice of instrument is motivated by the findings of Chan et al. (2017), who document that high stock price levels impede informed trading on stocks and thus reduce the informativeness of stock prices. The rationale behind is that some degree of uninformed trading is needed to facilitate informed trading, and high nominal stock prices can create barriers for uninformed investors to enter the market by imposing budget constraints or limiting risk-sharing capacity.

I implement IV-2SLS estimations in both the *NONSYNC sample* and the *PIN sample*. To minimize the effect from firms with abnormally low or high nominal stock price levels (e.g., penny stocks or Chipotle Mexican Grill stock with $1,000+ nominal share price), each sample is truncated at the 5th and 95th percentile based on the fiscal year-beginning stock price level. The full list of eight firm-level control variables is included in all regressions, in addition to firm and industry-by-year fixed effects. Standard errors are clustered at the firm level.

Table 2.8 presents the IV-2SLS estimation results. The results in columns (1) and (2) are from the *NONSYNC sample*. Column (1) presents the results from the first-stage regression for instrumenting *NONSYNC* with *ln_price*. Consistent with Chan et al. (2017), the coefficient on *ln_price* is negative and statistically significant at less than 1% level, indicating that a higher year-beginning nominal share price is associated with a substantially lower stock price informativeness as measured by *NONSYNC*. The magnitude, statistical significance, and F-statistics of the first-stage results alleviate the weak instrument concern, thereby validating the relevance condition of the nominal stock price level as an instrument. Column (2) reports the second-stage regression results that relate *Debt_Heterogeneity* to the instrumented price nonsynchronicity (*IV_NONSYNC*) based on the first-stage regression estimates. I find the instrumented SPI measure (*IV_NONSYNC*) positively predicts debt heterogeneity, highly significant at less than 1% level.

I find qualitatively identical patterns of the IV-2SLS estimates in the *PIN sample*. Column (3) presents the results from the first-stage regression for instrumenting *PIN* with *ln_price.* The coefficient on *ln_price* is negative and statistically significant at less than 1% level, satisfying the relevance condition. Column (4) reports the second-stage results. The coefficient on *IV_PIN* is positive and statistically significant at less than 5% level.

Summing up, the IV-2SLS results presented in Table 2.8 provide causal evidence that the increase in stock price informativeness leads firms to shift from a concentrated to a more heterogenous debt structure. With respect to the exclusion restriction, which is also an important condition that should be satisfied to be able to make a causal claim as above, I am not particularly aware of any theories or empirical evidence relating the nominal stock price level to corporate debt concentration structure through alternative channels as convincing as the SPI channel.

Taken collectively, the results from panel regressions using stock price fragility, the DiD results exploiting the BlackRock-BGI merger as a quasi-natural experiment, and the results of the IV-2SLS analysis provide broad and coherent support for the causal effects consistent with the distress costs hypothesis by alleviating concerns of potential endogeneity issues.

2.4 Cross-sectional Tests

The previous section establishes the causal effect of stock price informativeness on debt heterogeneity in line with the distress costs hypothesis. In this section, I conduct subsample analyses exploiting the cross-sectional nature of the sample to provide more concrete evidence on the channels through which SPI affects corporate debt concentration structure. I explore two specific channels that can bolster support for the validity of the proposition that the previous results are indeed in line with the predictions from the distress costs hypothesis: the expected distress cost channel and the information asymmetry channel. Given longer time series, I provide cross-sectional evidence with the *NONSYNC sample* and the *fragility sample*. The main dependent variable of interest is *Debt_Heterogeneity* throughout the analysis.

2.4.1 The Expected Costs of Financial Distress Channel

A natural prediction from the distress costs hypothesis is that the effect of stock price informativeness on debt concentration structure choice should be more pronounced for firms with higher expected costs of financial distress. Intuitively, if high SPI lowers financial distress costs which in turn leads to a more heterogeneous debt structure based on the trade-off framework suggested by Bolton and Scharfstein (1996), I expect more significant effects of SPI for firms facing higher expected distress costs since the marginal decline in such costs should be higher in response to the improved SPI.

To confirm whether the above prediction holds, I split firms on variables related to firms' default risk or expected loss conditioning on default, the two elements that mainly conceptualize expected financial distress costs. To proxy for default risk, I use default probability dictated by Merton Distance-to-Default (DD) model as delineated in Bharath and Shumway (2008) and cash flow volatility (*cf_vol*). Firms with higher predicted default probability and cash flow volatility should have higher distress costs. To proxy for expected loss conditioning on default, I use asset redeployability (Kim and Kung, 2017). Firms with low asset redeployability are likely to have lower liquidation value and thus have higher distress costs. Throughout the cross-sectional analysis, the *High* type firms belong to the top 40% with respect to a specific variable in a given fiscal year and the *Low* type firms the bottom 40%, unless otherwise stated.

Panel A of Table 2.9 provides cross-sectional evidence on the impact of expected costs of financial distress on the relation between debt heterogeneity and stock price informativeness when the SPI measure is *NONSYNC*. Column (1) shows that the coefficient on *NONSYNC* in the *High Default Prob* subsample is more than five times the size of that for the *Low Default Prob* subsample, with the former statistically significant at less than 1% level while the latter is insignificant. Similar patterns emerge in columns (2) and (3), in which only the coefficients in the *High CF vol* subsample and the *Low Redeploy* subsample are significant at less than 1% level. I also report the results from the permutation test to further verify that the difference in the coefficients from the high and low samples is statistically significant. The corresponding results indicate that the differences in the effect of *NONSYNC* across the probability of default, cash flow volatility, and asset redeployability subsamples are all statistically significant at least at the 5% level.

Panel B of Table 2.9 reports the results when the SPI measure is *StkFrag*. The pattern of the results is qualitatively identical to the *NONSYNC* results. The results in columns (1), (2), and (3) altogether provide evidence that the negative effect of stock price fragility on debt heterogeneity is more pronounced for firms that have higher expected distress costs. The results from the permutation test further reveal that the differences across the subsamples are statistically significant at least at the 5% level.

Overall, the cross-sectional evidence confirms the prediction that the effect of SPI on debt heterogeneity should be more pronounced for firms with higher expected distress costs. This, in turn, buttresses the interpretation that SPI induces firms to choose a more heterogenous debt structure by indeed reducing distress costs, one of the main mechanisms suggested by the distress costs hypothesis.

2.4.2 The Information Asymmetry Channel

The prediction from the distress costs hypothesis is also built upon another potential mechanism through which SPI affects corporate debt concentration structure. The main implication of this channel is that the effect of stock price informativeness on debt heterogeneity should be more pronounced for firms with a higher degree of information asymmetry.

To explore the information asymmetry channel, I split firms on variables related to their proclivity to suffer from information asymmetry, including *size*, firm-level exposure to uncertainty, and bank debt usage. Small firms are likely to suffer more from issues related to information asymmetry. When it comes to exposure to uncertainty, an increase in uncertainty can increase investor uncertainty about a firm's future cash flow, aggravating the firm's information asymmetry problems. Therefore, the costs of information asymmetry should also be higher for firms exposed to a high degree of uncertainty. To partition firms according to their exposure to uncertainty, I first obtain firm-level uncertainty instruments by Alfaro et al. (2022) from Economic Policy Uncertainty website[2]. Then, I create a firm-level uncertainty exposure score in a given fiscal year based on the sum of quintile ranks of two firm-level uncertainty variables, the option-implied volatility shock and exposure to the economic policy uncertainty (EPU). The sample is split on the

[2] https://www.policyuncertainty.com/firm_uncertainty.html

calculated uncertainty exposure score, with the top 40% firms belonging to the *High Uncertainty* subsample and the bottom 40% to the *Low Uncertainty* subsample. Lastly, I focus only on the unrated sample of firms, and split the unrated sample based on bank debt usage. Due to the superior information processing and monitoring roles performed by banks, firms with a high degree of information asymmetry are likely to rely more on bank financing.

Panel A of Table 2.10 provides cross-sectional evidence on the information channel of the effect of stock price informativeness on debt concentration structure when the SPI measure is *NONSYNC*. Column (1) shows that the coefficient on *NONSYNC* in the *Small Size* subsample is more than two times the size of that for the *Large Size* subsample, with the former statistically significant at less than 5% level while the latter is insignificant. Similar patterns emerge in columns (2) and (3), in which only the coefficients in the *High Uncertainty* subsample and the *High Bankdebt* subsample are significant at less than 1% level. Permutation tests reveal that the differences in the effect of *NONSYNC* across the size, firm-level uncertainty exposure, and bank debt usage subsamples are all statistically significant at least at the 5% level.

Panel B of Table 2.10 reports the results when the SPI measure is *StkFrag*. Again, the pattern of the results is qualitatively identical to the *NONSYNC* results. Column (1) shows that the coefficient on *StkFrag* in the *Small Size* subsample is statistically significant at the 5% level while that in the *Large Size* subsample is insignificant. Similarly, as shown in columns (2) and (3), only the coefficients in the *High Uncertainty* subsample and the *High Bankdebt* subsample are significant at less than 1% level. The results from the permutation test further reveal that the differences across the subsamples are statistically significant at less than 1% level for uncertainty and bank debt usage and significant at less than 10% level for firm size.

In sum, the above cross-sectional evidence strongly supports the notion that mitigating the information risk and alleviating overall information asymmetry indeed underlies the positive relationship between SPI and debt heterogeneity.

2.5 Conclusion

Motivated by the recent heightened attention on the implications of changing informativeness of price signals precipitated by the prevalence of passive investing, I ask whether and how the informativeness of stock prices affects debt heterogeneity, defined in the context of my study as the degree to which firms allocate their debt into multiple debt types. I generate competing predictions on how stock price informativeness should affect the level of debt heterogeneity based on the theoretical framework by Bolton and Scharfstein (1996). While the reduced expected financial distress costs hypothesis predicts a positive impact of SPI on debt heterogeneity, the substitution of governance mechanisms hypothesis predicts the opposite, rendering the direction of the effect purely an empirical question.

Throughout this study, I find strong and consistent evidence that SPI increases the degree of debt heterogeneity adopted by firms, consistent with the distress cost channel. The results are robust to alternative ways of measuring debt heterogeneity and stock price informativeness. I address potential endogeneity concerns with panel regressions using stock price fragility, difference-in-differences analysis exploiting a quasi-natural experiment, and an IV-2SLS analysis. The results from these analyses altogether indicate that typical endogeneity issues such as omitted variable bias or reverse causality do not seem to nullify my findings, providing broad and coherent support for the causal effects consistent with the distress costs hypothesis. Cross-sectional evidence further reveals that the reduction in distress costs and alleviation of information risk and asymmetry are genuinely behind the observed positive relation between SPI and debt heterogeneity. Overall, this study contributes to the literature by expanding the list of debt concentration structure determinants, of which our understanding is incomplete and causal evidence is not plenty.

Table 2.1: Summary statistics

This table reports summary statistics of the selected sample for the measures of debt concentration structure, stock price informativeness, and firm-level control variables. Financial firms (SIC codes from 6000 through 6999), regulated utilities (SIC codes from 4900 through 4949), and non-operating establishments (SIC codes from 9000 through 9999) are excluded from the sample. When the measurement of stock price informativeness is either based on stock price nonsynchronicity (*NONSYNC*) or stock price fragility (*StkFrag*), the sample spans fiscal years 2003 to 2018. When stock price informativeness is measured based on the probability of informed trading (*PIN*), the sample spans fiscal years 2003 to 2010. The presented summary statistics for debt structure and firm-level control variables are based on the sample when the SPI is measured with *NONSYNC*. All stock price informativeness and continuous firm-level control variables except *size* and *leverage* are winsorized at the 1st and 99th percentile. Detailed definitions and calculations of the variables are presented in Table B.1 of Appendix B.

Variable	**N**	**mean**	**sd**	**min**	**p10**	**p50**	**p90**	**max**
Debt Structure								
Debt_Heterogeneity	26971	0.264	0.261	0.000	0.000	0.188	0.621	0.929
Num_Debt_Types	26971	1.730	0.789	1	1	2	3	5
Stock Price Informativeness (SPI)								
NONSYNC	26971	1.129	1.066	-1.287	-0.140	0.961	2.725	4.052
PIN	13297	0.157	0.088	0.000	0.061	0.135	0.285	0.585
StkFrag	25846	0.129	0.106	0.000	0.012	0.108	0.277	0.604
Firm-level Controls								
size	26971	6.607	1.981	2.307	3.911	6.636	9.135	13.184
Tobins_Q	26971	1.634	0.761	0.470	0.925	1.416	2.651	5.015
tangibility	26971	0.270	0.238	0.001	0.036	0.189	0.667	0.918
profitability	26971	0.073	0.189	-1.218	-0.090	0.112	0.216	0.404
cf_vol	26971	0.017	0.021	0.002	0.004	0.010	0.037	0.201
R&D	26971	0.047	0.107	0.000	0.000	0.000	0.135	0.825
dividend_payer	26971	0.367	0.482	0	0	0	1	1
leverage	26971	0.262	0.198	0.000	0.024	0.234	0.533	1.000

Table 2.2: SPI and debt heterogeneity – Baseline panel OLS results

This table presents baseline results from the panel OLS regression of debt heterogeneity on stock price informativeness (SPI) and firm-level control variables. All regression specifications include firm and industry-by-year fixed effects, where the industry is defined under Fama and French 48-industry classification. In columns (1) to (4), firm SPI is measured with *PIN*, so the sample spans from fiscal years 2003 to 2010; In columns (5) to (8), firm SPI is measured with *NONSYNC*, so the sample spans from fiscal years 2003 to 2018. All SPI measures and continuous firm-level control variables (except *leverage*) that enter regressions are winsorized at the 1st and 99th percentile. t-statistics based on standard errors clustered at firm level are reported in parentheses below the coefficient estimates. *, **, *** represent statistical significance at the 10%, 5%, and 1% level, respectively. Detailed definitions and calculations of the variables are presented in Table B.1 of Appendix B.

