THREE ESSAYS IN COPORATE FINANCE

by

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A dissertation submitted to the faculty of
The University of North Carolina at Charlotte
in partial fulfillment of the requirements
for the degree of Doctor of Philosophy in
Business Administration

Charlotte

2019

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ABSTRACT

SHUANGSHUANG JI. Three Essays in Corporate Finance. (Under the direction of DR. DAVID C. MAUER)

This dissertation contains three chapters. In the first chapter, we empirically examine how diversification influences the relation between corporate governance and capital structure. We find better corporate governance in focused firms increases leverage, while better corporate governance in diversified firms decreases leverage. Further, the negative relation between leverage and corporate governance in diversified firms is stronger the larger is the diversification discount. Our results are robust when we correct for selection bias, account for the joint endogeneity of leverage, diversification, and governance using a system GMM estimator, and conduct a natural experiment focusing on exogenous shocks to corporate governance. The evidence suggests that the conflict in the literature about the relation between managerial entrenchment and financial leverage is because earlier studies do not condition on the diversification status of firms. Entrenched managers in focused firms eschew leverage, whereas entrenched managers in diversified firms take advantage of their better access to debt finance and use more financial leverage.

The literature finds empirical evidence that the human capital costs is a crucial factor for the capital structure decision. To further address the importance of the human capital costs on the corporate policies. In the second chapter, we study the relation between a firm's human capital costs and investment policy. We argue and show in the model that employees demand higher pay to compensate for the additional unemployment risk borne by a firm's investment riskiness. Empirically, we find a

significantly positive relation between investment riskiness and average employee pay, and the effect is more pronounced for employees in non-technology firms. We further investigate four channels by which investment riskiness influences human capital costs: corporate diversification, R&D expenditures, advertising expenditures, and total value of acquisitions. Consistently, we find that the average employee compensation is significantly lower in more diversified firms, and in firms that invest less in R&D, advertisement, and acquisition activity. Lastly, we test the feedback effect of human capital costs on investment policy. The findings suggest that labor intensive firms, on average, are more diversified firms, and invest less in R&D, advertisement, and acquisition activity. Our results are robust when we account for the joint endogeneity of investment riskiness, employee pay and leverage using a system GMM estimator, and conduct a natural experiment focusing on exogenous shocks to outside employment opportunities in manufacturing industries.

In the third chapter, I investigate the influence of bank mergers on lending relationship. In a large sample of US bank mergers, I track borrowers of acquirer and target banks from pre-merger to post-merger and examine how the merger affects loan spreads, credit availability and other non-price loan terms. Relationship borrowers, on average, enjoy lower interest rates and more favorable non-price loan terms compared to non-relationship borrowers. However, these benefits are significantly reduced post-merger. Specifically, compared to non-relationship borrowers, the merged bank charges relationship borrowers higher loan spreads and reduces the loan amount post-merger. The effect is more pronounced in mergers when a large bank acquires a small target. Moreover, the results are different across the borrower of acquiring bank and target bank.

Although relationship borrowers of the target bank are more negatively affected compared to other relationship borrowers in terms of higher loan spreads, we find that they benefit from less restrictive loan contract and larger loan availability after the bank merger.

DEDICATIONS

To my parents, who gave me a fabulous life, who initiated me ways to solve problems, who supported me always and never blame me on my failure, and who told me to be a happy girl rather than a successful woman.

To my lovely boyfriend, who be with me to come over challenges on this tough journey.

ACKNOWLEGEMENTS

I deeply appreciate my mentor and dissertation chair David C. Mauer for his valuable support, warm encourage, guidance, and patience. I also truly appreciate my mentor and co-author Yilei Zhang for helpful instructions and meticulous care. I am honored to complete my degree under their supervision.

I am thankful to other committee members Tao-Hsien Dolly King and Rob McGregor for all the support. I am grateful to my co-author Xinxin Li for constant encourage and hard work. I greatly appreciate faculties in the Belk Business School for valuable suggestions both on research and teaching. Especially, I am thankful to Weidong Tian, Ethan Chiang, Gene Lai, Yongqiang Chu and Yufeng Han for the time they spend on improving my work and personality. Finally, I am thankful to my cohorts for constant courage and long-time relationship.

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INTRODUCTION

This dissertation discusses three topics in the empirical corporate finance. The first part of the dissertation investigates how the diversification status influences the relation between firm leverage and managerial entrenchment. Previous literature fails to reach a consensus and we point out that diversification should influence the relation between governance and leverage. Empirically, diversified firms have higher leverage or lower cost of capital than comparable portfolios of stand-alone firms. In addition to a co-insurance effect, we argue that there is an agency channel in diversified firm that should also enhance the debt capacity. However, since the agency channel requires that managers are insulated from shareholders and market for corporate control, this channel further predicts that the excess leverage of diversified firms should decrease as governance improves. The reason of conflicts in the literature is because previous studies do not condition on diversification status of the firm. The second part of the dissertation studies the relation between human capital costs and investment policy. Rational employees will ask for a premium on wages or benefits to compensate the additional unemployment risk resulting from higher investment riskiness. Empirically, we find a consistent positive relation investment riskiness and average employee pay. Our results emphasizes the importance of the human capital costs in the literature on determinants to corporate policies. The last part of the dissertation investigates the effect of bank mergers on relationship lending. Banks, as delegated monitors, can collect valuable information about borrowers as part of their lending relationship. However, proprietary information that banks collected may also give them an information monopoly which allows bank to charge higher interests rates. I find that merged banks will excise their market power

gained from long-term lending relationship to extract more benefits from relationship borrowers after mergers, particularly, when market power of the merged bank increases sufficiently (i.e., the outside lending options significantly decreases to borrowers). Moreover, the effect of bank mergers on lending relationship is different across borrowers. The results emphasize the point made by Berger and Udell (2002) that it is important to study the effect of relationship lending in the context of organization structure.

Chapter 1
Managerial Entrenchment and Capital Structure: The Effect of Diversification

1.1 Introduction

The finance literature has conflicting views and empirical evidence on the relation between corporate governance and capital structure. Classic models of the agency problem of free cash flow (Jensen, 1986; Stulz, 1990; Hart and Moore, 1995) show that greater use of debt reduces agency costs, since it constrains managers from pursuing inefficient investment. When left to their discretion, however, entrenched managers may prefer less debt to reduce firm risk to protect their human capital (Fama, 1980; Amihud and Lev, 1981), or because of a dislike of performance pressures associated with commitments to disgorge cash (Jensen, 1986; Morellec, 2004).[1] This *managerial entrenchment hypothesis* predicts a negative relation between managerial entrenchment and leverage and that leverage will increase as governance improves (i.e., greater shareholder rights and more exposure to the disciplining influence of the market for corporate control). This hypothesis, which is the conventional view in the literature, has received some empirical support.[2]

In contrast, some theory suggests that managers of entrenched firms may want higher leverage and may have better access to debt finance because their incentives are more aligned with those of creditors. There are three main arguments in support of this *creditor alignment hypothesis*. First, debt financing helps avoid dilution of control rights and reduces the likelihood of a takeover attempt (Harris and Raviv, 1988; Stulz, 1988).

[1] By definition, entrenched managers are protected from the market for corporate control and are unwilling to voluntarily increase the firm's leverage to decrease their own discretion. Several papers have shown, particularly, Harris and Raviv (1988), Stulz (1988), and Zwiebel (1996), that a takeover threat can induce a manager to increase leverage.

[2] For example, see Agrawal and Mandelker (1987), Friend and Lang (1988), Mehran (1992), Jung et al. (1996), Berger et al. (1997), and Garvey and Hanka (1999).

Second, as shown by Lambrecht and Myers (2008), firms with lower investor protection have limited access to equity financing and therefore rely more heavily on debt financing. They argue that firms with weak governance use debt as a commitment device to minimize the agency cost of equity. Third, John and Litov (2010) argue that less equity-oriented managers have incentives that are more naturally aligned with creditors and this allows for greater access to debt markets. This *creditor alignment hypothesis* predicts that entrenched managers will choose higher leverage and leverage will decreases as governance improves. There is a growing body of empirical evidence that supports this hypothesis.[3]

The managerial entrenchment hypothesis is primarily a demand side theory about the reluctance of managers to take on debt financing. On the other hand, the creditor alignment hypothesis primarily relies on the willingness of creditors to supply debt, combined with management's willingness to accommodate. Since the managerial entrenchment and creditor alignment hypotheses make starkly different predictions about the relation between leverage and governance, it is surprising that there is no consensus on the relation in the empirical literature.[4] In this paper, we examine how a firm's diversification status influences the relation between managerial entrenchment and leverage. Diversification reduces the risk of a manager's undiversified portfolio. Therefore,

[3] Nielsen (2006), John and Litov (2010), Renneboog and Smulders (2014), and Zhao and Zou (2015) show that firms with weak shareholder rights, as proxied by the Gompers, Ishii, and Metrick (2003) G-index, have higher leverage ratios. Wald and Long (2007) and John and Litov (2010) also find that leverage increases after the adoption of state antitakeover amendments. Klock et al. (2005) and Chava et al. (2009) find that yields on bonds and loans, respectively, decrease as the G-index increases. In related work, Cremers et al. (2007) find that greater shareholder control decreases bond yields if the firm is protected from takeover risk as proxied by an index of three anti-takeover provisions. Ortiz-Molina (2006) finds that greater alignment between managers and shareholders increases the cost of debt. Finally, Ashbaugh-Skaife et al. (2006) and John and Litov (2010) find that bondholder credit ratings are higher when the firm is protected from takeovers.

[4] One explanation could be related to differences in empirical measures (e.g., for corporate governance). Another explanation could be that the relation between capital structure and governance has changed over time and therefore studies using different sample periods find different results.

managers in diversified firms are less concerned about the financial risk associated with debt and entrenched managers are more likely to pursue larger debt capacity provided by creditor alignment. Managers in focused firms, however, do not have the diversification status as a shield. Hence, entrenched managers in focused firms are more inclined to insure against downside risk by avoiding debt. Since all previous studies examine the relation between entrenchment and leverage in mixed samples of diversified and focused firms, it is perhaps not surprising that empirical results vary across samples.

To test whether diversification status influences the relation between capital structure and corporate governance, we construct a panel dataset of U.S. firms over the period 1998 to 2014. We use the Compustat Business Segment database to identify focused (single-segment) and diversified (multi-segment) firms. Based on empirical results in Hoechle et al. (2012) and Morellec et al. (2012), we select governance measures from ISS (formerly RiskMetrics), CDA/Spectrum, and ExecuComp that are significant determinants of firm value and policy decisions. The governance measures include the E-index of antitakeover provisions (Bebchuk et al., 2009), institutional ownership, CEO ownership, and an indicator of whether the CEO is powerful (i.e., chairman, president, and only insider on the board). We use these governance variables to construct equally-weighted and percentile-weighted governance indices, so that higher index values represent more equity alignment (less entrenchment).[5]

Since our empirical tests focus on how diversification affects the relation between corporate governance and leverage, we measure a firm's leverage net of the leverage of a comparable portfolio of focused firms. Following Berger and Ofek (1995), Ahn et al.

[5] As discussed in Section 1.4, our results are robust to alternative measures of governance, leverage, and diversification.

(2006), and Kuppuswamy and Villalonga (2016), this adjusted leverage is computed as the difference between a firm's actual leverage ratio and its imputed leverage ratio, where the imputed leverage ratio is computed as the segment asset-weighted average of median leverage ratios of single segment firms in each of the firm's segments' industries. In our baseline specification, we regress the adjusted leverage ratio (hereafter adjusted leverage) on diversification status (indicator for multi-segment firm), the governance index, the interaction of diversification status and governance index, a set of controls, and fixed effects. As discussed in the next section, the coefficient on diversification status measures the combined influence of "co-insurance" and creditor alignment on the leverage of diversified firms relative to focused firms.[6] The coefficient on governance measures the marginal effect of better governance (less entrenchment) on the leverage of focused firms. The coefficient on the interaction of diversification status and governance measures the marginal effect of better governance on the leverage of diversified versus focused firms.

We find significantly positive coefficients on diversification status and governance, and a significantly negative coefficient on their interaction. This indicates that the incremental effect of better governance increases the leverage of focused firms and decreases the leverage of diversified firms. These effects are economically significant. For example, a one-unit increase in the equally weighted governance index (reflecting a *decrease* in managerial entrenchment) decreases adjusted leverage of diversified firms by 19% of the sample mean and increases adjusted leverage of focused firms by 29% of the

[6] The term co-insurance, originally coined by Lewellen (1971), refers to the hypothesized positive influence of diversification on leverage through a reduction in default risk driven by imperfect correlation among a conglomerate's portfolio of businesses. As Kuppuswamy and Villalonga (2016) note, the precise way to test the co-insurance hypothesis—and for our purposes the effect of managerial entrenchment on leverage in diversified firms—is to compare a firm's actual leverage to the leverage of a comparable portfolio of same-industry single-segment firms.

sample mean. Thus, we find strong support for the creditor alignment hypothesis in diversified firms and strong support for the managerial entrenchment hypothesis in focused firms. These contrasting effects of corporate governance on leverage help explain why the literature finds mixed support for the effect of managerial entrenchment on leverage in samples containing both diversified and focused firms.[7]

The creditor alignment and managerial entrenchment hypotheses are both premised on agency problems. Specifically, entrenched managers in diversified firms have better access to debt finance because their incentives are more aligned with creditors than with shareholders and entrenched managers in focused firms avoid debt finance based on self-interest. Thus, as a further check on the agency channel through which governance influences leverage decisions, we compute Berger and Ofek (1995) excess value measures and separate the sample into firm-years with above- and below-median excess value.[8] This experiment is motivated by the empirical results of Hoechle et al. (2012), who estimate that 16-21% of the Berger and Ofek (1995) diversification discount is explained by poor corporate governance.[9] We find the effects of governance on the leverage of diversified

[7] In unreported regressions, we find positive coefficients on our governance indices in adjusted leverage regressions that do not condition on diversification status, which supports the conclusion that managerial entrenchment (lower governance index) decreases leverage. Since 35% of the sample is diversified, the positive coefficient on governance—and thereby support for the managerial entrenchment hypothesis—is tilted in favor of the 65% of the sample that is focused.

[8] Berger and Ofek (1995) excess value measures the degree to which a diversified (or focused) firm trades at a discount or premium to a comparable portfolio of focused firms in the same industry.

[9] Beginning with Lang and Stulz (1994) and Berger and Ofek (1995), a large literature documents that diversified firms have lower values than comparable portfolios of specialized firms. Campa and Kedia (2002) and Villalonga (2004b) argue that discounts are biased because the decision to diversify is endogenous and there are unobserved factors that influence firm diversification and value. However, Laeven and Levin (2007), Schmid and Walter (2009), Ammann et al. (2012), and Hoechle et al. (2012) find a robust diversification discount after accounting for endogeneity and omitted variable bias. Many other explanations for the discount have been put forward in the literature. For example, poor data quality in Compustat (Villalonga, 2004a) or poor benchmarking in the Berger and Ofek measure (Hund et al., 2017). However, our analysis does not depend on the existence of a diversification discount, since we use the Berger and Ofek (1995) excess value measure only as a metric to separate the sample into firms that are more or less likely to have governance problems.

and focused firms are much stronger in the below-median excess value group, where a lack of shareholder rights and therefore a larger value discount provide the necessary underpinnings for both the managerial entrenchment and creditor alignment hypotheses. Furthermore, we show that our results are robust to using a variety of different measures of leverage, corporate governance, and diversification.

A major concern with the analysis is the joint endogeneity of leverage, diversification, and governance. Further, addressing endogeneity is potentially more challenging in our case, since at least one of our variables is a binary choice variable (e.g., diversification status) and we model the interaction between endogenous variables (diversification status × governance). We use two estimation strategies to address these problems. First, we use a modified Heckman selection model developed by Chang et al (2016) to account for multiple endogenous variables and their interaction. Second, we use a three-step instrumental variables method (Adams et al., 2009; Angrist and Pischke, 2009; Wooldridge, 2010). Although each method is not without limitations, we continue to find robust evidence that entrenched managers in focused firms avoid financial leverage while entrenched managers in diversified firms choose higher financial leverage than non-entrenched managers. Overall, our results show that it is necessary to condition on diversification status to examine the relation between capital structure and corporate governance.

The remainder of the paper is organized as follows. Section 1.2 discusses hypotheses and empirical strategy. Section 1.3 describes the sample. Section 1.4 presents empirical results. Section 1.5 reports estimations accounting for the joint endogeneity of

leverage, diversification, and governance. Section 1.6 concludes. The Appendix provides variable definitions.

1.2. Hypotheses and baseline empirical specification

In this section, we discuss hypotheses for the relation between leverage and corporate governance and explain how diversification influences this relation. We then describe our baseline regression specification.

1.2.1. Managerial entrenchment and creditor alignment hypotheses

The conventional thinking in the literature is that entrenched managers will choose low leverage to minimize performance pressure to meet debt obligations and to protect their private benefits of control that would likely be lost in bankruptcy. Mehran (1992), Berger et al. (1997), and Garvey and Hanka (1999), among others, empirically document that more entrenched CEOs manage firms with less leverage. This *managerial entrenchment hypothesis* predicts that entrenched managers choose low leverage, and by extension, leverage will increase as governance improves (e.g., fewer antitakeover amendments).

However, there are at least three reasons to suggest that firms with poor governance have more debt and thereby, all else being equal, have higher leverage than firms with good governance. First, as argued by Harris and Raviv (1988) and Stulz (1988), managers may forgo equity finance in favor of debt finance to influence the distribution of voting rights to preserve their private benefits of control. Second, Lambrecht and Myers (2008) posit that conflicts between shareholders and managers that engender high agency costs of equity lead to less equity financing and more reliance on debt financing. Lastly, entrenched managers who pursue safe projects and/or build empires to protect and diversify their human capital have interests more aligned with creditors than with equity holders, which allows for greater access to debt finance. Among others, Nielsen (2006) and John and Litov

(2010) find empirically that firms with bad governance (many antitakeover amendments) use more leverage than firms with good governance (few antitakeover amendments). This *creditor alignment hypothesis* predicts that entrenched managers take advantage of their better access to debt finance and use more leverage. Importantly, it therefore also predicts that leverage will decreases as governance improves.

Since the managerial entrenchment and creditor alignment hypotheses make opposite predictions about the relation between leverage and governance, it is unclear why the empirical literature testing this relation has been unable to reach a consensus. Some likely culprits for the diverse results include differences in time periods and/or samples, and differences in variables used to measure leverage and corporate governance. We argue in this paper, however, that firm diversification status should influence the relation between leverage and governance, and that estimating the relation between leverage and governance without conditioning on diversification can produce almost any result.

The empirical literature finds that diversified firms have higher leverage relative to comparable portfolios of stand-alone firms (see, e.g., Berger and Ofek, 1995; Kuppuswamy and Villalonga, 2016). Lewellen (1971) attributes this additional debt capacity to a co-insurance effect, where the combination of diverse businesses under one corporate umbrella decreases variability of cash flows and thereby default risk. Hann et al. (2013) find strong support for the co-insurance effect, showing that diversified firms have, on average, a lower cost of capital than comparable portfolios of focused firms. They further find that the reduction in the cost of capital is larger when a diversified firm's segments have lower cash flow correlation.

There is an agency problem in diversified firms that also encourages greater use of leverage. The source of the additional debt capacity stems from an alignment of manager and creditor interests which encourages diversification and enhances access to debt finance. Amihud and Lev (1981) argue that risk-averse and/or under-diversified managers have a strong incentive to pursue (possibly value-reducing) diversification strategies (e.g., pure conglomerate mergers) to diversify their employment risk. Subsequent authors, including Jensen (1986), Stulz (1990), and Aggarwal and Samwick (2003) argue that managers insulated from the market for corporate control will maximize their private benefits by building large diversified empires of businesses. These self-interested incentives of managers to diversify to reduce risk naturally align their incentives with creditors. Importantly, Denis et al (1997) and Hoechle et al. (2012) directly link this agency motive to diversify to poor corporate governance by showing, respectively, that value-reducing diversification strategies are positively related to several indicators of poor corporate governance (e.g., low managerial and institutional share ownership) and that 16-21% of the estimated diversification discount can be explained by poor corporate governance.[10] Overall, this agency channel linking firm diversification to poor corporate governance establishes that the interests of entrenched managers in diversified firms will be more aligned with creditors than with shareholders. This in turn should allow these managers greater access to debt finance, which according to the creditor alignment hypothesis, predicts higher leverage in diversified firms than in focused firms.

Empirically, both co-insurance and agency channels predict a positive relation between leverage and diversification. However, since the agency channel requires that

[10] Denis et al. (1997) further show that diversification decreases when corporate governance improves resulting from external corporate control threats, financial distress, or management turnover.

managers are insulated from shareholders and the market for corporate control (i.e., entrenched), the agency channel further predicts that a diversified firm's access to credit should decrease as corporate governance improves and the incentives of managers become more aligned with shareholders. Thus, in accord with the creditor alignment hypothesis, leverage in diversified firms should decrease as governance improves.

The relation between leverage and governance in focused firms will likely depend on the incentives of entrenched managers to avoid debt. Unlike in diversified firms, where a manager's human capital is protected by diversification, entrenched managers in focused firms should be more inclined to insure against downside risk by avoiding debt. Consistent with the managerial entrenchment hypothesis, this suggests that leverage is lower in poorly governed focused firms and that leverage increases as governance improves.

1.2.2. Baseline empirical specification

Using our full sample of focused and diversified firms, we estimate the following baseline panel regression:

$$Lev_{it} = \alpha_t + \alpha_j + \beta_1 Div_{it} + \beta_2 Gov_{it} + \beta_3 Div_{it} \times Gov_{it} + \gamma' X_{it} + \varepsilon_{it} \quad (1)$$

where i indexes firms, t indexes time, j indexes industry, Lev_{it} is industry-adjusted leverage, Div_{it} is diversification, Gov_{it} is governance, α_t and α_j are year and industry fixed effects, and X_{it} is a vector of firm characteristics. In this specification, we focus on the signs and interpretation of the parameters, β_1, β_2, and β_3.

To illustrate, for simplicity assume that Div_{it} and Gov_{it} are indicator variables, such that $Div_{it} = 1$ for diversified firms and zero for focused firms, and $Gov_{it} = 1$ for firms with good corporate governance and zero for firms with bad corporate governance. Further, let subscript D (F) denote diversified (focused) and superscript G (B) denote good

(bad) corporate governance. Thus, letting Lev_D^G (Lev_f^G) and Lev_D^B (Lev_f^B) denote, respectively, expected leverage conditional on the firm being diversified (focused) with good corporate governance and expected leverage conditional on the firm being diversified (focused) with bad corporate governance, we can interpret the coefficients β_1, β_2, and β_3 in equation (1) as follows:

$\beta_1 = Lev_D^B - Lev_F^B$ measures the difference in expected leverage between diversified and focused firms with bad corporate governance.[11] Economically, β_1 reflects both the co-insurance effect of diversification and the influence of poor corporate governance (i.e., the agency channel) on the leverage of diversified firms relative to focused firms. The co-insurance and creditor alignment hypotheses both predict that $\beta_1 > 0$.

$\beta_2 = Lev_F^G - Lev_F^B$ measures the difference in expected leverage for focused firms with good and bad corporate governance. The managerial entrenchment hypothesis predicts that $\beta_2 > 0$.

$\beta_3 = (Lev_D^G - Lev_F^G) - (Lev_D^B - Lev_F^B)$ measures the difference in the differences of expected leverage between diversified and focused firms under good and bad corporate governance. Each leverage difference ($(Lev_D^G - Lev_F^G)$ and $(Lev_D^B - Lev_F^B)$) reflects a co-insurance component and a component attributable to good (first difference) or bad (second difference) corporate governance. Assuming the co-insurance components are the same

[11] Referring to equation (1), expected leverage conditional on the firm being diversified and having bad corporate governance is $E(Lev_{it}|Div_{it} = 1 \text{ and } Gov_{it} = 0) = \alpha_t + \alpha_j + \beta_1 + \gamma' X_{it}$, and expected leverage conditional on the firm being focused and having bad corporate governance is $E(Lev_{it}|Div_{it} = 0 \text{ and } Gov_{it} = 0) = \alpha_t + \alpha_j + \gamma' X_{it}$. The difference in expected leverage between diversified and focused firms with bad corporate governance is $Lev_D^B - Lev_F^B \equiv E(Lev_{it}|Div_{it} = 1 \text{ and } Gov_{it} = 0) - E(Lev_{it}|Lev_{it} = 0 \text{ and } Gov_{it} = 0) = \beta_1$. The coefficients β_2 and β_3 can be derived similarly.

under good and bad corporate governance, the coefficient β_3 measures the effect of good governance (relative to bad governance) on the expected leverage of diversified firms (relative to focused firms). Under the creditor alignment hypothesis, good governance negates the alignment of manager and creditor interests and thereby has a negative effect on leverage, i.e., $\beta_3 < 0$.

We also regress Lev on Gov in subsamples of diversified and focused firms. These regressions allow for different sensitivities of diversified and focused firms to firm characteristics (i.e., the vector of γ' coefficients in equation (1)). In diversified firm regressions, the creditor alignment hypothesis predicts a negative coefficient on Gov. In focused firm regressions, the managerial entrenchment hypothesis predicts a positive coefficient on Gov.

1.3. Sample and variable description

We start with all firms having data on both the Compustat Fundamentals Annual and Compustat Business Segment data files. The sample begins in 1998 to avoid changes in segment reporting rules that potentially invalidate comparisons of the number of reported segments before and after this date.[12] Following Berger and Ofek (1995), we exclude firm-years in which at least one segment is classified as being in the financial sector (SIC codes 6000-6999), total sales are less than $20 million, or the sum of segment sales is not within 1% of consolidated firm totals.[13] We further exclude American Depository Receipts (ADRs), firm-years that are incorporated outside the U.S., and firm-years with any segments classified as regulated utilities (SIC codes 4900-4999). A firm-year is classified as diversified if it has more than one business segment with different four-digit SIC codes; otherwise the firm is classified as focused.

Following Berger and Ofek (1995), Ahn et al. (2006), and Kuppuswamy and Villalonga (2016), we use industry-adjusted leverage as our primary leverage variable. For multi-segment firms, industry-adjusted leverage is the difference between a firm's actual leverage ratio and its imputed leverage ratio, where the imputed leverage ratio is the asset-weighted average of its segments' imputed leverage ratios. A segment's imputed leverage ratio in a year is the median leverage ratio of single-segment firms in the same industry and year based on three-digit SIC codes.[14] The computation is

[12] In June 1997, the Financial Accounting Standards Board (FASB) issued Statement of Financial Accounting Standards (SFAS) to replace FASB 14 for reporting information about operating segments, which is effective for fiscal years commencing after December 15, 1997. Therefore, we start our sample from 1998 to make sure our results are not influenced by the change in segment reporting standards.

[13] If the deviation of the sum of segment sales is within 1% of consolidated firm totals, we adjust each segment's sales up or down by the percentage deviation.

[14] If there are fewer than 5 single-segment firms in a segment's three-digit SIC code in a year, we use two-digit SIC code to define a segment's industry.

$$Adjusted\ Leverage = Leverage - \sum_{S=1}^{N} \frac{Asset_S}{\sum_{S=1}^{N} Asset_S} \times Medium(Leverage_S) \quad (2)$$

where S indexes segments, N is the total number of segments, and a firm's actual leverage ratio ($Leverage$) and the leverage ratios of single-segment firms ($Leverage_S$) used to compute the firm's imputed leverage ratio are computed as the ratio of total debt (long-term debt plus debt in current liabilities) to book value of total assets. For single-segment firms, $Adjusted\ Leverage$ in a year is simply the firm's leverage ratio minus its industry median leverage ratio in the year.

We also compute an adjusted *net* leverage ratio by subtracting cash and marketable securities from total debt in the computation of a firm's actual leverage ratio and the leverage ratios of single-segment firms used to compute the firm's imputed leverage ratio. This cash adjustment is motivated by the practical rule of thumb that cash may be viewed as negative debt and the finding in Duchin (2010) that diversified firms hold smaller cash balances than focused firms.[15] We report results below using adjusted leverage and adjust net leverage. In unreported results, we also compute adjusted leverage ratios using market leverage defined as total debt divided by the market value of assets (estimated as the book value of assets plus the difference between the market and book values of equity). The results reported below are similar if we use market leverage ratios instead of book leverage ratios.

[15] We recognize that building cash balances and using excess cash to reduce debt may not be equivalent strategies when firms face financing constraints, and therefore it may not be appropriate to treat cash as negative debt (see, e.g., Acharya et al., 2007). Nevertheless, the adjusted net leverage ratio can at least account for the different cash balances documented in diversified and focused firms.

For robustness, we also use the leverage and net (of cash) leverage ratios, and we alternatively measure leverage using the interest coverage ratio and the industry adjusted interest coverage ratio. The interest coverage ratio is the ratio of earnings before interest, taxes, depreciation, and amortization (EBITDA) to interest expense. The adjusted interest coverage ratio is the difference between a firm's interest coverage ratio and its imputed ratio, where the imputed ratio is the sales-weighted average of its segments' imputed ratios. A segment's imputed ratio is the median interest coverage ratio of single-segment firms in the same industry and year.

We would like a measure of diversification's effect on firm value to sharpen our tests of how diversification influences the relation between leverage and governance. This will allow us to focus on more heavily discount firms where, as shown by Hoechle et al. (2012), diversification is more likely the product of self-interested entrenched managers. Following Berger and Ofek (1995), we measure the value of diversification as the excess value of a diversified firm relative to a portfolio of industry-matched single-segment firms. We compute excess value as the logarithm of the ratio of the firm's actual market value to its imputed value. A firm's actual market value is the sum of the total book value of debt and market value of equity. A firm's imputed value is the sum of the imputed values of its segments, with each segment's imputed value equal to the segment's sales multiplied by its industry median ratio of total capitalization (books debt plus market value of equity) to sales. Industry median ratios are computed using only single-segment firms and are based on the narrowest SIC code grouping that yields at least five single-segment firms with data to compute the ratio. Following the literature, we exclude firm-years where a firm's actual value is more than four times or less than one-fourth imputed value.

There is a long list of governance variables that have been used in the literature to explain firm performance and policy decisions. Hoechle et al. (2012) use 14 governance variables in their baseline analysis and identify four that are reliable predictors of Berger and Ofek (1995) excess value. These include institutional stock ownership, CEO stock ownership, an indicator variable for whether the CEO is powerful, and the Gompers et al. (2003) count variable for antitakeover defenses (G-index). From a different perspective, Morellec et al. (2012) develop a dynamic tradeoff model of capital structure to examine the importance of manager-shareholder conflicts on leverage choice. They use the model's predictions for the statistical moments of leverage to determine the magnitude of manager-shareholder agency costs that best explains observed financing patterns. Of relevance for our search for reliable measures of corporate governance, they document that institutional stock ownership, managerial stock ownership, the Bebchuk et al. (2009) E-index of six antitakeover provisions (a subset of the G-index), and CEO power have the largest impact on agency costs and hence leverage decisions.

To capture the effect of governance on firms' capital structure decisions, we create a parsimonious governance index based on the four governance measures shown in Hoechle at al. (2012) and Morellec et al. (2012) to be the most reliable predictors of the value loss from diversification, manager-shareholder agency costs, and leverage. The governance measures in our index include the proportion of shares owned by institutional investors, the proportion of shares owned by the CEO, a dummy variable equal to one for powerful CEO, and the Bebchuk et al. (2009) count of six antitakeover provisions (E-index). A CEO is classified as powerful if she is the only insider on the board and serves as chairman and president. Our choice of E-index rather than the broader G-index is

primarily motivated by data availability. From 1990 to 2006, both indices are available for 1990, 1993, 1995, 1998, 2000, 2002, 2004, and 2006. However, the E-index is also available each year from 2007 and the G-index stops updating after 2006.[16] We obtain CEO ownership from ExecuComp, institutional ownership from CDA/Spectrum, and we construct the powerful CEO dummy and the E-index using data from ISS (formerly RiskMetrics).

