Three essays on corporate finance

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2016

Ma, Y. (2016). Three essays on corporate finance. Doctoral thesis, Nanyang Technological University, Singapore.

<https://hdl.handle.net/10356/66324>

<https://doi.org/10.32657/10356/66324>

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**NANYANG
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UNIVERSITY**

THREE ESSAYS ON CORPORATE FINANCE

MA YUJING

NANYANG BUSINESS SCHOOL

2016

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

2016

Acknowledgements

First and foremost, I would like to thank my supervisor Prof Huasheng Gao, without whose help this thesis would not have been possible. He's smart and intelligent, and inspired me with great research ideas. He's diligent and hardworking, and motivated me to devote more on research. He's careful and strict with research papers, and also required me to do so. He enlightened me when I was confused, taught me when I didn't know how, corrected me when I was wrong, and encouraged me when I was frustrated. He's a teacher and a friend for life. I couldn't wish for a better supervisor.

I give my special thanks to Prof Sie Ting Lau and Prof Jun-koo Kang, who gave me valuable advices and guidance on research and career planning since the very beginning of my doctoral study. Prof Sie Ting Lau also served as my supervisor for the first three years of my study and his valuable suggestions and guidance have benefited me throughout the years.

I appreciate the academic courses from Prof Jun-Koo Kang, Prof Chuan Yang Hwang, Prof Jiang Luo and Prof Zhanhui Chen, which led me into the world of research and helped me build a solid foundation for future research. I would also like to thank Prof Chishen Wei, Prof Andy Kim, Prof Mandy Tham, and Prof Steven Dimmok. I worked as their RA and for some research projects. They were all nice and patient to me, gave me valuable suggestions, and helped me develop certain research skills. I thank my committee members, Prof Chishen Wei and Prof Tao Chen for providing insightful and useful comments on my thesis and helping improve it.

I also want to thank my friends in the research office, especially Dr. Yaheng Lin. Yaheng is a senior and a very good friend to me. He offered me help even before I came

to NTU and is still helping me after he left NTU. I got many advices and help from my senior students, including Pang Caiji, Xu Linmin, Dong Yue, Qiang Wei, Cai Yu, Guo Xia and many more. My fellow students and dear friends, Liu Na, Wang Yuxi, Yang Bowen, Huang Xiaoran, Kris, Li Feiyuan, Wen Deer, Song Yue, Zhu Rui, Liu Yan, Zhang Yinggang, Zhuang Jinhong, and many more, all offered me with great support and companion. They made my time in NTU colorful and enjoyable.

I would also like to acknowledge the financial and academic support from the PhD program of Nanyang Business School. I appreciate help from Karen, Bee Hua, Florence and Cindy in the Ph.D. and Research Office throughout my doctoral study.

Last but not least, I would like to thank my parents, my twin sister, and my boyfriend for their continuous love and support for all these years. They cared for me, and were with me through good times and bad times. I'll be eternally grateful.

Introduction

In essay one, we present evidence that the desire to gain human capital is an important motive for corporate acquisitions. Our tests exploit the staggered recognition of the Inevitable Disclosure Doctrine by U.S. state courts, which prevents a firm's employees from working for other competitors. We find a significant increase in the likelihood of being acquired for firms headquartered in states that recognize such doctrine relative to firms headquartered in states that do not. Heterogeneous treatment effects confirm the human capital channel: our result is stronger for firms with greater human capital, for firms with fewer tangible assets and for firms whose employees previously could switch jobs more easily.

In essay two, based on gay-friendly corporate policies as a setting, we find that catering to local community's preference is an important driver of corporate social responsibility policies. We show that firms are more likely to have gay-friendly corporate policies when they are headquartered in states with greater gay-friendly public opinion. This positive relation is more pronounced for firms with greater human capital intensity, for firms with higher local institutional ownership, and for firms that are financially unconstrained. Furthermore, we show that a gay-friendly policy has a positive effect on firm performance when firms locate in gay-friendly states and has no such effect when firms locate elsewhere.

Essay three uses a sample of 236 LBOs and 787 matching deals of U.S. public targets for the period of 1984 to 2013, and finds that LBO target shareholders receive significantly less in takeovers than shareholders of non LBO targets. The low premium cannot be explained by the difference in target or deal characteristics. After controlling

acquirer, deal and target characteristics, LBO target shareholders still receive 16% less of pre-bid equity values than non LBO targets. We investigate the role of target information asymmetry in explaining the premium gap between LBO deals and matching non LBO deals, and we find as target information asymmetry measure increases by a standard deviation, the premium gap between LBOs and non LBOs widens by 5% to 10%. We also find the influence of information asymmetry on premium gap mainly happens before merger announcement, but not after merger announcement.

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Essay One: Human Capital Driven Acquisition: Evidence from the Inevitable Disclosure Doctrine

Abstract

We present evidence that the desire to gain human capital is an important motive for corporate acquisitions. Our tests exploit the staggered recognition of the Inevitable Disclosure Doctrine by U.S. state courts, which prevents a firm's employees from working for other competitors. We find a significant increase in the likelihood of being acquired for firms headquartered in states that recognize such a doctrine relative to firms headquartered in states that do not. Heterogeneous treatment effects confirm the human capital channel: our result is stronger for firms with greater human capital, for firms with fewer tangible assets and for firms whose employees have better ex-ante employment mobility.

Keywords: Acquisition; Human Capital; Labor Market Friction; Inevitable Disclosure Doctrine

JEL Classification: G34, J24, J62, M51, M54

Acquisitions are going to be an alternative to normal recruiting that people really haven't considered before.

Bernard Wysoki Jr. Wall Street Journal 06 Oct 1997

A1

1. Introduction

Anecdotal evidence suggests that obtaining human capital is a key driver of mergers and acquisitions (M&As), and that many M&As occur due to the acquirer's intention to acquire target firms' human capital. For example, on Dec 20, 1996, Apple announced its intention to purchase NeXT Software for \$400 million, and they wrote on their SEC filing that "as part of the agreement, Steve Jobs, Chairman and CEO of NeXT Software, will return to Apple -the company he co-founded in 1976" and that "acquisition brings new talent to Apple"¹. Medias believe Apple's main purpose is to bring back Steve Job and they describe the merger as a "stunning move".² Mark Zuckerberg, CEO of Facebook, also stated that "Facebook has not once bought a company for the company itself. We buy companies to get excellent people."³ Despite these examples, there is little empirical evidence on this matter. In this paper, we fill this gap and present evidence that the desire to gain human capital is an important motive for corporate acquisitions.

Our test exploits the staggered adoption of the Inevitable Disclosure Doctrine (IDD) by U.S. state courts, which prevents a firm's workers who have knowledge of the firm's trade secrets from working for another competitor. We predict that a state's recognition of the IDD could increase the likelihood of firms in that state being acquired for two reasons. First, from an *ex ante* perspective, the IDD prevents the potential acquirer to

¹ <http://www.sec.gov/Archives/edgar/data/320193/0000320193-96-000027.txt>

² <http://www.cnet.com/news/apple-acquires-next-jobs/>

³ http://www.huffingtonpost.com/2010/10/19/mark-zuckerberg-we-buy-co_n_767338.html

poach the target firm's employees directly from the labor market, making a corporate acquisition a more effective alternative for the acquirer to obtain the target's human capital. Second, from an *ex post* perspective, the IDD helps the acquirer retain human talent of the acquired company after the acquisition, which reduces the costs of acquisitions associated with the employees' departure from the acquired firms.

This setting of employing the staggered recognition of the IDD by U.S. state courts is highly appealing from an empirical standpoint for two reasons. First, the motivation behind the IDD centers around state courts' determination to enhance the protection of trade secrets for firms located in the state by reducing the risk that departing employees will reveal a firm's trade secrets to other competitors. As the IDD was not adopted with the intention of promoting M&As, potential effects on M&As are likely to be an unintended consequence of these policies. Second, the staggered adoption of the IDD in several U.S. states enables us to identify their effects in a difference-in-differences framework. Because multiple shocks affect different firms exogenously at different times, we can avoid the common identification difficulty faced by studies with a single shock: the potential biases and noise coinciding with the shock that directly affect corporate acquisitions (Roberts and Whited, 2012).

Using a panel of 122,367 U.S. public firms from 1980 to 2013 and a difference-in-differences approach, we show that, on average, firms headquartered in states that recognize the IDD experience an increase in the likelihood of being acquired by approximately 0.7 to 1 percentage point relative to firms headquartered in states that do not recognize such a doctrine. This effect is economically important considering that the unconditional probability for a firm to be acquired is around 5 percentage points in our

sample. Under treatment reversals, we find that the rejection of previously adopted IDD leads to a decrease in the firm's likelihood of being acquired by a similar magnitude. These results are robust to controlling for various firm and state characteristics.

The identifying assumption central to a causal interpretation of the difference-in-differences estimation is that treated and control firms share parallel trends prior to the state policy change. Our tests show that these firms' pre-treatment trends are indeed indistinguishable. Moreover, most of the impact of the IDD on acquisition likelihood occurs after the state policy change takes effect, which suggests a causal effect.

However, it is possible that the recognition of the IDD is triggered by unobserved local business conditions that in turn increase M&A activities (although we have controlled for various state characteristics, such as state GDP, population, unemployment rate, etc., in the regression). To mitigate this concern, we exploit the fact that economic conditions are likely to be similar in neighboring states, whereas the effects of the IDD stop at state borders. This discontinuity in the IDD allows us to difference away any unobserved confounding factors as long as they affect both the treated state and its neighbors. By comparing treated firms to their immediate neighbors, we can better identify how much of the observed change in firms' likelihood of being acquired is due to the IDD rather than other shocks to local business conditions. When we difference away changes in local business conditions by focusing on treated and control firms closely located on either side of a state border, we continue to find a significant increase in firms' likelihood of being acquired after their states recognize the IDD, relative to their

neighboring firms. These results suggest that our results do not seem to be driven by unobserved local economic shocks.

To provide further evidence that the effects of the IDD on corporate acquisitions are indeed tied to human capital, we apply a double difference-in-differences approach to examine heterogeneous treatment effects. We find that the treatment effects are stronger for firms with greater human capital, for firms with fewer tangible assets and for firms whose employees have better *ex ante* employment mobility. These cross-sectional variations in the treatment effects further increase our confidence in the presence of a human capital channel.

This paper provides at least three major contributions to the literature. First, our paper adds to the literature that examines the drivers of corporate acquisitions. Current research on this topic has focused on factors such as product market synergy (Hoberg and Phillips, 2010), technological overlaps (Bena and Li, 2014), relatedness of firms' industries (Fan and Goyal, 2006), customer-supplier relation (Ahern and Harford, 2014), and stock market misvaluation (Shleifer and Vishny, 2003), etc. Although these studies enhance our understanding of the motivation for M&As (especially from the perspectives of the product market and the stock market), the role of the labor market is largely overlooked. This lack of evidence makes it difficult to fully understand the drivers of corporate acquisitions, given that human capital is one of the most promising sources of a sustainable advantage and is usually a focus in acquisitions (Coff, 2002; Prahalad and Hamel, 1990). Our paper helps to fill this gap by providing evidence that acquiring talent from target firms is an important driver for M&A activities.

Second, our paper is broadly related to the surging literature examining the impacts of the labor market on corporate finance outcomes. Gao et al. (2015) find that employee job-hopping activities in the labor market have significant impacts on firms' compensation policies. Agrawal and Matsa (2013) and Simintzi et al. (2015) find that employee bargaining power significantly influences firms' capital structure decisions. Tate and Yang (2015) find that transferability of human capital is positively associated with corporate diversification. Ouimet and Zarutskie (2013) find that target firms with more employment usually have better post-acquisition employment outcomes. Our paper sheds new insights into this strand of literature by documenting the impacts of labor market frictions of obtaining human capital on corporate acquisition decisions.

Lastly, our paper has important policy implications. Although about 20 of the 50 U.S. states have adopted the IDD, legislators in the remaining states are still debating whether or not to follow suit, partially because the impacts of the IDD on the economy are still unclear. On one hand, the IDD provides better protections of firms' trade secrets. On the other hand, it limits employees' freedom in the labor market. Our paper provides evidence that this legislation (unintendedly) spurs M&A activities.

The remainder of the paper is organized as follows. Section 2 reviews the background on the IDD. Section 3 develops our hypothesis. Section 4 describes our sample and key variable construction. Section 5 presents the empirical results. We conclude in Section 6.

2. Background on Trade Secrets and the IDD

A trade secret is defined as any valuable business information that is not generally known and is subject to reasonable efforts to preserve confidentiality. Examples of trade secrets include software, techniques, business plans, designs, details about customers and suppliers, etc. Misappropriation of trade secrets occurs when the trade secret is acquired by improper means (e.g., theft or breach of a duty to obtain the secret) or by disclosure without consent by the person who obtained the secret under situations giving rise to a duty to maintain the secret or limit its use.

State courts adopt the IDD to enhance the legal protection of trade secrets for firms located in the state when an employee will inevitably use or disclose knowledge of such trade secrets in her new employment. The IDD maintains that if the new employment would inevitably lead to the disclosure of the firm's trade secrets to competitors and cause the firm irreparable harm, then state courts can prevent the employee from working for the firm's competitor or can limit the worker's responsibility in the new firm. Under the IDD, a firm's suit can be based on the threats of irreparable harm (even though the actual harm has not occurred), as long as the firm can provide evidence that (1) the departing employee had access to its trade secrets, (2) the employee's duty in the new firm would inevitably make her disclose the trade secrets, and (3) the disclosure of the trade secrets would lead to irreparable economic harm to the firm. Furthermore, the firm does not need to establish any actual wrongdoing by the employee or disclose the actual details of the underlying trade secrets in the lawsuit. As described by Malsberger (2004) and Garmaise (2011), the relevant jurisdiction for a trade secret-related lawsuit is typically in the state where the job-hopping employee's former employer is located. As a

result, the IDD prevents the job-hopping employee from working in a new firm even if the new firm operates in a state that has not adopted the IDD.

The legal case between PepsiCo and Mr. Redmond is a classic example in which the court applied the IDD. In 1994, PepsiCo sought an injunction against its former employee, William Redmond, from working for Quaker, a competitor of PepsiCo. PepsiCo's products "PowerAde" and "Fruitopia" competed with Quaker's "Gatorade" and "Snapple" brands, and Redmond had a senior-level marketing position at PepsiCo. In that position, Redmond had knowledge of PepsiCo's strategic plans, price structure, distribution system, marketing plan, and other trade secrets; and he was hired by Quaker for a similar marketing position. PepsiCo argued that, no matter how hard he tried to avoid it, Redmond could not help but use PepsiCo's trade secrets in his new position, and that disclosing these trade secrets would give Quaker an unfair advantage. Though there was no evidence that Redmond took any physical materials from PepsiCo, the court found the situation to be a classic case of inevitable disclosure and prohibited Redmond from taking the position.

The details of the IDD adoptions and rejections are collected from Klasa et al. (2015). As shown in Table 1, New York was the first U.S. state to adopt the IDD (in 1919). By the end of our sample period, 21 states adopted IDD once, 3 of which rejected their previously adopted IDD a few years after the initial adoption. This allows us to examine the treatment effects and reverse treatment effects.

Klasa et al. (2015) describe a few key differences between the IDD and employment contracts with a non-disclosure agreement (NDA) and/or a covenant not to compete (CNC). First, an NDA or CNC usually has specific geographic restrictions; the scope of

enforceable CNC/NDA is often within a county or a city, or within a 10 or 50-mile radius around the place of business. In contrast, the IDD typically can be enforced within a much broader geographic scope. Second, the IDD allows state courts to grant an injunction if allowing employment at the rival firm would inevitably lead to a future violation of NDAs (before the actual violation of NDAs), which greatly enhances the enforceability of NDAs because detecting and proving violation of an *ex post* violation of NDA is costly. Finally, the IDD allows courts to grant an injunction even if the job-hopping employee did not sign an NDA or CNC with her previous company.

Finally, it is also worth pointing out that the change in a state's IDD policy can largely be regarded as exogenous in our context of corporate acquisition tests. When considering the adoption or rejection of the IDD, state courts mainly aim to achieve a balance between the companies' interests of stronger protection of trade secrets and the employees' interests of labor market freedom (Godfrey, 2004; Harris, 2000). In other words, given that the primary purpose of the IDD policy change is either to better protect firms' trade secrets (IDD adoption) or to better protect employees' employment freedom (IDD rejection), the change in firms' likelihood of being acquired is likely to be an unintended consequence of these policy changes. Moreover, unlike other state laws whose adoption can be influenced greatly by interest groups, such as labor unions and companies, the adoption (rejection) of the IDD largely depends on judicial decisions based on the merits of the specific case. Further, state judges who make the judicial decision are deemed to be independent of the state and federal government and largely immune to lobby groups and political pressure (Klasa et al., 2015). In summary, the

staggered adoption of the IDD is unlikely to be triggered by factors that drive corporate acquisition activities.

3. Hypothesis Development

As pointed out by Zingales (2000), human capital is emerging as the most crucial asset for a firm. There are typically two ways for a firm to obtain human capital: hiring from the labor market or acquiring via a corporate acquisition. Compared to the former model, the latter model is advantageous when the firm wants to bring in teams of employees and achieve a large influx of human capital, and when it is difficult for the firm to directly poach its desired talent from the labor market (for example, a desired talent is closely tied to another firm and is unwilling or not allowed to switch jobs legally). Existing literature has provided ample evidence that acquisitions can bring desired knowledge and fresh talent to a firm, which in turn leads to greater innovation, speedy new product introduction, and enhanced firm performance (Ahuja and Katila, 2001; Puranam et al., 2006).

Despite these potential benefits, significant challenges exist for firms that pursue corporate acquisitions as a means to obtain human capital. Some physical assets and employees of the target firm may not be useful to the acquiring firm, and thus the acquirer needs to bear some additional costs to dispose of these redundant physical assets and employees (Kaplan and Weisbach, 1992). Moreover, following the completion of the acquisition, the target's pre-existing employees (who are desired by the acquirer) can choose to leave and are not acquired in the same way the new owner gains control for the

target's pre-existing physical assets. Considering that the target firm's knowledge is usually stored in the experience of its employees, the departure of employees immediately reduces the target's knowledge base and increases the risk of knowledge leakage to other competitors, which decreases the effectiveness of using acquisition as a means to obtain human capital. Existing research has shown that the method in which acquirers retain and motivate employees of acquired companies is critical to synergy realization and acquisition performance (Ashkenas et al., 1998; Buchholtz et al., 2003).

In equilibrium, the likelihood of acquiring human capital via corporate acquisition depends on the tradeoff of the above benefits and costs. We expect that a state's recognition of the IDD increases its local firms' likelihood of being acquired for the following two reasons. First, from an *ex ante* view, the IDD increases the labor market frictions for the potential acquirer to hire talent directly from the state's local firms, which in turn makes acquisition a relatively more effective way to obtain these firms' human capital. Second, from an *ex post* view, after the completion of the acquisition, the IDD helps to prevent employees of the target firms in the IDD-adopting states from leaving for other competitors, which reduces the risk associated with the potential departure of target firms' employees.

In summary, when an exogenous change in a state's doctrine increases the difficulty of hiring workers from the labor market and decreases the costs associated with employee departure in the post-acquisition stage, we expect that companies headquartered in this state are more likely to be acquired subsequently.

4. Sample Formation and Variable Construction

From CRSP-Compustat merged dataset, we start with all U.S. public firms traded on NYSE, AMEX or NASDAQ. To focus on more economically important companies, we require that our sample firms have a book value of total assets above \$10 million. We then obtain the firm's headquarter information from Compustat, Compact Disclosure (which records any changes in a firm's headquarters), and manually check any missing information.

Our dependent variable is the *Acquisition* indicator variable, which equals one if the firm is the target of an acquisition in a given year, and zero otherwise. Information on acquisition is obtained from Thomson Financial's SDC Database. We retain an acquisition deal only if the deal is completed and the acquirer owns 100% of the target firm after the deal's completion. Given that the SDC database might be less reliable before 1980, we start our sample in 1980. Our final sample consists of 122,367 firm-year observations (10,911 unique firms) from 1980 to 2013.

We control for a vector of firm characteristics that may affect a firm's likelihood of being acquired, and these controls are motivated by prior literature (e.g., Song and Walkling, 2000). These variables include firm size, asset tangibility, leverage, R&D expenditures, ROA, Tobin's Q , and excess stock return. We also account for various state-level variables in our regressions. Since larger and richer states may have more active M&A activities, we control for state GDP growth rate and state population. We further control for state business combination laws, which reduce the threat of hostile takeovers and thus affect the firm's likelihood of being acquired. We control for the state's enforceability of the covenant not to compete (CNC) policy, which limits the

mobility of informed workers in the labor market. We also include the state establishment entry rate, the state establishment exit rate, and the state unemployment rate to capture the local economic conditions. Data on state GDP growth is obtained from the Bureau of Economic Analysis, and data on state population is obtained from the U.S. Census Bureau. Information regarding state business combination laws is collected from Giroud and Mueller (2010). Data for the CNC enforceability is obtained from Garmaise (2011). State business entry and exit rates are obtained from the Business Dynamics Statistics database of the U.S. Census Bureau. Finally, state unemployment rates are obtained from the U.S. Bureau of Labor Statistics Local Area Unemployment Statistics Series. All explanatory variables are lagged by one year. To minimize the effect of outliers, we winsorize all continuous variables at the 1st and 99th percentiles. Detailed variable definitions are provided in the Appendix.