VARIABLES	(1) Debt_ Heterogeneity	(2) Debt_ Heterogeneity	(3) Num_Debt_ Types	(4) Num_Debt_ Types	(5) Debt_ Heterogeneity	(6) Debt_ Heterogeneity	(7) Num_Debt_ Types	(8) Num_Debt_ Types
PIN	0.151***	0.145***	0.589***	0.575***				
	(2.93)	(2.86)	(3.81)	(3.78)				
NONSYNC					0.014***	0.013***	0.045***	0.043***
					(4.47)	(4.19)	(4.75)	(4.52)
size	0.058***	0.065***	0.156***	0.181***	0.059***	0.060***	0.164***	0.173***
	(6.93)	(6.69)	(5.89)	(5.98)	(10.51)	(9.64)	(9.82)	(9.34)
leverage	0.309***	0.290***	0.832***	0.769***	0.307***	0.295***	0.795***	0.757***
	(10.43)	(9.69)	(9.88)	(9.19)	(15.78)	(14.97)	(14.56)	(13.70)
Tobins_Q		-0.005		-0.011		-0.010**		-0.027**
		(-0.82)		(-0.66)		(-2.27)		(-2.26)
tangibility		0.279***		1.020***		0.174***		0.653***
		(5.77)		(6.73)		(4.69)		(5.79)
profitability		-0.066**		-0.179**		-0.037*		-0.078
		(-2.44)		(-2.25)		(-1.80)		(-1.37)
cf_vol		-0.160		-0.577		-0.046		0.187
		(-0.83)		(-1.03)		(-0.34)		(0.48)
R&D		-0.027		-0.064		-0.012		-0.017
		(-0.44)		(-0.37)		(-0.29)		(-0.13)
dividend_payer		-0.011		-0.046		-0.011		-0.037
		(-0.92)		(-1.22)		(-1.20)		(-1.36)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ind×Yr FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	12,757	12,757	12,757	12,757	26,326	26,326	26,326	26,326
Adj. R-squared	0.561	0.564	0.536	0.540	0.502	0.504	0.477	0.480

Table 2.3: Stock price fragility and other SPI measures – Validating fragility

This table reports the results from panel OLS regressions for validating stock price fragility as a negative proxy for stock price informativeness (SPI). In a similar spirit to Bennett, Stulz, and Wang (2020), an SPI measure is regressed on the square root of estimated firm-level stock price fragility (*StkFrag*), a set of suggested firm-level control variables, and firm fixed effects and year fixed effects. Firm-level controls include *size, Tobins_Q, tangibility, leverage,* and *R&D*. *StkFrag* and firm-level control variables (except *leverage*) that enter regressions are winsorized at the 1st and 99th percentile. In column (1), the dependent variable is *PIN*, so the sample spans from fiscal years 2003 to 2010; In column (2), the dependent variable is *NONSYNC*, so the sample spans from fiscal years 2003 to 2018. t-statistics based on standard errors clustered at firm level are reported in parentheses below the coefficient estimates. *, **, *** represent statistical significance at the 10%, 5%, and 1% level, respectively. Detailed definitions and calculations of the variables are presented in Table B.1 of Appendix B.

VARIABLES	(1) PIN	(2) NONSYNC
StkFrag	-0.026***	-0.392***
	(-3.56)	(-5.57)
size	-0.042***	-0.347***
	(-19.34)	(-21.32)
Tobins_Q	-0.019***	-0.179***
	(-14.08)	(-15.51)
tangibility	0.002	-0.031
	(0.17)	(-0.38)
leverage	0.028***	0.336***
	(4.71)	(7.43)
R&D	0.011	-0.137
	(0.72)	(-0.95)
Firm FE	Yes	Yes
Year FE	Yes	Yes
Observations	11,895	25,110
Adj. R-squared	0.792	0.777

Table 2.4: Stock price fragility and debt heterogeneity – Panel OLS results

This table presents the results from panel OLS regression of debt variables on stock price fragility (*StkFrag*) as a negative proxy for stock price informativeness as well as additional firm-level control variables. All regression specifications include firm and industry-by-year fixed effects, where the industry is defined under Fama and French 48-industry classification. The sample spans from fiscal years 2003 to 2018, and SPI measure and continuous firm-level control variables (except *leverage*) that enter regressions are winsorized at the 1st and 99th percentile. Columns (1) and (2) report the results when firm debt concentration structure is measured with *Debt_Heterogeneity*. Columns (3) and (4) show the results when firm debt concentration structure is measured with *Num_Debt_Types*. In columns (5) and (6), *leverage* is regressed on stock price fragility and firm-level control variables to show the impact of stock price fragility on the amount of debt itself, aside from debt structure. t-statistics based on standard errors clustered at firm level are reported in parentheses below the coefficient estimates. *, **, *** represent statistical significance at the 10%, 5%, and 1% level, respectively. Detailed definitions and calculations of the variables are presented in Table B.1 of Appendix B.

VARIABLES	(1) Debt_ Heterogeneity	(2) Debt_ Heterogeneity	(3) Num_Debt_ Types	(4) Num_Debt_ Types	(5) leverage	(6) leverage
StkFrag	-0.083***	-0.088***	-0.189**	-0.206**	-0.005	-0.015
	(-3.04)	(-3.23)	(-2.27)	(-2.47)	(-0.30)	(-0.83)
size	0.053***	0.055***	0.141***	0.152***	0.028***	0.038***
	(9.04)	(8.40)	(8.28)	(8.00)	(5.90)	(7.60)
leverage	0.315***	0.302***	0.815***	0.774***		
	(15.46)	(14.66)	(14.29)	(13.46)		
Tobins_Q		-0.010**		-0.028**		-0.006
		(-2.33)		(-2.35)		(-1.60)
tangibility		0.172***		0.629***		0.086***
		(4.42)		(5.31)		(2.93)
profitability		-0.044**		-0.101*		-0.172***
		(-2.04)		(-1.69)		(-8.77)
cf_vol		0.012		0.410		0.516***
		(0.08)		(0.99)		(4.23)
R&D		-0.021		-0.055		-0.042
		(-0.44)		(-0.39)		(-0.82)
dividend_payer		-0.012		-0.041		-0.023***
		(-1.25)		(-1.45)		(-4.06)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Ind×Yr FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	25,232	25,232	25,232	25,232	25,232	25,232
Adj. R-squared	0.500	0.502	0.475	0.478	0.708	0.717

Table 2.5: Quasi-natural experiment results – BlackRock-BGI merger

This table reports difference-in-differences (DiD) estimation results of the quasi-natural experiment exploiting the BlackRock-BGI merger event as an exogenous shock to the stock price fragility. The sample spans from fiscal years 2006 to 2011. Treatment status is assigned based on the fund family holdings information as of 2009Q1, a quarter before the announcement of the merger event in June 2009 (i.e., 2009Q2). Treated firms are those jointly held by BGI and BlackRock in 2009Q1, while control firms are those held by only one of BGI and BlackRock in 2009Q1. *merger_treat* equals one for treated firms for fiscal years that end after 2009Q3 (i.e., September 2009, inclusive) and zero otherwise. In column (1), *Family_StkFrag*, the square root of stock price fragility calculated at the level of family of funds, is adopted as the dependent variable, in a similar spirit to Friberg, Goldstein, and Hankins (2024). All regression specifications include firm and industry-by-year fixed effects, where the industry is defined under Fama and French 48-industry classification. Continuous firm-level control variables (except *leverage*) are winsorized at the 1st and 99th percentile. t-statistics based on standard errors clustered at firm level are reported in parentheses below the coefficient estimates. *, **, *** represent statistical significance at the 10%, 5%, and 1% level, respectively. Detailed definitions and calculations of the variables are presented in Table B.1 of Appendix B.

	(1)	(2)	(3)	(4)
VARIABLES	Family_StkFrag	Debt_Heterogeneity	Num_Debt_Types	leverage
merger_treat	0.010***	-0.034**	-0.090**	0.008
	(3.78)	(-2.40)	(-1.98)	(1.22)
size	0.011***	0.075***	0.210***	0.061***
	(3.09)	(3.93)	(3.35)	(4.93)
Tobins_Q	-0.005**	-0.001	-0.024	-0.012*
	(-2.06)	(-0.07)	(-0.65)	(-1.80)
tangibility	0.019	0.383***	1.261***	0.074
	(1.13)	(4.25)	(4.36)	(1.31)
profitability	-0.013	-0.083	-0.180	-0.291***
	(-1.02)	(-1.13)	(-0.81)	(-7.13)
cf_vol	0.037	-0.292	-1.598	0.192
	(0.43)	(-0.66)	(-1.08)	(0.75)
R&D	0.009	-0.102	0.240	0.086
	(0.19)	(-0.42)	(0.30)	(0.37)
dividend_payer	-0.001	-0.028	-0.073	-0.007
	(-0.32)	(-1.42)	(-1.11)	(-0.99)
leverage	0.002	0.382***	0.965***	
	(0.20)	(6.72)	(5.89)	
Firm FE	Yes	Yes	Yes	Yes
Ind×Yr FE	Yes	Yes	Yes	Yes
Observations	4,476	4,476	4,476	4,476
Adj. R-squared	0.670	0.581	0.533	0.826

Table 2.6: PSM analysis – BlackRock-BGI merger matched DiD analysis

This table reports the results of the DiD estimation exploiting BlackRock-BGI merger in a matched sample. In this analysis, treated firms in the original BlackRock-BGI merger analysis sample are matched with control firms that also come from the original sample. The chosen matching methodology is 1-to-N matching with replacement based on the calculated propensity scores. The sample spans from fiscal years 2006 to 2011. merger_treat equals one for the matched treated firms for fiscal years that end after 2009Q3 (i.e., September 2009, inclusive) and zero otherwise. In column (1), *Family_StkFrag*, the square root of stock price fragility calculated at the level of family of funds, is adopted as the dependent variable, in a similar spirit to Friberg, Goldstein, and Hankins (2024). All regression specifications include firm and industry-by-year fixed effects, where the industry is defined under Fama and French 48-industry classification. Continuous firm-level control variables (except *leverage*) are winsorized at the 1st and 99th percentile. t-statistics based on standard errors clustered at firm level are reported in parentheses below the coefficient estimates. *, **, *** represent statistical significance at the 10%, 5%, and 1% level, respectively. Detailed definitions and calculations of the variables are presented in Table B.1 of Appendix B.

	(1)	(2)	(3)	(4)
VARIABLES	Family_StkFrag	Debt_Heterogeneity	Num_Debt_Types	leverage
merger_treat	0.009**	-0.048**	-0.152**	-0.012
	(1.97)	(-2.26)	(-2.20)	(-1.18)
size	0.004	0.061**	0.189*	0.058***
	(0.68)	(2.12)	(1.96)	(2.64)
Tobins_Q	-0.007**	0.022	0.074	-0.034***
	(-2.00)	(1.26)	(1.30)	(-3.12)
tangibility	0.027	0.183	0.835*	0.058
	(0.95)	(1.31)	(1.88)	(0.61)
profitability	-0.040*	-0.054	-0.265	-0.255***
	(-1.69)	(-0.57)	(-0.93)	(-3.77)
cf_vol	0.308	0.084	-0.876	0.365
	(1.59)	(0.16)	(-0.55)	(1.24)
R&D	-0.074	0.045	1.032	-0.142
	(-1.19)	(0.15)	(0.97)	(-0.56)
dividend_payer	-0.006	0.013	0.014	0.001
	(-1.05)	(0.42)	(0.12)	(0.06)
leverage	0.001	0.267***	0.716***	
	(0.05)	(3.56)	(3.24)	
Firm FE	Yes	Yes	Yes	Yes
Ind×Yr FE	Yes	Yes	Yes	Yes
Observations	1,940	1,940	1,940	1,940
Adj. R-squared	0.589	0.611	0.538	0.799

Table 2.7: Placebo test – Assuming BGI-BoA merger

This table reports the results of the placebo DiD test that assumes BGI merges with BoA (Bank of America) instead of BlackRock. All the other features of the analysis, other than assuming BGI-BoA merger instead of BGI-BlackRock merger, stay the same as the original analysis. The sample spans from fiscal years 2006 to 2011. Treated firms are those jointly held by BGI and BoA in 2009Q1, while control firms are those held by only one of BGI and BoA in 2009Q1. *merger_treat* equals one for treated firms for fiscal years that end after 2009Q3 (i.e., September 2009, inclusive) and zero otherwise. In column (1), *Family_StkFrag*, the square root of stock price fragility calculated at the level of family of funds, is adopted as the dependent variable, in a similar spirit to Friberg, Goldstein, and Hankins (2024). All regression specifications include firm and industry-by-year fixed effects, where the industry is defined under Fama and French 48-industry classification. Continuous firm-level control variables (except *leverage*) are winsorized at the 1st and 99th percentile. t-statistics based on standard errors clustered at firm level are reported in parentheses below the coefficient estimates. *, **, *** represent statistical significance at the 10%, 5%, and 1% level, respectively. Detailed definitions and calculations of the variables are presented in Table B.1 of Appendix B.

VARIABLES	(1) Family_StkFrag	(2) Debt_Heterogeneity	(3) Num_Debt_Types	(4) leverage
merger_treat	0.002	0.004	0.012	-0.008
	(0.61)	(0.29)	(0.26)	(-1.28)
size	0.012***	0.073***	0.206***	0.062***
	(3.14)	(3.86)	(3.30)	(5.05)
Tobins_Q	-0.005**	-0.000	-0.023	-0.012*
	(-2.05)	(-0.02)	(-0.63)	(-1.80)
tangibility	0.015	0.397***	1.294***	0.073
	(0.89)	(4.38)	(4.48)	(1.31)
profitability	-0.014	-0.083	-0.178	-0.289***
	(-1.09)	(-1.14)	(-0.82)	(-7.16)
cf_vol	0.016	-0.221	-1.412	0.186
	(0.19)	(-0.51)	(-0.97)	(0.73)
R&D	0.004	-0.095	0.255	0.104
	(0.09)	(-0.42)	(0.35)	(0.48)
dividend_payer	-0.001	-0.031	-0.075	-0.007
	(-0.21)	(-1.57)	(-1.15)	(-0.95)
leverage	0.003	0.378***	0.955***	
	(0.33)	(6.61)	(5.79)	
Firm FE	Yes	Yes	Yes	Yes
Ind×Yr FE	Yes	Yes	Yes	Yes
Observations	4,491	4,491	4,491	4,491
Adj. R-squared	0.669	0.582	0.535	0.827

Table 2.8: Stock price level, SPI, and debt heterogeneity – IV-2SLS results

This table reports the results from IV-2SLS estimation that uses fiscal year-beginning nominal stock price level as an IV for stock price informativeness (SPI). When the measurement of SPI is based on price nonsynchronicity (*NONSYNC*), the sample spans from fiscal years 2003 to 2018; when based on the probability of informed trading (*PIN*), the sample spans from fiscal years 2003 to 2010. To minimize the effect from firms with abnormally low or high nominal stock price levels (e.g., penny stocks or Chipotle Mexican Grill stock with $1,000+ nominal share price), the sample is truncated at the 5th and 95th percentile based on the fiscal year-beginning stock price level. All regression specifications include firm and industry-by-year fixed effects, where the industry is defined under Fama and French 48-industry classification. Continuous firm-level control variables (except *leverage*) are winsorized at the 1st and 99th percentile. t-statistics based on standard errors clustered at firm level are reported in parentheses below the coefficient estimates. *, **, *** represent statistical significance at the 10%, 5%, and 1% level, respectively. Detailed definitions and calculations of the variables are presented in Table B.1 of Appendix B.