We use these four governance measures to construct an equally-weighted and percentile-weighted index of good corporate governance. The equally-weighted index is the sum in a firm-year of zero/one indicator variables for the four governance measures. The E-index indicator variable is equal to one if a firm-year has a below median E-index for the year, and zero otherwise. The institutional and CEO ownership indicator variables, respectively, are equal to one if a firm-year has above median institutional and CEO ownership for the year, and zero otherwise. Lastly, the CEO power indicator variable is equal to one if the CEO is *not* powerful, and zero otherwise. The percentile-weighted index is computed as the sum in a firm-year of the percentile rankings of the four governance measures, which is then scaled to vary between zero and one. We first transform the powerful CEO dummy and E-index by computing (1 – powerful CEO) and (6 – E-index), respectively, so that higher values indicate better governance. We then compute firms' percentile rankings for institutional ownership, CEO ownership, (1 – powerful CEO), and (6 – E-index) by sample year and compute the percentile-weighted governance index as

[16] Prior to 2007, we follow the literature and assume that a firm's E-index is unchanged between reporting dates.

the simple average of the percentile rankings across the four governance measures.[17] In addition to our baseline governance indices, we also examine and discuss the robustness of our results to alternative measures of corporate governance.

We require firm-years to have complete data for industry-adjusted book leverage, excess value, governance variables, and all control variables in the baseline leverage regression to be included in the sample. Our final sample has 6,873 firm-year observations for 1,191 firms over 1998 to 2014.[18] Panel A in Table 1 reports mean and median values for our variables for the full sample and the diversified (multi-segment) and focused (single segment) subsamples. Diversified firms account for 35% of sample firm-years, with an average (median) of 2.73 (2) segments. Mean and median leverage, net leverage, and adjusted net leverage are significantly larger in diversified firms than in focused firms. Correspondingly, the interest coverage ratio and adjusted interest coverage ratio are significantly lower in diversified firms.

Consistent with the agency perspective that entrenched managers pursue diversification strategies, both the equally-weighted and the percentile-weighted good governance indices are significantly lower in diversified firms than focused firms. Further, the governance components of the indices all point in the direction that diversified firms have significantly worse corporate governance. Finally, we see that diversified firms are larger, have fewer growth opportunities, invest less in R&D, have lower risk as measured

[17] We assume the percentile ranking when (1 – powerful CEO) = 1 (i.e., powerful CEO = 0) is 0.50 and the percentile ranking when (1 – powerful CEO) = 0 (i.e., powerful CEO = 1) is zero. The results reported below are robust if we use 0.75/0 or 1/0 instead of 0.50/0.

[18] Requiring available governance variables significantly reduces our sample size as compared with other papers on diversification (without such a requirement). However, our sample size is comparable to that in Hoechle et al. (2012) who also require governance variables.

by cash flow volatility and proportion of investment grade bond ratings, and hold smaller cash balances than focused firms.

Panel B in Table 1 reports correlations between key variables. Leverage (interest coverage) variables are generally positively (negatively) related to diversification status and negatively (positively) related to our good governance indices, suggesting that entrenched managers use more debt. Consistent with Hoechle et al. (2012), we also see that better governed firms have larger Berger and Ofek (1995) excess value.

1.4. Empirical results

In this section, we first report univariate comparisons of key variables. We then report the results of regression analysis of the influence of diversification on the relation between leverage and corporate governance, followed by robustness checks on the sensitivities of the results to alternative measures of diversification, leverage, and corporate governance.

1.4.1. Univariate comparisons

Table 2 reports means of variables for the full sample and the diversified and focused subsamples grouped by good and bad corporate governance. A firm-year observation has good (bad) corporate governance if the equally-weighted governance index is above (below) the median index for the year.[19] For diversified firms, we see that leverage and industry-adjusted leverage are significantly larger, and the interest coverage ratio and industry adjust coverage ratio are significantly smaller in the bad corporate governance subsample. Consistent with agency motives for diversification where entrenched managers pursue empire building and/or lower risk investments to protect the value of their human capital, we see that poorly governed diversified firms are significantly larger, have more fixed assets and capital expenditures, and have lower cash flow volatility than better governed firms. For focused firms, however, adjusted leverage and interest coverage ratios are not significantly different between good and bad corporate governance groups.

We also see that poorly governed diversified and focused firms have lower cash balances. This is consistent with the "spending hypothesis" of Harford et al. (2008), which

[19] The results reported in Table 2 are similar if instead we group firms into good and bad governance subsamples using the percentile-weighted governance index.

predicts that weakly controlled managers choose to spend rather than stockpile cash. Coincidentally, poorly governed firms are more likely to pay dividends, which can also help explain the lower cash balances in these firms. Lastly, observe that poorly governed firms are roughly twice as likely to have an investment grade rating. This is noteworthy because it suggests that firms with poor corporate governance have better access to debt financing than firms with good governance, which is consistent with the creditor alignment hypothesis.

1.4.2. Baseline leverage regressions

Table 3 reports panel regressions of leverage on governance using the specifications discussed in Section 1.2.2. The dependent variable in columns (1)-(3) is adjusted leverage and the dependent variable in columns (4)-(6) is adjusted net leverage. Panel A uses the equally weighted (EW) governance index and Panel B uses the percentile-weighted (PW) governance index. Columns (1) and (4) use the full sample and report coefficient estimates on a dummy variable for multi-segment firms, *Diversified*, the governance index, *Governance*, and their interaction, *Diversified* × *Governance*. Columns (2) and (5) and columns (3) and (6) report regressions of leverage on governance in subsamples of diversified and focused firms, respectively. All regressions include year dummies, industry dummies based on Fama-French 49 industries, and the firm characteristics used in the leverage regressions of Coles et al. (2006) and Kuppuswamy and Villalonga (2016). Additionally, we include dummy variables for whether the firm has an investment grade or speculative grade S&P bond rating, where the left-out group is firm-years without a bond rating. Lastly, to control for the effect of M&A activity on capital structure, we include a dummy variable equal to one if the ratio of acquisition expenditures to book value of total

assets in the current year and/or previous year is greater than 5%.[20] The *t*-statistics reported in parentheses below parameter estimates are computed using robust standard errors corrected for clustering of observations at the firm level.

The coefficients on *Diversified* in columns (1) and (4) of both panels are significantly positive. Thus, consistent with the co-insurance *and* creditor alignment hypotheses, diversified firms borrow more than comparable portfolios of focused firms. The magnitude of this effect is economically significant. Using the coefficient on *Diversified* in Panel A of column (1), diversified firms' adjusted leverage is 3.2% greater than that of comparable single-segment firms in bad governance firms (with governance index = 0). The coefficients on *Governance* in columns (1) and (4) of both panels are also significantly positive. As the coefficient on *Governance* in the full sample regression captures the effect of better governance on the leverage of single segment firms, a positive coefficient on *Governance* supports the managerial entrenchment hypothesis for focused firms. That is, better governance mitigates the incentives of managers in focused firms to choose low leverage. Column (1) shows a one-unit increase in the governance index corresponds to a 0.9% increase of adjusted leverage in focused firms, which is 29% of the mean adjusted leverage ratio in the sample. The interaction of diversification status and governance, *Diversified* × *Governance*, captures the effect of better governance on the excess leverage of diversified relative to focused firms. The coefficients on the interaction term are significantly negative in columns (1) and (4) of both panels. Column (1) of Panel A shows that a one-unit increase in the governance index corresponds to a 0.6% (0.9% – 1.5%) decrease of adjusted leverage in diversified relative to focused firms, which is 19%

[20] We discuss whether acquisition activity can explain the positive relation between diversification and leverage in poorly governed firms and provide a test of this alternative explanation for our results below.

of the mean adjusted leverage ratio. Thus, consistent with the creditor alignment hypothesis, diversified firms decrease leverage as governance improves.

The different effects of better governance on the leverage decisions of diversified and focused firms can be seen directly in the subsample regressions in columns (2) and (3) for adjusted leverage and columns (5) and (6) for adjusted net leverage. As seen there, leverage is significantly decreasing in better governance for diversified firms and significantly increasing in better governance for focused firms. These effects are economically significant. Using the regressions in columns (2) and (3) in Panel A, a one-unit increase in the governance index decreases adjusted leverage by 31% (–0.009/0.029) of its mean in diversified firms and increases adjusted leverage by 37.5% (0.012/0.032) of its mean in focused firms. Overall, we find strong support for the creditor alignment hypothesis in diversified firms and the managerial entrenchment hypothesis in focused firms.

A possible alternative explanation of our findings is that poorly governed firms may use debt to fund value-destroying diversifying acquisitions, thereby generating a positive relation between leverage and diversification in poorly governed firms. If entrenched managers are otherwise adverse to using debt financing (e.g., as reflected in the debt policies of focused-firm managers), this could explain the different effects of governance on leverage in diversified and focused firms that we see in our sample.[21] To address this alternative explanation, Table 4 reports leverage regressions after excluding 1,965 (842 diversified and 1,123 focused) firm-years with acquisition expenditures to book assets in

[21] We thank an anonymous referee for suggesting this alternative explanation for our results.

the current and/or previous year greater than 5%.[22] We continue to find that better governance decreases leverage in diversified firms and increases leverage in focused firms. Thus, it is unlikely that our results are driven by entrenched managers pursuing debt-financed value-destroying diversifying acquisitions.

In unreported full sample leverage regressions that do not control for diversification and the interaction of diversification with governance, we find a positive and marginally significant (at the 10 percent level) coefficient on governance. Based only on this evidence, we would incorrectly conclude that our data support the managerial entrenchment hypothesis, when in fact poorly-governed diversified firms use more leverage than well-governed diversified firms. Overall, the different effects of corporate governance on leverage in diversified and focused firms help explain why the literature finds mixed support for the effect of managerial entrenchment on leverage in samples containing a mixture of diversified and focused firms.

1.4.3. Conditioning on the diversification discount

As discussed in Section 1.2, a prominent explanation for the diversification discount is agency problems resulting from a lack of alignment between managers and shareholders. Using the magnitude of the discount as a proxy for the costs resulting from these agency problems, we examine whether the effects of governance on leverage are more pronounced in firms with greater discounts. We use the Berger and Ofek (1995)

[22] As a robustness check, we merge our sample with the SDC M&A database and identify firm-years with major M&A activity as those engaged in mergers with a total deal value to the market value of the firm's equity greater than 1% (and alternatively 5%) in the current and/or previous year. Excluding these firm-year observations, we find similar results to those reported in Table 4. Results are available upon request.

excess value measure to estimate the effect of diversification on firm value.[23] For diversified firms, excess value is the discount or premium of actual value relative to what the firm would be worth if its business segments were standalone firms. For focused firms, excess value is the discount or premium of actual value to industry median firm value. We compute the median excess value each year for diversified firms and the median excess value each year for focused firms, and separate diversified and focused firms into above and below median excess value groups using their respective medians. We then place the above median excess value diversified and focused firms into one sample and the below median excess value diversified and focused firms into another sample.

Table 5 reports regressions of adjusted leverage and adjusted net leverage on diversification, governance, and their interaction for the above and below median excess value samples. Columns (1) to (4) report results using the equally-weighted governance index and columns (5) to (8) report results using the percentile-weighted governance index. Each regression has control variables (not reported) and industry and year fixed effects. We use a Chow test to assess whether the coefficients on *Diversified*, *Governance*, and *Diversified* × *Governance* are statistically different in the above and below median excess value subsamples. The numbers in square brackets in the below median excess value columns are *p*-values from Chow tests under the null hypothesis that the coefficients are equal.

The results show that the effects of governance on the leverage of diversified and focused firms are much stronger in the below-median excess value group, where poor governance and thereby lower excess value provide the necessary underpinnings for both

[23] See Section 1.3 and/or the Appendix for details on the construction of the Berger and Ofek (1995) excess value measure.

the creditor alignment and managerial entrenchment hypotheses. Using the estimates in column (3), a one-unit increase in the equally-weighted governance index in the below median excess value sample increases adjusted leverage for focused firms by 47% of its mean and decreases adjusted leverage for diversified firms by 18% of its mean.[24] The corresponding effects in the above median excess value sample reported in column (1) are statistically and economically insignificant. Further, according to Chow tests, the coefficient estimates on *Governance* and *Diversified* × *Governance* in columns (1) and (3) are significantly different. Similarly strong differences can be seen across above and below median excess value comparisons in columns (2) and (4), (5) and (7), and (6) and (8).

As a robustness check, we partition the sample based on excess value greater than or less than zero. Consistent with the results reported in Table 5, we find that the effects of governance on leverage are much stronger (or only present) in the negative excess value sample. These results are available upon request.

1.4.4. Alternative measures of diversification, leverage, and governance

We first examine the robustness of our results to alternative measures of diversification. Table 6 reports regressions of adjusted leverage and adjusted net leverage on diversification, governance, and the interaction of diversification and governance replacing the diversification dummy variable (*Diversified*) with *Number of segments* – 1 (Panel A) and 1 – *Herfindahl index* (Panel B). We subtract one from number of segments so that this variable is zero for focused firms and increasing in the number of segments for diversified firms. We compute the Herfindahl index as $\sum_{i=1}^{n} S_i^2$, where n is the number of

[24] Using the estimates in column (3), the calculations are 0.016/0.034 and (0.016 – 0.022)/0.034, respectively, where 0.034 is the mean adjusted leverage ratio for the below median excess value subsample.

segments, and S_i is segment i's sales to total firm sales. Since the Herfindahl index ranges from zero when the firm has many segments to one when the firm has only one segment, we use the variable 1 – *Herfindahl index* so that the measure is zero for focused firms and increasing in the degree of diversification otherwise. In each panel, we report full sample results and split-sample results based on above and below median excess value.

The results are highly robust to using these alternative diversification measures. We find positive coefficients on the count (Panel A) and continuous (Panel B) diversification measures, indicating that the additional debt capacity due to coinsurance and creditor alignment is increasing in the intensity of diversification. Furthermore, the positive coefficients on the governance indices indicate that better governance (i.e., greater manager-shareholder alignment) mitigates the negative effect of managerial entrenchment on the leverage of focused firms. The negative coefficients on the interactions of the intensity of diversification and the governance indices indicate that better governance offsets the additional debt capacity in diversified firms due to creditor alignment. Lastly, all the above effects are only significant in the below-median excess value subsamples.

We next explore alternative measures of leverage. First, we examine whether our results are sensitive to using industry-adjusted leverage. Panel A in Table 7 reports our baseline specification using an unadjusted leverage ratio and an unadjusted net leverage ratio. Although results are insignificant in the full sample and the above median excess value subsample, we continue to find that good corporate governance has a positive effect on the leverage of focused firms and a negative effect on the leverage of diversified firms in the below median excess value subsample. Second, we alternatively measure leverage using the interest coverage ratio and the adjusted interest coverage ratio. Consistent with

our leverage ratio results, Panel B in Table 7 shows that good corporate governance has a negative effect on the interest coverage ratio of focused firms and a positive effect on the interest coverage ratio of diversified firms, with the effects significant in the below median excess value subsample regressions.

Of potential concern, except for interest coverage, all reported leverage results are based on book leverage measures. The reason, as emphasized by Welch (2004) and Coles et al. (2006), is that market leverage measures may change passively due to changes in stock prices, and so may not reflect managerial decisions. Notwithstanding, all our results are robust to using market leverage measures. To save space, results are not tabulated but are available upon request.

Lastly, we consider alternative governance measures to assess whether our results are robust to how we measure governance. Table 8 reports baseline adjusted leverage and adjusted net leverage regressions using the E-index to measure governance. The E-index is one component of our governance indices, measuring how well a firm is insulated from the market for corporate control and thereby the degree to which a manager is entrenched (Bebchuk et al., 2009). We use the variable 6 – *E-index* so that a higher value indicates less entrenchment. As seen in the table, the results using this alternative governance measure confirm our conclusion that better corporate governance increases leverage in focused firms and decreases leverage in diversified firms.

We also use the 14 governance variables in Hoechle et al. (2012) to implement a principal component analysis as in Larcker et al. (2007) to identify the principal

components that explain the variance-covariance structure of the governance variables.[25] Using the first five principal components that cumulatively explain 90% of total variance, we compute the correlations between the principal components and the original 14 governance variables to identify the five governance variables with the highest correlations. The governance variables include: E-index, institutional ownership, proportion of independent board members, board size, and an indicator variable for powerful CEO. We use these five governance variables to construct equally-weighted and percentile-weighted governance indices. In unreported results, we find that our leverage regression results are robust to the use of these alternative indices. We also construct governance indices using the three governance variables used to construct the percentile-weighted governance index in Dittmar and Duchin (2016)—E-index, proportion of independent board members, and blockholder ownership—and find similarly robust results. It is worth noting that all our robust governance index measures have E-index as a component. The principal components analysis also shows that E-index is the most important governance measure among the 14 governance measures in Hoechle et al. (2012).

1.4.5. Additional tests

All our specifications control for industry fixed effects based on Fama-French 49 industries. However, the literature suggests that firm fixed effects are also important in explaining leverage (see, e.g., Lemmon et al., 2008). Therefore, we examine whether our results are robust to firm fixed effects. When firm fixed effects replace industry fixed

[25] Hoechle et al. (2012) use 14 of their 15 governance variables in baseline regressions because one variable—fraction of directors whose tenure predates the CEO—is missing for a significant fraction of the sample. We use the 14 governance variables in their baseline regressions in our principal component analysis.

effects in our regression models, the coefficients on governance, diversification, and their interaction are no longer statistically significant. The reason, as discussed and shown in Zhou (2001) and Cain et al. (2017), among others, is that both governance and diversification status are persistent and slow-moving. Thus, firm fixed effects wash out the primarily cross-sectional variation that we seek to explain (i.e., how the leverage of diversified and focused firms varies in the cross-section by corporate governance).

Notwithstanding, we examine the sensitivity of our results to finer as well as coarser industry fixed effects. Table 9 reports adjusted leverage regressions with industry fixed effects based on three-digit SIC codes, two-digit SIC codes, and Fama-French 30 industries.[26] As seen there, our results hold for different industry fixed effects.

Finally, we examine whether the relations between leverage and governance in diversified and focused firms are affected by the governance reforms enacted in the 2002 Sarbanes-Oxley Act (SOX) or by economic downturns. Using a difference-in-differences specification, we find no significant change in the relations between leverage and governance in diversified and focused firms in the post-SOX time period. We also include interactions of the governance indices with macro variables to investigate whether the leverage-governance relations in diversified and focused firms vary by state of the economy.[27] We find no evidence that the negative (positive) relation between leverage and better governance for diversified (focused) firms varies by state of the economy (i.e., the coefficients on the interactions of governance indices with economic downturn variables are not significantly different from zero). Overall, the influence of diversification on the

[26] In our sample, there are 207 different three-digit SIC codes and 53 different two-digit SIC codes.
[27] The macro variables include dummy variables for the 2001 NBER-defined recession and the 2007-2009 financial crisis, the growth rate in real GDP, and the VIX index.

relation between leverage and governance does not appear to be sensitive to Sarbanes-Oxley governance changes or to variation in the state of the economy.

1.5. Endogeneity of leverage, diversification, and governance

Campa and Kedia (2002) and Villalonga (2004b) argue that the decision to diversify is endogenous and show empirically that factors motivating firms to diversify are negatively correlated with firm value. In this way, the decision to diversify may not be causal to the diversification discount. In our analysis, there is a similar concern about the documented relations between diversification and leverage. For example, if the decision to diversify is endogenous, then characteristics that drive firms to diversify may be correlated with firm leverage. Furthermore, research by, for example, Hoechle et al. (2012) and Wintoki et al. (2012), shows that it is often prudent, and in some cases necessary to account for the endogeneity of corporate governance. In this section, we use a modified Heckman self-selection model and an instrumental variables (IV) method to account for the endogenous selection and the joint endogeneity, respectively, of leverage, diversification, and governance. We first discuss the two methods and then present the estimation results.

1.5.1. Modified Heckman selection model

In the estimation of equation (1), a potential concern is that diversification and governance are not random decisions by firms, and therefore the effects we observe are at least partially attributable to selection bias. If a firm's choices are correlated with its leverage policy, the error term in our regression model will be correlated with the firm's decision to diversify, choice of governance, and their interaction, inducing bias in our coefficient estimates. We use a modified Heckman (1979) two-stage procedure developed by Campa and Kedia (2002) and extended by Chang et al. (2016) to account for interactions of endogenous choice variables to mitigate this potential selection bias.

We start by modeling the firm's choices of governance and diversification as dummy variables. Thus, we define *GOV* as a dummy variable equal to one if a firm-year's governance index is above the sample median for that year and zero otherwise, and we define *DIV* as a dummy variable equal to one if the firm-year is diversified and zero otherwise. Further, we treat the interaction *DIV* × *GOV*, denoted *DG* for notational simplicity, as a separate standalone choice variable, following the procedure discussed in Chang et al. (2016). We then model the firm's choices of diversification, governance, and their interaction as latent variables that satisfy:

$$DIV_{it}^{*} = \beta_d Z_{it} + \eta_{1it} \quad \begin{matrix} DIV_{it} = 1 & if\ DIV_{it}^{*} > 0 \\ DIV_{it} = 0 & if\ DIV_{it}^{*} < 0 \end{matrix} \tag{3a}$$

$$GOV_{it}^{*} = \beta_g Z_{it} + \eta_{2it} \quad \begin{matrix} GOV_{it} = 1 & if\ GOV_{it}^{*} > 0 \\ GOV_{it} = 0 & if\ GOV_{it}^{*} < 0 \end{matrix} \tag{3b}$$

$$DG_{it}^{*} = \beta_{dg} Z_{it} + \eta_{3it} \quad \begin{matrix} DG_{it} = 1 & if\ DG_{it}^{*} > 0 \\ DG_{it} = 0 & if\ DG_{it}^{*} < 0 \end{matrix} \tag{3c}$$

where DIV_{it}^{*}, GOV_{it}^{*}, and DG_{it}^{*} are unobserved latent variables, Z_{it} is a set of firm characteristics and instrument variables that affect the diversification and governance decisions, and η_{1it}, η_{2it}, and η_{3it} are independently distributed error terms.

In the first stage, we estimate (3a)-(3c) using probit models. These models include all the firm characteristic variables in the second stage leverage regression and a set of instruments. The key criteria for the instruments are that they are correlated with the endogenous choice variables (i.e., relevant) and satisfy the exclusion restriction (i.e., not directly related to leverage except through their effects on the endogenous choice variables). We use as instruments two firm variables (S&P 500 index dummy and major

exchange dummy), four industry variables (fraction of diversified firms and fraction of sales by diversified firms in the same industry-year and fraction of good governance firms and fraction of sales by good governance firms in the same industry-year), and one macro variable (growth rate of real GDP).[28] Although we can test the relevance of these instruments with a Wald test, to our knowledge there is not a test of exclusion for a Heckman model using instruments in the first stage analogous to, for example, the Sargan-Hansen test of overidentifying restrictions for IV estimators. This is an important caveat when using the Heckman model to account for endogeneity.

Table 10 reports first stage probit regression estimates for the models in (3a)-(3c), where governance choice is based on the equally-weighted and percentile-weighted governance indices. As seen there, all instruments are significant in at least one probit model. Furthermore, reported Wald chi-square tests easily reject the null hypothesis that the coefficient estimates on the instruments are equal to zero.

From the first stage probit regressions, we obtain consistent estimates of β_d, β_g, and β_{dg} in (3a)-(3c), which we denote by $\hat{\beta}_d$, $\hat{\beta}_g$, and $\hat{\beta}_{dg}$. Using $\hat{\beta}_d$, we compute the inverse Mills ratios (IMR) for diversified (λ_{it}^d) and focused (λ_{it}^f) firms as

$$\lambda_{it}^d = \frac{\phi(\hat{\beta}_d Z_{it})}{\Phi(\hat{\beta}_d Z_{it})} \quad \text{and} \quad \lambda_{it}^f = \frac{-\phi(\hat{\beta}_d Z_{it})}{1 - \Phi(\hat{\beta}_d Z_{it})} \tag{4a}$$

[28] See the Appendix for detailed definitions of these instruments. Our choice of instruments for diversification status follows Campa and Kedia (2002) who investigate whether the choice of diversification causally influences Berger and Ofeck (1995) excess value. We follow Chang et al. (2016) in using only a subset of the instruments used by Campa and Kedia (2002) to avoid overfitting endogenous variables with the consequent problem of biasing second stage coefficient estimates toward their uninstrumented (biased) OLS values. Nevertheless, we test the sensitivity of our results to the choice of instruments by using all their instruments and various subsets. Our results are robust.

where $\phi(\cdot)$ and $\Phi(\cdot)$ are the probability density and cumulative distribution functions of the standard normal distribution. The ratio λ_{it}^{d} (λ_{it}^{f}) is an estimate of the probability that a firm decides to diversify (focus) over the cumulative probability of the firm's decision. Similarly, for good (g) and bad (b) governance firms, we compute the IMRs as

$$\lambda_{it}^{g} = \frac{\phi(\hat{\beta}_g Z_{it})}{\Phi(\hat{\beta}_g Z_{it})} \quad \text{and} \quad \lambda_{it}^{b} = \frac{-\phi(\hat{\beta}_g Z_{it})}{1 - \Phi(\hat{\beta}_g Z_{it})} \tag{4b}$$

And for diversified firms with good governance (dg) and all other interacted categories of firms (dg^c), we compute the IMRs as

$$\lambda_{it}^{dg} = \frac{\phi(\hat{\beta}_{dg} Z_{it})}{\Phi(\hat{\beta}_{dg} Z_{it})} \quad \text{and} \quad \lambda_{it}^{dg^c} = \frac{-\phi(\hat{\beta}_{dg} Z_{it})}{1 - \Phi(\hat{\beta}_{dg} Z_{it})} \tag{4c}$$

The IMRs in (4a)-(4c) are then used to compute estimates of the corrections for self-selection as

$$\lambda_{it}^{Div} = \lambda_{it}^{d} \times DIV_{it} + \lambda_{it}^{f} \times (1 - DIV_{it}) \tag{5a}$$

$$\lambda_{it}^{Gov} = \lambda_{it}^{g} \times GOV_{it} + \lambda_{it}^{b} \times (1 - GOV_{it}) \tag{5b}$$

$$\lambda_{it}^{Div \times Gov} = \lambda_{it}^{dg} \times DG_{it} + \lambda_{it}^{dg^c} \times (1 - DG_{it}) \tag{5a}$$

In the second stage leverage regression, the lambdas in (5a)-(5c) are included as separate regressors to the baseline specification in (1) and the equation is estimated using OLS. Assuming the errors in equations (1) and (3a)-(3c), ε_{it}, η_{1it}, η_{1it}, and η_{1it}, are multivariate normally distributed with zero means, standard deviations σ_ε and 1, and correlations $\rho_{\varepsilon\eta_1}$, $\rho_{\varepsilon\eta_2}$, and $\rho_{\varepsilon\eta_3}$, it is straightforward to show that the coefficients on λ_{it}^{Div}, λ_{it}^{Gov}, and $\lambda_{it}^{Div \times Gov}$ will be equal to $\rho_{\varepsilon\eta_1}\sigma_\varepsilon$, $\rho_{\varepsilon\eta_2}\sigma_\varepsilon$, and $\rho_{\varepsilon\eta_3}\sigma_\varepsilon$, respectively. For example, since $\sigma_\varepsilon > 0$, a positive coefficient on λ_{it}^{Div} indicates a positive correlation between the

firm's decision to diversify and leverage. This suggests that firm and/or market factors that drive diversification decisions also drive the decision to choose more leverage, which indicates that the coefficient on the diversification variable in equation (1) will be upward biased. The inclusion of the lambdas in the second stage regressions will reduce this bias and eliminate it asymptotically. Testing whether the estimated coefficients on the lambdas are different from zero is therefore a test of selection bias in the sample.

1.5.2. IV Estimator

We also use the instrumental variables (IV) method discussed in Adams et al. (2009), Angrist and Pischke (2009, p. 191), and Wooldridge (2010, p. 939) for binary endogenous variables. We start with the three endogenous dummy variables *DIV*, *GOV*, and *DG* (= *DIV* × *GOV*). The IV method mimicking two-stage least squares involves three steps. In the first stage (step), we use the estimated probit models reported in Table 10 to compute the fitted probabilities Prob(*DIV*), Prob(*GOV*), and Prob(*DG*). The second stage has two steps. First, the fitted probabilities are used as instruments in OLS regressions of the three endogenous dummy variables on all leverage regression controls and the corresponding instrument. These regressions are then used to compute the fitted values $\widehat{DIV}$, $\widehat{GOV}$, and $\widehat{DG}$. In the last step, the fitted values are used as regressors in the leverage regressions.[29]

1.5.3. Results

[29] Since each endogenous regressor has exactly one instrument (i.e., its fitted probability), the second stage is just-identified and we are unable to perform a Sargan-Hansen test of overidentifying restrictions. Thus, like the modified Heckman procedure described in Section 1.5.1 but for a different reason, we cannot conduct a formal test of the exclusion restriction. However, instrument relevance is not a problem for this IV estimator.

Table 11 reports estimates from second stage leverage regressions for the modified Heckman and IV methods. For comparison, the table also reports estimates from OLS regressions that do not account for the endogeneity of diversification, governance, and their interaction. Note that *Diversified*, *Governance*, and *Diversified* × *Governance* are dummy variables in the OLS and Heckman regressions, and are continuous fitted variables in the IV models. Panel A uses the equally-weighted governance index and Panel B uses the percentile-weighted governance index.

In comparison to the OLS results, the coefficient on *Governance* and *Diversified* × *Governance* are basically unchanged in the Heckman model. That is, we continue to find a positive relation between better governance and leverage in focused firms and a negative relation between better governance and leverage in diversified firms. The coefficients on the lambdas (λ^{Div}, λ^{Gov}, and $\lambda^{Div\times Gov}$) are generally significant in the Heckman specifications. This indicates the presence of selection bias and suggests that characteristics that determine firms' diversification and governance choices are correlated with leverage policy. Lastly, using the IV method to account for the joint endogeneity of leverage, diversification, governance, and the interaction of diversification and governance, we continue to find statistically and economically significant results, especially for the adjusted net leverage regressions. The coefficient on the interaction of diversification and governance is negative and significant in three out of four regressions. This is consistent with the creditor alignment hypothesis which predicts that good governance negates the alignment of manager and creditor interests and thereby has a negative effect on leverage.

1.6. Summary and conclusions

We find that corporate governance has opposite effects on financial leverage depending on whether a firm is diversified or focused. Using governance measures that are increasing in shareholder rights (decreasing in managerial entrenchment), we find a negative relation between governance and financial leverage in diversified firms. In contrast, we find a positive relation (or no relation) between governance and financial leverage in focused firms. These results are robust to a variety of different measures of leverage, diversification, and corporate governance. Our results are also robust when we correct for self-selection and when we account for the joint endogeneity of leverage, diversification, and governance.

The negative relation between better governance and financial leverage in diversified firms is consistent with the creditor alignment hypothesis, which posits that entrenched managers in diversified firms have additional debt capacity because their interests are more aligned with creditors. On the other hand, the generally positive relation between financial leverage and better governance in focused firms is consistent with the conventional thinking that entrenched managers choose low leverage to minimize performance pressure to meet debt obligations and to protect their private benefits of control that would likely be lost in bankruptcy. We argue that the managerial entrenchment hypothesis is primarily a demand side theory about the reluctance of managers to take on debt financing, while the creditor alignment hypothesis primarily relies on the willingness of creditors to supply debt, combined with management's willingness to accommodate. We use a firm's diversification status to identify the two hypotheses. Because diversification reduces the risk of a manager's undiversified portfolio, entrenched managers in diversified

firms are more likely to pursue larger debt capacity provided by creditor alignment. In contrast, entrenched managers in focused firms are more inclined to insure against downside risk by avoiding debt. Overall, the different effects of corporate governance on leverage in diversified and focused firms help explain why the literature finds mixed support for the effect of managerial entrenchment on leverage in samples containing a mixture of diversified and focused firms. Entrenched managers in focused firms eschew leverage, whereas entrenched managers in diversified firms take advantage of their better access to debt finance by using more financial leverage.