Table 2 provides summary statistics. The *Acquisition* indicator has a mean value of 0.05, indicating that, on average, 5% of our sample firms become the target in an acquisition deal. Our median sample firms have book value assets of \$450 million, are moderately levered with a book leverage ratio of 11.40%, and have 17.6% of total assets in the form of tangible assets. In terms of performance, sample firms perform well with a median ROA of 3.20%, sales growth of 9.30%, and Tobin's Q of 1.27. As to state characteristics, the average state has a population of 13.21 million, a GDP growth rate of 5.7%, and an unemployment rate of 6.23%. On average, the establishment entry and exit rates are 12.20% and 10.67%, respectively. Almost half of the states have adopted a business combination law.

5. Empirical Results

5.1 Baseline Regression

Several U.S. state courts recognized the IDD in different years during the sample period. Thus, we can examine the before-after effect of the change in the IDD in affected states (the treatment group) compared to the before-after effect in states in which such a change was not effected (the control group). This is a difference-in-differences test design in multiple treatment groups and multiple time periods as employed by Acharya et al. (2014), Klasa et al. (2015), and Imbens and Wooldridge (2009). We implement this test through the following linear probability regression⁴:

$$\begin{aligned} Acquisition_{i,t} = & \alpha + \beta_1 IDD_{s,t-1} + \beta_2 Firm\ Characteristics_{i,t-1} + \\ & \beta_3 State\ Characteristics_{i,t-1} + Firm\ FE + Region \times Year\ FE + \varepsilon_{i,t}, \end{aligned} \quad (1)$$

where i indexes firm, s indexes the state in which the firm's headquarter is located, and t indexes the year. The dependent variable is an indicator variable that takes the value of one if the firm is acquired in year t , and zero otherwise. The variable IDD is a dummy variable that equals one if the IDD is in place in state s in a given year, and zero otherwise. It is worth noting that the IDD indicator can change either from zero to one (a state starts to recognize the IDD) or from one to zero (a state rejects its previously adopted IDD). We include a set of control variables that may affect a firm's likelihood of being acquired, as discussed in Section 4. The firm fixed effects allow us to control for time-invariant differences in the likelihood of being acquired across firms. Following

⁴ Considering that we have a large number of fixed effects, a non-linear model (such as a logit or probit model) is impractical and likely to produce biased estimates due to incidental parameter problem (Lancaster, 2000). Moreover, the marginal effects in a linear probability model are easier to compute and interpret relative to non-linear models, especially for interaction terms. Nevertheless, in an untabulated analysis, we re-estimate our tests based on a logit or probit model and obtain the same inference.

Acharya et al. (2014), we also control for regional time trends through the interaction of region dummies with year dummies.⁵ These interactions enable us to nonparametrically account for time-varying differences between geographic regions of the U.S. in corporate acquisitions and in the recognition of the IDD. Given that our treatment is defined at the state level, we cluster standard errors by state.

The coefficient of interest in this model is the β_1 coefficient. As explained by Imbens and Wooldridge (2009), the employed fixed effects lead to β_1 being estimated as the *within-state* differences before and after the policy change as opposed to similar before-after differences in states that did not experience such a change during the same period.

It is helpful to consider an example. Suppose we want to estimate the effect of the IDD recognized in Ohio in 2000 on the likelihood of being acquired for firms in Ohio. We can subtract the likelihood before the IDD recognition from the likelihood after the IDD recognition for firms headquartered in Ohio. However, economy-wide shocks may occur at the same time and affect corporate acquisitions in 2000. To difference away such factors, we calculate the same difference in the likelihood of being acquired for firms in a control state that does not recognize the IDD. Finally, we calculate the difference between these two differences, which represents the incremental effect of the policy change on firms in Ohio compared to firms in the control state.

Table 3 presents the regression results. The coefficient estimates on *IDD* are positive and statistically significant in all columns. In column (1), we only include *IDD*,

⁵ Following Acharya et al. (2014), we distinguish four U.S. regions based on the classification of the U.S. Census Bureau: Northeast, South, Midwest, and West.

Firm FE, and *Region*×*Year FE* as the independent variables; and the coefficient estimate on the *IDD* indicator is positive and significant at the 5% level, suggesting a positive effect of the policy change on the firm’s likelihood of being acquired.

In columns (2) and (3), we additionally control for various firm and state characteristics, and we obtain similar results. For example, we control for the full set of firm and state characteristics in column (3) and find that the coefficient estimate on the *IDD* indicator is 0.009 and significant at the 5% level. The economic magnitude is also sizeable: the adoption of the *IDD* leads to an increase in the firm’s likelihood of being acquired by approximately 0.9 percentage points, relative to the unconditional probability of 5 percentage points (i.e., an increase of 18%).

With regards to control variables, firms with lower market valuation, firms with lower sales growth, R&D intensive firms, and firms with greater asset tangibility are more likely to be acquired. These results are broadly consistent with prior literature (e.g., Song and Walkling, 2000). Most of the state-level variables do not have significant coefficients, probably because we have already controlled for *Region* × *Year FE* in the regression.

Our *IDD* indicator variable captures both the recognition of the *IDD* (the most frequent event that dominates our sample) and the three rejections of the *IDD* by states that had recognized the *IDD* in prior years. In Table 4, we conduct our difference-in-differences tests separately for the events associated with adoptions and rejections of the *IDD*.

In Table 4 column (1), the regression specification follows column (3) of Table 3, except that we exclude all firm-year observations in Florida, Michigan, and Texas (the three rejection states). Removing these observations ensures that the rejections of the

IDD occurring in our sample period do not confound the estimated impact of the adoption of the IDD. The key independent variable *IDD Adoption* is an indicator variable, which takes the value of one beginning the year when the state first recognizes the IDD, and zero otherwise. We find a significant increase in the likelihood of being acquired for firms headquartered in the adopting states relative to those in non-adopting states. The coefficient estimate on the *IDD Adoption* indicator is 0.010 and significant at the 5% level, indicating that the adoption of the IDD leads to an increase in the firm's likelihood of being acquired by 1 percentage point.

Further, column (2) presents the results of examining the effects of a state's rejection of the IDD on corporate acquisition. Given that the first IDD rejection took place in 2001 (by Florida), we restrict the sample period from 1996 onwards, starting five years before the first rejection. To further ensure that the IDD adoptions in our sample period do not confound the estimated impacts of the IDD rejections on corporate acquisition, we also exclude all firm-year observations in states that adopted the IDD after 1996. The key independent variable is the *IDD rejection* indicator, which takes the value of one beginning the year when the state rejects its previously adopted IDD, and zero otherwise. We find a significant decrease in the likelihood of being acquired for firms headquartered in the rejecting states relative to firms headquartered in non-rejecting states. The coefficient estimate on the *IDD rejection* indicator is -0.015, indicating that the rejection of the IDD leads to a decrease in the firm's likelihood of being acquired by 1.5 percentage points.

Taken together, a state's adoption of the IDD leads to an increase in the likelihood of being acquired for firms in the state, while the rejection of the IDD leads to a decrease

in this likelihood. Moreover, the economic magnitude of the IDD adoptions is comparable to that of the IDD rejections. These results provide support to a causal effect of the IDD on the likelihood of a firm being acquired.

5.2 The Pre-treatment Trends

The validity of difference-in-differences estimation depends on the parallel trends assumption: absent the IDD, treated firms' likelihood of being acquired would have evolved in the same way as that of control firms. Table 5 presents the results that investigate the pre-trend between the treated group and control group.

The specification of examining the pre-trend follows Atanassov (2013) and Acharya et al. (2014). In column (1), we focus on the adoption of the IDD and re-estimate column (1) of Table 4 by replacing the *IDD adoption* indicator with the four indicator variables: $IDD\ adoption^{-2or-1}$, $IDD\ adoption^0$, $IDD\ adoption^1$, and $IDD\ adoption^{2+}$. These variables indicate the year relative to the adoption of the IDD. In particular, $IDD\ adoption^{-2or-1}$ indicates that it is one or two years before the IDD adoption; $IDD\ adoption^0$ indicates the year in which the IDD is adopted; $IDD\ adoption^1$ indicates that it is the year after the IDD adoption; and $IDD\ adoption^{2+}$ indicates that it is two or more years after the IDD adoption.

The coefficient on the $IDD\ adoption^{-2or-1}$ indicator is especially important because its significance and magnitude indicate whether there is any difference between the treatment group and the control group prior to the adoption of the IDD. The coefficient on this indicator is not significantly different from zero, suggesting that the parallel trend assumption of the difference-in-differences approach is not violated. The impact of the

IDD starts to show up two years after the adoption: the coefficient on the $IDD\ adoption^{2+}$ indicator becomes significantly positive.

In column (2) of Table 5, we focus on the rejection of the IDD and re-estimate column (2) of Table 4 by replacing the *IDD rejection* indicator with the four indicator variables, $IDD\ rejection^{-2or-1}$, $IDD\ rejection^0$, $IDD\ rejection^1$, and $IDD\ rejection^{2+}$. We also find that the coefficient on $rejection^{-2or-1}$ is not significantly different from zero, and that the impacts of the IDD rejection on corporate acquisitions show up after the IDD rejection.

Overall, Table 5 shows that the treated group and the control group share a similar trend in the likelihood of being acquired prior to the policy change, thus supporting the parallel trends assumption associated with the difference-in-differences estimation. Moreover, Table 5 also indicates that most of the impact of the IDD on corporate acquisitions occurs *after* the doctrine is recognized, which suggests a causal effect.

5.3 Unobservable Confounding Local Business Conditions

Although we have accounted for *observable* local business conditions in the regression, some *unobservable* local economic shocks may be associated with both the recognition of the IDD and corporate acquisition activities. In this subsection, we difference away unobservable local business conditions by focusing on treatment firms that are on one side of a state border and their neighboring control firms on the other side of the state border.

In particular, we exploit the discontinuity of the IDD and examine the change in the likelihood of being acquired in the treatment firms on the state border relative to their

neighboring control firms. The logic is as follows. Suppose that the IDD is driven by unobserved changes in local business conditions, and that it is these changes (not the IDD) that spur corporate acquisitions in reality. Then both firms in treated states and their neighbors in untreated states just across the state border would spuriously appear to react to the policy changes, because economic conditions, unlike state laws, have a tendency to spill across state borders (Heider and Ljungqvist, 2015). In this case, the change in acquisition likelihood in treated firms should be no different from that in the neighboring control firms that are located just across the state border.

To examine this possibility, we match each treated firm to a control firm that is in the same industry (based on 3-digit NAICS code), is in an adjacent state without recognizing the IDD, and is closest in firm size. Obviously, a treated firm may not necessarily share the same local economic conditions with its control firm in the adjacent state if the treated firm is in the middle of a large state. To alleviate this concern, we further require that the distance between the treated firm and its matched untreated firm be within a certain range (such as 40 to 100 miles). If the distance is beyond this range, we drop this pair from our sample. By doing so, we increase our confidence that our treated firm and control firm are truly close to each other geographically and thus face similar local economic shocks. Then, we re-estimate Equation (1) by focusing on this sub-sample of firms across the state borders. We also include a pair fixed effect for each pair of treated firms and neighboring control firms.

Table 6 presents the results. In column (1), we require that the distance between the treated firm and its neighboring control firm be within 40 miles. This requirement reduces the sample to 10,395 firm-year observations; yet, we still find a positive and

significant coefficient on the *IDD* indicator. As a robustness check, we require the distance between the treated firm and its neighboring control firm to be within 50, 60, 80, and 100 miles in columns (2)-(5), respectively; we continue to find that the likelihood for firms being acquired is significantly increased after the recognition of the *IDD*. Overall, these results suggest that unobserved local business conditions are unlikely to drive our results.

5.4 Double Difference-in-differences Tests

To provide further evidence that the effects of the *IDD* on acquisitions are indeed tied to the human capital mechanism, in this subsection we implement double difference-in-differences tests to examine the heterogeneous treatment effects.

First, if the enhanced likelihood of being acquired after the *IDD* adoption is due to bidding firms' desire to acquire human capital, we expect this treatment effect to be stronger for the target firms that possess more human capital. Following Coff (2002), we measure human capital intensity as the number of knowledge workers as a proportion of all workers in the industry. We obtain employment data from the Integrated Public Use Microdata Series database (IPUMS-USA, see Steven et al., 2010). Based on the IPUMS occupational codebook, we define knowledge workers to be those with an occupational code (1990 basis) below 200. This definition includes occupations such as managers, scientists, engineers, computer programmers, IT professionals, and so forth. The IPUMS provides Census and American Community Survey (ACS) data on individual workers' occupational code, industry, state, etc. Occupational data is available every ten years before 2000, and available annually since 2000; so we use census data for the years 1980,

1990, and 2000 for the periods of 1980-1985, 1986-1995, and 1996-2000, respectively, and the annual ACS survey data after 2000. From the IPUMS data, we calculate the proportion of the total workforce being knowledge workers for a given 3-digit NAICS industry in a given year, and then assign that measure to each focal firm in our sample. We then define the *High human capital intensity* indicator as one if the proportion of knowledge workers among all workers is above the sample median, and zero otherwise. Then, the *Low human capital intensity* indicator is defined as $(1 - \text{High human capital intensity})$. We re-estimate Equation (1) by replacing the *IDD* indicator with $IDD \times \text{High human capital intensity}$ and $IDD \times \text{Low human capital intensity}$ indicators.⁶

Table 7 column (1) presents the results. The coefficient on $IDD \times \text{High human capital intensity}$ is 0.017 and significant at the 1% level, while the coefficient on $IDD \times \text{Low human capital intensity}$ is only 0.003 and not significantly different from zero. This indicates that the treatment effect is significant for firms with great human capital, and is virtually absent for firms with little human capital.

Second, human capital, rather than physical capital, is particularly important for high-R&D companies (Zingales, 2000). Thus, we expect the treatment effects to be stronger for high-R&D companies. We define a *High R&D* indicator as one if the firm's R&D expenditure is above the sample median, and zero otherwise. The *Low R&D* indicator is defined as $(1 - \text{High R\&D})$. We then re-estimate Equation (1) by replacing the *IDD* indicator with $IDD \times \text{High R\&D}$ and $IDD \times \text{Low R\&D}$ indicators. The coefficient on $IDD \times \text{High R\&D}$ is positive and significant at the 1% level, while the coefficient on $IDD \times \text{Low R\&D}$ is not significantly different from zero. This result indicates that the

⁶ This specification of double difference-in-differences tests follows Acharya et al. (2014).

treatment effect is significant for high-R&D firms, whereas it is virtually absent in low-R&D firms.

Third, if acquirers are motivated by the desire to obtain human capital, they should be less likely to target firms with sizable tangible assets to avoid the large costs associated with the redundant tangible assets. Thus, we expect the treatment effects to be stronger for firms with fewer tangible assets. Tangible assets are measured as the book value of property, plant, and equipment, plus the book value of inventory divided by the book value of total assets. We then define a *High tangibility* indicator as one if the firm's tangible assets are above the sample median, and zero otherwise. The *Low tangibility* indicator is defined as $(1 - \text{High tangibility})$. We re-estimate Equation (1) by replacing the *IDD* indicator with $\text{IDD} \times \text{High tangibility}$ and $\text{IDD} \times \text{Low tangibility}$ indicators. The coefficient on $\text{IDD} \times \text{Low tangibility}$ is positive and significant at the 1% level, while the coefficient on $\text{IDD} \times \text{High tangibility}$ is not significantly different from zero. This result indicates that the treatment effect is significant for firms with few tangible assets, whereas it is virtually nonexistent for firms with large tangible assets.

Considering that the impact of the *IDD* on a firm's likelihood of being acquired is due to the increased labor market frictions to hire talent directly, we expect the treatment effects to be stronger for firms whose employees have higher *ex ante* mobility in the labor market. Ippolito (1985) and Dorsey (1995) argue that it is more difficult to poach key employees from firms with defined benefit pension plans, because retirement benefits from these plans are less portable and thus it is more costly for workers in such firms to change employers. Hence, we expect the treatment effect to be less pronounced for firms with defined benefit pension plans. In column (3), we define the *Defined benefit plan*

indicator as taking the value of one if the firm has a defined benefit plan, and zero otherwise. *Non-defined benefit plan* is $(1 - \text{Defined benefit plan})$. Then, we re-estimate Equation (1) by replacing the *IDD* indicator with $IDD \times \text{Defined benefit plan}$ and $IDD \times \text{Non-defined benefit plan}$ indicators. The coefficient on $IDD \times \text{Defined benefit plan}$ is not significantly different from zero, while the coefficient on $IDD \times \text{Non-defined benefit plan}$ is positive and significant at the 1% level. This result indicates that the treatment effect is significant for firms without defined benefit plans (whose employees are more likely to switch jobs *ex ante*), whereas it is virtually absent for firms with such a plan.

Lastly, Deng and Gao (2013) and Gao et al. (2015) show that employees have better employment mobility when there are a large number of industry peer firms available in the local labor market. Following their studies, we use the number of firms in the same 3-digit NAICS industry and same state as another proxy for the *ex ante* labor market mobility. We expect the treatment effect to be stronger when there are a large number of industry peer firms nearby. To examine this prediction, we define the *Many rivals* indicator as one if the number of firms in the same 3-digit NAICS industry and same state is above the sample median, and zero otherwise. The *Few rivals* indicator is defined as $(1 - \text{Many rivals})$. In column (4), we re-estimate Equation (1) by replacing the *IDD* indicator with $IDD \times \text{Many rivals}$ and $IDD \times \text{Few rivals}$ indicators. The coefficient on $IDD \times \text{Many rivals}$ is positive and significant at the 1% level, while the coefficient on $IDD \times \text{Few rivals}$ is not significantly different from zero. This result indicates that the treatment effect is significant for firms surrounded by many industry peers (and thus their employees are more likely to switch jobs *ex ante*), whereas it is virtually absent for firms surrounded by few industry peers.

Taken together, the effects of the IDD on a firm's likelihood of being acquired are stronger for firms with greater human capital, for firms with fewer tangible assets and for firms whose employees have better employment mobility *ex ante*. These results suggest that obtaining human capital via M&As is indeed the mechanism through which a state's recognition of the IDD influences its local firms' likelihood of being acquired.

5.5 Additional Analysis on Employee Job-hopping Activities

To provide further evidence that the IDD indeed reduces the employment mobility in the labor market, we conduct an additional analysis in Table 8. Following Gao et al. (2015), we measure the labor market mobility using the executive job-hopping activities recorded in the ExecuComp database, which covers the S&P 1500 firms starting from 1993. ExecuComp assigns a unique identifier (EXECID) to each executive in the database, allowing us to track EXECIDs to locate each executive's position across different firms over the sample period. We define an executive job-hopping event when one executive leaves her current firm and subsequently takes an executive position in another firm the following year. Considering that some job-hopping events may be confounded with forced executive turnover, we search all available news reports in Factiva, investigate the reason leading to each job-hopping event, and remove all "forced" ones, where a job-hopping event is regarded as "forced turnover" if the press reports that the executive is fired, is forced out, or resigns due to pressure (Parrino, 1997). Finally, we define the indicator variable *Job-hopping* as taking the value of one if a firm experiences a job-hopping event in a given year (i.e., its executives job-hop to other firms), and zero otherwise. In total, we have 33,978 firm-year observations from the

CRSP/Compustat/ExecuComp merged database over the period 1993 to 2013, among which 1,360 firm-year observations (4%) experience job-hopping events.

We re-estimate Equation (1) by using the *Job-hopping* indicator as the dependent variable. We find a negative and significant coefficient on the *IDD* indicator, indicating that the IDD reduces the executive job-hopping activities in the labor market. In columns (2) and (3), we separately examine the effect of IDD adoption and IDD rejections. We find that the coefficient on the IDD adoption is -0.017 and is significant at the 5% level. This indicates that the adoption of the IDD leads to a reduced likelihood of executive job-hopping by 1.7 percentage points, relative to the unconditional probability of job-hopping of 4 percentage points. The coefficient on the *IDD rejection* indicator is a significant 0.017, indicating that the rejection of the previously adopted IDD leads to an increase in executive job-hopping activities by the same magnitude as the IDD adoption.

Overall, based on the executive job-hopping activity as a proxy of labor market mobility, we find that the IDD indeed leads to more labor market frictions and prevents employees from joining other firms. This finding is broadly consistent with Klasa et al. (2015), who employ individual employees' information from the Census Bureau and find that the recognition of the IDD significantly reduces the probability that an individual employee job-hops to other companies. Our finding is also consistent with Png and Samila (2013), who find that the IDD significantly reduces the labor market mobility for engineers and scientists and makes it difficult for rival firms to poach these employees.