	(1)	(2)	(3)	(4)
VARIABLES	NONSYNC	Debt_ Heterogeneity	PIN	Debt_ Heterogeneity
IV_NONSYNC		0.073***		
		(2.85)		
IV_PIN				0.660**
				(2.44)
ln_price	-0.180***		-0.021***	
	(-15.30)		(-15.18)	
size	-0.244***	0.086***	-0.034***	0.091***
	(-14.03)	(7.71)	(-15.24)	(5.53)
Tobins_Q	-0.121***	0.004	-0.014***	0.005
	(-9.79)	(0.65)	(-10.25)	(0.70)
tangibility	-0.040	0.189***	-0.007	0.303***
	(-0.50)	(4.68)	(-0.74)	(5.83)
profitability	-0.116*	-0.025	-0.010	-0.035
	(-1.92)	(-1.10)	(-1.32)	(-1.16)
cf_vol	0.155	-0.080	-0.172***	-0.153
	(0.40)	(-0.51)	(-3.85)	(-0.71)
R&D	-0.373**	0.026	0.007	0.030
	(-2.40)	(0.51)	(0.40)	(0.39)
dividend_payer	-0.003	-0.009	0.001	-0.012
	(-0.18)	(-0.92)	(0.37)	(-0.95)
leverage	0.178***	0.281***	0.015**	0.261***
	(3.89)	(12.71)	(2.48)	(8.02)
Firm FE	Yes	Yes	Yes	Yes
Ind×Yr FE	Yes	Yes	Yes	Yes
Observations	23,474	23,474	11,253	11,253
Adj. R-squared	0.789	0.510	0.804	0.569

Table 2.9: Cross-sectional evidence – Expected costs of financial distress

This table presents the results of subsample analysis split on variables related to firms' default risk or expected loss conditioning on default for providing cross-sectional evidence on the impact of expected costs of financial distress on the relation between debt heterogeneity and stock price informativeness. In column (1), firms are split on default probability dictated by Merton Distance-to-Default (DD) model as delineated in Bharath and Shumway (2008). In column (2), firms are split on cash flow volatility (i.e., *cf_vol*), so *cf_vol* does not enter the regressions. In column (3), firms are split on asset redeployability (Kim and Kung, 2017). All regression specifications include firm and industry-by-year fixed effects, where the industry is defined under Fama and French 48-industry classification. The sample spans from fiscal years 2003 to 2018, and SPI measure and continuous firm-level control variables (except *leverage*) that enter regressions are winsorized at the 1st and 99th percentile. t-statistics based on standard errors clustered at firm level are reported in parentheses below the coefficient estimates. *, **, *** represent statistical significance at the 10%, 5%, and 1% level, respectively. Detailed definitions and calculations of the variables are presented in Table B.1 of Appendix B.

Panel A: NONSYNC results

	(1)		(2)		(3)	
	Debt_Heterogeneity		Debt_Heterogeneity		Debt_Heterogeneity	
VARIABLES	*High Default Prob*	*Low Default Prob*	*High CF vol*	*Low CF vol*	*Low Redeploy*	*High Redeploy*
NONSYNC	0.022***	0.004	0.019***	0.002	0.015***	0.006
	(3.46)	(0.83)	(4.31)	(0.38)	(3.18)	(0.84)
size	0.059***	0.057***	0.053***	0.074***	0.065***	0.055***
	(4.43)	(5.75)	(6.32)	(6.77)	(5.85)	(3.12)
Tobins_Q	-0.007	-0.002	-0.004	-0.031***	-0.009	-0.017
	(-0.61)	(-0.34)	(-0.87)	(-2.95)	(-1.39)	(-1.09)
tangibility	0.160**	0.156***	0.217***	0.159**	0.240***	0.270***
	(2.50)	(2.60)	(4.43)	(2.37)	(4.59)	(2.73)
profitability	0.024	-0.068*	-0.060***	0.003	-0.047*	-0.049
	(0.62)	(-1.95)	(-2.91)	(0.04)	(-1.82)	(-0.63)
cf_vol	-0.143	0.069			-0.132	-0.208
	(-0.54)	(0.29)			(-0.80)	(-0.39)
R&D	-0.108	0.017	-0.059	0.267	-0.026	-0.032
	(-1.12)	(0.23)	(-1.42)	(1.22)	(-0.49)	(-0.13)
dividend_payer	-0.009	-0.004	-0.011	-0.007	-0.015	-0.032
	(-0.48)	(-0.29)	(-0.80)	(-0.50)	(-0.89)	(-1.62)
leverage	0.218***	0.341***	0.263***	0.299***	0.277***	0.399***
	(5.79)	(9.41)	(9.96)	(8.21)	(9.21)	(6.78)
Diff(coef)	0.018***		0.017***		0.009**	
p-value	0.000		0.000		0.030	
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Ind×Yr FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,011	13,316	9,852	10,248	10,139	4,686
Adj. R-squared	0.600	0.482	0.506	0.539	0.527	0.529

Table 2.9 (continued)

Panel B: StkFrag results

	(1)		(2)		(3)	
	Debt_Heterogeneity		Debt_Heterogeneity		Debt_Heterogeneity	
	High	*Low*	*High*	*Low*	*Low*	*High*
VARIABLES	*Default Prob*	*Default Prob*	*CF vol*	*CF vol*	*Redeploy*	*Redeploy*
StkFrag	-0.115*	-0.038	-0.144***	-0.015	-0.140***	-0.016
	(-1.89)	(-1.04)	(-3.62)	(-0.34)	(-3.31)	(-0.26)
size	0.043***	0.060***	0.043***	0.075***	0.052***	0.059***
	(3.01)	(5.93)	(4.88)	(6.61)	(4.47)	(3.25)
Tobins_Q	-0.013	-0.002	-0.007	-0.030***	-0.012*	-0.014
	(-1.10)	(-0.33)	(-1.31)	(-2.75)	(-1.86)	(-0.88)
tangibility	0.173**	0.170***	0.223***	0.136**	0.250***	0.254**
	(2.52)	(2.78)	(4.40)	(1.98)	(4.59)	(2.42)
profitability	0.044	-0.077**	-0.060***	0.007	-0.058**	-0.042
	(1.12)	(-2.14)	(-2.76)	(0.08)	(-2.05)	(-0.53)
cf_vol	-0.130	0.101			-0.084	-0.116
	(-0.45)	(0.41)			(-0.48)	(-0.21)
R&D	-0.018	0.014	-0.064	0.284	-0.038	-0.010
	(-0.18)	(0.18)	(-1.37)	(1.29)	(-0.66)	(-0.04)
dividend_payer	-0.008	-0.001	-0.009	-0.004	-0.014	-0.037*
	(-0.44)	(-0.08)	(-0.64)	(-0.27)	(-0.84)	(-1.81)
leverage	0.229***	0.342***	0.271***	0.308***	0.281***	0.400***
	(5.76)	(9.17)	(9.91)	(8.08)	(8.95)	(6.41)
Diff(coef)	-0.077***		-0.129***		-0.123**	
p-value	0.000		0.000		0.020	
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Ind×Yr FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,648	12,958	9,442	9,819	9,633	4,472
Adj. R-squared	0.602	0.478	0.505	0.541	0.521	0.527

Table 2.10: Cross-sectional evidence – Information asymmetry

This table presents the results of subsample analysis split on variables related to firms' proclivity to suffer from information asymmetry for providing evidence on the information channel of the effect of stock price informativeness on debt concentration structure. In column (1), firms are split on the book value of assets, so the natural log of sales is used instead in regressions to control for firm size. In column (2), firms are split on their exposure to uncertainty. In column (3), the analysis uses unrated firms only, and the sample of unrated firms are split on their bank debt usage. For columns (1) and (2), the sample spans from fiscal years 2003 to 2018; for column (3), the sample spans from fiscal years 2003 to 2014 due to the coverage of the S&P credit rating data. All regression specifications include firm and industry-by-year fixed effects, where the industry is defined under Fama and French 48-industry classification. SPI measure and continuous firm-level control variables (except *leverage*) are winsorized at the 1st and 99th percentile. t-statistics based on standard errors clustered at firm level are reported in parentheses below the coefficient estimates. *, **, *** represent statistical significance at the 10%, 5%, and 1% level, respectively. Detailed definitions and calculations of the variables are presented in Table B.1 of Appendix B.

Panel A: NONSYNC results

	(1)		(2)		(3)	
	Debt_Heterogeneity		Debt_Heterogeneity		Debt_Heterogeneity	
VARIABLES	*Small Size*	*Large Size*	*High Uncertainty*	*Low Uncertainty*	*High Bankdebt*	*Low Bankdebt*
NONSYNC	0.010**	0.004	0.024***	0.005	0.021***	0.005
	(2.45)	(0.79)	(3.02)	(0.75)	(3.13)	(0.58)
size	0.021***	0.056***	0.066***	0.074***	0.019	0.060***
	(3.27)	(5.36)	(5.06)	(6.43)	(1.00)	(3.38)
Tobins_Q	-0.011**	-0.034***	-0.011	-0.007	-0.004	0.009
	(-2.11)	(-3.50)	(-1.02)	(-0.76)	(-0.33)	(0.92)
tangibility	0.123**	0.255***	0.358***	0.296***	-0.006	0.296***
	(2.55)	(4.12)	(4.93)	(4.14)	(-0.06)	(3.34)
profitability	-0.077***	-0.088*	0.012	-0.063	-0.003	-0.028
	(-2.74)	(-1.71)	(0.24)	(-1.15)	(-0.06)	(-0.54)
cf_vol	-0.366**	-0.497	-0.078	-0.124	-0.434	-0.222
	(-2.26)	(-1.44)	(-0.22)	(-0.39)	(-1.44)	(-0.67)
R&D	-0.125***	-0.149	0.268**	-0.015	0.001	0.050
	(-2.59)	(-0.61)	(1.98)	(-0.12)	(0.01)	(0.60)
dividend_payer	-0.027*	0.002	0.009	-0.017	-0.020	-0.039
	(-1.78)	(0.17)	(0.51)	(-1.18)	(-0.91)	(-1.47)
leverage	0.285***	0.287***	0.243***	0.261***	0.468***	0.276***
	(10.17)	(6.99)	(6.24)	(7.46)	(7.37)	(5.21)
Diff(coef)	0.006**		0.019***		0.016**	
p-value	0.020		0.000		0.020	
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Ind×Yr FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	9,929	10,548	4,810	7,049	3,520	3,425
Adj. R-squared	0.487	0.542	0.504	0.536	0.464	0.510

Table 2.10 (continued)

Panel B: StkFrag results

	(1)		(2)		(3)	
	Debt_Heterogeneity		Debt_Heterogeneity		Debt_Heterogeneity	
VARIABLES	*Small Size*	*Large Size*	*High Uncertainty*	*Low Uncertainty*	*High Bankdebt*	*Low Bankdebt*
StkFrag	-0.092**	-0.056	-0.185***	-0.031	-0.142**	-0.050
	(-1.96)	(-1.24)	(-3.26)	(-0.74)	(-1.99)	(-0.82)
size	0.019***	0.054***	0.056***	0.074***	-0.005	0.064***
	(2.84)	(4.82)	(4.20)	(6.25)	(-0.24)	(3.37)
Tobins_Q	-0.010*	-0.033***	-0.009	-0.004	-0.001	0.010
	(-1.84)	(-3.19)	(-0.82)	(-0.45)	(-0.13)	(0.95)
tangibility	0.148***	0.276***	0.356***	0.294***	0.021	0.325***
	(2.91)	(4.16)	(4.80)	(4.04)	(0.20)	(3.50)
profitability	-0.081***	-0.106*	0.015	-0.084	-0.001	-0.020
	(-2.78)	(-1.82)	(0.27)	(-1.47)	(-0.01)	(-0.36)
cf_vol	-0.289	-0.305	-0.089	-0.006	-0.554	-0.221
	(-1.62)	(-0.79)	(-0.23)	(-0.02)	(-1.62)	(-0.58)
R&D	-0.128**	-0.212	0.246*	-0.030	-0.016	0.067
	(-2.44)	(-0.91)	(1.80)	(-0.23)	(-0.16)	(0.68)
dividend_payer	-0.029*	0.002	0.004	-0.017	-0.048**	-0.033
	(-1.87)	(0.17)	(0.21)	(-1.09)	(-2.08)	(-1.19)
leverage	0.277***	0.305***	0.247***	0.267***	0.534***	0.266***
	(9.50)	(7.02)	(6.17)	(7.47)	(7.32)	(4.78)
Diff(coef)	-0.036*		-0.154***		-0.092***	
p-value	0.070		0.000		0.010	
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Ind×Yr FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	9,572	10,114	4,633	6,835	3,175	3,148
Adj. R-squared	0.490	0.542	0.501	0.533	0.497	0.492

Figure 2.1: Univariate analysis – SPI and debt heterogeneity

This figure presents the univariate analysis of the relationship between stock price informativeness and corporate debt concentration structure (as measured by *Debt_Heterogeneity*). For each year, firms are first sorted into deciles by size. Then, within each size decile for each year, firms are sorted into deciles according to the informativeness of their stock prices. Finally, the time-series mean of *Debt_Heterogeneity* for each stock price informativeness (SPI) decile is calculated. The figure at the top (Figure 2.1.a) plots the mean *Debt_Heterogeneity* for each *PIN* decile. The figure at the bottom (Figure 2.1.b) plots the mean *Debt_Heterogeneity* for each *NONSYNC* decile.