Appendix A: Variable definition

Variable	Definition (source of data)
Leverage	Total debt (long-term debt plus debt in current liabilities) divided by the book value of total assets. (Compustat)
Net leverage	Net leverage is total debt minus cash and marketable securities scaled by the book value of total assets. (Compustat)
Adjusted leverage	Industry adjusted leverage ratio. For multi-segment firms, adjusted leverage is the difference between a firm's leverage ratio and its imputed leverage ratio, where the imputed leverage ratio is the asset-weighted average of its segments' imputed leverage ratios. A segment's imputed leverage ratio is the median leverage ratio of single-segment firms in the same industry and year. For single segment firms, adjusted leverage is the difference between a firm's leverage ratio and its industry-year median leverage ratio. For segments of multi-segment firms and single segment firms, industry is based on the narrowest SIC grouping (from four-digit SIC code to two-digit SIC code) that includes at least five single-segment firms. (Segment/Compustat)
Adjusted net leverage	Industry adjusted net leverage is constructed the same way as adjusted leverage, except leverage is computed as total debt minus cash and marketable securities scaled by the book value of total assets. (Segment/Compustat)
Interest coverage ratio	Earnings before interest, taxes, depreciation, and amortization (EBITDA) divided by interest expense. (Compustat)
Adj. int. coverage ratio	Difference between a firm's interest coverage ratio and its imputed value, where the imputed ratio is the sales-weighted average of its segments' imputed ratios. A segment's imputed ratio is the median interest coverage ratio of single-segment firms in the same industry and year. For single segment firms, adjusted interest coverage is the difference between a firm's interest coverage and its industry-year median ratio. Industry is based on the narrowest SIC grouping that includes at least five single-segment firms (from four-digit SIC to two-digit SIC).
Governance variables	
EW governance index	Equally-weighted governance index, which equals the sum of zero/one indicator variables for four measures of corporate governance. The governance measures (defined below) include: E-index, institutional ownership, CEO ownership, and powerful CEO. The E-index indicator variable is equal to one if a firm-year has a below same-year median E-index, and zero otherwise. The institutional ownership indicator variable is equal to one if a firm-year has an above same-year median institutional ownership, and zero otherwise. The CEO ownership indicator variable is equal to one if the proportion of shares owned by the CEO in a year is above the same-year median proportion of shares owned by the CEO. The non-powerful CEO indicator variable is equal to one minus an indicator variable for powerful CEO.
PW governance index	Percentile-weighted governance index, which equals the sum of the percentile rankings of the four governance measures used in the construction of the EW governance index, and is then scaled to vary between zero and one. The E-index and powerful CEO variables are transformed to (6 – E-index) and (1 – powerful CEO), respectively, so that higher values indicated better governance. We assume the percentile ranking when (1 – powerful CEO) = 1 (i.e., powerful CEO = 0) is 0.50 and the percentile ranking when (1 – powerful CEO) = 0 (i.e., powerful CEO = 1) is zero.

E-index	Bebchuk, Cohen, and Ferrell (2009) entrenchment index based on the sum of zero/one indicator variables for six anti-takeover provisions: staggered boards, limits to shareholder bylaw amendments, poison pills, golden parachutes, and supermajority requirements for mergers and charter amendments. (ISS)
Institutional ownership	Number of shares owned by institutional investors divided by total number of shares outstanding. (CDA/Spectrum)
CEO stock ownership	Number of shares owned by CEO divided by total number of shares outstanding. (ExecuComp)
Powerful CEO	Dummy variable equal to one when the CEO is the only insider on the board of directors and serves as chairman of the board and president of the company, and otherwise zero. (ISS)
Diversification variables	
Diversified	Dummy variable equal to one if a firm-year has more than one business segment with different four-digit SIC code, and zero otherwise. (Compustat/Segment)
Number of segments	Number of segments with different four-digit SIC codes. (Compustat/Segment)
Herfindahl index	Herfindahl index is computed as $\sum_i^n S_i^2$ where n is the number of segments and S_i is the share of segment i sales to total firm sales. The Herfindahl index ranges from zero when the firm has many segments (high diversification) to one when the firm has only one segment (i.e., zero diversification). (Compustat/Segment)
Excess value (EV)	Logarithm of the ratio of a firm's actual market value to its imputed value. A firm's actual market value is the sum of the total book value of debt and market value of equity. A firm's imputed value is the sum of the imputed values of its segments, with each segment's imputed value equal to the segment's sales multiplied by its industry median ratio of total capital (book debt plus market value of equity) to sales. Industry median ratios are computed each year using only single-segment domestic firms, and are based on the narrowest SIC code grouping that yields at least five single-segment firms with sufficient data to compute the ratio. Single-segment domestic firms have no foreign sales and exports less than 10% of sales. Following Berger and Ofek (1995), we exclude from the analysis firm-year excess values where the firm's actual value is more than four times imputed value or less than one-fourth imputed value. We also exclude firm-year observations when the sum of segment sales deviates from the consolidated firm's total sales by more than 1%. (Compustat/Segment)
Control variables	
Log sales	Logarithm of sales in constant dollars using the CPI with base year 2014. (Compustat)
Market-to-book ratio	Ratio of book assets plus the difference between the market and book values of equity to the book value assets. (Compustat)
Return on assets	Ratio of earnings before interest and tax to the book value of assets. (Compustat)
Fixed assets ratio	Ratio of net property, plant, and equipment to the book value of assets. (Compustat)
R&D/sales	Ratio of research and development expense to sales, where research and development expense is set equal to zero when missing. (Compustat)

CAPEX/sales	Ratio of capital expenditures to sales. (Compustat)
Cash flow volatility	Standard deviation of the ratio of operating income after depreciation to assets over the prior four years. (Compustat)
Cash	Ratio of cash and marketable securities to the book value of assets. (Compustat)
Dividend payer	Dummy variable equal to one if the firm pays dividends, and zero otherwise. (Compustat)
Rated	Dummy variable equal to one if the firm has an S&P credit rating, and zero otherwise. (Compustat)
Investment rating	Dummy variable equal to one if S&P credit rating is BBB– and above, and zero otherwise. (Compustat)
Speculative rating	Dummy variable equal to one if S&P credit rating is below BBB–, and zero otherwise. (Compustat)
M&A activity	Dummy variable equal to one if the ratio of expenditures on acquisitions to book value of assets in current year and/or previous year is greater than 5%. (Compustat)

Instruments for diversification and governance

S&P 500 index dummy	Dummy variable equal to one if the firm belongs to the S&P 500 index, and zero otherwise. (ISS)
Major exchange dummy	Dummy variable equal to one if the firm is listed on the NYSE, Amex, or Nasdaq markets, and zero otherwise. (CRSP)
Fraction of diversified determined by firms	Fraction of diversified firms in the same industry-year, where industry is three-digit SIC code. (Compustat)
Fraction of sales by is diversified firms	Fraction of sales by diversified firms in the same industry-year, where industry determined by three-digit SIC code. (Compustat).
Fraction of good governance firms	Fraction of good governance firms in the same industry-year, where good governance is measured by above median EW or PW governance index and industry is determined by three-digit SIC code.
Fraction of sales by good governance firms	Fraction of sales by good governance firms in the same industry-year, where good governance is measured by above median EW or PW governance index and industry is determined by three-digit SIC code.
GDP growth rate	Change in logarithm of real U.S. GDP between two consecutive years. (BEA)

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Table 1.1 Descriptive statistics and correlations

We require firm-years to be in the Compustat Business Segment database and have governance and firm data. The full sample has 6,873 firm-year observations over 1998 to 2014. Panel A reports descriptive statistics. The diversified and focused subsamples are firm-year observations with multiple segments and one segment, respectively. All variables are defined in the Appendix. Asterisks on mean and median values in the diversified sample indicate whether they are significantly different from the corresponding mean and median values in the focused sample. Panel B reports Pearson correlation coefficients between leverage, diversification, and governance. There are fewer firm-year observations for interest coverage ratio and adjusted interest coverage ratio (6,020) because of zero interest expense. These variables are also winsorized at 5% and 95% to mitigate the influence of extreme observations. We use ***, **, and * to denote significance at the 1% level, 5% level, and 10% level, respectively.

Panel A. Descriptive statistics

	Full sample N = 6,873		Diversified sample N = 2,434		Focused sample N = 4,439	
	Mean	Median	Mean	Median	Mean	Median
Leverage measures						
Leverage	0.202	0.193	0.230***	0.232***	0.187	0.164
Net leverage	0.044	0.082	0.132***	0.160***	–0.005	0.01
Adjusted leverage	0.031	0	0.029	0.021	0.032	0
Adjusted net leverage	0.019	0.002	0.049***	0.041***	0.003	0
Interest coverage ratio	45.356	11.2	32.040***	9.841***	53.526	12.701
Adjusted interest coverage ratio	40.797	7.13	27.768***	6.130***	48.791	8.167
Diversification measures						
Diversified	0.354					
Number of segments	1.612	1	2.729***	2.000***	1	1
Herfindahl index	0.838	1	0.541***	0.513***	1	1
Excess value	0.098	0.073	–0.001***	–0.008***	0.152	0.117
Governance measures						
EW governance index	2.338	2	2.156***	2.000***	2.438	2
PW governance index	0.47	0.473	0.443***	0.450***	0.485	0.488
Governance components						
CEO stock ownership	0.021	0.003	0.020*	0.003	0.022	0.003
Institutional ownership	0.74	0.763	0.705***	0.725***	0.759	0.784
Powerful CEO	0.205		0.237***		0.187	
E-Index	1.969	2	2.085***	2.000***	1.905	2
Firm Characteristics						
Log sales	7.462	7.399	7.814***	7.693***	7.27	7.147
Market-to-book ratio	2.024	1.637	1.720***	1.502***	2.19	1.738
Return on assets	0.146	0.14	0.141***	0.136***	0.149	0.143
Fixed assets ratio	0.289	0.22	0.291	0.232***	0.288	0.211
R&D/sales	0.053	0.007	0.025***	0.007***	0.068	0.008
CAPEX/sales	0.08	0.038	0.063***	0.035***	0.089	0.039
Cash flow volatility	0.049	0.033	0.038***	0.026***	0.055	0.038
Cash	0.158	0.095	0.098***	0.055***	0.191	0.132
Dividend payer	0.54		0.707***		0.447	
Rated	0.483		0.609***		0.415	
Investment rating	0.308		0.430***		0.241	
Speculative rating	0.176		0.179		0.174	
M&A activity	0.286		0.346***		0.253	

Table 1.1 – *continued*

Panel B. Correlations between leverage, diversification, and governance

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
(1) Leverage	1										
(2) Net leverage	0.846***	1									
(3) Adj. leverage	0.613***	0.378***	1								
(4) Adj. net leverage	0.572***	0.609***	0.818***	1							
(5) Int. cov. Ratio	–0.503***	–0.526***	–0.263***	–0.326***	1						
(6) Adj. int. cov. Ratio	–0.490***	–0.523***	–0.256***	–0.318***	0.995***	1					
(7) Diversified	0.121***	0.226***	–0.009	0.095***	–0.117***	–0.114***	1				
(8) No. of segments	0.119***	0.210***	–0.011	0.079***	–0.126***	–0.121***	0.835***	1			
(9) Herfindahl index	–0.112***	–0.214***	–0.014	–0.117***	0.126***	0.122***	–0.884***	–0.846***	1		
(10) Excess value	–0.044***	–0.147***	–0.016	–0.168***	0.123***	0.117***	–0.145***	–0.138***	0.153***	1	
(11) EW gov. index	–0.083***	–0.104***	–0.011	–0.046***	0.065***	0.057***	–0.144***	–0.144***	0.140***	0.067***	1
(12) PW gov. index	–0.083***	–0.116***	–0.003	–0.053***	0.075***	0.068***	–0.156***	–0.157***	0.156***	0.084***	0.890***

Table 1.2 Univariate comparisons by good and bad corporate governance

The table reports means of variables for the full sample and diversified and focused subsamples grouped by good and bad corporate governance. A firm-year observation has good (bad) corporate governance if the EW governance index is above (below) the median index for the year. A firm-year observation is diversified (focused) if the firm has multiple (one) segments. All variables are defined in the Appendix. Asterisks on means in the bad governance columns indicate whether they are significantly different from the corresponding means in the good governance columns. We use ***, **, * to denote significance at the 1% level, 5% level, and 10% level, respectively.

	Full sample N = 6,873		Diversified sample N = 2,434		Focused sample N = 4,439	
	Good governance N = 3,016	Bad governance N = 3,857	Good governance N = 1,109	Bad governance N = 1,325	Good governance N = 2,128	Bad governance N = 2,311
Leverage	0.189	0.212***	0.212	0.245***	0.178	0.195***
Net leverage	0.016	0.065***	0.102	0.158***	–0.021	0.010***
Adjusted leverage	0.030	0.032	0.017	0.039***	0.035	0.030
Adjusted net leverage	0.010	0.026***	0.030	0.065***	0.001	0.004
Interest coverage ratio	50.505	41.613***	39.267	26.313***	56.235	51.186
Adjusted interest coverage ratio	45.323	37.508***	34.803	22.194***	50.692	47.149
Excess value	0.116	0.083**	0.012	–0.013	0.169	0.136**
Log sales	7.244	7.663***	7.590	8.001***	7.107	7.419***
Market-to-book ratio	2.075	1.984***	1.715	1.725	2.222	2.160
Return on assets	0.148	0.144*	0.141	0.140	0.152	0.147
Fixed asset ratio	0.266	0.307***	0.266	0.311***	0.266	0.308***
R&D/sales	0.054	0.052	0.024	0.026	0.066	0.070
CAPEX/sales	0.077	0.082	0.055	0.071***	0.087	0.091
Cash flow volatility	0.050	0.048***	0.040	0.036***	0.055	0.055
Cash	0.173	0.147***	0.111	0.087***	0.198	0.184***
Dividend payer	0.452	0.608***	0.609	0.790***	0.393	0.497***
Rated	0.385	0.561***	0.523	0.681***	0.334	0.489***
Investment rating	0.193	0.398***	0.309	0.531***	0.153	0.321***
Speculative rating	0.192	0.163***	0.214	0.149***	0.180	0.168
M&A activity	0.289	0.283	0.366	0.330*	0.261	0.244

Table 1.3 Effects of diversification and governance on leverage

The dependent variables are industry-adjusted leverage and industry-adjusted leverage net of cash and marketable securities. The full sample is firm-years in the Compustat Business Segment database with governance and firm data. The focused (diversified) sample is firm-years with (more than) one business segment with different four-digit SIC code. The regressions include industry fixed effects based on Fama-French 49 industries and year fixed effects. Panel A uses an equally-weighted (EW) governance index based on E-index, institutional ownership, CEO ownership, and powerful CEO. Panel B uses a percentile-weighted (PW) governance index based on E-index, institutional ownership, CEO ownership, and powerful CEO. Higher values for both indices indicate better governance and/or less entrenchment. All variables are defined in the Appendix. *T*-statistics (in parentheses) are computed using robust standard errors corrected for clustering of observations at the firm level. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively.

	Dependent variable: Adjusted leverage			Dependent variable: Adjusted net leverage		
	Full sample	Diversified sample	Focused sample	Full sample	Diversified sample	Focused sample
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Governance is EW governance index						
Diversified	0.032**			0.054**		
	(2.03)			(2.54)		
Governance	0.009**	–0.009*	0.012***	0.011*	–0.011	0.016***
	(2.29)	(–1.79)	(2.87)	(1.88)	(–1.61)	(2.63)
Diversified × governance	–0.015***			–0.016*		
	(–2.64)			(–1.90)		
Log sales	0.007**	0.004	0.008*	0.028***	0.017**	0.033***
	(2.07)	(0.78)	(1.95)	(5.28)	(2.44)	(4.79)
Market-to-book ratio	–0.004	–0.002	–0.006*	–0.026***	–0.018**	–0.029***
	(–1.47)	(–0.35)	(–1.92)	(–6.94)	(–2.03)	(–7.12)
Return on assets	–0.032	–0.126	–0.008	–0.009	–0.075	0.004
	(–0.61)	(–1.24)	(–0.13)	(–0.13)	(–0.57)	-0.05
Fixed asset ratio	–0.007	–0.032	0.004	0.197***	0.110*	0.246***
	(–0.25)	(–0.62)	(0.15)	(5.14)	(1.72)	(5.18)
R&D/sales	0.017	–0.049	0.029	–0.245***	–0.268	–0.212***
	(0.34)	(–0.39)	(0.56)	(–3.32)	(–1.18)	(–2.77)
CAPEX/sales	–0.022	0.036	–0.032	–0.019	0.05	–0.017
	(–1.03)	(0.84)	(–1.22)	(–0.66)	(0.83)	(–0.49)
Dividend payer	–0.015*	–0.044***	0.002	–0.017	–0.059***	0.005
	(–1.88)	(–3.26)	(0.17)	(–1.44)	(–3.28)	(0.36)
Cash flow volatility	0.05	–0.126	0.077	–0.181	–0.396*	–0.128
	(0.59)	(–0.73)	(0.78)	(–1.57)	(–1.77)	(–0.94)
Investment rating	0.070***	0.086***	0.063***	0.070***	0.100***	0.056***
	(6.44)	(5.21)	(4.61)	(4.74)	(4.72)	(2.95)
Speculative rating	0.148***	0.166***	0.140***	0.162***	0.178***	0.153***
	(12.4)	(8.93)	(9.12)	(10.26)	(7.17)	(7.68)
M&A activity	0.036***	0.047***	0.027***	0.089***	0.085***	0.087***
	(6.6)	(6.29)	(3.71)	(11.14)	(7.98)	(8.14)
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared	0.234	0.278	0.238	0.286	0.310	0.287
Number of observations	6,873	2,434	4,439	6,873	2,434	4,439

Table 1.3 – *continued*

	Dependent variable: Adjusted leverage			Dependent variable: Adjusted net leverage		
	Full sample	Diversified sample	Focused sample	Full sample	Diversified sample	Focused sample
	(1)	(2)	(3)	(4)	(5)	(6)
Panel B. Governance is PW governance index						
Diversified	0.042*			0.076**		
	(1.9)			(2.47)		
Governance	0.080**	–0.043	0.099***	0.092**	–0.081	0.127***
	(2.49)	(–1.14)	(3.03)	(2.03)	(–1.54)	(2.77)
Diversified × governance	–0.099**			–0.126*		
	(–2.19)			(–1.95)		
Log sales	0.007**	0.004	0.009**	0.028***	0.017**	0.033***
	(2.15)	(0.80)	(2.04)	(5.29)	(2.43)	(4.83)
Market-to-book ratio	–0.004	–0.002	–0.006*	–0.026***	–0.018**	–0.029***
	(–1.48)	(–0.35)	(–1.93)	(–6.96)	(–2.00)	(–7.14)
Return on assets	–0.035	–0.124	–0.012	–0.012	–0.074	–0.002
	(–0.67)	(–1.23)	(–0.21)	(–0.17)	(–0.56)	(–0.03)
Fixed asset ratio	–0.005	–0.03	0.006	0.199***	0.111*	0.248***
	(–0.18)	(–0.57)	(0.20)	(5.18)	(1.74)	(5.21)
R&D/sales	0.018	–0.044	0.03	–0.243***	–0.264	–0.211***
	(0.36)	(–0.35)	(0.58)	(–3.30)	(–1.16)	(–2.75)
CAPEX/sales	–0.024	0.038	–0.035	–0.021	0.051	–0.02
	(–1.08)	(0.88)	(–1.32)	(–0.73)	(0.84)	(–0.59)
Dividend payer	–0.014*	–0.043***	0.002	–0.016	–0.059***	0.006
	(–1.80)	(–3.18)	(0.23)	(–1.41)	(–3.25)	(0.41)
Cash flow volatility	0.05	–0.126	0.077	–0.182	–0.400*	–0.129
	(0.59)	(–0.73)	(0.77)	(–1.58)	(–1.78)	(–0.95)
Investment rating	0.070***	0.086***	0.064***	0.071***	0.100***	0.057***
	(6.49)	(5.22)	(4.65)	(4.76)	(4.70)	(2.98)
Speculative rating	0.149***	0.167***	0.140***	0.163***	0.179***	0.154***
	(12.41)	(8.93)	(9.14)	(10.29)	(7.19)	(7.69)
M&A activity	0.036***	0.047***	0.026***	0.089***	0.086***	0.087***
	(6.59)	(6.32)	(3.68)	(11.15)	(8.03)	(8.12)
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared	0.234	0.277	0.239	0.286	0.31	0.288
Number of observations	6,873	2,434	4,439	6,873	2,434	4,439

Table 1.4 Effects of diversification and governance on leverage excluding observations with M&A activity

The dependent variables are industry-adjusted leverage and industry-adjusted leverage net of cash and marketable securities. The full sample excludes firms-years if the ratio of expenditures on acquisitions to total book assets in the current and/or previous year is greater than 5%. The focused (diversified) sample is firm-years with (more than) one business segment with different four-digit SIC code. The regressions include industry fixed effects based on Fama-French 49 industries and year fixed effects. Panel A uses an equally-weighted (EW) governance index based on E-index, institutional ownership, CEO ownership, and powerful CEO. Panel B uses a percentile-weighted (PW) governance index based on E-index, institutional ownership, CEO ownership, and powerful CEO. Higher values for both indices indicate better governance and/or less entrenchment. All variables are defined in the Appendix. *T*-statistics (in parentheses) are computed using robust standard errors corrected for clustering of observations at the firm level. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively.

	Dependent variable: Adjusted leverage			Dependent variable: Adjusted net leverage		
	Full sample	Diversified sample	Focused sample	Full sample	Diversified sample	Focused sample
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Governance is EW governance index						
Diversified	0.043***			0.072***		
	(2.65)			(3.17)		
Governance	0.009**	–0.015***	0.011**	0.010*	–0.021***	0.014**
	(2.06)	(–2.92)	(2.38)	(1.66)	(–2.92)	(2.26)
Diversified × governance	–0.022***			–0.025***		
	(–3.48)			(–2.68)		
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared	0.222	0.263	0.228	0.268	0.298	0.267
Number of observations	4,908	1,592	3,316	4,908	1,592	3,316
Panel B. Governance is PW governance index						
Diversified	0.064***			0.110***		
	(2.75)			(3.36)		
Governance	0.066*	–0.111***	0.079**	0.073	–0.191***	0.104**
	(1.87)	(–2.62)	(2.18)	(1.56)	(–3.18)	(2.21)
Diversified × governance	–0.154***			–0.205***		
	(–3.18)			(–2.93)		
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared	0.222	0.263	0.228	0.269	0.301	0.267
Number of observations	4,908	1,592	3,316	4,908	1,592	3,316

Table 1.5 Effects of diversification and governance on leverage grouping by excess value

The dependent variables are industry-adjusted leverage and industry-adjusted leverage net of cash and marketable securities. The sample is firm-years in the Compustat Business Segment database with governance and firm data. We compute the median excess value each year for diversified firms and the median excess value each year for focused firms, and separate diversified and focused firms into above and below median excess value groups using their respective medians. We then place the above median excess value diversified and focused firms and below median excess value diversified and focused firms into separate samples. Diversification is measured with a dummy variable equal to one if the firm-year is multi-segment. The EW governance index is an equally-weighted governance index and the PW governance index is a percentile-weighted governance index. Both indices are based on E-index, institutional ownership, CEO ownership, and powerful CEO. Higher index values indicate better governance and/or less entrenchment. All regressions include the controls used in Table 3, industry fixed effects based on Fama-French 49 industries, and year fixed effects. All variables are defined in the Appendix. *T*-statistics (in parentheses) are computed using robust standard errors corrected for clustering of observations at the firm level. The numbers in square brackets are *p*-values from Chow tests of the null hypothesis that the coefficients in the above median excess value regressions are equal to the corresponding coefficients in the below median excess value regressions. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively.

	Governance is EW governance index				Governance is PW governance index			
	Above median excess value		Below median excess value		Above median excess value		Below median excess value	
	Adj. lev	Adj. net lev	Adj. lev	Adj. net lev	Adj. lev	Adj. net lev	Adj. lev	Adj. net lev
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Diversified	0.017	0.023	0.046***	0.080***	0.034	0.052	0.057**	0.110***
	(0.72)	(0.79)	(2.70)	(3.27)	(1.08)	(1.23)	(2.26)	(3.03)
			[0.111]	[0.023]			[0.378]	[0.104]
Governance	0.004	–0.002	0.016***	0.024***	0.053	0.028	0.124***	0.187***
	(0.69)	(–0.23)	(3.38)	(3.57)	(1.20)	(0.47)	(3.36)	(3.51)
			[0.013]	[0.000]			[0.048]	[0.001]
Diversified × governance	–0.009	0.004	–0.022***	–0.034***	–0.083	–0.041	–0.134**	–0.232***
	(–1.11)	(0.39)	(–3.19)	(–3.32)	(–1.33)	(–0.49)	(–2.44)	(–2.91)
			[0.083]	[0.000]			[0.362]	[0.013]
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared	0.212	0.262	0.265	0.28	0.212	0.262	0.266	0.281
Number of observations	3,444	3,444	3,428	3,428	3,444	3,444	3,428	3,428

Table 1.6 Effects of diversity intensity on the leverage and governance relation

The dependent variables are industry-adjusted leverage and industry-adjusted leverage net of cash and marketable securities. The sample is firm-years in the Compustat Business Segment database with governance and firm data. Panel A measures the intensity of diversification by the number of segments and Panel B measures the intensity of diversification by the Herfindahl index. We report regression results for the full sample, and above and below median excess value subsamples. Subsample construction is described in Table 5. For focused firms, Number of segments – 1 = 0 and 1 – Herfindahl index = 0. Governance is measured by the equally-weighted index of E-index, institutional ownership, CEO ownership, and powerful CEO. Higher index values indicate better governance and/or less entrenchment. All regressions include the controls used in Table 3, industry fixed effects based on Fama-French 49 industries, and year fixed effects. All variables are defined in the Appendix. *T*-statistics (in parentheses) are computed using robust standard errors corrected for clustering of observations at the firm level. The numbers in square brackets are *p*-values from Chow tests of the null hypothesis that the coefficients in the above median excess value regressions are equal to the corresponding coefficients in the below median excess value regressions. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively. Number of firm-year observations is 6,873.

	Dependent variable: Adjusted leverage			Dependent variable: Adjusted net leverage		
	Full sample (1)	Above median excess value (2)	Below median excess value (3)	Full sample (4)	Above median excess value (5)	Below median excess value (6)
Panel A. Diversification measure: Number of segments						
Number of segments – 1	0.010	0.001	0.017**	0.020**	0.005	0.031***
	(1.52)	(0.13)	(2.27)	(2.20)	(0.43)	(2.88)
			[0.056]			[0.026]
Governance	0.008*	0.002	0.013***	0.010*	–0.001	0.021***
	(1.98)	(0.44)	(3.10)	(1.81)	(–0.11)	(3.29)
			[0.014]			[0.001]
Number of segments – 1	–0.007**	–0.003	–0.010***	–0.008**	0.000	–0.015***
× governance	(–2.46)	(–0.81)	(–3.02)	(–2.12)	(0.04)	(–3.22)
			[0.083]			[0.003]
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared	0.233	0.226	0.270	0.285	0.303	0.295
Number of observations	6,873	3,444	3,428	6,873	3,444	3,428

(*continued*)

Table 1.6 – *continued*

	Dependent variable: Adjusted leverage			Dependent variable: Adjusted net leverage		
	Full sample (1)	Above median excess value (2)	Below median excess value (3)	Full sample (4)	Above median excess value (5)	Below median excess value (6)
Panel B. Diversification measure: Herfindahl index						
1 – Herfindahl index	0.061**	0.023	0.092***	0.110***	0.043	0.155***
	(2.20)	(0.56)	(2.91)	(2.86)	(0.80)	(3.49)
			[0.045]			[0.016]
Governance	0.009**	0.003	0.015***	0.010*	–0.001	0.023***
	(2.22)	(0.54)	(3.44)	(1.90)	(–0.18)	(3.47)
			[0.007]			[0.000]
1 – Herfindahl index	–0.031***	–0.015	–0.045***	–0.033**	0.006	–0.064***
× governance	(–2.82)	(–0.95)	(–3.49)	(–2.19)	(0.27)	(–3.50)
			[0.042]			[0.000]
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared	0.234	0.226	0.272	0.286	0.305	0.296
Number of observations	6,873	3,444	3,428	6,873	3,444	3,428

Table 1.7 Effects of diversification and governance on unadjusted leverage and interest coverage ratio

The dependent variables in Panel A are ratios of total debt (long-term debt plus debt in current liabilities) and total debt minus cash and marketable securities to the book value of total assets. The dependent variables in Panel B are interest coverage ratio (earnings before interest, taxes, depreciation, and amortization divided by interest expense) and industry-adjusted interest coverage ratio. The sample is firm-years in the Compustat Business Segment database with governance and firm data. We report regression results for the full sample, and above and below median excess value subsamples. Subsample construction is described in Table 5. Diversification is measured with a dummy variable equal to one if the firm-year is multi-segment. Governance is measured by the equally-weighted index of E-index, institutional ownership, CEO ownership, and powerful CEO. Higher index values indicate better governance and/or less entrenchment. All regressions include the controls used in Table 3, industry fixed effects based on Fama-French 49 industries, and year fixed effects. All variables are defined in the Appendix. *T*-statistics (in parentheses) are computed using robust standard errors corrected for clustering of observations at the firm level. The numbers in square brackets are *p*-values from Chow tests of the null hypothesis that the coefficients in the above median excess value regressions are equal to the corresponding coefficients in the below median excess value regressions. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively.

Panel A. Leverage is measured by unadjusted book leverage ratio

	Dependent variable: Unadjusted leverage			Dependent variable: Unadjusted net leverage		
	Full sample (1)	Above median excess value (2)	Below median excess value (3)	Full sample (4)	Above median excess value (5)	Below median excess value (6)
Diversified	0.008	–0.013	0.025	0.040*	0.007	0.065***
	(0.52)	(–0.58)	(1.39)	(1.86)	(0.23)	(2.61)
			[0.028]			[0.016]
Governance	0.006	–0.001	0.013**	0.008	–0.008	0.023***
	(1.42)	(–0.13)	(2.56)	(1.36)	(–0.93)	(3.20)
			[0.005]			[0.000]
Diversified × governance	–0.008	0.001	–0.016**	–0.012	0.011	–0.031***
	(–1.38)	(0.18)	(–2.34)	(–1.45)	(0.98)	(–3.21)
			[0.011]			[0.000]
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared	0.394	0.393	0.418	0.571	0.603	0.545
Number of observations	6,873	3,444	3,428	6,873	3,444	3,428

Table 1.7 – *continued*

Panel B. Leverage is measured by interest coverage ratio

	Dependent variable: Interest coverage ratio			Dependent variable: Adjusted interest coverage ratio		
	Full sample (1)	Above median excess value (2)	Below median excess value (3)	Full sample (4)	Above median excess value (5)	Below median excess value (6)
Diversified	–7.884	–4.458	–11.447	–9.172	–6.856	–11.65
	(–0.95)	(–0.33)	(–1.43)	(–1.10)	(–0.50)	(–1.49)
			[0.484]			[0.633]
Governance	–1.984	2.095	–5.137*	–2.574	1.67	–5.878**
	(–0.66)	-0.44	(–1.75)	(–0.85)	(0.34)	(–2.01)
			[0.031]			[0.026]
Diversified × governance	3.759	–0.873	8.349**	4.493	0.027	8.957***
	(1.11)	(–0.16)	(2.43)	(1.32)	(0.00)	(2.63)
			[0.025]			[0.032]
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared	0.263	0.282	0.245	0.258	0.275	0.245
Number of observations	6,020	2,920	3,097	6,020	2,920	3,097

Table 1.8 Effects of diversification and governance on leverage with governance measured by E-index

The dependent variables are industry-adjusted leverage and industry-adjusted leverage net of cash and marketable securities. The sample is firm-years in the Compustat Business Segment database with governance and firm data. We report regression results for the full sample, and above and below median excess value subsamples. Subsample construction is described in Table 5. Diversification is measured with a dummy variable equal to one if the firm-year is multi-segment. Governance is measured by the variable 6 – E-index. Since the E-index is the sum of 0/1 indictor variables for six anti-takeover provisions, higher values for 6 – E-index indicate better governance. All regressions include the controls used in Table 3, industry fixed effects based on Fama-French 49 industries, and year fixed effects. All variables are defined in the Appendix. *T*-statistics (in parentheses) are computed using robust standard errors corrected for clustering of observations at the firm level. The numbers in square brackets are *p*-values from Chow tests of the null hypothesis that the coefficients in the above median excess value regressions are equal to the corresponding coefficients in the below median excess value regressions. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively.

