ESSAYS ON CORPORATE INVESTMENT AND PAYOUT POLICIES

by

HUAN YANG

(Under the Direction of Jie He and Annette B. Poulsen)

ABSTRACT

This dissertation comprises two independent essays that examine how the shareholder-creditor conflicts affect corporate investment, and how the enhanced labor power influences corporate payout policies. In the first chapter, I analyze the impact of shareholder-creditor conflicts on corporate risk-taking. In particular, I examine the role played by institutional dual-holders (i.e., those simultaneously holding a same firm's debt and equity) in corporate innovation. I find that firms held by dual-holders generate fewer but more valuable patents. To establish causality, I use a difference-in-differences approach based on the quasi-natural experiment of financial institution mergers. I further find that the decreased sensitivity of managerial compensation to firm risk might be one possible channel through which dual-holders affect risk shifting. The results suggest that shareholder-creditor conflicts indeed exist and lead to risk shifting, and that institutional dual ownership can partially mitigate this problem. The second chapter studies labor power as an important but largely under-explored determinant of corporate payout policy. Using a regression discontinuity approach that exploits locally exogenous variation in labor's collective bargaining power, I find that an increase in labor power lowers corporate payout. Operating flexibility is a plausible underlying mechanism through which labor power influences corporate payout. Firms use the saved earnings from reductions in payout to invest in net working capital rather than paying off debt or increasing cash holdings. This essay sheds new light on the determinants of payout policy and the role of labor power in corporate finance decisions.

INDEX WORDS: Shareholder-creditor conflicts, Dual ownership, Asset substitution, Innovation, Payout policy, Labor power, Operating flexibility

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DEDICATION

To my mom and dad for their endless and unconditional love, support and encouragement.

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CHAPTER 1

INSTITUTIONAL DUAL HOLDINGS AND RISK SHIFTING: EVIDENCE FROM CORPORATE INNOVATION[1]

[1] Yang, Huan. To be submitted to *Journal of Financial Economics*.

Abstract

This paper analyzes the impact of shareholder-creditor conflicts on corporate risk-taking. In particular, I examine the role played by institutional dual-holders (i.e., those simultaneously holding a same firm's debt and equity) in corporate innovation. Baseline results show that firms held by dual-holders generate fewer but more valuable patents. To establish causality, I use a difference-in-differences approach based on the quasi-natural experiment of financial institution mergers. The evidence is consistent with the hypothesis that institutional dual-holders can mitigate shareholder-creditor conflicts and thus curb excessive risk-taking. I further find that the decreased sensitivity of managerial compensation to firm risk might be one possible channel through which dual-holders affect risk shifting. Overall, the paper offers new evidence that shareholder-creditor conflicts indeed exist and lead to risk shifting (asset substitution), and that institutional dual ownership can partially mitigate this problem.

1.1 INTRODUCTION

Conflicts of interest between shareholders and creditors have important implications for a firm's investment and value (Jensen and Meckling, 1976; Myers, 1977; Smith and Warner 1979; Leland, 1994). Corporate managers, who only bear fiduciary responsibility to shareholders, may have incentives to invest in risk-increasing, negative net present value (NPV) projects because if the projects fail the losses will be shared with creditors, but if the projects succeed the shareholders will keep all the residual profits. The root of the shareholder-creditor conflicts is the different nature of cash flows that shareholders and creditors are each entitled to. Although a large number of theoretical studies (e.g., John, 1987; Parrino and Weisbach, 1999) have analyzed the causes and consequences of shareholder-creditor conflicts, especially the risk shifting (asset substitution) problem, direct evidence on the causal impact of these conflicts on corporate risk-taking is still scant due to the difficulty of measuring such conflicts and potential endogeneity concerns. In fact, there has been no consensus on whether the shareholder-creditor conflicts actually exist and whether they are important enough to affect corporate decisions.[2]

This paper attempts to shed new light on the causal effect of shareholder-creditor conflicts on corporate risk-taking. Specifically, I examine this classical problem by exploiting a unique setting to assess the existence and magnitude of shareholder-creditor conflicts: the simultaneous holdings of a firm's debt and equity by the same institutional investor (henceforth, "dual-holder"), which have gained tremendous growth in the past few decades (Jiang, Li, and Shao, 2010). Institutional dual-holders, due to their large stakes in both a firm’s debt and equity, have strong incentives and abilities to internalize the conflicts of interest between shareholders

[2] For example, Fama and Miller (1972) argue that such conflicts are "probably unimportant" and Myers (1984) barely mentions them in his presidential address on capital structure. On the other hand, Smith and Watts (1992) emphasize that conflicts of interest between shareholders and creditors play an important role in shaping the capital structure. Further, Brealey, Myers, Allen and Mohanty (2012) and Eisdorfer (2008) suggest that stockholder-creditor conflicts are more serious when firms are in financial distress.

and creditors. By analyzing the extent to which a firm is owned by such dual-holders, I can identify the impact of shareholder-creditor conflicts on corporate risk-taking without directly measuring such conflicts, which is notoriously known to be difficult.

Different from the previous literature, I measure a firm's degree of risk-taking by its innovation activities which are highly risky and involve a high probability of failure because such activities depend on the exploration of unknown methods and areas rather than the exploitation of existing knowledge. Due to their unpredictable and idiosyncratic nature, the outcome of innovations involves many contingencies that are difficult to foresee and write contracts on, leading to an incomplete contracting problem between shareholders and creditors that could exacerbate the conflicts of interest between them (Stiglitz, 1985). Therefore, a firm's innovation activities directly reflect its risk-taking. In contrast, prior studies mainly use capital expenditures (e.g., Kaplan and Zingales, 1997; Mayers, 1998; Korkeamaki and Moore, 2004; Fazzari, Hubbard, Petersen, Blinder, and Poterba, 1988; Hoshi, Kashyap, and Scharfstein, 1991; Eisdorfer, 2008) to proxy for investment risk. However, these measures may not accurately reflect the riskiness of the new projects taken by the managers as capital expenditures could simply be used to maintain existing machines and equipment.[3] Moreover, they cannot directly reflect the value implications of the investments as capital expenditures are only one form of observable investment inputs.

In this paper, I use the number of patents generated by a firm to measure its investment intensity in risky projects (i.e. the quantity of risky projects).[4] More importantly, with detailed

[3] Fang and Zhong (2004) and Hjortshoj and Wei (2009) use contingent-claim approaches that are built on equity's call option features to back out the volatility of asset's market value to proxy for risk-taking. However, as the authors admit, this measure may not be accurate due to capital market frictions. Cash flow volatility has also been used to proxy for risk-taking (e.g., Laeven and Levine, 2009). But this measure does not directly measure risk-taking and is subject to product market competition (Irvine and Pontiff, 2009) and financial policies.

[4] Prior literature mainly uses R&D expenditures to measure a firm's innovation activity. However, R&D expenditures suffer from the problem of missing values. For example, more than 50% of firms do not report R&D

information about each patent's technology class classification, I am able to observe the scope of a firm's innovation which has a strong value implication because patents with a more focused scope on the firm’s core businesses are more valuable (i.e., having a higher NPV) (e.g., Gu, Mao and Tian, 2014; Hirshleifer, Low and Teoh, 2012; John and Ofek, 1995; Berger and Ofek 1995; Comment and Jarrell, 1995). Furthermore, using the data provided by Kogan, Papanikolaou, Seru, and Stoffman (2016), I can directly measure the market value of each patent. These unique features of patent-based risk-taking measures make them superior to those examined in the previous literature (e.g. Eisdorfer, 2008; Fang and Zhong, 2004; Hjortshoj and Wei, 2009; Laeven and Levine, 2009) as they allow us to directly examine the value implication of shareholder-creditor conflicts and the mitigation of them via institutional dual holdings rather than relying on some indirect measures (such as a firm’s equity value) to infer the value impact of such capital market frictions.

As discussed in the seminal work of Jensen and Meckling (1976) and Smith and Warner (1979), the objectives of a firm’s shareholders and creditors diverge from one another. While both of them share the same downside risk of a project, shareholders would grab most or all of the profits from the upside of the project. In particular, a levered firm’s equity carries the feature of a call option on the firm’s total assets and thus has a positive relationship with its risk (Merton, 1974). In contrast, the value of a firm’s debt is negatively related to the firm’s risk. Therefore, managers, who act in the interest of shareholders, might take highly risky projects whose NPV might even be negative as long as the expected gains in equity value due to the

expenditures in their financial statements in Compustat. However, not reporting R&D expenditures does not necessarily mean that firms do not engage in innovation activities. Another problem is that we cannot measure the economic value of a firm’s innovation activities by using its R&D expenditures because they are just one observable innovation input but not an output. The quality and value of innovation outcomes may vary dramatically among firms.

higher risk dominate the expected loss in debt value due to the decrease in total firm value.[5] As a result, managers, in the presence of corporate debt, have an incentive to increase firm risk by undertaking overly risky projects so as to increase the equity value. Although creditors understand shareholders' risk shifting incentives and can potentially protect themselves against such opportunistic behavior by charging higher interest rates and adding more stringent provisions to loan contracts such as covenants and collaterals, these protections are far from perfect in practice (Dichev and Skinner 2002; Chava and Roberts 2008), which could not eliminate the existence of risk shifting in equilibrium. Moreover, even if creditors, in anticipation of the risk shifting behavior, can fully protect themselves by adopting the various techniques mentioned above, shareholders (and managers) would still have an incentive to take risky, negative-NPV projects (that only destroy equity value but not debt value in this scenario) because they would be worse off if they refuse to do so.[6] Therefore, if the conflicts between shareholders and creditors do exist, the alignment of their interests from institutional dual holdings who internalize such negative externalities should induce firms to produce fewer

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[5] Here is a simple numerical example. Consider a firm whose asset's value is $100 million and whose variance in the assets is 0.16. The firm issued a zero-coupon bond with face value of $80 million that matures in ten years. If the risk-free rate is 10%, based on the Black-Sholes model, the equity value is $75.94 million and the debt value is $24.06 million. Now assume that the firm takes a risky project with a negative NPV of -$2 million. Since this project is highly risky, it pushes up the variance in the assets to 0.25. Under this new assumption, the new equity value would be $77.71 million and the new debt value would be $20.29 million. Even though the total firm value declines by $2 million, the equity value increases by $1.77 million, which comes at the expense of creditors whose wealth declines by $3.77 million.

[6] In other words, shareholders would bear the full agency costs of debt and experience a loss in equity value if creditors suspect their risk shifting incentives and take actions to protect the debt value. Due to shareholders' inability to credibly convince creditors *ex ante* (i.e., at the time of borrowing) that they would not take risky, negative-NPV projects *ex post*, rational creditors would take actions to protect their own interests, and such actions would make it optimal for shareholders to later take excessively-risky and value-destroying projects, even if such action ultimately reduces equity value. To illustrate further using the numerical example in footnote 5, if creditors anticipate shareholders' risk shifting incentives and the associated expected loss of $3.77 million at the time of lending, they could protect themselves by charging a higher interest rate and ask for an additional amount of $3.77 million in the debt contract *ex ante*. Then managers (and equity holders) would incur a total loss of $2 million ($1.77 - $3.77 = - $2 million) after they take the negative-NPV project *ex post*. Note that in this case, managers have to take the negative-NPV project because otherwise the equity value would lose even more (i.e., the $3.77 million upfront cost due to creditors' lack of trust).

patents (i.e., fewer risky projects) but the patents generated should have a higher average value (i.e., with higher average NPV).

My baseline ordinary least squares (OLS) regressions demonstrate that the presence of institutional dual-holders has a significantly negative impact on the quantity of innovation output after controlling for factors commonly believed to affect innovation, suggesting that firms with institutional dual ownership engage in fewer risk-taking activities. Further tests show that the reduction of innovation output concentrates on patents that are not related to firms' core businesses as those patents are less valuable to the firm (Gu, Mao, and Tian, 2014). Meanwhile, I find that firms with dual ownership generate patents with higher average market value, indicating that dual-holders induce firms to improve the quality and value of their risky innovation projects. All these results are robust to using alternative subsamples, alternative measures of dual ownership, and alternative empirical specifications.

While the baseline results are consistent with the existence of shareholder-creditor conflicts and dual-holders' risk-shifting-reduction role, an even more challenging job is to draw a causal inference on the effect of dual ownership on firms' risk-taking behavior. The choice of institutional investors to become dual-holders for a particular firm is clearly not exogenous. It could be correlated with the firm's unobservable characteristics such as managerial traits that also affect its innovation (i.e., the omitted variable concern).[7] Alternatively, a firm with a lower level of innovation activity is less risky and hence might be more likely to be dual-held by institutions (i.e., the reverse causality concern). Both concerns could hinder me from identifying a causal effect of dual ownership on innovation. To establish causality, I follow recent literature on institutional cross-holdings (e.g., He and Huang, 2017) to use a quasi-natural experiment of

[7] For example, Hirshleifer, Low, and Teoh (2012) find that firms with overconfident CEOs invest more in innovation and have greater return volatility which may make them less attractive to dual-holders.

financial institution mergers, which leads to a possibly exogenous change in a firm's dual ownership status but is not likely to be directly correlated with its innovation activities because these mergers are usually driven by deregulations and strategic business considerations. Using a difference-in-differences (DiD) approach, I find that due to an exogenous increase in dual ownership, treatment firms (i.e., firms whose equity is held by one party to the institution merger and whose debt is held by the other party before the merger) experience a larger decrease in the total number of patents, especially the number of non-core-business patents relative to control firms (i.e., firms whose equity/debt is held by one party to the institution merger and whose debt/equity is not held by the other party before the merger). Therefore, dual-holders lead to a more focused innovation scope that is within firms' core businesses. Meanwhile, in the DiD setting I continue to find that dual ownership has a significantly positive impact on the market value of patents generated. Furthermore, the effects of dual ownership on innovation outcomes are stronger when a firm is in financial distress where shareholder-creditor conflicts are likely to be exacerbated and when the firm's loan facilities involve participation of non-commercial-bank dual-holders who are more likely to take an active role in corporate decision-makings. In sum, my results are consistent with the hypothesis that dual-holders align the interests of creditors and equity holders so that the classical risk-shifting problem is mitigated to some extent.

A natural question to ask is why institutional lenders hold their borrower's equity simultaneously when such dual holdings make their portfolios of assets underdiversified. One reason discussed in the literature is that dual ownership would reduce the adverse selection problem and thus make the debt portion of such holdings less risky (Jiang, Li, and Shao, 2010). Another reason, shown in this paper, is that dual holdings could induce firms to reduce excessive, value-destroying investment. In both cases, dual ownership should increase firm value. To examine this possibility, I further investigate the effect of dual ownership on a firm's total value proxied by its Tobin's Q. I use the same DiD setting and find that treatment firms' Tobin's Q experiences a larger increase than that of control firms. This result confirms the conjecture that dual-holders are able to mitigate the shareholder-creditor conflicts by preventing managers from taking overly risky projects that might lower the total value of the firm.

In the final part of the paper, I attempt to identify possible mechanisms through which dual-holders affect risk-shifting. One mechanism I consider is managerial compensation. Previous literature finds that institutional investors can influence executive compensation (e.g., Hartzell and Starks, 2003; Almazan, Hartzell, and Starks, 2005; Chhaochharia and Grinstein, 2009) and that the sensitivity of managers' equity-based payoff to stock price volatility (i.e., *vega*) is used as a device to encourage managerial risk-taking (e.g., Guay, 1999; Coles, Daniel, and Naveen, 2006; Low, 2009; Chang, Fu, Low, and Zhang, 2015). To explore this possible channel, I examine how dual ownership affects *vega* in the institutional merger setting, and find that treatment firms' *vega* decreases to a larger extent than that of the control firms in response to an exogenous increase of dual ownership, suggesting that the lower sensitivity of managerial incentive compensation to stock price volatility might be a plausible mechanism through which dual-holders affect risk-shifting.

One might argue that corporate governance can serve as an alternative channel through which dual ownership reduces corporate risk-shifting. Recent studies show that institutional investors can improve corporate governance by making managers less entrenched (e.g., Brav, Jiang, Partnoy, and Thomas, 2008; Appel, Gormley, and Keim, 2016). Thus, when a financial institution simultaneously holds a firm’s debt and equity, it might have more power in shaping the firm’s corporate governance structure. If this is the case, then corporate governance may be an underlying mechanism through which dual ownership affects asset substitution. However, this explanation is inconsistent with my empirical findings. Here are the reasons. On the one hand, some papers argue that better governance reduces managerial entrenchment, which is actually bad for both the quantity and quality of corporate innovation (Stein, 1988; Chemmanur and Tian, 2015). Therefore, the documented negative effect of dual-holders on the number of patents could be partly caused by the fact that dual-holders actively improve corporate governance through their enhanced power and incentives, making managers less entrenched. However, this entrenchment-based governance effect cannot explain my finding that dual-holders improve (rather than reduce) the quality of generated patents (as proxied by the market value and industry-relatedness of the patents). On the other hand, due to moral hazard and career concerns, managers are also more likely to invest in fewer innovative projects and those projects that require less effort and human capital. Improved corporate governance may mitigate these problems by disciplining managers and forcing them to focus on more innovative projects with higher value (Atanassov, 2013). Thus, the documented negative effect of dual-holders on the value of generated patents could also be partly caused by the fact that dual-holders positively affect corporate governance and better discipline the managers. However, this moral-hazard-based governance effect cannot explain my finding that dual-holders decrease (rather than increase) the number (quantity) of generated patents. Overall, based on the patenting related evidence, corporate governance seems unlikely a main channel through which dual ownership mitigates risk shifting.

My paper makes three main contributions to the literature. First, using institutional dual ownership to measure shareholder-creditor conflicts, this paper provides large-sample empirical evidence on the impact of such conflicts on risk-shifting. More importantly, it establishes a causal link between the two by using a DiD approach based on the quasi-natural experiment of financial institution mergers. Second, by using output-based measures such as patents and their market value, this paper overcomes the limitation of using input-based measures to capture corporate risk taking, and thus contributes to the classical literature on asset substitution (risk shifting). Third, my paper extends the recent literature on the implications of institutional dual ownership for corporate behavior and identifies a new channel through which dual-holders improve firm value.

The rest of this paper is organized as follows. Section 2 discusses related literature and develops hypotheses. Section 3 describes sample selection and reports summary statistics. Section 4 presents the baseline results. Section 5 addresses endogeneity issues by using institutional mergers and acquisitions and carries out additional cross-sectional analyses. Section 6 studies the economic implication for firm value. Section 7 discusses possible underlying economic mechanisms. Section 8 concludes.

1.2 LITERATURE AND HYPOTHESIS DEVELOPMENT

1.2.1 Relation to the Existing Literature

This paper contributes to at least four strands of literature. First, this paper contributes to the literature on shareholder-creditor conflicts and firms' risk-shifting behavior. Theoretical papers, such as Jensen and Meckling (1976), Smith and Warner (1979), and Leland (1998), have studied how shareholder-creditor conflicts affect corporate risk-shifting incentives. However, very few empirical papers directly evaluate the actual existence of the shareholder-creditor conflicts due to the difficulty to measure such conflicts. For example, Graham and Harvey (2001) and De Jong and Van Dijk (2007) use surveys to study the corporate risk-shifting and find little evidence. Esty (1997) finds that risk-shifting was significant in the savings and loan industry in the 1980s through case studies. Parrino and Weisbach (1999) apply numerical techniques to illustrate how shareholder-creditor conflicts distort firms' investment. Andrade and Kaplan (1998) examine risk-shifting using investment activities of 31 financially distressed firms. In addition, Eisdorfer (2008) measures investment intensity using gross capital expenditures divided by property, plant, and equipment and finds evidence of risk-shifting in financially distressed firms.

My paper provides large sample empirical evidence of the impact of shareholder-creditor conflicts on corporate risk-shifting by analyzing firms in all industries (whether they are distressed or not) and contributes to this strand of literature in three important ways. First, my study cleanly measures the magnitude of shareholder-creditor conflicts by using institutional dual ownership. Second, the patent-based measures used in this paper allow me to better capture firms' risk-shifting behavior because innovation activities are by nature very risky and uncertain. Moreover, patent-based measures are output-based and enable us to directly examine the value implications of risk-taking behaviors. In contrast, the measures used in most previous studies are input-based and cannot directly reflect the quality and economic value of risky projects, preventing us from drawing clear implications about asset substitution (risk shifting). Last but not least, by using a DiD approach based on financial institution mergers, my paper mitigates potential endogeneity concerns and identifies the causal impact of shareholder-creditor conflicts on risk-shifting.

This paper also contributes to recent studies on institutional dual holdings of both equity and debt in the same firm. Jiang, Li, and Shao (2010) study how a firm's dual ownership affects its borrowing costs and find that syndicated loans with dual-holder participation have lower loan yield spreads than those without dual-holders. In addition, they also find that dual-holders have longer investment horizon than non-dual-holders and improve borrowers' credit ratings. Chava, Wang, and Zou (2015) investigate how creditors' simultaneous debt and equity holdings affect loan contracting and find that dual ownership significantly reduces the likelihood of a capital expenditure restriction covenant *ex ante* and dual-holders are more likely to grant unconditional waiver when firms violate covenants *ex post*. Bodnaruk and Rossi (2016) show that target firms with larger equity ownership by dual-holders experience lower equity premiums and larger abnormal bond returns in mergers and acquisitions (M&As), suggesting a coordination effect within dual holding institutions in M&As. A recent paper by Chu (2016) shows that dual-holders reduce dividend payout. My paper expands the scope of dual ownership research by studying its impact on firms' risk-shifting behavior and identifies a new channel through which incentive alignment and enhanced monitoring from dual ownership could curb excessive risk-taking to increase firm value.

This paper adds to an emerging literature on finance and innovation. In the past few years, the literature on innovation has been growing rapidly due to the importance of this topic. Recent theoretical studies investigate the relation between innovation and firm characteristics. For example, Robinson (2008) shows that firms with limited internal ability to innovate would choose to form strategic alliances to engage in innovative projects. Manso (2011) theoretically models managerial contracts that tolerate failure in the short run and reward success in the long run can motivate managers to conduct innovation activities. Ferreira, Manso, and Silva (2014) argue that it is optimal for firms to remain private when exploring innovative projects. In addition, a growing strand of empirical research examines the relationship between firm innovation and market characteristics including within-industry competition (Aghion, Bloom, Blundell, Griffith, and Howitt, 2005), financing (Amore, Schneider, and Zaldokas, 2013; Cornaggia, Mao, Tian, and Wolfe, 2015; Nanda and Rhodes-Kroff, 2016; Acharya and Xu, 2016), bankruptcy laws (Acharya and Subramanian, 2009), short sellers (He and Tian, 2015), and investors' tolerance toward failure (Tian and Wang, 2014). Other empirical studies also investigate the link between innovation and firms characteristics including corporate governance (Chemmanur and Tian, 2015), stock liquidity (Fang, Tian, and Tice, 2014), analyst coverage (He and Tian, 2013), firm boundaries (Seru, 2014), corporate venture capital (Chemmanur, Loutskina, and Tian, 2014), institutional ownership (Aghion, Reenen, and Zingales, 2013), and private equity ownership (Lerner, Sorensen, and Stromberg, 2011). However, there are no papers that study how institutional dual-holders influence a firm's innovation.

Lastly, my paper is also broadly related to the growing literature on institutional cross-holdings. In the last few decades, public firms have become increasingly connected through common institutional ownership. Matvos and Ostrovsky (2008) argue that institutional investors that hold stakes in both the acquirer and the target do not lose money around the public merger announcement. However, Harford, Jenter, and Li (2011) claim that such cross-holdings are too small to affect the outcome in most acquisitions. Azar, Schmalz, and Tecu (2015) show that common ownership impedes competition and causes higher product prices in the US airline industry. On the other hand, using more comprehensive data covering all industries, He and Huang (2017) documents that institutional cross ownership facilitates product market coordination and improves cross-held firms' market share growth.

1.2.2 Institutional Background and Hypotheses

During the past thirty years, institutional investors have enlarged their equity ownership in US public firms from about 30% in the 1980s to over 70% in the 2000s, and have been playing an increasingly important role in many aspects of corporate governance (Gompers, and Metrick, 2001; Cremers and Nair, 2005; Brav, Jiang, Partnoy, and Thomas, 2008; Brav, Jiang, and Kim, 2015; Appel, Gormley, and Keim, 2016; He and Huang, 2017). Meanwhile, the repeal of the Glass-Steagall Act in 1999 made commercial banks and equity-holding financial institutions subject to similar regulations, motivating them to enter the traditional business domain of each other. As a result, more and more institutional investors have started to participate in loan syndication which used to be the exclusive playground of commercial banks. Jiang, Li, and Shao (2010) document a significant growing participation rate of institutional investors in syndicated loans from between 10% and 30% of all the loan facilities in the DealScan database before 1996 to more than 60% after 1996. Furthermore, greater consolidation of financial sectors has created bigger conglomerates out of commercial banks, investment banks, and asset management and insurance firms. All of these developments in the financial system have led to a relatively new phenomenon: the simultaneous holding of a firm's both debt and equity (dual holding) by the same financial institution.[8] For example, in 1992, Bankers Trust Co. of New York was Northwest Airline's leading lender and equity holder when the latter was seeking additional loans to tide through the winter slump.[9] Another example is that General Electric's common shares accounted for 3.5% of Wellington Management Co.'s total equity

[8] Note that dual-holders existed even before the repeal of the Glass-Steagall Act. Although commercial banks cannot directly trade in firms' equity, they may still hold shares in an equity investment trust on behalf of their clients (Santos and Wilson, 2007). In addition, commercial banks are usually part of the bigger financial conglomerates with affiliated investment arms, including investment banks, mutual funds, pension funds, and insurance companies. Previous literature has documented that through such affiliations, financial conglomerates trade on their borrowers' stocks based on the information acquired from lending (e.g., Acharya and Johnson, 2007; Massa and Rehman, 2008; Ivashina and Sun, 2011).

[9] See at "Northwest Airline Wins Concessions Worth $900 Million", *Buffalo News*, November 20, 1992.

holdings in 2005. At the same time, 3.5% of Wellington's debt portfolio consisted of General Electric's long-term bonds.

While creditors have a fixed claim on firm value, shareholders only hold a residual claim on firm value and bear limited liabilities when things do not work out well. According to the agency theory of shareholder-creditor conflicts, the different nature of cash flow claims makes creditors vulnerable to shareholders' opportunistic behavior, especially their action of risk-shifting. Shareholders and creditors share the same downside risk of projects, but shareholders grab most of the profits from the upside of projects. Since equity is a call option on the value of a firm's total assets (Merton 1974), an increase in the risk in firm assets would lead to an increase in the value of equity. Managers, who hold ultimate fiduciary responsibility to the shareholders, may therefore engage in overly risky (and sometimes even value-destroying) projects that hurt the interests of creditors while making shareholders better off. Understanding such risk-shifting incentives of the equity holders, creditors may protect themselves against shareholders' opportunistic behavior by charging higher interest rates and adding more stringent provisions to loan contracts such as covenants and collaterals. When this happens, shareholders would bear the full agency costs of debt and experience a reduction in equity value because they would have been worse off if they do not continue to take the excessively-risky and value-destroying projects. Due to shareholders' inability to credibly convince creditors *ex ante* (i.e., at the time of borrowing) that they would not take risky, negative-NPV projects *ex post*, rational creditors would take actions to protect their own interests, which would force shareholders to actually take such excessively-risky and value-destroying projects in equilibrium even if doing so only hurts equity value. This is the classical problem of risk-shifting (asset substitution): due to the diverging attitudes of shareholders and creditors towards risk taking and the lack of a credible commitment mechanism between the two, inefficient investment would be made in equilibrium and firm (especially equity) value suffers. Institutional dual-holders, given their claims in a firm's both equity and debt, have the incentive and the ability to serve as a commitment mechanism and mitigate the conflicts of interest between shareholders and creditors in order to prevent managers from undertaking overly risky projects that destroy firm value. By doing so, dual-holders could increase the combined value of their debt and equity holdings. Meanwhile, dual-holders are generally influential investors and their voice is more likely to be heard by the management. Therefore, if the conflicts of interest between shareholders and creditors are large enough to lead to suboptimal risk-shifting behavior in terms of innovation activities, we would expect to observe a negative impact of dual holdings on the quantity of a firm's innovation output. This leads to the following hypothesis.

Hypothesis 1: All else equal, firms with institutional dual-holders generate fewer patents in general than those without dual-holders.

In addition, if dual-holders can mitigate risk-shifting incentives and induce managers to cut down suboptimal innovation projects, we would expect to observe a positive impact of dual holdings on the value and quality of a firm's average innovation project. One measure for innovation quality is the relatedness of a firm's patents to its core businesses. As argued in Gu, Mao, and Tian (2015), innovation activities that are not related to a firm's core business are likely to be suboptimal because these patents may be out of managers' expertise and hence are more likely to be the result of overly risky investment. Thus we should expect to see a negative effect of dual holdings on the number of patents that are unrelated to a firm's core businesses.

Another measure for innovation quality is the market value of each granted patent (Kogan, Papanikolaou, Seru, and Stoffman 2016). Hence, we should also expect a positive effect of dual holdings on the market value of a firm's generated patents. These arguments lead to the following two hypotheses.

Hypothesis 2: All else equal, firms with institutional dual-holders generate fewer patents that are unrelated to their core businesses than those without dual-holders.

Hypothesis 3: All else equal, the patents generated by firms with institutional dual-holders have a higher market value than those generated by firms without dual-holders.

1.3 SAMPLE SELECTION AND SUMMARY STATISTICS

1.3.1 Sample Selection

The sample examined in this paper includes US-listed firms with common stock outstanding during the period of 1987-2009.[10,11] The datasets used to conduct this study are obtained from various sources. Debt holdings are from Reuters Loan Pricing Corporation's (LPC) DealScan syndicated loan database. The DealScan database reports loan contracting information gathered from SEC filings, industry contracts, and directly from borrowers and lenders. Equity holdings by institutions are from Thomson Financial Ownership database which compiles institutional equity holdings disclosed in 13F filings. Firms' financial statements and

[10] There were not too many loan deals before 1987 (less than a couple of hundreds per year).
[11] The patent datasets from Kogan, Papanikolaou, Seru, and Noah (2016) and Harvard Business School actually end in 2010. Due to truncation problems, the data in 2010 is not reliable (Lerner and Seru 2015). However, my results still hold if I include it.

stock pricing information are from Compustat and The Center for Research in Security Prices (CRSP), respectively.