Figure 2.1.a

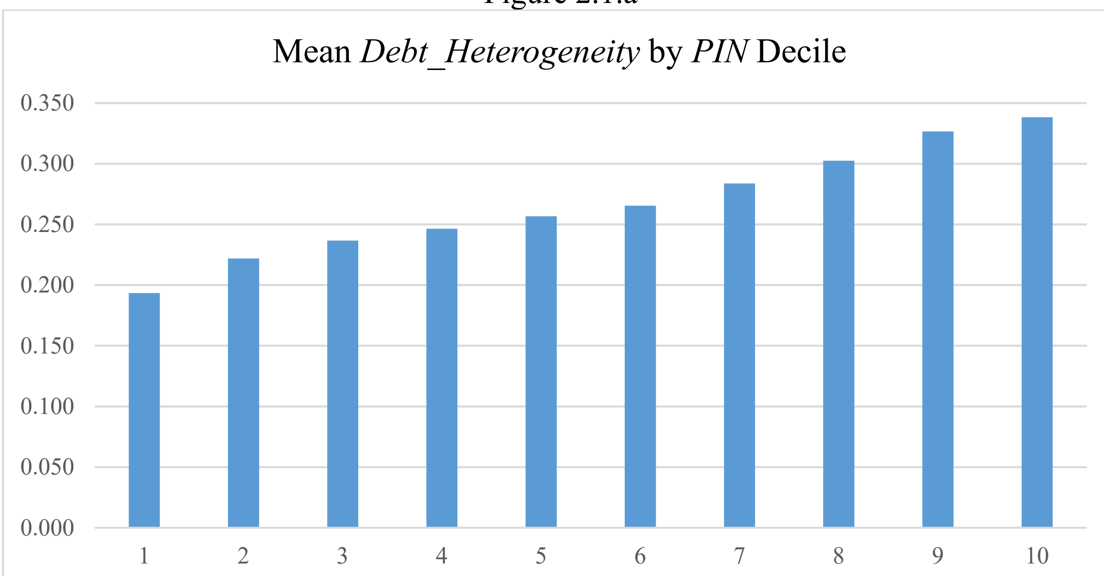


Figure 2.1.b

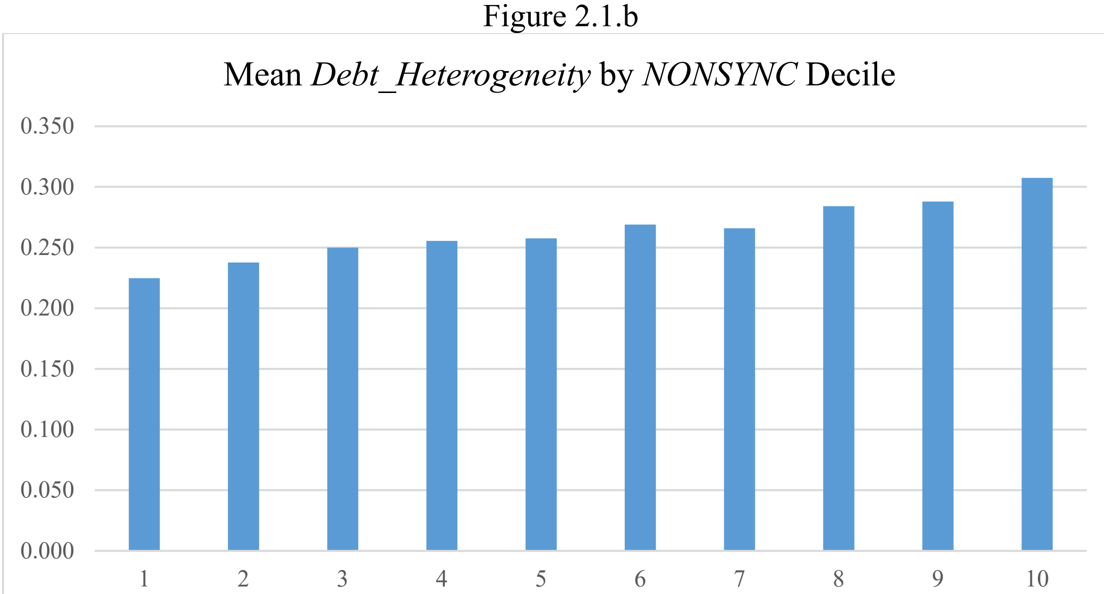


Figure 2.2: BlackRock-BGI merger DiD analysis – Dynamics of responses

This figure plots coefficient estimates from the following dynamic difference-in-differences regression model:

$$Family\_StkFrag_{it} \text{ or } Debt\_Heterogeneity_{it} = \sum_{\tau=-3,\tau\neq-1}^{\tau=2} \beta_\tau treat_i * \mathbb{I}\{t=\tau\} + \gamma X_{it} + Firm\ FE + Ind \times Yr\ FE\ + \varepsilon_{it}$$

where *treat* equals one if a firm is jointly held by BGI and BlackRock in 2009Q1 and zero if held by only one of BGI and BlackRock in 2009Q1; and X_{it} is a vector of firm-level characteristics controls. As this analysis is based on fiscal year-ends, the pre-event period is from 2006 ($\tau = -3$) to 2008($\tau = -1$), and the post-event period is from 2009 ($\tau = 0$) to 2011 ($\tau = 2$). The fiscal year 2008 ($\tau = -1$) is excluded as the reference level. The red dots correspond to estimates of the β_τ coefficients. The vertical vars correspond to 95% confidence intervals. Standard errors are clustered at the firm level.

Figure 2.2.a

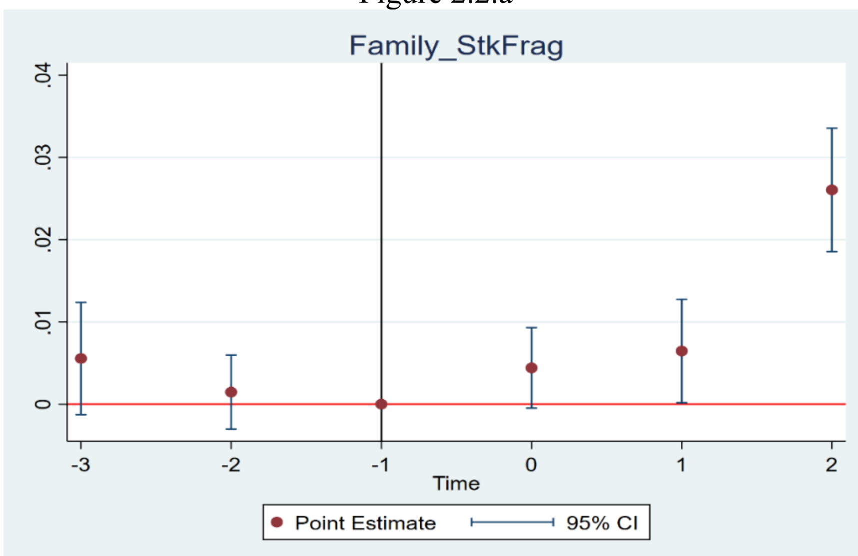
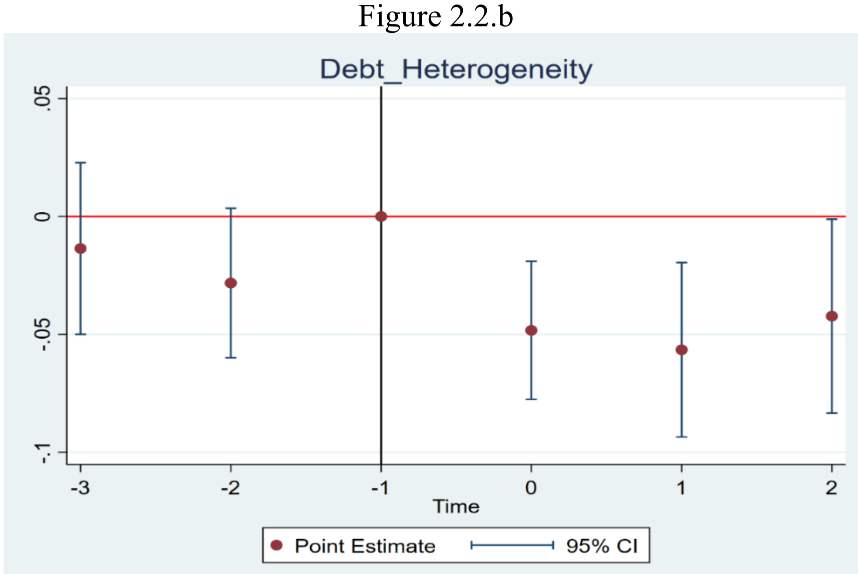


Figure 2.2.b



CHAPTER 3. Government Subsidies and the Choice between Bank and Public Debt

3.1 Introduction

Government subsidies are a prevalent form of political incentives extended to businesses in the United States. According to a report by Good Jobs First (GJF), government subsidies granted to U.S. firms within the past few decades have exceeded a staggering $64 billion.[1] Given the recent intensification of global trade wars and the revival of industrial policy, both the incidence and the amount of subsidies granted to U.S. corporations are expected to rise further. Against this backdrop, it becomes important to understand the potential implications of government subsidies on firm outcomes and policy choices. While the extant literature provides some insights into how the various forms of government-provided incentives influence corporate investment and productivity (e.g., Cerqua and Pellegrini, 2014; Criscuolo et al., 2019; Dong et al., 2023), innovation (e.g., Becker, 2015; Howell, 2017; Kong, 2020), financial reporting and disclosure (e.g., Huang, 2022; Pappas et al., 2024), and propensity to commit misconduct (e.g., Raghunandan, 2024), no studies have directly studied the potential impacts of receiving subsidies on firm debt structure. This present study addresses this gap in the literature.

This paper explores the potential interplay between government subsidies and firms' choice between bank and public debt. Not only are bank and public debt primary sources of external financing, the success of subsidy programs may hinge on the debt structure of recipient firms. For example, bank debt often comes with restrictive covenants and higher monitoring, potentially limiting the efficient use of subsidies for investment

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[1] https://goodjobsfirst.org/wp-content/uploads/docs/pdf/megadeals_report.pdf

opportunities (e.g., Berlin and Mester, 1992; Chava and Roberts, 2008; Giambona et al., 2021). In this context, public debt can provide firms with more financial flexibility and fewer constraints (e.g., Gilson and Warner, 1998; Atanassov, 2016), enabling them to fully leverage the benefit of subsidies so that they can deliver the promise they made. Moreover, government subsidies can render recipient firms' operations and cash flow more dependent on political dynamics, thus amplifying their exposure to political uncertainty and risk (e.g., Pastor and Veronesi, 2012). Given the different levels of difficulty in renegotiation associated with different debt types, subsidized firms' debt composition choice can impact their ability to respond to the shifting political landscape. In short, debt structure choices by subsidized firms may have ramifications on both the effectiveness of the subsidy initiatives and the adaptability of the recipient firms in the face of political uncertainties, warranting a systematic empirical investigation on the matter as a stepping stone to glean further policy implications.

I propose two competing hypotheses on the potential interplay between government subsidies and corporate debt structure choice. On the one hand, government subsidies may induce firms to shift their debt structure towards public debt and away from bank debt through several mechanisms, which I collectively term the 'governance hypothesis.' First, the scrutiny and enhanced governance channel posits that government subsidies can lead to increased scrutiny from both public and governmental entities (Huang, 2022), serving as a form of external governance. This additional oversight could lessen the need for the closer monitoring often associated with bank debt, thereby promoting a shift toward public debt (e.g., Boubaker et al., 2018; Bharath and Hertzel, 2019). Second, the transparency required by government subsidies could lead to a more open information environment surrounding a firm (Huang, 2022; Dong et al., 2023), thereby decreasing the information asymmetry between firms and potential investors. This reduction in information asymmetry could alleviate adverse selection and agency costs of debt often associated with public debt, inducing firms to reduce their reliance on bank debt further (e.g., Krishnaswami et al., 1999; Hadlock and James, 2002; Li et al., 2019). Finally, the government certification channel posits that government subsidies can function as a certification or signal of a firm's legitimacy and viability (Bellucci et al., 2023). The associated endorsement effect could lower the perceived riskiness of the firm for potential investors, making it more appealing to public debt investors and thereby facilitating a shift away from bank debt.

On the other hand, receiving government subsidies may induce firms to rely more on bank debt over public debt. First, the monitoring need channel posits that government subsidies could increase agency problems within the firm. Higher levels of free cash flow resulting from subsidies might lead to overinvestment or entrenchment problems (Jensen, 1986). Therefore, firms may opt for bank debt to take advantage of banks' monitoring and disciplining roles to mitigate such agency issues. Second, the potential downside risk channel suggests that while government subsidies might be a positive signal about a firm's future, they also introduce downside risk because they can be withdrawn or reduced. This risk could make firms more conservative in their financing decisions, inducing them to favor the close relationship and potential support offered by banks in times of financial distress over the impersonal nature of public debt markets (e.g., Bolton et al., 2016). Lastly, the political risk exposure channel suggests that receiving government subsidies might tie a firm's fortunes more closely to the political landscape, thereby increasing its exposure to political risk. That is, by becoming subsidy recipients, firms may find themselves more vulnerable to shifts in political direction or policy changes (e.g., Pastor and Veronesi, 2012). Anticipating this enhanced political risk exposure, firms might gravitate towards bank debt, which often provides greater potential for renegotiation than public debt (e.g., Gilson et al., 1990; Chemmanur and Fulghieri, 1994). I collectively term these channels 'free cash flow and risk hypothesis.'

To test the above competing hypotheses, I examine the relationship between government subsidies and firms' choice between bank and public debt using a sample of U.S. public firms. The results from baseline analysis indicate that receiving government subsidies is associated with a lower (higher) reliance on bank (public) debt, providing support for the governance hypothesis. Adopting Tobit specifications to account for the bounded nature of the dependent variables (i.e., bank debt to total debt and public debt to total debt ratios) yields similar results.

To address concerns related to selection on observables, I implement Propensity Score Matching (PSM) and entropy balancing methodologies in my analysis. These procedures aim to control for potential differences in observable characteristics between firms receiving government subsidies and those that do not, thereby ensuring that the documented association between subsidies and firms' debt choices is not merely a product of selection bias. In the PSM analysis, I match subsidized and unsubsidized firms based on a host of firm-level control variables and restrict control firms to those that never received any subsidy during the entire sample period (Huang, 2022). The results reaffirm the baseline findings, indicating that subsidies are negatively (positively) associated with a reliance on bank (public) debt. Furthermore, the entropy balancing approach, which reweights the dataset to equalize the moments of firm characteristics between the two groups, also corroborates the baseline results while preserving the sample's entirety. In both the PSM and entropy balanced analyses, receiving government subsidies do not significantly affect a firm's total leverage in the following year, suggesting that subsidies primarily impact the structure, not the level, of a firm's debt. Taken together, these findings alleviate concerns that the baseline results are primarily driven by observable differences between subsidized and unsubsidized firms and provide robust support for the governance hypothesis.

To further address concerns regarding selection on unobservables and the severity of endogeneity, I conduct an instrumental variable (IV) analysis using changes in congressional committee chairmanships and ranking minority memberships as an instrument for government subsidies. Following the approach proposed by Cohen et al. (2011), I leverage the fact that firms in states represented by newly appointed chairs or ranking minority members of key congressional committees are likely to experience an increase in subsidies due to increased federal spending allocations. This provides an exogenous source of variation in the likelihood and size of government subsidies, arguably unrelated to firms' debt structure decisions or unobserved factors potentially influencing such decisions. The results from the IV analysis also align with previous findings: subsidies are negatively associated with bank debt ratio and positively related to public debt choice. Moreover, again, subsidies do not significantly impact a firm's total leverage but rather influence the structure of the firm's debt, consistent with the results of the PSM and entropy balancing analyses.

Lastly, I conduct cross-sectional analyses to further examine the specific channels through which government subsidies influence firms' choices between bank and public debt. The results reveal that all three mechanisms of the governance hypothesis are at play. Specifically, I find that the impact of government subsidies on firms' switching from bank to public debt is more pronounced for firms with better governance and higher institutional ownership, consistent with the scrutiny and enhanced governance channel. Also, consistent with the alleviation of information asymmetry and government certification effect channels, firms with less informative stock prices and higher political sentiment show more pronounced shifts toward public debt.

This paper makes several contributions. First, it contributes to the literature on debt structure choice by showing that receiving government subsidies significantly induces firms to shift away from bank debt and toward public debt. To my knowledge, this paper is the first to provide empirical evidence on the interplay between government subsidies and firms' choice between bank and public debt. Taken together, my findings indicate that the incentive to avoid over-monitoring (over-governance) and alleviation of information asymmetry associated with transparency demands and government endorsement (certification) effects play an important role in determining subsidy recipient firms' choice between bank and public debt.

Second, my findings contribute to the literature examining the effects of government subsidies or other forms of political incentives on corporate policies or outcomes (e.g., Howell, 2017; Criscuolo et al., 2019; Kong, 2020; Huang, 2022; Dong et al., 2023; Raghunandan, 2024). Against the backdrop of the expected increase in government subsidies due to the intensified global trade war and ensuing industrial policy, examining how firms adjust their financing structure upon receiving government subsidies, particularly the mix of bank and public debt, commands particular attention due to its potential implication on the flexibility (e.g., less tight covenants and monitoring associated with public debt) versus stability (e.g., easier renegotiation and relationship lending associated with bank debt) trade-off. By showing that subsidized firms tilt toward more public debt, my findings provide suggestive evidence that flexibility motivation may dominate stability motivation when it comes to debt structure adjustment in response to receiving subsidies.