	Dependent variable: Adjusted leverage			Dependent variable: Adjusted net leverage		
	Full sample	Above median excess value	Below median excess value	Full sample	Above median excess value	Below median excess value
	(1)	(2)	(3)	(4)	(5)	(6)
Diversified	0.038**	0.034	0.036*	0.092***	0.083**	0.083***
	(2.10)	(1.29)	(1.70)	(3.59)	(2.36)	(2.74)
			[0.908]			[0.995]
6 – E-index	0.007**	0.005	0.009**	0.010**	0.006	0.013**
	(2.23)	(1.28)	(2.14)	(2.02)	(0.99)	(2.20)
			[0.364]			[0.191]
Diversified × (6 – E-index)	–0.011***	–0.010*	–0.010**	–0.019***	–0.013*	–0.020***
	(–2.61)	(–1.72)	(–2.08)	(–3.23)	(–1.69)	(–2.77)
			[0.898]			[0.308]
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared	0.233	0.227	0.268	0.287	0.307	0.293
Number of observations	6,873	3,444	3,429	6,873	3,444	3,429

Table 1.9 Effects of diversification and governance on leverage with different industry fixed effects

The dependent variable in all regressions is industry-adjusted leverage. The sample is firm-years in the Compustat Business Segment database with governance and firm data. We report regression results for the full sample, and above and below median excess value subsamples. Subsample construction is described in Table 5. Diversification is measured with a dummy variable equal to one if the firm-year is multi-segment. Governance is measured by the equally-weighted index of E-index, institutional ownership, CEO ownership, and powerful CEO. Higher index values indicate better governance and/or less entrenchment. All regressions include the controls used in Table 3, industry fixed effects, and year fixed effects. The industry fixed effects in regressions (1)-(3) are based on 207 different three-digit SIC codes in our sample, the industry fixed effects in regressions (4)-(6) are based on 53 different two-digit SIC codes in our sample, and the industry fixed effects in regressions (7)-(9) are based on Fama-French 30 industries. All variables are defined in the Appendix. *T*-statistics (in parentheses) are computed using robust standard errors corrected for clustering of observations at the firm level. The numbers in square brackets are *p*-values from Chow tests of the null hypothesis that the coefficients in the above median excess value regressions are equal to the corresponding coefficients in the below median excess value regressions. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively.

	Industry fixed effects: 3-digit SIC codes			Industry fixed effects: 2-digit SIC codes			Industry fixed effects: Fama-French 30		
	Full sample (1)	Above median EV (2)	Below median EV (3)	Full sample (4)	Above median EV (5)	Below median EV (6)	Full sample (7)	Above median EV (8)	Below median EV (9)
Diversified	0.028*	0.019	0.039**	0.023	0.006	0.039**	0.032**	0.017	0.046***
	(1.78)	(0.79)	(2.15)	(1.46)	(0.28)	(2.17)	(2.05)	(0.75)	(2.66)
			[0.300]			[0.077]			[0.118]
Governance	0.011***	0.006	0.016***	0.009**	0.004	0.015***	0.008*	0.003	0.015***
	(2.71)	(1.1)	(3.37)	(2.12)	(0.69)	(3.14)	(1.95)	(0.45)	(3.18)
			[0.039]			[0.021]			[0.009]
Diversified × governance	–0.016***	–0.01	–0.022***	–0.014**	–0.007	–0.023***	–0.016***	–0.009	–0.024***
	(–2.64)	(–1.11)	(–3.09)	(–2.45)	(–0.88)	(–3.24)	(–2.70)	(–1.05)	(–3.37)
			[0.115]			[0.033]			[0.042]
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared	0.293	0.312	0.323	0.224	0.225	0.259	0.22	0.218	0.246
Number of observations	6,873	3,444	3,429	6,873	3,444	3,429	6,873	3,444	3,429

Table 1.10 Probit regressions explaining the choice of diversification, governance, and their interaction

The table reports first-stage probit regressions for the choice of diversification, governance, and the interaction of diversification and governance on a set of instruments and controls. A firm-year is diversified if it has more than one business segment. A firm-year has good corporate governance if the equally-weighted (EW) governance index is above its sample median for the year (columns (1)-(3)), or if the percentile-weighted (PW) governance index is above its sample median for the year (columns (4)-(6)). The governance indices are based on E-index, institutional ownership, CEO ownership, and powerful CEO. Higher values for both indices indicate better governance and/or less entrenchment. The instrumental variables in the regressions are S&P 500 index dummy, major exchange dummy, fraction of diversified firms and fractions of sales accounted for by diversified firms in the same (3-digit SIC) industry-year, fraction of good governance firms and the fraction of sales accounted for by good governance firms in the same (3-digit SIC) industry-year, and growth rate of real GDP. The null hypothesis of the Wald test is that the coefficient estimates on the instrumental variables are equal to zero. *Z*-statistics (in parentheses) are computed using robust standard errors corrected for clustering of observations at the firm level. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable is choice of	Governance is measured by EW governance index			Governance is measured by PW governance index		
	Diversified	Governance	Div. × Gov.	Diversified	Governance	Div. × Gov.
	(1)	(2)	(3)	(4)	(5)	(6)
S&P 500 index dummy	0.054	–0.440***	–0.203	0.059	–0.465***	–0.139
	(0.44)	(–4.62)	(–1.56)	(0.48)	(–4.72)	(–1.07)
Major exchange dummy	0.285	0.11	0.397**	0.284	–0.037	0.268
	(1.14)	(0.80)	(2.15)	(1.14)	(–0.23)	(1.40)
Fraction of diversified firms	2.893***	0.032	1.651***	2.913***	0.111	1.886***
	(9.86)	(0.18)	(6.67)	(9.87)	(0.59)	(7.38)
Fraction of sales by diversified firms	0.199	–0.19	–0.05	0.195	–0.285*	–0.209
	(0.87)	(–1.31)	(–0.23)	(0.85)	(–1.92)	(–0.97)
Fraction of good governance firms	0.364	2.943***	1.689***	0.527**	2.929***	1.316***
	(1.55)	(13.01)	(6.24)	(2.19)	(12.89)	(5.07)
Fraction of sales by good governance firms	–0.408**	–0.099	–0.117	–0.420**	–0.077	0.059
	(–2.22)	(–0.57)	(–0.55)	(–2.32)	(–0.47)	(0.30)
GDP growth rate	2.565**	0.381	2.16	2.408*	0.043	1.408
	(2.09)	(0.35)	(1.54)	(1.95)	(0.04)	(1.07)
Log sales	0.133**	0.014	0.041	0.131**	0.029	0.053
	(2.46)	(0.39)	(0.82)	(2.41)	(0.81)	(1.10)
Market-to-book ratio	–0.093**	0.033	–0.045	–0.092**	0.045*	–0.049
	(–2.11)	(1.37)	(–0.93)	(–2.10)	(1.86)	(–1.00)
Return on assets	–1.279***	0.237	–0.641	–1.298***	0.525	–0.565
	(–2.99)	(0.70)	(–1.34)	(–3.04)	(1.51)	(–1.23)

Table 1.10 – *continued*

Dependent variable is choice of	Governance is measured by EW governance index			Governance is measured by PW governance index		
	Diversified	Governance	Div. × Gov.	Diversified	Governance	Div. × Gov.
	(1)	(2)	(3)	(4)	(5)	(6)
Fixed asset ratio	–0.375	–0.181	–0.104	–0.342	0.013	–0.013
	(–1.26)	(–0.98)	(–0.38)	(–1.15)	(0.07)	(–0.05)
R&D/sales	–2.008***	–0.008	–1.519**	–1.962***	0.135	–1.476**
	(–2.94)	(–0.03)	(–2.21)	(–2.88)	(0.43)	(–2.05)
CAPEX/sales	–0.141	0.319	–0.461	–0.149	0.288	–0.429
	(–0.37)	(1.53)	(–1.30)	(–0.39)	(1.32)	(–1.36)
Dividend payer	0.279***	–0.134**	0.005	0.282***	–0.186***	0.002
	-2.89	(–2.06)	-0.06	(2.91)	(–2.81)	(0.03)
Cash flow volatility	–0.617	–0.717	–0.545	–0.66	–0.297	–0.313
	(–0.64)	(–1.22)	(–0.57)	(–0.69)	(–0.48)	(–0.31)
M&A activity	0.217***	0.003	0.124**	0.220***	0.026	0.159***
	(3.39)	(0.07)	(1.99)	(3.44)	(0.55)	(2.67)
Investment rating	0.02	–0.275***	–0.158	0.032	–0.345***	–0.257*
	(0.15)	(–2.77)	(–1.19)	(0.23)	(–3.40)	(–1.91)
Speculative rating	0.029	–0.038	0.056	0.033	–0.111	0.029
	(0.23)	(–0.44)	(0.48)	(0.26)	(–1.25)	(0.26)
Pseudo R-squared	0.347	0.162	0.161	0.347	0.172	0.164
Wald test (chi-squared)	1,446.89***	892.82***	492.16***	1,447.57***	878.31***	554.13***
Number of observations	6,873	6,873	6,873	6,873	6,873	6,873

Table 1.11 Effects of diversification and governance on leverage accounting for selection bias and endogeneity

The dependent variables are industry-adjusted leverage and industry-adjusted leverage net of cash and marketable securities. The key right-hand-side variables are dummy variables for diversification, governance, and their interaction, where *Diversified* is equal to one if the firm-year has more than two segments and *Governance* is equal to one if the firm-year governance index is above the median governance index in that year. Panel A uses an equally-weighted (EW) governance index based on E-index, institutional ownership, CEO ownership, and powerful CEO. Panel B uses a percentile-weighted (PW) governance index based on E-index, institutional ownership, CEO ownership, and powerful CEO. Higher values for both indices indicate better governance, so that *Governance* equals one for good (above median) governance. Columns (1) and (4) report baseline OLS regressions of adjusted-leverage on *Diversified*, *Governance*, *Diversified* × *Governance*, control variables, and industry and year fixed effects. Columns (2) and (5) report adjusted-leverage regressions using a modified Heckman (1979) method to account for selection bias where we include in the OLS regression corrections for self-selection for *Diversified* (λ^{Div}), *Governance* (λ^{Gov}), and *Diversified* × *Governance* ($\lambda^{Div\times Gov}$) computed using the probit regressions reported in Table 10. Columns (3) and (6) report second-stage adjusted leverage regressions using the IV method discussed in Adams et al. (2009), Angrist and Pischke (2009, p. 191), and Wooldridge (2010, p. 939) to account for dummy variable endogenous variables. The first stage uses the estimated probit regressions in Table 10 to compute fitted probabilities for diversification, governance, and their interaction. These fitted probabilities are then used as instruments in OLS regressions of the three endogenous dummy variables on all leverage regression controls and the corresponding instrument. Fitted values from these OLS regressions are then used as regressors in the leverage regressions reported in columns (3) and (6). The control variables are those used in Table 3. All variable are defined in the Appendix. *T*-statistics (in parentheses) are computed using robust standard errors corrected for clustering of observations at the firm level. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively.

Panel A. Corporate governance based on EW governance index

	Dependent variable: Adjusted leverage			Dependent variable: Adjusted net leverage		
	OLS	Heckman	IV	OLS	Heckman	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Diversified	0.006	0.084*	0.018	0.028**	0.140**	0.054**
	(0.64)	(1.84)	(1.01)	(2.18)	(2.24)	(2.15)
Governance	0.015**	0.016**	0.026	0.018*	0.015	0.085***
	(2.11)	(2.11)	(1.56)	(1.82)	(1.42)	(3.59)
Diversified × governance	–0.023**	–0.023*	–0.071*	–0.024	–0.023	–0.140**
	(–1.98)	(–1.96)	(–1.86)	(–1.49)	(–1.45)	(–2.56)
λ^{Div}		–0.044*			–0.061*	
		(–1.69)			(–1.69)	
λ^{Gov}		–0.038*			–0.084***	
		(–1.69)			(–2.75)	
$\lambda^{Div\times Gov}$		0.058*			0.095**	
		(1.91)			(2.30)	
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared	0.233	0.234	0.228	0.286	0.288	0.267
Number of observations	6,873	6,873	6,873	6,873	6,873	6,873

Table 1.11 – *continued*

Panel B. Corporate governance based on PW governance index

	Dependent variable: Adjusted leverage			Dependent variable: Adjusted net leverage		
	OLS	Heckman	IV	OLS	Heckman	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Diversified	0.009	0.073*	0.00	0.035***	0.125**	0.060**
	(0.90)	(1.76)	(0.01)	(2.67)	(2.19)	(2.17)
Governance	0.016**	0.016**	0.014	0.021**	0.016	0.089***
	(2.22)	(2.15)	(0.89)	(2.01)	(1.48)	(3.94)
Diversified × governance	–0.026**	–0.025**	–0.024	–0.036**	–0.034**	–0.134**
	(–2.15)	(–2.07)	(–0.65)	(–2.12)	(–1.99)	(–2.53)
λ^{Div}		–0.036			–0.049	
		(–1.52)			(–1.48)	
λ^{Gov}		–0.028			–0.070***	
		(–1.53)			(–2.85)	
$\lambda^{Div \times Gov}$		0.048*			0.080**	
		(1.74)			(2.14)	
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared	0.233	0.234	0.233	0.286	0.289	0.27
Number of observations	6,873	6,873	6,873	6,873	6,873	6,873

Chapter 2
Human Capital, Investment Riskiness, and Investment Policy

2.1. Introduction

Aggressive investment policy is often associated with high business risk: if successful, it benefits the firm in the long run; if not, it may hasten business failure. The literature identifies one of the causes of corporate failure, as summarized in Argenti (1976), is insufficient considerations for research and development cost. Further, Dambolena and Khoury (1980) indicate that a substantial instability in firm ratios is associated with corporation failure. When large investments fail, a firm faces a high possibility of operating at a loss, which ultimately leads to plant shutdowns. Thus, investment riskiness is undeniably one of the most important determinants of business failure. On the other hand, the labor economics literature (e.g., Clark and Oswald (1994) and Clark, Georgellis, and Sanfey (2001)) shows that employees' fear of job loss is a major worry, regardless of whether employees can find a replacement job. The more aggressive the firm's investment policy, the riskier the firm, and hence the higher the risk of the human capital loss borne by employees. As a result, rational employees will demand a higher wage to compensate for this additional human capital risk. We will later illustrate this line of motivation using a simple theoretical framework in the next section. In this sense, aggressive investment activities may be associated with larger human capital cost for the firm. This is extremely important to the firm because if employees demand significantly higher pay to compensate for the human capital risk associated with risky investments, then discounted expected future cash flow will decrease while initial cash outlay stays the same. This will lead to a lower project NPV than what it would be with a less risky investment. Moreover, if the human capital cost of the investment

increases significantly, firms will have a strong incentive to forego risky projects to reduce human capital cost. Our finding provides a potential explanation for the underinvestment problem apart from the established agency theory.

The labor economics literature has long established that workers require firms to provide a premium in wages or benefits as compensation for potential job loss (e.g., Abowd and Ashenfelter (1981), Topel (1984)). However, the relation between human capital cost and corporate policy is relatively novel in the corporate finance literature. One stream of literature has linked human capital to a firm's financing policy. Berk, Stanton, and Zechner (2010), and Chemmanur, Cheng, and Zhang (2013) study the relation between human capital cost and a firm's financing policy. Berk et al. (2010) argue that employees become entrenched under an optimal labor contract for a levered firm, and therefore face large human capital cost in bankruptcy.[1] Chemmanur et al. (2013) empirically support the predictions of Berk et al. (2010)'s and find that wages have significant explanatory power for firm leverage. In addition, Agrawal and Matsa (2013) adds to this line of research by arguing that firms choose conservative financial policies to mitigate workers' exposure to unemployment risk. They further find that lower unemployment benefits (higher unemployment risk) lead to lower corporate leverage. Lee, Mauer, and Xu (2017) has linked human capital to major corporate events: they examine whether human capital relatedness is a key factor in mergers and acquisitions. They find that mergers are more likely, and merger returns and post-merger performance are higher when firms have higher related human capital. They argue that mergers with

[1] The only friction is the inability of employees to insure their human capital. In their model, entrenchment is the efficient response to this friction rather than an exogenously imposed inefficiency.

high human capital relatedness give firms greater ability to layoff low quality and/or duplicate employees to reduce human capital cost.

Another line of research has examined and interpreted the direct relation between CEO compensation and a firm's investment policy, proxied mainly by R&D expenditures. For example, Clinch (1991), Smith and Watts (1992), Baber et al. (1996), Gaver and Gaver (1998), Ryan and Wiggins (2002), and Coles et al. (2006), find positive relations between investment opportunity proxies and compensation tied to stock price performance. In contrast, Bizjak et al. (1993), Yermack (1995) and Lev and Sougiannis (1996) find negative relations associated with total compensation and cash compensation of CEOs. Matsunaga (1995) finds no significant association between R&D expenditures and the value of employee stock option grants. One possible reason for the mixed findings, as Cheng (2004) points out, is that, in general settings, it is unclear whether compensation committees should motivate more R&D expenditures because of the possibility of overinvestment in R&D.

On the other hand, very few studies have focused on the relation of average employees and investment policy. Among the few, Clinch (1991) studies key employee compensation and firms' R&D activities. He claims that three well-known determinants of compensation practices are motivation-based concerns (moral hazard), information-based concerns (adverse selection), and tax issues. The results are difficult to interpret from the motivation, information, and tax-based perspectives, because there are various factors that can influence the compensation design in each setting. In many cases, particularly for large companies or administrative positions, non-executive employees may have little involvement in a firm's investment decisions. Clinch (1991) continues to

argue that, if this is the case, it is not clear how to interpret any relation between risky investments (R&D expenditures) and features of observed compensation relations for the average employee.

Our paper focus on average employees and provides a novel explanation from a human capital cost perspective. We argue that the positive relation between investment risk and human capital cost is driven by human capital cost: average employees with under-diversified human capital risk will demand higher pay as additional compensation for potential job loss due to the risky investment policy. Consistently, we find a positive effect of investment riskiness on average employee pay. Our results indicate that total human capital cost is significantly positive in relation to the level of investment riskiness as measured by cash flow volatility and unlevered stock return volatility. We next examined how employees' sensitivity towards job losses affect the positive relation by comparing subsample results of lower-pay employees versus higher-pay employees. We find employee's sensitivity to job loss accentuates the positive relation between average employee pay and investment riskiness. Furthermore, we investigate the possible channels through which risky investments have influences on human capital cost. We examine corporate diversification, R&D expenditures, advertising expenditures, and acquisition. As diversification reduces total firm risk, we find that the greater the number of business segments with different four-digit SIC codes a firm has, the lower the human capital cost. On the other hand, R&D expenditures, advertising expenditures, and acquisition are considered to be the three channels for the level of investment riskiness. We observe a positive relation between each of the three channels and a firm's human capital cost, which is consistent with our hypotheses. Lastly, we finish the loop by

providing evidence on the feedback effect of increased human capital cost on a firm's investment policy. We show that labor-intensive firms have significantly lower risky investments.

Our results are robust to our best attempt to address endogeneity. Our baseline regressions include firm-year fixed effect to control for firm specific and time invariant biases. The biggest endogeneity concern would be whether the results are driven by employee skills. To address this problem, we first include a high-tech dummy variable as a control for skill. We then use system GMM regressions to account for concerns of omitted variables. Furthermore, we separate our average employee sample into non-high-tech firms and high-tech firms. We still observe the positive relation between investment riskiness and human capital cost in the non-technology subsample (unskilled workers). Lastly, we follow Kale, Ryan, and Wang (2016) to use the passage of North American Free Trade Agreement (NAFTA) in 1994 as an exogenous variation in the employment opportunities. Consistent with our hypothesis, we find that following the implementation of NAFTA, the relation between human capital cost and investment riskiness becomes significantly more positive in manufacturing industries.

We contribute to the literature in the following ways. First, our study contributes to the nascent but growing literature on the impact of the human capital by establishing the importance of human capital cost for a firm's investment decisions. We provide added understanding of the determinants of employee wages. Second, we offer a novel explanation for the underinvestment problem apart from the established agency theory. We find that investment riskiness as measured by cash flow volatility and unlevered stock return volatility has a significantly positive impact on human capital cost as

measured by average employee pay. In other words, employees will demand higher pay to compensate for the large human capital loss associated with their firm's investment risk. The additional labor cost could be sufficiently large to offset the positive NPV of the risky projects.[2] If managers consider the large additional labor cost in the estimation process of NPV, it could be optimal to pass on the risky projects.

The rest of the paper is organized as follows. Section 2.2 describes a theoretical setting that motivates our study and testable hypotheses. Section 2.3 discusses variable construction, data collection, and sample descriptive statistics. Section 2.4 presents the empirical results using an average employee sample, and includes robustness tests for potential endogeneity issues, respectively. Section 2.5 presents results for channel tests. Section 2.6 shows results on the feedback effect of labor intensity on firm's investment policy. Section 2.7 concludes.

[2] The impact of investment riskiness on labor cost is economically significant, as we will show in Section 2.4.1.

2.2. The conceptual model and hypotheses development

2.2.1. The model

Under the setting of employees' inabilities to insure their own human capital, Berk et al. (2010) endogenously derive managerial entrenchment as an optimal response to labor market competition. Their model predicts an inverse relation between entrenchment and leverage and provides evidence that bankruptcy costs borne by employees are large enough to offset the tax benefits of debt. One important implication of their model is that employees should care about the firm's likelihood of bankruptcy or shut down. Some variable such as credit rating can explicitly provide a link between firm's characteristics and probability of bankruptcy or shut down and serve as a reference to employees.

Different from Berk et al. (2010), we focus on the risk arising from the firm's expenditures on risky investments rather than assuming the firm earns the risk-free rate on all invested capital.[3] In this section, following Harris and Holmstrom (1982) and Berk et al. (2010), we present a simple conceptual model to motivate the potential positive relation between expenditures on risky investments and labor cost.

Assume an employee has a minimum reservation wage W_R. If a firm invests in risk-free investments only, then the equilibrium wage, W^*, must satisfy the condition

$$W^* = W_R \tag{1}$$

[3] The only source of risk in their model is the volatility of employees' output.

Consider a firm that makes risky investments, and assume the probability of failure (i.e., complete shutdown) is $P(I)$, where $P'(I) > 0$ and $P(0) = 0$.[4]

The equilibrium wage under these conditions must satisfy the condition:

$$E[\widetilde{W}] = P(0) + (1 - P)W^{**} = W_R \tag{2}$$

Or

$$W^{**} = \frac{W_R}{1-P} \tag{3}$$

Using $P = P(I)$, we may compute that

$$\frac{\partial W^{**}}{\partial I} = \frac{W_R P'(I)}{(1-P(I))^2} > 0 \tag{4}$$

The equilibrium wage increases with expenditures on risky investments. Thus, the labor cost is relatively higher in the firm with risky investments.

The critical assumption in this model is that the employee has firm-specific human capital that is not easily transferable to another firm. This means when an employee loses her job and returns to the job market, she would not be as highly compensated at another firm or would have to bear considerable expense re-tooling her human capital to match the needs of an alternative employer even if the new employer is willing to pay a similar wage as what she made at the previous firm. For example, labor market frictions exist and will translate to costs that are borne by the employee. She will not be able to find the same job without bearing non-trivial search and/or relocation costs. When the firm invests on risky projects, it increases the riskiness borne by the firm. As a

[4] We assume the riskiness borne by the firm is positively related to the capital expenditures on risky investments. See section 2.2.1 in Grundy and Li (2010). If a firm does not have risky investments, it is free of any shocks to demand in our setting.

result, the potential significant loss on human capital prompts the employee to demand higher compensation. The firm in turn may have to adopt conservative investment policy because of large labor cost associated with risky investments. We next motivate our hypotheses 1 and 2 based on the theoretical work.

2.2.2. Hypotheses development

As discussed earlier, employees may demand a higher wage to compensate for the potential job loss due to the level of risk their firm is taking. In this sense, high investment risk may cause high human capital cost. Based on our theoretical prediction discussed above, we have following testable hypotheses:

Hypothesis 1. Average employee pay increases with investment risk.

Since employees demand higher pay to compensate for the potential human capital loss induced by investment riskiness, an employee's sensitivity towards unemployment risk should be a crucial factor in determining the relation between investment riskiness and average employee pay. Marginal utility of wealth increases as wealth decreases, and this view should also hold, that the disutility from losing additional dollar would increase with wealth. In other words, the disutility from losing another dollar is highest for people with little wealth. Thus, wealthy people tolerate risk significantly more than others.[5] Hence, lower-pay workers should be associated with a higher sensitivity to job loss while higher-pay workers have a lower sensitivity to job loss. We formalize above discussion with the following testable hypothesis.

[5] See Shilon (2015).

Hypothesis 2. Sensitivity to job loss accentuates the positive relation between average employee pay and investment risk.

To further study the impact of investment policy on human capital cost, we examine the channels through which investment riskiness affects labor cost. Lewellen (1971) argues that the combined (more diversified) enterprise enhances lenders' safety and increases aggregate debt capacity. He attributes this additional debt capacity to a co-insurance effect, whereby combining firms' cash flows that are not perfectly correlated will, in general, reduce the overall variance of the combined firm's cash flows. Subsequent researchers, such as Berger and Ofek (1995) and Kuppuswamy and Villalonga (2015) find that diversified firms have higher leverage relative to comparable portfolios of stand-alone firms. We follow literature to argue that diversification (the opposite of specialization) level is a channel where risky investments operate, i.e., the less diversified a firm is, the riskier its investments. We use the number of business segments as a proxy for corporate diversification. R&D expenditures have long been established in literature as a popular measure for risky investment (e.g., Clinch (1991), Smith and Watts (1992), Baber et al. (1996), Gaver and Gaver (1998), and Ryan and Wiggins (2002)). Harris and Raviv (1991) argue that R&D expenditures and advertising expenditures can be interpreted as measuring the extent to which assets are intangible. Miller and Bromiley (1990) develop taxonomy of strategic risk that deals with the level of investment in physical capital and in the intangible resources that accrue from research and development and advertising expenditures. Following the literature, we adopt R&D expenditures and advertising expenditures as additional risky investment channels. Lastly, we adopt another possible channel for risky investment as total acquisition amount in a

year (acquisition). Lubatkin and O'Neill (1987) study how mergers influence capital market risk and find that all types of mergers are associated with significant increases in unsystematic risk. May (1995) studies whether managers consider personal risk when making decisions that affect firm risk. He finds that expenditures on diversifying acquisition decrease when CEOs have higher level of personal wealth vested in firm equity. In summary, we implement corporate diversification, R&D expenditures, advertising expenditures, and acquisition as four possible channels through which risky investments affect human capital cost. As diversification reduces investment risk while the other three are contributors to investment risk, we hypothesize as follows:

Hypothesis 3. A lower number of business segments, higher R&D expenditures, higher advertising expenditures, or higher acquisition increase human capital cost.

2.3. Variable construction, data, and descriptive statistics

In this section, we provide details of variable construction, sample selection, and the descriptive statistics of the variables.

2.3.1. Variable construction

Our measures for investment riskiness are direct measures and are non-policy related: cash flow volatility for operational risk and unlevered stock return volatility for asset risk. Cash flow volatility and stock return volatility are two commonly used measures for investment related firm risks. Ryan and Wiggins (2001) argue that firms with risky investments or volatile operating cash flows will use incentive compensation with non-linear payoffs to limit a manager's downside risk. They find that high R&D firms have a cash flow volatility measure of 0.50 vs. 0.24 for low R&D firms. Gilchrist and Himmelberg (1995) include cash flow as one of the observable fundamentals in the forecasting system used to predict future investment opportunities. Coles et al. (2006) study managerial incentives and risk taking. They use stock return volatility as a proxy for firm risk. In addition, the literature finds that cash flow volatility is closely related to stock return volatility (e.g., Campbell et al. (2001), Irvine and Pontiff (2008), and Huang (2009)). Therefore, we use both cash flow volatility and stock return volatility (unlevered) as proxies for risky investments. We use unlevered volatility variables because leverage also increases stock return volatility. We follow Childs, Mauer, and Ott (2005), and Schwert and Strebulaev (2014) for empirical measures of unlevered risk. Following Kuppuswamy and Villalonga (2015), cash flow volatility is calculated as the standard deviation of the ratio of operating income after depreciation to assets over the eight

quarters (two years) ending in each fiscal year. [6] We follow Childs et al. (2005) and Schwert and Strebulaev (2014) to calculate the unlevered stock return. Then the volatility is calculated as the standard deviation of daily stock returns in past two years to be consistent with timeline of cash flow volatility. For human capital cost, we adopt average employee pay as the measure.[7] For average employee pay, ideally, we would like to have detailed information on job titles, wages, and education level. Unfortunately, such data is not publicly available at firm level. We therefore follow Chemmanur et al. (2013) to use Compustat data to estimate average employee pay. We adopt two methods: 1. Staff expenses divided by the number of employees, and 2. Selling, general, and administrative expense (SGA) divided by the number of employees. We can use Compustat SGA as a proxy for wages since the correlation between SGA and staff expenses is very high at 0.9, and 78.8% of the whole sample has SGA (447,216 out of 567,376 observations), while staff expenses only have 45.9% (260,571) observations. All variable definitions are specified in detail in Appendix A.

2.3.2. Sample selection

For the average employee pay sample, we use information from the Compustat database to calculate average employee pay. We exclude financial and utility companies and firms with fewer than one hundred employees. We drop firm-years with non-positive book values of equity. We require non-missing information on risky investment measures,

[6] Alternatively, we used operating income before depreciation, the results still hold.

[7] We also adopt CEO compensation as an alternative measure. CEO total compensation is the sum of salary, bonus, other annual, restricted stock grants, long-term incentive plan payouts (LTIP), all other, and value of option grants. We further examine equity-based compensation and cash compensation separately. Cash compensation is calculated as the sum of salary and bonus, and equity-based compensation is computed as the total compensation minus salary, bonus, other annual pay, and LTIP. We find similar results as using average employee measure.

SGA, and firm characteristics. A total of 72,427 firm-year observations has all of the necessary information to be included in our OLS regressions of average employee sample, covering 1976 to 2015.[8] In addition, we use the number of segments with different four-digit SIC codes as a measure of corporate diversification level. This information is obtained from the Compustat Business Segment data files. We exclude firm-years in which at least one segment is classified as being in the financial sector. We obtain acquisition information from the mergers and acquisitions database in SDC platinum. This data is available from 1976. All dollar amounts are adjusted to 1992 constant dollars using the consumer price index (CPI), which is collected from Bureau of Labor Statistics. Industry classifications are adopted from Fama-French 49 industry classification.