Innovation variables are constructed from the latest version of the National Bureau of Economic Research (NBER) Patent Citation database.[12] This database contains detailed information on more than three million patents granted by the USPTO from 1976 to 2006. The patent database reports information on patent assignee (owner) names, the number of patents, the number of citations each patent receives, a patent's application year, a patent's grant year, and so on. In addition, information on patents granted from 2007 to 2009 is obtained from Kogan, Papanikolaou, Seru and Stoffman (2016) (available at https://iu.box.com/patents) and the citation database over this period is supplemented using the Harvard Business School patent database (available at http://dvn.iq.harvard.edu/dvn/dv/patent). To address the concern that my results are driven by the right-skewed distribution of the patent variables (since some firms do not have any patents), I require firms in the sample to generate at least one patent during the sample period.[13]

1.3.2 Variable Measurement

1.3.2.1 Measuring Innovation

Following prior literature, I use a firm's total number of patent applications filed in a given fiscal year that are eventually granted to gauge the firm's innovation productivity. The reason to use a patent's application year rather than grant year is that as suggested by the innovation literature (e.g., Griliches, Hall, and Pakes, 1991), the application year better captures the actual timing of innovation output. I also adjust this innovation measure to address truncation problems. The truncation problem arises from the fact that I can only observe patents which are

[12] See Hall, Jaffe, and Trajtenberg (2001) for more details.
[13] My regression results are robust to the inclusion of firms without any patents.

eventually granted by the end of 2010. Patents filed in the last few years may still be under review due to the lag between a patent's application year and its grant year (about more than two years on average). To correct for truncation bias, I follow the spirit of Hall, Jaffe, and Trajtenberg (2001, 2005) and adjust the patent data by using the weight factors which are developed from the shape of the application-grant distribution. The truncation-adjusted measures of patents are used in all the tests.

To measure the scope of a firm's patents, I classify the firm's patents into two groups: patents that are related to the firm's core business (related patents) and those that are unrelated to the firm's core business (unrelated patents). Specifically, in each fiscal year, the firm's patent number is classified into two groups, one that is related to the firm's main two-digit SIC code and one that is not related to the firm's main two-digit SIC code. Since USPTO adopts a patent classification system that assigns each patent into a three-digit technology class that is based on input technology category and SIC code is constructed according to the firm's final product category, Hsu, Tian, and Xu (2014) developed a concordance table that connects the USPTO technology class to two-digit SIC code. In that table, each USPTO technology class is mapped to one or multiple two-digit SIC codes. Then, in each fiscal year, I calculate each patent's weight in the firm's two-digit SIC code with its corresponding technology class-two-digit SIC mapping weight, and aggregate these weights to get the total number of related patents. The total number of unrelated patents is the difference between the total number of patents and the total number of related patents.[14]

As pointed out in Lerner and Seru (2015), the traditional citation-based patent quality measures have a more serious truncation bias. Because it is almost impossible for a patent to be cited by another patent until cited patent has been issued. Meanwhile, the patent application

[14] See Hsu, Tian and Xu (2014) for more details.

process takes an average of 32 months.[15] In addition, the citation is not instantaneous after the cited patent's issuance as the citing patents have to go through their own application process. Therefore, recent patents will be mechanically less cited even if their quality does not decline. To better capture the quality and the economic value of patents, I use the data provided by Kogan, Papanikolaou, Seru and Stoffman (2016) to directly measure the market value of patents. These authors use cumulative abnormal returns (CARs) of a firm's stock on its patents grant dates to measure patent market value. Specifically, they compute the market value of each patent as the product of the market value of the patent-filing firm and the three-day market adjusted CARs around the grant date of the patent. In my paper, I sum up the market value of all the patents filed and eventually granted in a given fiscal year and divide it by the total number of patents to capture the average market value of patents.

1.3.2.2 Measuring Dual Ownership

To identify a firm's dual ownership status in any given fiscal year, a lender of the firm with loan outstanding needs to hold a significant amount of equity in the borrower. Following Jiang, Li, and Shao (2010), the significant equity holding is defined as either exceeding 1% of the borrower's outstanding shares or over the value of $2 million in 2009 constant dollars. Therefore, a dummy variable $Dual_{i,t}$ is set to one if at least one lender's average equity holding in the firm i in fiscal year t meets such threshold and set to zero otherwise.

Besides the dummy indicator on whether there is presence of dual-holders, I also construct three other measures to gauge the presence of dual-holders. The second measure, $LnNDual_{i,t}$, is the natural logarithm of one plus the total number of unique holders holding firm i's loan and equity in fiscal year t. This measure captures the extent to which firm i is affected by

[15] http://www.uspto.gov/dashboards/patents/main.dashxml (accessed September, 2016).

dual ownership. The last two measures are based on dual-holders' equity stake. The intuition behind these measures is that conditioning on the firm's loan exposure, a larger equity stake in the firm means more influence from dual-holders. Thus, the third measure, $LnAvgOwn_{i,t}$, is the natural logarithm of one plus the average percentage of firm *i*'s equity held by each lending institution. More specifically, I first calculate firm *i*'s average equity holding by each lender during the four quarters in fiscal year *t* and then average across all the lending institutions. This measure captures the intensity of dual-holders' presence for the average institution. The fourth measure, $LnTotalOwn_{i,t}$, is similarly defined. It is the natural logarithm of one plus the sum of all the dual-holders' average equity holdings in firm *i* in the fiscal year *t*. This measure captures the total power of dual-holders to impact firm innovation activity if the dual-holders share similar objectives.

1.3.2.3 Measuring Control Variables

I control for a vector of firm and industry characteristics that may affect a firm's innovation output. All control variables are computed for firm *i* over its fiscal year *t*. The control variables include firm size (the natural logarithm of total assets), investment in R&D (R&D expenditures over total assets), profitability (return on total assets), leverage (long-term debt over total assets), firm age (the natural logarithm of the number of years listed on Compustat), asset tangibility (net PPE scaled by total assets), capital expenditures over total assets, growth opportunities (Tobin's Q), financial constraints (Hadlock and Pierce, 2010, SA index), acquisition expenditure over total assets, institutional ownership. In addition, I also include a block holder dummy variable if the firm is held by block holders and a loan dummy variable if the firm has loans outstanding. To control for non-linear effects of product market competition on innovation outputs (Aghion, Bloom, Blundell, Griffith, and Howitt, 2005), I also include the sales-based Herfindahl index using 3-digit SIC and the square of Herfindahl index in the regressions. The detailed variable definitions are in the Appendix A.

1.3.3 Summary Statistics

To ensure that outliers do not drive my results, all the continuous variables are winsorized at 1st and 99th percentiles. Table 1.1 reports the summary statistics of all the variables. On average, a firm has 12.2 patents per year and the average market value of a patent is 5.21 million. Dual ownership is present in about 30% of firm-year observations. About 45% of the firm-year observations have syndicated loans outstanding. In terms of other variables, on average, firms have book value assets of 3.68 billion, R&D to asset ratio of 5.9%, ROA of 8.6%, PPE to assets ratio of 54.7%, leverage of 16.5%, capital expenditure ratio of 5.0%, and Tobin's Q of 1.92, and SA index of -3.57.

1.4 BASELINE EMPIRICAL RESULTS

1.4.1 Ordinary Linear Regression Results

To study how the presence of dual ownership affects firm innovation, I run various forms of ordinary least square (OLS) models:

$$InnovationMeasures_{i,t+3} = \alpha + \beta DualMeasure_{i,t} + X_{i,t} + Year_{t} + Firm_{i} + \varepsilon_{i,t}, \quad (1)$$

$X_{i,t}$: firm level covariates

The dependent variables, *InnovationMeasures*, capture innovation quantity, scope, and market value. The natural logarithm of one plus the number of patents filed and eventually granted measures the innovation quantity (*LnNpat*) and the natural logarithm of one plus the number of patents filed and eventually granted that are (un)related to the firm's core businesses measures the innovation scope (*LnNPat(Un)Related*) respectively. The natural logarithm of one plus the average market value of patents filed and eventually granted measures the value of innovation (*LnPatValue*). The dual ownership measure, *DualMeasure*, is one of the four dual holding proxies discussed in the previous section for firm *i* during fiscal year *t*. $X_{i,t}$ is a vector of time-varying firm characteristics that may affect a firm's innovation activities. I also include year fixed effects and firm fixed effects. As innovation is a long term project which usually takes more than one year, the independent variables in OLS models are lagged by three years. The robust standard errors are clustered at the firm level. Existing literature suggests that a number of time-varying firm characteristics can directly impact firm's innovation. Thus a set of controls including *LnAsset*, *R&Dasset*, *ROA*, *Leverage, LnAge*, *PPEAsset*, and *CapexAsset* is added into the OLS model. I also control for Tobin's Q, financial constraints (SA index), and the Herfindahl Hirschman Index as well as its squared term. In addition, in order to control for the effect of institutional investors and the outstanding syndicated loan on innovation, the variable *InstOwn* and the dummy variable *Loan* are added. Furthermore, as large firms often enhance innovation by acquiring small firms (Liu, Sevilir, and Tian, 2012; Bena and Li, 2014), I also control for firms' acquisition expenses.

Table 1.2 provides the baseline OLS results for *InnovationMeasures*. Panel A in Table 1.2 reports the results using *Dual* dummy as the dual ownership measure. As reported in Column (1) of Panel A in Table 1.2, the coefficient estimate of *Dual* on *LnNpat* is negative, i.e., -0.155, and is significant at the 1% level, suggesting that dual ownership has a negative effect on the number of patents firms generate, consistent with *Hypothesis 1* that with the presence of dual-holders, firms engage in fewer risky projects by decreasing their overall innovation output. The Column (2) and (3) of Panel A in Table 1.2 report the results with dependent variables replaced with the number of patents related and unrelated to the firm's core businesses. The coefficient on *LnNpatRelated* is -0.028 and not significant. However, the coefficient on *LnNpatUnRelated* is -0.142, significant at the 1% level, suggesting that the decreased number of patents is mainly driven by those that are unrelated to the firm's core business. As the patents unrelated to the firm's core business are assumed to be less valuable to the firm, these results support *Hypothesis 2* that dual-holders restrain the firm from taking excessive negative NPV risky projects and make the firm focus on a more concentrated innovation scope that is more valuable to the firm. The last column of Panel A in Table 1.2 reports the regression results using the average market value of the patents as the dependent variable. Since this dependent variable is only well defined if a firm produces at least one patent in the corresponding year, I exclude firm-year observations in which the firm does not generate any patent. The coefficient estimate on *LnPatValue* is 0.128, significant at 1% level, suggesting that the dual-holders are positively associated with the average market value of patents. The evidence indicates that dual ownerships positively affect the economic value of the firm's innovation activities, consistent with *Hypothesis 3*.

Panel B in Table 1.2 report the coefficient estimates using alternative measures of dual ownership. The same set of control variables is used in this panel. To save space, I only show the coefficients on the dual ownership, and ignore the coefficients estimated of all the other control variables. The patterns of coefficient estimates on *InnovationMeasures* are nearly the same as these we observe in previous panel, supporting my hypotheses.

Overall, the baseline results suggest that shareholder-creditor conflicts indeed exist and lead firms to engage in risk-shifting, and that institutional dual ownership can partially mitigate this problem.

1.4.2 Robustness of Baseline Results

To check the robustness of the baseline results, I conduct a rich set of robustness tests. First, I show that my results are not sensitive to the regression specifications. I start with a parsimonious model that regresses the *LnNpat* in three years on the key variable of interest, *Dual*, and find a positive raw association between the number of patents and dual ownership. The coefficient estimate is 0.801 (P-value<0.01). Then I add both year and firm fixed effects into the regression. I also include a set of controls that are found to directly affect firm innovation output: firm size, R&D expenditures, profitability, leverage, firm age, asset tangibility, and capital expenditures. The coefficient estimate of *Dual* becomes negative and is significant at 10% level. These results indicate that the omission of these time-invarying and time-varying firm characteristics would bias the coefficient estimate upward. In Table 1.3 Panel A Column (3), I further add Tobin's Q, financial constraints (proxied by SA index), and the Herfindahl-Hirschman Index as well as its squared term, suggested by the recent literature (Hall, Jaffe, and Trajtenberg, 2005; Aghion, Bloom, Blundell, Griffith, and Howitt, 2005). The coefficient estimate continues to be negative and significant at 1% level. These results are consistent with those reported in Table 1.2 Panel A which further controls for institutional ownership, a loan outstanding dummy variable, block holder dummy variable, and acquisition expenses. In sum, my baseline regression results are robust to different model specifications.

Second, I focus on the subsample of firms that have more than three patents during the sample period.[16] The reason to run such tests is that my results may be driven by large number of firm-year observations with zero patents. To save space, I do not report the coefficients for control variables. Table 1.3 Panel B reports the coefficient estimates of *Dual* using the subsample

[16] In an untabulated analysis, I use samples including firms that never generate any patents or generate more than one or two patents during the sample period. The results are quantitatively and qualitatively similar.

of firms with different level of innovation activities.[17] As shown in the table, I continue to observe significantly negative coefficient estimates of the *Dual* on the number of patents and significantly positive coefficient estimates on the market value of patents.

Third, another concern is that innovation is a long term project which usually cannot be completed in a short time and there is no consensus on how many years we should lag on the independent variables. In the main OLS regressions, the independent variables are lagged by three years. For robustness, I run the regressions again using the independent variables lagged by two years. As reported in Table 1.3 Panel C, the coefficient estimates remain to be significantly negative on the number of patents and show significantly positive effect on the market value of patents. In an untabulated analysis, I obtain similar results if using concurrent independent variables and independent variables lagged by one year.

In an unreported analysis, I also use the citation-based measures as an alternative to the market value of patents. The results suggest that although dual ownership does not appear to have a significant effect on the quality of patents on average, further analyses reveal that the presence of dual-holders has a positive association with the average citations per patent that are within the scope of firms' core businesses, indicating that dual-holders induce firms to take on fewer innovative projects but with higher quality, consistent with the findings using market value of patents as the measures.

1.5 IDENTIFICATION

While I have established a negative relationship between the presence of dual-holders and corporate innovation activities from a standard OLS framework, it is difficult to argue that this

[17] In an untabulated analysis, the results are similar if the other three alternative measures of dual ownership are used in the regressions.

correlation is causal because of endogeneity issues. This is the key factor that hinders prior studies to establish a causal claim to link the conflicts of interest between shareholders and creditors with corporate risk-shifting. To overcome this endogeneity problem and establish causality, I implement a DiD estimation strategy based on the quasi-natural experiment of the mergers and acquisitions of financial institutions that generate plausibly exogenous variation in the presence of dual-holders.

1.5.1 Quasi-Natural Experiment

The experiment of financial institution mergers and acquisitions is similar to that of the brokerage mergers used in Hong and Kacperczyk (2010) and was first adopted in Huang (2015a, b). It relies on the fact that financial institutions (e.g. bank holding companies, asset management firms, security brokers, etc.) merge for reasons not related to the fundamentals of firms they hold. As a matter of fact, the mergers and acquisitions between financial institutions are usually driven by the change of policies such as deregulations (He and Huang, 2017). For example, commercial banks were able to acquire existing investment banks as Section-20 subsidiaries of bank holding companies in 1997. In the meantime, the repeal of the Glass-Steagall Act in 1999 blurred the regulatory restriction between commercial banks and other financial entities and resulted in a wave of mergers and acquisitions of financial institutions, combining the asset management arms and credit supplying arms. This is one of the reasons for the boom in the syndication loan market (Jiang, Li, and Shao 2010). Two examples of these mergers include the merger between Citigroup Inc. and Travelers Companies in 1998 and the acquisition between Chase Manhattan Corp. and J.P. Morgan & Co. in 2000.

Furthermore, the mergers and acquisitions between financial institutions are unlikely to be driven by the performance of institutions in their asset management returns or the characteristics of the assets that these institutions hold, but by the strategic business considerations such as economies of scale, economies of scope, and complementarities in geographic presence and distribution networks (Jayaraman, Khorana, and Nelling, 2002). After the merger of two financial institutions, the acquirer will take over the existing assets of the target and usually would not liquid these acquired holdings for an extended period of time as it may be too costly to do so (e.g. Holthausen, Leftwich, and Mayers, 1990; Keim and Madhavan, 1996). Thus, if before the merger, the acquirer (target) holds a loan of a firm and the target (acquirer) has an equity stake in the same firm, then after the merger the acquirer will become a dual-holder of this firm. Due to this exogenous change in the firm's dual ownership status, I investigate how the presence of dual-holders affects a firm's innovation after the financial institutions mergers.

1.5.2 Identifying Treatment and Control Firms

I construct my financial merger and acquisition sample from SDC's Mergers and Acquisitions database following similar procedures to Huang (2015 a, b). I require that: (1) the merger is between two institutions (or their parent firms) in the financial sector (with primary SIC codes in the 6000 to 6999 range) and announced during the period between 1990 and 2006; (2) the merging institutions cannot be controlled by the same parent firm before the merger; (3) the merger is completed within one year after the initial announcement; and (4) the target institution stops filing 13F forms within one year after the completion of the deal.

Figure 1.1 explains how to identify treatment and control firms in this quasi-natural experiment. Institution X (Y) is either an acquirer (target) or a target (acquirer). As shown in the figure, institution X only holds the equities of firm A and B during the last quarter before the merger announcement date while institution Y is the creditor of both firm A and C and has no equity stake in either firm A or C. As discussed before, the merger between institution X and Y is unlikely to be driven by the characteristics of these firms. Then after the merger, the firm A would become dual-held by the merging institution and its total number of dual-holders would increase on average. Therefore firm A is the treatment and firm B and C are the controls as their dual ownership status would not change around the merger.

This DiD framework has several advantages. First, the procedure described above does not depend on any *ex post* information about the actual dual ownership of the firms to identify treatments and controls so that I can largely alleviate the concern that the actual holding/selling decision of the acquiring institution post-merger is caused by the firm's future investment policy or risk-taking behavior. Second, as both treatment and control firms are from the same merger-institution pair, I can control for the potential confounding effects of the merging institutions' skills and investment styles, which might be also correlated with firm characteristics. In addition, I can also mitigate the concern that institutional holdings (either debt or equity holding rather than dual holding) or time-invariant institution effects drive my DiD results.

Finally, both treatment and control firms should have non-missing Compustat records for a symmetric seven-year (three years before the merger announcement and three years after the merger effective date plus the year of merger) window around the merger. The choice of a seven-year window involves a tradeoff between the model relevance and accuracy. Choosing too wide of a window may incorporate the effects of other events that also affect innovation and reduce the sample size as firms need to have non-missing data for a longer time. Furthermore, there is a lag between the firm's innovation activity and its innovation output. A too narrow window is unlikely to capture the influence of dual ownership on innovation. Due to these reasons, I choose a seven-year window. However, my results using a five-year window are broadly similar.

1.5.3 The DiD Estimation

As I do not match treatment and control firms based on observable characteristics, I apply a multivariate DiD method with a full set of interactions between firm fixed effects and the merger-institution pair effects to control for firms' cross-sectional heterogeneity or common time trends that affect both groups of firms. The estimation equations are shown below:

$$InnovationMeasures_{i,j,t+3}=\alpha_1+\beta_{11}*Treat*Post+\beta_{12}*Post+\gamma_1*Control_{i,t}+Firm_i*Merger_j+\varepsilon_{i,t}, \quad (2)$$

where i indexes firm, j indexes merger-institution pair, and t indexes time. *Treat* is dummy variable which equals one for treatment firms and zero for control firms. *Post* is dummy variable which equals one for post-event period and zero for pre-event period. *Control* is a set of control variables used in the baseline OLS regressions. *Firm* indexes firm fixed effects and *Merger* indexes merger-institution pair fixed effects. This estimation specification controls for cross-sectional heterogeneity and common time trends that affect both groups of firms. *Treat* is dropped from the regression as its effect has been fully captured by firm fixed effects.

Table 1.4 Panel A reports the DiD analysis results focusing on a seven-year window. As shown in the Table 1.4 Panel A, both coefficient estimates of *Treat*Post* on *LnNpat* and *LnNpatUnRelated* are negative and statistically significant, suggesting that treatment firms generate fewer innovation output than control firms due to the exogenous change of dual-holder status and this reduction is driven by the number of patents that is unrelated to firm's core business. Again, the coefficient estimate of *Treat*Post* on *LnNpatRelated* is insignificant.

Next, I run another DiD analysis on the average market value of patents, using the same previous sample of the treatment and control firms. Specifically, I compare the average market value of patents between treatment and control firms during the three-year period before and after the institution merger. The last column of Panel A in Table 1.4 reports the DiD result using *LnPatValue* as the dependent variable. Like before, each column includes the control variables used in the baseline OLS regressions. As shown in the Table 1.4, I do observe significantly positive change of the market value of patent between the treatment and control firms. Compared to control firms, the treatment firms' market value of patent is 22.8 % higher, consistent with my conjecture that dual-holders improve the value of innovation output.

The validity of a DiD approach relies on the satisfaction of the key identification assumption, the parallel trend assumption, which states that without the treatment effect, the observed DiD estimator is zero. So I perform a placebo test in which I implement the same estimation framework used above but examine the patent-based measures within the window of year -7 to year -1. In other words, I use year -4 as the pseudo-event year and run the regressions to analyze the patent-based measures of the treatment and control firms during a seven-year window symmetrically around this hypothetical event. Table 1.4 Panel B reports the placebo test results. None of the coefficient estimates of *Treat*Post* is statistically significant at conventional levels, indicating that the trends of innovation activities between these two groups of firms are not observably different before the exogenous shock, showing that the parallel trend assumption is likely to hold in my setting. Besides the tests of the parallel trend assumption, in untabulated analysis, I also check the DiD estimation for the number of dual-holders to verify another premise of the natural experiment, that exogenous shocks to the presence of dual-holders from the mergers should cause an average increase in the treatment firms relative to the control firms. In an unreported analysis, the regression results show a significant relative increase in the number of dual-holders by 11.7% around the mergers, consistent with this conjecture.

1.5.4 Robustness of DiD Results

I conduct various robustness tests for the DiD analysis and report the results in Table 1.5. First, I adopt an alternative multivariate DiD setting which controls for the firm fixed effects and the merger-institution pair fixed effects separately. Under this specification, *Treat* is identified because a given firm can serve as a control in one merger and a treatment in another merger. Panel A of Table 1.5 reports the results for this new setting. As we can see, the coefficient estimates before *Treat*Post* are similar, in terms of both magnitude and significance, to those reported in Table 1.4. Further, in unreported analysis, besides the firm fixed effects and merger-institution pair fixed effects, I also control for industry fixed effects and obtain broadly similar results.

Second, instead of using the year-by-year variation in innovation outcomes for both treatment and control firms, I aggregate the information in the pre- and post-merger periods and run similar DiD regressions. Specifically, I calculate the three-year average innovation measures for each firm either in the pre- event or the post-event period. The control variables are measured at year -4 for the pre-event period and at year 0 for the post-event period to mitigate simultaneity concern. Then I conduct the similar DiD regression framework as in Equation (2) and report the results in Table 1.5 Panel B. As shown in the table, the main DiD results continue to hold under this alternative definition of variables.

In untabulated analysis, in order to make my DiD setting as clean as possible, I keep my sample of financial institution mergers that occurred during the late 1990s since these mergers are more likely to be driven by the banking-industry deregulations (the Gramm-Leach-Bliley Act). The estimate coefficients of *Treat*Post* in the DiD tests are still very similar to those reported in main DiD results.

Overall, the DiD analyses suggest that an exogenous increase in a firm's dual ownership would lead to less corporate risk-shifting and more concentrated innovation scope and higher value of innovation output.

1.5.5 Cross-sectional Heterogeneity of the Effect of Dual Ownership on Innovation

In this section, I explore how cross-sectional heterogeneity in shareholder-creditor conflicts and how different types of dual ownership affect the relation between dual-holders and corporate innovation to provide further evidence that the prior results are driven by reduced conflicts of interest between shareholders and creditors.

1.5.5.1 Cross-Sectional Heterogeneity in Conflicts of Interest

Conflicts of interest between shareholders and debtholders usually become more severe when a firm is close to financial distress (Smith and Warner 1979; Gilson and Vetsuypens 1993; Eisdorfer, 2008). Therefore, the effect of dual-holders on corporate innovation should be more pronounced for firms that are more financially distressed. To test this conjecture, I construct two DiD subsamples based on the firm's Altman's Z-score, a widely used model of bankruptcy prediction. First I calculate each firm's Z-score immediately before the merger announcement. Following Altman (1968), firms with Z-scores smaller than 1.81 are classified as financially distressed and those with Z-scores above 2.99 are considered as not financially distressed. Then I re-estimate the DiD regressions using these two subsamples. The results are presented in Panel A1 and A2 in Table 1.6. Consistent with the conjecture that the conflicts of interest between shareholders and creditors are more severe when the firm is in financial distress and the effect of dual-holders on innovation would be stronger, only the results using the subsample of financially distressed firms demonstrate similar patterns with these shown in section 1.5.3. The effect of dual-holders is statistically insignificant in non-financially distressed firms.[18]

1.5.5.2 Cross-Sectional Heterogeneity in Types of Dual Ownership

As documented in Jiang, Li, and Shao (2010), commercial banks (CBs) and non-commercial banks (non-CBs) have different incentives to monitor firms' corporate operations due to the nature of their equity holdings. Specifically, CBs' equity holdings are more likely to be a result of their trust accounts. CBs bear fiduciary duties for their clients and will only monitor and vote on behalf of their clients. Therefore, CBs are unable to share the profits associated with the equity ownership (Santos and Wilson, 2009). On the contrary, non-CBs actively manage their equity holdings for profits because voting rights and cash flow rights attached to equity shares are combined together. As a result, non-CBs should have more motivation to actively monitor the firms invested than CBs do. Thus, if dual ownership helps align interests between shareholders and creditors, its effect should be stronger for firms with non-CB lenders. To test this conjecture, I divide the DiD sample into two groups based on if the firm's loan facility involves participation of non-CB dual-holders. If a firm's loan facility has at least one non-CB dual-holders, then it is grouped into non-CB sample; otherwise, it is categorized into CB sample. The results presented

[18] In untabulated analysis, I perform DiD tests based on Zmijewski (1984) probit model for predicting bankruptcy and obtain similar results.

in Panel B1 and B2, Table 1.6 show that the effect of dual-holders on corporate innovation concentrates in firms with non-CB dual-holders. The coefficient estimate is not statistically significant in the sample consisted of only CB dual-holders. Taken together, the results are consistent with the conjecture.

Overall, the results suggest that the effect of dual-holders on corporate innovation is more pronounced when the firm is in financial distress and when the firm's loan facility involves participation of non-CB dual-holders.

1.6 VALUE IMPLICATION OF DUAL OWNERSHIP

So far I have established a negative relationship between the presence of dual ownership and the quantity of corporate innovation output and a positive relationship between the dual ownership and the value of corporate innovation output. However, since the presence of dual-holder mitigates the shareholder-creditor conflicts through its impact on corporate innovations, how is this reflected on the firm value as a whole? A further natural question to ask is why lenders hold their borrowers' equity simultaneously? Herein, I will discuss the value implication of dual ownership.

From the perspective of portfolio diversification, all else equal, investors prefer to hold equity in a firm with which they do not have loans outstanding. Holding substantial equity and loan stakes in the same firm increases investors' exposure to firm-specific risk which could be alleviated if investors choose to invest in the equity of a comparable firm which has no lending relationship with them. The major cost to dual-holders is underdiversification. Therefore, an additional source of gains must be derived from the dual ownership to offset the loss of diversification.

The potential source of gains comes from the alignment of interests between shareholders and creditors. Shareholders have an incentive to expropriate creditors due to the different cash flow claims to which shareholders and creditors are entitled. Creditors who are wary of wealth expropriation would incorporate this incentive concern into the pricing of lending. This is the so-called agency cost of debt, which is eventually born by the shareholders. The presence of dual ownership may alleviate the conflicts of interest between shareholders and creditors and work as a device to mitigate creditors' concern about expropriation, lowering the borrowing cost for the firm. This argument is consistent with Jiang, Li, and Shao (2010)'s finding that the required return from the loans offered by dual-holders is lower than that for similar non-dual-holder loans. As the financing cost of firms with dual ownership is lower, firms' value should increase. Meanwhile, my paper argues that the presence of dual-holders could also prevent managers from taking opportunistic behavior arising from the conflicts of interest that only benefits the shareholders but not the creditors or the whole firm's value. Dual-holders may induce firms to reduce the overly risky value-destroying projects to increase firm value. Due to these two reasons, the presence of dual ownership should increase the whole firm's value so as to offset the negative effect of underdiversification.[19]

To examine the value implication of dual ownership on a firm's value, I use Tobin's Q to measure the firm value and apply the same DiD framework used before. The DiD result is shown in Table 1.7. The DiD coefficient estimate for Tobin's Q is 0.089 and significant at the 10% level, suggesting that treatment firms have a higher increase in value compared to control firms due to the exogenous increase of dual ownership.

[19] Another prediction is that the firm's asset volatility should decrease with the presence of dual-holders as managers are less likely to involving in risk shifting behavior. In untabulated analysis, I do find results consistent with this prediction.

1.7 POSSIBLE UNDERLYING MECHANISMS

My results so far suggest that the presence of dual ownership has a negative effect on a firm's risk-taking. In this section, I discuss possible underlying economic mechanisms through which this impact occurs.

1.7.1 Managers' Equity-based Compensation

First, an exogenous increase in dual ownership may affect managers' equity-based compensation which is used to induce managerial risk-taking behavior. Existing literature has shown that institutional investors can influence executive compensation. For example, Hartzell and Starks (2003) document that institutional investors can affect the pay-for-performance sensitivity of executive compensation and the level of compensation. Chhaochharia and Grinstein (2009) also find that outside blockholders on the board can impact CEO compensation decisions. Almazan, Hartzell, and Starks (2005) further argue that institutional investors have advantages in monitoring firms' management in terms of executive compensation. Meanwhile, there exists a wealth of empirical evidence on the positive effect of the sensitivity of manager's wealth to equity risk on manager's incentive to invest in risky projects. For example, Guay (1999) finds that a firm's stock-return volatility is positively related to the convexity of the manager's full compensation package (including previously granted stock and options), which is known as "*vega*". Coles, Daniel, and Naveen (2006) extend the findings of Guay (1999) to investigate whether *vega*, (i.e. the convexity in manager's compensation) has a causal effect on firm risk-taking behavior by using pooled time series cross-sectional regressions and simultaneous equations. Their results suggest that *vega* is positively related to R&D expenditures, leverage, and firm risk. Low (2009) draws a similar conclusion by using a law change in Delaware that increases takeover protections as an exogenous shock. Chang, Fu, Low, and Zhang (2015) document that even non-employee stock options' *vega* has a positive impact on the firm's innovation through enhanced risk-taking incentives. Therefore, if institutional investors can exert their influence on executive compensation and the sensitivity of managerial wealth to stock volatility can encourage risk-taking, then we would expect that the *vega* of manager's equity-based compensation will decrease after an exogenous increase in dual ownership.