6. Conclusions

In this paper, we investigate whether obtaining human capital is an important motivation for corporate acquisitions, by exploiting various exogenous shocks from the staggered recognition of the Inevitable Discourse Doctrine (IDD) by U.S. state courts. The recognition of this doctrine increases the cost for the acquirer to directly poach the target's employees from the labor market and decreases the likelihood of departure of the target's employees following the acquisition's completion, which makes acquisition a more effective way for the acquirer to obtain the target's human capital. Thus, we predict that a state's recognition of the IDD could increase the likelihood of being acquired for firms in the state.

Consistent with this prediction, we find a significant increase in the likelihood of being acquired for firms in states that recognize the IDD, relative to firms in states that do not. In support of a causal interpretation of our findings, our timing tests indicate that the firm's likelihood of being acquired changes after the recognition of the IDD. Our further tests of employing treated firms and their neighboring untreated firms across the state border indicate that our results are unlikely to be driven by unobserved local confounding factors. Finally, the cross-sectional variation of the treatment effects confirms the presence of a human capital channel: the impact of the IDD on the firm's likelihood of being acquired is more pronounced for firms with greater human capital, for firms with fewer tangible assets, and for firms whose employees have better employment mobility *ex ante*. Overall, our findings are consistent with the view that corporate acquisitions can be used as a means for acquiring firms to overcome labor market frictions and gain access to valuable human capital.

Lastly, although our paper focuses on the likelihood of firms being acquired, other aspects of M&As could also be influenced by the IDD. In particular, considering that the IDD prevents the acquirer from directly poaching a target's employees, the target firm can probably bargain for a higher premium, leading to a higher (lower) announcement return for the target (acquirer). In addition to M&As, labor market frictions associated with the IDD could also play an important role in other corporate finance decisions. For example, following a state's adoption of the IDD, are firms in this state less likely to use employee stock options to retain employees? To the extent that the IDD reduces firms' risk of losing key employees, will companies be more likely to make long-term investment? Examining these issues could be an interesting area for future research.

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Appendix. Variable Definitions

Variable	Definition
Acquisition	An indicator variable which equals one if the firm is acquired in an M&A deal in a given year, and zero otherwise.
Business combination law	An indicator variable which equals one if a state has adopted laws that reduce the threat of hostile takeover, and zero otherwise.
CNC index	An index constructed by Garmaise (2011), indicating state enforceability of covenants not to compete (CNCs).
Defined benefit plan	An indicator variable which equals one if the firm has a defined benefit plan, and zero otherwise.
Excess return	The difference between a firm's annual return and the annual return of CRSP value-weighted market index.
Few rivals	1–Many rivals.
Fixed assets	Book value of property, plant and equipment divided by the book value of total assets.
High human capital intensity	An indicator variable which equals one if the fraction of knowledge workers among all workers is above the sample median, and zero otherwise.
High R&D	An indicator variable which equals one if the firm's R&D expense is above the sample median, and zero otherwise.
IDD	An indicator variable which equals one if the state recognizes the IDD, and zero otherwise.
IDD adoption	An indicator variable which equals one beginning the year when the state first recognizes the IDD, and zero otherwise.
IDD rejection	An indicator variable which equals one beginning the year when the state rejects its previously adopted IDD, and zero otherwise.
Job-hopping	An indicator variable which equals one if the firm's executives leave the firm and join other firms in a given year, and zero otherwise.
Leverage	Book value of long-term debt divided by the book value of total assets.
Low human capital intensity	1–High human capital intensity.
Low R&D	1–High R&D.
Many rivals	An indicator variable which equals one if the number of firms in the same 3-digit NAICS industry and the same state is above the sample median, and zero otherwise.
Non-defined benefit plan	1– Defined benefit plan.
ROA	Return on assets, measured as net income over book value of total assets.
R&D	R&D expenditure divided by the book value of total assets (missing values are set to zero).
Sales growth	The percent increase of sales from the previous year.
State establishment entry	Establishment entry rate in the firm's state.
State establishment exit	Establishment exit rate in the firm's state.
State GDP growth	The annual growth rate of the GDP in the firm's state.
State population	Total population in the firm's state.

State unemployment rate	Unemployment rate in the firm's state.
Tobin's Q	Book value of total assets minus book value of equity plus market value of equity, divided by book value of total assets.
Total assets	Book value of total assets.

Table 1. List of the Adoption and Rejection Years of the IDD by State

This table presents the years in which U.S. state courts adopted the Inevitable Disclosure Doctrine (IDD) and rejected it after adoption. The data is obtained from Klasa et al. (2015).

State	Adopt Year	Reject Year
New York	1919	
Florida	1960	2001
Delaware	1964	
Michigan	1966	2002
North Carolina	1976	
Pennsylvania	1982	
Minnesota	1986	
New Jersey	1987	
Illinois	1989	
Texas	1993	2003
Massachusetts	1994	
Indiana	1995	
Connecticut	1996	
Iowa	1996	
Arkansas	1997	
Washington	1997	
Georgia	1998	
Utah	1998	
Missouri	2000	
Ohio	2000	
Kansas	2006	

Table 2. Summary Statistics

The sample consists of 122,367 firm-year observations during the 1980–2013 period, obtained from the CRSP-Compustat merged database. All sample firms are U.S. public firms traded on NYSE, AMEX, or NASDAQ. Variable definitions are provided in the Appendix. All dollar values are in 2013 dollars. All continuous variables are winsorized at the 1st and 99th percentiles.

	Mean	Std. Dev	P25	Median	P75
Acquisition	0.05	0.22	0.00	0.00	0.00
IDD	0.44	0.50	0.00	0.00	1.00
Total assets (\$million)	3557	11000	117	450	1879
ROA	1.00%	14.50%	0.40%	3.20%	7.30%
Excess return	4.60%	55.20%	-27.50%	-2.80%	24.10%
Tobin's Q	1.76	1.33	1.03	1.27	1.92
Sales growth	18.80%	46.40%	0.00%	9.30%	23.70%
Leverage	17.10%	18.40%	1.00%	11.40%	27.90%
R&D	3.30%	7.30%	0.00%	0.00%	2.90%
Fixed assets	25.20%	24.70%	4.10%	17.60%	38.60%
State population (million)	13.21	10.04	5.44	10.74	18.37
State GDP growth	5.70%	3.21%	3.90%	5.60%	7.60%
Unemployment rate	6.23%	1.95%	4.87%	5.82%	7.36%
State establishment entry	12.20%	2.05%	10.70%	12.00%	13.40%
State establishment exit	10.67%	1.42%	9.60%	10.50%	11.40%
Business combination law	0.49	0.50	0.00	0.00	1.00
CNC index	0.33	0.18	0.25	0.42	0.42

Table 3. The Inevitable Disclosure Doctrine and the Likelihood of Being Acquired

This **table** reports the difference-in-differences tests that examine the impacts of the Inevitable Disclosure Doctrine (IDD) on the firm's likelihood of being acquired. The dependent variable *Acquisition* is an indicator variable, which takes the value of one if the firm is acquired in a given year, and zero otherwise. The indicator variable *IDD* takes the value of one if the IDD is recognized in a state, and zero otherwise. Variable definitions are provided in the Appendix. All continuous variables are winsorized at the 1st and 99th percentiles. P-values based on robust standard error clustered at the state level are reported in parentheses. The superscript ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	Dependent Variable =Acquisition		
	(1)	(2)	(3)
IDD	0.007** (0.032)	0.008** (0.017)	0.009** (0.010)
Log(total assets)		-0.001 (0.399)	-0.001 (0.371)
ROA		0.013** (0.032)	0.013** (0.031)
Excess return		0.001 (0.142)	0.001 (0.147)
Tobin's Q		-0.011*** (0.000)	-0.011*** (0.000)
Sales growth		-0.017*** (0.000)	-0.017*** (0.000)
Leverage		0.008 (0.235)	0.008 (0.230)
R&D		0.095*** (0.000)	0.095*** (0.000)
Fixed assets		0.027*** (0.003)	0.027*** (0.003)
Log (state population)			0.001 (0.859)
State GDP growth			0.000 (0.851)
State unemployment rate			-0.002* (0.097)
State establishment entry			-0.000 (0.769)
State establishment exit			0.001 (0.470)

Business combination law			-0.003 (0.425)
CNC index			0.020 (0.255)
Constant	-0.019 (0.108)	-0.003 (0.804)	-0.014 (0.812)
Region $\times$ Year FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Observations	122,367	122,367	122,367
R-squared	0.031	0.035	0.035

Table 4. Adoption of the IDD vs. Rejection of the Previously Adopted IDD

This table reports the difference-in-differences tests that separately examine the adoption of the Inevitable Disclosure Doctrine (IDD) and the rejection of the previously adopted IDD on the firm's likelihood of being acquired. In column (1), we estimate the effect of the IDD adoption; to avoid the confounding effects from the IDD rejections, we exclude all firm-year observations in states which reject the previously adopted IDD during our sample period. The indicator variable *IDD adoption* takes the value of one beginning the year when the state first recognizes the IDD, and zero otherwise. In column (2), we estimate the effect of the IDD rejection; the sample in this column starts in 1996, five years before the first rejection (Florida in 2001). To avoid the confounding effects of the IDD adoption, we exclude all firm-year observations in states which adopt the IDD after 1996. The indicator variable *IDD rejection* takes the value of one beginning the year when the state rejects its previously adopted IDD, and zero otherwise. The dependent variable *Acquisition* is an indicator variable, which takes the value of one if the firm is acquired in a given year, and zero otherwise. Variable definitions are provided in the Appendix. All continuous variables are winsorized at the 1st and 99th percentiles. P-values based on robust standard error clustered at the state level are reported in parentheses. The superscript ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	Dependent Variable =Acquisition	
	(1)	(2)
IDD adoption	0.010** (0.039)	
IDD rejection		-0.015** (0.027)
Log(total assets)	-0.002 (0.347)	-0.011*** (0.000)
ROA	0.013** (0.049)	0.025*** (0.001)
Excess return	0.001 (0.518)	-0.002 (0.149)
Tobin's Q	-0.011*** (0.000)	-0.009*** (0.000)
Sales growth	-0.017*** (0.000)	-0.014*** (0.000)
Leverage	0.009 (0.144)	-0.001 (0.941)
R&D	0.096*** (0.000)	0.034* (0.072)
Fixed assets	0.032*** (0.002)	0.019* (0.088)
Log (state population)	0.003 (0.514)	0.003 (0.577)

State GDP growth	0.000 (0.867)	-0.001 (0.294)
State unemployment rate	-0.001 (0.368)	-0.001 (0.676)
State establishment entry	-0.002 (0.236)	0.003 (0.464)
State establishment exit	-0.001 (0.641)	0.001 (0.840)
Business combination law	-0.004 (0.269)	0.000 (0.985)
CNC index	0.041 (0.203)	0.044 (0.107)
Constant	-0.027 (0.750)	-0.055 (0.599)
Region $\times$ Year FE	Yes	Yes
Firm FE	Yes	Yes
Observations	103,850	59,819
R-squared	0.035	0.044

Table 5. Testing for Pre-treatment Trends and Reversals

This table investigates the pre-treatment trends between the treated group and control group. The regression specification in column (1) is the same as that in column (1) of Table 4, except that we replace the *IDD adoption* indicator with the *IDD adoption*^{-2or-1}, *IDD adoption*⁰, *IDD adoption*¹, and *IDD adoption*²⁺ indicators. These four indicators flag the year relative to the state adoption of the IDD. The regression specification in column (2) is the same as that in column (2) of Table 4, except that we replace the *IDD rejection* indicator with the *IDD rejection*^{-2or-1}, *IDD rejection*⁰, *IDD rejection*¹, and *IDD rejection*²⁺ indicators. These four indicators flag the year relative to the state rejection of the previously adopted IDD. The dependent variable *Acquisition* is an indicator variable, which takes the value of one if the firm is acquired in a given year, and zero otherwise. Variable definitions are provided in the Appendix. All continuous variables are winsorized at the 1st and 99th percentiles. P-values based on robust standard error clustered at the state level are reported in parentheses. The superscript ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	Dependent Variable =Acquisition	
	(1)	(2)
IDD adoption ^{-2or-1}	-0.002 (0.631)	
IDD adoption ⁰	0.009 (0.158)	
IDD adoption ¹	0.009 (0.173)	
IDD adoption ²⁺	0.009** (0.041)	
IDD rejection ^{-2or-1}		-0.012 (0.121)
IDD rejection ⁰		-0.003 (0.808)
IDD rejection ¹		-0.027** (0.018)
IDD rejection ²⁺		-0.017** (0.045)
Log(total assets)	-0.002 (0.237)	-0.011*** (0.000)
ROA	0.013* (0.061)	0.025*** (0.006)
Excess return	0.001 (0.631)	-0.002 (0.165)

Tobin's Q	-0.011*** (0.000)	-0.009*** (0.000)
Sales growth	-0.017*** (0.000)	-0.014*** (0.000)
Leverage	0.009 (0.161)	-0.001 (0.945)
R&D	0.096*** (0.000)	0.034 (0.297)
Fixed assets	0.032*** (0.000)	0.019 (0.175)
Log (state population)	0.003 (0.576)	0.004 (0.560)
State GDP growth	0.000 (0.903)	-0.001 (0.288)
State unemployment rate	-0.001 (0.443)	-0.001 (0.605)
State establishment entry	-0.002 (0.276)	0.003 (0.347)
State establishment exit	-0.001 (0.687)	0.001 (0.803)
Business combination law	-0.005 (0.315)	-0.001 (0.957)
CNC index	0.044 (0.207)	0.044 (0.176)
Constant	-0.027 (0.786)	-0.061 (0.612)
Region $\times$ Year FE	Yes	Yes
Firm FE	Yes	Yes
Observations	103,850	59,819
R-squared	0.035	0.044

Table 6. Treated Firms and Neighboring Control Firms across State Borders

This table reports the difference-in-differences tests that examine whether the impacts of the Inevitable Disclosure Doctrine (IDD) on the firm's likelihood of being acquired are confounded by unobserved changes in local business conditions. For each treated firm, we match to a control firm that is in the same 3-digit NAICS industry, in a neighboring state without adopting the IDD, closest in firm size, and the distance between the treated firm and control firm is no more than 40, 50, 60, 80, and 100 miles in columns (1)-(5), respectively. The dependent variable *Acquisition* is an indicator variable, which takes the value of one if the firm is acquired in a given year, and zero otherwise. The indicator variable *IDD* takes the value of one if the IDD is recognized in a state, and zero otherwise. Variable definitions are provided in the Appendix. All continuous variables are winsorized at the 1st and 99th percentiles. P-values based on robust standard error clustered at the state level are reported in parentheses. The superscript ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	Dependent Variable =Acquisition				
	(1) Within 40 miles	(2) Within 50 miles	(3) Within 60 miles	(4) Within 80 miles	(5) Within 100 miles
IDD	0.023** (0.020)	0.022** (0.013)	0.016** (0.032)	0.018*** (0.003)	0.014*** (0.002)
Log(total assets)	-0.003 (0.161)	0.001 (0.692)	0.002 (0.421)	-0.001 (0.556)	-0.003* (0.065)
ROA	-0.039* (0.079)	-0.016 (0.428)	0.014 (0.470)	0.025 (0.153)	0.031** (0.034)
Excess return	-0.005 (0.255)	0.007* (0.059)	0.004 (0.214)	0.004 (0.148)	0.002 (0.390)
Tobin's Q	0.001 (0.706)	-0.005** (0.011)	-0.006*** (0.000)	-0.007*** (0.000)	-0.008*** (0.000)
Sales growth	-0.001 (0.852)	0.001 (0.851)	-0.002 (0.616)	-0.007* (0.071)	-0.006** (0.042)
Leverage	-0.023 (0.243)	-0.010 (0.554)	0.014 (0.395)	0.029** (0.038)	0.045*** (0.000)
R&D	0.124** (0.010)	0.157*** (0.001)	0.188*** (0.000)	0.225*** (0.000)	0.184*** (0.000)
Fixed assets	0.099*** (0.001)	0.053** (0.035)	0.044** (0.049)	0.012 (0.488)	0.000 (0.999)
Log (state population)	0.018* (0.094)	0.021* (0.057)	0.031*** (0.003)	0.017*** (0.009)	0.012** (0.017)
State GDP growth	-0.007*** (0.000)	-0.011*** (0.000)	-0.008*** (0.000)	-0.006*** (0.000)	-0.003*** (0.007)
State unemployment	-0.003	-0.012***	-0.011***	-0.009***	-0.005***

rate					
	(0.351)	(0.000)	(0.000)	(0.000)	(0.002)
State establishment entry	0.003	0.009***	0.008***	0.008***	0.006***
	(0.437)	(0.009)	(0.010)	(0.003)	(0.002)
State establishment exit	0.002	-0.005	-0.003	-0.006**	-0.003
	(0.713)	(0.236)	(0.354)	(0.037)	(0.144)
Business combination law	-0.051***	-0.031***	-0.025***	-0.029***	-0.023***
	(0.000)	(0.001)	(0.005)	(0.000)	(0.000)
CNC index	-0.070	-0.104*	-0.153***	-0.067*	-0.022
	(0.280)	(0.081)	(0.007)	(0.076)	(0.421)
Constant	-0.260	-0.227	-0.382**	-0.148	-0.136*
	(0.113)	(0.187)	(0.016)	(0.162)	(0.087)
Region $\times$ Year FE	Yes	Yes	Yes	Yes	Yes
Pair FE	Yes	Yes	Yes	Yes	Yes
Observations	10,395	13,566	17,027	24,110	35,061
R-squared	0.072	0.060	0.051	0.038	0.030

Table 7. Heterogeneous Treatment Effects

This table reports the double difference-in-differences tests to examine the heterogeneous treatment effects. The dependent variable *Acquisition* is an indicator variable, which takes the value of one if the firm is acquired in a given year, and zero otherwise. The indicator variable *IDD* takes the value of one if the IDD is recognized in a state, and zero otherwise. In column (1), the indicator variable *High human capital intensity* takes the value of one if the proportion of knowledge workers among all workers is above the sample median, and zero otherwise. *Low human capital intensity* is (1– *High human capital intensity*). In column (2), the indicator variable *High R&D* takes the value of one if the R&D expense is above the sample median, and zero otherwise. *Low R&D* is (1– *High R&D*). In column (3), the indicator variable *High Tangibility* takes the value of one if the firm’s tangible assets are above the sample median, and zero otherwise. *Low Tangibility* is (1– *High Tangibility*). In column (4), the indicator variable *Defined benefit plan* takes the value of one if the firm has a defined benefit plan, and zero otherwise. *Non-defined benefit plan* is (1– *Defined benefit plan*). In column (5), the indicator variable *Many rivals* takes the value of one if the number of firms in the same industry and same state is above the sample median, and zero otherwise. *Few rivals* is (1– *Many rivals*). Variable definitions are provided in the Appendix. All continuous variables are winsorized at the 1st and 99th percentiles. P-values based on robust standard error clustered at the state level are reported in parentheses. The superscript ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

VARIABLES	Dependent Variable =Acquisition				
	(1)	(2)	(3)	(4)	(5)
IDD*High human capital intensity	0.017*** (0.001)				
IDD*Low human capital intensity	0.003 (0.578)				
IDD*High R&D		0.013*** (0.000)			
IDD*Low R&D		0.005 (0.210)			
IDD*High tangibility			0.006 (0.160)		
IDD*Low tangibility			0.013*** (0.006)		
IDD*Defined benefit plan				-0.001 (0.785)	
IDD*None defined benefit plan				0.014*** (0.002)	
IDD*High local rivals					0.014*** (0.003)
IDD*Low local rivals					0.005 (0.159)

High human capital intensity	-0.009 (0.123)				
High R&D		-0.001 (0.811)			
High tangibility			0.010** (0.016)		
Defined benefit plan				0.005 (0.286)	
High local rivals					-0.012*** (0.005)
Log(total assets)	-0.001 (0.335)	-0.001 (0.386)	-0.001 (0.387)	-0.001 (0.351)	-0.001 (0.400)
ROA	0.014** (0.029)	0.013** (0.031)	0.013** (0.032)	0.013** (0.035)	0.013** (0.032)
Excess return	0.001 (0.149)	0.001 (0.157)	0.001 (0.159)	0.001 (0.165)	0.001 (0.148)
Tobin's Q	-0.011*** (0.000)	-0.011*** (0.000)	-0.011*** (0.000)	-0.011*** (0.000)	-0.011*** (0.000)
Sales growth	-0.017*** (0.000)	-0.017*** (0.000)	-0.017*** (0.000)	-0.017*** (0.000)	-0.017*** (0.000)
Leverage	0.008 (0.219)	0.008 (0.224)	0.008 (0.218)	0.008 (0.213)	0.008 (0.239)
R&D	0.095*** (0.000)	0.094*** (0.000)	0.094*** (0.000)	0.095*** (0.000)	0.096*** (0.000)
Fixed assets	0.027*** (0.003)	0.027*** (0.003)	0.017* (0.074)	0.026*** (0.004)	0.027*** (0.004)
Log (State population)	0.001 (0.872)	0.000 (0.920)	0.001 (0.876)	0.000 (0.893)	0.002 (0.485)
State GDP growth	0.000 (0.830)	0.000 (0.844)	0.000 (0.847)	0.000 (0.882)	0.000 (0.833)
State unemployment rate	-0.002 (0.117)	-0.002* (0.100)	-0.002 (0.110)	-0.002 (0.127)	-0.002* (0.093)
State establishment entry	-0.000 (0.770)	-0.000 (0.768)	-0.000 (0.794)	-0.000 (0.753)	-0.000 (0.783)
State establishment exit	0.001 (0.529)	0.001 (0.469)	0.001 (0.485)	0.001 (0.548)	0.001 (0.487)
Business combination law	-0.003 (0.386)	-0.003 (0.378)	-0.003 (0.430)	-0.002 (0.495)	-0.003 (0.417)
CNC index	0.017 (0.321)	0.018 (0.297)	0.020 (0.250)	0.024 (0.173)	0.023 (0.167)
Constant	-0.007 (0.900)	-0.008 (0.882)	-0.018 (0.757)	-0.015 (0.797)	-0.040 (0.488)