2.3.3. Descriptive statistics

Table 1 presents descriptive statistics for the variables used in our baseline regressions. Detailed variable definitions are in Appendix A. Using staff expenses to proxy for average employee pay leads to a smaller sample of 6,710 firm-year observations with a mean average employee pay of $34,403, while using SGA increases sample size to 72,427 firm-years with a mean average employee pay of $51,134. The standard deviations of cash flow volatility and unlevered stock return volatility are relatively large (at 0.022 and 0.017 respectively) compared to their mean (at 0.020 and 0.030). Fixed asset ratio is computed as gross property, plant, and equipment scaled by total assets, and the sample mean is 24.9%. Number of segments, R&D expenditures, advertising expenditures, and acquisition are variables of interest for channel testing. On

[8] We start from all Compustat firms dating back from 1950. Since we use acquisition (collected from SDC platinum) as a channel for risky investment and this data availability starts from 1976, our final sample for average employee pay covers from 1976 to 2015.

average, a firm-year has about 2 segments in our sample. We report the scaled values by total sales for the other three channels for risky investments.

Table 2 reports pairwise correlations for all variables of interest. We see that both of the scaled average employee pay variables are positively correlated with the risky investment measures, providing first evidence that there is a positive relation between human capital cost and investment riskiness. It also shows that the scaled average employee pay variables are negatively correlated with number of segments (corporate diversification), positively correlated with R&D expenditures, advertising expenditures, and acquisition, which is consistent with Hypothesis 3.

2.4. Empirical tests and results on investment riskiness and average employee pay

In this section, we describe our empirical results of the impact of investment riskiness on average employee pay.

2.4.1. Baseline regression

Our baseline regression for average employee pay sample is specified as the following. Our objective is to estimate the effect of investment riskiness on average employee pay.

$$\begin{aligned} EmployeePay_{it} &= \delta_0 + \delta_1 InvestmentRisk_{it} + \delta_2 MktCap_{it} + \delta_3 MtB_{it} + \delta_4 MktLev_{it} \\ &+ \delta_5 AvgSale_{it} + \delta_6 PPE_{it} + \delta_7 ROA_{it} + \delta_8 ROE_{it} + \delta_9 Cash_{it} \\ &+ \delta_{10} FirmAge_{it} + \varepsilon_{it} \end{aligned} \quad (5)$$

where $AvgSale_{it}$ is average sales per employee, PPE_{it} is fixed assets ratio, and $Cash_{it}$ is ratio of cash and marketable securities to the book value of assets. Detailed definitions of each variable are in Appendix A.

Regression results are presented in Table 3 Panel A. Column 1 and 2 are regressions with our two investment riskiness measures with staff expense as the dependent variable, and column 3 and 4 use SGA as the dependent variable, respectively. In column (1) and (2) where staff expense is used to calculate average employee pay, we observe that cash flow volatility is positively significant at 5% level while unlevered stock return volatility is insignificant. When SGA is used to proxy for average employee pay in model (3) and (4), we observe both cash flow volatility and unlevered stock return volatility are significantly positive at 1% level. The results are consistent with Hypothesis

2. Economically, if the cash flow volatility increases by one standard deviation (0.022, as reported in Table1 panel B), average employee pay calculated by staff expenses increases by 10.56%.[9] Therefore, starting with the average value of firm's sales at $2,308.94 million, the additional cost on staff expense per employee would be $49,000. With an average of 10,250 employees per firm, that is about $490 million increase in human capital cost, a tremendously significant amount economically.[10]

2.4.2. Robustness tests

The biggest endogeneity concern in the average employee sample would be whether the results are driven by employee skills. To be specific, firms that invest more in risky projects (for example, pharmaceutical companies, high technology firms, etc.) may hire more skilled workers, and skilled workers are better paid than unskilled workers. To address this problem, we first included a High-tech dummy as a control for skill in our baseline regressions as showed in Panel B Table 3. With industry and year fixed effect, cash flow volatility remains at 5% significance level and unlevered stock return volatility is now positively significant at 10% level as observed in column (1) and (2). In column (3) and (4), we observe both cash flow volatility and unlevered stock return volatility are significantly positive at 1% level. To further address the potential endogeneity concern of "pay for skills", we divide our sample into non-technology firms and technology firms. We consider employees in non-technology firms as unskilled workers. If our "pay for risk" argument is valid, we should observe the positive effect of investment riskiness on average employee pay still exists in the sample of non-technology firms. We follow

[9] Using SGA instead, the economic effect is one standard deviation increase in cash flow volatility (unlevered stock return volatility) is associated with 11.88% (9.54%) increase in human capital cost.

[10] One of the reasons for the large economic significance is that the standard deviations of the two volatility variables are almost as large as their mean, if not larger, as showed and discussed in table 1.

Carpenter and Petersen (2002) to identify high-tech industries by using first three-digit SIC code of 283, 357, 361, 362, 366, 367, 382, 384, 386, and 387. Results are presented in Table 4. Panel A uses staff expenses to calculate the dependent variable, and panel B uses SGA. Results are generally consistent with what we expected. To be specific, Panel A (staff expenses) shows that cash flow volatility displays a 5% significance level in the non-technology firms. Panel B (SGA) shows that both investment riskiness measures are 1% significant in non-technology subsamples. In other words, the positive relation between investment riskiness and human capital cost still exists in the unskilled workers group. Results are very much in line with our expectation.

Next, we use system GMM regressions to further account for concerns of omitted variables. Results are reported in Table 5. The regressions use one lag of average labor costs and deeper lags of all other right-hand-side variables. All regressions pass the AR(1) and AR(2) tests, along with the Hansen *J*-test and the difference-in Hansen *J*-test proposed by Eichenbaum, Hanse, and Singleton (1988). If our exogeneity assumptions are valid, then the residuals in first differences should be correlated, but the residuals in second differences should not be correlated. This is what is observed in the table. Further, the Hansen *J*-test of over-identification for the equation in differences and the difference-in-Hansen *J*-test of over-identification for the equation in levels are not rejected. This implies that we cannot reject the hypothesis that the lagged level and lagged difference instruments in the system GMM are exogenous. In all regressions, there is a statistically significant positive relation between proxies for risky investments and average employee pay. This effect is also economically significant compared to the coefficient estimates in panel B of Table 3. In comparison, when SGA is the proxy for average employee pay, the

significance level on coefficients of risky investments reduces to 10%. This suggests that the endogeneity concern is more of a problem when SGA serves as the proxy for average employee pay. This makes sense because SGA (Selling, general, and administration fees) is noisier than staff expenses when it comes to proxy for average employee pay.

Lastly, we follow Kale et al. (2016) to use a quasi-natural experiment to further explore the relation between human capital cost and investment riskiness. Hakobyan and McLaren (2016) shows that NAFTA decreases the outside employment opportunities only primarily in the manufacturing sector. The implementation of NAFTA therefore allow us to pin down the effect of investment riskiness to human capital cost in these industries. Specifically, if fewer job opportunities are available in the open market, an employee should be more sensitive to job loss (hence more sensitive to the firm's investment riskiness) since it would be even harder to find a replacement job. Therefore, we expect to see the passage of the regulation to enhance the human capital cost/investment riskiness relation. NAFTA implementation was an external shock to labor markets in many industries, as a result, we test our prediction at the industry level. We reconstruct our average employee sample to 2-digit SIC industry level in two approaches: aggregate all data and construct variables for the aggregate industry using industry mean and industry median. We further divide the sample into subsamples of manufacturing industries (SIC code 2000-3999) and non-manufacturing industries. Results are presented in Table 6. Our variable of interest is the interaction between investment riskiness measures (cash flow volatility and unlevered stock return volatility) and the dummy variable NAFTA (equals one in 1994 and onward, and 0 before). Consistent with our prediction that worse outside employment opportunities heightens the

role of investment riskiness on human capital cost, we observe that the coefficients on interaction terms are positive and significant in manufacturing industries for both approaches when using cash flow volatility to proxy for investment riskiness.[11] When unlevered stock return volatility is the measure, we see the coefficients on the interaction terms are insignificant but still positive in manufacturing industries, and negative and significant in non-manufacturing industries. Overall, the industry analysis of the impact of NAFTA provides further evidence for the relation of human capital cost and investment riskiness within manufacturing industries, which are affected by the regulation, while industries that are not affected present no effect or opposite effect.

2.4.3. Average employee's sensitivity to job loss

As discussed in the hypothesis section, employee's sensitivity towards unemployment risk should be a crucial factor in determining the relation between risky investment expenditures and average employee pay. Lower-pay employees should be more sensitive to unemployment risk than higher-pay employees because the disutility of losing a dollar is highest for people with little wealth. In addition, higher-pay employees possess more resources and therefore would have more choices once unemployed. Our Hypothesis 3 is based on this notion. We classify high-pay firms as those whose average employee pay is higher than sample median grouped by each fiscal year, whereas low-pay firms are those whose average employee pay is lower than sample median grouped by each fiscal year. Results are presented in Table 7. Panel A and B use staff expenses and SGA to calculate the dependent variable, respectively. Results are generally consistent with what we expected. To be specific, Panel A (staff expenses) shows that

[11] The negative and significant coefficients on cash flow volatility could be a result from aggregating sample into industry level. See Kale et at. (2016) and Ravenscraft (1983) for more discussions.

cash flow volatility only displays significance for low-pay firms, while it is insignificant in the high-pay firms. Panel B (SGA) shows that similar results are found for low-pay and high-pay subsamples. Both volatility measures are significantly positive at 1% level. However, the economic significances are higher in low-pay firms than in high-pay firms. Results are consistent with hypothesis 3.

2.5. Risky investment channels

To further study the impact of investment policy on human capital cost, we continue to examine the possible channels through which investment riskiness affects human capital cost. Following the literature we discussed before, we investigate four possible channels for investment riskiness: corporate diversification, R&D expenditures, advertising expenditures, and acquisition. We next test the direct relation between the four identified channels and firms' investment riskiness. We expect to see R&D expenditures, advertising expenditures, and acquisition as contributors to investment risk; diversification, on the other hand, reduces risk. The results are presented in Table 8. The signs for each channel are generally consistent with what we expected.

We move forward to test our last hypothesis after the possible channels are identified and empirically verified. Since diversification reduces investment risk, and R&D expenditures, advertising expenditures, and acquisition increase investment risk, we expect to see that the more diversified the firm, the less human capital cost; the higher level of R&D expenditures, advertising expenditures, or acquisition, the more human capital cost. Table 9 report the results for each channel within the average employee sample. Panel A reports the results using staff expense to calculate the dependent variable. Column 1 presents results using the diversification level as a channel. We included a squared variable of the number of segments in the regression because literature suggests the level of diversification could have a nonlinear relation with compensation (e.g., Rose and Shepard (1994) and Duru and Reeb (2002)). We observe that the number of segments is significantly negative at 10%, and R&D expenditures are positively significant at 1%, which is consistent with Hypothesis 3. However, neither advertising expenditures nor

acquisition show any significance. Panel B reports the results using SGA to calculate the dependent variable, and we observe significance in all four specifications. In particular, the coefficient on the number of segments is negative significantly at 1% level, while coefficients on R&D expenditures, advertising expenditures, and acquisition are all positively significant at 1% level, which are all consistent with Hypothesis 4.

2.6. Labor intensity's feedback effect

To this point, we have completed both a theoretical and an empirical examination of the positive relation between human capital cost and a firm's investment riskiness. There is still one important and intriguing question left to answer: how will the relation eventually feedback to the firm's investment policy? Specifically, once the human capital cost is raised because of the increased investment riskiness, how would the firm's future investment policy react to the increased human capital cost? More labor-intensive firms face greater aggregate human capital cost from increasing investment risk, therefore, firms with higher labor intensity would reduce risky investments in order to reduce human capital cost. As a result, we expect to see the feedback effect of increased human capital cost to reduce the amount of risky investments, i.e., more labor-intensive firms are expected to be associated with less risky investments. Following Dewenter and Malatesta (2001) and Agrawal and Matsa (2013), we construct the labor intensity variable as the ratio of labor and pension expenses to total assets. We next empirically test this prediction by regressing labor intensity on each of the four risky investment channels. To be specific, we expect to see that labor intensity is positively related to the number of business segments, and negatively related to R&D expenditures, advertising expenditures, and acquisition. Results are reported in Table 10. Panel A and Panel B presents firm-level results and industry-level results, respectively. We see from Panel A that labor intensity is negatively and significantly associated with R&D expenditures, advertising expenditures, and acquisition. Panel B shows that labor intensity is positively and significantly associated with the number of business segments and negatively and significantly associated with R&D expenditures. The feedback effect shows that firms do

adjust their investment policy according to the costs of human capital, and the results are generally consistent with our prediction.

2.7. Conclusion

A few recent financial studies start to pay attention to the role of human capital cost in corporate policies. In this paper, we argue that employees bear large human capital loss because of the risky investments that the firm is taking. In our theoretical framework, we consider the risk borne by the firm (so as employees) arising from the decision on risky investments, and we conduct empirical tests on the relation between investment riskiness and human capital cost. Our results indicate that increased human capital cost due to investment riskiness can significantly discourage firms' decisions on valuable investments, resulting in a potential underinvestment problem.

Using two measures for investment riskiness, cash flow volatility and unlevered stock return volatility, we find that investment riskiness is significantly positively correlated to average employee pay. In a panel sample of average employee information from 1976 to 2015, we show that the positive relation is both statistically and economically significant. For example, we document that for one standard deviation increase in cash flow volatility, average employee pay increases 10.56%. Our results are evident after we try our best attempts to control for endogeneity. We further show that average workers who are more sensitive to unemployment risk have a stronger effect in the compensation and investment riskiness relation.

Next, we explore four possible channels for investment riskiness: corporate diversification, R&D expenditures, advertising expenditures, and acquisition. We find further support for the positive relation between investment riskiness and human capital cost. In particular, we find a firm's R&D expenditures, advertising expenditures, and acquisition are positively related to human capital cost, while diversification level is

negatively related. Lastly, we finish the loop by providing evidence on the feedback effect of increased human capital cost on a firm's investment policy. We show that labor-intensive firms have significantly lower risky investments. Overall, our study contributes to the nascent but growing literature of the impact of the human capital on a firm's investment policy.

Appendix B. Variable Definition

Variable	Description (source of data)
Employee characteristics	
Staff expense per employee	Labor expense per employee divided by total sales. (Compustat)
SGA per employee	Selling, general, and administrative expense per employee divided by total sales. (Compustat)
Number of employees	Total number of employees in a firm-year. (Compustat)
Proxies for risky investments	
Cash flow volatility	Standard deviation of the ratio of operating income after depreciation to assets over the eight quarters ending in each fiscal year. (Compustat)
Unlevered stock return volatility	Standard deviation of unlevered daily stock returns in past 2 years. (CRSP/Compustat)
No. of segments	Number of segments with different four-digit SIC code. (Compustat/Segment)
R&D	Ratio of research and development expense to total sales. (Compustat)
Advertisement	Ratio of advertisement expenditure to total sales. (Compustat)
Acquisition	Ratio of total value of acquisition in a year to total sales. (SDC/Compustat)
Proxies for labor intensity	
Labor intensity	Ratio of labor and pension expenses to total asset. Measure is based on the three digit North American Industry Classification System (NAICS). (Compustat)
Control variables	
Market Capitalization	Logarithm of market capitalization in constant dollars using the CPI with base year 1992. (Compustat)
Average sales per employee	Amount of total sales divided by number of employees. (Compustat)
Market leverage	Total debt divided by the market value of assets (book value of assets – book value of equity + market value of equity). (Compustat)
Market-to-book ratio	Ratio of book assets plus the difference between the market and book values of equity to the book value assets. (Compustat)
Marginal tax rate	Present value of current and expected future taxes paid on an additional dollar earned today. (Database of marginal tax rates provided by John Graham)
CAPEX	Ratio of capital expenditures to sales. (Compustat)
Fixed assets ratio	Ratio of net property, plant, and equipment to the book value of assets. (Compustat)
ROA	Ratio of operating income before depreciation to the book value of assets. (Compustat)
ROE	Ratio of operating income before depreciation to the book value of equity. (Compustat)
Cash	Ratio of cash and marketable securities to the book value of assets. (Compustat)

Firm age	Number of years from the first year recorded on the database to year t. (Compustat)
One-year return to price shareholder	Ratio of difference between stock price at year t plus dividend per share and stock at year t-1 to stock price at year t-1. (Compustat)
High-tech dummy	Defined as an indicator variable which takes a value of one if a firm is involved in high-tech industries, and zero otherwise. We follow Carpenter and Petersen (2002) to identify high-tech industries by using first three-digit SIC code of 283, 357, 361, 362, 366, 367, 382, 384, 386, and 387.
NAFTA	Dummy variable equals to one if fiscal year is 1994 and onward, zero otherwise.

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Table 2.1 Descriptive Statistics

We report descriptive statistics for the average employee sample. We require firm-years to be on the Compustat database and have cash flow volatility, unlevered stock return volatility, SGA (Selling, General and Administrative expense), and firm data. The full employee sample covers period from 1976 to 2015. All continuous variables are winsorized at the 1% and 99% percentiles of their distributions. All variables are defined in the Appendix A.

	N	Mean	Std Dev	Min	Median	Max
Staff expense per employee $thousand	6,710	34.403	19.593	1.553	34.737	93.166
Staff expense per employee	6,710	0.0002	0.0004	0.000	0.0002	0.003
SGA per employee $thousand	72,427	51.134	45.172	1.849	36.302	236.586
SGA per employee	72,427	0.0008	0.002	0.000	0.0002	0.010
Cash flow volatility	72,427	0.020	0.022	0.002	0.013	0.128
Unlevered stock return volatility	72,427	0.030	0.017	0.008	0.026	0.090
No. of segments	61,042	1.498	0.996	1.000	1.000	10.000
CAPEX	71,771	0.065	0.085	0.003	0.040	0.589
R&D	72,427	0.126	4.933	0.000	0.026	976.500
Advertisement	32,516	0.031	0.042	0.000	0.016	0.256
Acq. amount	72,427	0.037	0.167	0.000	0.000	1.285
Sales $mm	72,427	2,308	7,318	6.232	215.886	53,674
High-tech dummy	72,427	0.312				
Average sales per employee $thousand	72,427	173.063	147.578	20.433	134.491	967.888
Market leverage	72,427	0.148	0.153	0.000	0.105	0.629
Market-to-book	72,427	1.905	1.402	0.601	1.437	8.872
Fixed asset ratio	72,427	0.249	0.180	0.014	0.210	0.806
Market capitalization	72,427	5.440	2.108	1.147	5.292	10.863
ROA	72,427	0.103	0.137	-0.484	0.122	0.378
ROE	72,427	0.237	0.389	-1.451	0.250	1.908
Cash	72,427	0.181	0.191	0.001	0.108	0.794
Firm age	72,427	10.954	8.809	1.000	8.000	46.000
Number of employees thousands	72,427	10.250	25.064	0.107	1.520	165.000

Table 2.2 Correlations Matrix

We report Pearson correlation coefficients between human capital cost and proxies of investments riskiness. We require firm-years to be on the Compustat database and have cash flow volatility, unlevered stock return volatility, SGA (Selling, General and Administrative expense) and firm data. The full employee sample covers period from 1976 to 2015. ***, **, * indicates significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(1) Staff expense per employee	1							
(2) SGA per employee	0.758***	1						
(3) Cash flow volatility	0.329***	0.439***	1					
(4) Unlevered Stock return volatility	0.446***	0.468***	0.438***	1				
(5) No. of segments	-0.199***	-0.196***	-0.177***	-0.273***	1			
(6) R&D	0.087***	0.077***	0.024***	0.015***	-0.010**	1		
(7) Advertisement	0.131***	0.207***	0.221***	0.098***	-0.027***	0.135***	1	
(8) Acq. amount	0.023*	0.082***	0.055***	0.090***	-0.044***	0.002	0.064***	1

Table 2.3 Effects of Investment Riskiness on Average Employee Pay

The dependent variables are two proxies for average employee pay: staff expense per employee and SGA (Selling, General and Administrative expense) per employee. Regressions in Panel A include firm fixed effects and year fixed effects, regressions in Panel B include a dummy variable for technology firms and year fixed effects. We use cash flow volatility and unlevered stock return volatility as two proxies for risky investments. The coefficients are reported in terms of percentage. All continuous variables are winsorized at the 1% and 99% percentiles of their distributions. *T*-statistics (in parentheses) are computed using robust standard errors corrected for clustering of observations at the firm level. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively.

	Staff expense per employee		SGA per employee	
	(1)	(2)	(3)	(4)
Panel A. Firm-year fixed effects				
Cash flow volatility	0.096**		0.432***	
	(2.41)		(9.56)	
Unlevered stock return volatility		-0.009		0.449***
		(-0.12)		(6.71)
Market Capitalization	-0.005***	-0.006***	-0.018***	-0.018***
	(-4.84)	(-4.80)	(-16.82)	(-15.96)
Market-to-book	0.003***	0.003***	0.014***	0.015***
	(3.20)	(3.61)	(17.30)	(18.18)
Market leverage	-0.026***	-0.027***	-0.104***	-0.092***
	(-3.85)	(-3.57)	(-16.80)	(-14.12)
Average sales per employee	0.000	0.000	-0.000***	-0.000***
	(0.34)	(0.29)	(-3.46)	(-3.28)
Fixed asset ratio	0.006	0.006	-0.014	-0.012
	(0.78)	(0.78)	(-1.48)	(-1.27)
ROA	-0.066***	-0.070***	-0.393***	-0.406***
	(-4.22)	(-4.37)	(-27.71)	(-28.84)
ROE	0.002	0.003	0.029***	0.028***
	(1.37)	(1.44)	(11.18)	(11.02)
Cash	0.035***	0.036***	0.133***	0.134***
	(3.35)	(3.43)	(17.24)	(17.38)
Firm age	0.000	0.000	0.005***	0.005***
	(0.06)	(0.03)	(3.55)	(3.62)
Year fixed effect	Yes	Yes	Yes	Yes
Firm fixed effect	Yes	Yes	Yes	Yes
Adjusted R-squared	0.906	0.905	0.824	0.823
Number of observations	6,710	6,710	72,427	72,427

Table 2.3 – *continued*

	Staff expense per employee		SGA per employee	
	(1)	(2)	(3)	(4)
Panel B. Industry-year fixed effects				
Cash flow volatility	0.149**		0.678***	
	(2.10)		(13.67)	
Unlevered stock return volatility		0.284**		0.410***
		(2.16)		(5.16)
Market Capitalization	-0.007***	-0.007***	-0.016***	-0.016***
	(-13.13)	(-11.49)	(-30.25)	(-28.08)
Market-to-book	0.007***	0.007***	0.020***	0.022***
	(6.22)	(6.14)	(23.33)	(25.40)
Market leverage	-0.047***	-0.040***	-0.136***	-0.132***
	(-6.00)	(-4.92)	(-21.98)	(-19.51)
Average sales per employee	0.000***	0.000***	0.000*	0.000**
	(5.72)	(5.73)	(1.89)	(2.20)
Fixed asset ratio	0.005	0.004	0.011**	0.012**
	(1.15)	(0.97)	(2.12)	(2.44)
ROA	-0.169***	-0.166***	-0.606***	-0.629***
	(-8.87)	(-8.42)	(-43.28)	(-44.41)
ROE	0.006**	0.005**	0.044***	0.043***
	(2.17)	(2.12)	(15.62)	(15.15)
Cash	0.044***	0.044***	0.133***	0.131***
	(2.88)	(2.89)	(19.93)	(19.54)
Firm age	0.00	0.00	-0.000***	-0.001***
	(-0.36)	(-0.07)	(-4.54)	(-4.85)
High-tech dummy	0.004	0.004	0.018***	0.016***
	(1.36)	(1.30)	(8.20)	(7.21)
Year fixed effect	Yes	Yes	Yes	Yes
Industry fixed effect	Yes	Yes	Yes	Yes
Adjusted R-squared	0.45	0.45	0.537	0.531
Number of observations	6,710	6,710	72,427	72,427

Table 2.4 Robustness test on Non-high-tech vs. High-tech

The dependent variables are two proxies for average employee pay: staff expense per employee (Panel A) and SGA (Selling, General and Administrative expense) per employee (Panel B). We separate full sample into high-tech firms and non-high-tech firms by high-tech dummy. Regressions include firm fixed effects and year fixed effects. The coefficients are reported in terms of percentage. All continuous variables are winsorized at the 1% and 99% percentiles of their distributions. *T*-statistics (in parentheses) are computed using robust standard errors corrected for clustering of observations at the firm level. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively.

	Non-high-tech firms		High-tech firms	
	(1)	(2)	(3)	(4)
Panel A. Dependent variable = Staff expense per employee				
Cash flow volatility	0.107**		0.100	
	(2.34)		(1.19)	
Unlevered stock return volatility		0.031		-0.074
		(0.42)		(0.32)
Controls	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes
Adjusted R-squared	0.904	0.903	0.913	0.912
Number of observations	5,471	5,471	1,239	1,239
Panel B. Dependent variable = SGA per employee				
Cash flow volatility	0.473***		0.374***	
	(9.17)		(4.54)	
Unlevered stock return volatility		0.505***		0.400***
		(6.28)		(3.28)
Controls	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes
Adjusted R-squared	0.823	0.822	0.823	0.822
Number of observations	49,860	49,860	22,567	22,567

Table 2.5 System GMM Estimation of the Effects of Investments Riskiness on Average Employee Pay
The table reports the results of system GMM estimation of the effects of investment riskiness on average employee pay. The dependent variables are two proxies for average employee pay: staff expense per employee and SGA (Selling, General and Administrative expense) per employee. All control variables are considered to be endogenous with the exception of the year and industry dummy variables. We also include first lag of dependent variable in the dynamic GMM model. AR(1) and AR(2) are tests for first-order and second-order serial correlation in the first differenced residuals with the null hypothesis of no serial correlation. The null hypothesis of the Hansen test of overidentification is that all instruments are valid. The null hypothesis of the difference-in- Hansen test of exogeneity is that the instruments used for the equations in levels are exogenous. The coefficients are reported in terms of percentage. All continuous variables are winsorized at the 1% and 99% percentiles of their distributions. *T*-statistics (in parentheses) are computed using robust standard errors corrected for clustering of observations at the firm level. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively.

	Staff expense per employee		SGA per employee	
	(1)	(2)	(3)	(4)
Adjusted labor costs (one lag)	0.505***	0.584***	0.565***	0.423***
	(3.85)	(3.63)	(14.33)	(6.56)
Cash flow volatility	0.335***		0.619**	
	(2.59)		(2.24)	
Unlevered stock return volatility		0.240*		0.648**
		(1.91)		(2.18)
Market Capitalization	-0.001	-0.004	0.008*	-0.005
	(-0.10)	(-1.49)	(1.69)	(-0.98)
Market-to-book	0.001	0.003	-0.017***	-0.006
	(0.31)	(1.64)	(-4.24)	(-1.04)
Market leverage	-0.012	0.005	-0.020	-0.027
	(-0.53)	(0.38)	(-0.73)	(-0.71)
Average sales per employee	0.000	0.000	-0.000	-0.000
	(0.82)	(0.71)	(-0.70)	(-1.01)
Fixed asset ratio	0.003	0.037	-0.037	-0.008
	(0.08)	(1.43)	(-0.57)	(-0.08)
ROA	-0.122*	-0.046*	-0.236***	-0.143***
	(-1.85)	(-1.66)	(-2.88)	(-2.67)
ROE	0.005	0.002	0.001	-0.000
	(0.28)	(0.53)	(0.09)	(-0.00)
Cash	0.003	0.022**	0.056	0.068
	(0.11)	(2.29)	(1.46)	(1.39)
Firm age	-0.000	-0.000	0.000	0.000
	(-0.34)	(-0.25)	(1.29)	(0.75)
High-tech dummy	-0.037	-0.075	-0.203	-0.830*
	(-0.13)	(-1.26)	(-1.01)	(-1.73)
AR(1) test (p-value)	0.010	0.018	0.000	0.000
AR(2) test (p-value)	0.922	0.488	0.115	0.192
Hansen J-statistic (p-value)	1.000	1.000	0.107	0.292
Diff-in-Hansen J-statistic (p-value)	1.000	1.000	0.155	0.883
Number of observations	5,642	5,642	62,748	62,748

Table 2.6 Natural Experiment: the Influence of NAFTA on the Human Capital/Investment Risk Relation

This table presents industry level results of the effect of NAFTA on the human capital/investment riskiness relation. The dependent variable is staff expense per employee. Column 1-4 calculate industry level variables using industry mean; column 5-8 calculate industry level variables using industry median. Manufacturing industries have SIC codes between 2000 and 3999. Non-manufacturing industries have SIC codes smaller than 2000, and larger than 3999. NAFTA equals one for observations in year 1994 and onwards, and equals zero otherwise. Regressions include all control variables, industry fixed effects and year fixed effects. The coefficients are reported in terms of percentage. All continuous variables are winsorized at the 1% and 99% percentiles of their distributions. *T*-statistics (in parentheses) are computed using robust standard errors corrected for clustering of observations at the industry level. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively.

on	Industry mean				Industry median			
	Mfg. Industries		Non-mfg. Industries		Mfg. Industries		Non-mfg Industries	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Cash flow volatility	-6.827**		1.536		-2.305**		5.925	
	(-2.59)		(0.20)		(-2.20)		(0.71)	
Cash flow vol. * NAFTA	6.174**		5.015		6.836**		2.101	
	(2.37)		(0.67)		(2.36)		(0.26)	
Unl. stock return vol.		0.0534		19.74**		-2.758		17.86**
		(0.01)		(2.52)		(-1.43)		(2.78)
Unl. stock return vol.* NAFTA		4.712		-20.20**		4.576		-19.94***
		(1.25)		(-2.34)		(1.41)		(-2.97)
NAFTA	0.574	0.681	-0.366**	0.103	0.280	0.309	-0.129	0.166
	(1.35)	(1.60)	(-2.16)	(0.69)	(1.04)	(1.20)	(-0.76)	(1.32)
Adjusted R-square	0.483	0.475	0.413	0.429	0.533	0.530	0.370	0.378
Number of observations	587	587	635	635	587	587	635	635

Table 2.7 Sensitivity to Job Loss Subsample Analysis

The dependent variables are two proxies for average employee pay: staff expense per employee (Panel A) and SGA (Selling, General and Administrative expense) per employee (Panel B). We compute the median values of staff expense per employee and SGA per employee by year, and separate the full sample into high pay (above-median) and low pay (below-median) groups using the median value of staff expense per employee and SGA per employee, respectively. Regressions include all control variables, firm fixed effects and year fixed effects. The coefficients are reported in terms of percentage. All continuous variables are winsorized at the 1% and 99% percentiles of their distributions. *T*-statistics (in parentheses) are computed using robust standard errors corrected for clustering of observations at the firm level. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively.

	Low-pay firms		High-pay firms	
	(1)	(2)	(3)	(4)
Panel A. Dependent variable = Staff expense per employee				
Cash flow volatility	0.125**		0.066	
	(2.51)		(1.10)	
	[17.29%]			
Unlevered stock return volatility		-0.006		-0.154
		(-0.08)		(-0.94)
Controls	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes
Adjusted R-squared	0.883	0.881	0.934	0.934
Number of observations	3,544	3,544	3,166	3,166
Panel B. Dependent variable = SGA per employee				
Cash flow volatility	0.166***		0.449***	
	(4.26)		(7.61)	
	[12.28%]		[9.45%]	
Unlevered stock return volatility		0.165***		0.498***
		(2.90)		(5.13)
		[9.99%]		[7.78%]
Controls	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes
Adjusted R-squared	0.812	0.812	0.831	0.831
Number of observations	29,749	29,749	42,678	42,678

Table 2.8 Channels for Investment Riskiness

We test four possible channels for investment riskiness. The channels we investigate are number of segments, R&D expenditures, advertising expenditures and total value of all acquisition deals in a year. The coefficients are reported in in terms of percentage. All continuous variables are winsorized at the 1% and 99% percentiles of their distributions. *T*-statistics (in parentheses) are computed using robust standard errors corrected for clustering of observations at the firm level. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively.