To test this conjecture, I follow the existing literature (e.g., Guay 1999; Coles, Daniel, and Naveen, 2006) and calculate the sensitivity of manager's annual compensation to changes in stock return volatility in two ways: *Vega* is the value of the manager's current and all prior equity-based compensation for a 1% change in the stock price volatility and *vegaflow* is the value of the manager's current annual equity-based compensation for a 1% change in the stock price volatility. In the regressions, I use the natural logarithm of one plus *vega* and *vegaflow* to deal with outliers and right skewness of these measures. Details of the variable construction are provided in the Appendix A. Both measures of *vega* and *vegaflow* are stated in thousands of dollars and are winsorized at the 1st and 99th percentile. The number of observations in this sample is slightly less than that used in the main DiD regressions because some firms lack the information to calculate *vega* and *vegaflow*.

I perform a DiD analysis on managers' *LnVega* and *LnVegaFlow* in the same setting of financial institution mergers. Specifically, I compare managers' average *LnVega* and *LnVega-Flow* during the three-year period before and after the institution merger for both treatment and control firms based on the sample used in the previous section. Table 1.8 presents the DiD results using *LnVega* and *LnVegaFlow* as the dependent variables. Each column includes the full interaction of merger-institution fixed effects and firm fixed effects as before. I conduct a multivariate analysis by including common control variables used in the previous literature such as firm size, R&D expenditure, firm leverage, operating performance (*ROA*), capital expenditure, cash compensation, CEO's tenure, and so on (e.g. Low, 2009). As shown in Table 1.8, the estimated coefficients for *Treat*Post* are -0.154 and -0.221 for *LnVega* and *LnVegaFlow*, respectively. Both estimators are significant at the 10% level. The DiD results suggest that the *vega* of managers' total equity-based compensation portfolio experiences 15.4% more decrease for the treatment firms comparing to that of the control firms. At the same time, the *vega* of managers' current equity-based compensation also experiences 22.1% more decrease for the treatment group compared to that of the control group. Thus, the DiD results suggest that a lower sensitivity of CEO wealth to stock volatility might be a plausible underlying economic mechanism through which dual-holders affect corporate risk-taking.

1.7.2 Corporate Governance

While dual-holders may lower the sensitivity of CEO's wealth to stock volatility to curb managers' risk-taking behavior, corporate governance might also serve as an alternative mechanism through which dual-holders reduce risk-shifting. In this subsection, I explore this possible economic channel.

Previous literature has suggested that the presence of dual-holders might positively affect a firm's corporate governance. There is ample empirical evidence that institutional investors improve corporate governance. For example, Brav, Jiang, Partnoy, and Thomas (2008) find that hedge fund activists improve corporate governance mechanisms by enhancing board independence. Moreover, Appel, Gormley, and Keim (2016) show that even passive institutions may exert influence on firms' governance choices, resulting in more independent directors, removal of takeover defenses, and more equal voting rights. Furthermore, Dass and Massa (2011) document that a bank could improve a firm's corporate governance if the bank has a strong lending relationship with the firm. Thus, when a financial institution simultaneously holds a firm's debt and equity, it might have more power in shaping the firm's corporate governance structure.

If this is the case and if improved corporate governance could also reduce risk-shifting by decreasing the number of patents produced and increasing the value and quality of an average patent, then corporate governance may be an underlying mechanism through which dual ownership affects asset substitution. However, there has been no consensus on the impact of corporate governance on patenting activities, and the existing literature offers two competing perspectives. The first one argues that better governance reduces managerial entrenchment, which is actually bad for both the quantity and quality of corporate innovation. Unlike routine tasks such as mass production and marketing, innovation involves a long-term process that is full of risk and requires tolerance for failure (Tian and Wang, 2014). However, better corporate governance means more pressure from shareholders to achieve short-term success on the public equity market and less tolerance for failure (Stein, 1988). Therefore, managers in better governed firms may not be able to focus on innovation unless they are insulated from such short-term pressure. Consistent with this unique feature of innovation, Chemmanur and Tian (2015) find that firms with a larger number of antitakeover provisions, which make managers more entrenched, increase both the quantity and quality of patents. Therefore, the documented negative effect of dual-holders on the number of generated patents in my paper could be caused by the fact that dual-holders actively improve corporate governance through their enhanced power and incentives, making managers less entrenched. However, this entrenchment-based governance effect cannot explain my finding that dual-holders improve (rather than reduce) the quality of generated patents (as proxied by the market value and industry-relatedness of the patents).

An alternative view is based on moral hazard and argues that better corporate governance should lead to more innovation output. Managers who are not properly monitored tend to shirk and enjoy "quiet life" (Bertrand and Mullainathan 2003), which means that they would refrain from long-term innovative projects that are risky and difficult. Consistent with this argument, Atanassov (2013) shows a significant decline in the number of patents and citations per patent for firms incorporated in the states that pass antitakeover laws compared to those incorporated in the states that do not. Thus, the documented negative effect of dual-holders on the value of generated patents could also be partly caused by the fact that dual-holders positively affect corporate governance and better discipline the managers. However, this moral-hazard-based governance effect cannot explain my finding that dual-holders decrease (rather than increase) the number (quantity) of generated patents.

Overall, based on the evidence from patenting activities documented by this paper corporate governance is unlikely to be an underlying mechanism through which dual ownership affects risk shifting.

1.8 CONCLUSION

This paper examines the causal effect of shareholder-creditor conflicts on corporate risk-shifting. Specifically, I investigate how the simultaneous holdings of a firm's debt and equity by the same institutional investors affect corporate innovation activities. The baseline analyses show that the presence of dual ownership leads to fewer patents generated by the firm and this pattern is mainly driven by patents that are unrelated to the firm's core businesses. Moreover, dual-holders improve the average market value of patents. Using financial institutions' mergers and acquisitions as a quasi-natural experiment, I further establish a causal effect of dual ownership on corporate risk-shifting. I also find that the effect of dual-holders on corporate innovation is more pronounced when a firm is in financial distress and when the firm's loan facilities involve the participation of non-CB dual-holders. Furthermore, I document that dual ownership increases firm value proxied by Tobin's Q because dual-holders can curb excessive risk-taking. Lastly, I show that one possible mechanism through which dual-holders affect corporate innovation is to lower the sensitivity of managerial compensations to stock price volatility. I also discuss that corporate governance is unlikely to be a channel through which dual ownership affects risk-shifting. Overall, the paper provides empirical evidence that shareholder-creditor conflicts indeed lead to risk-shifting and that institutional dual ownership can partially mitigate this problem and prompt managers to reduce excessive risk-taking to increase firm value.

Institution X	Institution Y
Shareholder of Firm A	Creditor of Firm A
Shareholder of Firm B	Creditor of Firm C

Figure 1.1 Illustration of the quasi-natural experiment of financial institution mergers
The empirical methodology is to compare the innovation outcomes of firm A to those of firm B and firm C. Institution X (Y) is either the acquirer (target) or the target (acquirer). Before the merger, institution X only holds firm A's and B's equity and institution Y only holds firm A's and C's debt. After the merger, the firm A would become dual-held by the merging institution and its total number of dual-holders would increase on average. Therefore firm A is the treatment and firm B and C are the controls as their dual ownership status would not change around the merger.

Table 1.1 Descriptive statistics

This table reports summary statistics based on the sample U.S. listed firms from 1987 to 2009. Definitions of variables are in Appendix A.

Variable	P25	P50	P75	Mean	S.D.	N
NPat	0.000	0.000	4.000	12.177	43.621	21,656
NPatRelated	0.000	0.000	0.633	3.152	12.242	21,656
NPatUnRelated	0.000	0.000	2.878	8.609	30.467	21,656
PatValue	0.612	2.335	8.559	10.514	23.148	10,729
Dual	0.000	0.000	1.000	0.299	0.458	21,656
NDual	0.000	0.000	2.000	4.185	11.972	21,656
AvgOwn	0.000	0.000	0.003	0.004	0.0423	21,656
TotalOwn	0.000	0.000	0.013	0.032	0.187	21,656
Loan	0.000	0.000	1.000	0.451	0.498	21,656
Asset	0.072	0.310	1.500	3.675	22.124	21,656
Age	15.000	23.000	36.000	26.503	13.416	21,656
R&DAsset	0.000	0.021	0.074	0.059	0.127	21,656
ROA	0.060	0.120	0.174	0.086	0.236	21,656
PPEAsset	0.280	0.471	0.736	0.547	0.368	21,656
Leverage	0.006	0.124	0.256	0.165	0.198	21,656
CapexAsset	0.020	0.037	0.064	0.050	0.045	21,656
SAindex	-4.637	-3.575	-3.054	-3.565	0.754	21,656
TobinQ	1.094	1.442	2.102	1.923	2.001	21,656
Hindex	0.048	0.066	0.108	0.113	0.133	21,656

Table 1.2 Baseline regressions of innovation outcomes on dual ownership

This table reports OLS regressions of the innovation outcome variables on dual ownership and other control variables. Panel A reports baseline OLS results using *Dual* as the measure for dual ownership. Panel B reports baseline results using alternative measures for dual ownership. Definitions of variables are in Appendix A. Each regression includes a separate intercept. The estimations correct for error heteroskedasticity and within-firm error clustering. T-statistics are displayed in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Panel A. Baseline OLS regressions

Dep. Var.	$LnNPat_{t+3}$	$LnNPatRelated_{t+3}$	$LnNPatUnRelated_{t+3}$	$LnPatValue_{t+3}$
	(1)	(2)	(3)	(4)
$Dual_t$	-0.155***	-0.028	-0.142***	0.128***
	(-3.568)	(-1.185)	(-3.445)	(3.622)
$LnAsset_t$	0.170***	0.031	0.154***	0.554***
	(3.526)	(0.899)	(3.360)	(19.150)
$R\&DAsset_t$	0.420**	0.473***	0.280	1.109***
	(2.181)	(2.810)	(1.606)	(5.379)
ROA_t	0.062	0.027	0.037	0.485***
	(0.840)	(0.486)	(0.549)	(4.993)
$Leverage_t$	-0.203***	-0.184***	-0.141**	-0.163*
	(-2.784)	(-3.505)	(-2.073)	(-1.860)
$LnAge_t$	0.372***	0.180***	0.371***	0.319***
	(3.676)	(2.743)	(3.941)	(6.549)
$PPEAsset_t$	0.030	0.044	0.011	-0.026
	(0.477)	(1.081)	(0.192)	(-0.514)
$CapexAsset_t$	0.011	-0.058	-0.018	0.140
	(0.061)	(-0.543)	(-0.110)	(0.525)
$TobinQ_t$	0.035***	0.022***	0.028***	0.105***
	(5.061)	(4.193)	(4.651)	(12.649)
$SAindex_t$	-0.013	-0.217**	0.020	0.592***
	(-0.106)	(-2.465)	(0.179)	(7.761)
$Hindex_t$	-0.617	-0.266	-0.859	1.045
	(-1.473)	(-0.735)	(-1.459)	(1.609)
$Hindex_t^2$	0.547	0.079	1.361	-1.260*
	(1.130)	(0.146)	(1.495)	(-1.647)
$InstOwn_t$	-0.152*	-0.061	-0.138*	0.452***
	(-1.942)	(-1.166)	(-1.909)	(5.383)
$Loan_t$	-0.044**	-0.018	-0.039**	-0.017
	(-2.285)	(-1.308)	(-2.261)	(-0.624)
$Block_t$	0.003	-0.034**	0.013	-0.456***
	(0.129)	(-2.269)	(0.647)	(-16.779)
$Acquisition_t$	-0.071	0.031	-0.094	-0.054
	(-0.758)	(0.899)	(-1.047)	(-0.388)
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Observations	21,656	21,656	21,656	10,729
R-squared	0.854	0.841	0.841	0.716

Panel B. Baseline regressions using alternative dual ownership measures

Dep. Var.	$LnNPat_{t+3}$	$LnNPatRelated_{t+3}$	$LnNPatUnRelated_{t+3}$	$LnPatValue_{t+3}$
	(1)	(2)	(3)	(4)
$LnNDual_t$	-0.004*	-0.015	-0.062**	0.004*
	(-1.866)	(-0.876)	(-2.566)	(1.846)
$LnAvgOwn_t$	-6.915***	-1.036	-8.380***	1.494
	(-3.386)	(-0.955)	(-3.939)	(0.615)
$LnTotalOwn_t$	-0.767**	-0.306	-1.061***	0.612*
	(-2.385)	(-1.193)	(-2.795)	(1.816)
Controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Observations	21,656	21,656	21,656	10,729

Table 1.3 Robustness OLS regressions of innovation outcomes on dual ownership

This table reports robustness OLS regressions of the innovation outcome variables on dual ownership and other control variables. Panel A reports results using the same baseline sample (as in Table 1.2) but different model specifications. Panel B reports results using subsamples of firms that generated more than three patents over the entire sample period. Panel C reports results using the same baseline model (as in Table 1.2) with innovation outcome variables measured at year *t+2*. Definitions of variables are in Appendix A. Each regression includes a separate intercept. The estimations correct for error heteroskedasticity and within-firm error clustering. T-statistics are displayed in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Panel A: Regressions of the number of patents with different model specifications

Dep. Var.	$LnNPat_{t+3}$		
	(1)	(2)	(3)
$Dual_t$	0.801***	-0.087*	-0.201***
	(10.143)	(-1.926)	(-4.395)
$LnAsset_t$		0.148***	0.161***
		(5.353)	(3.367)
$R\&DAsset_t$		0.598***	0.408**
		(3.152)	(2.127)
ROA_t		0.139*	0.058
		(1.947)	(0.795)
$Leverage_t$		-0.227***	-0.208***
		(-3.123)	(-2.860)
$LnAge_t$		0.390***	0.383***
		(3.814)	(3.784)
$PPEAsset_t$		0.019	0.038
		(0.302)	(0.595)
$CapexAsset_t$		0.089	-0.007
		(0.490)	(-0.037)
$TobinQ_t$			0.033***
			(4.881)
$SAindex_t$			0.009
			(0.076)
$Hindex_t$			-0.628
			(-1.496)
$Hindex_t^2$			0.569
			(1.169)
Year FE	No	Yes	Yes
Firm FE	No	Yes	Yes
Observations	21,656	21,656	21,656
R-squared	0.066	0.853	0.854

Panel B. Regressions using firms that generate at least three patents over the sample period

Dep. Var.	$LnNPat_{t+3}$	$LnNPatRelated_{t+3}$	$LnNPatUnRelated_{t+3}$	$LnPatValue_{t+3}$
	(1)	(2)	(3)	(4)
$Dual_t$	-0.151***	-0.034	-0.152***	0.133***
	(-3.075)	(-1.166)	(-3.158)	(3.641)
Controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Observations	16,804	16,804	16,804	10,167
R-squared	0.843	0.842	0.835	0.714

Panel C. Regressions using independent variables lagged by two years

Dep. Var.	$LnNPat_{t+2}$	$LnNPatRelated_{t+2}$	$LnNPatUnRelated_{t+2}$	$LnPatValue_{t+2}$
	(1)	(2)	(3)	(4)
$Dual_t$	-0.159***	-0.010	-0.147***	0.104***
	(-3.807)	(-0.452)	(-3.720)	(3.251)
Controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Observations	21,656	21,656	21,656	10,729
R-squared	0.855	0.841	0.840	0.733

Table 1.4 Quasi-natural experiment of financial institution mergers: Innovation Outcomes

This table reports difference-in-differences (DiD) regression results on how exogenous shocks to the presence of dual ownership (due to financial institution mergers) affect innovation outcome variables. Panel A reports baseline DiD results using year 0 as the event year. Panel B reports results using year -4 as the pseudo-event year. Treatment firms are those whose equity are significantly held by one party to the merger during the last quarter before the merger announcement date and also have loans outstanding with the other party of the merger. Control firms are those which either have their equity or loans held by one of the merging institutions but do not belong to the treatment group. Both groups are also required to have non-missing Compustat records for a symmetric seven-year window around the merger year. The regressions use a panel of seven years (year -3 to year +3, including year 0) for both treatment and control firms. Definitions of variables are in Appendix A. Each regression includes a separate intercept and firm-merger pair fixed effects. The estimations correct for error heteroskedasticity and within merger-firm pair error clustering. T-statistics are displayed in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Panel A. [-3, 3] window using year 0 as the event year

Dep. Var.	$LnNPat_{t+3}$	$LnNPatRelated_{t+3}$	$LnNPatUnRelated_{t+3}$	$LnPatValue_{t+3}$
	(1)	(2)	(3)	(4)
*Treat*Post*	-0.126**	0.003	-0.115**	0.228***
	(-2.349)	(0.053)	(-2.032)	(2.756)
Post	-0.118***	-0.127***	-0.110***	0.287***
	(-9.404)	(-9.122)	(-8.375)	(13.192)
$LnAsset_t$	0.215***	0.145***	0.171***	0.169***
	(9.024)	(7.336)	(7.497)	(4.342)
$R\&DAsset_t$	-0.079	1.224***	-0.461*	2.589***
	(-0.281)	(4.436)	(-1.731)	(7.201)
ROA_t	0.584***	0.773***	0.484***	1.329***
	(6.115)	(9.096)	(5.020)	(8.587)
$Leverage_t$	0.046	-0.084*	0.180***	0.020
	(0.873)	(-1.831)	(3.370)	(0.188)
$LnAge_t$	0.862***	0.313***	0.888***	1.193***
	(9.671)	(4.554)	(10.198)	(7.691)
$PPEAsset_t$	0.119**	0.200***	0.216***	-0.013
	(2.260)	(4.461)	(4.150)	(-0.139)
$CapexAsset_t$	-0.116	-0.150	-0.201	0.287
	(-0.932)	(-1.552)	(-1.570)	(0.978)
$TobinQ_t$	0.052***	0.034***	0.061***	0.016**
	(8.880)	(6.301)	(10.790)	(2.032)
$SAindex_t$	0.128	-0.089	0.075	0.179
	(1.614)	(-1.150)	(0.936)	(1.235)
$Hindex_t$	0.831***	-1.125***	-0.594***	1.035***
	(3.407)	(-8.800)	(-3.843)	(2.715)
$Hindex_t^{\,2}$	-0.112*	1.069***	0.602***	0.366***
	(-1.864)	(7.646)	(3.577)	(3.952)
$InstOwn_t$	-0.230***	0.020	-0.284***	-0.339***
	(-4.137)	(0.398)	(-5.147)	(-3.372)
$Loan_t$	-0.112***	-0.052***	-0.112***	0.095***
	(-9.623)	(-4.443)	(-9.154)	(4.616)
$Block_t$	0.015	-0.042***	-0.112***	-0.117***
	(1.097)	(-3.528)	(-9.154)	(-5.053)
$Acquisition_t$	0.091	0.135***	0.096	-0.437***
	(1.330)	(2.585)	(1.441)	(-3.409)
Firm*Merger FE	Yes	Yes	Yes	Yes
Observations	54,769	54,769	54,769	36,193
R-squared	0.949	0.937	0.941	0.752

Panel B. Pseudo-event analysis using year -4 as the event year

Dep. Var.	$LnNPat_{t+3}$	$LnNPatRelated_{t+3}$	$LnNPatUnRelated_{t+3}$	$LnPatValue_{t+3}$
	(1)	(2)	(3)	(4)
*Treat*Post*	0.058	0.047	0.034	-0.090
	(0.964)	(0.885)	(0.502)	(-1.265)
Post	-0.069***	-0.061***	-0.066***	0.095***
	(-5.444)	(-4.818)	(-4.915)	(4.707)
Controls	Yes	Yes	Yes	Yes
Firm*Merger FE	Yes	Yes	Yes	Yes
Observations	38,930	38,930	38,930	25,439
R-squared	0.960	0.955	0.951	0.780

Table 1.5 Robustness DiD regressions of innovation outcomes on presence of dual ownership

This table reports robustness difference-in-differences (DiD) regressions of the innovation outcome variables on presence of dual ownership and other control variables. Panel A reports results using the same sample as in Table 1.4 Panel A but controls for firm and merger fixed effects separately. Panel B uses the same baseline DiD sample and regression framework as in Table 1.4 and examine the average innovation outcome variables for treatments and controls (i.e., three years before and three years after the merger). Control variables are measured at year -4 for the pre-merger period and at year 0 for the post-merger period. Treatment firms (*Treat* equals to one) are those whose equity are significantly held by one party to the merger during the last quarter before the merger announcement date and also have loans outstanding with the other party of the merger. Control firms (*Treat* equals to zero) are those which either have their equity or loans held by one of the merging institutions but do not belong to the treatment group. Both groups are also required to have non-missing Compustat records for a symmetric seven-year window around the merger year. Definitions of variables are in Appendix A. Each regression includes a separate intercept. T-statistics are displayed in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Panel A. DiD analysis that controls for firm and merger fixed effects separately

Dep. Var.	$LnNPat_{t+3}$	$LnNPatRelated_{t+3}$	$LnNPatUnRelated_{t+3}$	$LnPatValue_{t+3}$
	(1)	(2)	(3)	(4)
*Treat*Post*	-0.125***	0.016	-0.116**	0.223***
	(-2.602)	(0.371)	(-2.266)	(3.180)
Treat	0.077**	0.008	0.054	-0.143***
	(2.347)	(0.267)	(1.599)	(-3.437)
Post	-0.142***	-0.191***	-0.135***	0.310***
	(-11.982)	(-14.723)	(-10.939)	(17.397)
Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Merger FE	Yes	Yes	Yes	Yes
Observations	54,769	54,769	54,769	36,193
R-squared	0.934	0.910	0.924	0.703

Panel B. Using average innovation measures over the three-year period before or after the event

Dep. Var.	$AvgLnNPat_{t+3}$	$AvgLnNPatRelated_{t+3}$	$AvgLnNPatUnRelated_{t+3}$	$AvgLnPatValue_{t+3}$
	(1)	(2)	(3)	(4)
*Treat*Post*	-0.151**	0.011	-0.158**	0.302*
	(-2.174)	(0.165)	(-2.273)	(1.771)
Post	-0.220***	-0.213***	-0.204***	0.388***
	(-7.810)	(-7.897)	(-7.386)	(7.462)
Controls	Yes	Yes	Yes	Yes
Firm*Merger FE	Yes	Yes	Yes	Yes
Observations	16,656	16,656	16,656	12,634
R-squared	0.972	0.964	0.969	0.897

Table 1.6 Quasi-natural experiment of financial institution mergers: Cross-sectional Tests

This table reports difference-in-differences (DiD) regression results on how exogenous shocks to the presence of dual ownership (due to financial institution mergers) affect innovation outcome variables in terms of heterogeneity in conflicts of interest (Panel A1 and A2) and types of dual ownership (Panel B1 and B2). The Altman's Z-score, a financial distress measure, is used to proxy for the degree of the conflicts of interest. Panel A1 (A2) reports results using firms that are less (more) financially distressed. A firm with Z score higher (lower) than 2.99 (1.81) is considered as less (more) financially distressed. In Panel B1 and B2, the regressions are split by the firm's types of dual ownership (at least one non-commercial bank (CB) participation vs. CB participation only). Non-CB dual-holders have more incentives than CB dual-holders to mitigate conflicts of interest between shareholders and creditors. Treatment firms are those whose equity are held by the acquirer (target) during the last quarter before the merger announcement date and also have loan outstanding with the target (acquirer). Control firms are those whose equity are held by the acquirer (target) but do not have loan outstanding with the target (acquirer) or those whose equity are not held by the acquirer (target) but do have loan outstanding with the target (acquirer). Both groups should have non-missing Compustat records for a symmetric seven-year window around the merger year. Definitions of variables are in Appendix A. Each regression includes a separate intercept and firm-merger pair fixed effects. The estimations correct for error heteroskedasticity and within merger-firm pair error clustering. T-statistics are displayed in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Panel A1. Less financially distressed

Dep. Var.	$LnNPat_{t+3}$	$LnNPatRelated_{t+3}$	$LnNPatUnRelated_{t+3}$	$LnPatValue_{t+3}$
	(1)	(2)	(3)	(4)
*Treat*Post*	-0.114	0.060	-0.147	0.170
	(-1.270)	(0.855)	(-1.636)	(1.602)
Post	-0.082***	-0.121***	-0.073***	0.248***
	(-4.963)	(-6.205)	(-4.424)	(9.979)
Controls	Yes	Yes	Yes	Yes
Firm*Merger FE	Yes	Yes	Yes	Yes
Observations	36,193	36,193	36,193	23,116
R-squared	0.946	0.930	0.938	0.773

Panel A2. More financially distressed

Dep. Var.	$LnNPat_{t+3}$	$LnNPatRelated_{t+3}$	$LnNPatUnRelated_{t+3}$	$LnPatValue_{t+3}$
	(1)	(2)	(3)	(4)
*Treat*Post*	-0.159***	-0.040	-0.102*	0.368*
	(-3.006)	(-1.066)	(-1.686)	(1.923)
Post	-0.069**	-0.022	-0.104***	0.047
	(-2.551)	(-0.794)	(-2.783)	(0.460)
Controls	Yes	Yes	Yes	Yes
Firm*Merger FE	Yes	Yes	Yes	Yes
Observations	8,239	8,239	8,239	3,265
R-squared	0.946	0.947	0.941	0.684

Panel B1. Dual-holders with more incentives to mitigate conflicts

Dep. Var.	$LnNPat_{t+3}$	$LnNPatRelated_{t+3}$	$LnNPatUnRelated_{t+3}$	$LnPatValue_{t+3}$
	(1)	(2)	(3)	(4)
*Treat*Post*	-0.217**	0.012	-0.230**	0.595***
	(-2.104)	(0.229)	(-2.239)	(2.902)
Post	-0.107***	-0.102***	-0.108***	0.251***
	(-6.131)	(-4.981)	(-6.379)	(7.394)
Controls	Yes	Yes	Yes	Yes
Firm*Merger FE	Yes	Yes	Yes	Yes
Observations	31,830	31,830	31,830	20,760
R-squared	0.956	0.942	0.951	0.806

Panel B2. Dual-holders with fewer incentives to mitigate conflicts

Dep. Var.	$LnNPat_{t+3}$	$LnNPatRelated_{t+3}$	$LnNPatUnRelated_{t+3}$	$LnPatValue_{t+3}$
	(1)	(2)	(3)	(4)
*Treat*Post*	0.007	0.086	-0.019	-0.022
	(0.100)	(1.009)	(-0.273)	(-0.240)
Post	-0.227***	-0.202***	-0.218***	0.422***
	(-9.897)	(-9.748)	(-9.689)	(11.537)
Controls	Yes	Yes	Yes	Yes
Firm*Merger FE	Yes	Yes	Yes	Yes
Observations	22,939	22,939	22,939	15,433
R-squared	0.963	0.959	0.957	0.797

Table 1.7 Quasi-natural experiment of financial institution mergers: Tobin's Q

This table reports difference-in-differences (DiD) regression results on how exogenous shocks to the presence of dual ownership (due to financial institution mergers) affect a firm's Tobin's Q. Treatment firms are those whose equity are held by the acquirer (target) during the last quarter before the merger announcement date and also have loan outstanding with the target (acquirer). Control firms are those whose equity are held by the acquirer (target) but do not have loan outstanding with the target (acquirer) or those whose equity are not held by the acquirer (target) but do have loan outstanding with the target (acquirer). Both groups should have non-missing Compustat records for a symmetric seven-year window around the merger year. Definitions of variables are in Appendix A. Each regression includes a separate intercept and firm-merger pair fixed effects. The estimations correct for error heteroskedasticity and within merger-firm pair error clustering. T-statistics are displayed in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Dep. Var.	*Tobin's* Q_t
*Treat*Post*	0.089*
	(1.865)
Post	-0.142***
	(-8.593)
$LnAsset_t$	-0.101***
	(-3.812)
$R\&DAsset_t$	3.415***
	(5.972)
ROA_t	6.797***
	(36.447)
$Leverage_t$	-0.608***
	(-7.037)
$LnAge_t$	0.646***
	(3.475)
$PPEAsset_t$	-0.282***
	(-4.497)
$CapexAsset_t$	-0.290
	(-1.330)
$SAindex_t$	0.645***
	(4.972)
$Hindex_t$	0.055
	(1.008)
$Hindex_t^{\ 2}$	0.289
	(1.250)
$InstOwn_t$	0.623***
	(8.053)
$Loan_t$	0.038***
	(2.803)
$Block_t$	0.052***
	(3.699)
$Acquisition_t$	-0.334***
	(-4.440)
Firm*Merger FE	Yes
Observations	54,769
R-squared	0.806

Table 1.8 Quasi-natural experiment of financial institution mergers: Managers' Vega and Vegaflow
This table reports difference-in-differences (DiD) regression results on how exogenous shocks to the presence of dual ownership (due to financial institution mergers) affect sensitivity of managers' compensation to the stock volatility (*Vega* and *VegaFlow*). Treatment firms are those whose equity are held by the acquirer (target) during the last quarter before the merger announcement date and also have loan outstanding with the target (acquirer). Control firms are those whose equity are held by the acquirer (target) but do not have loan outstanding with the target (acquirer) or those whose equity are not held by the acquirer (target) but do have loan outstanding with the target (acquirer). Both groups should have non-missing Compustat records for a symmetric seven-year window around the merger year. Definitions of variables are in Appendix A. Each regression includes a separate intercept and firm-merger pair fixed effects. The estimations correct for error heteroskedasticity and within merger-firm pair error clustering. T-statistics are displayed in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Dep. Var.	$LnVega_t$	$LnVegaFlow_t$
	(1)	(2)
*Treat*Post*	-0.154*	-0.221*
	(-1.739)	(-1.703)
Post	0.629***	0.364***
	(43.686)	(17.827)
$LnAsset_t$	0.661***	0.651***
	(25.485)	(20.239)
$R\&DAsset_t$	1.807***	3.922***
	(5.639)	(7.693)
ROA_t	0.183	1.855***
	(1.294)	(9.625)
$Leverage_t$	-0.309***	-0.640***
	(-3.133)	(-4.805)
$CapexAsset_t$	0.544**	0.852***
	(2.026)	(2.596)
$CashCompensation_t$	0.000***	0.000***
	(12.173)	(3.784)
$Tenure_t$	-0.012***	-0.035***
	(-5.616)	(-16.492)
$Hindex_t$	-0.200	0.822**
	(-0.869)	(2.486)
$Hindex_t^2$	0.315	-0.329
	(1.217)	(-0.885)
Firm*Merger FE	Yes	Yes
Observations	50,492	50,492
R-squared	0.814	0.538

CHAPTER 2

PAYOUT POLICY UNDER ENHANCED LABOR POWER[20]

[20] Yang, Huan, Jie He, and Xuan Tian. To be submitted to *Management Science*.