Region*Year FE	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Observations	122,367	122,367	122,367	122,367	122,367
R-squared	0.035	0.035	0.035	0.035	0.035

Table 8. The IDD and Executive Job-hopping

This table reports the difference-in-differences tests to examine the impact of the Inevitable Disclosure Doctrine (IDD) on executive job-hopping activities. The dependent variable *Job-hopping* is an indicator variable, which takes the value of one if the firm's executives leave the firm and join other firms in a given year, and zero otherwise. Executive job-hopping information is obtained from ExecuComp database from 1993 to 2013. In column (1), we estimate the effect of the IDD on executive job-hopping activities. In column (2), we follow the specification in column (1) of Table 4 to estimate the effect of the IDD adoption. In column (3), we follow the specification in column (2) of Table 4 to estimate the effect of the IDD rejection. Variable definitions are provided in the Appendix. All continuous variables are winsorized at the 1st and 99th percentiles. P-values based on robust standard error clustered at the state level are reported in parentheses. The superscript ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	Dependent Variable = Job-hopping		
	(1)	(2)	(3)
IDD	-0.016*** (0.002)		
IDD adoption		-0.017** (0.039)	
IDD rejection			0.017** (0.024)
Log(total assets)	0.016*** (0.000)	0.018*** (0.000)	0.016*** (0.000)
ROA	-0.031** (0.015)	-0.025** (0.037)	-0.029 (0.117)
Excess return	0.000 (0.927)	-0.001 (0.618)	0.002 (0.532)
Tobin's Q	-0.001 (0.357)	-0.001 (0.522)	-0.002 (0.116)
Sales growth	-0.006* (0.075)	-0.008** (0.017)	-0.006 (0.119)
Leverage	-0.001 (0.894)	-0.004 (0.710)	-0.008 (0.503)
R&D	-0.009 (0.811)	-0.001 (0.986)	0.012 (0.782)
Fixed assets	0.001 (0.947)	-0.007 (0.739)	0.011 (0.519)
Log (state population)	0.016*** (0.001)	0.017*** (0.003)	0.009* (0.093)
State GDP growth	0.001* (0.070)	0.001 (0.350)	0.001 (0.141)

State unemployment rate	-0.001 (0.683)	-0.002 (0.468)	-0.000 (0.965)
State establishment entry	-0.007** (0.012)	-0.005* (0.091)	-0.006** (0.021)
State establishment exit	-0.011*** (0.000)	-0.011*** (0.001)	-0.012*** (0.000)
Business combination law	0.001 (0.923)	0.003 (0.812)	-0.012 (0.361)
CNC index	-0.000 (0.995)	-0.010 (0.723)	-0.016 (0.624)
Constant	-0.192* (0.057)	-0.208** (0.050)	-0.080 (0.440)
Region $\times$ Year FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Observations	33,978	31,259	28,811
R-squared	0.012	0.012	0.011

Essay Two: Local Community's Preference and Corporate Social Responsibility: Evidence from Gay-Friendly Policy

Abstract

Why do firms behave socially responsible? Based on gay-friendly corporate policies as a setting, we find that catering to local community's preference is an important driver of corporate social responsibility policies. We show that firms are more likely to have gay-friendly corporate policies when they are headquartered in states with greater gay-friendly public opinion. This positive relation is more pronounced for firms with greater human capital intensity, and for firms with higher local institutional ownership. Furthermore, we show that a gay-friendly policy has a positive effect on firm performance when firms locate in gay-friendly states and has no such effect when firms locate elsewhere.

Keywords: Local Public Opinion; Stakeholder preference; Corporate Social Responsibility; Gay-friendly Policy; Firm Performance

JEL Classification: G32, G34, M14

1. Introduction

Huge amount of money is being spent on Corporate Social Responsibilities (CSR) by companies. In 2012, 67 percent of Fortune 100 Companies in the US produced a CSR report.⁷ The Global Fortune 500 companies spend more than \$15 billion on CSR in 2013.⁸ Cost saving may pull some firms away from being socially responsible, but what motivates firms to be socially responsible? What drives firms' CSR policies?

Academic proponents of CSR investments argue that CSR activities have a positive effect on shareholder wealth because caring stakeholders' interest increases their willingness to support a firm's operation, which will in turn benefit firms and shareholders (Jensen, 2001; Jawahar and McLaughlin, 2001; Freeman et al., 2004; Deng et. al., 2013; etc). Yet do firms in reality realize and approve this theory? If so, a value-maximizing firm should adopt CSR policies that are most appreciated by stakeholders and that could increase stakeholders' cooperation willingness most efficiently, which means firms should cater to stakeholder's preference when making CSR policies.

Anecdotal evidence shows that firms do cater to local stakeholders' preference for CSR activities. For example, Anec Dan Bross, Microsoft's Senior Director of Citizenship and Public Affairs claim that "Our citizenship mission is to serve the needs of communities around the world and to fulfill our responsibilities to the public. This has been part of our DNA for the past 30-plus years."⁹ Despite some circumstantial examples, there is little empirical evidence on how local community's preference

⁷ See Review of 2012 Fortune 100 Corporate Reports.
http://www.methodologie.com/stuff/contentmgr/files/0/f3294b37dcd9b2178135eaba379a268b/files/mth_report_ofreports_wtrends_20140422.pdf

⁸ See "Fortune 500 companies spend more than \$15bn on corporate responsibility," *Financial Times*, Oct 12, 2014.

⁹ See "The Companies with the Best CSR Reputations" *Forbes*, Dec 11, 2012.

influences corporate decision on CSR. This lack of evidence is probably because (1) it is difficult to measure local community's preference, and (2) there is a lack of variation in many of these preferences (for example, everyone prefers cleaner water, fresher air, less pollution, etc.). In this paper, we fill this gap and empirically reveal the importance of local community's preference in driving corporate CSR policies, by focusing on corporate gay/lesbian-friendly¹⁰ policy.

We focus on gay/lesbian-friendly policy (one aspect of CSR) for the following reasons. First, the public opinion towards homosexuality is available; Pew Research Center conducts public polls containing questions about people's attitude towards homosexuality across the U.S., which makes it possible to measure the local community's preference. Second, there is large variation in public opinions towards homosexuality across regions and across time. For example, in 2009 the top five states with the highest gross support rate for the same-sex marriage are Rhode Island (35%), North Dakota (34%), Massachusetts (34%), New Jersey (31%) and Maine (28%), while the bottom five states with the least gross support rate are Wyoming (5%), Kentucky (6%), Mississippi (6%), New Mexico (7%) and Utah (8%). This great variation in public opinions gives opportunities to implement meaningful empirical tests to investigate how corporate gay-friendly policies vary with local people's attitude towards homosexuality. Lastly, the direct monetary cost to implement gay/lesbian friendly policy should be minimal, which means firms can choose freely between gay-friendly and gay-unfriendly policies without much financial concern. This allows us to test the catering hypothesis in a clear setting. In contrast, other CSR policies, such as installing better waste management systems or using

¹⁰ Throughout this paper, gay people include both lesbians and gays. Gay and homosexual are used interchangeably; gay-friendly and gay/lesbian-friendly are used interchangeably.

environmental-friendly raw materials, would incur substantial financial costs and thus may jeopardize firms' ability to adopt such policies even if firms wish to cater to local preferences.

We use state level gross and net support rate for same-sex marriage from survey data conducted by Pew Research Center as a proxy for local preference for gay-friendliness. We use local community's preference for two reasons. First, local stakeholders, including local investor, local employees, local lenders, local suppliers, local customers, and local communities should be of special importance to firms. Second, from firms' perspective, local stakeholders' preferences, in contrast to remote stakeholders' preferences, are more observable. If firm are to cater to some stakeholders' preferences, naturally they would cater to those preferences they can observe.

We expect firms more likely to have a gay-friendly policy when the local community has a more favorable opinion towards homosexuality and less likely to have such policies when local community has negative opinions. Supporting the prediction above, we find that firms are more likely to have gay-friendly corporate policies when they are headquartered in states with high support rates for gay-marriage than in states with low support rates for gay-marriage. We find an increase in gross support rate for gay marriage by one standard deviation (11.42%) will lead to an approximately 0.95% increase in probability for the firm to have gay-friendly corporate policies, compared to the unconditional probability of 15%.

We also examine the adoption of gay-friendly policy and local public opinion. We find that in states with high support rate for gay marriage, gay-unfriendly firms are more

likely to switch to gay-friendly, while in states with low support rate gay-unfriendly firms are more likely to remain gay-unfriendly.

Next, we implement additional tests to provide further evidence that the positive relation between local community's gay attitude and corporate gay-friendly policy is truly due to firms' catering to local community's preference. The positive relation between local public opinion and corporate policy should be stronger if the firm rely more on local stakeholders. We propose two channels through which local community's preference influences corporate decisions, namely local investor channel and employee channel. We show that the positive relation between local public opinion towards gay marriage and corporate gay-friendly policy is more pronounced when firms have high local institutional ownership, and when human capital is of high importance to firms.

Finally, we shed lights into the valuation effect of CSR on firm. Does the catering behavior payoff or does the CSR policy itself payoff? We find that gay-friendly corporate policy has a positive effect on firm performance only when local community is more favorable to homosexuals. However, while in places where local people are hostile to homosexuals, gay-friendly corporate policy has no effect on firm performance. Overall, our findings suggest that firms make CSR in response to local community's preference, and CSR increases firm performance only when local community appreciate such activities.

Our research contributes to the literature in several ways. Firstly, past empirical papers on CSR all focus on the "supply" side of CSR and usually attribute the motivation for CSR activities to internal factors, such as high cost of equity capital (Dhaliwal et al., 2011), political affiliation (Rubin, 2008; Di Giuli and Kostovetsky, 2014), agency

problem (Hemingway and MacLagan, 2004; Krüger, 2015), etc. To the best of our knowledge, we are the first paper that empirically examines the "demand" for CSR. We identify an external factor, local community preference to be a strong driving force for CSR activities.

Second, despite the growing importance of CSR in U.S. firms' operations, the effect of CSR on firm performance is still under debate. One group of researchers argues that CSR results in positive effects because focusing on the interests of other stakeholders increases their willingness to support a firm's operation, which in turn increases the firm's performance (Deng et al., 2013; Jensen, 2001). In contrast, other groups of researchers believe that CSR is a wealth transfer from shareholders to other stakeholders and thus reduces firm performance (Cronqvist et al., 2009; Friedman, 1988; Pagano and Volpin, 2005; Di Giuli and Kostovetsky, 2014). Our paper links the two views and shows that the effect of CSR on firm value depends on local community's attitudes towards the CSR matter: CSR increases firm value only when such CSR is appreciated by local community

Lastly, our paper also adds to the literature that examines how firms' headquarter location influences corporate policies. Becker et al. (2011) find that firms pay more dividends when seniors constitute a large fraction of the local population. Deng and Gao (2013) find the firms pay CEO more when local living environment is poorer. Kedia and Rajgopal (2009) find the geographic location of a firm impacts its broad based stock option plan. Di Giuli and Kostovetsky (2014) find firms engage in more CSR activities when they are headquartered in Democratic-leaning than in Republican-leaning states.

Our paper reveals that firms' local community's attitude plays an important role in shaping firms' employment policies.

The remaining of the paper is organized as follows. In Section 2 we describe the data used in the paper and provide summary statistics. Section 3 presents the empirical results. We conclude the paper in Section 4.

2. Sample Formation and Variable Construction

2.1 Measure of corporate gay-friendly policy

We measure a firm's gay-friendliness using "Gay and Lesbian Policies" ratings (DIV_STR_G) from the Kinder, Lydenberg, and Domini (KLD) database, which is widely used by researchers to study corporate social responsibility as well as gay-friendly corporate policies in particular. For example, Deng et al. (2013) and Di Giuli et al. (2014) use it to study corporate social responsibility, Fombrun (1998) construct indices of corporate reputational rankings and ratings, which uses the KLD ratings of gay and lesbian policies. Blazovich, Cook, Huston, and Strawser (2013) use it to study gay-friendly corporate policies and firm value.

A variety of data source, including filings of companies, data from government and nongovernment organizations, media sources, and direct communication with company employees is used by KLD to assign ratings. Each year KLD assigns 0/1 ratings for the absence or presence of various strength and concerns in a firm, which are categorized into seven major areas: environment, community, corporate governance, diversity, employee relations, human rights, and product quality.

"Gay and Lesbian Policies" is a subcategory under diversity, and it evaluates whether

“the company has implemented notable progressive policies toward its gay and lesbian employees”. This subcategory was added by the KLD in 1995, and was originally titled the Progressive Gay/Lesbian Policy strength. Our main variable *gay-friendly* is a dummy variable, which takes the value of 1 if the firm has implemented notable progressive policies toward its gay and lesbian employees and is considered gay-friendly, and 0 otherwise.

We also compute the sum of the strength of the rest subcategories minus the sum of the concerns in all subcategories as a control variable, *CSR*, to control for firms' overall performance in corporate social responsibility.

2.2 Measure of local community's attitudes to homosexuals

We use local state public opinion towards gay-marriage to proxy for the local attitudes toward gay. The data is from multiple public polls conducted by Pew Research Center for the People & the Press. The Pew Research Center for the People & the Press is a subsidiary of the Pew Charitable Trusts and it conducts public opinion polling, provides independent public opinion survey research about American attitudes toward politics and policy, and implements other empirical social science research as well. The data is available at the website <http://www.pewresearch.org/data>.

We use all national wide landline and cell phone surveys from 2003 to 2009 which cover questions about respondents' attitude towards gay marriage. All survey respondents are randomly selected from across the continental United States, and the state information of each respondent allows us to estimate the public opinion of each state.

The specific wording of the question regarding gay marriage is “Do you strongly

favor, favor, oppose or strongly oppose gay and lesbian couples to marry legally?” The answers are organized into five categories: strongly favor, favor, oppose, strongly oppose and neutral. We compute the proportion of interviewees choosing strongly favor or favor as gross support rate for gay marriage, denoted as *Gross support*, and the proportion choosing strongly favor or favor minus the proportion of strongly oppose and oppose as net support rates for gay marriage, denoted as *Net support*. If there is more than one survey in a year, we aggregate the survey data by using the arithmetic average of percentages.

2.3 Sample selection

Our sample consists of all firms covered by the KLD database with non-missing “Gay and Lesbian Policies” ratings from 2003 to 2009 (17,783 firm-year observations). We start in 2004 because the first poll conducted by Pew Research Center that contains the question regarding gay marriage appeared in 2003 and we apply one year lag between public opinion and corporate policy. We end in 2009 because KLD discontinued collecting and reporting many diversity strength and concern ratings after 2009.

We then obtain firm financial information from Compustat. We obtain firm’s headquarter information from Compustat, Compact Disclosure (which records headquarters’ changes), and SEC filing. We exclude firms in Alaska and Hawaii because poll information from Pew Research Center is not available for these two states. We also exclude firms headquartered outside the United States. Our final sample consists of 16,993 firm-year observations from 2003 to 2009.

We control for a few state-level variables, which are from various sources. Gay

population is the percentage of same-sex couples among all households in the U.S. from American Community survey (ACS) conducted by US Census Bureau. State GDP per capital is from the Bureau of Economic Analysis. State level population, percentage of white people, percentage of senior citizens, which is defined as 60 years old and above, percentage of high school graduates, percentage of bachelor's degree holders, sex ratio and insurance coverage, which counts both private and government health insurance are all obtained from the Current Population Survey conducted by the US Census Bureau. Marriage rate is measured as provisional counts of marriage per 1000 population and is obtained from National Vital Statistics System. State unemployment rate is from U.S. Bureau of Labor Statistics Local Area Unemployment Statistics Series. Percentage of religious people is defined total religious adherence among whole population from the 2000 and 2010 census. Due to lack of annual data, we use the 2000 census religious people data for the period of 2003 to 2005 and the 2010 census data for 2006 to 2009. All continuous variables are winsorized at the 0.5% and 99.5% levels and all dollar value variables are measured in 2009 constant dollars. The detailed description of the construction of the variables is provided in the appendix.

2.4 Summary statistics

In Table 1 we present the summary statistics of the sample firms. About 15% of sample firms-year observations reports gay-friendly corporate policies during our sample period. Median *Gross support* is 33% and median *Net support* is -16%, which indicate a general negative public opinion towards gay marriage coinciding with the lack of popularity of gay-friendly corporate policies. Our average sample firms have book value

assets of \$8.32 billion, score -0.66 on CSR, and are 22 years old. They hold a sizeable amount of cash with a cash ratio of 18% of total assets and are moderately levered with a book leverage ratio of 19%. The average R&D and capital expenditure account for 3% and 4% of total assets, respectively. In terms of performance, sample firms perform well with an average ROA of 10%, Tobin's Q of 1.91 and yearly market adjusted stock return of 7.8%.

Table 2 presents the gay-friendly policy and support rate for gay marriage across states. The top 5 states with the highest gross approval ratings are Vermont (53%), Rhode Island (51%), District of Columbia (51%), Connecticut (45%) and Massachusetts (44%), and all of them have positive net support rates. California and New York are ranked 6th and 9th, respectively among the 48 continental states and the District of Columbia. The bottom states with the lowest support rates are Alabama (14%), Arkansas (15%), Mississippi (18%), West Virginia (18%) and Tennessee (18%), and all of which have very negative net support rates. The top ten states have clearly higher percentage of gay-friendly firms than the bottom ten states.

Table 3 shows that average support rates for gay marriage and percentage of gay-friendly firms across all states by year. Both gross and net support rates grow steadily by year, with two exceptions in 2005 and 2009, where the support rates decline slightly. The increase reflects a more acceptable social view towards homosexuality. Correspondingly, the proportion of gay-friendly firms also increases with time, with also two exceptions in 2008 and 2009. In 2004, the first year of our sample period, only 9% of firms adopted gay-friendly policies; and by 2009, 17% of firms have done so.

3. Empirical Results

3.1 Corporate Gay-Friendly Policy and Local Public Opinion

We estimate the following logistic regression to examine whether corporate gay-friendly policy is related to local public opinion about gay-friendliness.

$$\begin{aligned} \text{Logit Gay friendly} = & \alpha + \beta_1 \text{Gross /Net support} + \text{Firm characteristic controls} \\ & + \text{State level controls} + \text{Industry FEs} + \text{Year FEs} + \varepsilon \end{aligned} \quad (1)$$

In Columns (1) to (3) of Table 4, we use gross support rate for gay marriage as a proxy for local demand for gay-friendliness and in Columns (4) to (6) we use net support rate. We report the marginal effect of the independent variables, where marginal effects are measured at their mean value.

In Column (1) and (4), we include only *Gross support* or *Net support* and industry and year fixed effects in the regressions. Industries are defined as Fama-French 48 industries. The estimated coefficients on *Gross support* and *Net support* are 0.377 and 0.168 respectively, both significant at the 1% level.

In Column (2) and (5), we add firm characteristics, including size, firm age, corporate social responsibilities (*CSR*), *ROA*, *R&D* expenditure, capital expenditure, leverage, cash, Tobin's *Q*, and 12-month market adjusted return and still obtain significant and positive estimated coefficients on *Gross support* and *Net support*. The tables also shows that relatively larger, older, more social responsible, more profitable firms with higher *R&D* intensity and higher cash holding are more likely to have gay-friendly corporate policies, while capital expenditure, leverage, and Tobin's *Q* do not appear to have significant relationship with corporate gay-friendly policies.

We further control for state characteristics by including state level GDP per capital, population, unemployment rate, percentage of senior citizen, percentage of white people, percentage of high school graduates, percentage of Bachelor's degree holders, marriage rate, sex ratio, and percentage of religious people in the regression and report results in Column (3) and (6). We control for GDP per capita and unemployment rate, which measures the local economic condition. We control for percentage of gay population because contact with lesbian and gay people leads to more support for pro-gay public policies (Barth and Parry, 2009) and similarly managers' contact with homosexual people may drive them to support gay-friendly corporate policies. Psychologists and sociologists have identified several factors that may influence attitudes towards homosexuality, including gender, age, race, education, family structure, gender role orientation, previous contact with gay people, religious beliefs, fear of AIDS (Cotten-Huston and Waite, 1999; Loftus, 2001; Lewis, 2004; Eldridge, Mack, and Swank, 2006; McVeigh and Diaz, 2009), etc., so we also include all relevant demographic characteristics in our regression.