Relatedly, my findings may have policy implications; even though the political risk exposure channel seems to be dominated by other channels in the data, it may still be true that government subsidies increase firms' exposure to political risk, and I find that firms rely more on public debt upon receiving subsidies, which can make them less insulated from potential political shock events due to relative difficulty in renegotiation for public debt contracts.

3.2 Data, Sample, and Variables

3.2.1 Data

In addition to Compustat database for firm-level accounting information, I employ two additional datasets to study the relationship between government subsidies and corporate debt structure. First, I obtain government subsidy and recipient firm information from the Subsidy Tracker database provided by Good Jobs First (GJF). The Subsidy Tracker is a comprehensive, company-specific record of subsidies given to businesses from federal, state, and local economic development initiatives. More specifically, it provides detailed information on each subsidy program included, such as the name and description of the program, the recipient firm's name and CIK (of the parent company), the year of the grant, the dollar value of subsidy granted, the subsidy type (e.g., cash grant, cost reimbursement, tax credit, loan), and the level of the granting government (e.g., federal, state, or local). In creating the database, GJF collates subsidy information from various sources such as government reports and disclosure, the media, corporate press releases, and Freedom of Information Act (FOIA) requests. Since most subsidy information is sourced from government entities or via FOIA requests, the potential for bias in disclosure is relatively low (Pappas et al., 2024).

Firm debt structure information comes from S&P Capital IQ database. Capital IQ decomposes each firm's total debt into seven different debt types that are mutually exclusive: commercial paper (CP), drawn credit lines (DC), term loans (TL), senior bonds and notes (SBN), subordinated bonds and notes (SUB), capital leases (CL), and other debt (Other). Based on this information, I calculate the ratio of bank debt to total debt and public debt to total debt, respectively, to measure firms' choice between bank and public debt.

3.2.2 Sample

The main sample of this study is constructed from the intersection of the Subsidy Tracker, Compustat, and Capital IQ datasets. I merge the Subsidy Tracker with Compustat based on CIK and Compustat with Capital IQ on GVKEY. As the Subsidy Tracker provides the subsidiary-parent linkage information at the time of the data release only, I use the parent-subsidiary linktable created by Aneesh Raghunandan (Raghunandan, 2021;

Raghunandan, 2024) to identify parent companies for each subsidiary at the time of subsidy grants (i.e., 'historical' parents).[2]

I apply additional sample screenings after merging the three datasets. First, I drop financial firms (SIC codes from 6000 through 6999), regulated utilities (SIC codes from 4900 through 4949), and non-operating establishments (SIC codes from 9000 through 9999). Additionally, I exclude from the sample (1) firm-year observations with missing or non-positive total book assets; (2) firm-years with missing or non-positive book equity; (3) firm-years with missing or negative value for total debt; and (4) firm-years with total book assets less than $1 million. Lastly, I remove observations with missing values for any debt structure, subsidy, and firm-level control variables in the regression analysis (discussed later).

As the coverage of the Subsidy Tracker data becomes much more complete after 2004 and onward (Raghunandan, 2024) and less complete for the years 2017 and onward (De Simone et al., 2022), I restrict the sample period to the years spanning 2005 to 2016. As the main dependent variables of the study are debt structure choice variables, I also exclude the financial crisis years of 2008 and 2009 to avoid potential distortionary effects from the turbulent period. After the above series of refinement, the final sample comprises 21,178 firm-year observations from 4,190 unique U.S. public firms.

3.2.3 Variables

Following previous research (e.g., Lin et al., 2013; Chen et al., 2021; Huang et al., 2023), I construct two measures using the debt structure information from Capital IQ to

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[2] I deeply appreciate Aneesh Raghunandan for sharing his manually matched data of historical parent company information.

examine firms' choice between bank and public debt: *BankDebt*, which is the ratio of bank debt to total debt, and *PublicDebt*, the ratio of public debt to total debt. These two debt structure variables are measured in the year following the subsidy receipt (i.e., year t+1) to adequately capture the firm's adjustment in debt structure influenced by the subsidies received in the previous year.

I employ two measures to capture firms' receipt of government subsidies for a given year. *subsidy_dummy* is a dummy variable that equals one when a firm receives any types of government subsidies in year t, and zero otherwise. *subsidy_amt* is the natural logarithm of one plus the aggregate dollar amount of government subsidies a firm receives in year t.

I include ten firm-level control variables suggested by the literature as debt structure determinants and/or influencing firms' propensity to receive government subsidies (e.g., Krishnaswami et al., 1999; Hadlock and James, 2002; Lin et al., 2013; Boubaker et al., 2018; Huang, 2022): *size, asset growth, MB, tangibility, ROA, cfvol, cash, R&D, employee,* and *leverage*. Table 3.1 presents the summary statistics. Detailed definition and calculation of the variables are summarized in Table C.1 of Appendix C.

3.3 Main Analyses

3.3.1 Baseline Results

I employ the following baseline regression model to examine the impact of government subsidies on firms' choice between bank and public debt:

$$y_{i,t+1} = \alpha + \beta \times SUBSIDY_{i,t} + \gamma X_{i,t} + Ind\ FE + Year\ FE + \varepsilon_{i,t}$$

where $y_{i,t+1}$ is debt structure choice variable (i.e., *BankDebt* or *PublicDebt*), $SUBSIDY_{i,t}$ is either $subsidy\_dummy$ or $subsidy\_amt$, and $X_{i,t}$ is a vector of firm-level controls. All regression specifications include industry and year fixed effects, where the industry is defined under Fama and French 49-industry classification. Standard errors are clustered at the firm level. To account for potential mechanical correlation between firm debt capacity and preference over a specific type of debt, *leverage* is included as one of the firm-level controls in all specifications.

Table 3.2 presents the baseline results. In columns (1) and (2), government subsidies are measured using the dummy variable *subsidy_dummy*, and in columns (3) and (4), measured using *subsidy_amt*. The coefficients on *subsidy_dummy* and *subsidy_amt* are negative (positive) and statistically significant at less than 1% level when the dependent variable is *BankDebt* (*PublicDebt*), suggesting that subsidized firms have a higher reliance on public debt financing over bank financing compared to unsubsidized firms. The results are also economically significant. Specifically, the coefficients on *subsidy_dummy* imply that firms that receive government subsidies have *BankDebt* (*PublicDebt*) following the year of subsidy that is approximately 22.65% lower (24.72% higher) than firms that do not receive subsidies.

Overall, baseline findings are in line with the governance hypothesis, which predicts higher public debt preference and less reliance on bank debt by subsidized firms than unsubsidized firms.

3.3.2 Robustness Tests

In this subsection, I assess the robustness of my baseline findings to alternative model specifications, selection biases, and alternative explanations. I also conduct an IV analysis to alleviate endogeneity concerns behind the main findings.

3.3.2.1 Tobit Specification

I estimate the baseline model using Tobit regression instead of OLS because the dependent variables are ratio variables bounded by 0 and 1. All the other aspects of the model specification remain unchanged. The results shown in Table 3.3 indicate that the results remain quantitatively and qualitatively similar to the baseline OLS results.

3.3.2.2 Propensity Score Matching (PSM) Analysis

It is possible that subsidy-receiving firms are fundamentally different from unsubsidized firms and such systematic difference between the two groups of firms is entirely driving the documented results. In this regard, I conduct a PSM analysis to alleviate the concern that the baseline findings are merely the artefact of selection bias.

In implementing the PSM analysis, I match subsidized firm-years with unsubsidized firm-years. Importantly, I restrict the control firms to those that never receive any government subsidy throughout the entire sample period because a firm that receives a subsidy in one year may still see the influence of that subsidy on its debt structure choice in a year when it does not receive a subsidy, in a similar spirit to Huang (2022). Moreover, this approach ensures that firm-years from firms that receive subsidy at least once during the sample period are not overrepresented in the PSM sample, a valid concern to the extent that firm characteristics are persistent over time.

I adopt nearest-neighbor matching without replacement, imposing a caliper width of 0.01. The matching is based on all the ten firm-level control variables used in the baseline estimation. The resulting covariate balance tests are reported in Table C.2. After the matching is implemented, I observe no systematically significant differences for any covariates between the subsidized and unsubsidized groups after matching, which suggests that the matching procedure is effective.

I proceed to estimate the same baseline model using the matched sample, of which results are reported in Table 3.4. I continue to find a significant negative (positive) association between government subsidies and the ratio of bank (public) debt to total debt, all statistically significant at less than 1% level. Interestingly, within the matched sample, I also find that receiving a government subsidy in year t does not significantly predict the firm's leverage ratio in the following year. This offers suggestive evidence that government subsidies may not considerably impact a firm's total indebtedness, but instead, they primarily impact its choice between different types of debt. Taken together, the results from the PSM analysis at least mitigate the concern that selection on observables is the main driver of my results.

3.3.2.3 Entropy Balancing Approach

One caveat of the PSM approach is that the matching procedure can lead to a much smaller sample size, compromising the generalizability of the results. To address this point and supplement the results from the PSM analysis, I also conduct an analysis employing entropy balancing (Hainmueller, 2012). Unlike the PSM method, the advantage of this approach is that it utilizes a reweighting scheme to ensure similarity between the two groups in terms of moments, thereby preserving all observations, which avoids the potential data loss associated with the PSM method.

Table C.3 reports the weight balance after applying the entropy balancing approach to the original sample. After balancing, the two groups of firms exhibit identical means for all ten firm characteristics. Also, the second moments for those variables between the two groups are highly similar, indicating that the approach could successfully generate weights that nearly equalize both the first and second moments of the observed firm characteristics between the two groups.

I estimate the same baseline OLS regression model on the entropy-balanced sample, of which results are reported in Table 3.5. The findings are in line with both the baseline and the PSM analysis; there is a significant negative (positive) association between government subsidies and *BankDebt* (*PublicDebt*), all statistically significant at less than 1% level. Again, I find statistically insignificant coefficients for subsidy variables when the dependent variable is the following year's leverage ratio, echoing the interpretation that government subsidies may not be a determinant of a firm's total leverage, but rather, they may only influence the structure of a firm's debt. Supplementing the results from the PSM analysis, these findings further corroborate the argument that observable differences between subsidized and unsubsidized firms are unlikely to be the main driver behind the baseline results.

3.3.2.4 Alternative Explanation – Credit Ratings and Access to Finance

Another potential alternative explanation for the observed relation between government subsidies and firms' choice between bank and public debt is that subsidized firms, which on average are bigger, more profitable, and more stable, have better credit ratings and, consequently, have different levels of access to various forms of debt financing compared to unsubsidized firms. In order to address this possibility, I control for differences in firms' access to finance due to varying credit ratings by modifying the baseline regression model to include a far more stringent industry x credit rating group x year fixed effects.[3] This specification effectively allows for a comparison of the debt structure choice of subsidy-receiving firms to that of non-subsidized firms within the same industry and credit ratings group in a given year, after additionally accounting for time-varying firm-level covariates. Due to the availability of S&P credit rating data, the sample period for this analysis is only up to 2014.

Table 3.6 presents the results. Even after incorporating more stringent fixed effects, the findings continue to align with those from all previous analyses. Specifically, regardless of whether the subsidy status is measured with the dummy variable (columns 1 and 2) or amounts (columns 3 and 4), the results consistently suggest a significant negative relationship between government subsidies and the choice of bank debt and a significant positive relationship with the choice of public debt, all statistically significant at less than the 1% level. Therefore, even after accounting for the potential alternative explanation related to the varying credit ratings and the ensuing difference in access to finance, the empirical results still provide robust evidence in support of the governance hypothesis of this study.

[3] I classify firms into seven different credit rating groups. For example, AAA-rated firms belong to group 1, AA+ to AA- to group 2, A+ to A- to group 3, and so on.

3.3.2.5 Instrumental Variable (IV) Analysis

To address the remaining concern regarding unobserved omitted variable bias and further deal with endogeneity issues, I employ an instrumental variable (IV) approach motivated by the findings of Cohen et al. (2011). Specifically, I use changes in congressional committee chairmanships and ranking minority memberships as an IV for government subsidies. The rationale behind this approach is that, as evidenced by Cohen et al. (2011), promotions to chairmanships or ranking minority positions in powerful congressional committees can cause a significant increase in federal spending allocations to the home state of the senator newly appointed to such positions. This boost in federal funding can subsequently enhance the likelihood and size of subsidies granted to firms domiciled in that state. As changes in chairmanships or ranking minority memberships are primarily determined by seniority rather than any characteristics of individual firms, the proposed IV thus provides a plausibly exogenous source of variation in the likelihood and size of government subsidies that are arguably unrelated to firms' choice on their debt structure or unobserved factors potentially influencing such decisions.

To implement the IV strategy, I define a dummy variable, *COMMITTEE*, that takes the value of one if the firm is headquartered in a state represented by a senator who has been newly appointed as the chairperson or the ranking minority member of one of the five most powerful committees - Finance, Veterans Affairs, Appropriations, Rules, or Armed Services - and zero otherwise. With this IV, I conduct two-stage least squares (2SLS) regression analyses, with the results presented in Table 3.7.[4]

[4] In the main analysis, I use *COMMITTEE* to instrument for the *subsidy_amt*, in order to consider both the extensive and intensive margins of the effect of committee leadership changes on firms' subsidy receipts. The alternative approach using *subsidy_dummy* yields qualitatively similar results.

As shown in column (1), the first-stage regression results show a significant positive association between *COMMITTEE* and *subsidy_amt*, affirming the relevance condition of a valid instrument. F-statistics of 14 surpasses the rule-of-thumb threshold of 10, alleviating the weak instrument concern. The results support the idea that firms are more likely to receive more subsidies when headquartered in a state represented by a newly appointed leader of powerful committees. Columns (2) to (4) present the results from the second-stage regressions. The IV estimates consistently show a negative relation between government subsidies and the choice of bank debt and a positive relation with the choice of public debt, both statistically significant at the 5% level. Nevertheless, no significant result is obtained when the following year's leverage ratio is regressed on the instrumented subsidy amount. These findings are all in line with the results obtained from the earlier analyses, further bolstering the robustness of the baseline results while at the same time alleviating concerns about the severity of endogeneity issues behind the results.

3.4 Cross-Sectional Analyses

The preceding sections establish a robust negative (positive) association between government subsidies and the reliance on bank (public) debt. As discussed, I interpret the documented relationship as consistent with the prediction from the governance hypothesis. In this section, I further test the validity of such interpretation by conducting cross-sectional analyses, which can help flesh out the specific channels or mechanisms through which government subsidies affect the choice between bank and public debt.

3.4.1 The Governance Channel

The governance channel postulates that government subsidies may lead to increased scrutiny from both public and government entities, potentially functioning as a form of external governance. This could alleviate the need for the closer monitoring often associated with bank debt, thereby leading to a shift towards public debt.