Abstract

We study labor power as an important but largely under-explored determinant of corporate payout policy. Using a regression discontinuity approach that exploits locally exogenous variation in labor's collective bargaining power, we find that an increase in labor power lowers corporate payout. Operating flexibility is a plausible underlying mechanism through which labor power influences corporate payout. Firms use the saved earnings from reductions in payout to invest in net working capital rather than paying off debt or increasing cash holdings. Our paper sheds new light on the determinants of payout policy and the role of labor power in corporate finance decisions.

2.1 INTRODUCTION

What determines a firm's payout policy? Since Miller and Modigliani (1961), financial economists have proposed a number of economic factors that determine a firm's payout policy including corporate taxes, signaling motives, agency considerations, compensation practices, managerial incentives, behavioral biases, etc. However, very few studies have analyzed the role played by a firm's employees, one of the most important groups of stakeholders that both contribute to and get affected by firm operations, in shaping its payout decisions. This is surprising, given the large body of literature examining the relation between labor (as well as human capital) and other corporate policies such as capital structure (e.g., Titman, 1984; Titman and Wessels, 1988; Chemmanur et al., 2013, etc.), mergers and acquisitions (John et al., 2015), corporate governance (Atanassov and Kim, 2009), and technological innovation (Acharya et al., 2013, 2014). In this paper, we aim to examine whether and how labor power, a previously underexplored economic factor, affects corporate payout policy.

As a claimant to firms' resources, employees generally compete with shareholders in extracting economic rents created by the business and thus prefer to retain cash flows within the firms as opposed to paying them out.[21] Understanding this motive, shareholders and the management would design payout policy to maximize their own benefits.[22] On the one hand, managers would want to keep a low payout ratio to cater to the preference of their employees so as to minimize the disruptive effect that an unhappy workforce might have on firms' operations. On the other hand, they would want to set a high payout ratio to avoid leaving resources behind

[21] An anecdote provides a good example in support of this argument: GM announced a dividend cut in February 2006 due to its persistent poor performance in the past few years since 2003. One important underlying force for the dividend cut was to get more concessions from the United Auto Workers, the union that represents GM's employees, because "everyone needs to share the pain, including shareholders." (Dow Jones News Service, February 7, 2006)

[22] Note that employees are not only competing with shareholders for corporate resources but also cooperating with shareholders in generating profits and firm value (i.e., the "size of the pie"). However, even if employees understand the consequences of payout policy for firm profitability, to the extent that they care more about their own benefits, they would still prefer a lower (as opposed to a higher) payout than what the shareholders would optimally choose.

for labor's rent extraction. The optimal payout policy therefore reflects the above tradeoffs and crucially depends on how labor influences firms' operations and decision making. Since unions represent a prominent form of organized labor, we study the influence of labor power on corporate payouts by using union elections as an empirical setting that helps identify exogenous variation in labor's collective bargaining power.

We develop two competing hypotheses regarding the effect of labor power on a firm's payout policy. Our first hypothesis conjectures that labor power reduces a firm's payout ratios. To reduce cash flow risk and make their operations more flexible, firms need to increase the variable component of their cost structure, which entails linking their labor usage and expenses (salaries and other employee compensation) to sales revenue.[23] However, organized labor, such as unions, has a reputation for making wages sticky and layoffs costly, thereby increasing the adjustment cost of a firm's labor stock. Furthermore, unions frequently intervene in a firm's restructuring activities, such as blocking plant closures to save workers' jobs, which makes it harder for the firm to adjust its physical capital. As a result, enhanced labor power will increase a firm's cash flow risk and reduce its operating flexibility. In response to the threat from stronger labor power, firms may decrease their payout ratios (i.e., payouts relative to income) to accumulate precautionary cash to hedge against cash flow risk and to save for profitable investment opportunities. In addition, labor unions often bargain with their employers for higher and more stable wages and better working conditions, putting an additional financial constraint on firm managers when they make corporate decisions.[24] As a result, an increase in labor power

[23] Consistent with this idea, Giroud and Mueller (2015) find that high leverage firms would decrease their work force more than low-leverage firms in the face of household demand shocks. By the same token, Kovenock and Phillips (1997) find that firms with greater debt are more likely to close plants, and Hanka (1998) shows that higher debt is associated with more frequent employment reductions, lower wages, and reduced pension funding.

[24] Using a RD design and the U.S. Census data, Frandsen (2012) finds that the passage of unionization elections in an establishment has a positive, causal effect on the wages of its employees, especially those of lower-income employees.

might tighten up a firm's financial situation, prompting it to cut down payouts to shareholders so as to free up some internal retained earnings for future investment use. Hence, the first hypothesis we propose, the *flexibility hypothesis*, argues that firms would cut their payout ratios after labor power increases.

An alternative hypothesis makes the opposite prediction. Firms can take a variety of strategic actions to improve their bargaining position against their workers. For example, DeAngelo and DeAngelo (1991) argue that, to gain concessions from labor unions, managers must create a credible perception that the firm's competitiveness is threatened by current economic conditions. Consistent with this argument, they show that unionized firms manage their earnings downward to create this perception prior to labor negotiations. Moreover, other studies such as Bronars and Deere (1991), Dasgupta and Sengupta (1993), Hanka (1998), and Matsa (2010) find that issuing more debt helps firms to improve their bargaining position against workers, supporting the idea that by committing themselves to paying out a large portion of future cash flows to lenders, firms can effectively reduce their cash flows available for labor expropriation.[25] Following the similar logic discussed above, since dividends enable firms to shield their cash flows from labor extraction, firms would prefer to commit to a high payout in response to enhanced labor power. Therefore, our second hypothesis, the *bargaining hypothesis*, argues that firms would increase their payout ratios after labor power increases.[26]

While there might be an element of truth in both hypotheses, in practice it is difficult to identify the causal effect of labor power on payout policy because of the endogeneity problem.

[25] In addition, Monacelli et al. (2011) and Quadrini and Sun (2014) show that the enhanced bargaining position arising from high leverage increase a firm's incentive to hire more workers, and Klasa et al. (2009) show that firms in more unionized industries strategically hold less cash to gain more bargaining power.

[26] Note that an alternative interpretation of the bargaining hypothesis may actually predict the opposite, i.e., a negative effect of labor power on corporate payout: in order to bargain well and get more concessions from organized labor, firm managers may voluntarily reduce payout to cater to the preferences of employees/unions. However, the additional analysis reported in Section 6 (to be discussed later in the paper) fails to find support for this alternative argument.

The existence of strong organized labor in a firm could be correlated with its unobservable characteristics that affect the payout policy (the omitted variable concern). Alternatively, workers in a firm with reduced permanent cash flows and hence a lower expected payout ratio may be more likely to form unions to protect their own interests (the reverse causality concern). Both problems make it difficult to draw causal inferences from a standard ordinary least squares (OLS) regression that regresses a firm's payout ratio on its labor power.

To overcome the endogeneity problem and establish causality, we rely on union elections that substantially alter labor power. We collect data on union election results from the National Labor Relations Board (NLRB), which allows us to compare the payouts for firms that elect to become unionized to those that vote against it. Our main identification strategy is to use a regression discontinuity (RD) design that relies on "locally" exogenous variation in union status generated by these elections that pass or fail by a small margin of votes. This approach compares firms' payouts subsequent to union elections that barely pass with those subsequent to elections that barely fail. The RD design is a powerful and appealing identification strategy because for these close-call elections, passing is very close to an independent, random event and therefore is unlikely to be correlated with firm unobservable characteristics.

We first perform a variety of diagnostic tests to ensure that the key identifying assumption of the RD design, namely, there is no precise manipulation of votes by either workers or firms around the known threshold (50%) for a winning union election, is not violated. We also confirm that firms barely passing union elections and those barely failing to pass exhibit similar pre-election characteristics that could affect a firm's payout policy.

We then show that labor power appears to have a causal, negative effect on a firm's payout ratios. According to our nonparametric local linear regression estimation, passing a union election leads to a 10.1% lower dividend payout ratio and a 19.3% lower total payout ratio (including both dividends and share repurchases) compared to failing an election in the following year. This result is robust to alternative choices of kernels and bandwidths, alternative measures of corporate payout (such as dividend per share, dividend yield, dividend over sales, dividend over cash flows, and dividend over assets), and is absent at artificially chosen thresholds that determine union election outcomes.

We next show that operating flexibility is a possible underlying channel through which enhanced labor power negatively affects firms' payout ratios. Since organized labor increases a firm's cash flow risk and reduces the flexibility of its operations, we expect a stronger negative effect of union election passages on payout policy for firms with an *ex ante* lower level of operating flexibility. This is because such firms would face a larger threat from the winning elections and thus will pay out less to be better able to save for profitable investment opportunities in the future. We find evidence consistent with this conjecture. To lend further support for operating flexibility as a possible channel, we find that unionization has a significantly negative effect on the announcement returns only for firms with inflexible operations but not for others.

Another possible mechanism through which organized labor negatively affects firms' payout policy is financial flexibility. Labor unions often bargain with their employers for a higher and more stable wage level and better working conditions, putting an additional layer of financial constraint on firm management when they make investment decisions. Hence, if a firm is already financially constrained, the passage of union elections makes its financial situation even tighter, prompting it to cut down payouts so as to free up some internal retained earnings for future investment use. This argument suggests a stronger negative effect of passing union elections on payout ratios for firms with *ex ante* more stringent financial constraints. However, after examining a comprehensive set of existing measures of financial constraints, we find mixed evidence regarding this hypothesis, which suggests that either financial flexibility is not an important channel through which labor power affects payout policy or the existing financial constraint measures have their own limitations (as argued by recent studies such as Farre-Mensa and Ljungqvist, 2015).

Our results so far suggest that firms reduce corporate payout and retain more internally generated earnings to cope with the upcoming operating inflexibility under more enhanced labor power. A natural question is where the saved earnings (net income) end up going. Do firms keep these additional retained earnings in the form of liquid assets (e.g., cash and cash-equivalent marketable securities, net working capital, etc.) or use them to pay off debt so that they can increase their borrowing capacity? To answer this question, we analyze a firm's other corporate policies and find that firms use the saved earnings to invest in net working capital rather than paying off debt or increasing cash holdings.

Finally, we examine two plausible alternative explanations for the negative effect of labor power on payout. One alternative explanation is labor's innate distaste for dividends and its greater influence on managerial decision making after workers coordinate among themselves more efficiently through unionization. In other words, the lower payout ratio in unionized firms, though suboptimal from the shareholders' perspective, is preferred by the employees and thus managers cater to the preferences of labor to decrease payouts. If this alternative explanation is true, we should observe that the negative effect of organized labor on payout is less pronounced if there is a better alignment of interests between employees and shareholders. However, we do not find evidence supporting this argument. Another alternative explanation is managers' perverse incentives to keep corporate resources within the firm to enjoy private benefits. In the presence of such agency conflicts, managers would use the passage of union elections as an excuse to convince shareholders to agree with cutting payouts. If this explanation is true, we would expect that the negative effect of organized labor on payout ratios is more pronounced in firms with worse corporate governance. We, however, actually find the opposite, which suggests that our results are more likely to be driven by firms' optimal response to enhanced labor power than by agency conflicts.

Our paper mainly contributes to the literature by identifying labor power as an important yet underexplored determinant for corporate payout. Although a few previous studies (to be discussed in detail in Section 2) also explore the implications of labor unions for payout policy, they come to a different conclusion than we do because these studies either rely on small-sample evidence from selected industries or time periods, or document an association by focusing on industry-level union power. In contrast, our paper examines all publicly listed firms that experience union elections over the entire 32-year period between 1980 and 2011, and uses the RD design as the main identification strategy to establish a causal link between labor power and corporate payout policy. Therefore, we offer a cleaner and more comprehensive perspective on this important research topic. Moreover, we make an attempt to identify plausible underlying economic mechanisms through which unions affect payout decisions. In the meantime, our RD analysis on cash, debt, and working capital provides new evidence on the causal relation between labor power and other important corporate financial policies.

The rest of our paper proceeds as follows. Section 2 discusses related literature. Section 3 describes the data and presents summary statistics. Section 4 provides our main results and robustness checks. Section 5 investigates underlying economic mechanisms. Section 6 discusses alternative explanations for our main findings. Section 7 concludes.

2.2 RELATION TO THE EXISTING LITERATURE

Our paper is related to the large literature on payout policy, starting from Bhattacharya (1979), John and Williams (1985), and Miller and Rock (1985), which identifies a variety of major factors influencing a firm's payout policy including corporate taxes, signaling motives, agency considerations, compensation practices, and management incentives (See DeAngelo et al. (2008) and Farre-Mensa et al. (2014) for excellent surveys).[27] Our results suggest that operating flexibility shaped by organized labor could be another important determinant of payout policy, consistent with the findings of Brav et al. (2005) that nowadays financial executives do not consider signaling, clientele effects, catering, or agency as explanations for payout levels but still believe that the perceived stability of future earnings affects dividend policy.

Our paper extends the few existing studies on the effect of labor power on payout policy. DeAngelo and DeAngelo (1991), using a sample of 7 major US steel firms in the 1980s, show that these firms reduce work force, manipulate earnings downward, cut management pay, and reduce dividends during union negotiations. They conclude that firms make these changes in order to gain concession from the union leaders out of bargaining concerns. Although our finding of a negative effect of unionization on payout policy is consistent with their clinical evidence, the underlying channels in the two studies are quite different: while they rely on a bargaining story, we find that operating flexibility rather than bargaining motives drives our results. Moreover, we differ from their study in three crucial dimensions. First, and perhaps most importantly, we use the RD design as our identification strategy that allows us to establish a causal link between

[27] John et al. (2011) show that a firm's geographic location, besides all other well documented determinants, also affects its payout policy.

unionization and payout policy. Second, instead of studying a few major firms in one industry, our sample covers all publicly listed firms that experience a union election between 1980 and 2011 and it spans across a wide range of industries. Third, we make an attempt to pinpoint possible underlying economic mechanisms through which labor power affects corporate payout policy.[28]

Two other closely related studies also examine the implications of labor power for payout policy but come to a different conclusion from ours. Matsa (2006) finds an *insignificantly* negative relation between union bargaining power and dividend policy. However, different from our sample that covers 32 years, his sample covers three cross-sectional snapshots in 1977, 1987, and 1999. Chino (2016) finds that firms in industries with stronger labor unions tend to have higher payouts when they are highly profitable but commit to lower payouts when they are not profitable. Different from the above papers, we use a RD approach to address the endogeneity problem in union power and to establish causality, and find strong evidence that *firm-level* union power significantly reduces corporate payout.[29]

Finally, our paper adds to the emerging literature on labor and finance, which explores how employee preferences and labor power affect firm policies such as capital structure (Berk et al., 2010; Bae et al., 2011; Chemmanur et al., 2013; Simintzi et al., 2014), mergers and acquisitions (John et al., 2015), corporate financing decisions (Agrawal and Matsa, 2013), corporate governance (Atanassov and Kim, 2009), and technological innovation (Acharya et al.,

[28] Our paper also makes a direct contribution to a relatively small literature on the causes and consequences of dividend reductions. Kalay and Loewenstein (1986) find that late announcements of dividends are disproportionately associated with dividend reductions. Michaely et al. (1995) study the price reactions to dividend omissions. Benartzi et al. (1997) show an improvement in operating performance after dividend cuts. Chemmanur and Tian (2012, 2014) find that a significant proportion of dividend cutting firms prepare the market by releasing some information before dividend cuts. Our paper documents an additional factor that forces firms to cut dividends.
[29] In contrast to Chino (2016), who only exploits industry-level variation in unionization, our RD approach using firm-level union election data finds no evidence that highly profitable firms (i.e., firms whose ROA right before union elections is above the sample median or above the 65 percentile of its distribution, as defined in Chino (2016)) commit to higher payouts under stronger union power. These untabulated results are available upon request.

2013, 2014; Bradley et al. 2016).

2.3 DATA AND DESCRIPTIVE STATISTICS

Our data come from several sources. Union election data, including the closing date of an election, the number of eligible participants/voters, and the outcome of the election, are collected from the National Labor Relations Board (NLRB). We study union elections held between 1980 and 2011, and drop those either with a missing voting outcome or with fewer than 50 eligible participating employees.[30] Then we manually match our union election sample to Compustat by company name and address so that we can extract relevant financial statement information and other firm characteristics from Compustat. Since our study aims to analyze corporate payout policy within a three-year period following a labor union election, we need to minimize the confounding effect of other recent elections held by the same firm. To this end, we require a firm conducting a union election to be included in our sample only if it has no other elections during the past three years. In case there are multiple elections held by the same firm within one year, we retain the one with the largest number of eligible voters because this election is likely to matter most for corporate decision making.[31] Our sampling procedure results in 1,234 unique union elections.

Our main payout policy variables are *Dividend Payout* (the dividend-to-earnings ratio), which is the total annual cash dividends distributed by a firm divided by its net income over the same fiscal year, and *Total Payout* (the total-payout-to-earnings ratio), which is the sum of total annual cash dividends and stock repurchases divided by net income. We follow Grullon and

[30] We focus on elections with at least 50 eligible participating employees because union elections with a smaller size may only have a negligible impact on corporate decisions. In addition, elections with fewer participants may also be subject to precise manipulation of votes, which violates the crucial identifying assumption of the RD design. This type of filter is commonly adopted by the labor union election literature (e.g., Lee and Mas, 2012).

[31] Another way to deal with multiple elections held by the same firm is to adopt a dynamic RD model as in Cuñat, Gine, and Guadalupe (2012). We explore this alternative empirical model in the Appendix C Table A.2.

Michaely (2002) to measure the amount of stock repurchases. Specifically, we define repurchases as the total expenditures on the purchase of common and preferred stocks minus any reduction in the redemption value of the net preferred stocks outstanding. Following recent literature such as Chay and Suh (2009), we drop firm-year observations with negative earnings but with non-negative cash dividends or total payouts because it is difficult to interpret such payout ratios.[32] To mitigate the concern for outliers (especially due to extremely small earnings), we winsorize payout ratios at the 5% level. We also winsorize all the rest of the firm characteristics, described in Appendix B, at the 1% level.[13]

Panel A of Table 2.1 reports summary statistics for our union election sample. On average 42.8% of votes are in favor of unionization, with a standard deviation of 20.9%. Out of these 1,234 elections between 1980 and 2011, unions win 28.7% of them. Panel B of Table 2.1 presents summary statistics of the payout variables as well as other firm characteristics that are likely to affect a firm's payout decisions, including firm size (*Assets* and *Market Value*), profitability (*ROA*), asset tangibility (*PPE/Assets*), investment (*Capx/Assets*), leverage (*Debt/Assets*), cash holdings (*Cash/Assets*), Tobin's Q (*TobinQ*), firm age (*Age*), cash flow volatility (*Cashflow Volatility*), institutional ownership (*Institutional Ownership*), sales growth (*Sales Growth*), and GDP growth of the state where the firm's headquarters is located (*State GDP Growth*). We summarize all variables over the predetermined year (the year before the reported closing date of the union elections) and the three years following the union elections. The average dividend payout ratio is 24.0% with a standard deviation of 30.2%, and the average total payout ratio is 44.9% with a standard deviation of 54.7%.

Figure 2.1 plots the trend of union election frequencies and passage rates in our sample

[32] Our results are qualitatively similar if we drop all firm-years with negative earnings, regardless of payouts.

period. There is a considerable decline in the number of union elections over time, which is consistent with recent literature on union membership rate (see, e.g., Visser, 2006). The second graph in Figure 2.1 describes the passage rates (frequencies) for union elections held in each year. Despite the wide variation in passage rates across time, the majority of union elections fail to pass except for year 2008. The average passage rate of 28.7% in our sample is consistent with recent studies, such as Lee and Mas (2012), who find an average passage rate of 29.9% in their sample.[33]

As a starting point, we check the association between payout ratios and unionization status in our sample by adopting an OLS regression framework. Table 2.2 presents the results. The dependent variables are *Dividend Payout* and *Total Payout* N year(s) after the union elections (N=1, 2, or 3). The variable of interest is a unionization dummy that equals one if a union election wins and zero otherwise. Panel A presents the univariate OLS results without any controls. Panel B presents the results of multivariate OLS analysis that controls for the covariates mentioned above at the predetermined year. As we can see, there exists a negative (though only marginally significant) association between union elections and subsequent payout ratios in univariate OLS analysis but this association becomes insignificant if we control for other firm characteristics and year/industry fixed effects.[34]

[33] To further mitigate the concern that local RD estimation is sensitive to outliers, we winsorize our dependent variables at the 10% level in all of our local RD analysis. The results using 5% winsorization are very similar.

[34] In untabulated OLS analysis, we find that the association between union elections and subsequent payout ratios becomes statistically insignificant if we control for firm fixed effects.

2.4 RD APPROACH AND MAIN RESULTS

2.4.1 Empirical strategy and diagnostic tests

We estimate the causal effect of labor power on payout policy by adopting a RD design, which assigns a firm's unionization status to our sample firms based on a simple majority (50%) passing rule. The RD design exploits a unique feature of the union election data—we observe the percentage of votes for unionization in every union election.

The intuition behind our RD strategy is as follows: elections that pass (leading to unionization and hence enhanced labor power in the firm) or fail (not leading to new unions inside the firm) within a narrow bandwidth around the 50% threshold should follow the pattern of a quasi-randomized experiment. Essentially, this empirical approach compares payout policy of firms that barely pass union elections to that of firms that barely fail to pass elections. It is a powerful and appealing identification strategy for our purpose because for a close-call union election, unionization is "locally" exogenous in the sense that it is unlikely to be systematically correlated with any unobservable characteristics that could affect a firm's payout policy. In other words, the assignment of a treatment effect (i.e. unionization status) to the group of our sample firms is likely to be random, which helps us to identify the causal effect of labor power on corporate payout policy. This feature of the RD approach means that our empirical test design is not prone to the usual endogeneity problems or sample selection issues. Another advantage of the RD design is that we do not have to include observable covariates (i.e., control variables) in our analysis (as in a standard multiple regression framework) because firms falling in a narrow band around the threshold ought to be similar in all dimensions of characteristics. Hence, firm covariates are unnecessary for identification (see earlier survey papers on the RD approach such as Imbens and Lemieux (2008) and Lee and Lemieux (2010) for a more detailed discussion on this less stringent requirement).

The success of a RD strategy hinges on the satisfaction of the key assumption of imperfect control by any agent, which requires that agents (workers and firms) in an election cannot *precisely* manipulate the forcing variable (i.e., the share of favorable votes) near the known cutoff.[35] The implication is that the distribution of the forcing variable should not have any jumps around the discontinuity point (i.e., the 50% threshold). To check the validity of this assumption, we perform two diagnostic tests.

First, Figure 2.2 plots a histogram of the sample distribution of vote shares (i.e., the percentage of votes in an election in favor of unionization) across 50 equally-spaced bins (with a bin width of 2%). As we can observe, the vote share distribution is continuous within close proximity of the cutoff point (the 50% threshold), which indicates no sign of precise manipulation by workers or firms.

Second, we perform a formal statistical test, developed by McCrary (2008), for discontinuity in the density of the vote shares, which is depicted in Figure 2.3. The dots represent the density estimates and the bold line is the fitted density function of the forcing variable (union vote shares) surrounded by the 95% confidence interval. As one can observe, the density of vote shares appears smooth and its fitted curves show little indication of strong discontinuity near the 50% threshold. The Z-statistic for the McCrary test of discontinuity is 1.384 (the coefficient of estimate is 0.299 with a standard error of 0.216), which is statistically insignificant. Thus we are unable to reject the null hypothesis that the density function at the cutoff is continuous, suggesting that no agents have precisely manipulated the votes around the known threshold to

[35] Note that this assumption does not require the absence of vote manipulation in the elections. In reality, both firms and unions run campaigns to push for success, which leads to some degrees of the manipulation of shares. However, as long as agents do not have *precise* control over the forcing variable (even though some manipulation exists), an exogenous discontinuity still allows for random assignment to the treatment (see, e.g., Lee, 2008).

achieve their desired unionization status. Our finding of no precise manipulation around the known cutoff is consistent with the previous literature using the same election data from the NLRB (e.g., DiNardo and Lee, 2004).

Another important assumption of the RD design is that there is no discontinuity in firm characteristics other than the unionization status across the known cutoff point. In other words, firms close to the left and the right of the cutoff point (i.e., those with vote shares slightly above or below the 50% threshold) should be similar in terms of observable, predetermined characteristics that might affect the outcome (payout policy) and/or the assignment variable (vote shares). If there are any significant “jumps” in the distribution of these firm characteristics near the 50% threshold, then the treatment effect we observe using the RD design could be biased.[36]

Hence, we perform a diagnostic test for this assumption by a visual check and report the figures in the Appendix C Figure A.1. The format of these figures is similar to that of Figure 2.4 below (see a detailed discussion in Section 4.2) with variables on the y-axis replaced by firm characteristics (summarized in Panel B of Table 2.1). We do not observe discontinuity in the distribution of any of these variables around the 50% threshold.

In summary, the above two sets of tests show that the key identifying assumptions of the RD design, namely, agents’ imprecise manipulation and the absence of discontinuity in other firm characteristics, are not violated, supporting our test premise that the variation in unionization status is as good as that from a randomized experiment (Lee, 2008).

[36] Note, however, that this assumption is much less restrictive than textbook assumptions regarding endogeneity (such as the exclusion restrictions) in that it does not require those predetermined characteristics to be exogenous: as long as they are determined prior to the assignment variable (the voting share) and continuously distributed around the cutoff point (i.e. with no jumps), then the RD procedure will still yield valid and consistent estimates. See Lee and Lemieux (2010) for a more detailed discussion of this assumption and related tests.

2.4.2 Main RD results

In this subsection, we report the main RD results. We consider both short-term and long-term effects of enhanced labor power. Hence, we examine payout policy one, two, and three years subsequent to the union elections. Before we explore the effect of labor power on payout policy in a rigorous regression framework, we first check the relation between passing a union election and payout ratios visually. Figure 2.4 demonstrates the RD results graphically in an intuitive way. Figures in the left column plot *Dividend Payout* and those in the right column describe *Total Payout*. The x-axis is the forcing variable, vote share, which is the percentage of votes in favor of unionization. To the left of the 50% cutoff point, firms fail to unionize after the labor union elections; to the right of the cutoff, firms succeed in becoming unionized. As in previous figures, the spectrum of vote share is divided into 50 equally-spaced bins (with a bin width of 2%).[37] The dots in the graphs represent the average payout ratio in each bin, and the solid curve is the result of a fitted quadratic polynomial (with a 95% confidence interval). From Figure 2.4, we observe discontinuity in both payout variables across the cutoff in each of the three years post the union election: we observe a significant drop in payout when moving the vote share from the left to the right of the 50% threshold. These patterns suggest a negative effect of unionization on corporate payouts.

Next, we adopt a global polynomial model with parametric estimation to implement the RD approach (e.g., Cuñat, Gine, and Guadalupe, 2012) using all observations in our sample. The global polynomial model estimates the following specification:

$$PayoutRatio_{t+N} = \alpha + \beta Union_t + P_l(v,c) + P_r(v, c) + \varepsilon_t \qquad (1)$$

where t indexes time (i.e., year of the election) and N = 1, 2, or 3. $Pl\ (v,\ c)$ is a flexible polynomial function for observations on the left-hand side of the threshold c with different

[37] Alternative choice of bin widths does not change our results qualitatively.

orders; *Pr (v, c)* is a flexible polynomial function for observations on the right-hand side of the threshold *c* with different polynomial orders; *v* is the vote share of an election (percentage of votes in favor of unionization). Since union elections win with a simple majority of support among the voters, *c* equals 50% in our setting. *PayoutRatio* is either *Dividend Payout* or *Total Payout*, and *Union* is a dummy to indicate unionization, which equals one if the vote share exceeds 50%, and zero otherwise. In this regression, *β* measures the difference in the slopes of these smoothed functions (*Pl (v, c)* and *Pr (v, c)*) at the cutoff point, capturing the causal effect of passing a union election on firm payout N (*N*=1, 2, or 3) years down the road. However, since the RD estimates are essentially weighted average treatment effects where the weights are the ex-ante probabilities that the vote shares fall in the neighborhood of the win region (Lee and Lemieux, 2010), this coefficient should be interpreted locally within close vicinity of the 50% cutoff.

We present the results estimating Equation (1) in Table 2.3. Panel A performs the estimation with polynomials of order three. As one can observe, the coefficient estimates on *Union* are all negative and statistically significant at the 1% or 5% level in most years, except for *Total Payout* in year 3, suggesting a negative effect of labor power on corporate payout policy. In terms of economic magnitudes, the estimates for year 1 (N=1) suggest that firms passing union elections will have a dividend payout ratio 18.1% lower and a total payout ratio 23.2% lower than those without successful union elections one year after the elections. Panels B and C estimate Equation (1) by using polynomials of order two and four, respectively, and find qualitatively similar results.[38] Even though firm covariates are unnecessary for identification in

[38] In an untabulated analysis, we repeat the analysis using polynomials of other orders such as one or five, and find similar results. For example, if we use polynomials of order five, the RD estimate for *Dividend Payout* one year after the union election is -0.173 (with a t-stat of 1.65) and that for two years after the union election is -0.282 (with a t-stat of 3.59).

the RD framework (Imbens and Lemieux, 2008; Lee and Lemieux, 2010), we check the robustness of our main findings in Panel D by using polynomials of order three and controlling for a comprehensive set of other firm characteristics at the predetermined year and industry and year fixed effects. We continue to observe a significantly negative effect of unionization on payout ratios. Comparing the RD results in Table 2.3 to the OLS results in Table 2.2, we observe that the OLS estimates are severely biased upward due to endogeneity issues, justifying the use of a cleaner identification strategy (in our case the RD methodology) to analyze the causal impact of labor power on firm payouts.

The results from a global polynomial model point to a negative effect of labor power on payout policy. However, one potential concern with the above methodology is that it uses all union elections in the sample, even those with voting shares far away from the cutoff point, although they are weighted much less than the elections with voting shares close to the cutoff point during estimation. To provide more convincing evidence on the causal effect of labor power on payout policy, we implement a nonparametric local linear estimation in the vicinity of the 50% threshold using the optimal bandwidth suggested by Imbens and Kalyanaraman (2012) that minimizes the mean squared error in a sharp RD setting. Compared to the global polynomial method, the local linear estimation model has better local fitness (Bakke and Whited, 2012), more attractive rate optimality, and superior bias properties (Fan and Gijbeles, 1992, and Hahn, Todd, and van der Klaauw, 2001).