After controlling for all firm characteristics and state characteristics, the estimated coefficients on *Gross support* and *Net support* remain positive and significant at the 5% level and economically significant: an increase in gross support rate for gay marriage by one standard deviation (11.42%) will lead to an approximately 0.95% increase in probability for the firm to have gay-friendly corporate policies, compared to the unconditional probability of 15%. Similarly, an increase in net support rate by one standard deviation (22.41%) leads to an increase of gay-friendly corporate policy by 0.96%. We also find that local gay population proportion, local senior citizen proportion and local religious people proportion affect corporate gay-friendly policies, while other

local economic and demographic characteristics do not seem to affect gay-friendly policies significantly.

In summary, Table 4 shows a significant and robust positive association between corporate gay-friendly policy and local gay-friendliness, which indicates that local community preference is an important determinant for CSR strategies.

It is possible that our results are mainly driven by the states of California and New York, because these two states account for about 17% and 8% of our sample, respectively, and both states have high support rate for the same-sex marriage.¹¹ To investigate the possibility that our results are mainly driven by these two states, we perform additional tests excluding observations in these two states.

In Table 5 Column (1), we remove the firms in California and the coefficient on *Gross support* is 0.084 and significant at the 1% level. We remove the firms in New York in Column (2), and the coefficient on *Gross support* is still positive and significant at the 5% level. Further excluding the observations in both California and New York, we continue to find a positive and significant (at the 5% level) coefficient on the *Gross support* variable. In Columns (4)-(6), we repeat our analysis based on the net support rate and we obtain similar results. Thus, the positive relation between local support for the same-sex marriage and the corporate gay-friendly policy is not mainly driven by California or New York.

3.2 The Adoption of Corporate Gay-Friendly Policy and Local Public Opinion

Although we find positive correlation between firms' gay-friendly policy and local

¹¹ As shown in Table 2, Californian ranks the 6th and New York ranks the 9th among the 48 continental U.S. states in terms of gross support rate for the same-sex marriage.

community's gay-friendliness, there is reverse causality concern. The positive correlation could be due to gay-friendly firms seeking out gay-friendly states to set up their headquarters rather than these firms catering to local public opinion. We study the adoption of corporate gay-friendly policy and local public opinion to alleviate this concern.

Theoretically, if gay-friendly firms seek out gay-friendly states to set up their headquarters, they should appear as gay-friendly since establishment or since relocation if they relocate their headquarters to gay-friendly states, and the positive correlation we find between local public opinion and firms' gay-friendly policy should be concentrated in these firms. So we exclude these firms, only look at the firms that were initially gay-unfriendly, and examine what drives the change of corporate policy.

We define *Adoption* as an indicator variable that is equal to one if a firm is gay-friendly in year t and was gay-unfriendly in year $t-1$, and zero otherwise. We restrict the sample to firms that start in our sample as gay-unfriendly, and we exclude firms after their first year of adoption because they can no longer "adopt" the policy. We also exclude firm that ever relocate headquarters in our sample period. After these restrictions, we have 9,820 firm year observations from 2004 to 2009, and the mean of *Adoption* is 0.017. We then re-estimate the baseline regression by replacing the dependent variable with *Adoption* and using the restricted sample. The results are reported in Table 6. The marginal effect of *Gross Support* is 0.017, and the marginal effect of *Net Support* is 0.008, both significant at the 5% level. It means that in states with high support rate for gay marriage, gay-unfriendly firms are more likely to switch to gay-friendly, while in states with low support rate gay-unfriendly firms are more likely to remain gay-

unfriendly. This test alleviates the reverse causality concern that our results are driven by gay-friendly firms seeking out gay-friendly states to set up their headquarters.

3.3 The Effect of Human Capital Intensity

Employees should be an important channel through which local community's preference could influence corporate gay-friendly policies. Past literatures suggest that firms take employees' benefits into consideration when making certain decisions, for example, Bae et al. (2011) show firms maintain low debt ratio to provide fair treatment to their employees. We think firms should take employees' preference into consideration when making gay-friendly corporate policies. To the extent that the firm's employees are likely to be part of the local community and thus local community's preference should largely represent employee preference, we expect the positive relation between local community's opinion towards gays and corporate gay friendly policy to be more pronounced for firms with greater human capital intensity.

We use three variables to measure the importance of human capital to firms: proportion of knowledge workers, R&D expense, and labor intensity. We then split our samples three times based on whether each measure is above or below the sample median for the respective characteristic, and repeat our baseline model in the sub-samples separately to see how firms are influenced by local community preferences when human capital is of special importance to firms and when it is not.

First, following prior research (e.g. Farjoun, 1994; Coff, 2002; Klasa et al., 2014; Younge et al., 2015), we measure the level of knowledge workers as a proportion of total workers in the industry. We obtain employment data from Integrated Public Use

Microdata Series (IPUMS-USA) database, which provide annual American Community Survey (ACS) data on individual worker's occupational code, industry, state, etc. Based on IPUMS occupational codebook, we define knowledge workers to be those with a Standard Occupational Classification code below 300000. This definition includes managers, professionals, arts, science and technical occupations. From the IPUMS data, we calculate the proportion of the total workforce being knowledge workers for a given three-digit NAIC industry in a given year, and then assign that measure to each firm in our sample.

Second, human capital, rather than physical capital, is particularly important for high-R&D industries (Zingales, 2000). Thus, we also use R&D expenditure scaled by total assets as our second measure of human capital importance.

Lastly, following Dewenter and Malatesta (2001) and Ouimet and Zarutskie (2014), we use the number of employees scaled by fixed assets as our third measure of the firm's human capital intensity.

In Table 7 Panel A, we use *Gross support* to measure the local attitudes towards gay. In Columns (1) and (2), we split the sample into *Low knowledge worker* and *High knowledge worker* subsamples. In the former subsample, the coefficient on *Gross support* is 0.05 and is not significantly different from zero. In the latter subsample, the coefficient on *Gross support* is twice as big (0.103) and is significant at the 5% level. Thus, the positive relation between local attitude towards gay and corporate gay-friendly policy is mainly driven by firms with a greater level of knowledge workers.

In Columns (3) and (4), we split the sample into low and high R&D subsamples, and we find that the coefficient on *Gross support* is a significant 0.185 (at the 1% level) in

high R&D subsample and is insignificant in the low R&D subsample. This indicates that the positive relation between local attitude towards gay and corporate gay-friendly policy is mainly driven by high R&D firms.

Lastly, in Columns (5) and (6), we split the sample into low and high employee intensity subsamples, and we find that the coefficient on *Gross support* is a significant 0.136 (at the 1% level) in high employee intensity subsample and is insignificant in the low employee intensity subsample. This indicates that the positive relation between local attitude towards gay and corporate gay-friendly policy is mainly driven by firms with higher employee intensity.

We repeat our analysis by using net support rate in Panel B, and we find similar results. Overall, our results suggest that firms are strongly influenced by local community preferences when human capital is particularly important.

3.4 The Effect of Local Institutional Ownership

Investors are another group of important stakeholders. If a firm's shareholder base consists of a larger proportion of local investors, then we expect this firm to be more likely to cater to local community's preference, which we call the local investor channel.

We collect institutional ownership information from Thomson Reuters institutional (13f) holdings and we find each institution's primary addresses from Thomson One database and Capital IQ database, which provide firms' address information. We define an institution as a local institution if the institutional investor's primary address is in the same state as the firm.

In Table 8, we show regression results using low local institutional ownership sub-

sample in Column (1) and Column (3), and results using high local institutional ownership sub-sample in Column (2) and (4). Clearly, firms with high local institutional ownership are more responsive to local preference when making their gay-friendly policies. In the low local institutional ownership subsample (Column (1)), the coefficient on *Gross support* is an insignificant 0.017. In contrast, the corresponding coefficient on *Gross support* in the high local institutional ownership subsample (Column (2)) is much larger in magnitude (0.171) and is significant at the 1% level. Replacing *Gross support* with *Net support* in Columns (3) and (4), we find similar results: the coefficient on *Net support* is not significantly different from zero in the subsample of low local institutional ownership, and is positively significant and much larger in magnitude in the subsample of high local institutional ownership. In summary, our results show that a firm with high local institutional ownership is responsive to local community's preference when making gay-friendly corporate policies.

3.5 Gay-friendly Corporate Policy, Local Community Preference, and Firm Performance

The effect of CSR on firm performance is under debate. Proponents argue that CSR results in positive effects because focusing on the interests of other stakeholders increases their willingness to support a firm's operation, which in turn increases the firm's performance (Deng et al., 2013; Jensen, 2001); while opponents view CSR as expense to shareholders. (Cronqvist et al., 2009; Friedman, 1988; Pagano and Volpin, 2005; Di Giuli and Kostovetsky, 2014). Our theory links the two views together: If a firm's CSR policy efficiently makes stakeholders happier and increases their willingness to support, such policy will improve firm's performance; if a firm's CSR policy is not appreciated by

stakeholders, it becomes a burden to firm performance.

We still use gay-friendly policy to test this hypothesis. Theoretically, in high support states, more local stakeholders favor gay-friendly policy and are more supportive to gay-friendly firms, which will help improve those firms' performance; while in low support states, local stakeholders favor less or even hate gay-friendly policy and are less supportive to gay-friendly firms, which should lead to weaker firm performance.

We use ROA as a measure of operation performance and Tobin's Q as a measure of stock market performance. Based on the sample median gross support rates in a given state, we split our sample into the ones with high and low support rates. We run lead-lag regressions with the ROA and Tobin's Q as dependent variables and gay-friendly policy and other firm characteristics as the independent variables.

The results are reported in Table 9. In Columns (1) and (2), the regression is based on the firms in states with high support rate for the same-sex marriage. We find that the coefficients on the *Gay friendly* indicator are positive and significant at or lower than the 10% level. All else equal, gay-friendly policy increases ROA by 0.5% and Tobin's Q by 4.9%, which are both statistically and economically significant.

In contrast, for firms in low support states (Columns (3) and (4)), gay-friendly policy has no effect on firm performance at all. The coefficients on the *Gay friendly* indicator are basically zero and not significantly different from zero, which suggests that having gay-friendly policy does not improve performance if firm is located in low support area.

Our findings have implications for firms when they are making CSR investment. The effect on CSR on performance depends on local community's preference towards such CSR activities. If local community appreciates such activity, then engaging in such CSR

may have a positive effect on performance value. In contrast, when local community does not appreciate such activity, then engaging in it may not necessarily increase performance.

4. Conclusion

In this paper, we investigate the influence of local community's preference on corporate social responsibility, by focusing on corporate gay-friendly employment policy. Gay-friendly policy is a good setting for this research largely because people in the U.S. are likely to have very different views towards homosexuality (thus, greater variation in preference). Moreover, as the gay rights become a more and more important issue in the U.S. society, having a gay-friendly employment policy becomes a significant consideration for the U.S. corporations.

We find that firms are more likely to have a gay-friendly policy when their local community is more friendly to gay. This relation is more pronounced for firms that are less financially constraint, for firms that rely more on human capital, and for firms with greater local investor base. We further find that adopting gay-friendly policy increases firm performance only when the local community is also friendly to gay and adopting gay-friendly policy has no effect on firm performance otherwise. Overall, our findings suggest that local community's preference is an important driver of corporate social responsibility.

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Appendix. Variable Definitions

Variable	Definition
Adoption	An indicator variable that is equal to one if a firm is gay-friendly in year t and was gay-unfriendly in year t-1, and zero otherwise
Gay-friendly	An indicator variable, which is the 0/1 rating assigned by KLD in the category of "Gay and Lesbian Policies".
Gross support	Percentage of gay marriage supporters in a given state.
Net support	Percentage of gay marriage supporters net of percentage of gay marriage opponents in a given state.
Total Assets	Book value of total asset (US\$ millions).
Firm age	Number of years since the firm's first appearance in Compustat.
CSR	Corporate social responsibility, measured as the sum of environment, community, corporate governance, diversity, employee relations, human rights, and product quality strengths except for "Gay and Lesbian Policies" in the KLD database minus the sum of all concerns.
ROA	Return on assets, measured as operating income before interest, tax, depreciation and amortization divided by the book value of total assets.
R&D	Research and development expenditures divided by the book value of total assets. If R&D expenditures variable is missing, we set the missing value to zero.
Capex	Capital expenditures divided by the book value of total assets.
Leverage	Book value of long-term debt divided by the book value of total assets.
Cash	Cash and marketable securities divided by the book value of total assets.
Tobin's Q	Book value of total assets minus book value of equity plus market value of equity minus balance sheet deferred taxes, divided by book value of total assets.
Year return	One-year buy-and-hold abnormal return.
Gay population	Percentage of same-sex couples among all households in the U.S.
GDP per capital	GDP per capital in a given state (US \$ thousand).
Population	Total population in a given state (thousand).
Unemployment rate	Unemployment rate in a given state.
White people	Percentage of white people in a given state.
Senior citizen	Percentage of people above 60 in a given state.
High school graduates	Percentage of high school graduates in a given state.
Bachelor's degree	Percentage of people that have Bachelor's or higher degrees in a given state.
Marriage Rate	Provisional counts of marriage per 1000 population in a given state.
Sex Ratio	Male population to female population in a given state.
Insurance Coverage	Percentage of people that are covered by either private or government health insurance in a given state.
Religious people	Total religious adherence divided by total population in a given state.

Table 1. Summary Statistics

The sample consists of 16,933 firm-year observations from 2003 to 2009. All the firms are covered by the KLD database with non-missing “Gay and Lesbian Policies” ratings and by Compustat with non-missing state information. We use poll information from Pew Research Center for the People & the Press data archive from 2003 to 2009. We use all national wide polls conducted by Pew Research Center containing the specific question “Do you strongly favor, favor, oppose, or strongly oppose allowing gay and lesbian couples to marry legally?” We exclude firms in Alaska and Hawaii because poll information is not available for these two states. All dollar value variables are measured in 2009 constant dollars and all continuous variables are winsorized at the 0.5% and 99.5%. The detailed description of the construction of the variables is provided in the appendix.

Variable	N	Mean	Std. Dev	P25	Median	P75
Gay-friendly	16933	0.15	0.36	0	0	0
Gross support	16933	32.91%	11.42%	24.46%	33.32%	40.43%
Net support	16933	-16.54%	22.41%	-33.52%	-15.56%	2.44%
Total assets (\$ million)	16933	8320.04	33750.78	417.40	1253.45	3819.22
Firm age	16933	21.77	15.64	10.00	16.00	31.00
CSR	16933	-0.66	2.00	-2.00	-1.00	0.00
ROA	16933	9.62%	13.58%	3.17%	10.29%	16.36%
R&D	16933	3.47%	7.68%	0.00%	0.00%	3.26%
Capex	16933	4.37%	5.76%	0.85%	2.58%	5.47%
Leverage	16933	18.71%	20.22%	0.84%	13.10%	29.40%
Cash	16933	17.68%	20.64%	2.85%	8.80%	25.47%
Tobin's Q	16933	1.91	1.30	1.11	1.46	2.19
Yearly return	16933	7.79%	51.61%	-20.80%	-0.80%	23.24%
Gay population	16933	0.66%	0.15%	0.56%	0.65%	0.76%
GDP per capital (\$ thousand)	16933	47.29	9.72	42.30	46.72	51.58
Population (million)	16933	15.04	11.33	6.03	11.48	22.39
Unemployment rate	16933	5.91%	1.75%	4.84%	5.42%	6.60%
White people	16933	79.69%	7.65%	75.06%	79.56%	85.42%
Senior citizen	16933	21.14%	2.27%	19.05%	21.37%	22.53%
High school graduates	16933	84.67%	3.61%	81.10%	85.40%	87.50%
Bachelor's degree	16933	28.70%	4.54%	25.20%	29.00%	31.70%
Marriage Rate	16933	0.70%	0.31%	0.60%	0.65%	0.73%
Sex Ratio	16933	0.49	0.01	0.49	0.49	0.50
Insurance Coverage	16933	85.29%	4.61%	82.20%	86.00%	88.70%
Religious people	16933	50.54%	7.98%	45.00%	51.21%	55.65%

Table 2. Gay-Friendly Policy and Local Public Opinion across States

The sample consists of 16,993 firm-year observations from 2003 to 2009. All the firms are covered by the KLD database with non-missing “Gay and Lesbian Policies” ratings and by Compustat with non-missing state information. We use poll information from Pew Research Center for the People & the Press data archive from 2003 to 2009. We use all national wide polls conducted by Pew Research Center containing the specific question “Do you strongly favor, favor, oppose, or strongly oppose allowing gay and lesbian couples to marry legally?” We exclude firms in Alaska and Hawaii because poll information is not available for these two states. All dollar value variables are measured in 2009 constant dollars and all continuous variables are winsorized at the 0.5% and 99.5%. The detailed description of the construction of the variables is provided in the appendix.

State	Gross support	Net support	# of firm-year obs	Gay-friendly firms (%)
Vermont	52.98%	26.19%	21.00	9.52%
Rhode Island	51.27%	26.69%	44.00	54.55%
District of Columbia	50.59%	15.23%	92.00	25.00%
Connecticut	45.18%	8.77%	396.00	16.92%
Massachusetts	44.27%	10.61%	836.00	16.27%
California	42.17%	3.40%	2896.00	20.93%
New Hampshire	41.93%	-4.71%	43.00	16.28%
Maine	41.47%	7.69%	24.00	33.33%
New York	39.78%	0.55%	1393.00	22.33%
Washington	39.62%	-1.94%	313.00	26.84%
New Jersey	38.59%	-1.30%	643.00	18.35%
Nevada	37.85%	-3.81%	118.00	8.47%
Oregon	37.66%	-5.52%	170.00	24.71%
New Mexico	34.40%	-20.39%	16.00	0.00%
Colorado	33.67%	-11.59%	315.00	12.06%
Minnesota	33.19%	-15.89%	411.00	24.57%
Illinois	32.88%	-15.36%	817.00	14.08%
Maryland	32.82%	-15.85%	330.00	16.06%
Wisconsin	32.74%	-17.18%	279.00	9.68%
Arizona	32.54%	-16.32%	218.00	8.72%
Pennsylvania	32.12%	-18.73%	870.00	10.00%
Montana	29.84%	-21.31%	28.00	0.00%
Florida	29.59%	-22.41%	601.00	7.99%
Michigan	29.41%	-25.35%	356.00	18.26%
Iowa	28.19%	-27.90%	115.00	19.13%
South Dakota	28.18%	-20.26%	35.00	0.00%
Ohio	27.51%	-28.02%	641.00	11.23%
Virginia	26.94%	-32.64%	460.00	13.26%
Kansas	26.00%	-26.89%	79.00	7.59%
Nebraska	25.98%	-33.35%	73.00	5.48%
Indiana	25.43%	-35.53%	242.00	11.98%
Idaho	25.40%	-42.80%	43.00	0.00%
Missouri	24.53%	-36.38%	312.00	10.26%
Texas	24.18%	-36.95%	1548.00	8.27%
Delaware	21.76%	-37.16%	67.00	10.45%
Georgia	21.70%	-44.56%	441.00	17.01%
North Carolina	20.79%	-45.48%	350.00	8.29%

Wyoming	20.16%	-50.32%		
Louisiana	18.69%	-49.11%	146.00	4.11%
Utah	18.38%	-52.63%	110.00	3.64%
Kentucky	18.34%	-53.11%	132.00	12.12%
South Carolina	18.28%	-50.86%	84.00	0.00%
Oklahoma	18.13%	-51.32%	140.00	0.71%
Tennessee	17.74%	-53.66%	322.00	5.59%
West Virginia	17.62%	-52.22%	36.00	0.00%
North Dakota	17.56%	-53.12%	17.00	0.00%
Mississippi	17.29%	-57.71%	65.00	0.00%
Arkansas	15.45%	-57.49%	98.00	3.06%
Alabama	13.68%	-60.47%	147.00	4.08%
Full sample	29.68%	-24.66%	16933.00	14.82%

Table 3. Percentage of Gay-Friendly Firms and Local Public Opinion by Year

The sample consists of 16,993 firm-year observations from 2003 to 2009. All the firms are covered by the KLD database with non-missing “Gay and Lesbian Policies” ratings and by Compustat with non-missing state information. We use poll information from Pew Research Center for the People & the Press data archive from 2003 to 2009. We use all national wide polls conducted by Pew Research Center containing the specific question “Do you strongly favor, favor, oppose, or strongly oppose allowing gay and lesbian couples to marry legally?” We exclude firms in Alaska and Hawaii because poll information is not available for these two states. All dollar value variables are measured in 2009 constant dollars and all continuous variables are winsorized at the 0.5% and 99.5%. The detailed description of the construction of the variables is provided in the appendix.