To test this channel, I examine the moderating effect of corporate governance quality on the relationship between government subsidies and firms' choices of debt structure. If the suggested channel is indeed at play, I would expect to observe a more pronounced shift from bank debt to public debt for firms with stronger corporate governance mechanisms following the receipt of government subsidies, as the external governance induced by subsidies may render the close monitoring performed by bank redundant, possibly leading to an "over-governance/monitoring" situation.

The results of the relevant cross-sectional tests are presented in Table 3.8. Unless explicitly stated otherwise, the 'high' group refers to the top 40% of firms based on the pertinent moderating variable, and the 'low' group denotes the bottom 40% throughout the cross-sectional analyses section. In Panel A, where a firm-level hostile takeover susceptibility index by Cain et al. (2017) is used as a measure of corporate governance quality, firms with stronger governance mechanisms exhibit a more pronounced shift from bank debt to public debt when receiving government subsidies; the coefficient for the interaction term of *subsidy_dummy* (or *subsidy_amt*) and *high_governance* is negative and significant in the bank debt regressions and positive and significant in the public debt regressions. Panel B presents similar results using institutional ownership as the proxy for governance quality. Firms with higher institutional ownership also show a stronger tendency to shift from bank to public debt upon receipt of government subsidies, as evidenced by the statistically significant and directionally consistent coefficients for the interaction terms of main interest. Overall, these findings lend additional support to the governance hypothesis, particularly emphasizing the role of scrutiny and enhanced governance as one of the key mechanisms.

3.4.2 Certification Benefit

I also test the cross-sectional implications of the two interrelated channels, the information asymmetry and the government certification channels. The crux of the information asymmetry channel is that the transparency requirements and the ensuing open information environment associated with government subsidies could decrease the information asymmetry between firms and potential investors, facilitating the firm's access to the public debt market. Relatedly, the government certification channel postulates that government subsidies can serve as a certification or signal of the firm's legitimacy and viability, reducing the perceived riskiness or opaqueness of the firm for potential external investors.

The term "certification benefit" encapsulates the idea that these channels both work towards enhancing the perceived credibility and reducing the information asymmetry surrounding a firm, thereby augmenting its ability to tap into public debt markets and lessen its reliance on bank debt, especially in the context of receiving government subsidies. The main implication is that firms with a higher degree of information asymmetry or firms that stand to benefit most from the endorsement effect of government subsidies should have more pronounced shifts from bank debt to public debt. To test this prediction, I employ stock price informativeness measured with price-nonsynchronicity (i.e., 1-R^2) and firm-level political sentiment by Hassan et al. (2019)[5] as moderating variables.

Table 3.9 presents the results of the cross-sectional tests examining the impact of the expected degree of certification benefit on the relationship between government subsidies and firms' choices of debt structure. In Panel A, where stock price informativeness is used as a moderating variable, I find that firms with less informative stock prices, and thus higher levels of information asymmetry, exhibit more pronounced changes in their debt structure upon receiving government subsidies, as evidenced by the negative and significant coefficients for *subsidy_dummy*low_SPI* and *subsidy_amt * low_SPI*, respectively. Correspondingly, there is an increase in their utilization of public debt, as indicated by the positive and significant coefficients for the interaction terms when the dependent variable is *PublicDebt*.

In Panel B, I use firm-level political sentiment as a moderating variable. If the government certification channel is at work, I expect to find a more pronounced effect of subsidies on debt structure choice for firms with higher political sentiment. The rationale behind this prediction is that firms expressing a positive tone or view regarding their own political situation can be seen as having a more favorable standing in political matters or better political connections, which in turn can amplify the endorsement effects of government subsidies; the synergistic effect reinforces their legitimacy and credibility, decreases their perceived risk, and consequently, may lead to a more significant shift from bank debt to public debt when they receive government subsidies. Consistent with these

[5] Firm-level political sentiment measures the sentiment expressed by call participants when specifically discussing politics-related issues. It is constructed using textual analysis of quarterly earnings conference call.

lines of arguments, the results indicate that firms with higher political sentiment show more substantial shifts in their debt structure, underpinned by a reduction in bank debt and an increase in public debt upon receiving government subsidies, as reflected by the negative (positive) and significant coefficients for the interaction terms when the dependent variable is *BankDebt* (*PublicDebt*).

Taken together, these results provide empirical support for the idea that the certification benefit, embodied in both the information asymmetry channel and the government certification channel, plays a significant role in shaping firms' debt structure decisions in the context of government subsidies.

3.5 Conclusion

In this paper, I study whether and how government subsidies influence recipient firms' choice between bank and public debt. The constraints and oversights often associated with bank debt financing may inhibit optimal subsidy utilization. Public debt financing, in contrast, can offer greater flexibility, enabling subsidized firms to fully leverage the benefit of subsidies. However, subsidies can heighten receiving firms' political risk by tying them closer to political dynamics, rendering the ease with which they can renegotiate the terms of debt an important consideration. In this regard, how subsidized firms adjust their debt structure can influence both the subsidy program's success and the firms' resilience to political shocks, underscoring the need for an empirical investigation as a stepping stone for gaining further policy insights.

Using a novel dataset that tracks government subsidies granted to U.S. business entities, I provide robust large-sample evidence that receiving government subsidies is positively (negatively) associated with firms' tendency to rely on public (bank) debt, consistent with the prediction of the governance hypothesis. The documented effect is stronger for firms with better corporate governance, a higher degree of information asymmetry, and more positive political sentiment, in line with the argument that the enhanced external scrutiny and increased perceived credibility stemming from the endorsement effects of subsidies diminish the need for the traditional monitoring and informational roles of bank debt.

Overall, the empirical relationship between government subsidies and firms' choice between bank and public debt reveals an interesting policy implication - even though the political risk exposure channel seems to be dominated by other channels in the data, government subsidies still are expected to increase firms' exposure to political uncertainty and risk to some extent. Firms, on average, shift towards more public debt upon receiving subsidies, and such debt structure adjustment behaviors of subsidy-receiving firms can make them less insulated from potential political shocks due to the relative difficulty in renegotiation for public debt contracts.

Table 3.1: Summary statistics

This table reports summary statistics of the selected sample for the measures of debt structure, government subsidies, and firm-level control variables. Financial firms (SIC codes from 6000 through 6999), regulated utilities (SIC codes from 4900 through 4949), and non-operating establishments (SIC codes from 9000 through 9999) are excluded from the sample. The sample spans 2005 to 2016, excluding the financial crisis period (i.e., 2008 and 2009). All continuous firm-level control variables are winsorized at the 1st and 99th percentile. Detailed definitions and calculations of the variables are shown in Table C.1 of Appendix C.

Variable	**N**	**Mean**	**Std Dev**	**Q1**	**Median**	**Q3**
Debt Structure						
BankDebt	21178	0.362	0.415	0	0.109	0.852
PublicDebt	21178	0.356	0.415	0	0.026	0.823
Subsidy Measures						
subsidy_dummy	21178	0.219	0.414	0	0	0
subsidy_amt	21178	2.837	5.465	0	0	0
Firm-level Controls						
size	21178	6.374	2.169	4.884	6.431	7.854
leverage	21178	0.194	0.182	0.008	0.164	0.314
asset growth	21178	0.136	0.372	-0.027	0.056	0.176
MB	21178	2.092	1.519	1.198	1.604	2.377
tangibility	21178	0.238	0.231	0.063	0.154	0.340
ROA	21178	-0.023	0.225	-0.023	0.038	0.078
cfvol	21178	0.020	0.025	0.007	0.011	0.022
cash	21178	0.216	0.234	0.042	0.128	0.307
R&D	21178	0.055	0.112	0.000	0.004	0.062
employee	21178	0.005	0.008	0.001	0.003	0.006

Table 3.2: Baseline OLS results

This table presents baseline OLS results for the relation between government subsidies and firms' choice of debt structure. All regression specifications include industry and year fixed effects, where the industry is defined under Fama and French 49-industry classification. Debt structure variables are measured in year t+1, while all independent variables are measured in year t. All continuous firm-level control variables that enter regressions are winsorized at the 1st and 99th percentile. t-statistics based on standard errors clustered at firm level are reported in parentheses below the coefficient estimates. *, **, *** represent statistical significance at the 10%, 5%, and 1% level, respectively. Detailed definitions and calculations of the variables are shown in Table C.1 of Appendix C.

	(1)	(2)	(3)	(4)
VARIABLES	BankDebt	PublicDebt	BankDebt	PublicDebt
subsidy_dummy	-0.082***	0.088***		
	(-7.50)	(8.05)		
subsidy_amt			-0.007***	0.007***
			(-8.44)	(9.02)
size	-0.053***	0.088***	-0.051***	0.087***
	(-17.60)	(31.47)	(-16.85)	(30.32)
leverage	0.290***	0.643***	0.288***	0.645***
	(9.48)	(22.04)	(9.43)	(22.12)
asset_growth	0.057***	-0.020***	0.057***	-0.020***
	(7.21)	(-2.63)	(7.20)	(-2.61)
MB	0.006**	-0.034***	0.006*	-0.033***
	(2.00)	(-11.48)	(1.83)	(-11.28)
tangibility	-0.039	-0.005	-0.039	-0.005
	(-1.23)	(-0.15)	(-1.21)	(-0.19)
ROA	0.061**	-0.216***	0.058**	-0.213***
	(2.36)	(-9.32)	(2.25)	(-9.20)
cfvol	-1.395***	0.945***	-1.377***	0.926***
	(-6.97)	(4.98)	(-6.89)	(4.89)
cash	-0.597***	0.036	-0.597***	0.037
	(-20.91)	(1.53)	(-20.94)	(1.55)
RnD	0.158**	-0.177***	0.155**	-0.174***
	(2.55)	(-3.36)	(2.51)	(-3.31)
employee	3.170***	-0.591	3.164***	-0.585
	(3.75)	(-0.73)	(3.74)	(-0.72)
Industry FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	21,178	21,178	21,178	21,178
Adjusted R-squared	0.212	0.377	0.213	0.378

Table 3.3: Alternative specification – Tobit estimation results

This table reports the Tobit estimation results as an alternative specification for exploring the relationship between government subsidies and firms' choice of debt structure. All regression specifications include industry and year fixed effects, where the industry is defined under Fama and French 49-industry classification. Debt structure variables are measured in year t+1, while all independent variables are measured in year t. All continuous firm-level control variables that enter regressions are winsorized at the 1st and 99th percentile. t-statistics based on standard errors clustered at firm level are reported in parentheses below the coefficient estimates. *, **, *** represent statistical significance at the 10%, 5%, and 1% level, respectively. Detailed definitions and calculations of the variables are shown in Table C.1 of Appendix C.

	(1)	(2)	(3)	(4)
VARIABLES	BankDebt	PublicDebt	BankDebt	PublicDebt
subsidy_dummy	-0.133***	0.120***		
	(-6.51)	(6.08)		
subsidy_amt			-0.011***	0.010***
			(-7.24)	(6.39)
size	-0.075***	0.171***	-0.072***	0.170***
	(-13.04)	(27.55)	(-12.45)	(26.76)
leverage	0.679***	1.429***	0.676***	1.432***
	(11.57)	(23.46)	(11.53)	(23.49)
asset_growth	0.105***	-0.006	0.105***	-0.006
	(6.47)	(-0.32)	(6.47)	(-0.32)
MB	-0.005	-0.069***	-0.006	-0.069***
	(-0.61)	(-8.24)	(-0.73)	(-8.16)
tangibility	-0.051	-0.006	-0.050	-0.008
	(-0.92)	(-0.11)	(-0.90)	(-0.13)
ROA	0.036	-0.546***	0.032	-0.541***
	(0.57)	(-8.84)	(0.50)	(-8.77)
cfvol	-3.220***	1.412***	-3.185***	1.388***
	(-6.24)	(2.86)	(-6.18)	(2.82)
cash	-1.451***	-0.170***	-1.451***	-0.170***
	(-20.02)	(-2.68)	(-20.03)	(-2.68)
RnD	0.394**	-0.412***	0.390**	-0.409***
	(2.47)	(-2.74)	(2.45)	(-2.72)
employee	6.151***	-1.150	6.146***	-1.125
	(4.06)	(-0.60)	(4.06)	(-0.58)
Industry FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	21,178	21,178	21,178	21,178
Pseudo R-squared	0.139	0.237	0.140	0.237

Table 3.4: Propensity score matching results

This table presents the Propensity Score Matching (PSM) results. In the PSM process, nearest-neighbor matching without replacement is employed, with a caliper choice of 0.01. Firm-years that receive subsidies are matched with non-subsidized firm-years. In doing so, the selection of control firms is restricted to those that have never received any subsidies throughout the sample period. All regression specifications include industry and year fixed effects, with the industry defined under the Fama and French 49-industry classification. Debt structure variables are measured in year t+1, while all independent variables are measured in year t. All continuous firm-level control variables used in regressions are winsorized at the 1st and 99th percentile. t-statistics, based on standard errors clustered at the firm level, are reported in parentheses below the coefficient estimates. *, **, *** denote statistical significance at the 10%, 5%, and 1% level, respectively. Detailed definitions and calculations of the variables are shown in Table C.1 of Appendix C.

VARIABLES	(1) BankDebt	(2) PublicDebt	(3) leverage(t+1)	(4) BankDebt	(5) PublicDebt	(6) leverage(t+1)
subsidy_dummy	-0.058***	0.069***	-0.006			
	(-3.24)	(3.88)	(-0.74)			
subsidy_amt				-0.004***	0.006***	-0.001
				(-3.09)	(4.10)	(-0.98)
size	-0.082***	0.123***	0.020***	-0.081***	0.122***	0.020***
	(-10.41)	(16.09)	(5.71)	(-10.22)	(15.80)	(5.68)
leverage	0.103*	0.720***		0.101*	0.723***	
	(1.93)	(13.25)		(1.89)	(13.29)	
asset_growth	0.069***	-0.054***	0.045***	0.069***	-0.054***	0.045***
	(3.68)	(-2.80)	(4.86)	(3.70)	(-2.82)	(4.86)
MB	0.020**	-0.063***	-0.010***	0.020**	-0.063***	-0.011***
	(2.42)	(-9.14)	(-3.10)	(2.39)	(-9.11)	(-3.10)
tangibility	-0.175***	0.096*	0.130***	-0.173***	0.093*	0.131***
	(-3.42)	(1.83)	(4.74)	(-3.38)	(1.77)	(4.76)
ROA	0.083	-0.256***	-0.400***	0.077	-0.249***	-0.401***
	(1.26)	(-3.77)	(-10.61)	(1.17)	(-3.68)	(-10.62)
cfvol	-0.240	0.125	-0.590***	-0.230	0.109	-0.587***
	(-0.50)	(0.30)	(-2.73)	(-0.48)	(0.26)	(-2.71)
cash	-0.760***	0.245***	-0.311***	-0.759***	0.243***	-0.311***
	(-11.83)	(4.15)	(-10.86)	(-11.79)	(4.11)	(-10.86)
RnD	-0.128	-0.040	-0.221***	-0.134	-0.032	-0.222***
	(-0.88)	(-0.27)	(-2.59)	(-0.92)	(-0.21)	(-2.60)
employee	0.119	1.576	-4.288***	0.062	1.650	-4.295***
	(0.07)	(0.76)	(-8.03)	(0.04)	(0.79)	(-8.03)
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,212	4,212	4,212	4,212	4,212	4,212
Adjusted R-squared	0.213	0.348	0.322	0.212	0.348	0.322

Table 3.5: Entropy balancing results

This table reports OLS regression results for examining the relationship between government subsidies and firms' choice of debt structure using an entropy-balanced sample. The entropy balancing ensures balance on all observed covariates between the treatment and control groups. All regression specifications include industry and year fixed effects, where the industry is defined under Fama and French 49-industry classification. Debt structure variables are measured in year t+1, while all independent variables are measured in year t. All continuous firm-level control variables that enter regressions are winsorized at the 1st and 99th percentile. t-statistics based on standard errors clustered at firm level are reported in parentheses below the coefficient estimates. *, **, *** represent statistical significance at the 10%, 5%, and 1% level, respectively. Detailed definitions and calculations of the variables are shown in Table C.1 of Appendix C.