Panel A of Table 2.4 presents the local linear estimation results using the triangular kernel, because the statistics literature has shown that a triangular kernel is optimal for estimating local linear regressions at the boundary due to its more weight on observations closer to the cutoff point (Fan and Gijbeles, 1992). Consistent with the results from the global polynomial estimation, the coefficient estimates on *Union* are all negative and statistically significant at the 1% or 5% level in most years after the union elections. Specifically, firms with winning union elections have a cash dividend payout ratio 10.1% lower and a total payout ratio 19.3% lower than those that fail to pass union elections one year afterwards. To check the robustness of our findings, we adopt a rectangular kernel in local linear estimation in Panel B. We obtain qualitatively similar results.[39] Overall, the evidence presented in this subsection suggests a negative, causal effect of labor power on corporate payout, consistent with the *flexibility hypothesis*.

As argued before, an important assumption of the RD design is that there is no discontinuity in firm characteristics other than the unionization status across the known cutoff point. Besides the visual inspection reported in the Appendix C Figure A.1, we perform a formal diagnostic test for this assumption by running local linear regressions on various firm characteristics (summarized in Panel B of Table 2.1) at the predetermined year (i.e., the year before the reported closing date of the union elections).

Table 2.4 Panel C reports local linear regression results for firm characteristics that have been shown to affect corporate payout in the literature. None of the local linear RD estimates for these firm characteristics are statistically significant, suggesting that there is no discontinuity in the distribution of these variables around the known threshold. Most importantly, the predetermined values of our key dependent variables, *Dividend Payout* and *Total Payout*, do not show discontinuity around the narrow bandwidth of the cutoff point, suggesting that our main

[39] In an untabulated analysis, we find that the negative effect of labor power on payout ratios is stronger when the elections take place in the same city as a firm's headquarters or when a firm has fewer geographic segments (as reported by the business segment data in Compustat), consistent with the notion that our results are stronger when establishment-level union elections matter more for corporate central decision making.

RD results are unlikely to be driven by *ex-ante* differences in payout policy between firms passing union elections and those whose elections fail.

2.4.3 Robustness checks

In this section, we report a comprehensive set of robustness checks that examines the sensitivity of our RD results to various model assumptions. First, we check whether our local linear regression estimates are sensitive to the choice of bandwidths, which reflects a classical tradeoff between bias and precision. On the one hand, a wider bandwidth makes use of more observations within the local neighborhood of the cutoff and thus yields more precise estimates. On the other hand, wide bandwidths could introduce more noise and bias into the estimation because it has used more "non-local" observations away from the cutoff and made linear approximation inaccurate. The converse is true for narrower bandwidths.

Hence, to address the concern that our results in Table 2.4 are driven by the bandwidth we have chosen, we plot the estimated local RD coefficients along with their 95% confidence intervals (on the vertical axis) as a function of the chosen bandwidth (on the horizontal axis) in Figure 2.5. A value of "100" on the horizontal axis represents the optimal bandwidth suggested by Imbens and Kalyanaraman (2012). "200" means 200% of (i.e., two times of) the optimal bandwidth, "300" means 300%, and so forth. The left-hand side figures describe *Dividend Payout* and the right-hand side ones are for *Total Payout*. As one can observe, the local RD estimates are almost always negative and stable, with both economic and statistical significance, over the whole spectrum of bandwidth choices. This result shows that our local linear RD estimates are unlikely to be driven by any specific choice of bandwidths.

Second, if our RD estimation truly reflects a negative, causal effect of labor power on corporate payout, we should not observe a similar effect if we artificially assume a threshold other than 50% that determines union election outcomes. Hence, we run placebo tests to check whether we still observe discontinuity in payout ratios at randomly selected thresholds that are different from the true 50% threshold. We run this placebo test 1,000 times and plot a histogram of the distribution of the corresponding local RD estimates in Figure 2.6. The vertical dashed line stands for the value of the local RD estimate obtained using the true cutoff point of 50%. As we can observe, all of the histograms in Figure 2.6 are approximately centered around 0, suggesting that the negative effect of labor power on payout is absent if we artificially pick a cutoff point other than 50%. This placebo analysis enhances our confidence in the RD procedure and the resulting estimates, because it rules out chances as an explanation for our main findings in the previous subsection.

Third, we examine alternative measures of a firm's payout policy and present the results in Table 2.5. Row (1) shows that unionization has a significantly negative effect on firms' dividend yields (i.e., cash dividends per dollar of stock owned) and total yields (i.e., total payout amount per dollar of stock owned) in each of the three years after the election. Specifically, firms with winning union elections have a dividend yield 0.9% lower and a total yield 1.4% lower than those that fail to pass union elections one year afterwards. Rows (2) and (3) examine payout ratios using a firm's sales and free cash flows (rather than its earnings), respectively, as the denominator, and find similar (though slightly weaker) results to our main local RD results in Table 2.4.[40] In Row (4), we do not scale payout amounts but instead directly analyze cash dividends and total payouts on a per share basis. Row (5) examines dividends and total payouts scaled by total assets, and Row (6) examines the natural logarithm of un-scaled dollar amounts of dividends and total payouts. The local RD estimates remain negative and statistically significant

[40] Scaling dividends and total payouts using operating cash flows yields very similar results.

for the majority of columns.

Finally, we try alternative ways of constructing our sample with a variety of data filters (such as how to deal with multiple elections held by the same firm in one year, the number of eligible employees for voting, and firms with zero payout). We find that our main results are robust in these alternative samples. We also estimate a dynamic RD model with cubic polynomials in the spirit of Cuñat, Gine, and Guadalupe (2012) and find results consistent with our baseline findings. To save space, we present these results in the Appendix C Table A.1.[41]

2.5 UNDERLYING MECHANISMS

So far our empirical evidence suggests a negative, causal effect of labor power on corporate payout, consistent with the *flexibility hypothesis*. In this section, we aim to further understand the underlying economic mechanisms through which enhanced labor power reduces firm payouts. The *flexibility hypothesis* suggests two plausible mechanisms: operating flexibility and financial flexibility.

2.5.1 Operating flexibility

The first underlying mechanism is labor power's adverse effect on firms' operating flexibility. To reduce cash flow risk and make their operations more flexible, firms need to increase the variable component of their cost structure, which entails linking their labor expenses (salaries and other employee compensation) to sales revenue. However, organized labor represented by unions can make wages sticky and layoffs costly, thereby increasing the adjustment costs of a firm's labor stock. Furthermore, unions frequently intervene in a firm's

[41] In unreported analysis, we find that the negative effect of labor power on firm payout is absent in firms located in states with right-to-work legislation but without work stoppage provisions, where unions have smaller power to expropriate rents. The results are also contained in the Appendix C Table A.3.

restructuring activities to save workers' jobs, such as blocking plant closures, which makes it harder for the firm to adjust its physical capital. Therefore, enhanced labor power will increase a firm's cash flow risk and reduce its operating flexibility.[42]

This argument implies a stronger negative effect of the passage of union elections on payout policy for firms with an *ex ante* lower level of operational flexibility because these firms would face a larger threat from the winning elections and thus decrease their payouts to a greater degree. By doing so, these low-operating-flexibility firms can prevent their cash flow risk from going up further, which allows them to become better able to save for profitable investment opportunities in the future. As a result, we expect that the negative effect of labor power on payout policy is stronger when firms have a higher level of *ex ante* operational leverage (i.e., lower operating flexibility).

We follow the existing literature (e.g., Mandelker and Rhee, 1984) to estimate a firm's operational leverage as the elasticity of the firm's earnings before interest and taxes (EBIT) with respect to its sales, using the most recent 12 quarterly observations up to the predetermined year. Firms with operational leverage higher than the sample median are those with lower operating flexibility. We report the local RD estimation results for firms in the high operational leverage subsample (the top panel) and the low operational leverage subsample (the bottom panel) in Table 2.6.

Consistent with our conjecture, the coefficient estimates on *Union* are all negative and significant in the top panel (i.e. for firms with high *ex ante* operational leverage), suggesting that the negative effect of labor power on payout is mostly concentrated in firms with less flexible operations. On the contrary, the local RD estimates for firms with low *ex ante* operational

[42] Chen et al. (2011a, b) show that the reduced operating flexibility due to labor unions increases a firm's cost of capital.

leverage (in the bottom panel) are much smaller in magnitude than those in the top panel and statistically insignificant. This finding is possibly due to the fact that firms with low *ex ante* operational leverage have flexible operations to begin with and thus enhanced labor power has no material effect on their cash flow risk or operating flexibility, making it less necessary for them to reduce their dividend payouts.

To examine whether the coefficient estimates on *Union* between the two subsamples are statistically different from each other, we use a global RD method. Specifically, we estimate a polynomial model (with order three) as specified in Equation (1) for the two subsamples and report the results in Panels C and D. To save space, the coefficients for polynomial variables are omitted. As we can see, the results estimated using the global polynomial RD model are similar, both statistically and economically, to those using a local RD model. Moreover, we report at the bottom of Table 2.6 the p-values for the one-tailed Chi-squared statistics testing that the coefficients before *Union* in Panel C are smaller (i.e., more negative) than those in Panel D. The results reveal that the differences in the coefficient estimates on *Union* between the two subsamples are significant at the 5% or 10% level in the first two years after the union elections.

Another test we perform to examine the operating flexibility channel is to conduct an event study on union elections. First, we run nonparametric local linear regressions on the cumulative abnormal returns (CARs) around the closing dates of union elections (i.e., the events). We examine CARs calculated from the market model during various event windows, including those from day -3 to day +3, from day -1 to day +3, and from day 0 to day +5. We report the results in Table 2.7. Panel A examines the CARs using all the union elections. Panels B1 and B2 examine the CARs for subsamples separated by the level of operational leverage. As we can observe, the market overall does not react significantly to the passage or failure of the union elections, which is consistent with Lee and Mas (2012). However, unionization has significantly negative effect on the CARs for firms with a high level of operational leverage (i.e., those having low operating flexibility), but has a small and insignificant effect on the CARs for firms with a low level of operational leverage. These results suggest that the market only deems unionization as detrimental for a firm's equity value if the firm is operationally inflexible, which is consistent with our hypothesis that enhanced labor power reduces the operating flexibility of a firm and thus hurts its future operations and long-term value.[43]

2.5.2 Financial flexibility

The second possible economic mechanism we consider is a firm's financial flexibility. On behalf of the employees, labor unions often bargain with their employers for a higher and more stable wage level and better working conditions, putting an additional layer of constraint on firm management when they make investment decisions. Even if a profitable project suddenly becomes available, the firm managers may not be able to immediately grasp the investment opportunity because the high setup costs at the initial stage and subsequent cash flow uncertainties could lead to volatile cash outflows. This problem results in a possible delay in sending out paychecks or even a drastic restructuring of the company's workforce including layoffs and forced turnovers, all of which will invite severe criticism and face vehement opposition from the union leaders. To the extent that organized labor has bargaining power with the board and management, the firm may end up having to give up many value-enhancing investment opportunities unless it has enough financial flexibility (e.g., enough internal retained

[43] In untabulated analysis, we find that the market responses to the passage of union elections more negatively for firms with lower operating flexibility in a multivariate OLS regression framework, which again supports our conjecture that operating flexibility is a plausible economic mechanism through which labor power alters firms' payout policy.

earnings, easy access to external capital, or few pre-commitments to pay out dividends).

Hence, if a firm is already financially constrained, enhanced labor power makes its financial situation even tighter, prompting it to cut down payouts so as to free up some internal retained earnings for future investment use. On the other hand, firms that are less financially constrained do not need to drastically change their payout policy because enhanced labor power is unlikely to affect their future investment activities significantly. The above argument suggests a stronger negative effect of passing union elections on payout policy for firms with *ex ante* more stringent financial constraints.

To examine this conjecture, we use a comprehensive set of existing measures of financial constraints, such as the Kaplan-Zingales (KZ) index (Kaplan and Zingales, 1997), the Whited-Wu (WW) index (Whited and Wu, 2006), the Size and Age (SA) index developed in Hadlock and Pierce (2010), and the Hoberg-Maksimovic text-based (HM) index (Hoberg and Maksimovic, 2015), as of the predetermined year to proxy for the strength of a firm's financial conditions. Firms with a given index above (below) the sample median are considered to be more (less) financially constrained.[44] We report the local RD estimation results for firms in the more financially constrained subsample and the less financially constrained subsample in Table 2.8. Panels A1 and A2 examine the KZ Index, Panels B1 and B2 explore the WW Index, Panels C1 and C2 study the SA Index, and Panels D1 and D2 examine the HM Index.

Our results are mixed regarding the financial flexibility channel. Consistent with our conjecture, the coefficient estimates on *Union* are mostly negative and significant in Panel A1 (i.e. for firms with more stringent financial constraints as measured by the KZ Index), suggesting

[44] Note that the HM Index is available for Compustat firms only between 1997 and 2009, which reduces our sample size. Moreover, following the suggestions of Hoberg and Maksimovic (2015), we treat Compustat firms with missing HM index as the least financially constrained ones (i.e., setting their HM Index to the sample minimum before splitting the sample based on medians).

that the negative effect of unionization on payout is concentrated in financially constrained firms. By contrast, the local RD estimates for less constrained firms measured by the KZ Index (in Panel A2) are much smaller in magnitudes than those in the top panel and are statistically insignificant. Moreover, two of the six estimated coefficients are actually positive, though statistically insignificant.

However, the evidence based on the WW Index (in Panels B1 and B2) seems to suggest the opposite: for most (four out of six) columns, the negative effect of unions on corporate payout is stronger for less financially constrained firms. We continue to observe mixed (i.e., inconclusive) results in Panels C1-C2 (the SA Index) and D1-D2 (the HM Index).

In an untabulated analysis, we examine a variety of firm characteristics that reflect a firm's financial flexibility used in the existing literature, such as firm size, firm age, cash flows, Tobin's Q, the existence of bond ratings, and firm profitability. We continue to find mixed evidence.

One caveat for the analysis in this section is that the literature so far has not reached a consensus as to which of the financial constraint measures is the best (or the "right") one (see recent papers such as Farre-Mensa and Ljungqvist (2015) and Hoberg and Maksimovic (2015) for a more detailed discussion on this issue). That is why we choose to examine a comprehensive set of financial constraint measures to ensure that our results are not driven by any particular measure adopted. While our mixed evidence above suggests that financial flexibility might not be an important underlying economic mechanism through which labor power decreases firm payout, it could also reflect the possible limitations of existing financial constraint measures, i.e., these measures fail to capture a firm's true level of financial constraints.

Another possible explanation for the lack of consistent evidence on the financial flexibility channel is that not paying dividends itself is an indication of tight (i.e., constrained) financial situation for a firm, as argued by previous literature such as Fazarri et al. (1988), Almeida and Campello (2007) and Denis and Sibilkov (2010). Therefore, we do not observe much variation in payout ratios (especially dividend payouts) among firms that are already financially constrained because the majority of such firms pay zero dividends anyway, regardless of their unionization status. In contrast, less financially constrained firms have more variation in their payout ratios, which could allow us to observe a stronger effect of unions on payout ratios. In other words, the fact that dividend payment is one indication of financial strength biases against us finding results consistent with our hypothesis, even if unions do affect payout ratios through the channel of financial flexibility.

Overall, based on the above discussions, we cannot conclude with confidence that financial inflexibility is an underlying mechanism through which labor power affects corporate payout.

2.5.3 Other financial policies

Our results so far suggest that firms reduce corporate payout and retain more internally generated earnings to cope with the upcoming operating inflexibility caused by enhanced labor power. A natural question is where the saved earnings (net income) end up going. Do firms keep these additional retained earnings in the form of liquid assets (e.g., cash and cash-equivalent marketable securities, net working capital, etc.) or use them to pay off debt so that they can increase their borrowing capacity? To answer this question, we analyze, in this subsection, a firm’s other corporate polices (such as leverage, cash holdings, and working capital management) following its union elections.

To be consistent with our main payout measures, we scale a firm's change of balance sheet items (such as long-term debt, cash, and working capital) over a fiscal year following the union elections by its earnings (net income) during the same year. These normalized change-variables can then indicate how the firm makes use of these saved earnings after cutting payouts to its shareholders. However, in these tests, we have to drop those firm-year observations with negative or zero earnings because otherwise there is nothing to "save" (i.e., it is difficult to interpret the results with non-positive earnings).

Table 2.9 reports the local linear RD regression results of other financial policies. Panel A examines a firm's capital structure decisions, measured by its change in total debt over earnings (*Δ Total Debt over Earnings*), within the first three years after the passage of union elections. As we can see, the coefficient estimates on *Union* are economically small and statistically insignificant, suggesting that firms do not use the saved retained earnings to pay off their total debt.[45] Similarly, Panel B shows that a firm's cash policy, measured by its change in cash and short-term investments over earnings (*Δ Cash over Earnings*), is not significantly affected by the passage of union elections either. However, Panel C of Table 2.9, which examines a firm's working capital management, measured by its change in net working capital over earnings (*Δ Working Capital over Earnings*), shows that the coefficient estimates on *Union* are positive and significant in the first two years after the union elections, suggesting that firms use saved earnings mostly in the investment of its net working capital (i.e., liquid assets not necessarily in the form of cash).[46]

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[45] The results remain qualitatively similar if we examine long-term debt only.

[46] Note that debt, cash, and working capital are balance sheet items whereas earnings and payout (dividends) are income statement items. We examine changes (rather than levels) of these balance-sheet variables because they are directly related to the saved earnings by paying out less. For example, a firm with a lower payout ratio could use the larger retained earnings to pay down debt, invest in net working capital, or increase its cash holdings.

The above results suggest that strategic (bargaining) concerns of firm managers after unionization, as documented in previous literature (e.g., Klasa et al., 2009; Matsa, 2010), might have limited their abilities to keep saved earnings within the firm in the form of very liquid assets (i.e., cash and cash-equivalent marketable securities) or to use such savings to pay off debt.[47] This is because unions could have easily observed and verified such usage of corporate resources and thus pressured firm managers to use these additional savings to improve worker welfare instead, nullifying managers' effort to maintain operating flexibility after the enhancement of labor power. In contrast, firms would use their saved earnings (from reduced payout) mostly to invest in the net working capital such as increasing its accounts receivables and/or decreasing its accounts payables, because they could make easy arguments to union leaders why such operational changes are necessary to the firm's long-run sustainability.

In summary, this subsection shows that to deal with the operating inflexibility brought by the passage of union elections, firms would reduce corporate payout to shareholders and keep saved earnings within the firm in the form of liquid assets that are not easily grasped or negotiated away by the unions.

2.6 ALTERNATIVE INTERPRETATIONS

While the concerns over operating inflexibility could be the main mechanism through which labor power reduces corporate payout, several alternative mechanisms could give rise to the same empirical results documented above.

One alternative explanation for the negative effect of labor power on corporate payout is labor's innate distaste for dividends and its greater influence on managerial decision making

[47] In an untabulated analysis, we examine alternative measures of capital structure (such as the book and market-value leverage ratios) and alternative measures of cash policy (such as cash over assets or PPE) in the local RD setting, and continue to find insignificant coefficient estimates on *Unionization.*

after workers coordinate among themselves more efficiently through unionization. As a claimant to firms' resources, workers generally compete with shareholders in extracting economic rents created by the business and thus prefer to retain cash flows within the firms as opposed to paying them out. In fact, they sometimes regard corporate payout as "wages paid to shareholders" (e.g., Michaely and Roberts, 2012) and thus generally oppose the idea of increasing dividend payouts. When workers organize themselves more efficiently through unionization, they gain more bargaining power against shareholders in shaping the firm's financial policy by either sending more of their representatives onto the board of directors (Hunter, 1998; Appelbaum and Hunter, 2004) or negotiating with corporate managers more aggressively about limiting/reducing corporate payout. Thus, our documented negative effect of labor power on corporate payout could be due to the fact that unions actively pressure managers to reduce their dividends and total payouts, rather than due to a natural and possibly optimal response of shareholders (and managers) to the passage of union elections out of concerns for future operating inflexibility.

To examine this alternative explanation, we divide our sample of firms based on the alignment of interests between employees and shareholders, which is measured by employees' stock ownership through pension plans. We obtain detailed asset holdings of a firm's pension plans (including both defined contribution plans and defined benefit plans), required to be reported in the IRS Form 5500, from Boston College's Center for Retirement Research (CRR) Data Enclave website.[48] Following Rauh (2006), we adopt two measures of employees' stock ownership through pension plans: the fraction of firm equity ownership by pensions (i.e., the percentage of the firm's equity market value held by employees through pension plans) and the percentage of pension holdings invested in the firm's own securities (i.e., the market value of the

[48] Since such pension plan data are only available from 1992 to 2007, we limit our tests to union elections held during this time period when doing this subsample test.

firm's own securities held by their pension funds divided by the pensions' total asset holdings).

Larger employee stock ownership through pension plans means a better alignment of interests between employees and shareholders, because employees in such firms care more about shareholder wealth than those in firms with smaller employee stock ownership. Hence, unions in firms with a larger employee stock ownership are less likely to use their influence over firm management to push for employee-friendly decisions that may ultimately lead to a decline in shareholder wealth. If the alternative explanation for our findings (based on labor's innate distaste for dividends and union power) is true, we would expect that the negative effect of labor power on payout is weaker in firms with a larger employee stock ownership because workers in such firms would have lower incentives to use their enhanced bargaining power to limit or reduce corporate payout.

Table 2.10 reports the local RD estimation results for the subsample analysis based on the fraction of firm equity ownership by pensions (Panels A and B) and the percentage of pension holdings invested in the firm's own securities (Panels C and D). As we can observe, there is no clear evidence that the negative effect of labor power on corporate payout is weaker in firms with a larger employee stock ownership (in Panels A and C). In fact, there is some mild evidence that the effect is slightly stronger in firms with a larger pension ownership (when comparing Panels A and B), which is inconsistent with the alternative explanation based on labor's distaste for dividends and the direct influence of union power after the passage of union elections. In fact, the evidence in Panels A and B is more consistent with the voluntary response of shareholders (and managers) to enhanced labor power. This is because, for firms with a larger employee stock ownership through pensions, more of their employees have the same concerns as other shareholders over the upcoming operating inflexibility after the passage of union elections and thus are willing to support the firms' decisions to limit or reduce corporate payout.[49]

Another alternative explanation for our results is managers' distaste for corporate payout due to their empire building incentives or preferences for private benefits of control, which motive them to keep corporate resources within the firm rather than distributing them back to shareholders. In the presence of such agency conflicts, managers would use the passage of union elections as an excuse to convince shareholders to agree with cutting dividends in the name of "preserving corporate resources for future unexpected operational disruptions from organized labor" or "reducing payout as a voluntary concession to unions/workers in order to enhance their loyalty, morale, and productivity." Under this alternative explanation, the lower payout in unionized firms is not caused by the optimal response of shareholders to cope with future operating inflexibility, but rather caused by perverse incentives of managers whose interests divert from those of shareholders.

To examine this alternative explanation, we divide our sample of firms based on their levels of corporate governance, measured by the entrenchment index (E-index) developed by Bebchuk, Cohen, and Ferrell (2009).[50] A higher level of the E-index indicates poorer corporate governance because the managers are more entrenched. Then we partition our sample into two subsamples based on whether a firm's E-index is above the sample median, and test how our local RD results differ across these two subsamples. If the alternative explanation for our findings (based on managers' distaste for dividends due to agency reasons) is true, we would expect that the negative effect is weaker in firms with better corporate governance (i.e., lower E-

[49] In an untabulated analysis, we examine alternative measures of interest alignment between employees and shareholders (such as the ratio of defined benefit plans' holdings to defined contribution plans' holdings) and find similar results.

[50] The six provisions are staggered boards, limits to bylaw amendments, limits to charter amendments, supermajority requirements for mergers, poison pills, and golden parachutes.

index) because managers in such firms would have fewer opportunities to use their power/influence to limit or reduce corporate payout.

We report the local RD estimation results for the subsample analysis based on the level of corporate governance in Table 2.11. Opposite to the predictions of the above alternative explanation, the negative effect of labor power on payout is much stronger (in terms of both statistical significance and economic magnitude) in firms with better corporate governance, suggesting that lower payout ratios after the passage of union elections are unlikely to be driven by the agency conflict between shareholders and managers who hope to retain corporate resources within the firms for their personal benefits.[51]

In summary, this section shows that the documented negative effect of labor power on corporate payout is not likely to be driven by the preferences and control power of either employees or managers (i.e., agency conflicts), but more likely to be driven by firms' optimal response to enhanced labor power.

2.7 CONCLUSION

In this paper, we have studied labor power as an important yet under-explored determinant of a firm's payout policy. Using a regression discontinuity design that exploits locally exogenous variation in labor's collective bargaining power generated by close-call union elections, we show that passing a union election leads to a 10.1% lower dividend payout ratio and a 19.3% lower total payout ratio than failing the election in the following year. Our results are robust to alternative choices of kernels and bandwidths, alternative measures of corporate payout, and are absent at artificially chosen voting thresholds that determine union election

[51] In an untabulated analysis, we examine alternative measures of corporate governance used in the literature (such as product market competition and institutional ownership) and find very similar results to the ones using the E-index.

outcomes. We show that operating leverage is one possible mechanism through which enhanced labor power negatively affects corporate payout, and that firms use the saved earnings from reductions in payout to invest in net working capital rather than paying off debt or increasing cash holdings.

Overall, we find evidence consistent with the hypothesis that firms cut down corporate payout as a response to increased labor power to maintain their operating flexibility and reduce their cash flow risks. Our paper sheds new light on the determinants of payout policy and how organized labor influences corporate financial decisions. However, one caveat of our study, which possibly applies to all other papers adopting the RD method, is that the RD design has strong local validity but weak external validity (Roberts and Whited, 2013). Hence, one should interpret the results in this paper with caution. For example, we cannot speak to the causal link between labor power and payout policy for firms that do not hold union elections at all.

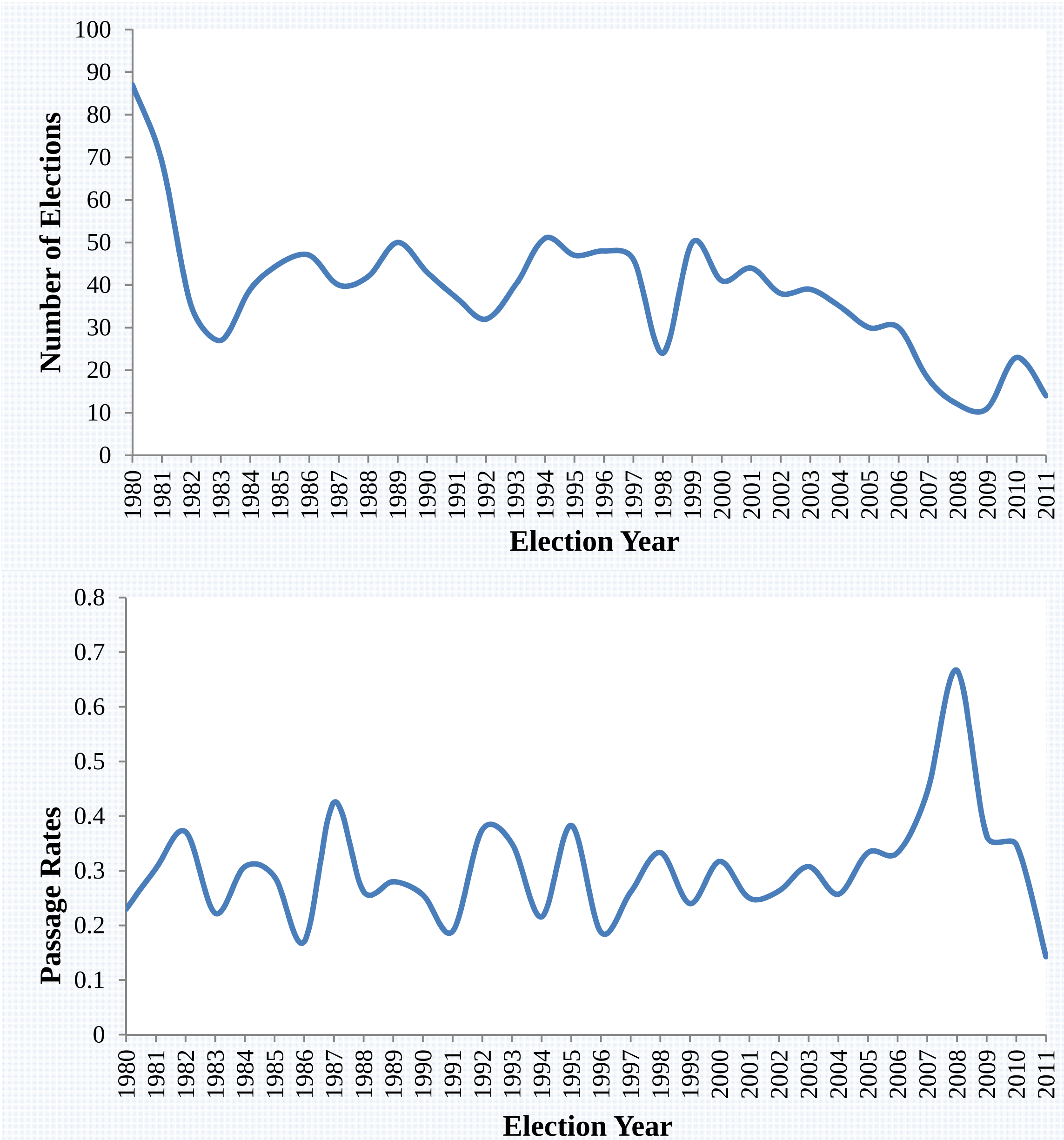

Figure 2.1: Number of union elections and passage rates by year
This figure plots the number of union elections by year (top) and the average passage rates by year (bottom). Union election results are from the National Labor Relations Board (NLRB) over 1980 to 2011.