Year	Average gross support	Average net support	# of Obs	# of gay-friendly firms	% of gay-friendly firms
2003	33.43%	-24.39%	2351	209	8.89%
2004	33.95%	-20.34%	2411	294	12.19%
2005	35.85%	-19.63%	2443	368	15.06%
2006	29.62%	-17.63%	2420	383	15.83%
2007	38.43%	-14.14%	2365	412	17.42%
2008	39.40%	-9.93%	2451	424	17.30%
2009	20.12%	-10.18%	2492	420	16.85%
Full Sample	32.91%	-16.54%	16933	2510	14.82%

Table 4. Gay-Friendly Policy and Local Public Opinion

This table presents the results of logistic regressions in which we regress the gay-friendly dummy on gross (net) support rate for gay marriage in the state where the firm's headquarter is located, a set of firm characteristics and state characteristics. *Gross support* is the proportion of gay marriage supporters among all survey interviewees in the state, and *Net support* is the proportion of gay marriage supporters net of opponents in the state. The appendix provides detailed definitions of variables. All sample firms are covered by the KLD database and the Compustat Fundamental Annual database from 2003 to 2009. All continuous variables are winsorized at 0.5% and 99.5%. All dollar value variables are measured in 2009 real dollars. The symbol ***, **, * denotes significance at the 1%, 5% and 10% levels, respectively. All values are marginal effects at means. The P-values in parentheses are based on robust standard error clustered at the firm level.

	<u>Using gross support in the regression</u>			<u>Using net support in the regression</u>		
	(1)	(2)	(3)	(4)	(5)	(6)
Gross support	0.377*** (0.000)	0.212*** (0.000)	0.084** (0.016)			
Net support				0.168*** (0.000)	0.095*** (0.000)	0.043** (0.018)
Ln (Total assets)		0.063*** (0.000)	0.061*** (0.000)		0.063*** (0.000)	0.061*** (0.000)
Ln (Firm age)		0.013*** (0.010)	0.014*** (0.005)		0.013*** (0.009)	0.014*** (0.005)
CSR		0.013*** (0.000)	0.012*** (0.000)		0.013*** (0.000)	0.012*** (0.000)
ROA		0.102*** (0.001)	0.101*** (0.002)		0.102*** (0.001)	0.100*** (0.002)
R&D		0.274*** (0.000)	0.236*** (0.000)		0.272*** (0.000)	0.235*** (0.000)
Capex		0.001 (0.991)	0.025 (0.755)		0.000 (0.997)	0.024 (0.763)
Leverage		-0.028 (0.119)	-0.024 (0.161)		-0.028 (0.127)	-0.024 (0.164)
Cash		0.106*** (0.000)	0.094*** (0.000)		0.107*** (0.000)	0.094*** (0.000)
Tobin's Q		0.003 (0.283)	0.003 (0.288)		0.003 (0.274)	0.003 (0.285)
Yearly return		-0.003 (0.443)	-0.002 (0.539)		-0.003 (0.461)	-0.002 (0.543)
Gay population			0.076** (0.028)			0.074** (0.033)
Ln (GDP per cap)			-0.014 (0.733)			-0.014 (0.716)
Ln (Population)			0.008			0.007

			(0.153)			(0.205)
Unemployment rate			0.005*			0.005
			(0.090)			(0.103)
White people			0.001			0.001
			(0.269)			(0.291)
Senior citizen			-0.005*			-0.006**
			(0.052)			(0.043)
High school graduates			0.001			0.001
			(0.574)			(0.566)
Bachelor's degree			0.001			0.001
			(0.317)			(0.400)
Marriage Rate			-0.001			-0.001
			(0.451)			(0.416)
Sex Ratio			-1.041			-1.093
			(0.352)			(0.333)
Insurance Coverage			0.001			0.000
			(0.680)			(0.736)
Religious people			-0.001***			-0.001***
			(0.009)			(0.008)
Observations	16,933	16,933	16,933	16,933	16,933	16,933
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 5. Excluding California and New York

This table presents the results of sub-sample logistic regressions of gay-friendly dummy on gross (net) support rate and other control variables. In Column (1) and (4) we exclude observations in Californian; in Column (2) and (5) we exclude New York; in Column (3) and (6) we exclude both California and New York. All continuous variables are winsorized at 0.5% and 99.5%. All dollar value variables are measured in 2009 real dollars. The symbol ***, **, * denotes significance at the 1%, 5% and 10% levels, respectively. All values are marginal effects at means. The P-values in parentheses are based on robust standard error clustered at the firm level.

	<u>Using gross support in the regression</u>			<u>Using net support in the regression</u>		
	Exclude CA (1)	Exclude NY (2)	Exclude CA and NY (3)	Exclude CA (4)	Exclude NY (5)	Exclude CA and NY (6)
Gross support	0.084*** (0.010)	0.068** (0.036)	0.061** (0.038)			
Net support				0.039** (0.015)	0.034** (0.046)	0.028* (0.053)
Ln (Total assets)	0.051*** (0.000)	0.058*** (0.000)	0.047*** (0.000)	0.051*** (0.000)	0.058*** (0.000)	0.047*** (0.000)
Ln (Firm age)	0.010** (0.020)	0.015*** (0.004)	0.010** (0.018)	0.010** (0.020)	0.015*** (0.003)	0.010** (0.018)
CSR	0.010*** (0.000)	0.012*** (0.000)	0.010*** (0.000)	0.010*** (0.000)	0.012*** (0.000)	0.010*** (0.000)
ROA	0.067** (0.037)	0.082** (0.011)	0.040 (0.208)	0.067** (0.038)	0.082** (0.010)	0.040 (0.209)
R&D	0.072 (0.266)	0.232*** (0.000)	0.065 (0.306)	0.071 (0.272)	0.231*** (0.000)	0.064 (0.312)
Capex	0.029 (0.707)	0.018 (0.822)	0.021 (0.781)	0.028 (0.716)	0.017 (0.827)	0.021 (0.787)
Leverage	-0.025 (0.138)	-0.020 (0.254)	-0.021 (0.215)	-0.025 (0.141)	-0.020 (0.259)	-0.021 (0.219)
Cash	0.102*** (0.000)	0.085*** (0.000)	0.090*** (0.000)	0.102*** (0.000)	0.085*** (0.000)	0.090*** (0.000)
Tobin's Q	0.004 (0.218)	0.001 (0.724)	0.002 (0.565)	0.004 (0.214)	0.001 (0.719)	0.002 (0.560)
Yearly return	-0.003 (0.366)	-0.001 (0.807)	-0.002 (0.556)	-0.003 (0.369)	-0.001 (0.809)	-0.002 (0.559)
Gay population	0.054* (0.067)	0.072** (0.035)	0.045 (0.111)	0.053* (0.076)	0.071** (0.038)	0.044 (0.118)
Ln (GDP per cap)	-0.011 (0.734)	-0.025 (0.515)	-0.018 (0.576)	-0.011 (0.749)	-0.025 (0.519)	-0.017 (0.597)
Ln (Population)	0.005 (0.294)	0.003 (0.567)	0.000 (0.931)	0.004 (0.378)	0.003 (0.616)	0.000 (0.997)
Unemployment rate	0.003 (0.349)	0.006* (0.052)	0.003 (0.204)	0.003 (0.354)	0.005* (0.062)	0.003 (0.210)
White people	0.001 (0.193)	0.001 (0.399)	0.001 (0.221)	0.001 (0.194)	0.001 (0.419)	0.001 (0.219)
Senior citizen	-0.006**	-0.004	-0.005*	-0.006**	-0.004	-0.005**

	(0.027)	(0.115)	(0.053)	(0.022)	(0.101)	(0.048)
High school graduates	0.002	0.000	0.002	0.002	0.000	0.002
	(0.281)	(0.823)	(0.318)	(0.245)	(0.806)	(0.288)
Bachelor's degree	0.001	0.002	0.001	0.000	0.002	0.001
	(0.611)	(0.163)	(0.399)	(0.712)	(0.211)	(0.463)
Marriage Rate	-0.001	-0.001	-0.001	-0.001	-0.002	-0.001
	(0.646)	(0.278)	(0.466)	(0.633)	(0.265)	(0.463)
Sex Ratio	-1.618	-0.067	-0.958	-1.729	-0.135	-1.043
	(0.219)	(0.953)	(0.437)	(0.190)	(0.907)	(0.399)
Insurance Coverage	-0.000	0.001	-0.000	-0.000	0.001	-0.000
	(0.762)	(0.570)	(0.718)	(0.679)	(0.611)	(0.663)
Religious people	-0.001**	-0.001**	-0.001**	-0.001**	-0.001**	-0.001**
	(0.016)	(0.014)	(0.022)	(0.015)	(0.013)	(0.022)
Observations	14,037	15,540	12,644	14,037	15,540	12,644
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 6. Local Public Opinion and the Adoption of Gay-Friendly Policy

This table presents the results of logistic regressions in which we regress the Adoption dummy on gross (net) support rate for gay marriage in the state where the firm's headquarter is located, a set of firm characteristics and state characteristics. Adoption as an indicator variable that is equal to one if a firm is gay-friendly in year t and was gay-unfriendly in year $t-1$, and zero otherwise. We restrict the sample to firms that start in our sample as gay-unfriendly, and we exclude firms after their first year of adoption because they can no longer "adopt" the policy. We also exclude firm that ever relocate headquarters in our sample period. All continuous variables are winsorized at 0.5% and 99.5%. All dollar value variables are measured in 2009 real dollars. The symbol ***, **, * denotes significance at the 1%, 5% and 10% levels, respectively. All values are marginal effects at means. The P-values in parentheses are based on robust standard error clustered at the firm level.

	Dependent variable = Adoption					
	(1)	(2)	(3)	(4)	(5)	(6)
Gross support	0.030*** (0.001)	0.023*** (0.000)	0.017** (0.023)			
Net support				0.014*** (0.001)	0.011*** (0.001)	0.008** (0.046)
Ln (Total assets)		0.007*** (0.000)	0.006*** (0.000)		0.007*** (0.000)	0.006*** (0.000)
Ln (Firm age)		-0.001* (0.100)	-0.001 (0.142)		-0.001* (0.096)	-0.001 (0.143)
CSR		0.001*** (0.001)	0.001*** (0.002)		0.001*** (0.001)	0.001*** (0.002)
ROA		0.015** (0.021)	0.015** (0.016)		0.015** (0.020)	0.015** (0.016)
R&D		0.020* (0.060)	0.019* (0.070)		0.021* (0.055)	0.019* (0.068)
Capex		-0.004 (0.723)	-0.001 (0.902)		-0.004 (0.723)	-0.001 (0.901)
Leverage		-0.004 (0.241)	-0.004 (0.205)		-0.004 (0.242)	-0.004 (0.209)
Cash		0.009** (0.033)	0.008** (0.035)		0.009** (0.030)	0.008** (0.034)
Tobin's Q		0.001 (0.121)	0.001 (0.143)		0.001 (0.129)	0.001 (0.143)
Ln (GDP per cap)			0.004 (0.580)			0.004 (0.575)
Ln (Population)			-0.000 (0.822)			-0.000 (0.797)
Unemployment rate			0.002** (0.042)			0.002** (0.041)
White people			-0.000			-0.000

			(0.555)			(0.594)
Senior citizen			-0.000			-0.000
			(0.546)			(0.548)
High school graduates			0.000			0.000
			(0.258)			(0.291)
Marriage Rate			-0.000			-0.000
			(0.804)			(0.740)
Sex Ratio			-0.025			-0.018
			(0.883)			(0.914)
Religious people			-0.000**			-0.000**
			(0.026)			(0.023)
Observations	9,820	9,820	9,820	9,820	9,820	9,820
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 7. Human Capital and Local Public Opinion

Panel A (Panel B) presents the results of sub-sample logistic regressions of gay-friendly dummy on gross (net) support rate and other control variables. Level of knowledge workers is measured at industry (three-digit NAICS) level as the percentage of workforce in the managerial, technical and scientific occupations (Standard Occupational Classification code below 300000) in the industry using data from the annual American Community Survey (ACS) from Integrated Public Use Microdata Series (IPUMS). Column (1) low knowledge worker subsample consists of firm-year observations with level of knowledge workers below sample median, and Column (2) high knowledge worker subsample consists of firm-year observations with level of knowledge workers above sample median. R&D is defined as research and development expense scaled by total assets. Column (3) low R&D subsample consists of firm-year observations with R&D below sample median, and Column (4) high R&D subsample consists of firm-year observations with R&D above sample median. Employee density is defined as the ratio of number of employees to fixed asset. Column (5) low employee density sub-sample consists of firm-year observations with employee density below sample median, and Column (6) high employee density sub-sample consists of firm-year observations with employee density above sample median. All continuous variables are winsorized at 0.5% and 99.5%. All dollar value variables are measured in 2009 real dollars. The symbol ***, **, * denotes significance at the 1%, 5% and 10% levels, respectively. All values are marginal effects at means. The P-values in parentheses are based on robust standard error clustered at the firm level.

Panel A. Using gross support in the regression						
	Low knowledge workers (1)	High knowledge workers (2)	Low R&D (3)	High R&D (4)	Low employee intensity (5)	High employee intensity (6)
Gross support	0.050 (0.262)	0.103** (0.036)	0.014 (0.632)	0.185*** (0.002)	0.028 (0.415)	0.136*** (0.007)
Observations	8,378	8,555	8,466	8,467	8,466	8,467
Firm Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
State Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Test of equal coefficients (p-value)	0.4384		0.088*		0.14	
Panel B. Using net support in the regression						
	Low knowledge workers (1)	High knowledge workers (2)	Low R&D (3)	High R&D (4)	Low employee intensity (5)	High employee intensity (6)
Net support	0.031 (0.170)	0.047* (0.065)	0.007 (0.633)	0.090*** (0.006)	0.009 (0.592)	0.074*** (0.006)
Observations	8,378	8,555	8,466	8,467	8,466	8,467
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Test of equal coefficients	0.67		0.1096		0.0693*	

(p-value)

Table 8. Local Institutional Ownership and Local Public Opinion

This table presents the results of sub-sample logistic regressions of gay-friendly dummy on gross (net) support rate and other control variables. Institutional ownership is from Thomson Reuters institutional (13f) holdings. We define an institution as a local institution if the primary address of the institution is in the same state as the holding company. We find the primary addresses of institutions by matching names of institutions to all names of financial firms recorded by Thomson One database and Capital IQ database, which provide firms' address information. Column (1) and (3) low local IO sub-sample consists of firm-year observations with local institutional ownership below sample median, and Column (2) and (4) high local IO sub-sample consists of firm-year observations with local institutional ownership above sample median. All continuous variables are winsorized at 0.5% and 99.5%. All dollar value variables are measured in 2009 real dollars. The symbol ***, **, * denotes significance at the 1%, 5% and 10% levels, respectively. All values are marginal effects at means. The P-values in parentheses are based on robust standard error clustered at the firm level.

	<u>Using gross support in the regression</u>		<u>Using net support in the regression</u>	
	Low Local IO (1)	High Local IO (2)	Low Local IO (3)	High Local IO (4)
Gross support	0.017 (0.532)	0.171*** (0.006)		
Net support			0.002 (0.890)	0.109*** (0.001)
Observations	8,466	8,467	8,466	8,467
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes

Table 9. Firm performance, Firm Policy and Local Public Opinion

This table presents the results of sub-sample OLS regressions of leading ROA (using EBITDA) and Tobin's Q on gay-friendly dummy and other firm characteristics. The 49 states are divided into high support states and low support states based on historical average of gross support rates for gay marriage. Column (1) and (2) include firm-year observations with historical average support rates above sample median, and Column (3) and (4) include firm-year observations with historical average support rates below sample median. All continuous variables are winsorized at 0.5% and 99.5%. All dollar value variables are measured in 2009 real dollars. The symbol ***, **, * denotes significance at the 1%, 5% and 10% levels, respectively. The P-values in parentheses are based on robust standard error clustered at the firm level.

	<u>High support states</u>		<u>Low support states</u>	
	ROA $t+1$ (1)	Tobin's Q $t+1$ (2)	ROA $t+1$ (3)	Tobin's Q $t+1$ (4)
Gay friendly	0.005** (0.038)	0.049* (0.054)	-0.000 (0.880)	-0.001 (0.958)
Ln (Total assets)	0.002** (0.024)	-0.014* (0.074)	0.001** (0.044)	-0.012** (0.040)
Ln (Firm age)	0.004*** (0.002)	0.025* (0.079)	0.003*** (0.006)	0.006 (0.526)
CSR	0.000 (0.312)	0.007* (0.086)	0.000 (0.228)	-0.003 (0.342)
ROA	0.783*** (0.000)	0.510*** (0.001)	0.705*** (0.000)	0.343** (0.031)
R&D	-0.075** (0.016)	1.198*** (0.000)	-0.143** (0.022)	1.268*** (0.002)
Capex	0.075*** (0.004)	0.041 (0.871)	-0.007 (0.816)	-0.262 (0.180)
Leverage	0.006 (0.276)	0.151** (0.010)	0.013* (0.056)	0.037 (0.450)
Cash	-0.048*** (0.000)	0.436*** (0.000)	-0.044*** (0.000)	0.224** (0.013)
Tobin's Q	0.009*** (0.000)	0.671*** (0.000)	0.012*** (0.000)	0.712*** (0.000)
Yearly return	0.012*** (0.000)	-0.093*** (0.000)	0.012*** (0.000)	-0.074*** (0.000)
Constant	-0.010 (0.191)	0.464*** (0.000)	0.011 (0.163)	0.519*** (0.000)
Observations	8,072	8,072	7,910	7,910
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes

Essay Three: LBO Price and the Role of Target Information

Asymmetry

Abstract

Using a sample of 236 LBOs and 787 matching deals of U.S. public targets for the period of 1984 to 2013, this paper finds that LBO target shareholders receive significantly less in takeovers than shareholders of non LBO targets. The low premium cannot be explained by the difference in target or deal characteristics. After controlling acquirer, deal and target characteristics, LBO target shareholders still receive 16% less of pre-bid equity values than non LBO targets. We investigate the role of target information asymmetry in explaining the premium gap between LBO deals and matching non LBO deals, and we find as target information asymmetry measure increases by a standard deviation, the premium gap between LBOs and non LBOs widens by 5% to 10%. We also find the influence of information asymmetry on premium gap mainly happens before merger announcement, but not after merger announcement.

Keywords:

Leveraged buyout; Merger and acquisition; Takeover premium, information asymmetry

JEL Classification: G14, G34

1. Introduction

Leveraged buyout (LBO) is characterized by the involvement of financial sponsors, which are also known as private equity (PE) investors¹², and the usage of high leverage in the transaction. Back in the late 1984s, LBO was a popular occurrence in the market of corporate control, and was depicted as an overheated phenomenon by Kaplan and Stein (1993). The volume of LBO declined significantly after entering the early 1990s, and increased by a moderate extent in the late 1990s and early 2000s. Since 2004, however, the number and volume of LBO deals rose sharply and peaked in 2006, with the annual total LBO deal volume in the U.S. exceeding \$400 billion (Gaughan, 2010). Except for the year of 2008 and 2009, LBO remains at high level in recent years and constitutes a significant portion of merger and acquisition deals in the U.S.

The prevalence of LBO transactions has drawn much attention from academic research. Proponents of LBOs, as led by Jensen (1986), believe PE investors create value in target firms by strengthening managerial incentives, reducing agency costs of free cash flow and improving operating efficiency. Their argument is well supported by empirical researches. For example, Cotter and Peck (2001) find evidence that buyout specialist monitor and motivate LBO targets. Kaplan and Stromberg (2009) suggest private equity investors create value in LBO deals by enhancing post-buyout capital structure, management incentives, and corporate governance. Guo et al. (2011) find that LBOs create value through operating gain, industry valuation multiple increase and tax benefit. Boucly et al. (2011) argue that LBO targets become more profitable and grow faster than

¹² We use the terms PE (private equity) investor, financial sponsor, and LBO acquirer interchangeably, as do Kaplan and Stromber (2009) and Officer et al. (2010).

non LBO targets post-transactions. Chung (2011) show that LBOs relieve private targets' investment constraint.

In the meantime, critics of LBOs argue PE investors expropriate target shareholders in such transactions. Perry and Williams (1994) find managers manipulate discretionary accruals to reduce announced earnings prior to the announcement of Management Buyout (MBO) proposals. Hafzalla (2009) find managers selectively disclose more bad news and quote more pessimistically to denigrate their firms before MBO transactions. Officer et al. (2010) provide direct evidence that target shareholders receive significantly less in club deal LBOs than in sole-sponsored LBO, and they believe collusion among PE investors to limit the number of competing bidders could be one explanation.

In this paper, we compare LBO premium to non-LBO premium to investigate whether PE investors pay significantly less to target shareholders than other M&A deals. We also explore whether targets' information environment affects the premium gap between LBOs and non-LBOs. PE investors, compared to corporate acquirers have more expertise in acquisition, and stronger incentive to bargain for lower price. When targets have very low information asymmetry, all bidders can fairly evaluate the target even without much expertise and bid on the same ground. When targets have very high information asymmetry, however, PE investors have advantage of their acquisition experience and expertise, they may know how to get better access to information, or how to better evaluate the target absent of high quality information, and thus may bid better than corporate acquirers.

We use a sample of 236 LBO deals and 787 matching non LBO deals that target the U.S. public firms during the period of 1984 to 2013. We find that LBO target

shareholders receive significantly less in takeovers than shareholders of non LBO targets and that the premium gap between LBOs and non LBOs is enlarged when targets' information asymmetry is high.