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	BankDebt	PublicDebt	leverage(t+1)	BankDebt	PublicDebt	leverage(t+1)
subsidy_dummy	-0.035***	0.040***	-0.008			
	(-3.46)	(3.45)	(-1.37)			
subsidy_amt				-0.003***	0.003***	-0.001
				(-3.57)	(3.59)	(-1.34)
size	-0.089***	0.128***	0.007**	-0.089***	0.127***	0.007**
	(-23.50)	(28.98)	(2.55)	(-22.74)	(28.08)	(2.56)
leverage	0.057	0.605***		0.056	0.606***	
	(1.41)	(13.29)		(1.40)	(13.30)	
asset_growth	0.080***	-0.081***	0.034***	0.080***	-0.081***	0.034***
	(6.81)	(-5.53)	(4.99)	(6.81)	(-5.53)	(4.99)
MB	0.019***	-0.058***	-0.001	0.019***	-0.057***	-0.001
	(3.12)	(-8.24)	(-0.34)	(3.07)	(-8.20)	(-0.36)
tangibility	-0.083**	-0.009	0.098***	-0.082**	-0.011	0.098***
	(-2.05)	(-0.20)	(4.72)	(-2.02)	(-0.24)	(4.72)
ROA	0.119**	-0.211***	-0.362***	0.116**	-0.206***	-0.363***
	(2.27)	(-3.45)	(-11.99)	(2.20)	(-3.39)	(-12.03)
cfvol	-0.134	0.087	-0.467*	-0.128	0.078	-0.465*
	(-0.37)	(0.20)	(-1.92)	(-0.36)	(0.18)	(-1.91)
cash	-0.640***	0.227***	-0.314***	-0.639***	0.226***	-0.313***
	(-13.25)	(4.58)	(-12.04)	(-13.23)	(4.55)	(-12.04)
RnD	-0.204*	-0.010	-0.283***	-0.206*	-0.008	-0.283***
	(-1.76)	(-0.06)	(-4.05)	(-1.77)	(-0.05)	(-4.06)
employee	2.155*	1.562	-4.370***	2.138	1.579	-4.373***
	(1.65)	(0.93)	(-7.13)	(1.63)	(0.93)	(-7.13)
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	21,178	21,178	21,178	21,178	21,178	21,178
Adjusted R-squared	0.260	0.384	0.282	0.260	0.384	0.282

Table 3.6: Incorporating credit rating group fixed effects

This table shows OLS regression results incorporating industry-CRgroup-year fixed effects. This specification allows for a comparison of the debt structure choice of subsidy-receiving firms to that of non-subsidized firms within the same industry and credit ratings group in a given year, after additionally accounting for time-varying firm-level covariates. Industry is defined using the Fama and French 49-industry classification, while the credit ratings group is based on S&P credit ratings data. Given the availability of S&P credit ratings data, the sample period for these analyses is only up to 2014. Debt structure variables are measured in year t+1, while all independent variables are measured in year t. All continuous firm-level control variables that enter regressions are winsorized at the 1st and 99th percentile. t-statistics based on standard errors clustered at firm level are reported in parentheses below the coefficient estimates. *, **, *** represent statistical significance at the 10%, 5%, and 1% level, respectively. Detailed definitions and calculations of the variables are shown in Table C.1 of Appendix C.

VARIABLES	(1) BankDebt	(2) PublicDebt	(3) BankDebt	(4) PublicDebt
subsidy_dummy	-0.039***	0.041***		
	(-3.33)	(3.52)		
subsidy_amt			-0.003***	0.003***
			(-3.63)	(3.82)
size	-0.020***	0.053***	-0.020***	0.052***
	(-4.86)	(13.55)	(-4.78)	(13.44)
leverage	0.432***	0.613***	0.431***	0.614***
	(11.60)	(17.24)	(11.59)	(17.26)
asset_growth	0.034***	0.000	0.034***	-0.000
	(3.68)	(0.00)	(3.69)	(-0.01)
MB	-0.005	-0.022***	-0.005	-0.022***
	(-1.28)	(-6.53)	(-1.31)	(-6.49)
tangibility	-0.071**	0.011	-0.070**	0.010
	(-2.04)	(0.35)	(-2.02)	(0.32)
ROA	0.017	-0.160***	0.016	-0.159***
	(0.57)	(-6.15)	(0.55)	(-6.12)
cfvol	-0.997***	0.626***	-0.991***	0.620***
	(-4.20)	(2.78)	(-4.17)	(2.75)
cash	-0.639***	0.054**	-0.639***	0.054**
	(-20.41)	(2.12)	(-20.41)	(2.12)
RnD	0.082	-0.089	0.081	-0.088
	(1.15)	(-1.44)	(1.13)	(-1.43)
employee	3.239***	-1.078	3.230***	-1.069
	(3.60)	(-1.28)	(3.59)	(-1.27)
Ind x CRgroup x Year FE	Yes	Yes	Yes	Yes
Observations	16,389	16,389	16,389	16,389
Adjusted R-squared	0.244	0.419	0.244	0.419

Table 3.7: Instrumental Variable (IV) analysis

This table presents the results from IV-2SLS estimation conducted in a similar spirit to Cohen et al. (2011), in which the variable of interest, *subsidy_amt*, is instrumented on a dummy variable *COMMITTEE*. *COMMITTEE* is set to one if the senator of a firm's state first assumes the role of chairman or the ranking minority member of one of the five top powerful congressional committees (i.e., Finance, Veterans' Affairs, Appropriation, Rules, and Armed Services). All regression specifications include industry and year fixed effects, where the industry is defined under Fama and French 49-industry classification. Debt structure variables are measured in year t+1, while all independent variables are measured in year t. All continuous firm-level control variables that enter regressions are winsorized at the 1st and 99th percentile. t-statistics based on standard errors clustered at firm level are reported in parentheses below the coefficient estimates. *, **, *** represent statistical significance at the 10%, 5%, and 1% level, respectively. Detailed definitions and calculations of the variables are shown in Table C.1 of Appendix C.

VARIABLES	(1) subsidy_amt	(2) BankDebt	(3) PublicDebt	(4) leverage(t+1)
IV_subsidy_amt		-0.056**	0.043**	-0.005
		(-2.30)	(2.09)	(-0.58)
COMMITTEE	0.599***			
	(3.83)			
size	1.373***	0.016	0.038	0.023*
	(34.15)	(0.47)	(1.33)	(1.79)
leverage	-0.365	0.270***	0.658***	
	(-1.07)	(7.48)	(20.34)	
asset_growth	-0.189**	0.048***	-0.013	0.041***
	(-2.24)	(4.83)	(-1.44)	(9.33)
MB	-0.405***	-0.014	-0.019**	-0.014***
	(-11.27)	(-1.36)	(-2.09)	(-3.58)
tangibility	-0.034	-0.042	-0.003	0.108***
	(-0.09)	(-1.18)	(-0.09)	(6.20)
ROA	-1.690***	-0.024	-0.153***	-0.226***
	(-7.01)	(-0.48)	(-3.62)	(-11.65)
cfvol	10.147***	-0.876***	0.560*	-0.420***
	(5.17)	(-2.65)	(1.94)	(-2.93)
cash	-1.258***	-0.660***	0.082**	-0.300***
	(-4.39)	(-15.27)	(2.26)	(-18.65)
RnD	-0.818	0.111	-0.142**	-0.086***
	(-1.41)	(1.62)	(-2.43)	(-2.88)
employee	21.576***	4.240***	-1.369	-2.981***
	(2.60)	(3.74)	(-1.35)	(-6.36)
Industry FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	21,178	21,178	21,178	21,178
Adjusted R-squared	0.311	0.132	0.169	0.180

Table 3.8: Cross-sectional tests – The effect of corporate governance

This table reports the results of cross-sectional tests examining the impact of the quality of corporate governance on the relationship between government subsidies and firms' choices of debt structure. In Panel A, the moderating variable is the firm-level hostile takeover susceptibility index by Cain et al. (2017). In Panel B, the moderating variable is institutional ownership. Unless explicitly stated otherwise, the high group refers to the top 40% of firms based on the pertinent moderating variable, and the low group denotes the bottom 40%. All regression specifications include industry and year fixed effects, where the industry is defined under Fama and French 49-industry. Debt structure variables are measured in year t+1, while all independent variables are measured in year t. All continuous firm-level control variables that enter regressions are winsorized at the 1st and 99th percentile. t-statistics based on standard errors clustered at firm level are reported in parentheses below the coefficient estimates. *, **, *** represent statistical significance at the 10%, 5%, and 1% level, respectively. Detailed definitions and calculations of the variables are shown in Table C.1 of Appendix C.

Panel A. Firm-level hostile takeover susceptibility index

	(1)	(2)	(3)	(4)
VARIABLES	BankDebt	PublicDebt	BankDebt	PublicDebt
subsidy_dummy * high_governance	-0.061**	0.066**		
	(-1.97)	(2.26)		
subsidy_amt * high_governance			-0.005**	0.005**
			(-2.09)	(2.40)
high_governance	-0.050***	0.050***	-0.050***	0.050***
	(-3.48)	(4.09)	(-3.45)	(4.07)
subsidy_dummy	-0.017	0.024		
	(-0.58)	(0.90)		
subsidy_amt			-0.002	0.002
			(-0.81)	(1.21)
size	-0.054***	0.088***	-0.053***	0.087***
	(-15.19)	(27.22)	(-14.43)	(26.05)
leverage	0.212***	0.707***	0.210***	0.708***
	(5.60)	(19.57)	(5.56)	(19.63)
asset_growth	0.040***	-0.010	0.040***	-0.009
	(3.85)	(-0.92)	(3.84)	(-0.91)
MB	0.007*	-0.033***	0.006	-0.033***
	(1.67)	(-8.91)	(1.51)	(-8.74)
tangibility	0.005	-0.043	0.005	-0.043
	(0.14)	(-1.21)	(0.13)	(-1.21)
ROA	0.083**	-0.237***	0.080**	-0.234***
	(2.50)	(-7.91)	(2.40)	(-7.80)
cfvol	-1.464***	1.119***	-1.439***	1.093***
	(-5.22)	(4.31)	(-5.13)	(4.22)
cash	-0.616***	0.040	-0.617***	0.040
	(-17.77)	(1.38)	(-17.81)	(1.40)
RnD	0.128	-0.112	0.125	-0.109
	(1.55)	(-1.60)	(1.52)	(-1.56)
employee	1.819*	0.116	1.807*	0.131
	(1.84)	(0.11)	(1.83)	(0.13)
Industry FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	12,277	12,277	12,277	12,277
Adjusted R-squared	0.213	0.395	0.214	0.396

Table 3.8 (continued)

Panel B. Institutional ownership

VARIABLES	(1) BankDebt	(2) PublicDebt	(3) BankDebt	(4) PublicDebt
subsidy_dummy * high_InstOwn	-0.051***	0.058***		
	(-2.73)	(3.14)		
subsidy_amt * high_InstOwn			-0.004***	0.005***
			(-2.77)	(3.34)
high_InstOwn	0.064***	-0.079***	0.063***	-0.079***
	(5.34)	(-7.54)	(5.26)	(-7.49)
subsidy_dummy	-0.044***	0.045***		
	(-2.82)	(2.73)		
subsidy_amt			-0.004***	0.004***
			(-3.48)	(3.15)
size	-0.058***	0.097***	-0.056***	0.096***
	(-17.81)	(31.83)	(-17.04)	(30.72)
leverage	0.239***	0.662***	0.237***	0.664***
	(7.45)	(20.95)	(7.39)	(21.03)
asset_growth	0.059***	-0.026***	0.059***	-0.026***
	(6.82)	(-3.06)	(6.82)	(-3.05)
MB	0.007**	-0.036***	0.007*	-0.036***
	(2.14)	(-11.12)	(1.96)	(-10.92)
tangibility	-0.055	0.004	-0.054	0.003
	(-1.64)	(0.13)	(-1.61)	(0.10)
ROA	0.060**	-0.211***	0.057**	-0.208***
	(2.12)	(-7.99)	(2.02)	(-7.89)
cfvol	-1.377***	0.788***	-1.359***	0.768***
	(-6.40)	(3.88)	(-6.32)	(3.79)
cash	-0.595***	0.058**	-0.595***	0.058**
	(-19.10)	(2.11)	(-19.12)	(2.12)
RnD	0.207***	-0.183***	0.204***	-0.180***
	(2.96)	(-2.88)	(2.92)	(-2.84)
employee	3.415***	-0.531	3.410***	-0.521
	(3.42)	(-0.54)	(3.41)	(-0.53)
Industry FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	17,348	17,348	17,348	17,348
Adjusted R-squared	0.202	0.383	0.203	0.384

Table 3.9: Cross-sectional tests – The effect of expected certification benefit

This table presents the results of cross-sectional tests examining the impact of the expected degree of certification benefit on the relationship between government subsidies and firms' choices of debt structure. In Panel A, the moderating variable is stock price informativeness, measured by price nonsynchronicity. In Panel B, the moderating variable is firm-level political sentiment by Hassan et al. (2019). Unless explicitly stated otherwise, the high group refers to the top 40% of firms based on the pertinent moderating variable, and the low group denotes the bottom 40%. All regression specifications include industry and year fixed effects, where the industry is defined under Fama and French 49-industry. Debt structure variables are measured in year t+1, while all independent variables are measured in year t. All continuous firm-level control variables that enter regressions are winsorized at the 1st and 99th percentile. t-statistics based on standard errors clustered at firm level are reported in parentheses below the coefficient estimates. *, **, *** represent statistical significance at the 10%, 5%, and 1% level, respectively. Detailed definitions and calculations of the variables are shown in Table C.1 of Appendix C.