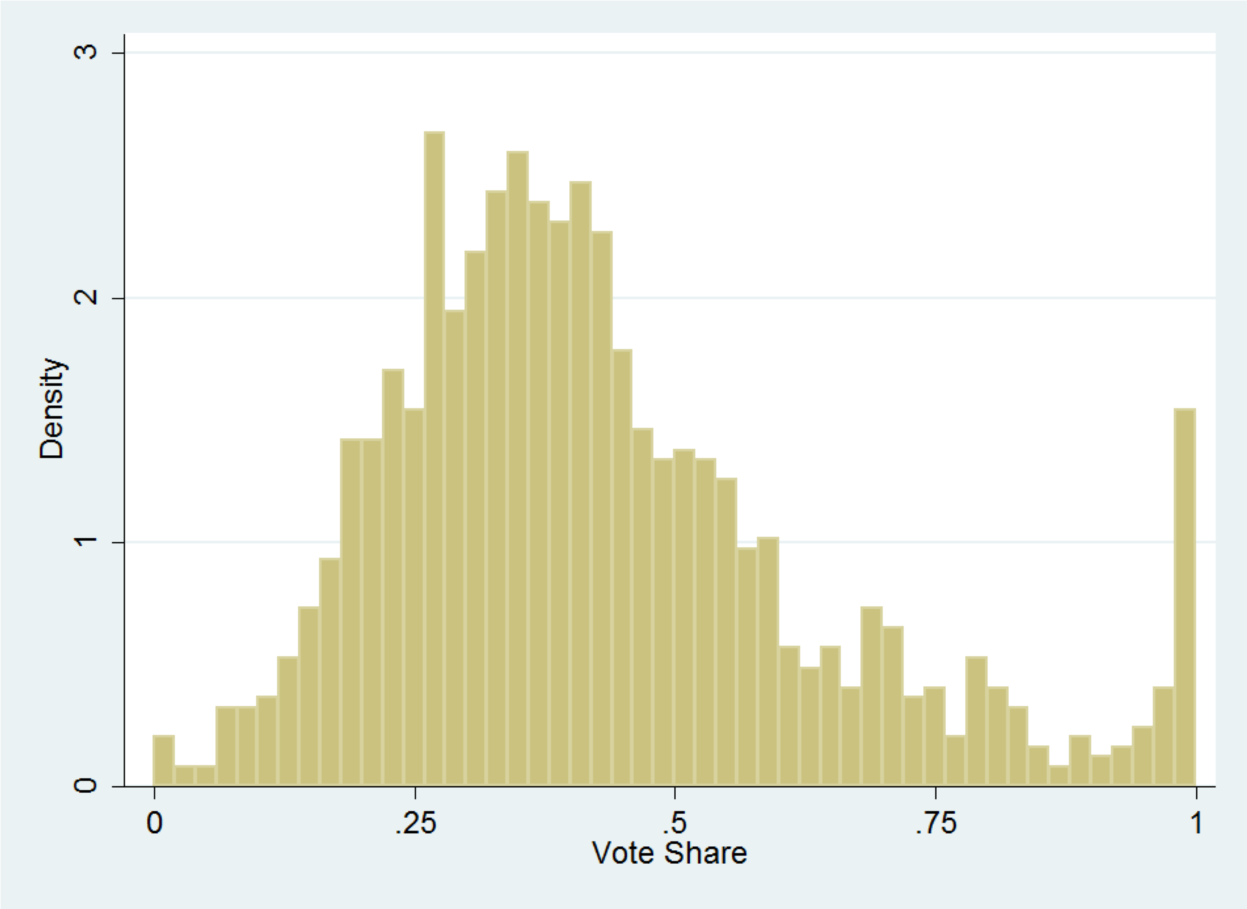

Figure 2.2: Distribution of vote share
This figure plots a histogram of the sample distribution of vote share (i.e., the percentage of votes in an election in favor of unionization) across 50 equally-spaced bins (with a bin width of 2%). Union election results are from the NLRB over 1980 to 2011.

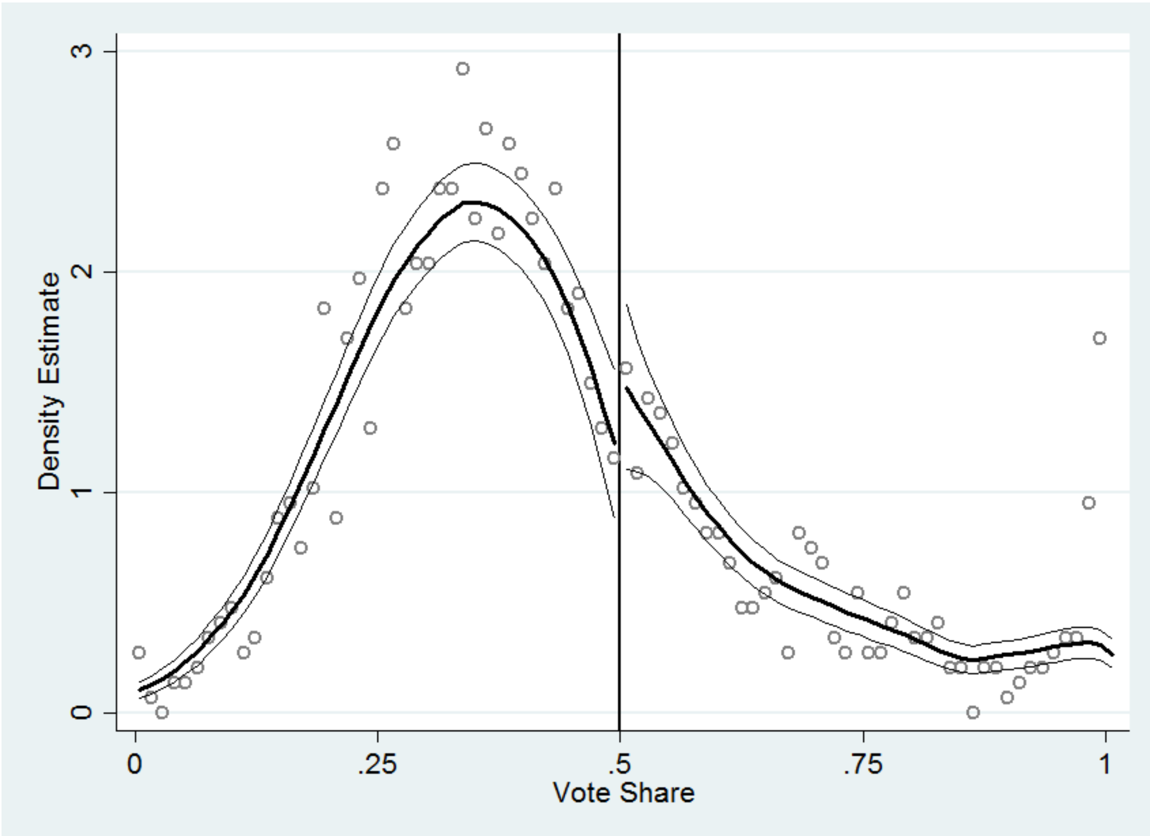

Figure 2.3: Density of union vote share

This figure plots the density of union vote share (i.e., the percentage of votes in an election in favor of unionization) following the procedure in McCrary (2008). The x-axis is union vote share. The dots represent the density estimate for each chosen bin and the bold line is the fitted density function of union vote shares with a surrounding 95% confidence interval. Union election results are from the NLRB over 1980 to 2011.

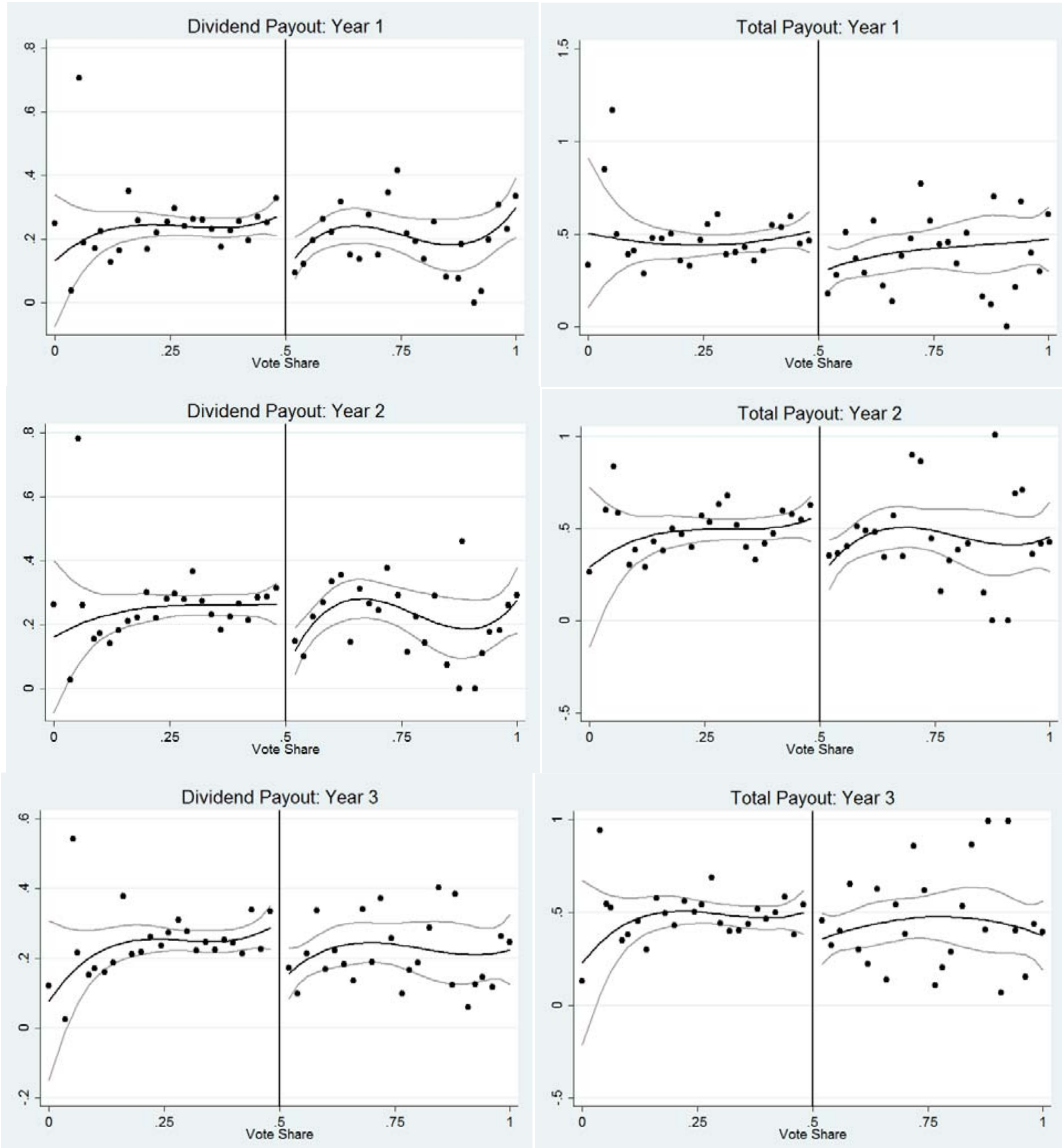

Figure 2.4: Regression discontinuity plots for dividend and payout ratios

This figure presents regression discontinuity plots using a fitted cubic polynomial estimate with a 95% confidence interval around the fitted value. The x-axis is union vote share (i.e., the percentage of votes in an election in favor of unionization). The dots depict the average *Dividend Payout* (left) and *Total Payout* (right), defined in Appendix B, in each of the 50 equally-spaced bins (with a bin width of 2%). Union election results are from the NLRB over 1980 to 2011. Payout data are from the Compustat.

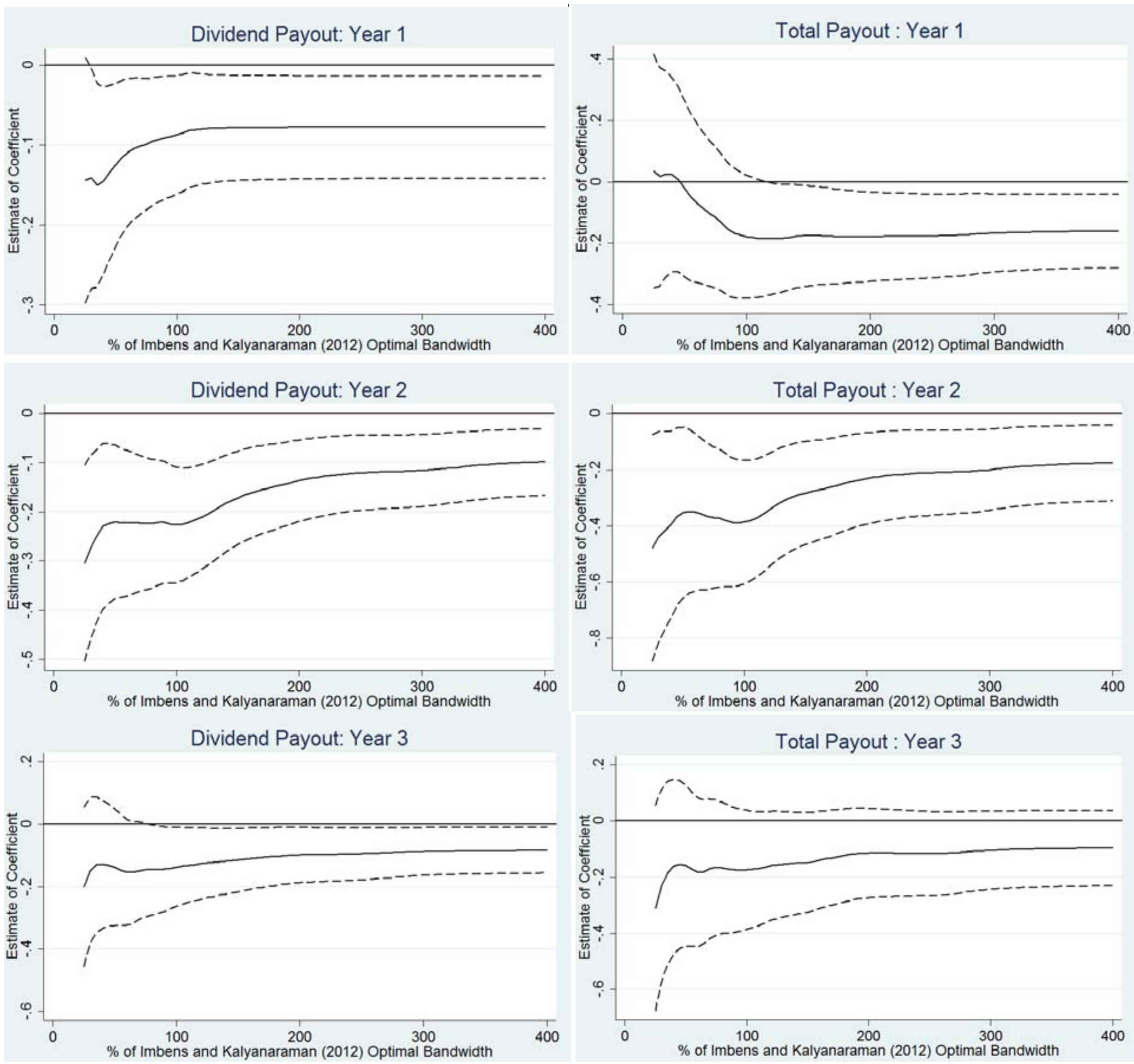

Figure 2.5: Alternative RD bandwidths
This figure plots the estimated local RD coefficients along with their 95% confidence intervals (on the vertical axis) against alternative values of bandwidths (on the horizontal axis). A value of "100" on the horizontal axis represents the optimal bandwidth suggested by Imbens and Kalyanaraman (2012). "200" means 200% of (i.e., two times of) the optimal bandwidth, “300” means 300%, and so forth. The left-hand side figures describe *Dividend Payout* and the right-hand side ones are for *Total Payout*. Union election results are from the NLRB over 1980 to 2011. Payout data are from the Compustat.

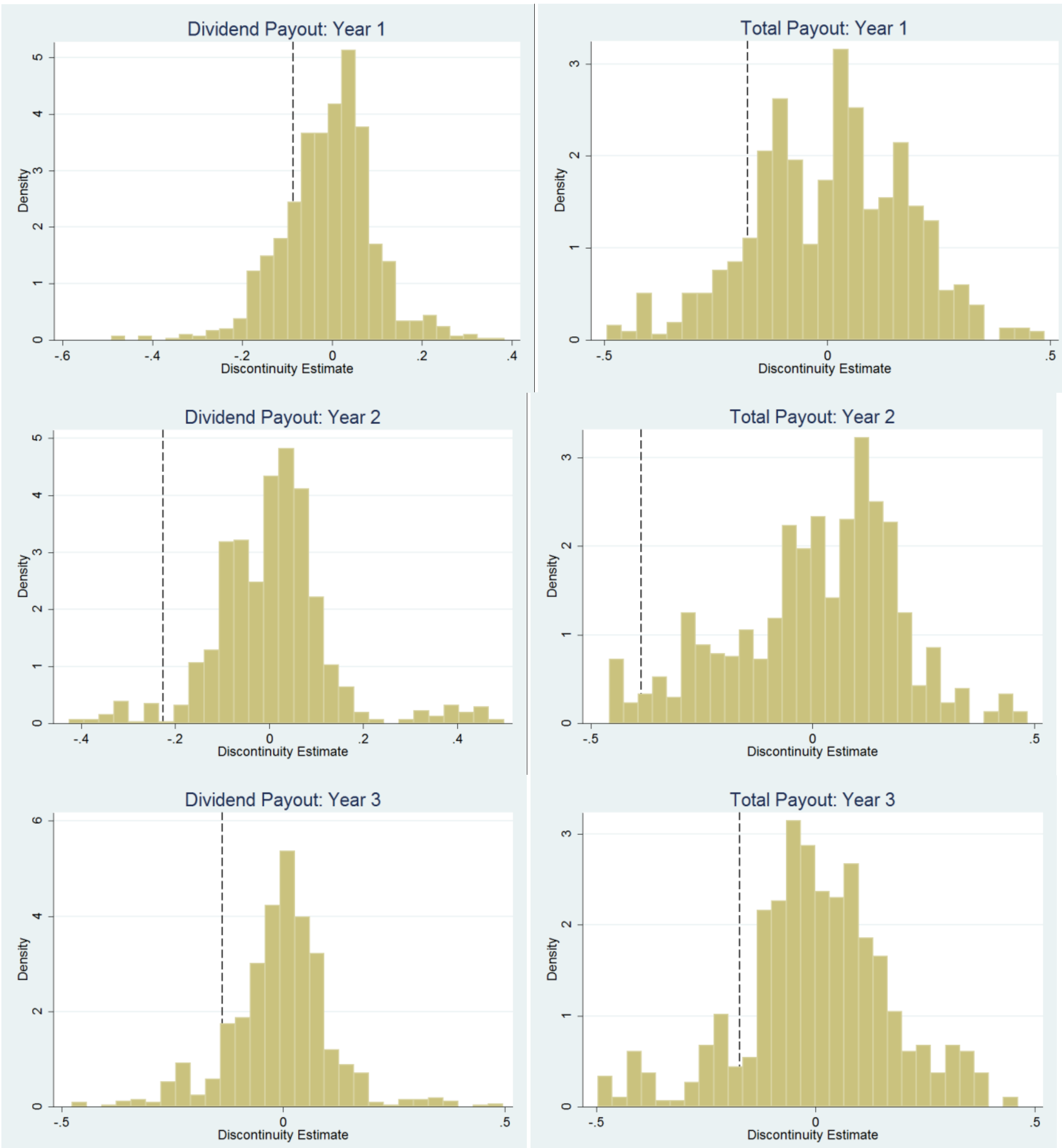

Figure 2.6: Placebo tests
This figure plots a histogram of the distribution of the local RD estimates from 1000 placebo tests. The x-axis represents the RD estimates from the placebo tests that artificially select an alternative threshold other than 50%. The dashed vertical line represents the RD estimate at the true 50% threshold. The y-axis is the density of the estimated coefficients for *Dividend Payout* (left) and *Total Payout* (right). Union election results are from the NLRB over 1980 to 2011. Payout data are from the Compustat.

Table 2.1: Descriptive statistics

This table presents descriptive statistics of our sample, which includes 1,234 union elections taken place between 1980 and 2011. Panel A reports union election statistics. "*Vote*" is the percentage of votes in an election in favor of unionization (i.e., vote shares). "*Passage*" is an indicator variable that equals one if a firm is unionized as a result of an election, and zero otherwise. Panel B reports summary statistics of variables used in our study, summarized over the predetermined year (the year before the reported closing date of the union elections) and the first three years after union elections. All variables are defined in Appendix B. Union election results are from the NLRB. Payout and other financial variables are from Compustat.

Panel A: Election statistics

Variables	Obs.	Mean	Median	Std. Dev.
Vote	1,234	0.428	0.388	0.209
Passage	1,234	0.287	0.000	0.452

Panel B: Summary statistics

Variables	Mean	Min	25th	Median	75th	Max	Std. Dev.
Dividend Payout	0.240	0.000	0.000	0.131	0.378	1.160	0.302
Total Payout	0.449	0.000	0.000	0.272	0.657	2.084	0.547
Assets(in Billions)	3.834	0.003	0.146	0.718	2.942	56.469	8.639
Market Value(in Bil.)	3.122	0.002	0.088	0.468	2.258	56.537	7.925
Leverage	0.247	0.000	0.107	0.215	0.344	0.906	0.192
PPE/Assets	0.365	0.018	0.213	0.333	0.494	0.876	0.200
Cash/Assets	0.071	0.000	0.013	0.035	0.095	0.475	0.090
Capex/Assets	0.068	0.004	0.032	0.056	0.088	0.280	0.052
TobinQ	1.483	0.657	1.021	1.254	1.702	4.544	0.721
Age(in Years)	20.385	1.000	7.000	17.000	31.000	37.000	14.786
Cashflow Volatility	0.041	0.000	0.019	0.032	0.052	0.307	0.036
Institutional Ownership	0.446	0.006	0.211	0.446	0.659	1.000	0.261
Sales Growth	0.099	-0.505	0.003	0.076	0.159	0.874	0.204
State GDP Growth	0.067	-0.012	0.044	0.062	0.089	0.142	0.033

Table 2.2: Naive OLS regressions

This table presents results from naive OLS regressions. The dependent variables are *Dividend Payout* (left) and *Total Payout* (right) N year(s) after the union elections (N=1, 2, or 3). The variable of interest is a unionization dummy (*Union)* that equals one if a union election wins and zero otherwise. Panel A presents results of univariate OLS analysis without any controls. Panel B presents results of multivariate OLS analysis that controls for other covariates at the predetermined year (the year before the reported closing date of the union elections) and year and industry fixed effects. Union election results are from the NLRB over 1980 to 2011. Payout data are from the Compustat. T-statistics adjusted for heteroskedasticity and within-firm clustering are displayed in the parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Panel A: Univariate OLS analysis

	Dividend Payout$_{t+N}$			*Total Payout*$_{t+N}$		
	N=1	N=2	N=3	N=1	N=2	N=3
Union	-0.031	-0.038*	-0.041**	-0.073**	-0.072*	-0.069*
	(-1.540)	(-1.777)	(-1.971)	(-2.024)	(-1.797)	(-1.770)
Observations	1,234	1,234	1,234	1,234	1,234	1,234
R-squared	0.002	0.003	0.004	0.004	0.003	0.003

Panel B: Multivariate OLS analysis

	Dividend Payout $_{t+N}$			*Total Payout* $_{t+N}$		
	N=1	N=2	N=3	N=1	N=2	N=3
Union	-0.017	0.006	0.017	-0.095*	-0.055	-0.009
	(-0.751)	(0.191)	(0.580)	(-1.783)	(-0.785)	(-0.150)
Ln Assets	0.029***	0.021*	0.036***	0.093***	0.087***	0.102***
	(3.410)	(1.733)	(3.163)	(3.820)	(3.208)	(4.033)
ROA	0.327*	0.227	0.296	1.306***	0.370	0.267
	(1.720)	(1.015)	(1.061)	(2.822)	(0.633)	(0.441)
Leverage	-0.301***	-0.240***	-0.203**	-0.671***	-0.634***	-0.581***
	(-5.306)	(-3.350)	(-2.052)	(-4.126)	(-3.288)	(-3.360)
PPE/Assets	0.198***	0.297***	0.258***	0.097	0.355**	0.258*
	(4.092)	(4.097)	(3.754)	(0.844)	(2.375)	(1.776)
Capx/Assets	-0.319	-0.461	-0.687**	-0.334	-0.744	-1.316**
	(-1.439)	(-1.467)	(-2.588)	(-0.620)	(-1.101)	(-2.488)
Tobin's Q	0.000	-0.017	0.016	-0.010	0.041	0.107*
	(0.023)	(-0.922)	(0.514)	(-0.227)	(0.832)	(1.697)
Cash/Assets	-0.098	0.040	0.193	0.219	0.091	-0.030
	(-0.635)	(0.269)	(1.097)	(0.615)	(0.240)	(-0.085)
Institutional Own	-0.032	0.047	-0.116*	-0.119	0.202	-0.069
	(-0.642)	(0.673)	(-1.655)	(-0.738)	(1.222)	(-0.421)
Cashflow Vol	-0.544*	-0.584	-0.677*	-0.548	0.492	-0.259
	(-1.821)	(-1.399)	(-1.836)	(-0.627)	(0.482)	(-0.318)
Log Age	0.004***	0.004***	0.004***	0.003	0.003	0.001
	(4.913)	(3.005)	(3.034)	(1.323)	(1.113)	(0.198)
Sales Grow	-0.027	0.105	0.030	-0.260*	-0.047	-0.018
	(-0.509)	(1.470)	(0.456)	(-1.874)	(-0.271)	(-0.121)
State GDP Grow	-0.610	0.009	0.382	-0.996	0.378	0.407
	(-1.093)	(0.013)	(0.746)	(-0.754)	(0.231)	(0.297)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	541	516	489	541	516	489
R-squared	0.496	0.392	0.408	0.350	0.279	0.382

Table 2.3: Regression discontinuity: Global polynomial

This table presents RD results from estimating a polynomial model specified in Equation (1). The dependent variables are *Dividend Payout* (left) and *Total Payout* (right) N year(s) after the election (N=1, 2, or 3). The variable of interest is a unionization dummy (*Union)*. *Vote* is the vote share for an election and *c* is the cutoff point for passing an election (which is 50% in our case). Union election results are from the NLRB over 1980 to 2011. Payout data are from the Compustat. Panels A, B, and C use polynomials of order three, two, and four, respectively. Panel D uses polynomials of order three but controls for other covariates at the predetermined year (the year before the reported closing date of the union elections) and year and industry fixed effects. T-statistics adjusted for heteroskedasticity and within-firm clustering are displayed in the parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Panel A: Payout ratios after the elections (polynomial order of three)

	Dividend Payout$_{t+N}$			*Total Payout*$_{t+N}$		
	N=1	N=2	N=3	N=1	N=2	N=3
Union	-0.181***	-0.207***	-0.175**	-0.232**	-0.332***	-0.180
	(-2.711)	(-3.120)	(-2.415)	(-1.994)	(-2.622)	(-1.419)
Vote-c	0.790	0.135	0.956	0.625	1.108	1.083
	(1.043)	(0.159)	(1.119)	(0.496)	(0.716)	(0.735)
(Vote-c)^2	4.061	0.946	5.019	1.188	5.289	7.171
	(1.069)	(0.222)	(1.190)	(0.179)	(0.702)	(0.958)
(Vote-c)^3	6.168	2.192	8.029	0.006	8.390	12.318
	(1.123)	(0.353)	(1.336)	(0.001)	(0.790)	(1.128)
Union×(Vote-c)	1.366	3.013**	0.452	0.361	2.338	-0.145
	(1.054)	(2.158)	(0.325)	(0.152)	(0.914)	(-0.058)
Union×(Vote-c)^2	-14.183***	-14.588***	-10.316	-3.593	-18.870	-8.563
	(-2.209)	(-2.082)	(-1.474)	(-0.301)	(-1.491)	(-0.667)
Union×(Vote-c)^3	7.023	14.224	-2.315	2.318	6.714	-12.989
	(0.814)	(1.462)	(-0.249)	(0.137)	(0.388)	(-0.747)
Observations	1,234	1,234	1,234	1,234	1,234	1,234
R-squared	0.010	0.013	0.010	0.008	0.009	0.006

Panel B: Payout ratios after the elections (polynomial order of two)

	Dividend Payout$_{t+N}$			*Total Payout*$_{t+N}$		
	N=1	N=2	N=3	N=1	N=2	N=3
Union	-0.090*	-0.119**	-0.112**	-0.221**	-0.221**	-0.128
	(-1.864)	(-2.404)	(-2.128)	(-2.504)	(-2.279)	(-1.309)
Vote-c	0.067	-0.121	0.030	0.625	0.123	-0.333
	(0.204)	(-0.315)	(0.083)	(1.053)	(0.181)	(-0.501)
(Vote-c)^2	-0.017	-0.499	-0.250	1.185	-0.252	-0.893
	(-0.023)	(-0.572)	(-0.308)	(0.836)	(-0.169)	(-0.586)
Union×(Vote-c)	0.275	0.997*	0.586	0.042	1.235	1.364
	(0.510)	(1.777)	(1.030)	(0.043)	(1.214)	(1.328)
Union×(Vote-c)^2	-0.292	-0.900	-0.786	-1.861	-2.065	-1.000
	(-0.265)	(-0.751)	(-0.696)	(-0.910)	(-1.009)	(-0.481)
Observations	1,234	1,234	1,234	1,234	1,234	1,234
R-squared	0.006	0.009	0.008	0.008	0.007	0.005

Panel C: Payout ratios after the elections (polynomial order of four)

	Dividend Payout$_{t+N}$			*Total Payout*$_{t+N}$		
	N=1	N=2	N=3	N=1	N=2	N=3
Union	-0.174**	-0.282***	-0.197**	-0.163	-0.477***	-0.261*
	(-2.038)	(-3.589)	(-2.192)	(-1.107)	(-3.138)	(-1.704)
Vote-c	3.125**	3.598**	2.609	1.003	4.969*	1.975
	(2.221)	(2.338)	(1.597)	(0.436)	(1.671)	(0.688)
(Vote-c)^2	26.694**	34.491***	21.303	4.846	42.519*	15.913
	(2.275)	(2.652)	(1.571)	(0.237)	(1.710)	(0.644)
(Vote-c)^3	82.957**	116.242**	64.045	12.466	134.565*	42.320
	(2.235)	(2.801)	(1.499)	(0.186)	(1.710)	(0.537)
(Vote-c)^4	83.294**	124.179**	61.571	13.596	137.025*	32.955
	(2.147)	(2.839)	(1.382)	(0.190)	(1.663)	(0.401)
Union×(Vote-c)	-3.955	-0.972	-2.025	-3.599	0.602	1.703
	(-1.591)	(-0.397)	(-0.788)	(-0.797)	(0.128)	(0.373)
Union×(Vote-c)^2	-7.657	-43.035**	-18.567	27.725	-76.908*	-44.155
	(-0.368)	(-2.074)	(-0.853)	(0.710)	(-1.893)	(-1.082)
Union×(Vote-c)^3	-	-116.540*	-84.525	-124.317	-51.130	44.769
	(-2.588)	(-1.730)	(-1.250)	(-1.025)	(-0.389)	(-0.345)
Union×(Vote-c)^4	13.883	-107.047	-34.832	103.005	-207.149	-122.640
	(0.217)	(-1.555)	(-0.499)	(0.824)	(-1.535)	(-0.904)
Observations	1,234	1,234	1,234	1,234	1,234	1,234
R-squared	0.016	0.019	0.011	0.009	0.011	0.007

Panel D: Payout ratios after the elections (polynomial order of three) with controls