	Cash flow volatility	Unlevered stock return volatility
	(1)	(2)
No. of segments	-0.001***	-0.00001
	(-3.69)	(-0.11)
R&D	0.008**	0.001
	(2.35)	(0.67)
Advertisement	0.071***	0.005
	(10.24)	(1.55)
Acq. amount	-0.0004	0.002***
	(-0.44)	(5.14)
Market Capitalization	-0.003***	-0.003***
	(-19.77)	(-34.77)
Market leverage	-0.017***	-0.035***
	(-10.99)	(-36.96)
Market-to-book	0.004***	0.001***
	(19.86)	(14.10)
ROA	-0.049***	-0.027***
	(-21.04)	(-23.26)
Fixed asset ratio	0.001	0.001
	(0.30)	(0.51)
Cash	0.001	0.004***
	(0.52)	(4.75)
Year fixed effects	Yes	Yes
Industry fixed effects	Yes	Yes
Adjusted R-squared	0.330	0.613
Number of observations	27,428	27,428

Table 2.9 Effects of Investment Risk Channels on Average Employee Pay

We test four channels through which investment riskiness may affect average employee pay. The channels we investigate are number of segments, R&D expenditures, advertising expenditures and total value of all acquisition deals in a year. The dependent variable in Panel A is staff expense per employee, in Panel B is SGA per employee. All regressions include firm fixed effects and year fixed effects. The coefficients are reported in terms of percentage. All continuous variables are winsorized at the 1% and 99% percentiles of their distributions. *T*-statistics (in parentheses) are computed using robust standard errors corrected for clustering of observations at the firm level. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
Panel A. Dep. Var = Staff expense per employee				
No. of segments	-0.004*			
	(-1.87)			
No. of segments square	0.0005*			
	(1.87)			
R&D		0.0003***		
		(59.16)		
Advertisement			0.081	
			(1.15)	
Acq. amount				-0.002
				(-0.75)
Market Capitalization	-0.008***	-0.006***	-0.006***	-0.006***
	(-4.73)	(-5.01)	(-4.06)	(-4.89)
Market-to-book	0.003***	0.003***	0.003**	0.003***
	(3.36)	(3.68)	(2.48)	(3.66)
Market leverage	-0.037***	-0.026***	-0.023**	-0.027***
	(-4.02)	(-4.00)	(-2.45)	(-3.90)
Average sales per employee	-0.000	0.000	0.000	0.000
	(-0.66)	(1.07)	(1.25)	(0.27)
Fixed asset ratio	0.013	0.007	0.005	0.006
	(1.21)	(0.92)	(0.43)	(0.79)
ROA	-0.075***	-0.068***	-0.071***	-0.070***
	(-3.82)	(-4.33)	(-2.87)	(-4.40)
ROE	0.003	0.003	0.004*	0.003
	(1.12)	(1.44)	(1.70)	(1.44)
Cash	0.044***	0.036***	0.029**	0.036***
	(3.11)	(3.40)	(2.34)	(3.43)
Firm age	0.000	0.001	0.000	0.000
	(0.17)	(0.46)	(0.10)	(0.04)
Year fixed effects	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes
Adjusted R-squared	0.896	0.910	0.908	0.905
Number of observations	4,580	6,710	2,975	6,710

Table 2.9 – *continued*

	(1)	(2)	(3)	(4)
Panel B. Dep. Var = SGA per employee				
No. of segments	-0.012***			
	(-5.12)			
No. of segments square	0.002***			
	(5.07)			
R&D		0.001***		
		(4.32)		
Advertisement			0.659***	
			(7.93)	
Acquisition				0.017***
				(4.86)
Market Capitalization	-0.020***	-0.019***	-0.020***	-0.019***
	(-17.28)	(-17.58)	(-12.50)	(-17.44)
Market-to-book	0.015***	0.015***	0.015***	0.015***
	(18.69)	(19.18)	(12.16)	(18.76)
Market leverage	-0.115***	-0.109***	-0.104***	-0.108***
	(-17.84)	(-17.54)	(-12.31)	(-17.38)
Average sales per employee	-0.000***	-0.000***	0.00	-0.000***
	(-4.07)	(-3.01)	(-0.17)	(-3.08)
Fixed asset ratio	-0.007	-0.013	0.018	-0.013
	(-0.69)	(-1.28)	-1.36	(-1.33)
ROA	-0.408***	-0.414***	-0.356***	-0.415***
	(-27.54)	(-29.30)	(-18.27)	(-29.30)
ROE	0.030***	0.029***	0.028***	0.029***
	(10.36)	(11.36)	(8.06)	(11.38)
Cash	0.140***	0.135***	0.133***	0.132***
	(17.72)	(17.39)	(13.58)	(17.02)
Firm age	0.004**	0.005***	0.005*	0.005***
	(2.57)	(3.69)	(1.94)	(3.60)
Year fixed effects	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes
Adjusted R-squared	0.828	0.823	0.855	0.823
Number of observations	61,042	72,427	32,516	72,427

Table 2.10 Effect of Labor intensity on Investment Riskiness Channels (Feedback Effect)

We test the feedback effect of investment riskiness on human capital cost, in particular, effect of investment riskiness channels on labor intensity. The channels we investigate are number of segments, R&D expenditures, advertising expenditures and total value of all acquisition deals in a year. The variable of interest in Panel A is labor intensity at firm level, in Panel B is labor intensity at industry level by NAICS 3 digit. The coefficients are reported in terms of percentage. All continuous variables are winsorized at the 1% and 99% percentiles of their distributions. *T*-statistics (in parentheses) are computed using robust standard errors corrected for clustering of observations at the firm level. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively.

	No. of segments	R&D	Advertisement	Acq. amount
	(1)	(2)	(3)	(4)
Panel A: Labor intensity at firm level using staff expense				
Labor intensity	0.305	-0.031*	-0.013**	-0.026**
	(1.02)	(-1.78)	(-2.38)	(-2.54)
Log(sales)	0.199***	-0.000	0.001	-0.006***
	(3.96)	(-0.26)	(0.49)	(-2.71)
Market-to-book	-0.145**	0.004	0.004**	0.010**
	(-2.44)	(1.47)	(2.17)	(2.14)
Book leverage	-0.021	-0.000	0.005	-0.009
	(-0.10)	(-0.00)	(0.45)	(-0.46)
Surplus cash	-1.021**	-0.017	-0.024**	0.132**
	(-2.72)	(-0.63)	(-2.11)	(2.26)
Sales growth	0.023	0.043	-0.001	-0.027**
	(0.20)	(1.34)	(-0.12)	(-2.20)
Annual stock return	0.111**	0.006	-0.006***	-0.005**
	(2.13)	(1.07)	(-2.94)	(-2.25)
Free cash flow	-0.107	-0.036	0.006	-0.312**
	(-0.37)	(-0.78)	(0.42)	(-2.24)
Year fixed effects	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes
Adjusted R-squared	0.372	0.038	0.427	0.518
Number of observations	1,846	3,130	1,192	3,130

Table 2.10 – *continued*

	No. of segments	R&D	Advertisement	Acq. amount
	(1)	(2)	(3)	(4)
Panel B: Labor intensity at industry level using NAICS 3-digit				
Labor intensity	0.717***	-0.066***	-0.013	-0.017
	(2.76)	(-2.75)	(-0.86)	(-0.75)
Log(sales)	0.124***	-0.008***	0.001***	-0.004***
	(12.72)	(-16.58)	(2.86)	(-8.48)
Market-to-book	-0.040***	0.014***	0.003***	0.015***
	(-8.41)	(16.43)	(6.30)	(12.86)
Book leverage	0.130**	-0.069***	-0.013***	0.004
	(2.53)	(-11.34)	(-3.01)	(0.76)
Surplus cash	-0.524***	0.290***	-0.026***	0.002
	(-10.78)	(26.52)	(-5.06)	(0.17)
Sales growth	-0.113***	0.024***	0.011***	0.053***
	(-8.04)	(7.12)	(5.43)	(11.41)
Annual stock return	0.049***	-0.010***	-0.003***	0.006***
	(7.80)	(-11.28)	(-5.99)	(3.30)
Free cash flow	0.264***	-0.586***	-0.046***	0.003
	(4.85)	(-42.13)	(-8.22)	(0.27)
High-tech dummy	-0.092***	0.051***	-0.012***	-0.00
	(-4.07)	(19.84)	(-7.90)	(-0.18)
Year fixed effects	Yes	Yes	Yes	Yes
Adjusted R-squared	0.114	0.44	0.096	0.043
Number of observations	45,562	54,499	23,292	54,499

Chapter 3
Lending Relationships and the "Hold-up" Effect: Evidence from Bank Mergers

3.1. Introduction

Considerable work has been done on relationship lending to test whether firms benefit from lending relationships by obtaining lower interest rates, fewer covenants/restrictions, and better credit availability. On the one hand, the literature on the financial intermediation emphasizes that banks can collect valuable information about borrowers (Diamond, 1984, 1991). Boot (2000) argues that relationship lending can pave the way for more informative credit contracting decisions based on a better exchange of information, and also increase the availability of credit to information-sensitive borrowers. This argument has received strong empirical support.[1] On the other hand, the dark sides of the lending relationship are also broadly studied. There are two primary costs of the lending relationship: the "soft-budget" constraint and the "hold-up" problem. Seniority of bank debt may facilitate timely intervention. Bank loans commonly include covenants that give lenders significant control rights over borrower actions (Smith and Warner, 1979; Berlin and Mester, 1992). A strong relationship allows the bank to create an "information monopoly" in that it is costly for the borrower to switch lenders. Banks can also charge high loan rates to borrowers who suffer the most from information asymmetries (Sharpe, 1990; Rajan, 1992). Houston and James (1996) find that multiple banking relationships or borrowing in public debt markets may be two ways to mitigate potential "hold-up" problems. They further find that the existence of other private debt does not enable firms

[1] See, e.g. Petersen and Rajan, 1994, 1995; Berger and Udell, 1995; Cole, 1998; D'Auria, Foglia and Reedtz, 1999; Degryse and Van Cayseele, 2000; Boot, 2000; Bharath, Dahiya, Saunders and Srinivasan, 2011.

to avoid "hold-up" problems. [2] Dass and Massa (2011) argue that a strong relationship makes the bank a potentially more informed agent in the equity market. This information asymmetry increases adverse selection for the other market participants and lowers the firm's stock liquidity.

Although the literature generally finds that firms benefit from a strong bank-borrower relationship, it has not reached a consensus on how the borrower benefits from a relationship, and especially the length of the relationship. Specifically, Petersen and Rajan (1994) find that the benefits of lending relationship come mainly from better credit availability instead of lower interest rates (see, also, Lehmann and Neuberger, 2001). Berger and Udell (1995) find that the loan rate is negatively related to the length of the relationship, with the result holding only for firms with total assets above $500,000. Cole (1998) and Elsas and Krahnen (1998) find that relationship length is unimportant to credit supply. D'Auria, Foglia and Reedtz (1999) find a significant positive relation between the loan rate and the length of the relationship (see, also, Angelini, Di Salvo and Ferri, 1998; Degryse and Van Cayseele, 2000). Few papers look at relationship switching. Farinha and Santos (2002) find that the likelihood of a firm substituting from a single relationship to multiple relationships increases with the duration of the relationship. This substitution is more likely to occur for firms with more growth opportunities and for firms with poor performance. Overall, the rather mixed results in the literature suggest that the effects of relationship lending are dynamic and the likelihood of a bank acquiring an "information monopoly" increases with the length of the single relationship.

[2] The result is consistent with Petersen and Rajan (1994) and Bharath et al., 2011. It seems that the multiplicity of relationships matter more than the multiplicity of creditors.

Government deregulation of interstate banking in the 1990s sparked a rapid increase in bank mergers (consolidation) in the United States. Berger and Udell (2002) point out that researchers should view relationship lending in an organization context. They argue that relationship lending is associated with a fundamentally different lending process than transactions-based lending technologies. Cole, Goldberg and White (2004) find that large banks use financial statements in the loan decision process, whereas small banks rely more on "soft" information about the borrower. Relationship loans tend to be more on "soft" information and this may create more contracting problems for large institutions. Accordingly, large banks are less likely to make relationship loans to small businesses.[3] Studies of the effects of bank consolidation on small business lending generally find that mergers and acquisitions involving large banks reduces credit supply to small business, although consolidations between small banks often increases small business lending (Peek and Rosengren, 1998; Strahan and Weston, 1998; Berger et al., 1998; Degryse, Masschelein, and Mitchell, 2005). However, these studies focus on small business lending only. Therefore, their results cannot provide a complete answer to the question of whether a strong relationship with a merging bank influences the effect of a bank merger on borrowers. Although the literature finds that bank mergers improve bank operating efficiency (see, e.g. Akhavein et al., 1997; Calomiris, 1999; Berger et al., 1999; DeYoung et al, 2009), there are a couple of important unanswered questions. First, are the gains passed on to borrowers? Second, what is the effect of relationship lending in this context? Do relationship borrowers benefit more from mergers, or do consolidating banks use their

[3] The dynamic changes of lending behavior depend not only on the bank size and organizational complexity, but also on other factors. For example, increased interbank competition will increase relationship lending while increased capital market competition will reduce total bank lending as well as relationship lending (Boot and Thakor (2000)).

enhanced market power to extract rents from relationship borrowers? These questions have not been answered directly in the literature. In this paper, we take a step to fill this gap by examining the influence of bank mergers on relationship borrowers. Our empirical analysis focuses on publicly traded firms and bank mergers in the US. We use several measures of the strength of the bank relationship and use a difference-in-differences methodology to provide answers to the question we pose.

Although the literature does not directly test how relationship borrowers fare in bank mergers, empirical results provide important insights. The literature emphasizes that market overlap, merger size and type, and customer type are important factors influencing the consequences of bank mergers. Sapienza (2002) makes a distinction between in-market mergers (mergers between banks that operate in the same local market) and out-of-market mergers (mergers between banks that previously operate in different geographical areas). Using Italian data, she finds that although borrowers on average benefit from lower interest rates after a bank merger, the magnitude of the reduction is significantly reduced by a large market overlap among merging banks. In particular, interest rates increase after mergers when a large bank acquires a small out-of-market target bank. Using aggregate bank data in the US, Erel (2011) studies the effect of bank mergers on loan prices. She finds that the average reduction in loan spreads is both larger and more persistent for the non-mega acquirers. Mega acquirers reduce spreads within the first two years after the mergers, but this decline is reversed in the third year. The reduction in spreads is substantially larger after in-market mergers in which the merging banks have some geographical overlap of markets before the merger. However, if the overlap is sufficiently extensive so that

concentration of banking markets substantially increases the market power of the acquirer, the effect reverses and spreads on loans significantly increase after mergers.

Although these papers do not consider the effect of relationship strength in their empirical analysis, their results suggest that the consolidated bank charges higher loan rates when the market power effect is sufficiently large. Interestingly, Sapienza argues that the consolidated bank extracts more rents from borrowers with four to eight lending relationships; presumably because the reduction in outside options due to bank consolidation has the most significant impact on such borrowers. We posit that the consolidated bank will extract more rents from relationship borrowers. First, shocks to banks generally have larger effects on relationship borrowers (see, e.g. Kang and Stulz, 2000; Chava and Purnanandam, 2011; Chen and Vashishtha, 2017). Further, a strong relationship can create an "information monopoly" that could increase borrower switching costs (see, e.g. Ongena and Smith, 2001; Dass and Massa, 2011). Thus, we anticipate that bank mergers—and especially those more likely to reduce borrowing options—will more negatively affect relationship borrowers.

In the empirical analysis, we study the loan contracts of continued borrowers in a five years window before bank mergers and a two years window after mergers. We find that compared to non-relationship borrowers, the merged bank will charge higher loan spreads to relationship borrowers after a bank merger ("hold-up" effect). This result could be driven by the inefficient lending decisions of target banks before merger. In particular, Panetta, Schivardi and Shum (2009) show that bank mergers improve a bank's ability to screen borrowers. To rule out this alternative explanation for our results, we separate our sample into post-merger loans to continued acquirer borrowers (borrowers who borrowed

from the acquiring bank both before and after the merger), continued target borrowers (borrowers who borrowed from the target bank both before and after the merger), and switching target borrowers (borrowers who borrowed from the target bank before the merger and start to borrow from the acquiring bank after the merger). We find the "hold-up" effect in last two samples, and the effect is stronger in the sample of continued target borrowers.

Few papers directly investigate how a bank merger influences the likelihood that a borrower will continue to borrow from the consolidated bank. Degryse, Masschelein, and Mitchell (2011) find that target bank borrowers have a higher discontinuation rate than do non-merging bank borrowers, whereas the acquiring bank borrowers have a lower discontinuation rate than non-merging bank borrowers. In addition, borrowers with high relationship intensities (measured as the proportion of the firm's total loans borrowed from that bank) with the target bank are more likely to be dropped. Karceski, Ongena and Smith (2005) also find that bank mergers lead to higher relationship exit rates among target borrowers. Interestingly, both papers suggest that target bank borrower drops may not be efficient since the target borrowers dropped on average perform better than otherwise similar dropped borrowers of non-merging banks over the three years after the drop. Our results indicate that these drops may be driven by borrowers' choices rather than banks'. We find that the consolidated bank charges higher loan spread to relationship borrowers compared to non-relationship borrowers ("hold-up" effect) and the effect is more pronounced for target bank borrowers. The consolidated bank charges even higher loan spreads to both types of target borrowers with multiple but not many banking relationships. Therefore, if the switching costs for these borrowers are less than the higher loan spread

charged post-merge, they would have switched to another bank. However, if the switching cost is high, then they are "held-up" by the current relationship.

As discussed above, the market power effect of a bank merger is larger in mergers of banks with large market overlap, and in mergers when a large bank acquires a small bank (Hankir, Rauch and Umber (2011)). Consistently, we find that the magnitude of the "hold-up" effect is more pronounced in these cases. Moreover, we also examine non-price terms of loans. Generally speaking, we find that the consolidated bank commits to monitor relationship borrowers more compared to non-relationship borrowers after the bank merger. The net effect depends on bank size, merger type, and market overlap between banks, and is different across the type of borrowers. Lastly, we examine the interaction between relationship strength, bank merger, and the availability of credit. The results suggest that compared to non-relationship borrowers, the consolidated bank will increase the credit availability to relationship borrowers for the initial loan after the merger, but significantly decrease the loan availability to relationship borrowers afterwards. More interestingly, we only find such results in the sample of continued acquirer borrowers. This suggests that although target bank borrowers are charged higher loan spreads, continuing target customers benefit from larger loan supply after bank mergers.

The remainder of paper is organized as follows. Section 3.2. discuss hypotheses and empirical methodology. Section 3.3. describes the sample. Section 3.4. presents empirical results. Section 3.5. concludes. The Appendix contains variable definitions.

3.2. Hypotheses and baseline empirical specification

In this section, we discuss and present the specific hypotheses for the relation between relationship lending, bank mergers, and loan contract. We then describe our baseline regression.

3.2.1. Hypotheses

As suggested by the literature on relationship lending, borrowers with a long-term lending relationship with the bank can enjoy a lower interest rate and more favorable non-price terms than other borrowers (See, e.g. Berger and Udell, 1995; D'Auria et al., 1999; Bharath et al., 2011). However, the close relationship between the bank and borrower may become a channel through which the effect of shocks are transmitted or amplified (Dahiya et al., 2003; Murfin, 2012; Chen and Vashishtha, 2017). Berger and Udell (2002) point out that even if a dropped relationship customer can be picked up quickly by other banks, it takes considerable time for a new lender to collect "soft information" and the borrower may face less favorable loan terms. Importantly, Sapienza (2002) finds that the effect of bank merger is different across borrowers. The size of the merger is more positively related to yield spreads for borrowers with several lending options. This suggests that the consolidation significantly changes the bank's relative market power over such borrowers. The view that the net effect of bank merger varies across borrower types has received much empirical support (Sapienza, 2002; Karceski et al., 2005; Erel, 2011; Degryse et al., 2011). Besides, they generally find that the merger type and size, market overlap and customer types are important determinants to the net effect of the bank merger on borrowers. Therefore, we can expect that the effect of the bank merger is different across the strength of lending relationship with banks. After a bank consolidation, compared to borrowers

without close relationship with the bank, the market power effect may be more significant to relationship borrowers since banks already produce valuable information about those borrowers and can create information monopoly over those borrowers ex-ante. In other words, consolidated bank extract more market power from relationship borrowers after the bank merger. Specifically, we test the following hypothesis on the cost of loans:

Hypothesis 1. Compared to non-relationship borrowers, the consolidated bank charges higher loan spreads for relationship borrowers after the bank merger (the "hold-up" effect).

Consequences of the bank consolidation is different across merger type, the size of involved banks, market overlap among involved banks, customer types, and number of outside lending options. Using Italian data, Sapienza (2002) finds that in-market mergers (involving banks that operate in the same geographical area) benefit borrowers in terms of a lower loan spread. However, as the local market share of the merged bank increases, the efficiency effect is offset by market power. Erel (2011) examines how U.S. bank mergers affect the price of loans. She finds that the average reduction in loan spreads is both larger and more persistent for the non-mega acquirers. The reduction in spreads is substantially larger when the merging banks have some geographical overlap of markets before the merger. However, if the overlap is sufficiently extensive so that concentration of banking markets substantially increases the market power of the acquirer, the effect reverses and spreads on loans significantly increase after the merger. Degryse et al. (2011) focus on the discontinuation rate of lending relationship after bank mergers in Belgium and find that borrowers with high relationship intensities (measured as the proportion of the firm's total loans borrowed from that bank) with the target bank are most likely to be dropped. Therefore, we expect the "hold-up" effect in Hypothesis 1 will be stronger when the market

power effect of the merger is more pronounced. Specifically, the effect is expected to be stronger in mergers that a large bank acquires a small bank and in mergers with significant market overlap.

Further we expect that the effect is different across the borrowers of the acquiring bank and target bank. The literature finds that customers of the acquiring bank have lower exit rates and higher abnormal returns on equity value (Karceski et al., 2005; Degryse et al., 2011). Therefore, the relation in Hypothesis 1 (the "hold-up" effect) is expected to be stronger in the sample of target bank borrowers. We therefore expect the "hold-up" effect is more pronounced for borrowers of the target bank.

For non-price loan terms, the literature generally focuses on credit supply to small businesses. Berger et al. (1998) find that although the consolidated bank reduces credit supply to small businesses, the reduction is almost completely offset by credit supplied by other banks. Berger et al. (2004) also find that bank mergers significantly increase new entry, which helps the supply of credit to small business borrowers. The credit availability to borrowers and other non-price loan terms have been extensively studied in the literature on relationship lending. The literature generally finds that the lending relationship is associated with larger credit availability and less collateral requirements (see, e.g. Petersen and Rajan, 1994; Degryse and Cayseele, 2000; Bharath et al., 2011). Since loan price and non-price terms are jointly determined, it is not unreasonable to assume that the relation predicted in Hypothesis 1 translates into a prediction of less favorable non-price terms for relationship loans compared to non-relationship loans after bank mergers.

The bank requires collateral to control the borrower's moral hazard incentives. Strong relationships with banks are empirically associated with reduced collateral

requirements (see, e.g., Berger and Udell, 1995; Bharath et al., 2011). A strong relationship with the bank, by facilitating better monitoring, can improve the borrower's corporate governance (see, e.g. Dass and Massa, 2011; Chen and Qiu, 2017), so that collateral is less required by the bank. However, the effect of a bank merger on collateral requirement is unclear. The consolidated bank may reduce the collateral requirement due to the information-based efficiency gains through the merger. However, given that the consolidated bank generally becomes more powerful and efficient after the bank merger, the consolidated bank may commit to monitoring borrowers, especially relationship borrowers more intensively by increasing the collateral requirements.

A similar non-price loan term is loan covenants. Covenants in loan contracts mitigate the conflicts between shareholders and debtholders and reduce agency costs by restricting managers' behavior (Myers (1977)). The empirical evidence suggests that riskier firms receive contracts with stricter covenants (Billett, King and Mauer, 2007; Rauh and Sufi, 2010). Chen and Qiu (2017) find that a strong relationship with a bank can improve corporate governance of the borrowing firm by inducing better monitoring. Prilmeier (2017) finds that the covenant tightness is reduced over the duration of a lending relationship. In particular, the collateral requirement is one type of covenant. Therefore, consistent with the effect of lending relationship on the collateral requirements presented in the literature, we expect that a relationship lender requires a smaller number of covenants compared to non-relationship lenders. Berger et al. (2005) argue that small banks are better able to collect and act on "soft' information than large banks. Large banks are less willing to lend to risky firms. Thus, large banks may use more covenants in loans to risky firms compared to small banks. Therefore, the consolidated bank may play a stronger monitoring

role by requiring more covenants after a bank merger. However, if the size-related diversification effect is sufficiently large (e.g., equal-sized mergers), we expect the consolidated bank will reduce covenant intensity after a bank merger. Since the relationship can be viewed as a commitment to monitor (Bharath et al. (2011)), we expect the effect of bank merger is more pronounced for relationship borrowers. Above discussions lead to the following hypothesis:

Hypothesis 2a. Compared to non-relationship borrowers, the consolidated bank is more likely to require collateral and impose more covenants in the loan contracts to relationship borrowers after a bank merger.

The Diamond (1991) model predicts that relationships could both increase and decrease the loan maturity, depending on the credit rating of the firm. Bharath et al. (2011) find that, on average, relationship loans have shorter maturity than non-relationship loans. The size-related diversification effect of the bank merger (Diamond (1984)) may allow the consolidated bank to provide longer maturity loans. However, as discussed above, the consolidated bank may monitor relationship borrowers more intensively by providing short maturity loans. Therefore, it is unclear that whether the consolidated bank will shorten or lengthen the loan maturity to relationship borrowers after a bank merger.

Lastly, we examine the effect of bank mergers on the credit availability. Considerable work has been done testing whether firms benefit from lending relationships by obtaining better credit availability. These studies generally find that a stronger lending relationship is generally associated with increased credit availability (see, e.g., Petersen and Rajan, 1994, 1995; Cole, 1998). Berger and Udell (2002) argue that researchers should view relationship lending in an organizational context, and argue that small business

lending is generally relationship-based lending, while large firm lending is generally transactional-based lending. Empirically, the literature finds that ratios of small-business loans to assets tend to decline after large banks are involved in mergers and acquisitions (e.g., Peek and Rosengren, 1998; Strahan and Weston, 1996). The literature also finds that the credit supply to small business decreases after bank mergers with the reduction almost completely offset by the reaction of other banks (see, e.g. Berger et al., 1998; Berger et al., 2004; Berger et al., 2005). Strahan and Weston (1998) find consolidation among small banks serves to increase bank lending to small businesses and there is a non-monotonic relation between small business loans per dollar of assets and the bank size. Some studies test the effect on both large and small borrowers and find empirical evidence that the effect of the bank merger is different across borrowers. Sapienza (2002) finds that large banks acquiring small banks tends to cut off more small borrowers than do other bank mergers. Using Belgian data, Degryse et al. (2011) find that target-bank borrowers are more likely to be dropped, while borrowers with multiple borrowing options are less harmed. We posits that the "hold-up" effect may also negatively affect credit supplied to borrowers, and especially relationship borrowers. That is, the consolidated bank will exercise market power by reducing loan availability to borrowers and the reduction will be larger to relationship borrowers. However, this negative relation could also be driven by demand as well. Chen and Vashishtha (2017) find that borrowers increase disclosure if their banks engage in mergers, especially for borrowers who have close relationships with banks. This suggests that borrowers may choose to reduce their reliance on bank loans to avoid potential "hold-up" problems. The total effect can be a combination of both supply and demand for loans. We test the following hypothesis:

Hypothesis 2b. Compared to non-relationship borrowers, consolidated banks reduce supply of loans to relationship borrowers after mergers.

Recent literature emphasizes that many factors can influence the effect of a bank merger on loan contracts, such as merger type and size, acquirer size, market overlap, borrower outside options, and borrower type (see, e.g., Degryse et al., 2011; Erel, 2011; Sapienza, 2011). For example, target bank borrowers are generally more negatively affected by bank mergers and mergers among small banks serves to increase the loan supply to small businesses. Ultimately, it is an empirical question what is the marginal effect of a bank merger on the credit availability and other non-price terms after controlling for merger and borrower characteristics. In the empirical analysis, we also separately examine the influence of bank mergers on acquirer borrowers and target borrowers. We expect the results are different in the two samples.

3.2.2. Baseline empirical specification

Since the paper tests the effects of bank mergers and relationship lending on the loan contract and credit availability, we construct a loan sample where the lender is involved in at least one bank merger during the sample period. The bank merger has a one-year clean window.[4] We use a five years window for loan-year observations issued by merging banks before the merger announcement and a two years window for loan-year observations issued by consolidated bank after the completion time of the bank merger. This setting allows us to apply a difference-in-differences methodology. The full sample

[4] We use same acquirer more than once but the mergers are at least one year apart.

has 46,321 loan-year observations issued by banks involved in 487 bank mergers. The sample period is 1986 to 2017. We estimate the following baseline regression:

$$\begin{aligned} Loan\ spread = {} & \beta_0 + \beta_1 \times REL(M) + \beta_2 \times Post\ Merger + \beta_3 \times REL(M) \\ & \times Post\ Merger \times First + \beta_4 \times REL(M) \times Post\ Merger \times Others \\ & + \beta_5 \times Log(acquirer\ size) + \beta_6 \times Relative\ size \\ & + \sum \beta_i \times loan\ charateristics + \sum \beta_i \times borrower\ charateristics \\ & + \sum \beta_i \times other\ controls \qquad (1) \end{aligned}$$

where $Loan\ spread$ is yield spread over LIBOR (or LIBOR equivalent) on the drawn amount plus the annual fee in basis points, $REL(M)$ is a measure of relationship strength, $Post\ Merger$ is a dummy variable equal to one for loan-year observations within a two years window *after* the completion of the bank merger, and zero for loan-year observations within a five years window *before* the merger announcement. *First* is a dummy variable equal to one for the first relationship loan issued by the consolidated bank after the merger. *Others* is a dummy variable equal to one for all other relationship loans except the first one issued by the consolidated bank. *Log(acquirer size)* is the natural logarithm of the gross total assets one quarter before the merger, and *Relative size* is the ratio of the target bank size to the *acquirer size* where the target bank size is the gross total assets one quarter before the merger. In addition to loan and borrower characteristics, we include dummy variables for Fama-French 49 industries, and loan calendar year in all regressions.

To illustrate the interpretation of the coefficients, let subscript *R (NR)* denote relationship (non-relationship) loans and subscript *B (A)* denote before (after) completion

of the bank merger. We can interpret the coefficients β_1, β_2, β_3, and β_4 and in equation (1) as follows:

$\beta_1 = Loan\ spread_R^B - Loan\ spread_{NR}^B$ measures the difference in loan spreads between relationship and non-relationship loans before the bank merger. It is an empirical question whether relationship borrowers enjoy a lower loan spread. The long-term lending relationship reduces information asymmetries between the lender and the borrower, but it also increases the lender's control over the borrower. Therefore, β_1 is negative if the benefit of the lending relationship dominates the potential cost, while it is positive if the "hold-up" effect dominates.

$\beta_2 = Loan\ spread_{NR}^A - Loan\ spread_{NR}^B$ measures the difference in loan spreads for non-relationship loans before and after the bank merger. The sign of β_2 is positive if the market power effect of bank merger dominates the efficiency gains, and negative otherwise.