Panel A. Stock price informativeness (price nonsynchronicity)

	(1)	(2)	(3)	(4)
VARIABLES	BankDebt	PublicDebt	BankDebt	PublicDebt
subsidy_dummy * low_SPI	-0.052**	0.077***		
	(-2.33)	(3.78)		
subsidy_amt * low_SPI			-0.004**	0.006***
			(-2.44)	(3.69)
low_SPI	-0.000	-0.018	-0.002	-0.014
	(-0.01)	(-1.28)	(-0.13)	(-1.03)
subsidy_dummy	-0.022	0.015		
	(-1.03)	(0.77)		
subsidy_amt			-0.002	0.002
			(-1.34)	(1.21)
size	-0.065***	0.103***	-0.064***	0.101***
	(-15.58)	(27.21)	(-14.77)	(25.97)
leverage	0.342***	0.598***	0.340***	0.600***
	(8.27)	(15.50)	(8.23)	(15.56)
asset_growth	0.088***	-0.031***	0.087***	-0.030***
	(7.65)	(-2.91)	(7.62)	(-2.87)
MB	0.014***	-0.043***	0.013***	-0.042***
	(3.05)	(-10.66)	(2.89)	(-10.49)
tangibility	-0.050	0.032	-0.049	0.031
	(-1.19)	(0.86)	(-1.17)	(0.83)
ROA	0.054	-0.241***	0.051	-0.238***
	(1.35)	(-7.27)	(1.28)	(-7.20)
cfvol	-1.405***	0.760***	-1.391***	0.746***
	(-4.73)	(3.12)	(-4.69)	(3.06)
cash	-0.608***	0.058*	-0.608***	0.058*
	(-14.89)	(1.86)	(-14.91)	(1.86)
RnD	0.168*	-0.186***	0.166*	-0.184***
	(1.84)	(-2.64)	(1.82)	(-2.62)
employee	4.728***	-0.952	4.728***	-0.946
	(3.95)	(-0.78)	(3.95)	(-0.77)
Industry FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	12,384	12,384	12,384	12,384
Adjusted R-squared	0.233	0.433	0.233	0.433

Table 3.9 (continued)

Panel B. Firm-level political sentiment

VARIABLES	(1) BankDebt	(2) PublicDebt	(3) BankDebt	(4) PublicDebt
subsidy_dummy * high_PSentiment	-0.032**	0.037**		
	(-2.03)	(2.29)		
subsidy_amt * high_PSentiment			-0.002**	0.003**
			(-1.97)	(2.45)
high_PSentiment	0.004	-0.005	0.004	-0.005
	(0.39)	(-0.49)	(0.35)	(-0.53)
subsidy_dummy	-0.034**	0.034**		
	(-2.36)	(2.27)		
subsidy_amt			-0.003***	0.003***
			(-2.80)	(2.62)
size	-0.075***	0.116***	-0.074***	0.114***
	(-19.31)	(31.24)	(-18.54)	(29.97)
leverage	0.164***	0.691***	0.162***	0.693***
	(4.45)	(18.95)	(4.39)	(19.02)
asset_growth	0.104***	-0.068***	0.104***	-0.068***
	(8.25)	(-5.96)	(8.25)	(-5.95)
MB	0.020***	-0.050***	0.020***	-0.050***
	(4.03)	(-10.89)	(3.92)	(-10.74)
tangibility	-0.080**	0.003	-0.079**	0.002
	(-2.14)	(0.09)	(-2.11)	(0.05)
ROA	0.029	-0.284***	0.025	-0.280***
	(0.65)	(-6.99)	(0.57)	(-6.90)
cfvol	-1.325***	0.669**	-1.313***	0.655**
	(-4.03)	(2.14)	(-3.99)	(2.10)
cash	-0.698***	0.160***	-0.698***	0.160***
	(-18.38)	(4.42)	(-18.38)	(4.43)
RnD	0.035	-0.209**	0.034	-0.208**
	(0.32)	(-2.14)	(0.31)	(-2.13)
employee	1.990*	0.818	1.976*	0.833
	(1.71)	(0.64)	(1.69)	(0.65)
Industry FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	11,705	11,705	11,705	11,705
Adjusted R-squared	0.224	0.409	0.224	0.410

Appendices

Appendix A: Chapter 1

Table A.1: Variable definition

Variable	Definition and Calculation
total_debt	Firm leverage: (dlc + dltt) / at
secdebt_asset	Secured debt to asset ratio: dm / at
nonsecdebt_asset	Nonsecured debt to asset ratio: (dlc + dltt - dm) / at
secdebt_debt	Secured debt ratio: dm / (dlc + dltt)
NewNetDebt	Dummy variable that equals one if a firm issues new debt, regardless of secured or unsecured, that is equal to or more than 1% of lagged book assets for a given year and zero otherwise
NewSecured	Dummy variable that equals one if a firm issues new secured debt that is equal to or more than 1% of lagged book assets for a given year and zero otherwise
NewUnsecured	Dummy variable that equals one if a firm issues new unsecured debt that is equal to or more than 1% of lagged book assets for a given year and zero otherwise
size	Natural log of total book assets: ln(at)
Tobins_Q	Tobin's Q: (at + csho*prcc_f- ceq - txdb) / (0.9*at + 0.1*(at + csho*prcc_f- ceq - txdb))
tangibility	Tangibility: ppent / at
profitability	Profitability: ni / at
cash	Cash holdings: che / at
cf_vol	Cash flow volatility: sd_oancf / at, where sd_oancf is the standard deviation of oancf over five years

Appendix B: Chapter 2

Table B.1: Variable definition

Variable	Definition and Calculation
Debt_Heterogeneity	A continuous measure of debt heterogeneity calculated using Capital IQ Debt Structure data. $$Debt\_Heterogeneity_{it} = 1 - \left(\frac{SS_{it} - \frac{1}{7}}{1 - \frac{1}{7}} \right)$$ $$SS_{it} = \left(\frac{CP_{it}}{TD_{it}}\right)^2 + \left(\frac{DC_{it}}{TD_{it}}\right)^2 + \left(\frac{TL_{it}}{TD_{it}}\right)^2 + \left(\frac{SBN_{it}}{TD_{it}}\right)^2 + \left(\frac{SUB_{it}}{TD_{it}}\right)^2 + \left(\frac{CL_{it}}{TD_{it}}\right)^2 + \left(\frac{Other_{it}}{TD_{it}}\right)^2$$
Num_Debt_Types	A count measure of debt heterogeneity calculated using Capital IQ Debt Structure data. When counting the number of debt types, I consider only debt types that account for at least five percent of a firm's total debt.
NONSYNC	The average of quarterly price nonsynchronicity over the previous four quarters (i.e., q-4 to q-1), where $Price\ Nonsynchronicity = \ln\left(\frac{1-R^2}{R^2}\right)$. R^2 is estimated from the regression $R_{it} = \alpha_i + \beta_{1i}R_{m,t} + \beta_{2i}R_{ind,t} + u_{it}$, where R_{it} is stock i's return on day *t*, $R_{m,t}$ is CRSP value-weighted market return on day *t*, and $R_{ind,t}$ is the return on the Fama and French 48-industry portfolio to which firm *i* belongs on day *t*. I estimate the above regression for a quarter interval on a rolling basis, requiring a firm-quarter to have a minimum of 30 trading days (given about 63 trading days in a quarter). Calculated *NONSYNC*s are matched to other firm-level variables at fiscal year t with corresponding calendar quarter q.
PIN	The average of quarterly PINs over the previous four quarters (i.e., q-4 to q-1). Calculated *PIN*s are matched to other firm-level variables at fiscal year t with corresponding calendar quarter q. I obtain quarterly PINs from Stephen Brown's website(https://terpconnect.umd.edu/~stephenb/EKOpins.html).
StkFrag	The average of the square root of quarterly stock price fragility over the previous four quarters (i.e., q-4 to q-1). Stock price fragility of stock i at time t is defined as $fragility_{it} = \left(\frac{1}{\theta_{it}}\right)^2 W_{it}'\Omega_t W_{it}$, where θ_{it} is the market capitalization of the firm's stock, W_{it} is a vector of each mutual fund's portfolio allocation weight to stock i, and Ω_t is the variance-covariance matrix dollar fund flows. I calculate quarterly stock price fragility, closely following Greenwood and Thesmar (2011), using Thomson Reuters S12 database of 13F filings, CRSP mutual fund file, and MFLINKS prepared by WRDS. Calculated *StkFrag*s are matched to other firm-level variables at fiscal year t with corresponding calendar quarter q.
size	Natural log of total book assets: ln(at)
Tobins_Q	Tobin's Q: (at + csho*prcc_f- ceq - txdb) / (0.9*at + 0.1*(at + csho*prcc_f- ceq - txdb))
tangibility	Tangibility: ppent / at
profitability	Profitability: oibdp / at
cf_vol	Cash flow volatility: sd_oibdpq / at, where sd_oibdpq is the standard deviation of oibdpq over twelve quarters
R&D	R&D expenditure: xrd / at (I fill out zeros for missing xrd)
dividend_payer	Dividend payer dummy: one if dvc > 0 and zero otherwise
leverage	Book leverage: (dlc + dltt) / at

Table B.2: PSM analysis – Post-matching differences in firm characteristics

This table presents the means and standard errors of firm-level characteristics and debt variables after the propensity score matching (PSM), separately for the matched treated and control firms. In the PSM process, the treated firms in the original BlackRock-BGI merger analysis sample are matched with control firms that also come from the original sample. The chosen matching methodology is 1-to-N matching with replacement based on the calculated propensity scores. p-values of t-tests of differences in mean values of the two groups are presented in the last column.

	treated		control			
	Mean	SE	Mean	SE	Diff	p-value
Debt_Heterogeneity	0.304	0.019	0.272	0.017	0.032	0.218
leverage	0.234	0.012	0.236	0.011	-0.002	0.926
size	7.776	0.118	6.740	0.066	**1.036**	**0.000*****
Tobins_Q	1.820	0.043	1.731	0.045	0.089	0.161
tangibility	0.293	0.017	0.284	0.015	0.009	0.700
profitability	0.142	0.009	0.126	0.008	0.016	0.185
cf_vol	0.012	0.001	0.014	0.001	-0.001	0.293
R&D	0.029	0.004	0.033	0.005	-0.004	0.559
dividend_payer	0.497	0.037	0.403	0.032	0.094	**0.057***

Appendix C: Chapter 3

Table C.1: Variable definition

Variable	Definition and Calculation
BankDebt	The ratio of bank debt to total debt in year t+1. Bank debt is the sum of term loans and revolving credit (from Capital IQ). Total debt is calculated as dlc + dltt (from Compustat).
PublicDebt	The ratio of public debt to total debt in year t+1. Public debt is the sum of senior bonds and notes, subordinated bonds and notes, and commercial paper (from Capital IQ). Total debt is calculated as dlc + dltt (from Compustat).
subsidy_dummy	A dummy variable that equals one when a firm receives any types of government subsidies in year t, and zero otherwise (Source: Subsidy Tracker).
subsidy_amt	The natural logarithm of one plus the aggregate dollar amount of government subsidies a firm receives in year t (Source: Subsidy Tracker).
size	Firm size as the natural logarithm of firm market value: ln(MV), where MV = csho*prcc_f
leverage	Book leverage: (dlc + dltt) / at
asset_growth	Asset growth rate: the growth rate of book assets over the previous year
MB	Market to book ratio: (at - ceq + MV) / at, where MV = csho*prcc_f
tangibility	Tangibility: ppent / at
ROA	Return on asset: ib / at
cfvol	Cash flow volatility: sd_oibdpq / at, where sd_oibdpq is the standard deviation of oibdpq over twelve quarters
cash	Cash: che / at
R&D	R&D expenditure: xrd / at (I fill out zeros for missing xrd)
employee	Employee: emp / at

Table C.2: Propensity score matching – Covariate balance

This table presents the means and standard errors of firm-level characteristic variables after Propensity Score Matching (PSM), shown separately for the matched subsidy and non-subsidy firm-years. The PSM process employs nearest-neighbor matching without replacement, with a caliper choice of 0.01. Firm-years receiving subsidies are matched with non-subsidized firm-years, restricting the selection of control firms to those that have never received subsidies throughout the sample period. p-values of t-tests of differences in mean values of the two groups are presented in the last column. Detailed definitions of the variables are shown in Table C.1.

	Subsidized firm-years (N = 2106)		Matched unsubsidized firm-years (N = 2106)		Difference in mean	p-value for difference
	Mean	Std Err	Mean	Std Err		
size	7.239	0.029	7.185	0.032	0.053	0.217
leverage	0.216	0.004	0.216	0.004	0.000	0.995
asset growth	0.135	0.007	0.144	0.007	-0.008	0.416
MB	2.021	0.030	1.993	0.025	0.028	0.474
tangibility	0.264	0.005	0.259	0.006	0.005	0.474
ROA	0.028	0.003	0.021	0.003	0.006	0.115
cfvol	0.014	0.000	0.014	0.000	0.000	0.686
cash	0.169	0.004	0.174	0.004	-0.006	0.306
R&D	0.039	0.002	0.042	0.002	-0.003	0.204
employee	0.006	0.000	0.005	0.000	0.000	0.152

Table C.3: Entropy balancing – Weighting balance

This table presents the means and variances of firm-level characteristics for subsidized and non-subsidized firm-years, both before and after the application of entropy balancing weights. The entropy balancing approach is employed to ensure balance across all observed covariates between the treatment (subsidized) and control (non-subsidized) groups. Detailed definitions of the variables are shown in Table C.1.

	Before weighting				After weighting			
	subsidy_dummy = 1		subsidy_dummy = 0		subsidy_dummy = 1		subsidy_dummy = 0	
	Mean	Variance	Mean	Variance	Mean	Variance	Mean	Variance
size	8.350	2.928	5.816	3.980	8.350	2.928	8.350	3.232
leverage	0.239	0.025	0.181	0.035	0.239	0.025	0.239	0.029
asset growth	0.117	0.088	0.141	0.152	0.117	0.088	0.117	0.082
MB	1.994	1.321	2.120	2.581	1.994	1.321	1.994	1.102
tangibility	0.259	0.040	0.233	0.057	0.259	0.040	0.259	0.060
ROA	0.048	0.009	-0.044	0.061	0.048	0.009	0.048	0.009
cfvol	0.012	0.000	0.022	0.001	0.012	0.000	0.012	0.000
cash	0.139	0.023	0.238	0.062	0.139	0.023	0.139	0.022
R&D	0.030	0.004	0.062	0.015	0.030	0.004	0.030	0.004
employee	0.005	0.000	0.006	0.000	0.005	0.000	0.005	0.000

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Curriculum Vitae

Kyuyoung Oh

Education

University of Nebraska-Lincoln — May 2017
B.S., Business Administration (Finance)
Chancellor's Scholar

Keimyung University — Aug 2017
B.S., International Business (Dual Degree Program)

Fellowships, Honors, and Awards

SFA Best Paper Award in Corporate Finance, Winner	2023
FMA Best Paper Award in Corporate Finance, Semi-finalist	2023
Nomination for Gatton Teaching Excellence Award	2022
Gatton Graduate Assistantship	2018 - Present
Block Funding Award	2020 - 2022
Luckett Fellowship	2019 - 2020
Gatton Fellowship	2018 - 2020