	Dividend Payout$_{t+N}$			*Total Payout*$_{t+N}$		
	N=1	N=2	N=3	N=1	N=2	N=3
Union	-0.150***	-0.121*	-0.078*	-0.321**	-0.250*	-0.163
	(-2.715)	(-1.836)	(-1.657)	(-2.386)	(-1.702)	(-0.998)
Vote-c	-0.302	-0.141	-0.132	2.059	0.636	1.919
	(-0.400)	(-0.150)	(-0.211)	(1.340)	(0.342)	(0.957)
(Vote-c)^2	-3.068	-1.789	-0.331	5.742	1.188	9.138
	(-0.790)	(-0.378)	(-0.106)	(0.732)	(0.130)	(0.903)
(Vote-c)^3	-6.054	-4.141	-0.125	2.175	-1.015	11.322
	(-1.046)	(-0.594)	(-0.028)	(0.191)	(-0.079)	(0.770)
Union×(Vote-c)	3.028**	2.429*	2.095**	-0.091	1.986	-0.166
	(2.557)	(1.656)	(2.009)	(-0.036)	(0.658)	(-0.054)
Union×(Vote-c)^2	-10.057*	-7.563	-8.312	-16.033	-9.847	-16.690
	(-1.685)	(-1.108)	(-1.577)	(-1.293)	(-0.698)	(-1.040)
Union×(Vote-c)^3	23.298***	14.908	10.534	12.505	8.991	-2.760
	(2.744)	(1.434)	(1.443)	(0.726)	(0.446)	(-0.130)
Ln Assets	0.021***	0.018*	0.027***	0.072***	0.076***	0.084***
	(2.777)	(1.930)	(4.017)	(4.252)	(3.696)	(4.263)
ROA	0.505***	0.469***	0.504***	0.965***	0.455	0.496
	(3.058)	(2.727)	(3.333)	(3.146)	(1.036)	(1.258)
Leverage	-0.322***	-0.292***	-0.208***	-0.558***	-0.513***	-0.419***
	(-6.516)	(-5.184)	(-3.989)	(-4.923)	(-3.714)	(-3.139)
PPE/Assets	0.152***	0.202***	0.127***	0.120	0.255**	0.241**
	(3.476)	(3.829)	(3.523)	(1.491)	(2.363)	(2.221)
Capx/Assets	-0.074	-0.167	-0.136	-0.492	-0.545	-0.892**
	(-0.355)	(-0.657)	(-0.834)	(-1.312)	(-1.031)	(-2.307)
Tobin's Q	-0.019	-0.031**	-0.012	0.001	0.043	0.071
	(-1.341)	(-2.155)	(-0.762)	(0.019)	(1.099)	(1.619)
Cash/Assets	-0.016	0.107	0.114	0.146	0.179	-0.041
	(-0.126)	(0.911)	(1.083)	(0.583)	(0.628)	(-0.154)
Institutional Own	-0.114***	-0.081	-0.103***	-0.077	0.089	-0.045
	(-2.725)	(-1.644)	(-2.807)	(-0.767)	(0.781)	(-0.417)
Cashflow Vol	-0.587**	-0.514	-0.404*	-0.449	0.114	-0.310
	(-2.182)	(-1.619)	(-1.779)	(-0.691)	(0.154)	(-0.511)
Log Age	0.004***	0.003***	0.002***	0.004**	0.003*	0.000
	(5.727)	(3.720)	(3.709)	(2.173)	(1.650)	(0.216)
Sales Grow	-0.054	0.001	-0.016	-0.196**	-0.109	-0.113
	(-1.216)	(0.015)	(-0.394)	(-2.027)	(-0.886)	(-0.966)
State GDP Grow	0.369	0.373	0.442	0.124	0.239	0.268
	(1.136)	(1.074)	(1.616)	(0.196)	(0.297)	(0.345)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	541	516	489	541	516	489
R-squared	0.475	0.402	0.483	0.378	0.298	0.343

Table 2.4: Regression discontinuity: Nonparametric local linear regression

This table presents local linear regression results using the optimal bandwidth following Imbens and Kalyanaraman (2012). The dependent variables are *Dividend payout* (left) and *Total Payout* (right) N year(s) after the election (N=1, 2, or 3). Triangular kernels and rectangular kernels are used in Panel A and Panel B, respectively. In Panel C, the dependent variables are other firm characteristics as of the predetermined year (the year before the reported closing date of the union elections). Union election results are from the NLRB over 1980 to 2011. Payout data are from the Compustat. Z-statistics are displayed in the parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Panel A: Payout ratios after the elections (triangular kernels)

	Dividend Payout$_{t+N}$			*Total Payout*$_{t+N}$		
	N=1	N=2	N=3	N=1	N=2	N=3
Union	-0.101***	-0.192***	-0.121**	-0.193***	-0.311***	-0.142*
	(-2.647)	(-4.067)	(-2.434)	(-2.649)	(-3.499)	(-1.821)

Panel B: Payout ratios after the elections (rectangular kernels)

	Dividend Payout$_{t+N}$			*Total Payout*$_{t+N}$		
	N=1	N=2	N=3	N=1	N=2	N=3
Union	-0.095**	-0.192***	-0.128**	-0.200***	-0.295***	-0.151*
	(-2.451)	(-3.880)	(-2.542)	(-2.696)	(-3.176)	(-1.860)

Panel C: Other firm characteristics at the predetermined year

Variable	Coefficient	Z-statistic
Dividend Payout	-0.039	-1.254
Total Payout	-0.042	-0.536
Assets	-0.270	-0.566
Market Value	-0.492	-1.403
ROA	-0.002	-0.211
Leverage	0.037	1.233
PPE/Assets	0.000	-0.005
Capx/Assets	0.001	0.207
Cash/Assets	-0.007	-0.829
TobinQ	-0.111	-1.329
Age	-2.617	-1.119
Cashflow Volatility	0.006	1.471
Institutional Ownership	-0.052	-1.455
Sales Growth	-0.022	-0.948
State GDP Growth	-0.002	-0.644

Table 2.5: Robustness tests for local linear regressions

This table presents robustness tests on alternative payout measures using local linear regressions with the optimal bandwidth following Imbens and Kalyanaraman (2012). Results using triangular kernels are reported. Row (1) examines *Dividend Yield* (left) and *Total Yield* (right). Row (2) examines *Dividend Sales Ratio* and *Total Payout Sales Ratio*. Row (3) examines *Dividend over Free Cash Flows* and *Total Payout over Free Cash Flows*. Row (4) examines *Dividend Per Share* and *Total Payout Per Share*. Row (5) examines *Dividend Assets Ratio* and *Total Payout Assets Ratio*. Row (6) examines *Ln Dividend* and *Ln Total Payout*. Union election results are from the NLRB over 1980 to 2011. Payout data are from the Compustat. Z-statistics are displayed in the parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

(1)	*Dividend Yield*$_{t+N}$			*Total Yield*$_{t+N}$		
	N=1	N=2	N=3	N=1	N=2	N=3
Union	-0.009***	-0.008***	-0.006***	-0.014***	-0.011**	-0.009**
	(-3.350)	(-2.926)	(-2.647)	(-3.087)	(-2.285)	(-1.970)
(2)	*Dividend Sales Ratio*$_{t+N}$			*Total Payout Sales Ratio*$_{t+N}$		
	N=1	N=2	N=3	N=1	N=2	N=3
Union	-0.004***	-0.004***	-0.004*	-0.008***	-0.006*	-0.004
	(-2.608)	(-2.741)	(-1.765)	(-2.706)	(-1.753)	(-1.017)
(3)	*Dividend over Free Cash Flow*$_{t+N}$			*Total Payout over Free Cash Flow*$_{t+N}$		
	N=1	N=2	N=3	N=1	N=2	N=3
Union	-0.037	-0.144**	-0.093*	-0.050	-0.172*	-0.151*
	(-0.827)	(-2.324)	(-1.711)	(-0.640)	(-1.888)	(-1.729)
(4)	*Dividend Per Share*$_{t+N}$			*Total Payout Per Share*$_{t+N}$		
	N=1	N=2	N=3	N=1	N=2	N=3
Union	-0.164***	-0.178**	-0.150*	-0.373***	-0.314**	-0.104
	(-2.630)	(-2.380)	(-1.844)	(-3.497)	(-2.168)	(-0.544)
(5)	*Dividend Assets Ratio*$_{t+N}$			*Total Payout Assets Ratio*$_{t+N}$		
	N=1	N=2	N=3	N=1	N=2	N=3
Union	-0.006***	-0.006**	-0.004**	-0.009***	-0.006*	-0.003
	(-2.598)	(-2.479)	(-1.976)	(-2.973)	(-1.864)	(-0.674)
(6)	*Ln Dividend*$_{t+N}$			*Ln Total Payout*$_{t+N}$		
	N=1	N=2	N=3	N=1	N=2	N=3
Union	-0.722**	-0.732***	-0.562**	-0.939***	-1.018***	-0.477
	(-2.413)	(-3.046)	(-1.965)	(-3.444)	(-2.923)	(-1.267)

Table 2.6: Subsample analysis based on operational leverage

Panels A and B of this table present local linear regression results using the optimal bandwidth following Imbens and Kalyanaraman (2012) for firms with high *ex ante* operational leverage versus firms with *ex ante* low operational leverage. We follow Mandelker and Rhee (1984) and estimate a firm's operational leverage as the elasticity of its earnings before interest and taxes (EBIT) with respect to its sales, using the twelve most recent quarterly observations up to the predetermined year. We then split the firms into two subsamples based on the sample median operational leverage. Results using triangular kernels are reported. The dependent variables are *Dividend Payout* (left) and *Total Payout* (right). Union election results are from the NLRB over 1980 to 2011. Payout data are from the Compustat. Z-statistics are displayed in the parentheses of Panels A and B. Panels C and D present global RD results from estimating a polynomial model (with order three) specified in Equation (1). To save space, the coefficients for polynomial variables are omitted. T-statistics adjusted for heteroskedasticity and within-firm clustering are displayed in the parentheses of Panels C and D. The p-values reported at the bottom of the table are for one-tailed Chi-squared tests testing that the coefficients before *Union* in Panel C are smaller than those in Panel D. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Panel A: Local RD for high operational leverage

	Dividend Payout$_{t+N}$			*Total Payout*$_{t+N}$		
	N=1	N=2	N=3	N=1	N=2	N=3
Union	-0.167***	-0.313***	-0.205**	-0.344***	-0.415***	-0.212*
	(-3.040)	(-3.977)	(-2.375)	(-2.937)	(-2.830)	(-1.725)

Panel B: Local RD for low operational leverage

	Dividend Payout$_{t+N}$			*Total Payout*$_{t+N}$		
	N=1	N=2	N=3	N=1	N=2	N=3
Union	-0.081	-0.123	-0.051	-0.111	-0.240	0.019
	(-1.059)	(-1.492)	(-0.601)	(-1.027)	(-1.495)	(0.125)

Panel C: Global RD for high operational leverage

	Dividend Payout$_{t+N}$			*Total Payout*$_{t+N}$		
	N=1	N=2	N=3	N=1	N=2	N=3
Union	-0.359***	-0.338***	-0.345***	-0.508***	-0.442**	-0.420**
	(-4.023)	(-3.183)	(-2.750)	(-2.757)	(-2.104)	(-1.970)
Cubic Polynomials	Yes	Yes	Yes	Yes	Yes	Yes
Observations	388	388	388	388	388	388
R-squared	0.058	0.032	0.048	0.030	0.040	0.016

Panel D: Global RD for low operational leverage

	Dividend Payout$_{t+N}$			*Total Payout*$_{t+N}$		
	N=1	N=2	N=3	N=1	N=2	N=3
Union	-0.170	-0.083	-0.180	-0.070	0.169	-0.361
	(-1.504)	(-0.676)	(-1.215)	(-0.340)	(0.717)	(-1.432)
Cubic Polynomials	Yes	Yes	Yes	Yes	Yes	Yes
Observations	388	388	388	388	388	388
R-squared	0.021	0.012	0.010	0.008	0.011	0.009

Chi-squared tests of the coefficients before *Union* in Panels C and D

P-Value	0.089*	0.063*	0.188	0.043**	0.019**	0.428

Table 2.7: Nonparametric local linear regression on cumulative abnormal returns around union elections

This table presents local linear regression results using the optimal bandwidth following Imbens and Kalyanaraman (2012) on the cumulative abnormal returns (CARs) around union elections' closing dates. Results using triangular kernels are reported. The dependent variables are CARs calculated from the market model using the union elections' closing dates as the event dates. We examine event windows from day -3 to day +3, from day -1 to day +3, and from day 0 to day +5. Panel A examines the CARs using all the elections. Panels B1 and B2 examine the CARs for subsamples separated by the level of operational leverage. Union election results are from NLRB over 1980 to 2011. Stock returns are from CRSP. Z-statistics are presented in the parenthesis. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Panel A: All elections

	CARs		
	[-3, 3]	[-1, 3]	[0, 5]
Union	-0.002	-0.015	-0.012
	(-0.075)	(-1.236)	(-1.086)

Panel B1: High operational leverage

	CARs		
	[-3, 3]	[-1, 3]	[0, 5]
Union	-0.051**	-0.038**	-0.023*
	(-2.055)	(-2.138)	(-1.714)

Panel B2: Low operational leverage

	CARs		
	[-3, 3]	[-1, 3]	[0, 5]
Union	0.026	-0.004	0.010
	(0.709)	(-0.132)	(0.349)

Table 2.8: Subsample analysis based on financial constraints

This table presents local linear regression results using the optimal bandwidth following Imbens and Kalyanaraman (2012) for firms that are more financially constrained (Panels A1, B1, C1 and D1) versus firms that are less financially constrained (Panels A2, B2, C2, and D2). Panels A1 and A2 use the Kaplan-Zingales (KZ) index to proxy for a firm's financial constraints. Firms with KZ index below sample median are less constrained, and firms with KZ index above sample median are more constrained. Panels B1 and B2 use the Whited-Wu (WW) index, Panels C1 and C2 use the Hadlock and Pierce (SA) index, and Panels D1 and D2 use the Hoberg-Maksimovic (HM) text-based index. Results using triangular kernels are reported. The dependent variables are *Dividend Payout* (left) and *Total Payout* (right). Union election results are from NLRB over 1980 to 2011. Payout data are from the Compustat. Z-statistics are displayed in the parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Panel A1: More financially constrained (the KZ Index)

	Dividend Payout$_{t+N}$			*Total Payout*$_{t+N}$		
	N=1	N=2	N=3	N=1	N=2	N=3
Union	-0.147***	-0.180**	-0.120**	-0.275***	-0.448***	-0.174*
	(-3.036)	(-2.536)	(-2.160)	(-3.247)	(-3.706)	(-1.687)

Panel A2: Less financially constrained (the KZ Index)

	Dividend Payout$_{t+N}$			*Total Payout*$_{t+N}$		
	N=1	N=2	N=3	N=1	N=2	N=3
Union	-0.049	-0.103*	-0.011	-0.075	0.062	0.009
	(-0.678)	(-1.673)	(-0.188)	(-0.661)	(0.437)	(0.074)

Panel B1: More financially constrained (the WW Index)

	Dividend Payout$_{t+N}$			*Total Payout*$_{t+N}$		
	N=1	N=2	N=3	N=1	N=2	N=3
Union	-0.153*	-0.184**	-0.246**	-0.047	-0.176	-0.128
	(-1.869)	(-2.062)	(-2.532)	(-0.393)	(-1.243)	(-1.156)

Panel B2: Less financially constrained (the WW Index)

	Dividend Payout$_{t+N}$			*Total Payout*$_{t+N}$		
	N=1	N=2	N=3	N=1	N=2	N=3
Union	-0.181**	-0.333***	-0.107	-0.449***	-0.665***	-0.228
	(-2.518)	(-4.168)	(-1.351)	(-3.736)	(-4.259)	(-1.473)

Panel C1: More financially constrained (the SA Index)

	Dividend Payout$_{t+N}$			*Total Payout*$_{t+N}$		
	N=1	N=2	N=3	N=1	N=2	N=3
Union	-0.095*	-0.131*	-0.139*	-0.190*	-0.168*	-0.157
	(-1.738)	(-1.956)	(-1.935)	(-1.947)	(-1.904)	(-1.263)

Panel C2: Less financially constrained (the SA Index)

	Dividend Payout$_{t+N}$			*Total Payout*$_{t+N}$		
	N=1	N=2	N=3	N=1	N=2	N=3
Union	-0.132	-0.208***	-0.070	-0.182	-0.483***	-0.141
	(-1.564)	(-3.071)	(-0.898)	(-1.574)	(-2.895)	(-1.047)

Panel D1: More financially constrained (the HM Index)

	Dividend Payout$_{t+N}$			*Total Payout*$_{t+N}$		
	N=1	N=2	N=3	N=1	N=2	N=3
Union	0.107	-0.046	0.215	0.092	-0.310*	0.544*
	(0.872)	(-0.384)	(1.098)	(0.310)	(-1.654)	(1.757)

Panel D2: Less financially constrained (the HM Index)

	Dividend Payout$_{t+N}$			*Total Payout*$_{t+N}$		
	N=1	N=2	N=3	N=1	N=2	N=3
Union	-0.306**	-0.309**	-0.118	-0.623***	-0.811***	-0.225
	(-2.372)	(-2.217)	(-1.353)	(-2.796)	(-3.094)	(-1.315)

Table 2.9: Nonparametric local linear regression of other financial policies

This table presents local linear regression results for other financial policies. Panel A examines a firm's capital structure decisions, measured by *Δ Total Debt over Earnings*. Panel B examines a firm's cash holding decisions, measured by *Δ Cash over Earnings*. Panel C examines a firm's working capital management, measured by *Δ Working Capital over Earnings.* Results using the optimal bandwidth following Imbens and Kalyanaraman (2012) and triangular kernels are reported. Union election results are from the NLRB over 1980 to 2011. Payout data are from the Compustat. Z-statistics are displayed in the parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Panel A: Change in total debt

	Δ Total Debt over Earnings$_{t+N}$		
	N=1	N=2	N=3
Union	0.279	-0.052	-0.210
	(1.096)	(-0.184)	(-0.518)

Panel B: Change in cash holdings

	Δ Cash over Earnings$_{t+N}$		
	N=1	N=2	N=3
Union	0.216	-0.177	0.085
	(1.372)	(-1.309)	(0.630)

Panel C: Change in working capital

	Δ Working Capital over Earnings$_{t+N}$		
	N=1	N=2	N=3
Union	0.397*	0.605**	0.253
	(1.659)	(2.074)	(1.232)

Table 2.10: Subsample analysis of alternative explanations: Employee preferences

This table presents local linear regression results on subsamples based on the fraction of firm equity owned by pensions and the percentage of pension holdings invested in the firm's own securities. Panel A examines firms with larger pension ownership (i.e., those whose equity market value held by employees through pension plans exceeds sample median), and Panel B examines those with smaller pension ownership. Panel C examines firms with a larger fraction of pension holdings invested in the firms' own securities (i.e., those whose percentage pension holdings in the form of their own securities are above the sample median), and Panel D examines those with a smaller percentage of pension holdings invested in the firms' own securities. Results using the optimal bandwidth following Imbens and Kalyanaraman (2012) and triangular kernels are reported. The dependent variables are *Dividend Payout* (left) and *Total Payout* (right). We use union election results from the NLRB over 1992 to 2007. Payout data are from the Compustat. Z-statistics are displayed in the parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Panel A: Larger pension ownership

	Dividend Payout$_{t+N}$			*Total Payout*$_{t+N}$		
	N=1	N=2	N=3	N=1	N=2	N=3
Union	-0.119*	-0.309**	-0.057	-0.471**	-0.525**	0.066
	(-1.684)	(-2.477)	(-0.743)	(-2.473)	(-1.999)	(0.364)

Panel B: Smaller pension ownership

	Dividend Payout$_{t+N}$			*Total Payout*$_{t+N}$		
	N=1	N=2	N=3	N=1	N=2	N=3
Union	-0.055	-0.179	0.231	-0.228	-0.274	0.350
	(-0.477)	(-1.311)	(1.433)	(-1.202)	(-0.857)	(1.558)

Panel C: Larger fraction of pension holdings invested in the firm's own securities

	Dividend Payout$_{t+N}$			*Total Payout*$_{t+N}$		
	N=1	N=2	N=3	N=1	N=2	N=3
Union	-0.042	-0.282**	-0.097	-0.258	-0.742***	0.082
	(-0.504)	(-2.321)	(-0.821)	(-1.025)	(-2.802)	(0.355)

Panel D: Smaller fraction of pension holdings invested in the firm's own securities

	Dividend Payout$_{t+N}$			*Total Payout*$_{t+N}$		
	N=1	N=2	N=3	N=1	N=2	N=3
Union	-0.218	-0.282*	0.046	-0.492**	-0.247	0.084
	(-1.369)	(-1.904)	(0.256)	(-2.067)	(-0.718)	(0.362)

Table 2.11: Subsample analysis of alternative explanations: Agency conflicts

This table presents local linear regression results on subsamples based on the level of corporate governance measured by the entrenchment index (E-index). Panel A examines firms with an E-index that is below the sample median (i.e., with better corporate governance) and Panel B examines those with an E-index above sample median (i.e., with worse corporate governance). Results using the optimal bandwidth following Imbens and Kalyanaraman (2012) and triangular kernels are reported. The dependent variables are *Dividend Payout* (left) and *Total Payout* (right). We use union elections over 1990 to 2006. Payout data are from the Compustat. Z-statistics are displayed in the parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Panel A: Better corporate governance

	Dividend Payout$_{t+N}$			*Total Payout*$_{t+N}$		
	N=1	N=2	N=3	N=1	N=2	N=3
Union	-0.339***	-0.394***	-0.383***	-0.375**	-0.590***	-0.399***
	(-4.665)	(-5.516)	(-5.033)	(-2.357)	(-3.963)	(-2.923)

Panel B: Worse corporate governance

	Dividend Payout$_{t+N}$			*Total Payout*$_{t+N}$		
	N=1	N=2	N=3	N=1	N=2	N=3
Union	-0.101	-0.026	-0.026	-0.222	-0.036	-0.094
	(-0.866)	(-0.253)	(-0.382)	(-1.111)	(-0.169)	(-0.564)

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APPENDIX A

Definition of Variables in Chapter 1

Variable	**Definition**
LnNPat	Natural logarithm of one plus firm *i*'s total number of patents filed and eventually granted in fiscal year *t*.
LnNPatRelated	Natural logarithm of one plus firm *i*'s total number of patents filed and eventually granted in fiscal year *t* that are related to the firm's core businesses. See Hsu, Tian and Xu (2014) for more details.
LnNPatUnRelated	Natural logarithm of one plus firm *i*'s total number of patents filed and eventually granted in fiscal year *t* that are unrelated to the firm's core businesses. See Hsu, Tian and Xu (2014) for more details.
LnPatValue	Natural logarithm of one plus firm *i*'s average market value of patents filed and eventually granted in year *t*, where the market value of each patent is provided by Kogan, Papanikolaou and Seru (2016) and defined as the market capitalization of firm *i* times the three-day market-adjusted cumulative abnormal return (CAR) around the issuance/grant date of the patent.
Dual	Indicator variable that is equal to one if at least one participating lender on average holds a significant amount of equity in the borrower in year *t*. Similar to Jiang, Li, and Shao (2010), the significant equity holding is defined as either exceeding 1% of the borrower's outstanding shares or over the value of $2 million in 2009 constant dollars.
LnNDual	Natural logarithm of one plus the total number of unique holders holding firm *i*'s loan and equity in fiscal year *t*.
LnAvgOwn	Natural logarithm of one plus the average percentage of firm *i*'s equity held by each lending institution in fiscal year *t*.
LnTotalOwn	Natural logarithm of one plus the average percentage of the total dual-holders' equity holdings in firm *i* in the fiscal year *t*.
Loan	Indicator variable that is equal to one if the firm has an outstanding syndicated loan and zero otherwise in year *t*.
Assets	Book value of total assets (in billions) measured at the end of fiscal year *t* (item #6).
Age	Firm *i*' age, approximated by the number of years listed on Compustat.
R&DAsset	Research and development(R&D) expenditure (#46) divided by book value of total assets (#6) measured at the end of fiscal year *t*.
ROA	Return on assets defined as operating income before depreciation (#13) divided by book value of total assets (#6) measured at the end of fiscal year *t*.
Leverage	Firm *i*'s leverage ratio, defined as book value of long-term debt (#9) divided by book value of total assets (#6), measured at the end of fiscal

	year *t*.
PPE/Assets	Property, plant & equipment (#8) divided by book value of assets (#6) measured at the end of fiscal year *t*.
Capx/Assets	Capital expenditures (#128) divided by book value of total assets (#6) measured at the end of fiscal year *t*.
TobinQ	Firm *i*'s market to book ratio, defined as market value of equity plus book value of assets minus book value of equity minus deferred taxes (set to zero if missing) divided by book value of assets, measured at the end of fiscal year *t*.
SAindex	Following Hadlock and Pierce (2010), firm *i*'s SA index measured at the end of fiscal year *t*, calculated as (-0.737×*Size*) + (0.043×$Size^2$)-0.040**Age*, where *Size* is the log of inflation-adjusted book assets, and *Age* is the number of years firm *i* is listed with a non-missing stock price on Compustat. To calculate this index, *Size* is capped at (the log of) $4.5 billion, and *Age* is capped at 37 years.
Acquisition	Acquisitions (#129) divided by book value of total assets (#6) measured at the end of fiscal year *t*.
Block	Indicator variable that is equal to one if the firm has at least one block holder defined as holding more than 5% equity stake and zero otherwise in year *t*.
InstOwn	Institutional holdings (percent) for firm i over fiscal year t, calculated as the arithmetic mean of the four quarterly institutional holdings reported through form 13F.
Hindex	Herfindahl index of two-digit standard industrial classification (SIC) industry *j* where firm *i* belongs, measured at the end of fiscal year *t*.
Altman's Z-score	Altman’s *Z*-score model for predicting bankruptcies is: 1.2 × (current asset (#4)-current liability (#5))/assets (#6) + 1.4 × (Retained earnings (#36) / assets (#6) + 3.3 × (net income (#172) + interest expense (#15) + income taxes (#16))/ assets (#6) + 0.6 × (common shares outstanding (#25) × price (#199))/Book value of total liabilities(#181)) + 0.999 × (Sales (#1/Total assets).
Vega	Manager's change in wealth (expressed in thousands of dollars) for a 0.01 increase in the annualized standard deviation of firm's stock returns. Calculated from managers' complete portfolio of stock and options from Execucomp using the methodology from Core and Guay (2002). The value of the option portfolio is the sum of the Black–Scholes values of the newly granted options and previously granted unexercisable and exercisable options.
Vega Flow	Same as *vega* but calculated using only options granted in given year *t*.
Cash Compensation	Sum of salary and bonus.
Tenure	Time between fiscal year-end and the day executive became chief executive officer.

APPENDIX B

Definition of Variables in Chapter 2

Variable	**Definition**
Measures of payout policy	
Dividend *Payout*	Dividends (Compustat *data* #21) divided by income before extraordinary items (#237) measured at the end of fiscal year *t*.
Total Payout	Sum of Dividends (#21) and repurchases divided by income before extraordinary items (#237) measured at the end of fiscal year *t*. Repurchases are measured as expenditures on the purchase of common and preferred stocks (#115) minus any reduction in the redemption value of the net number of preferred shares outstanding (#56).
Dividend *Yield*	Dividends (#21) divided by market value of equity (#199×#25) measured at the end of fiscal year *t*.
Total Yield	Sum of Dividends (#21) and repurchase divided by market value of equity (#199×#25) measured at the end of fiscal year *t*. Repurchases are measured as expenditures on the purchase of common and preferred stocks (#115) minus any reduction in the redemption value of the net number of preferred shares outstanding (#56).
Dividend *Sales Ratio*	Dividends (#21) divided by sales (#12) measured at the end of fiscal year *t*.
Total Payout Sales Ratio	Sum of Dividends (#21) and repurchases divided by sales (#12) measured at the end of fiscal year *t*. Repurchases are measured as expenditures on the purchase of common and preferred stocks (#115) minus any reduction in the redemption value of the net number of preferred shares outstanding (#56).
Dividend *Per Share*	Dividends (#21) divided by common shares outstanding (#25) measured at the end of fiscal year *t*.
Total Payout Per Share	Sum of Dividends (#21) and repurchases divided by common shares outstanding (#25) *measured* at the end of fiscal year *t*. Repurchases are measured as expenditures on the purchase of common and preferred stocks (#115) minus any reduction in the redemption value of the net number of preferred shares outstanding (#56).
Dividend *Assets Ratio*	Dividends (#21) divided by book value of total assets measured at the end of fiscal year *t* (#6).
Total Payout *Assets Ratio*	Sum of Dividends (#21) and repurchases divided by book value of total assets measured at the end of fiscal year *t* (#6). Repurchases are measured as expenditures on the purchase of common and preferred stocks (#115) minus any reduction in the redemption value of the net number of preferred shares outstanding (#56).

Ln Dividend	Natural logarithm of one plus the dividends (#21) measured at the end of fiscal year *t*.
Ln Total Payout	Natural logarithm of one plus the sum of dividends (#21) and repurchases measured at the end of fiscal year *t*. Repurchases are measured as expenditures on the purchase of common and preferred stocks (#115) minus any reduction in the redemption value of the net number of preferred shares outstanding (#56).
Measures of firm characteristics and other variables	
Assets	Book value of total assets measured at the end of fiscal year *t* (#6).
Market Value	Market value of equity measured at the end of fiscal year *t*, calculated as #199×#25.
ROA	Return on assets defined as operating income before depreciation (#13) divided by book value of total assets (#6) measured at the end of fiscal year
Leverage	Firm *i*'s leverage ratio, defined as book value of long-term debt (#9) divided by book value of total assets (#6), measured at the end of fiscal year *t*.
PPE/Assets	Property, plant & equipment (#8) divided by book value of assets (#6) measured at the end of fiscal year *t*.
Cash/Assets	Cash and short-term investments (#1) divided by book value of total assets (#6), measured at the end of fiscal year *t*.
Capx/Assets	Capital expenditures (#128) divided by book value of total assets (#6) measured at the end of fiscal year *t*.
TobinQ	Firm *i*'s market to book ratio, defined as market value of equity (#199×#25) plus book value of assets (#6) minus book value of equity (#60) minus deferred taxes (#74) (set to zero if missing) divided by book value of assets, measured at the end of fiscal year *t*.
Age	Firm *i*'s age, defined as the number of years it has been listed on Compustat.
Cashflow Volatility	The standard deviation of firm *i*'s cash-flow-over-asset ratios using the most recent twelve quarterly observations before the end of fiscal year *t*. Cash flows are *calculated* as operating income before depreciation (#13) minus interest (#15) minus taxes (#16) minus changes in (current assets (#4) minus current liabilities (#5)).
Institutional Ownership	Institutional holdings for firm *i* over fiscal year *t*, calculated as the arithmetic mean of the four quarterly institutional holdings reported by 13F filings.
Sales Growth	The logarithm of firm *i*'s sales (#12) measured at the end of fiscal year *t* minus the logarithm of its sales at the end of fiscal year *t-1*.
State GDP Growth	The GDP growth rate for the state where firm *i*'s headquarters is located, calculated as the logarithm of the state's GDP measured at the end of year *t* minus the logarithm of its GDP at the end of year *t-1*. State GDP data are obtained from the *Bureau of Economic Analysis* (www.bea.gov).