$\beta_3 = \left(Loan\ spread_{R,First}^A - Loan\ spread_{NR}^A\right) - (Loan\ spread_R^B - Loan\ spread_{NR}^B)$, and $\beta_4 = \left(Loan\ spread_{R,Others}^A - Loan\ spread_{NR}^A\right) - (Loan\ spread_R^B - Loan\ spread_{NR}^B)$, measures the difference in the differences of loan spreads between relationship and non-relationship loans before and after the bank merger. β_3 captures the immediate consequences of bank merger while β_4 captures the long-term (two years) consequences of bank merger on the lending relationship. Under Hypothesis 1, lending relationships accentuate the market power effect of the bank merger and thereby have a positive effect on loan spreads ($\beta_3 > 0, \beta_4 > 0$), and we would expect the "hold-up" effect is larger when the consolidated bank fully exercises the market power ($|\beta_4| > |\beta_3|$).

3.3. Data and sample selection

The paper mainly uses three data sources. Data on individual loans comes from the DealScan database maintained by Loan Pricing Corporation (LPC). In general, the loan agreements in the DealScan database cover a significant fraction of the dollar value of outstanding consumer and industrial loans. We use loans issued after 1986 since the number of loans reported in DealScan before 1986 is very limited. While the DealScan reports detailed information on relatively large U.S. and foreign loans since the early 80s, it does not provide much information on borrowers. To control for the differences in the firm characteristics between relationship borrowers and non-relationship borrowers, we collect necessary financial information from Compustat database. Strahan (1999) and Chava and Roberts (2008) provide good descriptions of the LPC DealScan database and the link table between the identifier of the loan facility and borrowing firm's financials in Compustat. [5] The link table is updated to December 2017. Therefore, our sample period is from 1986 to 2017. Financial firms are excluded. DealScan reports the facility start date as the legal effective date of the loan. However, the terms of a loans are negotiated well prior to this date. To ensure that accounting information are available at the time of a loan, we employ the same procedure as in the Bharath et al. (2011): if the loan effective date is six months or later than the fiscal year ending month in calendar year t, we use the data of that fiscal year. Otherwise, we use the data from the fiscal year ending in calendar year t-1.

The data source for the bank merger is S&P Global Market Intelligence (S&P) mergers and acquisitions database. [6] There was an extensive mergers and acquisition

[5] We thank Michael Roberts for making the link table available.
[6] S&P database has many advantages compared to other datasets. First, it reports the actual acquirer (can be a subsidiary) for each bank merger and detailed location information for both acquiring and target banks. It

activity in the U.S. banking section in 1980s, and a bank may do several bank mergers in a year. Therefore, we use a one-year clean window for bank mergers to mitigate the ambiguity driven by multiple bank mergers. If there is more than one merger of a given acquirer within the same year, it is used as a single data-point in the empirical analyses. In addition to merger variables, we also control for the size of merging banks. We use the gross total assets within one quarter before the merger announcement date as a proxy for the bank size reported on Federal Deposit Insurance Corporation Bank Regulatory database (FDIC).

We construct the loan sample and identify the loan type (relationship loan or non-relationship loan) for each loan using the lender information provided in the DealScan database. Following Sufi (2007) and Bharath et al. (2011), we identify a bank as a lead lender if it is a single bank that is coded as the lead arranger credit by LPC; or the bank is coded as one of the following roles: agent, administrative agent, arranger, or lead bank, and retains a significant share of the loan (>=25%). We focus on the US market and exclude all foreign firms and foreign lead lenders. [7] DealScan sometimes records regional branch names, or the name of a subsidiary, as the lender. We assign regional branches and subsidiaries to their parent institutions using the ownership structure information in the Federal Financial Institutions Examination Council's National Information Center (NIC) and Bloomberg. For example, Bank of America Arizona and Bank of America Oregon are

allows me to do an accurate match between bank mergers and lenders reported on DealScan database. Second, it reports a summary for each deal including total market deposits of merging banks. Lastly, it reports some useful variables for the bank merger, such as the market overlap of banks before the merger.

[7] Among 11,887 unique lenders in DealScan database (whose borrowers have information in Compustat), only 2,301 lenders have geography information and 832 of them are in the US. We search online by company names and dates of loan issuances to determine the location of lenders. Overall, 1,441 US lead lenders can be identified in DealScan database.

listed as lenders of record, then we combine two regional offices under a single bank name: Bank of America.

Following Bharath et al. (2011), for each loan, we look back and search all loans over the previous five-year window for the borrower. If the lead lender was a lead lender to the same borrower on a loan in the previous five years, the loan is classified as a relationship loan.[8, 9] The first relationship measure, *REL(dummy)*, is a dummy variable equal to 1 if the loan is classified as a relationship loan, and zero otherwise. We also construct two additional continuous relationship measures for each loan as

$$REL(number)_{ij} = \frac{Number\ of\ loans\ by\ bank\ j\ to\ borrower\ i\ in\ last\ five\ years}{Total\ number\ of\ borrower\ i\ loans\ in\ last\ five\ years};$$

$$REL(amount)_{ij} = \frac{Amount\ of\ loans\ by\ bank\ j\ to\ borrower\ i\ in\ the\ last\ five\ years}{Total\ amount\ of\ borrower\ i\ loans\ in\ the\ last\ five\ years}.$$

If a relationship loan has multiple lead lenders, we use the largest value of $REL(amount)_{ij}$ and $REL(number)_{ij}$ in our empirical analysis.[10]

As discussed above, over the sample period 1986 to 2017, there was an extensive mergers activity in the US banking sector. We hand-match the lead lenders identified from DealScan to the S&P mergers and acquisitions database using bank names and geographic information, whenever available. We verify matches using information National Information Center, Bloomberg and online news. Overall, we can identify a total of 1,572

[8] Note that one loan could have more than one lead lenders. The loan is classified as a relationship loan as long as one of them had been a lead lender in the past five-years. For example, Bank A and Bank B are lead lenders of Loan C. If the borrower of Loan C borrowed another loan (Loan D) from Bank A two years prior to Loan C and Bank A was the lead lender of Loan D. Then we classify Loan C as a relationship loan.

[9] Following the literature, we require that there be at least one loan in the previous five-year window prior to the loan origination date. Otherwise, we could not identify whether the loan is a relationship loan or not according to our definition. Therefore, the first loan of any borrower are excluded.

[10] For around 98% of loans in the final sample, our identification procedure for the lead lenders results in a single lender being classified as the lead lender.

bank mergers over 1986 to 2017. This allows us to trace lending relationships through the time of bank mergers even if the original lender disappears due to a merger. For example, First Chicago merged with National Bank of Detroit and created First Chicago NBD Corporation in 1995. In 1998, First Chicago NBD merged with Banc One Corporation of Columbus, Ohio and the merged company was renamed Bank One Corporation. In 2004, Bank One Corporation merged into JPMorgan Chase & Co.. In the case that a lender retains an independent brand after an acquisition, DealScan may continue to report the lending activity of the lender. For example, FleetBoston Financial Corporation continues to appear in DealScan after its acquisition by Bank of America. In this case, we treat the target institution separately. Alternatively, we aggregate all wholly owned subsidiaries under the ultimate parent. Notice that the main results of the paper are not significantly influenced by these choices.

Since we focus on the effect of bank mergers across loan types, non-merging lenders are excluded. In the empirical analysis, we use a one-year clean window for bank mergers and require loans for either acquirer bank or target bank within a five years window before a bank merger and a two years window after a bank merger. [11] Thus, a five-year period before the first and a two-year period after the last bank merger are required facilitate a difference-in-differences methodology. Specifically, we require that first, either acquirer bank or target bank has loans within a five-year window before the bank merger and the consolidated bank has loans within a two-year window after the bank merger.

[11] We make this choice for two reasons. First, the literature generally uses up to two years for the consolidated firm after the merger (Focarelli and Panetta, 2003) to test the effect of bank mergers. Second, the shorter time period will make certain that the loans after the merger freshly reflect the consequences of the merger. A five years window for loan-year observations before the merger is chosen to be consistent with the definition of relationship loans.

Overall, we have 487 bank mergers that satisfy all criteria over 1991 to 2015. We further drop the loans issued between the announcement and the completion dates of the mergers and loans issued to borrowers who establish their relationship with the bank purely post-merger.[12] To mitigate the bias due to changes in the composition of borrowers, we restrict the sample to continuing borrowers. Continuing borrowers are borrowers who have borrowed from either the acquirer or the target bank before the merger and borrow from the consolidated bank as well. The final loan sample contains 46,321 loan-year observations for 2,994 borrowers over 1986 to 2017, including a five-year period before the first merger and a two-year period after the last bank merger in the sample.

We use those bank mergers to define our merger variable, *Post Merger*. *Post Merger* is a dummy variable equal to one for loan-year observations within a two-year window *after* the completion of bank merger, and zero for loan-year observations within a five-year window before the merger. The literature generally finds that target bank borrowers are more likely to be dropped after a bank merger compared to acquirer bank borrowers (see, e.g. Erel, 2011; Sapienza, 2011; Degryse et al., 2011). Therefore, it is possible that the effect of bank mergers across loan types is different between acquirer borrowers and target borrowers. To test such differences, we exclude borrowers of target bank (i.e., exclude borrowers who ever borrowed from the target bank) to construct a sample of continuing borrowers of the acquiring bank. The sample of continuing borrowers of target bank is constructed in a similar way (Note that in this sample, the target bank should have loans in a five-year window before the merger and in a two-year window after

[12] We have cases that the first post-merge loan issued to the borrower is still a non-relationship loan while the second post-merger loan will become a relationship loan. We delete such loans since it invalidates the comparison of bank merger effect between relationship loans and non-relationship loans.

the bank merger, i.e., the target bank appears in the DealScan after the bank merger within a two-year window. Only 36 bank mergers satisfy this requirement and the sample is biased by relatively large target banks). Another interesting group of borrowers are borrowers who switch from the target bank to the acquiring bank after the merger. This sample can particularly reflect the effect of bank merger frictions on borrowers of target bank (loss of knowledge accumulated within each of the merging banks) and serve as a complimentary to results on target bank borrowers.

3.4. Empirical Results

3.4.1. Univariate comparisons

Table 3.1 provides the summary statistics for main variables used in tests. Loans in the sample are mainly large loans (the mean is 206 million) with relatively low loan spreads (the mean is 200 basis points; the mean of loan spreads is 233 basis points without restrictions on bank mergers). Borrowers in the sample are mainly large firms and 35.2% of them are firms with S&P credit rating. Table 3.2 reports the year distribution of bank mergers in the sample and number of loans issued by these banks grouped by lending relationship. *Number of bank mergers* for each year is a count of the bank mergers completed in the year. Loans issued by these merging banks in a year are also reported and grouped by loan types. Around 67% if bank mergers in the sample completed in 90s while there are only a few mergers completed every year after 2010.

Panel A of Table 3.3 reports means and medians of variables for relationship and non-relationship loans. The results of univariate tests of differences in means and medians provide strong evidence that relationship loans, on average, have significantly lower loan spreads, as well as better non-price terms such as less collateral requirements, larger loan size and lower number of covenants. It suggests that banks are more willing to have a long-term lending relationship with high quality firms. In Panel C, we segregate the relationship and non-relationship loans by completing time of bank mergers. Although relationship loans enjoy lower loan spreads compared to non-relationship loans both before and after bank mergers, the difference between loan spreads across loan types significantly reduces after bank mergers. The reduction is much more significant in the sample of target bank borrowers. It indicates that bank mergers have different effects on relationship and non-

relationship borrowers and relatively, the increased market power of the consolidated bank after the merger has more negative impacts on relationship borrowers, especially the borrowers of the target bank. However, it is likely that relationship borrowers are fundamentally different from non-relationship borrowers. Thus, we compare several firm characteristics across loan types, and results are reported in Panel B. On average, the relationship borrowers are larger, more profitable and less risky than non-relationship borrowers. Such borrowers are more likely to have an investment grade rating. Therefore, it is less likely that the results in Panel C are entirely driven by borrowers' performance.

3.4.2. Loan spread regressions

Table 3.4 reports panel regressions of loan spread on relationship lending and bank mergers using the specifications discussed in Section 2.2. Panel A presents the effect of bank mergers on loan spreads without conditioning on lending relationship; Panel B and Panel C present results conditioning on lending relationship. The dependent variable is loan spread in basis points. *Post Merger* is a dummy variable equal one for loan-year observations within a two-year window *after* the completion of the bank merger. *Acquirer size* is the gross total assets of the acquirer measured one quarter prior to the acquisition announcement. *Relative size* is equal to the target bank size divided by acquirer size. Controls for firm and loan characteristics follow Bharath et al. (2011) and Kubick et al. (2017). All regressions include dummy variables for calendar year, Fama-French 49 industries, loan type, loan purpose, credit rating, syndicate size and headquarter state.

Column (1) to (3) of Panel A report loan spread regressions in the full sample and different merger types. On average, bank mergers do not have significant effect on loan spreads while the in-market mergers significantly reduce the loan spreads (Column (2)). It

suggests that borrowers can benefit from bank mergers with potentially large information efficiency gains. However, *Relative size* is positively related to loan spreads and is significant at 1%. Therefore, when the target bank is sufficiently large, the marginal reduction on loan spreads due to the efficiency gains can be offset by increased market power of the consolidated bank after the merger. For merging banks that operate in different areas prior to the merger, the consolidated bank even charges a higher loan spreads after the merger. Previous studies find that borrowers of target bank are more easily to be cut off after a bank merger (Degryse et al. (2011)). Thus, we exclude borrowers who borrowed from the target bank before and/or after the merger, then test the effect of bank mergers in Column (4). The result is similar as in the full sample. We then do the same test for continuing target bank borrowers. Those borrowers only borrowed from the target bank both before and after the bank merger. The results are reported in Column (5). The coefficient of *Post Merger* is negative and significant at 10%. We find similar but stronger results on post-merger borrowers of the acquiring bank who borrowed from the target bank prior to the merger (Column (6)). It seems that target bank borrowers, on average, benefit from bank mergers as long as they can continue to borrow from the consolidated bank after the merger.

Panel B reports results of the difference-in-differences specification in Equation (1). Same control variables are used in Panel B and we only report results for main variables. Three variables are used to measure the strength of a lending relationship: a dummy variable (*REL(dummy)*) for repeated borrowing from the same lead lender in last five years and two continuous variables, *REL(number)* and *REL(amount)*. *First* is a dummy variable equal one for the first relationship loan issued by the consolidated bank and *Others* is a

dummy variable equal one for all post-merger relationship loans except the first one. Column (1) to (3) report the regression results using the full sample. Regardless of which relationship measure is used, the coefficient on the relationship variable is significantly negative. Before a bank consolidation, relationship borrowers enjoy a lower loan spread by 8.726 basis points compared to non-relationship borrowers. The coefficient on *Post Merger* is insignificant but negative. On average, the bank merger has no significant impacts on non-relationship loans. The interaction of relationship and post-merger dummy, $REL(M) \times Post\ Merger \times First$ and $REL(M) \times Post\ Merger \times Others$, capture differences between the effect of bank mergers on loan spreads of relationship and non-relationship loans. The coefficient on the first interaction term is insignificantly positive and the second interaction term is significantly positive in two of three relationship measures at 10% level. The results provide clear evidence to "hold-up" problems and suggest that it takes some time for the consolidated bank to fully exercise the increased market power. Using the results in Column (2), the economic significance is 2.8%. The borrowing costs of relationship borrowers are indeed lower than the borrowing costs of non-relationship borrowers, however, such benefit is significantly reduced when the market power of the lender increases substantially after a bank merger. In other words, compared to non-relationship borrowers, the market power effect is more pronounced for relationship borrowers. We then test the same specification across borrower types as in the Panel A. Results are posted in Column (4) to (6). We find consistent results on the "hold-up" effect using the sample of continued target borrowers and switching borrowers. Interestingly, we find that continued relationship borrowers of the acquiring bank will *benefit* more from the bank merger compared to non-relationship borrowers while the reduction on the loan

spreads disappears for later loans. Holding everything else constant, compared to non-relationship loans, the consolidated bank reduces the loan spreads to the first relationship loan by 14.483 basis points after a bank merger. A potential explanation is that relationship borrowers of acquiring bank are larger and have more market power compared to borrowers of the target bank. Therefore, they can extract benefits from the consolidated bank. The loss of "soft information" on target bank borrowers due to the consolidation can also be a driver.

To further confirm our hypothesis that a long-term relationship with the bank helps the bank to extract benefits from borrowers. We add a dummy variable, *Old*, to the specification and report results in Panel C. *Old* is a dummy variable equal one for loan-year observations if the borrower was already a relationship borrower prior to the merger. i.e., this variable distinguishes relationship borrowers with long time relationship with the bank prior to the merger from relationship borrowers who set up the relationship across the merger. As expected, the "hold-up" effect is more pronounced in Panel C and will exacerbate over time. In the sample of continued acquiring bank borrowers, we still find that relationship borrowers significantly benefit more from the bank merger compared to non-relationship borrowers, but the magnitude is smaller and less significant compared to that in Panel B.

Since the literature generally finds that the effect of bank mergers is different across merger types. We test the same regression in two subsamples. In table 3.5, we examine the effect of bank mergers on lending relationship in mergers when a large bank acquires a small bank, and in mergers which merging banks have partial market overlap prior to the merger. Panel A reports the effect of bank mergers without conditioning on lending

relationship and Panel B reports the effect of bank mergers across the lending relationship. When a large bank acquires a small bank, the efficient gains are limited, thus, the consolidated bank is more likely to exercise its increased market power over the relationship borrowers. Consistent with our expectation, we find the "hold-up" effect for both borrowers of the acquiring bank and borrowers of the target bank. Another merger type brings our attention is the merger between banks with partial market overlap prior to the merger. Merging banks could gain information efficiency and achieve more diversification through such consolidation while the market power would not increase dramatically. Therefore, we would expect relationship borrowers will not be harmed or are able to benefit more from the merger. The results in Panel B confirm that the consolidated bank charges 22 basis points less on loan spreads to relationship borrowers of the acquiring bank, compared to non-relationship borrowers of the acquiring bank after the merger.

In sum, the consolidated bank exercise the increased market power by charging higher loan spreads to relationship borrowers. The effect exacerbates with the relationship length. Borrowers benefit most from in-market mergers among small banks. Generally, the consolidated bank is more likely to extract loan spreads from relationship borrowers of the target bank. Borrowers of acquiring bank can extract benefits from the consolidated bank compared to non-relationship borrowers while the average level of loan spreads increases after the merger. Both Table 3.4 and Table 3.5 provide some empirical evidence to the existence of "hold-up" problems, and the magnitude depends on merger types, bank types, market overlap of banks, and borrower types.

3.4.3. Non-price terms regressions

In this section we investigate whether bank mergers has different effects on non-price loan terms across loan types. We focus on the probability of collateral, loan maturity, number of loan covenants and credit availability.

We test the effect of bank mergers and lending relationship on the collateral requirement using a *Probit* model and results are reported in Table 3.6. The dependent variable is a binary variable equal to one if a loan facility is secured by collateral and zero otherwise. We control for bank merger, borrower and loan characteristics. First three columns present the effect of bank mergers without conditioning on lending relationship. On average, the bank merger has no significant effect on the probability of collateral requirements, and the merger size and market overlap are important factors to determine the overall effect. When merging banks operate in the same market prior to the merger, the consolidated bank is less likely to require collateral after the merger while the effect can be opposite when the target bank is sufficiently large. For mergers between banks without any market overlap prior to the merger, the consolidated bank will be slightly more likely to require collateral after the merger.

In column (4) to (8) of Table 3.6, we test the effect of bank mergers conditioning on the lending relationship. Consistent with the literature, we find that banks are less likely to require collateral on relationship loans prior to the merger. The interaction terms between lending relationship and bank mergers, $REL(dummy) \times Post\ Merger \times First$ and$REL(dummy) \times Post\ Merger \times Others$, both have a significant and positive coefficient at 5% level. Compared to non-relationship loans, the consolidated bank is more likely (by 4%) to require collateral on relationship loans after a bank merger. The effect is more pronounced over time. In Column (7) and (8), we focus on acquirer borrowers and target borrowers separately. For acquirer borrowers, the consolidated bank is more likely to require collateral to

relationship loans compared to non-relationship loans. Interestingly, we find opposite results in the sample of target borrowers. The consolidated bank is less likely to require collateral to relationship borrowers of the target bank, compared to non-relationship loans.

In Table 3.7, we examine how bank mergers and lending relationship affect the loan maturity. The dependent variable is the natural log of the loan maturity in months. When merging banks operate in the same market prior to the merge, although borrowers enjoy a lower loan spreads and less restrictive loan covenant after the merger, the consolidated bank significantly decreases the loan maturity. A possible explanation is that the cost of monitoring may decrease due to the large efficiency gains through in-market mergers. Thus, the consolidated bank can renew the loan contracts more frequently. The results in Column (3) to (6) indicate that relationship loans, on average, have shorter maturity than non-relationship loans prior to the merger. This pattern is more significant for target borrowers. However, after the merger, The coefficients on interaction terms suggest that the consolidated bank will lengthen the loan maturity to relationship borrowers after the merger, compared to non-relationship borrowers. We only find such results for borrowers of the target bank and the positive effect on the loan maturity is stronger over time.

Loan covenant is another non-price term that banks generally use to control managers behavior. Following Bradley and Roberts (2015), we assign one point for each of five covenants: secured debt, dividend restrictions, asset sale sweep, debt issuance sweep and equity issuance sweep, and define a dummy variable for loans with more than two covenants on restricted financial ratios. Then the covenant index is defined as the sum of these six indicator variables and varies from zero to six. In Table 3.8, we test the effect of lending relationship and bank mergers on the covenant index. On average, the consolidated bank decreases the intensity of covenants after a bank merger. The effect is stronger for in-

market mergers and the size of the acquiring bank has a consistently negative impact on the covenant index. It indicates that mergers among small banks are more beneficial than other merger types. These results are more consistent with the effect of information-based efficiency gains effect and size-related diversification effect through the bank merger. Sapienza (2011) finds that efficiency gains effect of the bank consolidation is larger for in-market mergers where banks operate in the same market before the bank merger. Column (4) to (8) present results conditioning on the lending relationship. We find that before the bank merger, relationship loans have lower covenant index compared to non-relationship loans. However, compared to non-relationship loans, the consolidated bank will use more covenants in loan contracts to relationship borrowers after a bank merger, though the results only show up in the sample of acquiring bank borrowers. These results are consistent with results on collateral requirement (see Table 3.6) and suggest that while relationship borrowers of acquiring bank may enjoy a (larger) reduction on the loan spreads compared to non-relationship borrowers after the merger, the consolidated bank will monitor relationship borrowers more intensively by issuing more restrictive loans to relationship borrowers.

Besides loan spreads and non-price loan terms we discussed above, another crucial feature of loan contract is the amount of loan. We test the effect of bank mergers and lending relationship on credit availability in Table 3.9. Since the credit availability cannot be observed directly, we use the loan amount scaled by the total asset of the borrower as a proxy. A high ratio implies better credit availability. Overall, we do not find any significant effect of bank mergers. The significantly negative coefficients on the *Relative size* indicate that mergers among large banks might harm some small business. Across different merge

types, we find that the consolidated bank significantly increase the credit availability to borrowers after an in-market merger while the consolidated bank decreases the credit availability after the merger when merging banks have some market overlap prior to the merger. We include the effect of lending relationship in Column (4) to (8). Relationship borrowers, on average, enjoy a larger loan availability than non-relationship borrowers. However, coefficients of interaction terms between relationship and post-merger dummy are significantly negative using all three relationship measures. It suggests that compared to non-relationship borrowers, the consolidated bank will significantly reduce loan availability to relationship borrowers after a bank merger. It is interesting to observe that the first interaction term, $REL(dummy) \times Post\ Merger \times First$ has a significantly positive impact on the credit availability while the second interaction term, $REL(dummy) \times Post\ Merger \times Others$ is has a significantly negative impact on the credit availability. It suggests that the consolidated bank might be willing to share some benefits gained through the consolidation (such as more access to deposits and larger diversifications) with borrowers, but then extract benefits from borrowers when they fully exercise the increased market power. The results could also be driven by the different consequences of bank mergers across borrower types. In the sample of acquiring bank borrowers, we find that the consolidated bank will reduce the credit supply to relationship loans compared to non-relationship loans, but the reduction realizes in later time after the merger (Column (7)). As for borrowers of the target bank, the consolidated bank will slightly increase the loan supply to relationship borrowers compared to non-relationship borrowers after the merger while such additional credit availability disappears quickly. Another interesting finding is that the consistently negative relation between *Relative size* and credit availability. Those results suggest that the mergers involved large banks tend to cut more loan supply to continued borrowers compared to other

merge types, and it takes longer for merged banks to decide to drop a borrower than it does for them to change the loan rates.

In sum, borrowing from a relationship lender can benefit from not only lower loan spreads, but also better non-price terms such as less collateral requirements, lower number of loan covenants and larger loan availability compared to borrowing from a non-relationship lender. However, benefits associated with lending relationship are significantly reduced after a significantly change on the market structure of lenders such as bank consolidation. Our empirical results suggest that relationship borrowers are more negatively affected by bank mergers and the effect is different across borrowers.

3.5. Discussion and Conclusion

We find that compared to non-relationship borrowers, merged banks require higher loan spreads from relationship borrowers. This "hold-up" effect is more pronounced in mergers when a large bank acquires a small bank. Target bank borrowers are more negatively affected compared to acquirer bank borrowers. Results on non-price loan terms suggest that compared to non-relationship borrowers, the consolidated bank monitors relationship borrowers more intensively by requiring more collateral and imposing more covenants. However, the consolidated bank will lengthen the loan maturity to relationship borrowers compared to non-relationship borrowers after the merger. Finally, we find strong evidence that compared to non-relationship borrowers, the supply of credit to relationship borrowers decreases after bank mergers. Bank mergers have small negative impacts on loan spreads to relationship borrowers, though the consolidated bank significantly increases the loan restrictive and reduce the loan supply to relationship borrowers after the merger. Relationship borrowers of the target bank are charged higher loan spreads compared to non-relationship borrowers after the merger, but the consolidated bank simultaneously issues less restrictive loans and slightly increase the loan supply to them after the merger. The literature generally finds that borrowers of target banks are more likely to be dropped after the merger. Our results suggest that the merged bank does not harm all borrowers of small targets but lengthen the loan maturity and increase the loan availability to relationship borrowers after the merger. Moreover, we find that the consequences of bank mergers on lending relationship are realized immediately after the bank merger and the effects exacerbate over the time. The only exception is the credit availability. It takes some time for the consolidated bank to reduce the loan supply to

relationship borrowers. This indicates that either the consolidated bank exercises the market power on relationship borrowers by cutting loan supply, or relationship borrowers choose to borrow less from relationship lenders to mitigate the negative effect of potential "hold-up" problems. More information on the borrowers' financing behavior and reactions of other market participants are necessary to disentangle the two possibilities.

It is difficult to fully understand the consequences of relationship lending in the context of bank organization, since so many factors can influence the final results, such as borrower characteristics, loan characteristics, merging banks' characteristics, deal characteristics, and reactions of non-merging banks and other market participants. This paper takes a small step to add some insights to this topic. There are many other interesting questions in this framework. For example, whether relationship borrowers are more likely to be dropped post-merger, and what are the reactions of non-merging banks or other market participants? Our empirical framework allows us to capture some dynamic changes attributable to relationship lending. It can be applied to study other shocks in the banking industry, or how the borrower's behavior influences a bank's decision to make relationship loans. A caveat is the joint determination of price and non-price features of loans. For example, covenants reduce borrowing costs and improves access to credit (see, e.g. Smith and Warner, 1979; Bradley and Roberts, 2015). We find that although the "hold-up" effect is stronger on loan spreads of target bank borrowers compared to acquirer borrowers, merged banks decrease the availability of credit more to acquirer borrowers than to target borrowers. Continuing target borrowers appear to benefit from larger loan availability post-merger.

Appendix. Variable definitions

Variable	Definition (source of data)
Relationship measures	
REL(dummy)	Dummy variable that equals one if there is a past relationship with any of the lead lenders in the last five years before the present loan, and zero otherwise. (DealScan)
REL(number)	Continuous measure of relationship strength. For each lead lender of the loan, it is defined as $\frac{Number\ of\ loans\ by\ bank\ j\ to\ borrower\ i\ in\ the\ last\ five\ years}{Total\ number\ of\ borrower\ i\ loans\ in\ the\ last\ five\ years}$. If the loan has multiple lead lenders, the highest *REL(amount)* is used. (DealScan)
REL(amount)	Continuous measure of relationship strength. For each lead lender of the loan, it is defined as $\frac{Dollar\ amount\ of\ loans\ by\ bank\ j\ to\ borrower\ i\ in\ the\ last\ five\ years}{Total\ dollar\ amount\ of\ borrower\ i\ loans\ in\ the\ last\ five\ years}$. If the loan has multiple lead lenders, the highest *REL(number)* is used. (DealScan)
Merger variables	
Post Merger	Dummy variable that equals one for loan-year observations within two years after the completion of the bank merger, and zero for loan-year observations within five years prior to the announcement of the bank merger. (DealScan/S&P)
Post Acquirer	Dummy variable that equals one for post-merger loans made to continuing borrowers of the acquiring bank within two years after the completion of the acquisition, and zero for loans made to continuing borrowers of the acquiring bank within five years prior to the announcement of the acquisition. Continuing borrowers of the acquiring bank are borrowers who borrow from the acquirer bank both before and after an acquisition. (DealScan/S&P)
Post Target	Dummy variable that equals one for post-merger loans made to continuing borrowers of the target bank within two years after the completion of the acquisition, and zero for loans made to continuing borrowers of the target bank within five years prior to the announcement of the acquisition. Continuing borrowers of the target bank are borrowers who borrow from the target bank both before and after an acquisition. (DealScan/S&P)
In-state merger	Dummy variable that equals one if both acquirer and target bank are headquartered in the same state, and zero otherwise. (DealScan/S&P)
Acquirer size	The gross total assets of the acquirer as of one quarter before the merger. (FDIC)
Target size	The gross total assets of the target bank as of one quarter before the merger. (FDIC)
Relative size	Gross assets of target bank divided by gross assets of acquirer as of one quarter before the merger . (FDIC)
Loan characteristics	
Loan spread	The all-in drawn spread from the DealScan database. It is the amount the firm pays in basis points above LIBOR (or LIBOR equivalent) plus any additional fees for each dollar drawn down from the loan facility. (DealScan)
Maturity	The loan maturity measured in months. (DealScan)

Loan size	The loan amount measured in constant dollars using the CPI with base year 2003. (DealScan)
Secured loan	Dummy variable that equals one if the loan is secured by collateral, and zero otherwise. (DealScan)
Syndicate	Dummy variable that equals one if the loan is a syndicated loan, and zero otherwise. (DealScan)
Covenant index	Count index of the number of covenants in the loan. The covenant index assigns one point for each of six covenants: secured debt, dividend restriction, more than two covenants placing restrictions on financial ratio, asset sale sweep, debt issuance sweep, and equity issuance sweep. The maximum covenant index is 6 and the minimum index is 0. (DealScan)
Performance pricing	Dummy variable that equals one if the loan facility has a performance pricing feature, and zero otherwise. (DealScan)
Syndicate size	Number of lenders for each loan facility. Foreign lenders are also included. (DealScan)
Loan Concentration	Ratio of present loan amount to the sum of existing debt (the sum of long-term debt and current liabilities) and loan amount. (DealScan/ Compustat)
Firm characteristics	
Assets	Total book assets measured in constant dollars using the CPI with base year 2003. (Compustat)
Book leverage	Total debt (long-term debt plus debt in current liabilities) divided by the book value of total assets. (Compustat)
Coverage	Ratio of earnings before interest, taxes, depreciation, and amortization (EBITDA) to interest expense. (Compustat)
Profitability	Ratio of earnings before interest, taxes, depreciation, and amortization (EBITDA) to total sales. (Compustat)
Current ratio	Ratio of total current assets to total current liabilities. (Compustat)
Market-to-book	Ratio of market value of equity plus the book value of debt to total book value of assets. (Compustat)
R&D/sales	Ratio of research and development expense to sales, where research and development expense is set equal to zero when missing. (Compustat)
Investment grade	Dummy variable that equals one if S&P credit rating is BBB– and above, and zero otherwise. (Compustat)
Rated dummy	Dummy variable that equals 1 if the firm has an S&P long-term debt rating. (Compustat)
Asset maturity	Asset maturity is the weighted average of current assets divided by the cost of goods sold, and Net PPE divided by depreciation and amortization. (Compustat)

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Table 3.1 Descriptive statistics

Panel A reports the statistics for the entire loan sample from 1986 to 2017. The sample covers a total of 46,321 loan-year observations for this time period, including a five-year period before the first and a two-year period after the last merger in the sample. Panel B reports the statistics for the sample of bank mergers. The sample covers 487 bank mergers by 135 acquirers. The bank merger has a one-year clean window, i.e., we use the same acquirer more than once, but the mergers are at least one year apart. We require that either acquirer or target bank has loans within five years before the merger announcement and the consolidated bank has loans within two years after the merger completion. *REL (dummy)* is a dummy variable equal to one if there is a past relationship with a lead lender in the most recent five years. *Post Merger* is a dummy variable equal to one for all loan-year observations in a two-year window after the completion of the bank merger. All continuous variables (except *REL(number) and REL(amount)*) are winsorized at the 1% and 99% levels. All variables are defined in the Appendix.

Variable	N	Mean	Std Dev	Minimum	25th Pctl	Median	75th Pctl	Maximum
Panel A. Firm Loan sample								
REL(dummy)	46,321	0.663						
REL(number)	46,321	0.357	0.359	0.000	0.000	0.250	0.636	1.000
REL(amount)	46,321	0.350	0.377	0.000	0.000	0.202	0.667	1.000
Post Merger	46,321	0.393						
Post Acquirer	39,365	0.435						
Post Target	6,956	0.158						
In-state merger	31,465	0.365						
Loan spread (basis points)	44,209	199.758	127.323	17.500	100.000	200.000	275.000	630.000
Maturity (months)	43,794	45.363	25.502	4.000	24.000	48.000	60.000	116.000
Loan size (millions)	46,321	206.218	367.820	1.000	20.000	75.000	205.431	2,330.000
Secured loan	46,321	0.537						
Syndicate	46,321	0.826						
Covenant index	46,321	0.918	0.804	0.000	0.000	1.000	2.000	6.000
Performance pricing	46,321	0.362						
Syndicate size	46,321	6.742	8.681	1.000	1.000	4.000	9.000	162.000
Assets (millions)	42,161	2,726.720	6,880.170	7.215	124.532	435.421	1,712.800	45,293.000
Book leverage	43,360	0.353	0.255	0.000	0.168	0.320	0.484	1.000
Coverage	43,,360	16.010	46.942	0.000	1.957	4.587	10.089	352.159
Profitability	41,760	0.145	0.151	-0.473	0.071	0.125	0.203	0.626
Current ratio	40,212	1.873	1.235	0.281	1.085	1.588	2.286	7.588
Market-to-book	35,485	1.707	0.961	0.710	1.120	1.405	1.930	6.351
R&D/sales	43,360	0.015	0.038	0.000	0.000	0.000	0.009	0.216
Investment grade	46,321	0.166						
Rated dummy	46,321	0.352						
Panel B. Bank merger sample								
Acquirer size (billion)	438	62.032	123.050	0.095	10.905	26.850	62.094	1,502.160
Target size (billion)	341	14.454	33.321	0.031	0.460	3.232	14.096	310.897
Relative size	296	0.250	0.324	0.000	0.017	0.095	0.344	1.467

Table 3.2 Distribution of bank mergers and loan types

The table reports the year distribution of bank mergers in the sample and number of loans from merging banks in a five-year window before the merger announcement and a two-year window after the merger completion. Number of bank mergers is the number of bank mergers completed in a year. Number of relationship loans is the number of all loans with *REL(dummy)* equal to one in a year. Number of non-relationship loans is the number of all loans with *REL(dummy)* equal to zero in a year.

Panel A. Distribution of bank mergers by year		**Panel B. Distribution of bank loans by year**		
Calendar year	Number of bank mergers	Calendar year	Number of relationship loans	Number of non-relationship loans
1991	31	1986	15	13
1992	33	1987	192	115
1993	34	1988	530	301
1994	31	1989	715	318
1995	37	1990	657	312
1996	41	1991	793	364
1997	44	1992	1,052	376
1998	37	1993	1,426	533
1999	19	1994	1,814	772
2000	21	1995	1,741	775
2001	23	1996	2,176	1,220
2002	12	1997	2,629	1,567
2003	11	1998	1,520	1,161
2004	14	1999	1,849	1,143
2005	17	2000	2,026	919
2006	12	2001	1,487	854
2007	15	2002	1,390	863
2008	14	2003	1,145	672
2009	6	2004	1,520	696
2010	7	2005	1,497	619
2011	10	2006	1,405	565
2012	8	2007	995	470
2013	4	2008	649	366
2014	3	2009	330	162
2015	3	2010	219	117
		2011	323	96
		2012	214	80
		2013	155	79
		2014	93	42
		2015	71	33
		2016	35	17
		2017	25	14
Total	487		30,688	15,633

Table 3.3 Relationship loans versus non-relationship loans

The table reports means and medians for the sample grouped by relationship and non-relationship loans. Panel A reports means and medians for various price and non-price terms of the loan contract. Relationship loans are loans with *REL(dummy)* equal to one. Non-relationship loans are loans with *REL(dummy)* equal to zero. Panel B reports means and medians for borrower specific characteristics according to firm-year observations. Panel C further reports the mean and median of loan spreads before and after bank merger. "Diff" equals the mean value of loan spreads of non-relationship loans minus the mean value of loan spreads of relationship loans. The asterisks on the mean and median values of variables in the relationship loan sample indicate whether they are significantly different from the corresponding mean and median values of variables in the non-relationship loan sample (*t*-test for differences in means and Wilcoxon's rank sum test for differences in medians). All continuous variables are winsorized at the 1% and 99% levels. All variables are defined in the Appendix.

	Non-relationship loans		Relationship loans	
	Mean	Median	Mean	Median
Panel A. Loan characteristics				
Loan spread (basis points)	220.469	225.000	189.391***	175.000***
Maturity (months)	45.875	48.000	45.106***	48.000**
Loan size (in millions)	143.424	46.400	238.206***	100.000***
Secured loan	0.588		0.511***	
Syndicate	0.770		0.855***	
Covenant index	0.992	1.000	0.881***	1.000***
Performance pricing	0.355		0.366***	
Syndicate size	5.040	2.000	7.609***	4.000***
Panel B. Borrower characteristics				
Assets (in millions)	1988.510	254.968	3098.600***	571.657***
Book leverage	0.337	0.297	0.360***	0.329***
Coverage	17.515	4.462	15.249***	4.633***
Profitability	0.130	0.117	0.152***	0.129***
Current ratio	1.963	1.625	1.826***	1.570***
Market-to-book	1.719	1.393	1.701**	1.412***
R&D/sales	0.019	0.000	0.013***	0.000***
Investment grade	0.119		0.190***	
Rated dummy	0.274		0.391***	

Table 3.3 – *continued*

Panel C. Comparison across bank merger

	Five years before bank merger					Two years after bank merger				
	Non-relationship loan		Relationship loan		Diff	Non-relationship loan		Relationship loan		Diff
	Mean	Median	Mean	Median		Mean	Median	Mean	Median	
Continued borrowers of acquiring bank										
Loan spread	224.173	225	190.677***	175***	33.496	221.10	225	189.617***	175***	31.483*
Continued borrowers of target bank										
Loan spread	223.961	225	196.737***	187.5***	27.224	202.771	200	182.109***	175***	20.662***

Table 3.4 Effect of lending relationship and bank mergers on cost of loans

The table presents the effect of bank mergers and lending relationship on loan spreads for the full sample and different subsamples. The dependent variable is the yield spread over LIBOR (or LIBOR equivalent) on the drawn amount plus the annual fee in bps. "In-market mergers" are bank mergers where all the target's active branches are in markets that the acquirer also has active branches. Market expansion mergers" are bank mergers where the target has no active branches operating in common markets with the buyer. We exclude borrowers that borrowed from target banks prior to the bank merger and/or after the merger and report the results in Column (4). In Column (5), we only include borrowers that borrowed from target banks both before and after the bank merger. In Column (6), we focus on the borrowers who borrowed only from the target bank before the merger (i.e., no loans from the acquiring bank prior to the merger) but started to borrow from the acquiring bank after the merger. Panel A reports regressions that do not condition on lending relationship. Panel B and Panel C report regressions that condition lending relationship (unreported control variables are same as in Panel A). *REL(M)* is the measure of relationship strength, estimated in three different ways: *REL(dummy), REL(number)* and *REL(amount). Post Merger* is a dummy variable that equals one for loan-year observations within two years after the completion of the bank merger, and zero for loan-year observations within five years prior to the announcement of the merger. *First* is a dummy variable that equals one for first relationship loan issued by the merged bank (i.e., the loan-year observations with *REL(dummy)*=1 and are closest to the completion of the merger), and zero for all other loans. *Others* is a dummy variable that equals one for all relationship loans issued by the merged bank except for the first one (i.e., *Others* equals zero when *First* equals one), and zero for all other loans. *Old* is a dummy variable that equals one if the borrower was a relationship borrower prior to the merger. Dummy variables for Fama-French 49 industries, calendar year, loan type, loan purpose, credit rating, syndicate size, and headquarter state are included in all regressions. T-statistics (in parentheses) are computed using robust standard errors corrected for clustering of observations at the firm level. All variables are defined in the Appendix and continuous variables are winsorized at 1% and 99%. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	All mergers	In-market mergers	Market expansion mergers	Borrowers of acquirer	Borrowers of target	Tar_acq borrowers
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Without conditioning on lending relationship						
Post Merger	1.406	-10.165***	6.526*	3.915	-17.303*	-14.126**
	(0.42)	(-3.30)	(1.91)	(1.41)	(-1.86)	(-2.00)
Log(acquirer size)	-0.611	-3.729	-0.146	-0.412	-3.826*	2.632
	(-0.45)	(-0.91)	(-0.08)	(-0.18)	(-1.96)	(0.43)
Relative size	1.289	8.810***	3.574	4.387	3.314	18.440**
	(0.24)	(2.85)	(1.29)	(0.62)	(1.46)	(2.38)
	(-4.86)	(-2.83)	(-3.51)	(-4.51)	(-2.58)	(0.09)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared	0.569	0.589	0.604	0.574	0.575	0.689
Number of observations	18,994	4,862	9,464	14,176	3,336	1,108

Table 3.4 – *continued*

	All mergers	All mergers	All mergers	Borrowers of acquirer	Borrowers of target	Tar_acq borrowers
	(1)	(2)	(3)	(4)	(5)	(6)
Panel B. Conditioning on lending relationship						
REL(dummy)	-8.726**			-8.302**	-6.523	-8.232*
	(-2.02)			(-2.23)	(-1.57)	(-1.84)
*REL(*number*)*		-7.113*				
		(-1.83)				
REL(amount)			-7.202*			
			(-1.89)			
Post Merger	-3.503	-3.975	-3.491	13.466	-18.420	-20.368**
	(-1.35)	(-1.61)	(-1.63)	(1.59)	(-1.62)	(-2.45)
REL×Post Merger×First	6.778			-14.483*	28.925**	16.003
	(0.90)			(-1.91)	(2.06)	(1.15)
REL×Post Merger×Others	8.970			-8.844	16.006	21.300*
	(1.30)			(-0.48)	(1.55)	(1.67)
REL×Post Merger×First		8.064				
		(1.01)				
REL×Post Merger×Others		15.812*				
		(1.81)				
REL×Post Merge× First			9.550			
			(1.20)			
REL×Post Merger×Others			13.085*			
			(1.65)			
Log(acquirer size)	-0.735	-0.740	-0.724	0.099	2.968	-3.104
	(-0.52)	(-0.52)	(-0.51)	(0.06)	(0.28)	(-0.89)
Relative size	1.222	1.112	1.094	4.461*	-2.069	10.161
	(0.42)	(0.38)	(0.45)	(1.79)	(-0.46)	(1.13)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared	0.568	0.568	0.568	0.571	0.573	0.681
Number of observations	18,994	18,994	18,994	14,176	3,336	1,108

Table 3.4 – *continued*

	All mergers	All mergers	All mergers	Borrowers of acquirer	Borrowers of target	Tar_acq borrowers
	(1)	(2)	(3)	(4)	(5)	(6)
Panel C. Conditioning on lending relationship						
*REL(dummy)*Old*	-9.417**			-10.137***	-15.175**	-8.420
	(-2.10)			(-2.63)	(-2.57)	(-0.88)
*REL(*number*)*Old*		-11.621**				
		(-2.16)				
*REL(amount)*Old*			-13.133***			
			(-2.69)			
Post Merger	-3.672	-2.066	-3.894	12.997*	-17.562**	-25.750**
	(-1.31)	(-1.46)	(-1.42)	(1.76)	(-2.49)	(-2.09)
REL×Old×Post Merger×First	8.044			-13.134*	21.529*	17.077
	(1.29)			(-1.77)	(1.72)	(1.07)
REL×Old×Post Merger×Others	9.412*			2.054	25.006**	23.166**
	(1.70)			(0.39)	(2.20)	(2.28)
REL×Old×Post Merger×First		9.549*				
		(1.76)				
REL×Old×Post Merger×Others		14.036**				
		(2.21)				
REL×Old×Post Merge× First			10.565**			
			(2.00)			
REL×Old×Post Merger×Others			13.080**			
			(2.17)			
Log(acquirer size)	-0.100	0.157	0.194	0.931	4.145	-6.160
	(-0.06)	(0.09)	(0.11)	(0.47)	(0.46)	(-1.04)
Relative size	3.232	3.217	3.348	6.331	-0.446	9.812
	(0.94)	(0.94)	(0.98)	(1.30)	(-0.04)	(0.96)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared	0.574	0.573	0.573	0.573	0.573	0.594
Number of observations	18,994	18,994	18,994	14,176	3,336	1,108

Table 3.5 The effect of merger types on cost of loans

The table presents the effect of different types of mergers on loan spreads for relationship borrowers and non-relationship borrowers. The dependent variable is the yield spread over LIBOR (or LIBOR equivalent) on the drawn amount plus the annual fee in bps. "Large acquirer & small target" are bank mergers where the target bank is small (bank size less than $0.5 billion) and the acquiring bank is large (bank size greater than $60 billion). "Partial overlap mergers" are bank merger where the buyer and target have active branches operating in some common markets, but the buyer has additional branches operating in different markets as well. In Panel A, we report the effect of bank mergers on cost of loans for all borrowers, relationship borrowers, and non-relationship borrowers separately. In Panel B, we report the regressions that condition on lending relationship. We exclude borrowers that borrowed from target banks prior to the bank merger and/or after the merger and report the results in Column (2) and (5) of Panel B. In Column (3) and (6) of Panel B, we only include borrowers that borrowed from target banks before and after the bank merger. *REL(M)* is the measure of relationship strength, estimated in three different ways: *REL(dummy), REL(number)* and *REL(amount).* *Post Merger* is a dummy variable that equals one for loan-year observations within two years after the completion of the bank merger, and zero for loan-year observations within five years prior to the announcement of the merger. *First* is a dummy variable that equals one for first relationship loan issued by the merged bank (i.e., the loan-year observations with *REL(dummy)*=1 and are closest to the completion of the merger), and zero for all other loans. *Others* is a dummy variable that equals one for all relationship loans issued by the merged bank except for the first one (i.e., *Others* equals zero when *First* equals one), and zero for all other loans. Unreported control variables are same as in Table 4, Panel A. Dummy variables for Fama-French 49 industries, calendar year, loan type, loan purpose, credit rating, syndicate size, and headquarter state are included in all regressions. T-statistics (in parentheses) are computed using robust standard errors corrected for clustering of observations at the firm level. All variables are defined in the Appendix and continuous variables are winsorized at 1% and 99%. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	Large acquirer & small target			Partial overlap mergers		
	All borrowers	Relationship borrowers	Non-relationship borrowers	All borrowers	Relationship borrowers	Non-relationship borrowers
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Without conditioning on lending relationship						
Post Merger	-23.310*	-16.388	-29.486*	11.063*	3.253	21.703**
	(-1.86)	(-0.94)	(-1.84)	(1.89)	(0.41)	(2.30)
Log(acquirer size)	-8.518**	-6.296*	-9.340	1.361**	1.872	1.156***
	(-2.40)	(-1.91)	(-1.60)	(2.09)	(0.17)	(3.65)
Relative size	-42.588	-57.067*	-56.480	-3.123	6.059	-9.130
	(-0.72)	(-1.84)	(-0.56)	(-0.27)	(0.83)	(-0.68)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared	0.607	0.654	0.589	0.585	0.610	0.595
Number of observations	1,228	838	390	2,296	1,375	921

Table 3.5 – *continued*

	Large acquirer & small target			Partial overlap mergers		
	All borrowers	Borrowers of acquirer	Borrowers of target	All borrowers	Borrowers of acquirer	Borrowers of target
	(1)	(2)	(3)	(4)	(5)	(6)
Panel B. Conditioning on lending relationship						
REL(dummy)	-10.123*	-11.254*	-2.770	-5.399	-9.228*	-3.780
	(-1.78)	(-1.83)	(-0.21)	(-1.63)	(-1.89)	(-0.99)
Post Merger	-28.414***	-16.277**	-55.895*	20.798**	22.618**	-15.629
	(-2.67)	(2.21)	(-1.74)	(2.36)	(2.38)	(-1.19)
REL×Post Merger×First	16.961	10.910	67.256*	-15.547	-22.152*	25.193
	(1.34)	(1.17)	(1.91)	(-1.37)	(-1.82)	(0.97)
REL×Post Merger×Others	23.213**	15.414**	70.450**	6.036	-4.953	32.591
	(2.34)	(2.11)	(2.14)	(1.49)	(-0.44)	(1.65)
Log(acquirer size)	-8.114**	-5.971	26.734	1.760**	1.616**	4.736
	(-2.60)	(1.33)	(1.36)	(2.47)	(2.21)	(0.92)
Relative size	-58.218	-50.516*	76.204	-3.755	-3.154	10.495
	(-0.93)	(-1.69)	(1.39)	(-0.29)	(-1.05)	(1.07)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared	0.613	0.624	0.636	0.571	0.583	0.621
Number of observations	1,228	944	284	2,296	1,791	505

Table 3.6 Lending relationship, bank mergers, and probability of pledging collateral

The table reports marginal effects from probit regressions of the effects of lending relationship and bank mergers on the probability of collateral requirement. The dependent variable, *secured loan*, is a binary variable equal to one if a loan facility is secured by collateral, and zero otherwise. "In-market mergers" are bank mergers where all the target's active branches in markets that the buyer also has active branches. "Market expansion mergers" are bank mergers where the target has no active branches operating in common markets with the buyer. We exclude borrowers that borrowed from target banks prior to the bank merger and/or after the merger and report the results in Column (7). In Column (8), we only include borrowers that borrowed from target banks before and after the bank merger. Panel A reports the results without any restrictions on the relationship borrowers. *REL(M)* is the measure of relationship strength, estimated in three different ways: *REL(dummy)*, *REL(number)* and *REL(amount)*. *Post Merger* is a dummy variable that equals one for loan-year observations within two years after the completion of the bank merger, and zero for loan-year observations within five years prior to the announcement of the merger. *First* is a dummy variable that equals one for first relationship loan issued by the merged bank (i.e., the loan-year observations with *REL(dummy)*=1 and are closest to the completion of the merger), and zero for all other loans. *Others* is a dummy variable that equals one for all relationship loans issued by the merged bank except for the first one (i.e., *Others* equals zero when *First* equals one), and zero for all other loans. All columns report, respectively, marginal effects on the probability of security for a one standard deviation increase in continuous variables and for a change from zero to one for dummy variables holding all other variables at their means. Marginal effects and standard errors for interactions are computed using the methods in Ai and Norton (2003). Dummy variables for the Fama-French 49 industries, calendar year, loan type, loan purpose, credit rating, and syndicate size are included in all regressions. All variables are defined in the Appendix and continuous variables are winsorized at 1% and 99%. Z-statistics (in parentheses) are computed using robust standard errors corrected for clustering of observations at the firm level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	All mergers	In-market mergers	Market expansion mergers	All mergers	All mergers	All mergers	Borrowers of acquirer	Borrowers of target
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post Merger	0.017	-0.055**	0.034*	-0.015	0.006	0.003	-0.017	-0.035
	(1.59)	(-2.46)	(1.72)	(-0.90)	(0.60)	(0.33)	(-1.32)	(-1.13)
REL(dummy)				-0.029**			-0.042***	0.024
				(-2.03)			(-3.02)	(0.70)
REL×Post Merger×First				0.038**			0.059***	-0.063*
				(2.30)			(2.79)	(-1.75)
REL×Post Merger×Others				0.044**			0.065***	-0.085**
				(2.42)			(3.03)	(-1.96)
REL(number)					-0.032**			
					(-2.15)			
REL×Post Merger×First					0.021			
					(1.43)			
REL×Post Merger×Others					0.037			
					(1.58)			
REL(amount)						-0.035**		
						(-2.13)		
REL×Post Merger×First						0.029*		
						(1.65)		
REL×Post Merger×Others						0.038*		
						(1.71)		
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pseudo R-squared	0.338	0.343	0.380	0.338	0.338	0.338	0.349	0.343
Number of observations	19,176	4,908	9,683	19,176	19,176	19,176	14,311	3,368

Table 3.7 Lending relationship, bank mergers, and loan maturity

The table presents the effect of bank mergers and lending relationship on loan maturity. The dependent variable is the natural log of the loan maturity in months. "In-market mergers" are bank mergers where all the target's active branches in markets that the buyer also has active branches. "Market expansion mergers" are bank mergers where the target has no active branches operating in common markets with the buyer. We exclude borrowers that borrowed from target banks prior to the bank merger and/or after the merger and report the results in Column (7). In Column (8), we only include borrowers that borrowed from the target bank before and after the bank merger. *REL(M)* is the measure of relationship strength, estimated in three different ways: *REL(dummy), REL(number) and REL(amount). Post Merger* is a dummy variable that equals one for loan-year observations within two years after the completion of the bank merger, and zero for loan-year observations within five years prior to the announcement of the merger. *First* is a dummy variable that equals one for first relationship loan issued by the merged bank (i.e., the loan-year observations with *REL(dummy)*=1 and are closest to the completion of the merger), and zero for all other loans. *Others* is a dummy variable that equals one for all relationship loans issued by the merged bank except for the first one (i.e., *Others* equals zero when *First* equals one), and zero for all other loans. *Asset maturity* is the weighted average of current assets divided by the cost of goods sold, and Net PPE divided by depreciation and amortization. Dummy variables for the Fama-French 49 industries, calendar year, loan type, loan purpose, credit rating, and syndicate size are included in all regressions. T-statistics (in parentheses) are computed using robust standard errors corrected for clustering of observations at the firm level. All variables are defined in the Appendix and continuous variables are winsorized at 1% and 99%. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	All mergers	In-market mergers	Market expansion mergers	All mergers	All mergers	All mergers	Borrowers of acquirer	Borrowers of target
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post Merger	-0.014	-0.052***	0.047	-0.018	-0.045*	-0.038*	0.003	-0.058**
	(-1.07)	(-3.08)	(1.21)	(-0.83)	(-1.90)	(-1.76)	(0.31)	(-2.34)
REL(dummy)				-0.048***			-0.035***	-0.066**
				(-3.05)			(-2.60)	(-2.20)
REL×Post Merger×First				-0.020			-0.023	0.054*
				(-1.38)			(-1.52)	(1.76)
REL×Post Merger×Others				-0.014			-0.017	0.078**
				(-0.80)			(-1.21)	(2.05)
REL(number)					-0.042**			
					(-2.43)			
REL×Post Merger×First					0.045			
					(1.55)			
REL×Post Merger×Others					0.060**			
					(2.10)			
REL(amount)						-0.037**		
						(-2.43)		
REL×Post Merger×First						0.035		
						(1.24)		
REL×Post Merger×Others						0.047		
						(1.61)		
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared	0.655	0.635	0.689	0.655	0.655	0.655	0.656	0.647
Number of observations	18,386	4,732	9,353	18,386	18,386	18,386	13,922	3,229

Table 3.8 Lending relationship, bank mergers, and number of loan covenants

The table reports Poisson regressions for the effects of lending relationship and bank mergers on the number of loan covenants. The dependent variable is a count index of the number of covenants in the loan. Following Bradley and Roberts (2015), the covenant index assigns one point for each of six covenants: secured debt, dividend restriction, a dummy variable for loans with more than two covenants restricting financial ratios, asset sale sweep, debt issuance sweep, and equity issuance sweep. The maximum covenant index is 6 and the minimum index is 0. "In-market mergers" are bank mergers where all the target's active branches in markets that the buyer also has active branches. "Market expansion mergers" are bank mergers where the target has no active branches operating in common markets with the buyer. We exclude borrowers that borrowed from the target bank prior to the merger and/or after the merger and report the results in Column (7). In Column (8), we only include borrowers that borrowed from the target bank before and after the merger. *REL(M)* is the measure of relationship strength, estimated in three different ways: *REL(dummy), REL(number) and REL(amount). Post Merger* is a dummy variable that equals one for loan-year observations within two years after the completion of the bank merger, and zero for loan-year observations within five years prior to the announcement of the merger. *First* is a dummy variable that equals one for first relationship loan issued by the merged bank (i.e., the loan-year observations with *REL(dummy)*=1 and are closest to the completion of the merger), and zero for all other loans. *Others* is a dummy variable that equals one for all relationship loans issued by the merged bank except for the first one (i.e., *Others* equals zero when *First* equals one), and zero for all other loans. Dummy variables for Fama-French 49 industries, calendar year, loan type, and credit rating are included in all regressions. All variables are defined in the Appendix and all continuous variables are winsorized at 1% and 99%. Z-statistics (in parentheses) are computed using robust standard errors corrected for clustering of observations at the firm level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	All mergers	In-market Mergers	Market expansion mergers	All mergers	All mergers	All mergers	Borrowers of acquirer	Borrowers of target
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post Merger	-0.018*	-0.095***	0.001	-0.048*	-0.029	-0.027	-0.039*	-0.182*
	(-1.82)	(-2.88)	(0.04)	(-1.81)	(-1.29)	(-1.22)	(-1.68)	(-1.90)
REL(dummy)				-0.028**			-0.031**	-0.015*
				(-2.32)			(-2.38)	(-1.92)
REL×Post Merger×First				0.046			0.048*	-0.025
				(1.29)			(1.89)	(-1.11)
REL×Post Merger×Others				0.051			0.075*	0.043
				(1.37)			(1.83)	(0.58)
REL(number)					-0.054***			
					(-2.59)			
REL×Post Merger×First					0.011			
					(0.21)			
REL×Post Merger×Others					0.047			
					(0.86)			
REL(amount)						-0.042**		
						(-2.45)		
REL×Post Merger×First						0.006		
						(0.12)		
REL×Post Merger×Others						0.043		
						(0.82)		
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pseudo R-squared	0.146	0.147	0.154	0.146	0.142	0.142	0.143	0.148
Number of observations	19,332	5,020	9,788	19,332	19,332	19,332	14,628	3,315

Table 3.9 Lending relationship, bank mergers and credit availability

The table presents the effect of bank mergers and lending relationship on credit availability. The dependent variable is the ratio of loan facility amount to total book assets. "In-market mergers" are bank mergers where all the target's active branches in markets that the buyer also has active branches. "Partial overlap mergers" are bank merger where the buyer and target have active branches operating in some common markets, but the buyer has additional branches operating in different markets as well. We exclude borrowers that borrowed from the target bank prior to the merger and/or after the merger and report the results in Column (7). In Column (8), we only include borrowers that borrowed from the target bank before and after the merger. *REL(M)* is the measure of relationship strength, estimated in three different ways: *REL(dummy), REL(number) and REL(amount). Post Merger* is a dummy variable that equals one for loan-year observations within two years after the completion of the bank merger, and zero for loan-year observations within five years prior to the announcement of the merger. *First* is a dummy variable that equals one for first relationship loan issued by the merged bank (i.e., the loan-year observations with *REL(dummy)*=1 and are closest to the completion of the merger), and zero for all other loans. *Others* is a dummy variable that equals one for all relationship loans issued by the merged bank except for the first one (i.e., *Others* equals zero when *First* equals one), and zero for all other loans. Dummy variables for Fama-French 49 industries, calendar year, loan type, loan purpose, and credit rating are included in all regressions. T-statistics (in parentheses) are computed using robust standard errors corrected for clustering of observations at the firm level. All variables are defined in the Appendix and all continuous variables are winsorized at 1% and 99%. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	All mergers	In-market mergers	Partial overlap mergers	All mergers	All mergers	All mergers	Borrowers of acquirer	Borrowers of target
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post Merger	0.009	0.046**	-0.035**	0.019	0.010	0.012	0.022*	0.016
	(1.44)	(2.35)	(-2.10)	(1.41)	(0.98)	(1.30)	(1.90)	(0.15)
REL(dummy)				0.014			0.016	0.005
				(1.09)			(1.11)	(0.96)
REL×Post Merger×First				0.015			0.015	0.066*
				(1.54)			(1.43)	(1.79)
REL×Post Merger×Others				-0.024***			-0.025**	-0.077
				(-2.59)			(-2.29)	(-1.05)
REL(number)					0.015			
					(1.11)			
REL×Post Merger×First					0.035***			
					(2.58)			
REL×Post Merger×Others					-0.028**			
					(-2.28)			
REL(amount)						0.019**		
						(2.23)		
REL×Post Merger×First						0.032**		
						(2.48)		
REL×Post Merger×Others						-0.024*		
						(-1.86)		
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared	0.203	0.206	0.302	0.203	0.203	0.204	0.194	0.291
Number of observations	19,332	5,020	2,446	19,332	19,332	19,332	14,628	3,315

CONCLUSION

In the first chapter, we find that corporate governance has opposite effects on financial leverage depending on whether a firm is diversified or focused. Entrenched managers in focused firms eschew leverage, whereas entrenched managers in diversified firms take advantage of their better access to debt finance and use more financial leverage. Our evidence shows that the conflict in the literature on the relation between leverage and managerial entrenchments is because earlier empirical studies do not condition on the diversification status of firms. In the second chapter, using two measures for investment riskiness, cash flow volatility and unlevered stock return volatility, we find that investment riskiness is significantly positively correlated to average employee pay. Our results emphasize the importance of human capital costs in a firm's investment policy and provide a new insight for the underinvestment problem. In the third chapter, we find that compared to non-relationship borrowers, merged banks require higher loan spreads from relationship borrowers. This "hold-up" effect is more pronounced in mergers when a large bank acquires a small bank. We find that although the "hold-up" effect is stronger on loan spreads of target bank borrowers compared to acquirer borrowers, merged banks decrease the availability of credit more to acquirer borrowers than to target borrowers. Continuing target borrowers appear to benefit from larger loan availability post-merger.

It is difficult to fully understand the consequences of relationship lending in the context of bank organization, since so many factors can influence the final results, such as borrower characteristics, loan characteristics, merging banks' characteristics, deal characteristics, and reactions of non-merging banks and other market participants. This paper takes a small step to add some insights to this topic. There are many other interesting

questions in this framework. For example, whether relationship borrowers are more likely to be dropped post-merger, and what are the reactions of non-merging banks or other market participants? Our empirical framework allows us to capture some dynamic changes attributable to relationship lending. It can be applied to study other shocks in the banking industry, or how the borrower's behavior influences a bank's decision to make relationship loans.