We use five proxies for information asymmetry, namely size, firm age, institutional ownership, residual volatility and number of analysts following. With a premium window defined from 42 days prior to deal announcement till deal completion, we find a premium gap between LBOs and non LBOs around -16% to -28% when target information asymmetry is above median, and a premium gap around 0 to -10% when target information asymmetry is below median. The negative relationship between premium gap and target information asymmetry stays robust when we control for industry and year fixed effects, acquirer, deal, and target characteristics. As target information asymmetry measure increases by a standard deviation, the premium gap between LBOs and non LBOs widens by 5% to 10%. We also find that the influence of information asymmetry on premium gap mainly happens before merger announcement, but not after merger announcement.

The remainder of the paper is organized as follows. Section 2 describes our sample of LBOs matching deals, and variable construction. Section 3 investigates the premium gap between LBO deals and non-LBO deals. Section 4 explores the relationship between premium gap and target information asymmetry. Section 5 discusses other possible explanations. Section 6 summarizes and concludes our findings.

2. Sample Formation and Variable Construction

2.1 The Sample of LBO and Matching Deals

We collect our sample of LBO deals and matching non LBO deals from the Securities Data Company's (SDC) U.S. Merger and Acquisition Database and use the LBO flag from SDC Merger and Acquisition database as our identifier of LBO deals. The first deal labeled as LBO appears in 1984, so we set our sample period 1984 to 2013. For both LBO and non LBO matching deals, we restrict our sample to completed takeover deals of U.S. public targets where the acquirer seeks to own 100% of the shares of the target and the transaction value is greater than \$10 million (in CPI-adjusted 2013 dollars). We exclude all transactions where targets are in the utilities industries (SIC code 4900-4999) or finance industries (SIC code 6000-6999). We exclude transactions labeled as spinoffs, recaps, self-tenders, repurchases or privatizations. We further require target firms to have non-missing return data at least 1 year before the merger announcement and non-missing book value of total assets for the fiscal year preceding the deal announcement in the COMPUSTAT-CRSP merged database. We exclude penny stocks, of which stock prices are lower than 2 dollars 252 trading days prior to deal announcement. This procedure leaves us 414 LBO deals. Following the comparable industry transaction technique used by Officer (2007), for each LBO deal we find its industry-size matching non LBO deals, where the targets of matching non LBO deals are in the same Fama-French 48 industries as the targets of LBO deals and have transaction value excluding assumed liabilities within $\pm 20\%$ of that of LBO deals, and the deals are announced within 18 months before or after the announcement of the LBO deals. We apply the same requirements to matching deals as to LBO deals. For 178 LBOs we can't find a matching deal, so we exclude these LBO observations. The final sample consists of 236 LBO deals and 787 matching non LBO deals.

We obtain acquirer and deal characteristics from the SDC database. We use targets' accounting and return information from COMPUSTAT-CRSP merged database, targets' institutional ownership data from Thomson Reuters Institutional (13f) Holdings database, and number of analysts following from Thomson Reuters Institutional Brokers' Estimate System (I/B/E/S) database. All money values are in constant 2013 dollars and all continuous variables are winsorized at the 1% and 99% levels.

Table 1 presents the distribution of LBO and matching non LBO deals over time. There are obvious trends in LBO intensity, with the most of LBO deals concentrated in the periods of 1986-1989, 1999-2001, 2005-2007, and 2010-2013. Matching deals follow roughly the same trends as we require matching deals to be announced within 18 months before or after the announcement of LBO deals. The similarity in time distribution of LBOs and matching non LBOs alleviates the concern of time variation in premium. Nevertheless, we control for year fixed effects in multivariate analyses.

Table 2 shows the distribution of LBO and matching non LBO deals across target industries. We follow the Fama-French 12 industry definitions in this part and as we exclude all targets in the utilities and finance industries, there are only 10 industries left in total. Both LBOs matching non LBOs have the most targets in the business equipment industry, the second most targets in other industry, the least targets in the chemicals and allied products industry, and the second least targets in the oil, gas, and coal extraction and products industry. Again, the similar distributions of LBOs and matching non LBOs across target industries help alleviate the concern of industry variation in premium. In the section of multivariate analysis, we control for industry fixed effects at the Fama-French 48 industry level.

2.3. Measuring Takeover Premium

Following Schwert (1996) and Officer et al. (2010), we calculate targets' stock return over four intervals relative to deal announcement date: day -42 to day -1 is labeled as run up, day 0 to day+126 or the delisting date, whichever is earlier is labeled as markup, day-42 to day+126 or the delisting date is labeled as premium, and day -1 to day +1 is labeled as CR3. All dates are trading days relative to day 0, and day 0 is the deal announcement date or the first trading day after deal announcement if the deal is not announced on a trading day. We also calculate three return measures: raw returns, which are targets' raw compound returns, buy-and-hold abnormal returns (BHARs), which are targets' compound returns minus compound CRSP value weighted market index (including dividend distributions), and cumulative abnormal returns (CARs), which are the sum of the targets' daily market model residuals over the corresponding periods. The market-model parameters are estimated over the -379 to -127 day period. These return measures are widely used in the M&A literature, for example Barger et al. (2008), Betton et al. (2009) and Schwert (2000).

2.4 Measuring Target Information Asymmetry

Following prior literatures (see e.g. Stoll, 1978; Blackwell et al., 1990; Brennan and Subramanian, 1995; Krishnaswami and Subramaniam, 1999; Baik et al., 2010, etc) , we use five proxies that are commonly used to measure information asymmetry, namely size, firm age, institutional ownership, residual volatility and number of analysts following. Firm size is measured by market capital for the fiscal year prior to deal

announcement; firm age is the number of years since the firm first appears in the Compustat database; institutional ownership is the total ownership held by Thomson Reuters 13f institutions four quarters before deal announcement; residual volatility is the standard deviation of market-adjusted daily stock returns in the year preceding deal announcement; number of analysts following is the number of analysts that make earning forecasts for the firm during the year before deal announcement.

Smaller and younger firms with lower institutional ownership, larger residual volatility and smaller number of analysts following are usually considered to have more information asymmetry problems, while large, old firms with higher institutional ownership, smaller residual volatility and larger number of analysts following are usually considered less information asymmetric.

2.5 Data Characteristics

Table 3 displays the averages and differences of return and target information asymmetry measures, as well as other acquirer, deal and target characteristics of all LBO and matching non LBO deals. As shown in Panel A, all measures of target runup, markup, announcement return and premium are significantly lower for LBOs than for matching non LBOs. The average return gaps between LBO targets and their matches are around 3%-4% lower in runup, 6% lower in announcement return, 7%-8% lower in markup, and 14% lower premium. In terms of absolute premiums, the raw, buy-and-hold abnormal return, and cumulative abnormal return premiums for LBO targets are 34.51%, 29.39% and 28.64%, respectively, and 48.66%, 43.21%, and 42.39% for non LBO targets, respectively. Although using a slightly different time window and not exactly the same

sample selection criteria, our premium measures are qualitatively similar to those of Officer et al. (2010), who report raw premiums of 24.04% for club private equity investor deals, 36.11% for sole private equity investor deals, 44.10% for private acquirers deals and 43.81% for public acquirers deals. As premiums measure the total (abnormal) return gained by target shareholders for the deal, we mainly focus on the premium measures in the remaining analysis of this paper and we use buy-and-hold abnormal return measures in multivariate analyses.

Panel B presents the means and differences of the information asymmetry measures for LBO targets and non LBO targets. We find LBO targets and non LBO targets have similar market capital and similar levels of institutional ownership; LBO targets tend to be older and less volatile, but have fewer analysts following than non LBO targets. It is inconclusive whether LBO targets are more information asymmetric than non LBO targets, as different measures point to different directions. Although an important motivation of M&A activities is to discipline target management (Jensen and Ruback, 1983) and past research finds PE investors actually enhance management incentives and corporate governance after transaction (Kaplan and Stromber, 2009), here we do not seem to find *ex-post* evidence that PE investors specifically target firms with information asymmetry problems.

Panel C shows acquirer, target and deal characteristics of LBOs and non LBOs differ in many aspects. Private acquirer, financial sponsor involvement, are an indicator variables that are equal to one if acquirer is a private firm, if at least one buyout or financial sponsor is involved in the acquirer, and if acquirers include target management, respectively. Compared to non LBO acquirers, LBO acquirers are much more likely to be

private firms (75% relative to 11%), much more likely to involve a buyout or financial sponsor (100% relative to 12%) and much more likely to include target management (43% relative to 0). Toehold, tender offer, all cash, and hostile are indicator variables that are equal to one if target's common shares owned by acquirer at deal announcement is greater than 5%, if the deal is a tender offer, if payment is all in cash, and if it is a hostile takeover, respectively. As we observe, compared to non LBO deals, LBO deals are more likely to have toehold (21% relative to 10%) and more likely to be settled in cash (84% relative to 61%). In terms of tender offer and hostility, LBOs and non LBOs do not seem to differ. We also investigate target characteristic differences. As reported, LBO targets generally have higher leverage, higher fixed assets, lower cash holding, lower R&D expense, lower Tobin's Q and lower ROA than targets of matching deals. Because all these acquirer and deal characteristics are claimed by academics to be strong predictors of M&A pricing (see Barger et al., 2008; Officer et al., 2010; Hartzell et al. 2004; Barger et al., 2010; Betton and Eckbo, 2010; Fich et al., 2011; Fich et al., 2013, Ang et al., 2014; Bris, 2002; Wansley et al., 1983; Huang and Walkling, 1987), and target characteristics are common controls in explaining M&A premiums (see, e.g. Officer, 2003; Barger, 2008), we control for all these characteristic in the following multivariate analyses.

3. The Low Premium of LBO deals

Taken into consideration the many differences in acquirer, target and deal characteristics between LBOs and non LBOs, we perform multivariate regressions to control for the observable differences in these characteristics and show the results in

Table 4. The dependent variables are our three premium measures: raw premium, BHAR premium and CAR premium. Our main variable of interest is the LBO dummy, indicating whether the deal is labeled as LBO in SDC Merger and Acquisition database. The other explanatory variables include observable acquirer, deal and target characteristics that are commonly controlled in the M&A literature. We use the natural logarithm of target total assets and number of analysts following. All other control variables are defined previously. We control for both year and industry fixed effects in all regressions to correct for the time and industry variation in premiums.

Our main results are statistically significant and economically important: after controlling for acquirer, deal and target characteristics, LBO prices are still 16% lower than non LBO deals, representing a more than 30% discount in premiums. Our other results are mostly consistent with the existing M&A pricing literature. For example, consistent with Betton and Eckbo (2010), Bris (2002), Wansley et al. (1983), Huang and Walkling (1987), we find that deals without toeholds, tender offers and deals settled by cash have higher premiums. In accordance with the valuation views of M&A (Shleifer and Vishny, 2003), targets with low Tobin's Q and high ROA are associated with high premiums. Larger and old targets are associated with lower premiums. This may make sense because large targets are less likely to find potential buyers and are subject to more agency problems. We use column (2) of Table 4 as our baseline model and BHAR premium as our primary premium measure in the remaining analyses.

4. Premium Gap and Information Asymmetry

Next we explore the role of target's information environment. To the extent that when targets are in bad information environment, more acquisition expertise is needed for acquirers to fairly evaluate the target, PE investors are in better position than corporate acquirers and thus the premium gap between LBOs and non-LBOs should enlarge.

In Table 5 we perform the difference in difference univariate test to explore the relationship between LBO premium gap and target information asymmetry. In Panel A, B, C, D and E, we divide the acquisition sample into two subsamples based on the medians of market capital, firm age, institutional ownership, residual volatility and number of analysts following, respectively. Column (1), (2) and (3) report the average premiums for LBOs, non LBOs and premium gaps between LBOs and non LBOs for the high information asymmetry groups; Column (4), (5) and (6) report the average premiums for LBOs, non LBOs and premium gaps between LBOs and non LBOs for the low information asymmetry groups; Column (7) reports the difference in premium gaps between the high information asymmetry groups and the low information asymmetry groups.

In Panel A, for the subsample of small targets, LBO premiums are more than 20% lower than non LBO premium; in contrast, for the subsample of big targets, LBO premiums are only moderately lower than or even the same as non LBO premiums and the premium gaps are not statistically significant. The differences in premium gaps vary from -15% to -28%, depending on return measures, and are all statistically significant. Similar patterns of results are found in Panel B, C, D and E. These results suggest that LBO acquirers pay 16% to 28% lower than non LBO acquirers when targets have high

levels of information asymmetry, and pay 0 to 10% lower than non LBOs when targets have low levels of information asymmetry.

We perform OLS regressions to test whether LBO target shareholders systematically receive deep premium discount when targets have asymmetric information environment. Results are reported in Table 6. The dependent variable is BHAR premium. The main independent variable is the interaction terms of LBO dummy and information asymmetry proxies, and we control for all acquirer, target, and deal characteristics listed in Table 3. We also control for both year and industry fixed effects in all regressions to correct for the time and industry variation in premiums.

All coefficient estimates on interaction terms are statistically significant, three at the 1% level, one at the 5% level, and one at the 10% level. The economic meanings are also important. A standard deviation decrease in Ln(Market capital) (1.36), in Ln(Firm age) (0.54), in institutional ownership (31.57), in Ln(Number of analysts following) (0.97) and a standard deviation increase in residual volatility (1.79) widen the premium gap between LBOs and non LBOs by 9.7%, 8.3%, 7.5%, 5.4% and 9.9%, respectively. The strong relationship between target information asymmetry and LBO premium gap indicates LBO acquirers take advantage of targets' information asymmetry problems and expropriate target shareholders.

It is worth noting that, for non LBO deals, the role of target information asymmetry generally has positive effects on takeover premium, as advised by the negatively significant coefficients on size and firm age, and the positively significant coefficients on residual volatility. The effects of institutional ownership and number of analysts following are not significant, which might result from multicollinearity with other

measures. These results coincide with Chatterjee et al. (2012), who find information asymmetry and investors' divergence of opinions increase takeover premiums.

We also investigate whether the premium gap between LBOs and matching deals under asymmetric environment come from pre-announcement runup, or post-announcement markup, or both. We reestimate the regressions in Table 6 by replacing the dependent variable with BHAR Runup and BHAR Markup and report results in Table 7 Panel A and B, respectively. In Panel A, all coefficient estimates on interaction terms are significant at least at the 10%, meaning information asymmetry plays a role in widening premium gaps between LBOs and non LBOs pre-announcement.

In contrast, none of the coefficient estimates on interaction terms is statistically significant in Panel B, suggesting target information asymmetry has no significant influence on widening premium gaps post-announcement. The result also makes sense because M&A announcements usually attract much attention from stakeholders and institutions, and thus even information asymmetric targets go under scrutiny post-announcement.

5. Discussion

Perry and Williams (1994) find managers manipulate discretionary accruals to reduce announced earnings prior to the announcement of Management Buyout (MBO) proposals. Hafzalla (2009) find managers selectively disclose more bad news and quote more pessimistically to denigrate their firms before MBO transactions. We examine whether pre-acquisition earnings management is directly related to takeover premium.

We followed Perry and Williams (1994) to construct the total accruals and abnormal accruals. The average abnormal accruals and total accruals in the year prior to deal announcement for LBO targets are -0.018 and -22.22, respectively, and for LBO targets are -0.007 and -26.04, respectively. We perform univariate tests; the difference between LBO targets and non-LBO targets are not statistically significant. We then include abnormal accruals, total accruals, and their interaction term with the LBO dummy in the regression, and report the results in Table 8. None of the interaction terms are significant. Furthermore, the abnormal accruals itself is not significant in the premium regression, probably because earnings management is usually done one year prior to deal announcement (Perry and Williams, 1994), but our premium measure covers from 42 days prior to deal announcement. The result does not conflict with Perry and Williams (1994), as they compared the abnormal accruals of MBO target to control firms of non-takeover targets and find MBO target firms have negative abnormal accruals. We also find negative abnormal accruals for LBO targets, but when compared to non-LBO takeover targets, the difference is not significant. Although our results show that earnings management is not directly related to takeover premium, it is still possible that management may benefit from earnings management if the manipulation leads to lower stock return one year or even earlier prior to MBO deal announcement.

We also explore whether the premium gap can be explained by target managers' ownership changes. We manually searched the SEC filing for 75 of the MBO deals, and find that on average before acquisition targets' management teams own 34% of the target, target's CEOs own 57% of the target, and after acquisition target's management teams own 58% and CEOs own 39% of target shares. However, when we include the change in

target's management ownership or change in target's CEO ownership in the regression, it does not significantly affect takeover premium. CEO's positions after acquisition are mentioned only in less 20% of the deals, and all of these deals also mention the grant of stock options. We don't exclude the possibility that in LBO deals, target managers may trade acquisition premium for personal benefit such as stock option grant, lump-sum payoff or after deal position, but with our current data, we couldn't prove it.

There are many other possible explanations for the low premium of LBO deals. For example, LBO targets may be riskier and thus can only ask for lower price to entice buyers. Although we control for target industry, size, firm age, leverage, profitability, and many other characteristics in the regression, there still could be other observable and unobservable risk factors that we haven't controlled for.

Another possible explanation is the lack of synergy gains. An important rationale for the overall very positive takeover premiums is the synergy gain (Devos et al., 2009), yet in LBO deals, such synergy gains are largely nonexistent, unless PE investors recombine firms in their portfolio. In unreported results, we match LBO deals to non-LBO deals in which acquirer and target are different industries, and then reestimate our main regression with this sample. We find that LBO targets still receive significantly less, even when compared to deals without horizontal integration potentiality. Nevertheless, we still couldn't exclude the lack of synergy hypothesis, as without post-acquisition data, it's very difficult to estimate synergy gains. We'll keep trying to find the reason(s) for the low premium of LBO deals in the future.

6. Conclusion

Using a sample of 236 LBOs and 787 matching deals of U.S. public targets for the period of 1984 to 2013, this paper finds that LBO target shareholders receive significantly less in takeovers than shareholders of non LBO targets. The low premium cannot be explained by the difference in target or deal characteristics. After controlling acquirer, deal and target characteristics, LBO target shareholders still receive 16% less of pre-bid equity values than non LBO targets.

We investigate the role of target information asymmetry in explaining the premium gap between LBO deals and matching non LBO deals. We use five proxies for information asymmetry, namely size, firm age, institutional ownership, residual volatility and number of analysts following. With a premium window defined from 42 days prior to deal announcement till deal completion, we find a premium gap between LBOs and non LBOs around -16% to -28% when target information asymmetry is above median, and a premium gap around 0 to -10% when target information asymmetry is below median. The negative relationship between premium gap and target information asymmetry stays robust when we control for industry and year fixed effects, acquirer, deal, and target characteristics. An increase of one standard deviation in information asymmetry measure widens the premium gap by 5% to 10%. We also find that the influence of information asymmetry on premium gap mainly happens before merger announcement, but not after merger announcement.

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Table 1. Sample Distribution across Time

This table presents the number and aggregate deal value of LBO deals and matching non LBO deals from year 1984 to 2013. Year is deal announcement year and the LBO flag provided by SDC is used to identify LBO and non LBO deals. We restrict our sample to completed takeover deals of U.S. public targets where the acquirer seeks to own 100% of the shares of the target. We further require the transaction value to be greater than \$10 million and target to have non-missing return data and total asset data in Compustat-CRSP merged database. We exclude targets in the utilities or finance industries. Matching deals should have target in the same Fama-French 48 industries as the LBO target, have transaction value excluding assumed liabilities within $\pm 20\%$ of that of LBO deals, and are announced within 18 months before or after the announcement of the LBO deals. All dollar value variables are measured in 2013 constant dollars and all continuous variables are winsorized at 1% and 99% levels.

Year	LBOs		Non LBOs		LBOs % of All deals	
	N	Agg. Deal Value	N	Agg. Deal Value	N	Agg. Deal Value
1984	3	834.3	1	610.4	75%	58%
1985	2	194.7	3	237.8	40%	45%
1986	9	1801.9	20	4773.5	31%	27%
1987	10	2118.9	34	5486.3	23%	28%
1988	22	4985.3	32	5588.4	41%	47%
1989	7	1076.0	32	7949.2	18%	12%
1990	3	672.9	5	996.0	38%	40%
1991	2	105.2	2	319.3	50%	25%
1992	0	0.0	1	72.5	0%	0%
1993	3	487.4	2	156.1	60%	76%
1994	2	139.0	8	754.6	20%	16%
1995	1	18.3	4	510.1	20%	3%
1996	2	3418.0	9	4183.9	18%	45%
1997	9	4443.6	21	13769.9	30%	24%
1998	4	1340.0	46	10572.8	8%	11%
1999	18	4852.7	63	13730.1	22%	26%
2000	16	3683.9	37	12883.3	30%	22%
2001	10	3307.6	39	5315.6	20%	38%
2002	4	840.4	17	2325.4	19%	27%
2003	4	715.5	13	1612.8	24%	31%
2004	4	4951.8	12	10517.5	25%	32%
2005	11	10646.1	44	37427.7	20%	22%
2006	15	21805.6	71	75601.0	17%	22%
2007	14	23208.2	56	81676.2	20%	22%
2008	5	4092.3	19	3248.3	21%	56%

2009	6	1419.7	23	18902.1	21%	7%
2010	12	9620.9	68	54542.5	15%	15%
2011	18	18621.1	45	39147.2	29%	32%
2012	13	11516.3	37	37886.1	26%	23%
2013	7	4579.8	23	16600.6	23%	22%
All	236	145497.3	787	467397.5	23%	24%

Table 2. Sample Distribution across Target Industries

This table presents the number and aggregate deal value of LBO deals and matching non LBO deals in each target industry. Industries are constructed following Fama-French 12 industry definitions and the LBO flag provided by SDC is used to identify LBO and non LBO deals. We restrict our sample to completed takeover deals of U.S. public targets where the acquirer seeks to own 100% of the shares of the target. We further require the transaction value to be greater than \$10 million and target to have non-missing return data and total asset data in Compustat-CRSP merged database. Matching deals should have target in the same Fama-French 48 industries as the LBO target, have transaction value excluding assumed liabilities within $\pm 20\%$ of that of LBO deals, and are announced within 18 months before or after the announcement of the LBO deals. We exclude targets in the utilities or finance industries, so there are 10 industries left in total. All dollar value variables are measured in 2013 constant dollars and all continuous variables are winsorized at 1% and 99% levels.