Operational Leverage	The elasticity of firm *i*'s earnings before interest and taxes (#178) with respect to its sales (#12) using the most recent twelve quarterly observations before the end of fiscal year *t*.
KZ Index	Firm *i*'s Kaplan and Zingales index measured at the end of fiscal year *t*, calculated as –1.002×cash flow[(#18+#14)/#8]+0.283×Q[(#6+#199×#25-#60-#74)/#6]+3.139×leverage[(#9+#340)/(#9+#34+#216)]-39.367×dividends[(#21+#19)/#8]–1.315×cash holding[(#1/#8)], where #8 is lagged.
WW Index	Firm *i*'s Whited and Wu index measured at the end of fiscal year *t*, calculated as –0.091×[(income before extraordinary items + depreciation and amortization) /(assets)][(#18+#14)/#6)]–0.062×[indicator set to one if the sum of common dividends (#21) and preferred dividends (#19) is positive, and zero otherwise] +0.021×leverage[(#9)/(#6)]-0.044×log(assets)[log(#6)]+0.102×average industry sales growth [estimated separately for each three-digit SIC industry and each year, with sales growth defined as above]–0.035×sales growth.
SA Index	Firm *i*'s size and age (SA) index measured at the end of fiscal year *t*, calculated as –0.737×Size+0.043×Size^2–0.040×Age, where Size equals the log of inflation-adjusted total book assets (#6) (in 2004 dollars), and Age is the number of years firm *i* has been publicly listed with non-missing stock price information (in Compustat). When calculating the index, we follow Hadlock and Pierce (2010) to cap Size at the log of $4.5 billion and cap Age at 37 years.
HM Index	Firm *i*'s Hoberg and Maksimovic index measured at the end of fiscal year *t*. This text-based financial constraint measure, explained in more details in Hoberg and Maksimovic (2015), is derived purely from a firm’s 10-K (specifically, the Management's Discussion and Analysis (MD&A) section). For each MD&A section, the Liquidity and Capital Resources subsection is extracted and processed by using the text processing software provided by meta Heuristica LLC to identify which firm-year filings contain statements suggesting that the firm may have to delay its investments due to financial constraints.
Δ Total Debt over Earnings	The change of total (short-term and long-term) debt divided by income before extraordinary items (#237) measured at the end of fiscal year *t*. The change of total debt is measured as the sum of debt in current liabilities (#34) and long-term debt (#9) at the end of fiscal year *t* minus the sum of short-term debt and long-term debt at the end of fiscal year *t-1*.

Δ Cash over Earnings	The change of cash and short-term investments divided by income before extraordinary items (#237) measured at the end of fiscal year *t*. The change of cash and short-term investments is measured as cash and short-term investments (#1) at the end of fiscal year *t* minus cash and short-term investments (#1) at the end of fiscal year *t-1*.
Δ Working Capital over Earnings	The change of working capital divided by income before extraordinary items (#237) measured at the end of fiscal year *t*. The change of working capital is measured as the difference between current assets (#4) and current liabilities (#5) at the end of fiscal year *t* minus the difference between current assets (#4) and current liabilities (#5) at the end of fiscal year *t-1*.
E-index	Entrenchment Index (E-index) data is obtained from Lucian Bebchuk's website (http://www.law.harvard.edu/faculty/bebchuk/data.shtml). It is the sum of six anti-takeover provisions including staggered boards, limits to bylaw amendments, limits to charter amendments, supermajority requirements for mergers, poison pills, and golden parachutes.

APPENDIX C

Supplemental Analyses and Robustness Tests for Chapter 2

This Appendix provides supplemental analyses and robustness tests to the main results presented in Chapter 2. Section 1 examines the graphical RD results with respect to firm characteristics at the predetermined year (the year before the reported closing date of the union elections) following the format of Figure 2.4 of the paper. Section 2 provides additional robustness tests to our main results using a variety of alternative ways of constructing the sample. Section 3 estimates a dynamic RD model with cubic polynomials in the spirit of Cunat, Gine, and Guadalupe (2012). Section 4 presents the tests exploring how cross-sectional heterogeneity in union collective bargaining power alters the main results.

1. Graphical checks for no discontinuity of other firm characteristics

In this section, we graphically examine RD results with respect to firm characteristics at the predetermined year (the year before the reported closing date of the union elections) following the format of Figure 2.4 of the paper. Figure A.1 presents regression discontinuity plots for other firm characteristics at the predetermined year using a fitted cubic polynomial estimate with a 95% confidence interval around the fitted value. The variables we examine include the dividend-to-earnings ratio (*Dividend Payout*), the total-payout-to-earnings ratio (*Total Payout*), firm size (*Assets* and *Market Value*), profitability (*ROA*), asset tangibility (*PPE/Assets*), investment (*Capx/Assets*), leverage (*Debt/Assets*), cash holdings (*Cash/Assets*), Tobin's Q (*TobinQ*), firm age (*Age*), cash flow volatility (*Cashflow Volatility*), institutional ownership (*Institutional*

Ownership), sales growth (*Sales Growth*), and GDP growth of the state where the firm's headquarter is located (*State GDP Growth*).

As we can see, none of the regression discontinuity plots for these firm characteristics show any sign of discontinuity in their respective distributions around the known threshold, which supports the validity of our RD procedure. More importantly, the predetermined values of our key dependent variables, *Dividend Payout* and *Total Payout*, do not show discontinuity around the narrow bandwidth of the cutoff point, suggesting that our main RD results are unlikely to be driven by *ex-ante* differences in payout policy between firms passing union elections and those whose elections fail.

2. Robustness checks with alternative ways of constructing the sample

We try alternative ways of constructing our sample and report the results in Table A.1. Recall that to construct our main RD sample, we retain the election with the largest number of eligible voters if there are multiple elections held by the same firm within one year. Another way of dealing with multiple elections held by the same firms is to simply keep the first one. Presented in Row (1) of Table A.1, the local linear regression results using the first elections within a year are both qualitatively and quantitatively similar to our main local RD results in Table 2.3 of the paper. Another filter we apply when constructing our main sample is that we require a firm holding an election not to have other union elections in the previous three years so as to avoid the confounding effects of such historical events. An alternative way of tackling the confounding effects of multiple elections held by the same firm is to require the firm not to have other elections both in the previous and the next three years. Although this is a more stringent filter, which reduces our sample size and thus the power of our tests, we still obtain similar (but slightly weaker) local linear RD results, which we summarize in Row (2) of Table A.1.

Row (3) adopts an even stricter data filter, requiring a firm to hold only one election throughout our sample period. Despite the fact that this filter substantially reduces our sample size, the local RD estimates continue to remain significantly negative for almost all regressions. Further, to check how our results are affected by the confounding effects of multiple elections held by the same firm, we keep all elections held by the same firm in our sample period and run the local linear regressions using this enlarged sample. The results, presented in Row (4) of Table A.1, show that unionization still has a significantly negative effect on payout policy within the three-year period after the union elections, though the economic magnitudes of the estimated RD coefficients are much smaller than those in Table 2.3 of the paper, possibly due to the confounding (or offsetting) effects of multiple elections. Row (5) of Table A.1 examines union elections with at least 100 eligible participating employees (rather than 50 such employees, as in our baseline analysis) and our local RD estimates continue to remain negative and significant for the majority of columns, despite the decreased sample size due to this stricter sample filter. Lastly, to address the concern that a sizable fraction of the firms in our sample have zero total payouts and thus may behave differently from nonzero-payout firms in response to union election outcomes, we exclude those firms that have zero total payout in the year before union elections and report the results in Row (6). Our baseline local RD results continue to hold in this subsample of firms.

3. Dynamic RD model in the spirit of Cunat, Gine, and Guadalupe (2012)

In this section, we estimate a dynamic RD model with cubic polynomials in the spirit of Cunat, Gine, and Guadalupe (2012) and report the results in Table A.2. For this analysis, we keep all elections held by the same firm in our sample period (without requiring the absence of confounding elections in the past three years). Following Cunat, Gine, and Guadalupe (2012), we sum up the union election outcomes (equals one if pass and zero if fail) and the polynomials of vote shares of all elections held by the same firm in a given year. *Unionization* is the sum of the outcomes of the elections held by a firm in a year. For each firm and each election year, we examine its dividend and payout ratios in the three years after the election year. *Dummy (N=1)* is a dummy that equals one if the observation is one year after the election year, and zero otherwise. *Dummy (N=2)* and *Dummy (N=3)* are similarly defined. Each regression includes a separate intercept and controls for calendar year fixed effects, distance (number of years) to the election year (1, 2, or 3), and firm-election year fixed effects.

As we can observe, enhanced labor power continues to exert a significantly negative effect on dividend and total payout ratios in each of the three years following the union elections, even after we control for the dynamic nature of such elections.

4. Cross-sectional heterogeneity in labor power

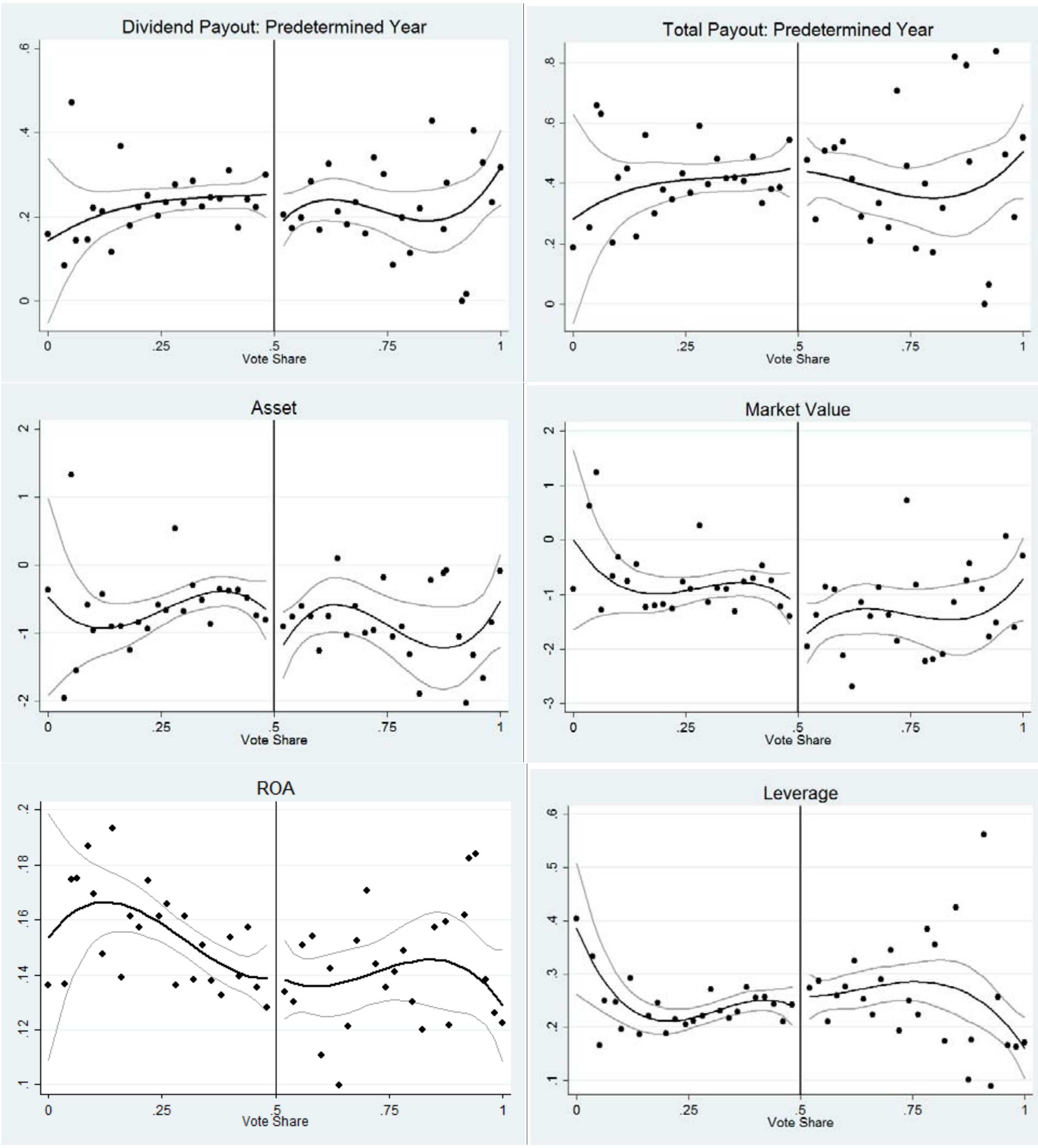
In this section, we explore how cross-sectional heterogeneity in union bargaining power alters the relation between unions and corporate payout.

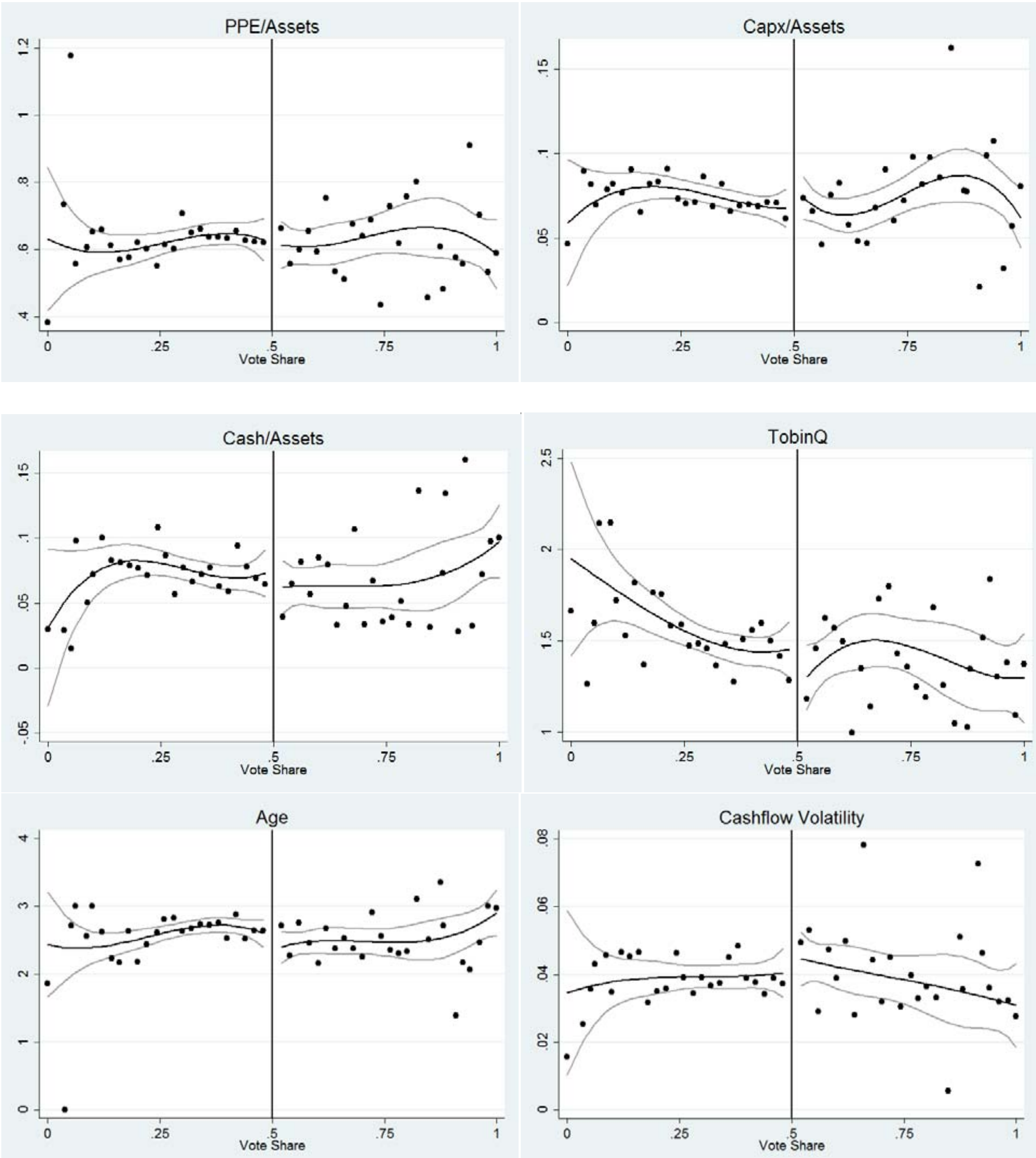
Since firms in states with right-to-work laws cannot force their employees to join the union or pay union dues as preconditions of employment, unions have significantly less bargaining power in such states than those in non-right-to-work states. As a result of the weaker union bargaining power in a right-to-work state, the passing of unionization elections is likely to have a smaller effect on payout in such a state than in states without similar legislations. By the same token, state-level work stoppage provisions, which permit strikers to collect unemployment insurance during a labor dispute if their employer continues to operate at or near normal capacity, have been shown to affect union bargaining power in a positive way because under such regulations labor strikes (often organized by unions) effectively become less costly for participating workers (see, e.g., Matsa, 2010). Consequently, the effect of unionization on corporate payout should be stronger for firms located in states that have adopted work stoppage provisions than those states that have not. We test the above conjectures in this subsection.

Table A.3 reports the local linear RD estimates for firms located in those states either without right-to-work legislations or with work stoppage provisions (the top panel) as opposed to those located in those states with right-to-work laws but without work stoppage provisions (the bottom panel).

As expected, firms winning union elections in states either without right-to-work legislation or with work stoppage provisions (which give unions more bargaining power and thus make unionizations more relevant for corporate decision making) have significantly lower payout ratios than those failing the elections, both economically and statistically, within three years after the unionization. This result confirms our main finding that enhanced labor power leads to a lower payout ratio. On the other hand, the coefficient estimates on *Unionization* for firms located in right-to-work states but without work stoppage provisions (where unions have the weakest power) are negative and statistically insignificant across all three post-election years for the two payout measures, suggesting that enhanced labor power has little effect on payout policy in the states where unions are not favored by regulations.

To compare the coefficient estimates on *Unionization* between the two subsamples, we use the global RD method with parametric estimation. Specifically, we estimate a polynomial model (with order three) as specified in Equation (1) of the paper for the two subsamples and report the results in Panels C and D of Table A.3. To save space, the coefficients for polynomial variables are omitted. As we can see, the results estimated using the global polynomial RD model are similar, both statistically and economically, to those using a local RD model. Moreover, we report at the bottom of Table A.3 the p-values for the one-tailed Chi-squared statistics testing that the coefficients before *Unionization* in Panel C are smaller (i.e., more negative) than those in Panel D. We find that the differences in the coefficient estimates on *Unionization* between the two subsamples are generally significant.


Dividend Payout: Predetermined Year
Total Payout: Predetermined Year
Asset
Market Value
ROA
Leverage
Vote Share


PPE/Assets
Capx/Assets
Cash/Assets
TobinQ
Age
Cashflow Volatility
Vote Share
0
.25
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.75
1

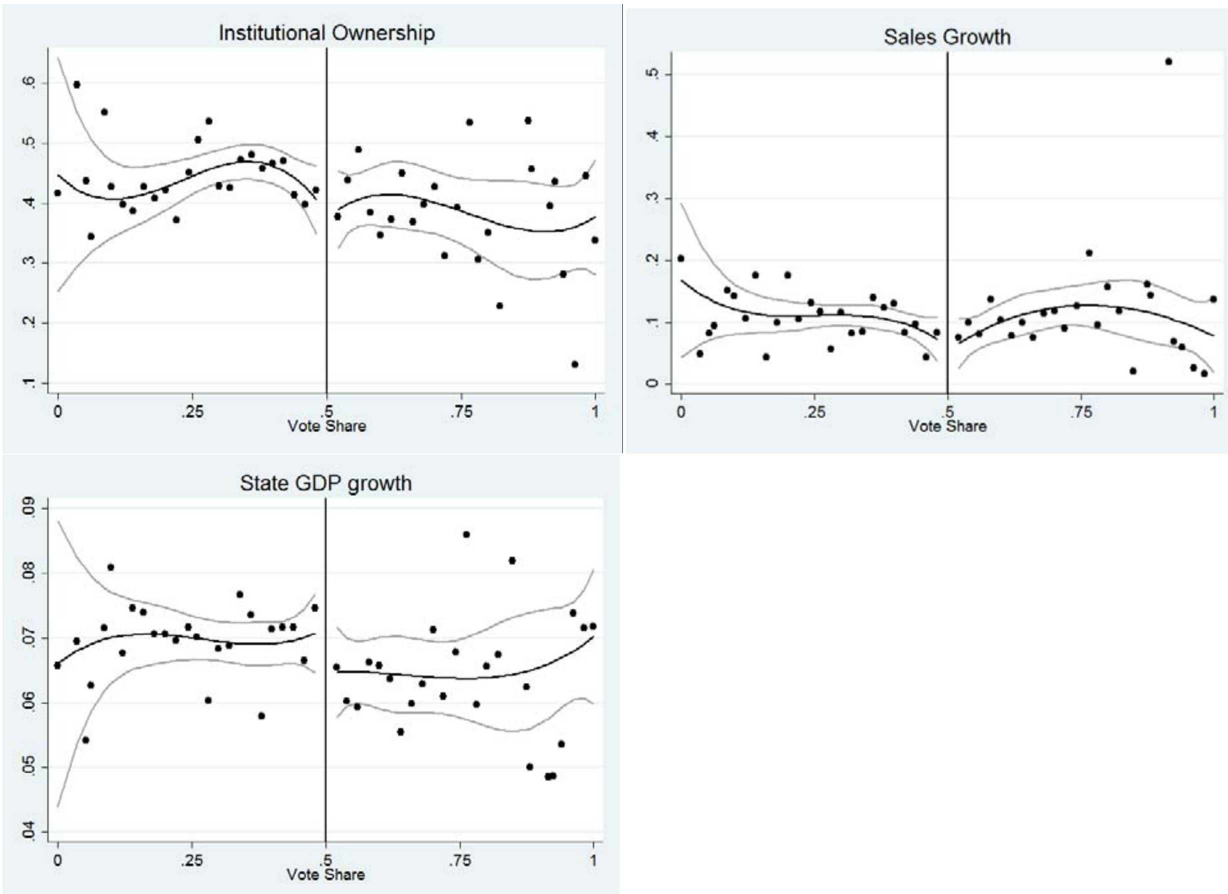

Figure A.1: Regression discontinuity plots for other firm characteristics at predetermined year
This figure presents regression discontinuity plots for other firm characteristics at the predetermined year using a fitted cubic polynomial estimate with a 95% confidence interval around the fitted value. The x-axis is union vote share (i.e., the percentage of votes in an election in favor of unionization). The dots depict the average of each variable, defined in Appendix B, in each of the 50 equally-spaced bins (with a bin width of 2%). Union election results are from the NLRB over 1980 to 2011. Data are from the Compustat.

Table A.1: Robustness tests for local linear regressions

This table presents robustness tests for local linear regressions using the optimal bandwidth following Imbens and Kalyanaraman (2012) based on alternative ways of constructing the sample. Results using triangular kernels are reported. The dependent variables are *Dividend Payout* (left) and *Total Payout* (right). Row (1) keeps the first (earliest) election held by a firm in the fiscal year. Row (2) requires a firm holding an election to have no other elections both in the previous and the next three years. Row (3) only analyzes firms holding exactly one union election throughout the whole sample period. Row (4) keeps all elections held by the same firm in our sample period. Row (5) examines union elections with more than 100 eligible employees. Row (6) examines firms with non-zero payouts before union elections. Union election results are from the NLRB over 1980 to 2011. Payout data are from the Compustat. Z-statistics are displayed in the parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

	Dividend Payout$_{t+N}$			*Total Payout*$_{t+N}$		
	N=1	N=2	N=3	N=1	N=2	N=3
(1) First election in a year						
Unionization	-0.079**	-0.185***	-0.094**	-0.184**	-0.291***	-0.146*
	(-2.503)	(-3.791)	(-2.312)	(-2.480)	(-3.326)	(-1.685)
(2) No other elections in the previous and next three years						
Unionization	-0.117**	-0.132***	-0.101**	-0.246***	-0.240**	-0.183
	(-2.217)	(-2.579)	(-1.981)	(-2.648)	(-2.350)	(-1.637)
(3) Firms with only one election throughout sample period						
Unionization	-0.173***	-0.141**	-0.192**	-0.261*	-0.326***	-0.460**
	(-2.659)	(-2.302)	(-2.397)	(-1.679)	(-2.913)	(-2.357)
(4) All elections						
Unionization	-0.062**	-0.070***	-0.048*	-0.037	-0.165***	-0.049
	(-2.134)	(-2.788)	(-1.764)	(-0.647)	(-3.269)	(-1.198)
(5) Elections with more than 100 eligible employees						
Unionization	-0.138***	-0.196***	-0.111*	-0.238**	-0.327***	-0.146
	(-2.750)	(-3.112)	(-1.854)	(-2.221)	(-2.882)	(-1.389)
(6) Firms with non-zero payouts before union elections						
Unionization	-0.166**	-0.213***	-0.085	-0.231**	-0.263**	-0.074
	(-2.461)	(-3.016)	(-1.326)	(-2.403)	(-2.169)	(-0.731)

Table A.2: Regression discontinuity: Dynamic Regressions

This table presents dynamic RD results from estimating a cubic polynomial model specified in Cunat, Gine, and Guadalupe (2012). The dependent variables are *Dividend Payout* (left) and *Total Payout* (right). Union election results are from the NLRB over 1980 to 2011. Payout data are from the Compustat. For this analysis, we keep all elections held by the same firm in our sample period without requiring the absence of confounding elections in the past three years. Following Equation (7) of Cunat, Gine, and Guadalupe (2012), we aggregate the outcomes (equals one if pass and zero if fail) and the polynomials of vote shares of all elections held by the same firm in a given year. *Unionization* is the sum of the outcomes of the elections held by a firm in a year. For each firm and each election year, we examine its dividend and payout ratios in the three years after the election year. *Dummy (N=1)* is a dummy variable that equals one if the observation is one year after the election year, and zero otherwise. *Dummy (N=2)* and *Dummy (N=3)* are similarly defined. Each regression includes a separate intercept and controls for calendar year fixed effects, distance (number of years) to the election year (1, 2, or 3), and firm*election year fixed effects. T-statistics adjusted for heteroskedasticity and within-firm clustering are displayed in the parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

	Dividend Payout	*Total Payout*
Unionization*Dummy(N=1)	-0.034***	-0.093**
	(-2.876)	(-2.394)
Unionization*Dummy(N=2)	-0.032**	-0.123***
	(-2.471)	(-3.556)
Unionization*Dummy(N=3)	-0.042***	-0.131***
	(-2.997)	(-3.805)
Year FE	Yes	Yes
Distance to Election Year	Yes	Yes
Firm*Election Year FE	Yes	Yes
Cubic Polynomials	Yes	Yes
Observations	13,124	13,124
R-squared	0.717	0.555

Table A.3: Subsample analysis based on right-to-work laws and work stoppage provisions
Panels A and B of this table present local linear regression results using the optimal bandwidth following Imbens and Kalyanaraman (2012) for firms located in states either without right-to-work laws or with work stoppage provisions (*RTW/WSP*=1) versus those located in states with right-to-work laws but without work stoppage provisions (*RTW/WSP*=0). Results using triangular kernels are reported. The dependent variables are *Dividend Payout* (left) and *Total Payout* (right). Union election results are from the NLRB over 1980 to 2011. Payout data are from the Compustat. Z-statistics are displayed in the parentheses of Panels A and B. Panels C and D present global RD results from estimating a polynomial model (with order three) specified in Equation (1). To save space, the coefficients for polynomial variables are omitted. T-statistics adjusted for heteroskedasticity and within-firm clustering are displayed in the parentheses of Panels C and D. The p-values reported at the bottom of the table are for one-tailed Chi-squared tests testing that the coefficients before *Unionization* in Panel C are smaller than those in Panel D. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Panel A: Local RD for *RTW/WSP*=1

	Dividend Payout$_{t+N}$			*Total Payout*$_{t+N}$		
	N=1	N=2	N=3	N=1	N=2	N=3
Unionization	-0.171***	-0.248***	-0.156**	-0.172**	-0.399***	-0.125
	(-2.787)	(-3.988)	(-2.359)	(-2.156)	(-3.815)	(-1.331)

Panel B: Local RD for *RTW/WSP*=0

	Dividend Payout$_{t+N}$			*Total Payout*$_{t+N}$		
	N=1	N=2	N=3	N=1	N=2	N=3
Unionization	-0.082	-0.058	-0.049	-0.098	-0.143	-0.187
	(-0.785)	(-0.582)	(-0.424)	(-0.156)	(-0.641)	(-0.922)

Panel C: Global RD for *RTW/WSP*=1

	Dividend Payout$_{t+N}$			*Total Payout*$_{t+N}$		
	N=1	N=2	N=3	N=1	N=2	N=3
Unionization	-0.190***	-0.205***	-0.184***	-0.243*	-0.423***	-0.182
	(-3.333)	(-3.405)	(-2.986)	(-1.885)	(-2.690)	(-1.242)
Cubic Polynomials	Yes	Yes	Yes	Yes	Yes	Yes
Observations	911	911	911	911	911	911
R-squared	0.021	0.021	0.015	0.008	0.012	0.005

Panel D: Global RD for *RTW/WSP*=0

	Dividend Payout$_{t+N}$			*Total Payout*$_{t+N}$		
	N=1	N=2	N=3	N=1	N=2	N=3
Unionization	0.021	-0.005	0.034	-0.029	-0.046	-0.108
	(0.174)	(-0.047)	(0.276)	(-0.079)	(-0.231)	(-0.373)
Cubic Polynomials	Yes	Yes	Yes	Yes	Yes	Yes
Observations	323	323	323	323	323	323
R-squared	0.039	0.040	0.055	0.039	0.037	0.063
Chi-squared tests of the coefficients before *Unionization* in Panels C and D						
P-Value	0.064*	0.060*	0.052*	0.312	0.091*	0.392