Target Industry	LBOs		Non LBOs		LBOs % of All deals	
	N	Agg. Deal Value	N	Agg. Deal Value	N	Agg. Deal Value
Consumer NonDurables	8	1363.5	7	1362.2	53%	50%
Consumer Durables	6	1142.6	7	1519.6	46%	43%
Manufacturing	23	12004.4	56	28955.6	29%	29%
Oil, Gas, and Coal Extraction and Products	4	1404.2	11	2818.0	27%	33%
Chemicals and Allied Products	1	157.1	5	1457.2	17%	10%
Business Equipment	77	45036.6	399	244048.2	16%	16%
Telephone and Television Transmission	7	9366.3	14	19633.0	33%	32%
Wholesale, Retail, and Some Services	35	15533.2	53	19535.0	40%	44%
Healthcare, Medical Equipment, and Drugs	26	27850.7	54	55244.2	33%	34%
Other	49	31638.7	181	92824.6	21%	25%
All	236	145497.3	787	467397.5	23%	24%

Table 3. Acquirer, Target and Deal Characteristics of LBO and non LBO deals

Panel A and B report means and differences of target return and target information asymmetry measures of LBOs and matching non LBOs, respectively. Panel C reports means and differences of acquirer characteristics, deal characteristics and other target characteristics of LBOs and matching non LBOs. P value is tested for the null hypothesis of zero difference in mean between the two sub-samples. All dollar value variables are measured in 2013 constant dollars and all continuous variables are winsorized at 1% and 99% levels.

	LBOs	Non LBOs	Diff	P valu e (1)- (2)
Panel A. Target return (%)	(1)	(2)	(1)-(2)	(1)- (2)
Raw returns				
Runup	6.83	11.37	-4.54**	0.01
CR3	21.26	27.74	-6.47***	0.00
Markup	27.27	34.42	-7.15***	0.01
Premium	34.51	48.66	-14.15***	0.00
Buy-and-hold abnormal returns				
Runup	5.50	8.73	-3.23**	0.05
CR3	21.17	27.46	-6.29***	0.00
Markup	23.40	31.35	-7.94***	0.00
Premium	29.39	43.21	-13.82***	0.00
Cumulative abnormal returns				
Runup	5.47	10.10	-4.63**	0.01
CR3	20.83	27.11	-6.28***	0.00
Markup	22.59	31.19	-8.61***	0.00
Premium	28.64	42.39	-13.75***	0.00
Number observations	236	787		
	LBOs	Non LBOs	Diff	P valu e (1)- (2)
Panel B. Target Information Asymmetry	(1)	(2)	(1)-(2)	(1)- (2)
Market capital (\$ million)	481.36	458.61	22.75	0.71
Firm age	14.51	13.11	1.40**	0.03
Institutional ownership (%)	42.16	43.35	-1.18	0.61
Residual volatility (%)	3.30	3.83	-0.53***	0.00

Number of analysts following	5.94	7.25	-1.31**	0.02
Number observations	236	787		
<hr/>				
	LBOs	Non LBOs	Diff	P valu e
Panel C. Acquirer, Deal and Other Target Characteristics	(1)	(2)	(1)-(2)	(1)- (2)
<hr/>				
Acquirer Characteristics				
Private acquirer	0.75	0.11	0.64***	0.00
Financial sponsor involvement	1.00	0.12	0.88***	0.00
Acquirer include target management	0.43	0.00	0.42***	0.00
 Deal Characteristics				
Toehold	0.21	0.10	0.11***	0.00
Tender offer	0.29	0.32	-0.02	0.48
All cash	0.84	0.61	0.23***	0.00
Hostile	0.01	0.01	0.00	0.90
 Other Target Characteristic				
Target leverage	0.21	0.13	0.07***	0.00
Target fixed assets	0.27	0.20	0.07***	0.00
Target R&D	0.03	0.07	-0.04***	0.00
Target capital expenditure	0.06	0.05	0.00	0.76
Target Tobin's Q	1.39	1.72	-0.32***	0.00
ROA	0.12	0.06	0.06***	0.00
Target 12 month abnormal return	-0.02	-0.07	0.05	0.17
Number observations	236	787		

Table 4. Multivariate Regressions Explaining Takeover Premiums

This table reports the results of OLS regressions of Raw premium, BHAR premiums and CAR premium on the LBO dummy and a set of target, acquirer and deal characteristics. All variables are defined previously and all regressions include year and industry effects. All dollar value variables are measured in 2013 constant dollars and all continuous variables are winsorized at 1% and 99% levels. P values based on t-tests with heteroskedasticity-robust standard errors are in parentheses. ***, ** and * indicate that the coefficient estimate is significantly different from zero at the 1%, 5% and 10% levels, respectively.

	Raw premium (1)	BHAR premium (2)	CAR premium (3)
LBO	-24.374*** (0.000)	-23.265*** (0.000)	-16.090*** (0.006)
Ln(Market capital)	-6.538*** (0.001)	-6.147*** (0.002)	-10.019*** (0.000)
Ln(Firm age)	-5.514* (0.070)	-5.918** (0.048)	-4.181 (0.140)
Institutional ownership	0.008 (0.907)	0.007 (0.918)	0.077 (0.267)
Residual volatility	2.509* (0.057)	2.510* (0.050)	0.032 (0.982)
Ln(Number of analysts following)	1.496 (0.495)	1.487 (0.496)	3.985* (0.088)
Target leverage	9.390 (0.274)	6.817 (0.407)	1.234 (0.869)
Target fixed assets	6.869 (0.566)	4.798 (0.684)	8.217 (0.523)
Target R&D	14.107 (0.558)	16.945 (0.464)	-41.809 (0.107)
Target capital expenditure	24.103 (0.541)	16.723 (0.669)	57.832 (0.128)
Target Tobin's Q	-4.976** (0.015)	-5.258*** (0.008)	-2.647 (0.238)
Target ROA	30.373** (0.043)	31.074** (0.029)	-46.609*** (0.008)
Target 12 month abnormal return	3.249 (0.307)	3.544 (0.246)	-21.461*** (0.000)
Private acquirer	-4.197 (0.357)	-5.166 (0.253)	-1.903 (0.677)

Financial sponsor involvement	7.836	8.107	3.068
	(0.146)	(0.117)	(0.488)
Acquirers include target management	2.135	3.168	0.352
	(0.697)	(0.565)	(0.951)
Toehold	-3.643	-7.383	3.394
	(0.430)	(0.101)	(0.502)
Tender offer	4.056	5.276*	4.823*
	(0.194)	(0.085)	(0.100)
All cash	7.474**	7.003**	6.156*
	(0.032)	(0.038)	(0.065)
Hostile	6.981	-5.366	13.747*
	(0.564)	(0.630)	(0.055)
Constant	65.464***	51.064**	43.806*
	(0.004)	(0.028)	(0.086)
Observations	1,023	1,023	1,023
R-squared	0.190	0.200	0.318
Year FE	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes

Table 5. Premium Gap and Information Asymmetry

This table contains average premiums for LBOs and non LBOs, premium gaps between LBOs and non LBOs for different subsamples, and differences in premium gaps across different subsamples. In Panel A, B, C, D and E, we divide the acquisition sample into two subsamples based on the medians of market capital, firm age, institutional ownership, residual volatility and number of analysts following, respectively. All continuous variables are winsorized at 1% and 99% levels. ***, ** and * indicate that the difference is significantly different from zero at the 1%, 5% and 10% levels, respectively.

Panel A. Size	Small			Big			Diff in Diff
	LBO	Non LBO	Diff (1)-(2)	LBO	Non LBO	Diff (4)-(5)	Diff (3)-(7)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Raw return premium	35.24	56.99	-21.76***	33.70	40.55	-6.84	-14.91**
Buy-and-hold abnormal return premium	30.25	52.45	-22.19***	28.44	34.24	-5.80	-16.40**
Cumulative abnormal return premium	28.49	56.66	-28.17***	28.80	28.50	0.30	-28.47***
Number observations	124	388		112	399		

Panel B. Firm Age	Young			Mature			Diff in Diff
	LBO	Non LBO	Diff (1)-(2)	LBO	Non LBO	Diff (4)-(5)	Diff (3)-(7)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Raw return premium	37.49	53.96	-16.46***	32.24	42.30	-10.07**	-6.40
Buy-and-hold abnormal return premium	31.66	48.96	-17.30***	27.67	36.33	-8.66*	-8.64
Cumulative abnormal return premium	30.57	51.22	-20.66***	27.17	31.79	-4.63	-16.03**
Number observations	102	429		134	358		

Panel C. Institutional Ownership	Low			High			Diff in Diff
	LBO	Non LBO	Diff (1)-(2)	LBO	Non LBO	Diff (5)-(6)	Diff (3)-(7)
	(1)	(2)	(3)	(5)	(6)	(7)	(9)
Raw return premium	34.83	52.28	-17.46***	29.85	39.36	-10.83**	-6.62

Buy-and-hold abnormal return premium	28.94	47.06	-18.12***	30.74	36.29	-9.51*	-8.61
Cumulative abnormal return premium	26.57	48.46	-21.89***	34.69	41.89	-5.55	-16.34**
Number observations	119	394		117	393		

Panel D. Residual Volatility	High			Low			Diff in Diff
	LBO	Non LBO	Diff (1)-(2)	LBO	Non LBO	Diff (4)-(5)	Diff (3)-(7)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Raw return premium	34.23	54.63	-20.40***	34.69	41.89	-7.19	-13.21*
Buy-and-hold abnormal return premium	30.00	49.86	-19.87***	28.99	35.68	-6.69	-13.18**
Cumulative abnormal return premium	24.08	50.31	-26.23***	31.65	33.41	-1.76	-24.47***
Number observations	94	418		142	369		

Panel E. Number of Analysts Following	Low			High			Diff in Diff
	LBO	Non LBO	Diff (1)-(2)	LBO	Non LBO	Diff (4)-(5)	Diff (3)-(7)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Raw return premium	34.64	52.39	-17.74***	34.30	44.68	-10.38*	-7.36
Buy-and-hold abnormal return premium	29.17	46.58	-17.41***	29.74	39.63	-9.89	-7.52
Cumulative abnormal return premium	25.91	45.10	-19.19	32.91	39.50	-6.58	-12.61*
Number observations	144	406		92	381		

Table 6. LBO Premiums and Target Information Asymmetry

This table reports the results of OLS regressions of BHAR premiums on the LBO dummy, the interaction terms of LBO and target information asymmetry measures, and a set of target, acquirer and deal characteristics. All variables are defined previously and all regressions include year and industry effects. All dollar value variables are measured in 2013 constant dollars and all continuous variables are winsorized at 1% and 99% levels. P values based on t-tests with heteroskedasticity-robust standard errors are in parentheses. ***, ** and * indicate that the coefficient estimate is significantly different from zero at the 1%, 5% and 10% levels, respectively.

	Dependent variable=BHAR premium				
	(1)	(2)	(3)	(4)	(5)
LBO	-62.757*** (0.000)	-62.635*** (0.000)	-35.855*** (0.000)	-6.343 (0.426)	-32.755*** (0.000)
LBO*Ln(Market capital)	7.110*** (0.007)				
LBO*Ln(Firm age)		15.410*** (0.008)			
LBO*Institutional ownership			0.239** (0.027)		
LBO*Residual volatility				-5.508*** (0.007)	
LBO*Ln(Number of analysts following)					5.565* (0.090)
Ln(Market capital)	-8.071*** (0.000)	-6.328*** (0.001)	-6.038*** (0.002)	-6.307*** (0.001)	-6.179*** (0.002)
Ln(Firm age)	-6.202** (0.036)	-9.844*** (0.006)	-5.876** (0.049)	-6.025** (0.046)	-6.061** (0.043)
Institutional ownership	0.022 (0.755)	0.014 (0.842)	-0.050 (0.498)	0.013 (0.854)	0.007 (0.924)
Residual volatility	2.400* (0.059)	2.446* (0.055)	2.423* (0.058)	3.603** (0.010)	2.467* (0.053)
Ln(Number of analysts following)	1.290 (0.554)	1.392 (0.522)	1.258 (0.564)	1.228 (0.572)	0.147 (0.951)
Target leverage	6.866 (0.401)	8.925 (0.278)	6.319 (0.440)	7.978 (0.338)	6.659 (0.417)
Target fixed assets	5.106 (0.665)	3.763 (0.747)	5.222 (0.656)	6.603 (0.576)	5.556 (0.637)

Target R&D	14.843 (0.521)	15.624 (0.496)	17.385 (0.453)	18.684 (0.420)	16.505 (0.476)
Target capital expenditure	16.732 (0.668)	13.837 (0.722)	16.347 (0.675)	14.618 (0.707)	15.175 (0.698)
Target Tobin's Q	-5.135*** (0.010)	-5.501*** (0.006)	-5.284*** (0.008)	-5.573*** (0.005)	-5.270*** (0.008)
Target ROA	32.183** (0.024)	34.470** (0.017)	32.372** (0.023)	33.676** (0.018)	31.092** (0.029)
Target 12 month abnormal return	4.241 (0.164)	3.623 (0.233)	3.755 (0.219)	3.732 (0.218)	3.830 (0.206)
Private acquirer	-5.787 (0.197)	-5.616 (0.216)	-5.956 (0.182)	-5.436 (0.223)	-5.618 (0.208)
Financial sponsor involvement	8.196 (0.108)	7.866 (0.124)	8.475* (0.099)	8.317 (0.107)	8.279 (0.107)
Acquirers include target management	9.826 (0.131)	5.316 (0.333)	9.085 (0.130)	5.944 (0.292)	5.841 (0.313)
Toehold	-6.526 (0.148)	-7.534* (0.094)	-7.772* (0.087)	-7.354 (0.101)	-7.956* (0.079)
Tender offer	5.143* (0.093)	5.410* (0.077)	5.303* (0.082)	4.841 (0.114)	5.027* (0.098)
All cash	7.118** (0.034)	7.466** (0.027)	7.276** (0.031)	8.212** (0.016)	7.293** (0.031)
Hostile	-4.853 (0.620)	-1.974 (0.860)	-6.776 (0.514)	-1.489 (0.886)	-5.896 (0.587)
Constant	59.506*** (0.009)	62.292*** (0.008)	57.147** (0.019)	46.207* (0.059)	57.519** (0.020)
Observations	1,023	1,023	1,023	1,023	1,023
R-squared	0.206	0.205	0.204	0.206	0.202
Year FE	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes

Table 7. Runup and Markup

This table reestimates the regressions in Table 6 by replacing the dependent variable with BHAR Runup in Panel A, and with BHAR Markup in Panel B. All variables are defined previously and all regressions include year and industry effects. All dollar value variables are measured in 2013 constant dollars and all continuous variables are winsorized at 1% and 99% levels. P values based on t-tests with heteroskedasticity-robust standard errors are in parentheses. ***, ** and * indicate that the coefficient estimate is significantly different from zero at the 1%, 5% and 10% levels, respectively.

Panel A.	Dependent variable=BHAR Runup				
	(1)	(2)	(3)	(4)	(5)
LBO	- 29.024*** (0.000)	-17.533** (0.047)	- 11.461** (0.011)	- 2.135 (0.594))	- 11.164** (0.011)
LBO*Ln(Market capital)	4.459*** (0.000)				
LBO*Ln(Firm age)		5.195* (0.093)			
LBO*Institutional ownership			0.136*** (0.009)		
LBO*Residual volatility				- 2.082* (0.051))	
LBO*Ln(Number of analysts following)					4.049** (0.011)
Observations	1,023	1,023	1,023	1,023	1,023
R-squared	0.216	0.207	0.210	0.209	0.210
Other controls	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes

Panel B.	Dependent variable=BHAR Markup				
	(6)	(7)	(8)	(9)	(10)
LBO	-21.038 (0.146)	- 32.232*** (0.008)	- 15.724** (0.038)	- -7.137 (0.247))	- 14.620** (0.048)
LBO*Ln(Market capital)	1.144				

	(0.589)				
LBO*Ln(Firm age)		6.868			
		(0.113)			
LBO*Institutional ownership			0.020		
			(0.824)		
LBO*Residual volatility				-2.456	
				(0.114)	
				)	
LBO*Ln(Number of analysts following)					-0.037
					(0.989)
Observations	1,023	1,023	1,023	1,023	1,023
R-squared	0.195	0.197	0.195	0.197	0.195
Other controls	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes

Table 8. LBO Premiums and Target Earnings Management

This table reports the results of OLS regressions of BHAR premiums on the LBO dummy, the interaction terms of LBO and target abnormal accruals and total accruals, and a set of target, acquirer and deal characteristics. All variables are defined previously and all regressions include year and industry effects. All dollar value variables are measured in 2013 constant dollars and all continuous variables are winsorized at 1% and 99% levels. P values based on t-tests with heteroskedasticity-robust standard errors are in parentheses. ***, ** and * indicate that the coefficient estimate is significantly different from zero at the 1%, 5% and 10% levels, respectively.

	Dependent variable=BHAR premium			
	(1)	(2)	(3)	(4)
LBO	-20.650*** (0.001)	-20.837*** (0.001)	-21.103*** (0.002)	-21.133*** (0.002)
LBO*Abnormal accruals		2.655 (0.883)		3.534 (0.845)
LBO*Total accruals			-0.014 (0.797)	-0.016 (0.772)
Abnormal accruals	-3.973 (0.615)	-5.479 (0.543)		-4.716 (0.603)
Total accruals	-0.027 (0.458)		-0.026 (0.549)	-0.023 (0.596)
Ln(Market capital)	-6.605*** (0.002)	-6.140*** (0.002)	-6.604*** (0.002)	-6.589*** (0.002)
Ln(Firm age)	-6.383** (0.034)	-6.169** (0.042)	-6.444** (0.033)	-6.350** (0.035)
Institutional ownership	0.004 (0.956)	0.000 (0.997)	0.005 (0.945)	0.003 (0.969)
Residual volatility	2.548** (0.047)	2.582** (0.045)	2.588** (0.042)	2.544** (0.048)
Ln(Number of analysts following)	1.580 (0.464)	1.752 (0.417)	1.488 (0.495)	1.540 (0.479)
Target leverage	4.662 (0.557)	5.690 (0.485)	4.441 (0.579)	4.851 (0.540)
Target fixed assets	1.577 (0.894)	3.512 (0.765)	1.576 (0.894)	1.728 (0.884)
Target R&D	13.617 (0.560)	13.727 (0.559)	13.122 (0.572)	13.747 (0.558)
Target capital expenditure	15.500 (0.691)	15.853 (0.684)	15.536 (0.691)	15.562 (0.690)
Target Tobin's Q	-4.205**	-4.474**	-4.237**	-4.179**

	(0.039)	(0.026)	(0.039)	(0.040)
Target ROA	30.642**	30.346**	30.644**	30.711**
	(0.034)	(0.036)	(0.034)	(0.034)
Target 12 month abnormal return	3.814	3.868	3.705	3.807
	(0.212)	(0.207)	(0.227)	(0.213)
Private acquirer	-3.562	-3.543	-3.814	-3.554
	(0.430)	(0.433)	(0.398)	(0.432)
Financial sponsor involvement	5.204	5.592	5.348	5.253
	(0.295)	(0.268)	(0.282)	(0.291)
Acquirers include target management	2.419	2.343	2.522	2.496
	(0.656)	(0.669)	(0.649)	(0.655)
Toehold	-7.523*	-7.351	-7.674*	-7.605*
	(0.097)	(0.103)	(0.090)	(0.093)
Tender offer	5.191*	5.198*	5.153*	5.152*
	(0.094)	(0.094)	(0.096)	(0.097)
All cash	6.611**	6.409*	6.624*	6.755**
	(0.050)	(0.058)	(0.051)	(0.048)
Hostile	-3.425	-3.507	-3.467	-3.441
	(0.759)	(0.752)	(0.755)	(0.756)
Constant	36.903**	33.850*	36.148**	36.584**
	(0.038)	(0.063)	(0.043)	(0.042)
Observations	995	995	995	995
R-squared	0.194	0.193	0.194	0.194
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes